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From the Rise of the Group Concept to the Stormy Onset of Group Theory in the New Quantum Mechanics. A saga of the invariant characterization of physical objects, events and theories.

L. Bonolis(*)

Dipartimento di Fisica, Università di Roma "La Sapienza"
P.le Aldo Moro 5, I-00185 Rome, Italy

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"If man had limited himself to the accumulation of facts, then science would be nothing but a sterile nomenclature and the great laws of nature would have remained unknown forever".

Pierre Simon de Laplace

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(*) E-mail: luisa.bonolis@roma1.infn.it

PART I

1. – Introduction

The XIX century was the century in which mathematics and physics emerged as separate subjects. An increased concern for rigor, the rebirth of the axiomatic method, the emergence of abstraction, were some of the general mathematical features of that century which roused the feeling that "mathematics" was strictly speaking only "pure mathematics", and that all the rest, particularly applied mathematics, should have a respectable but subordinate position. Physicists on their side were not very interested in mathematics: to work in physics meant mostly making experiments, and in this sense there was a deep gap between mathematics on the one side and physics on the other; even Fourier's work was regarded as mathematics; things changed with Maxwell, who thought that in teaching "natural philosophy" —which at the time meant training in pure mathematics on the one side and in experiment on the other— each realm of physics is not to be regarded "merely as a collection of facts to be coordinated by means of the formulae laid up in stone by the pure mathematicians, but as itself a new mathesis by which new ideas may be developed" [Maxwell 1873]. Hertz's celebrated dictum: "Maxwell's theory is Maxwell's system of equations" well expresses the spirit of the transition to modern theoretical physics, underlying Maxwell's synthesis. Nevertheless, physicists working in the second half of the XIX century continued to be above all empirically minded, and barely interested in "formal investigations", which were on the contrary the real aim of "mathematical physicists", who did not care very much about physics, the root of their inspiration.

Towards the end of the XIX century, theoretical physics grew in importance in discovering a new commitment to mathematics, and theoretical physicists began also distancing from experimentalists, but the role of theoretical physicist as such was far from being well established. It is a well-known story that when in 1874 Planck entered the University of Munich and discussed the prospects of research in physics with Philipp von Jolly, the professor of physics there, he was told that physics was essentially a complete science, particularly after the discovery of energy conservation law, with little prospect of further developments. Fortunately Planck decided to study physics despite the bleak future for research that was presented to him, and later recalled that when in 1889 he himself succeeded Kirchhoff as Professor at Berlin University he was the only theoretician in a world of experimentalists, who moreover considered theoretical physics a "superfluous" discipline. In its gradual emergence as an autonomous discipline physics became a subject with a dominant experimental branch, and a smaller theoretical one [Jungnickel and McCormmach 1986], until 1915 when Wilhelm Wien, who had recently been awarded the Nobel Prize for physics, could speak of an equal union of the two branches to form "the now mighty theoretical physics".

Calculus had been the mathematical language of physics during the XIX century, when mathematics of differential equations initiated by Newton and Leibniz had triumphally exploded. The importance of these equations and of the related computational techniques in physics is underlined by the circumstance that many treatises on mathematical analysis included a chapter with the explicit title "The equations of mathematical physics", a habit which persisted up to the famous book by Whittaker and Watson, *Modern Analysis*, published in 1902. This triumph was certainly one of the main reasons to think, at the end of the XIX century, that theoretical physics was eventually a complete science, that had reached its conclusion; nothing more would be discovered, everything could be computed, from astronomical events to the invisible Maxwell fields. Heinrich Hertz stated that Maxwell's electromagnetic theory of light was his equations, and theoretical physicists, such as Hendrik Antoon Lorentz, known for his high mastery of mathematical tools, were also valued for their creative manipulation of physical imagery. Mathematics was more and more taken as a natural aspect of physics, but calculus no longer seemed adequate to describe domains such as electrodynamics and thermodynamics, and physicists had developed forms of mathematics such as vector algebra and statistical mechanics for their own purposes, within the context of their own problems.

Moving from the more concrete metric aspects concerning space, that is from geometry, mathematics had just invented a great number of formal instruments that at first sight were quite far from a practical description of reality: geometry was unveiling its most abstract face. At the beginning of the XX century the physicist Ernst Mach expressed his ideas about space and geometry which were incorporated into his last book *Erkenntniss und Irrtum* published in 1905. Jeremy Gray has duly recalled how Mach "allowed that at a symbolic level one could study Riemann-style n-dimension manifolds, but this was analysis, and the more the symbols eclipsed the objects the less fruitful the enquiry became" [Gray 1999a, 74]. In reconstructing the history of geometry leading up to the discovery of non-Euclidean geometry by Bolyai and Lobachevskii, and the physical conception of geometry presented by Riemann, Mach pointed out the risks of researches conducted "with no idea of being applied to reality". In support of this remark he mentioned "the advances made in mathematics by Clifford, Klein, Lie and others", and commented that

> "Seldom have thinkers become so absorbed in revery, or so far estranged from reality, as to imagine for our space a number of dimensions *exceeding the three of the given space of sense*, or to conceive of representing that space by any geometry that departs appreciably from the Euclidean. Gauss, Lobachevskii, Bolyai, and Riemann were perfectly clear on this point, and cannot certainly be held responsible for the grotesque fictions which were subsequently constructed in this domain."

In that same period, properties representing a generalization of concepts widespread in geometry such as Lorentz transformations of special relativity, together with the power and possibilities of vector, and particularly of tensor analysis which set the stage for general relativity, definitely shifted the mathematical language of physics away from the "Cartesian shell". A new form of interaction between mathematics and physics, atypical of classical physics, manifested itself and mathematics became more and more a structural foundation for the description of physical reality, in a process which also forged a new relationship between mathematicians and physicists.

From the strong push toward a certain kind of self-definition of mathematicians, also derived the view that mathematics should possibly be without reference to, or motivation from, physical situations. But the interaction between mathematicians and physicists continued to be very strong, so that they came to share an area of common intellectual and cultural background at the turn of the XIX century, when Einstein's conceptual revolution changed Man's understanding of the physical universe([1]). All this happened not without some rivalries, but physicists needed mathematics as it was developing in the research of mathematicians, and mathematicians drew inspiration once again in the problems of physics, which might be the occasion for new mathematical insight. In 1905 Poincaré was the first to note the invariance in form of Maxwell equations respect to the 10-parameter group of extended Lorentz transformations, and later Minkowski showed that the space-time geometry determined by the Poincaré group was much more natural and mathematically elegant than that on which physics had previously based. And indeed Galileo's group is a special case of Poincaré's group obtained by letting the parameter c (the velocity of light), on which the elemens of the group depend, tend to infinity.

But a theory based on the Poincaré group of linear transformation, even being the latter the symmetry group of space-time, provides no natural explanation for the equality of the inertial and the gravitational mass, and it cannot alone prescribe the form of the dynamical laws. Einstein was thus led to postulate that "the general laws of nature are to be expressed by equations which hold good for all systems of coordinates, that is are co-variant with respect to any substitution whatever (generally covariant)" [Einstein 1916]. Einstein's program of general relativity, which required the invariance under a group of differentiable transformations, as a way to determine the explicit form of gravitational interaction, found its clue in the methods of tensor calculus, where mathematicians had already solved the problem of the transition from the rectangular Minkowskian coordinates to arbitrary curvilinear coordinates. It was an epoch-making period in establishing a completely new relationship between physics and geometry since Newton's *Principia* —where mechanics had triumphed using the language of Euclidean geometry— and Lagrange's *Mécanique analytique*, where the author had boldly declared that "no geometrical figures would be found" —whereas about 250 where present in Newton's work.

In showing that gravity was nothing but the geometrical curvature of four-dimensional space Einstein had astonishlingly forced geometry into physics, well beyond mathematicians' most extreme dreams: "The discovery of general relativity forced a revision of our views on the relationship of geometry in an entirely new setting. . . ", as John von Neumann, a mathematician deeply committed to physics, commented much later. [von Neumann 1961, 3]

2. – The nature of Geometry, a new view: Felix Klein, Sophus Lie and the transformation groups

Einstein's request that the laws governing gravitation should be covariant with respect to all coordinate transformations sounded quite familiar to mathematicians like Felix Klein, who about thirty years earlier, when he was only 23, had pronounced in his *Erlanger Programm* that what is important are not geometrical figures, but the groups of transformations under which they remain invariant. According to his unified approach each of the geometries must be reduced to the study of motions and properties of a space that are invariant under a particular group of transformations, and infinite groups can be used to classify different geometries: Euclidean geometry, for example, is characterized by the group of translations and rotations in the plane.

([1]) Einstein always objected to the idea that relativity was revolutionary and stressed that his theory was the natural completion of the work of Faraday, Maxwell and Lorentz [Einstein 1921], and for many years after 1905 he used the expression "relativity principle", rather than "relativity theory".

The invariance group of general relativity determined not only the geometry, but even the physics of gravitation. As a consequence of the invariance under the group of differentiable transformations, space-time was to be regarded as a four-dimensional Riemannian manifold. The coefficients of the quadratic differential form $g_{ik}\mathrm{d}x^i\mathrm{d}x^k$, which represents the square of the distance between two neighboring points of the manifold, determine both the geometrical properties of the manifold and the properties of the gravitational fields. Thus Klein recognized that there was "only one step" from the ideas at the basis of the special and general theory to his *Erlanger Programm*, remarking that the special theory could be thought of as the invariant theory of the group of Lorentz transformations [Klein 1910].

The German Felix Klein and the Norwegian Sophus Lie had encountered group theory in Paris, in the person of Camille Jordan, a leading mathematician, who had just published his monumental *Traité des substitutions et des équations algèbriques* (700 pages) considered for a long time the "Bible" of the theory of finite permutation groups([2]). They had met in Berlin in 1869 establishing a strong personal and scientific relationship. Klein recalled later their short stay in Paris in the Spring of 1970, during which they derived from Jordan a deep interest in group theory and in the notion of symmetry which had a crucial role in their subsequent scientific careers: "Einen großen Eindruck machte mir Camille Jordan, dessen 'traité des substitutions et des équations algébriques' eben erschienen war und uns ein Buch mit sieben Siegeln erschien" (Camille Jordan impressed me very much. His "Traité des substitutions et des équations algébriques", just published, was to us a book with seven seals) [Wussing 1984, 180].

At the end of the XVIII century the study of various permutations groups had become increasingly important through the work of Joseph Louis Lagrange, Paolo Ruffini, Carl Friedrich Gauss, Niels Abel, Augustine Cauchy. Before the word "group" emerged in the mathematical literature, there was a long period of development in which mathematicians applied group-theoretical results without the concept of group being explicitly defined [Kleiner 1986, Wussing 1984]. Group theory emerged as a unifying concept which was the final abstraction of ideas that were common to a number of different areas: number theory (Leonhard Euler, Adrien Marie Legendre and Karl Friedrich Gauss)([3]), geometry and crystallography. But the basic problem that for the first time gave rise explicitly to the group concept was the problem of the solvability of algebraic equations, and it is in this area that the word "group" was first applied. Up to about the end of the XVIII century, algebra consisted in large part of the study of solutions of polynomial equations. Evariste Galois, whose aim went beyond finding a method for solvability of equations, stressed how "From the beginning of this century [XIX] computational procedures have become so complicated that any progress by those means has become impossible"([4]).

([2]) In 1866, at the age of seventeen, Klein had become Plücker's assistant in the Physics Department of the University in Bonn. Klein held a chair from the early age of 23 in Erlangen.

([3]) Although Euler's work is, of course, not stated in group-theoretic terms he does provide examples of what will be called Abelian groups. Gauss in 1801 was to take Euler's work much further; a considerable part of his work represents a fair amount of theory of Abelian groups.

([4]) Permutations were first studied by Lagrange in his long paper (over 200 pages) on the theory of algebraic equations (*Réflexions sur la résolution algébrique des équations*, 1770/1771). He used the group concept only implicitly, but several fundamental ideas of Galois's theory are already present in his work, which had an extraordinarily great influence on the further development of the theory, in particular on Ruffini, Abel and Galois himself. In 1799 Ruffini, who was a student and admirer of Lagrange, published a work whose purpose was to demonstrate

In 1831 Galois was the first to really understand that the algebraic solution of an equation was related to the structure of a group of permutations related to the equation, and to use the term "group" in a technical sense. Galois called his theory "une simplification intellectuelle", and was well aware he was standing on the beginning of a new methodological orientation in algebra. He was beginning to think in term of structures: it was not through voluminous calculations that the solvability question would be decided, but rather through analysis of the structure of a permutation group. Galois died in 1832, after having been fatally wounded during a duel; his work had disclosed the power of the theory that would trigger the development of the permutation-theoretic group concept in the following decades, but his papers were published by Liouville only in 1846(5). Until that year the connection between permutation theory and the theory of solvability of equations was loose and indirect; after that date the two theories began to influence one another directly. The first important publications linking the two theories are due to Enrico Betti, who presented Galois theory in several works beginning in 1851. Betti, one of the leaders of the Italian mathematical school, aimed at systematizing the theory developing the group-theoretic foundations in particular, and was the first to prove that the Galois group of an equation is actually a group of permutations in the mathematical sense (*i.e.* it is multiplicatively closed).

In fact Galois' work remained nearly unknown for a long time, and only by Jordan's *Traité* was the newly gained field opened up to a wider circle of mathematicians(6). Alfred Clebsch, who had been Klein's menthor when he studied in Göttingen, actively encouraged Jordan's application of group theory to geometry, but at that time in Paris there was a tendence to consider his work on finite group theory too abstract, not worth of further research. Convinced of the outstanding role of group theory for the future development of mathematics, Jordan inspired Klein and Lie with these relevant ideas, setting the future trend in research for both.

The intensive expansion of geometry in the XIX century witnessed the evolution toward modes of thought abandoning the idea that geometry is something unique, so that its content, methods and goals are no more conceivable in terms of the millennial Euclidean tradition. Some aspects of this evolution were starting points of an implicit group-theoretic mode of thought in geometry. The novelties introduced by the discovery of new types of geometries —including non-Euclidean, affine, projective, differential, etc— made a matter of topical interest the question of the inner unity of geometry and the organizational principles for classifying its branches giving a general description of

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the insolubility of the general quintic equation by radicals. Ruffini's work goes well beyond that of Lagrange on wich is based. In 1826 Niels Abel, independently of Ruffini, succeded in solving the problem of the solution of the quintic equation by proving the impossibility of a radical expression that represents a solution of the general fifth —or higher— degree equation. In his work of 1829, on what became known as Abelian polynomial equations, Abel remarked the connection between the solvability of certain polynomial equations and the commutativity of the group of permutations of their roots; he showed a sort of commutativity property which since then has been attached to the term "Abelian". In 1844 Cauchy published a major work which sets up the theory of permutations as a subject in its own right, and where he took the conspicuously non-commutative behaviour of permutations seriously.

(5) In his prefatory remarks Liouville, in regarding a central achievement of the theorem about the solvability of equations, completely missed the group-theoretic core of Galois's method.

(6) During the period 1860-1880 Jordan wrote over 30 articles on groups, so that his *Traité* embodied the substance of most of Jordan's publications up to 1870.

all the geometric systems. The introduction of an arbitrarily large, but finite, number of dimensions led also to abstraction within geometry itself. Starting from 1800, with the development of projective geometry, "descriptive" and "metric" characters of geometric notions invariant under isometries begins to receive a clear distinction. In another move toward abstraction, a "space" (*Raum*), according to Riemann, became a "number manifold" (*Zahlenmannigfaltigkeit*) of dimension determined by the number of independent parameters. The development of non-Euclidean geometry and the view of space as a number manifold separated the study of objective physical space from the study of mathematical spaces, of physics from geometry. Both facts had a decisive importance for group theory in the last third of the XIX century, and provided the epistemological basis for the definitive classification of "geometries" undertaken by Felix Klein.

Physics, and especially mechanics and electromagnetism, increasingly found itself forced to consider simultaneously more than two physical magnitudes. The close connection between mathematics and physics particularly in the work of Lagrange, Jacobi and Gauss gave a strong impulse to the development of n-dimensional geometry, which also imparted an essential impulse to the effort to classify all of geometry([7]). Klein, who was particularly concerned with discrete transformation groups, placed the concept of group and the notion of invariance at the heart of his *Erlanger Programm* (1872), where a group-theoretic classification of the different geometrical methods then in use was given. With Klein geometry becomes the study of motions and properties of a space that are invariant under a given group of transformations; he showed that infinite groups can be used to classify different geometries. The old apparent monolithic unity was gone. Accepting the study of group structure as a research program, Klein put forward a new unified view according to which both Euclidean geometry and non-Euclidean geometry achieved the same "status": any geometry consists of a space of points and a group of transformations that move the figures in the space around while preserving the properties appropriate to that geometry. The study of geometry was the study of transformation groups.

By the time he was in Paris, Lie was already interested in the application of group-related notions to differential equations. Seeking a generalization of the Galois theory for algebraic equations, in 1871/1872 Lie began an intensive study of systems of partial differential equations inspired especially by Jacobi's analytical ideas([8]). Lie had observed

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([7]) Klein's work as Plücker physics assistant had an obvious influence on the development of his group-theoretic thinking in geometry. In the same years he followed mechanical-physical interests and was well aware of what he himself called the "intimate connection" of the different directions of development in geometry and in mechanics. In 1875 he was offered a chair at the Technische Hochschule at Munich where he taught advanced courses to large numbers of excellent students such as Adolf Hurwitz, Walter von Dyck, Carl Runge, Max Planck, Luigi Bianchi and Gregorio Ricci-Curbastro. Arnold Sommmerfeld was Klein's pupil first, and later his assistant for many years. In 1886 Klein moved to the University of Göttingen which he sought to establish as the foremost mathematics research centre in the world. He introduced weekly discussion meetings and a reading room with a mathematical library. Actually Göttingen was to serve as a model for the best mathematical and physical research centres throughout the world such as the *Institute for Advanced Study in Princeton* and the French *Institut des Hautes Etudes Scientifiques* at Bures-sur-Yvette near Paris.

([8]) Without the work of Jacobi it is difficult to imagine the birth of Lie's groups, together with his geometrical research, as is well explained by T. Hawkins in his landmark study that represents the culmination of his contributions to the field [Hawkins 2000, 73]. Jacobi's canonical transformation theory, based on the strategy of applying transformations of the variables that leave the Hamiltonian equations invariant, although introduced for the "merely instrumenthat most ordinary differential equations whose solution was known at the time were invariant under some class of continuous transformations groups, and concentrated on the way in which certain transformations of the variables can make a partial differential equation easier to solve. He worked on the problem of how to use the knowledge that a differential equation or a set of them is invariant under an infinitesimal transformation group, for the integration of those equations. His research on differential equations and their symmetry group led him to investigate the structure of infinite-dimensional continuous groups, which would become fundamental in gauge theories developed by physicists in the second half of the XX century. At Klein's encouragement, the young Friedrich Engel went to work with Lie in Christiania starting in 1884. When Klein left the chair at Leipzig in 1886 and moved to Göttingen, Lie was appointed to succeed him. The collaboration between Engel and Lie continued for nine years culminating with their joint major publication *Theorie der Transformationsgruppen* in three volumes between 1888 and 1893. Lie was well aware of the relevance of the group-theoretical point to physics. In the preface of the third and last volume he wrote: "Die Prinzipien der Mechanik haben einen gruppentheoretischen Ursprung, die Integrationstheorien der Mechanik liefern eines der schönsten Beispiele zu meinen Theorien... Selbst für die Optik und überhaupt die mathematische Physik erscheinen meine Ideen fruchtbar." (The principles of mechanics have a group-theoretical origin, the integration theories of mechanics supply one of the most beautiful examples to my theory... Even if applied to opticks and particularly to mathematical physics my ideas appear to be fruitful.)

The idea of continuous groups led to a wealth of applications to geometry and differential equations culminating in the classification by Wilhelm Killing of the simple finite-dimensional Lie groups. From 1888 to 1890 Killing published four major papers on the structure of finite-dimensional simple Lie algebras over the complex numbers. The extraordinary and unprecedented nature of Killing's work attracted the attention of Élie Cartan who devoted the early stages of his career to perfecting and extending Killing's methods and results. The fundamental propositions concerning structure and representations for Lie's infinitesimal groups were demonstrated by Cartan in his thesis work of 1894, considered one of the most important mathematical papers ever produced. By 1900 the culmination of all this work was a profound classification theorem for the "groups", and the original connection with the theory of partial differential equations was almost completely obliterated.

In France Lie's theory was highly praised and developed in large scale: "Voilà Paris devenir un centre de groupes; tout cela fermente dans ces jeunes cerveaux, et on aura un excellent vin quand les liqueurs seront un peu reposées" (Paris is becoming a center for groups, it is all fermenting in young minds, and one will have an excellent wine after the liquors have settled a bit), as Picard wrote to Lie in May 1893 [Hawkins 2000, 188]. Among Lie's students many came from France, encouraged by Henri Poincaré, Charles-Émile Picard and Jean-Gaston Darboux, who highly appreciated group-theoretic ideas.

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tal" purpose of solving dynamical problems, led also to the general study of physical theories in terms of their transformation properties, and notably to the studies of invariants under canonical transformations. Examples of this are the studies of invariants under canonical transformations, such as Poisson brackets or Poincaré's integral invariants; the theory of continuous canonical transformations due to S. Lie; and, finally, the connection between the study of physical invariants and the algebraic and geometric theory of invariants that flourished in the second half of the nineteenth century, and which laid the foundation for the geometrical approach to dynamical problems [Lanczos 1949].

However, it was its "utilitarian" side which was particularly appreciated, in view of its diverse applications to geometry and mathematical analysis. Even Picard and Poincaré expressed their concern that a proper balance should be maintained "between the 'artistic' tendency within mathematics, *i.e.* to develop mathematics for its own sake, Galois, and the 'utilitarian' tendency, *i.e.* to develop a mathematical theory in relation to its applications to science and engineering, or simply to other branches of mathematics...For Picard, Fourier epitomized the utilitarian tendency in mathematics, whereas Galois's highly abstract ideas (to use Picard's characterization) epitomized the artistic tendencey" [Hawkins 2000, 190].

The spirit of Klein's *Erlanger Programm* was clearly expressed in the chapter "L'Espace et la Géometrie" of Poincaré's *La Science et l'Hypothèse* published in 1902: "Ce qui est l'objet de la géometrie, c'est l'étude d'un 'groupe' particulier; mais le concept général de groupe préexiste dans notre esprit au moins en puissance. Il s'impose à nous, non comme forme de notre sensibilité, mais comme forme de notre entendement. Seulement, parmi tous les groupes possibles, il faut choisir celui qui sera pour ainsi dire l'*étalon* auquel nous rapporterons les phénomènes naturels". The radical view that "Geometry is nothing else than the study of a group" had already been expressed by Poincaré in his essay *Sur les hypothèses de la géometrie* of 1887, where he claimed that Lie's groups are the generators of geometries([9]).

In Berlin Lie and his theory were criticized and even denigrated. Weierstrass felt that it was so unrigorously developed that it would have to be redone from the ground up and Frobenius at that time dismissed it as a useless "doctrine of methods" (*Methodenlehre*) that did nothing more than show how problems (differential equations) solved by Euler and Lagrange in natural ways could be solved much more circuitously by means of Lie's methods.

3. – A further move toward abstraction: Cayley, Grassmann and Hamilton

The extension of the group concept to that of transformation group inaugurated by Lie and Killing was a development which produced in the 1870s and 1880s a tremendous expansion of the range of application of group theory, but the shaping and axiomatization of the abstract group concept required input from other areas of mathematical progress.

In 1846 the English mathematician Arthur Cayley had published a *Mémoire sur les Hyperdéterminants*, which, as stressed by Hermann Weyl, "one may look upon as the birth certificate of invariant theory". He passed from the consideration of determinants, which turned out to be the first and simplest invariants, to more general ones: in his two papers "On the theory of linear transformations" (1845) and in "Linear Transformations" (1846) Cayley was the first to state the problem of algebraic invariance in the most general terms. But actually, terms such as *invariant, covariant, contravariant* were invented by Sylvester, who was the principal responsible for the creation of the new vocabulary of

([9]) [Gray 1999a, 71-72]. Gray points out that "Poincaré raised the question of how his approach differed from Riemann's, which seemed to offer infinitely many geometries. He replied: the hypotheses at the base of any geometry are not experimental facts, analytic judgements, or synthetic *a priori* truths. Not experimental facts, else geometry would be subject to the same process of endless revision that characterized science —and plainly it was not. Not analytic or synthetic *a priori* truths, because one cannot change such foundations, and plainly one can (there is more than one geometry). No, said Poincaré, a geometry was nothing other than the study of a group".

the theory([10]). In the hands of Cayley, Sylvester, and others, the investigation of forms, originally a means to geometric ends, became divorced from geometry.

The aim of the theory of invariants was the construction of a finite, complete "fundamental system" of (independent) invariants and covariants, such that every invariant and covariant of a form, or a system of forms, could be expressed as an entire rational function with numerical coefficients in the invariants and covariants of the fundamental system. In Germany Alfred Clebsch, and his student and collaborator Paul Gordan became adherents and promoters of the new discipline which, in a sense, dominated algebra from the 1860s to the 1880s. Gordan, a true specialist of the theory, became known as the "king of invariants".

The central question in any given problem in invariant theory was always to find a complete basis of invariants and covariants in terms of which all other could be expressed as sums and products. The quest to seek out and catalogue all possible invariant algebraic forms was abruptly swept aside in 1888, when the young David Hilbert, Klein's pupil, used a completely novel method to provide a proof of Gordan's theorems of 1868-1870. Departing from the older invariant theoretic approach Hilbert completely avoided emphasis on calculation and demonstrated the existence of a finite basis for any number of variables but in an entirely abstract way, by an existential argument. Although he proved that a finite basis existed, his methods did not construct such a basis. Typical of the opposition to this abstract viewpoint, was Gordan's proclaim: "Das ist nicht Mathematik, das ist Theologie." (This is not mathematics, this is theology.) [Crilly 1994, 787].

In the meantime Arthur Cayley, whose mathematical range was incredibly wide (he produced about 1000 works in his whole life), had carried on other most remarkable researches, contributing to the shaping of the abstract group concept. In 1854 Cayley made the first advance towards the abstract conception of a finite group in the sense of an arbitrary system of elements determined only by defining relations, and characterized the structure of the group by means of a group table([11]). Cayley's realization of the generalizability of the group concept extended also to particular calculi, such as the multiplication of matrices and the theory of quaternions, which at the time were considered of great interest in England, due to the publication in 1853 of William Rowan Hamilton's *Lectures on Quaternions.*

In 1858 Cayley, who was known to be "thoroughly conversant with everything that had been done in every branch of mathematics", published "A Memoir on the theory of matrices" which is remarkable for containing the first abstract definition of a matrix, even if the first to use the term "matrix" to denote a regular array of numbers was Sylvester in 1850.

([10]) Thanks to the "invariant trio", as Charles Hermite called Cayley, Sylvester and the Irish mathematician George Salmon, the notion of an n-dimensional vector space became accessible to the average student of mathematics, particularly through Salmon's textbooks of analytic geometry and higher algebra, which were translated into practically all European languages. Even Klein got acquainted with and was greatly influenced by Cayley's ideas through them (*Lessons introductory to the modern higher algebra* (1859) and *A treatise on the analytic geometry of three dimensions* (1862)).

([11]) "A set of symbols 1, α, β, ... all of them different, and such that the product of any two of them (no matter in what order), or the product of any one of them into itself, belongs to the set, is said to be a *group.* It follows that if the entire group is multiplied by any one of the symbols, either as further or nearer factor, the effect is simply to reproduce the group"; Cayley also remarked that "These symbols are not in general convertible [commutative], but are associative" (A. Cayley, On the theory of groups, as depending on the symbolic equation $\theta^n = 1$).

The development of matrix theory had been stimulated by the study of quadratic forms occurring in many branches of mathematics, in particular differential equations, aspects of mechanics such as moments of inertia and number theory([12]). Matrix algebra has two main sources: bilinear forms and linear substitutions. Each of these two mathematical objects is specified by an $n\times n$ array or matrix of coefficients a_{ij}, and in the first half of the nineteenth century some mathematicians, including Gauss, Eisenstein and Cauchy, were aware of the advantage of virtually identifying bilinear forms and linear substitutions with its array of coefficients. In 1844 Eisenstein, regarding a matrix as a single object, conveniently used for it the notation by single letters as $\mathbf{A} = (a_{ij})$, and explained how they can be added and multiplied (composed) much like ordinary numbers, except for the failure of the commutative law([13]).

Cayley's theory of matrices helped the move towards more general abstract systems and suggested to the latter the idea of analytical geometry of n dimensions using determinants as fundamental tools for its development. He was above all a pure mathematician, far from the physical sciences, yet P. G. Tait uttered the incredible prophecy that "*Cayley was forging the weapons for future generations of physicists*"[Knott 1911, 166].

Cayley and Sylvester development of the theory of matrices following the way of abstract algebraic structure was quite different from the geometrical approach of William Rowan Hamilton and Hermann Grassmann. In his two editions of *Die lineale Ausdehnungslehre, ein neuer Zweig der Mathematik* (Theory of Extension, 1844 and 1862), Grassmann introduced an ingenious algebraic tool which serves to study the subspaces of an n-dimensional vector space. He developed the idea of non-commutative products —which were absolutely new for his contemporaries— in the general frame of vector calculus in any number of dimensions, where he applied his newly invented linear algebra to geometry([14]).

([12]) Quadratic forms were studied by Gottfried Leibniz but nascent matrix theory is evident in the work of Joseph Louis Lagrange and Pierre Simon Laplace. In perfecting the analytical methods of their predecessors, men such as D'Alembert, Euler, Lagrange and Laplace, demonstrated their power by systematically and successfully applying them to the problems in terrestrial and celestial mechanics suggested by Newton's *Principia*. Lagrange's and Laplace's attempt to prove stability of the Solar System required knowledge, in modern terms, of the latent roots and vectors of the characteristic equation $|\mathbf{C}-\lambda\mathbf{I}| = 0$, where $\mathbf{C}$ is the matrix of known coefficients of the equations. They worked with the corresponding quadratic form which later strongly interested Augustine Louis Cauchy and who explicitly thought of the coefficients as an array, and gave the matrix the name "tableau" in the context of quadratic forms in n variables. In extending this work Lagrange and the young Siméon-Denis Poisson came across the important "bracket" solutions to Newton's equations which still bear their name. Joseph Fourier's first model of heat diffusion, which led to his series, marked the origin of infinite matrix theory, a topic which did not really develop until the late nineteenth century, closely driven by functional analysis and integral equations. In this context the name "spectral theory" has its origins. For a short but thorough history of matrix theory see [Grattan-Guinness and Ledermann 1994].

([13]) [Hawkins 1977, 85]. Eisenstein seems to have been the first person to draw attention to the non-commutative nature of matrix multiplication in 1844, while Cauchy's work of 1845 contains explicit reference to the non-commutative behaviour of permutations. In 1837 the English Murphy began the study of non-commutative operators in the context of differential equations [Gray 1980, 383].

([14]) The first was in 1844 but it was a very difficult work to read, and clearly did not find favour with mathematicians, so Grassmann tried to produce a more readable version which appeared in 1862 [Crowe 1967].

The work of Grassmann and Cayley, one of the sources of n-dimensional geometry, was in a very abstract form, pure intellectual daring. The necessity of dealing mathematically with directed physical quantities gave rise to Hamilton's vector and quaternion calculus. With the discovery of vectorial quantities in physics, different from the classical use for forces, notably in electromagnetism, physicists began to see the need for their mathematical representations and an algebra that distinguished their behavior from that of scalars. The stage was gradually set for the introduction of a simplifying notation, but Grassmann's treatise was too difficult so the treatment that caught on was Hamilton's, in which spatial vectors appeared as the imaginary part of a quaternion. Hamilton called "vectors" the objects which Grassmann named "extensive magnitudes". He presented complex numbers as a two-dimensional vector space over the reals, and the ensuing search for quaternions and hypercomplex numbers was naturally full of such objects. In 1843 Hamilton presented *On a new Species of Imaginary Quantities connected with a theory of Quaternions*; quaternions, were an important example of a 4-dimensional vector space, which directly sprang from Hamilton's idea of abandoning commutative law for multiplication in the frame of his attempts to extend ordinary complex numbers from the plane to space. Only after many failures did Hamilton become convinced that one could not retain for quaternions the commutativity of multiplication, an item which was clearly recognized by Cayley also in connection with matrices.

Maxwell had pointed out the necessity of distinguishing between the behavior of vector quantities from that of scalars, but his main mathematical arguments were still in Cartesian form, and vectors appear marginally throughout his *Treatise on electricity and magnetism* (1880, 1891)([15]). For over a decade vector algebra was not used in electrodynamics, where physicists continued to think in Cartesian, or quaternionic terms([16]). For some time the vector calculus existed only in the form of "quaternion calculus", and in fact the fundamental Maxwell equations of electromagnetic field theory were first written using quaternions, which in the following editions of Maxwell's *Treatise* were substituted with vector calculus created at the beginning of 1870s by Heaviside, Gibbs and Christoffel.

Vector calculus —both vector algebra and vector analysis— acquired its modern form in the works of the American physicist Josiah Willard Gibbs *Elements of Vector Analysis* and in *Electromagnetic Theory* of the English engineer Oliver Heaviside. In much the same way that Grassmann had been motivated by the theory of tides and a desire to improve on the tools provided in Lagrange's *Mécanique analytique*, so Gibbs and Heaviside were influenced by a desire to provide a mathematical treatment of James Clerk Maxwell's field theory. Heaviside was fascinated by Maxwell treatise, and after a systematic study of vector algebra he transformed Maxwell's electromagnetism making physical processes visible through the notions of electric and magnetic force, rather than centering it on the analytical potential functions. In the hands of Gibbs and Heaviside Maxwell's theory was transformed into the vector notation familiar today. Today we call them "Maxwell's equations" forgetting that they are in fact "Heaviside's equations"([17]). Vector analysis

([15]) Both Maxwell and Tait, who became close friends, studied quaternion theory in Cambridge. Later Tait wrote *Elementary Treatise on Quaternions* (1873).

([16]) In his study of quaternions Hamilton laid also the foundations of vector analysis: he considered the "symbolic vector" $\nabla = i\frac{\partial}{\partial x} + j\frac{\partial}{\partial y} + k\frac{\partial}{\partial z}$ which he called "nabla" after the Biblical instrument "nebela", a kind of triangularly shaped harp [Yaglom 1988, 201].

([17]) Gibbs separated the vector part from the full quaternion, and bringing together the thinking of mathematicians and physicists, showed how successfully and elegantly vectors could be used was also introduced by the mathematician Heinrich Weber in his 1900/1901 revision of Riemann's lectures in connection with the concept of physical "field": a directed magnitude was associated with each point in space, and he showed how vector analysis could enable the physicist to study the spatial distribution of a vector and its changes over time.

Both quaternions and Grassmannian algebra influenced the English mathematician William Clifford's work on biquaternions in 1873. He obtained an important enlargement of Grassmann "extensive expressions" when in 1878 discovered two classes of algebras (later named after him) that contain the complex numbers and the quaternions as special cases. In Clifford algebra the square of each unit $[e_i,\ e_i] = -1$, as in the algebra of complex numbers, and not zero as in Grassmann's case. In generalized Clifford numbers, also called *alternions* and discovered by Paul Dirac in the course of his work on quantum mechanics, the squares of the principal units may equal ± 1. Use of quaternions in modern physics was later revived by Felix Klein, who showed in 1910, in a lecture before the Göttingen Mathematical Society, that Lorentz transformations, conceived as four-dimensional rotations in Minkowski space, can be conveniently expressed in terms of quaternions [Jammer 1966, 205]. A more systematic use of quaternions in quantum mechanics began when it was recognized that the Pauli spin matrices were essentially quaternion basis elements.

4. – The evolution of the Abstract Group Concept and the Representation Theory

Klein's and Lie's explicit use of groups in geometry influenced conceptually the evolution of group theory. The study of transformation groups, in extending the scope of the concept of a group, shifted the development of group theory from a preoccupation with permutation groups and Abelian groups to the study of groups of transformations. It also provided important examples of infinite groups, and greatly extended the range of applications of the group concept to include number theory, the theory of algebraic equations, geometry, the theory of differential equations (both ordinary and partial), and function theory (automorphic functions, complex functions). These developments were

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in physics. The main elements of Heaviside's operational calculus were developed between 1880 and 1887. Heaviside had learned the elements of the technique from the 1872 edition of George Boole's *Treatise on Differential Equations*, where the algoritm of linear differential operators was introduced to generate invariants and covariants, and in which symbolic methods were regarded as representing the "highest abstraction". Heaviside introduced his operational calculus to enable him to solve the ordinary differential equations which came out of the theory of electrical circuits. To analyze networks of linear conductors with inductances, capacities, and resistances, Heaviside replaced the differential operator $\mathrm{d}/\mathrm{d}x$ by a variable p transforming a differential equation into an algebraic equation. The solution of the algebraic equation could be transformed back using conversion tables to give the solution of the original differential equation. He also defined what he wrote as *div* **R** and *curl* **R** for the modern divergence and curl of a vector function **R**. Applying this method also when partial differential equations were involved he paid little attention to questions of mathematical rigor. "Mathematics, he declared, is an experimental science, and definitions do not come first, but later on" [Heaviside 1893]. His operational calculus, developed between 1880 and 1887, caused much controversy however. Tait championed quaternions against the vector methods of Heaviside and Gibbs and sent frequent letters to *Nature* attacking Heaviside's methods, which where highly successful, but where proved correct only successively. Edmund Whittaker rated Heaviside's operational calculus as one of the three most fruitful discoveries of the late nineteenth century.

also instrumental in the emergence of the concept of an abstract group [Kleiner 1986, 207]. Cayley's first abstract definition of group of 1854, did not attract any attention at the time, even though Cayley was well known. The mathematical community was apparently not ready for abstractions, and even less for formal approaches. When Cayley, whose concern with the abstract foundations of mathematics had been partly influenced by the abstract work of G. Boole, returned to this point of view in 1878; his four papers inspired a number of fundamental group-theoretic works.

The old theory of finite groups was greatly advanced after 1870 through the systematic and extensive work of Camille Jordan, Otto Hölder, William Burnside, and by 1880 the equivalence of the abstract content of several basic modes of reasoning common to the already developed "permutation group theory" and to number theory was recognized. It was W. Von Dyck who, in an influential paper in 1882 entitled "Group-theoretic studies" included and combined, for the first time, all of the major historical roots of abstract group theory —the algebraic, number theoretic, geometric, and analytic: "The aim of the following investigations is to continue the study of the properties of a group in its abstract formulation. In particular, this will pose the question of the extent to which these properties have an invariant character present in all the different realizations of the group, and the question of what leads to the exact determination of their essential group-theoretic content" [Kleiner 1986, 210].

These developments gave an essential impulse to the shaping of the abstract group concept, which gained a central role in mathematics. Both the permutation groups and the groups known from number theory were subsumed under one abstract-group concept, though, of course, encompassing only finite groups. This second advance towards the concept of abstract group can be found in the work of Georg Frobenius, L. Stickelberger, Eugen Netto. Several theorems that had been proved earlier for permutation groups were recognized as general theorems holding for finite groups.

By this time four fundamental types of groups were known: discrete (equivalently – up to 1940 "discontinuous" [Chandler and Magnus 1982, 72]) finite groups (as permutations groups), discrete infinite groups (as those appearing in the theory of automorphic functions), continuous finite Lie groups (exemplified by Klein's transformation groups and by the most general analytic Lie transformations) and continuous infinite Lie groups defined by differential equations.

A major step towards the abstract group concept was made in 1882 by Eugen Netto, whose *Substitutionentheorie und ihre Anwendung auf die Algebra* was among the first treatises in group theory together with Jordan's "bible" of 1870. Both books were on the theory of permutation groups a good dose of which was contained also in the second volume of J.-A. Serret's *Cours d'Algèbre Supérieure* (3rd ed., 1866), one of the most popular algebra texts of the day, the first textbook-like presentation of Galois theory([18]).

It is remarkable that Klein was among those who did not applaude the abstract point of view in group theory: "abstract formulation is excellent for the working out of proofs but it does not help one find new ideas and methods", and added that "in general, the disadvantage of the [abstract] method is that it fails to encourage thought" [Wussing 1984, 228].

([18]) The first edition of J.-A. Serret *Cours d'Algèbre supérieure* of 1849, an excellent and innovative textbook of higher algebra, did not include Galois discoveries, which finally appeared in the third edition of 1866, the first textbook-like presentation of Galois theory and, in particular, the earliest algebra-oriented presentation of group theory.

The formal approach became prevalent in other fields of mathematics, and the abstract group concept spread rapidly during the 1880s and 1890s.

These important developments were exposed towards the end of the century in different new treatises as the two volume *Lehrbuch der Algebra* by Heinrich Weber published in 1895/1896, which became a standard text and one of the most influential still in the first decades of the XX century([19]), and the *Theory of groups of finite order* by William Burnside (1897), the text which definitively brought group theory into the limelight. These books played a major role in shaping the views of the next generation of mathematicians; however, group-theoretic monographs based on the abstract group concept did not appear until the beginning of the XX century, such as the book by J. A. de Séguier of 1904 entitled *Elements of the Theory of Abstract Groups.*

One of the most surprising turning points of group theory was the study of algebraic representations of abstract groups. In 1854 Cayley had already remarked that every finite abstract group can be represented by a permutation group. During his research on the theoretical possibilities for the internal structure of crystals —the mathematical analysis of the classification of discrete repetitive pattern— Auguste Bravais had studied the group of linear transformations in three variables. His work influenced Jordan who began to study analytical representation of groups, as he called it. Joseph-Alfred Serret had already introduced in his *Cours d'algèbre supérieure* of 1866 representations of substitutions through transformations of the form $x' = \frac{ax+b}{cx+d}$. Representations of substitutions through linear trasformations of the form $x'_i = \sum_{j=1}^{n} a_{ij}x_j, \quad i = 1, 2, \ldots\ldots n$, introduced by Jordan in 1878, were extended to the study of all finite abstract groups by Ferdinand Georg Frobenius, William Burnside, Theodor Molien and Issai Schur between the end of the XIX and the beginning of the XX century. In the spring of 1896 Dedekind wrote a letter to Frobenius posing the problem of factoring a special kind of determinant associated with a finite group. The solution of this abstract problem led Frobenius to the invention of character theory, and subsequently to the creation of representation theory of finite groups([20]).

The invertible matrices of degree n over a field form a multiplicative group. A powerful method of studying abstract groups consists in making a homomorphic image of them within a matrix group. A representation by linear substitutions of a group is thus a matrix group onto which the group to be represented is homomorphic. The principal goal of representation theory is to determine all inequivalent, ireducible representations of a given abstract group in terms of which any given representation is to be analyzed([21]).

([19]) The first edition of Weber's *Lehrbuch* embodied the spirit of the classical XIX century image of algebra: groups are subordinate to the main classical tasks of algebra.

([20]) A full historical account of the development of the theory of representation of finite groups and associative algebras has been given in a series of articles by T. Hawkins [Hawkins 1971, 1972, 1974]; see also [Curtis 1999].

([21]) Let G: $1, x, y \ldots$ be an abstract group, and suppose that an invertible complex matrix $\mathbf{A}(x)$ of degree n is associated with each group element x in such a way that $\mathbf{A}(xy) = \mathbf{A}(x)\mathbf{A}(y)$, where x and y are arbitrary elements of G. Then we say that $\mathbf{A}(x)$ forms a representation of G of degree n. The trace of the matrix $\mathbf{A}(x)$, that is, the function $\chi(x) = \sum_{i=1}^{n} a_{ii}(x)$, $x \subset$ G, is called the character of the representation. It truly characterizes most of the relevant features of the representation, such as reducibility and faithfulness, and it helps to reveal important properties of the underlying group. See [Grattan-Guinness and Ledermann 1994, 783]. But the

The theory of representation of groups by linear transformation was created above all by Frobenius during the years 1896-1903; Burnside, independently of him, and Schur in continuation of his work, found an essentially simpler approach by emphasizing the representing matrix itself rather than its trace, the Frobenius character. These transformations and their matrices —the so-called linear transformations— proved to be a very powerful tool in the study of groups, the most efficacious representations of abstract groups.

Within a decade, principally through the work of Frobenius, Schur and Burnside, the representation theory had already established itself as the most powerful technique for penetrating the structure of a finite group. Nevertheless the understanding of Frobenius's and Burnside's accounts as gateways to the new subject presented serious obstacles. An important step was taken by Schur in 1905 when he gave a new introduction to the theory based on elementary facts from linear algebra, that opened the subject to a wide audience. Some of his ideas, such as those based on the result known as Schur's Lemma became familiar to mathematicians everywere. The fundamental propositions concerning structure and representations for Lie's infinitesimal groups were demonstrated by Élie Cartan, who later largely employed Lie groups as a tool of unification in Geometry with his theory of "generalized spaces".

George Mackey has clearly expressed the far-reaching meaning of representations without which group theory would only be "the shadow of itself" [Mackey 1978, 461].

At the time physicists were far from being interested in group theory. In 1910 the British physicist James Jeans and the mathematician Oswald Veblen were discussing the reform of the mathematical curriculum at Princeton University. "We may as well cut out group theory", said Jeans. "That is a subject which will never be of any use in physics". Luckily Veblen disregarded Jean's advice, and group theory continued to be taught in Princeton [Dyson 1964].

5. – Symmetry of physical states: Pierre Curie

Symmetry considerations entered into the solutions of physical problems at the very beginning of mathematical physics. Symmetry laws gradually gained in importance in physics, mainly as properties with which one got acquainted in crystallography and electromagnetism, where, actually, invariance was recognized *a posteriori*. The symmetry properties of space-time in classical physics had played an outstanding role in the theory of crystal symmetry, since Nicolaus Steno's investigations about crystal angles at the end of the XVII century. As Hermann Weyl recalled in 1928 "...the most wonderful symmetrical structures are exhibited in crystals, the symmetry of which is described by those congruency transformations of Euclidean space which bring the atomic lattices of the crystal into coincidence with themselves. The most important application of group theory to natural science heretofore has been in this field" [Weyl 1949, 113]. The classification of crystals according to their symmetry properties had its origin in ancient times, and crystallography, a major success of XIX century physics, developed before the abstract language of group theory had been accepted, but became the motive for a detailed study of infinite discrete groups of motions. In the XVIII century Romé de l'Isle gave a phenomenologic classification of crystals, trying to order them into symmetry classes. In 1815 René Just Haüy defined symmetry morphologically as the equality of

defining relation $\chi(xy) = \chi(yx)$ holds as long as $\chi(x)$ and $\chi(y)$ commute, so problems arise for non-Abelian groups. To remedy this situation for finite non-Abelian groups, in 1896 Frobenius introduced the notion of "group representation".

the appearance of a primitive form to the eye, when looked upon from different views, and used it as a regulating principle for equal parameters in the process of building the crystal form (*Law of Rational Indices*)([22]).

The mathematization process started with Haüy, but only in the XIX century crystallography was transformed into an empirical and strongly mathematized science in the modern sense. A strong impetus in this direction was provided by the elaboration of symmetry concepts and their underlying mathematical theories. Given an arbitrary crystal, one can easily figure out all symmetry operations that leave it unaffected: reflection through certain planes or inversion with respect to the center, rotations around given axes through the center (only the angles $2\pi/n$, with $n = 2, 3, 4$, or 6, will be compatible with the periodicity of the crystalline lattice), or any combination of these. From the observation that only certain angles occur in rotational symmetries of crystals, one can be led to the atomic hypothesis, that a solid is not a continuum, but is rather built up from discrete subunits in a regular repetitive pattern.

The systematic investigation of the theoretical possibilities for the internal structure of crystals —the mathematical analysis of the classification of discrete repetitive patterns— had begun in 1816 with the classification of the 7 crystallographic systems (P. E. Weiss); in 1830 J. F. C. Hessel, apparently independently of Frankenheim (1826) had studied the 32 point groups establishing that there exist exactly 32 different combination of symmetry properties, and accordingly crystals can be subdivided into 32 crystal classes. So all this was achieved well before the idea of group had been explicitly formulated by Galois, and independently of the first developments of the theory implemented by mathematicians. Starting from 1849, when Galois' newly printed papers were still barely known, Auguste Bravais arrived in 1850 at a classification of the space lattices into 7 lattice systems and 14 lattice types. In 1851 he developed a mathematized crystal structure theory from which he derived a detailed list of possible combinations of polyhedral and lattice symmetries. Bravais's studies of crystal structures motivated Jordan's large-scale classification of groups of motions in his "Mèmoire sur les groupes de mouvements" of 1868/1869. But it took some time before crystallographers started to apply the group concept to the investigation of crystal structure. One of the first was the German physicist L. Sohncke, but he could not find all the groups relevant for crystallography in Jordan's work. For a given class the set of symmetry operations form a group with a finite number of elements. Each crystal class corresponds to such a group, which is called a point group, since it consists of symmetry operations around a fixed point; thus there are 32 different point groups. Combining these with the lattice translations for each of the 14 different types of Bravais lattices, the complete list of the so-called crystallographic groups (space groups) turned out to be 230, illustrating the peculiar original role of group theory in physics, namely, to organize data in a rational fashion. This pioneering work culminated

([22]) In 1784 Haüy published a work entitled *Essai d'une théorie sur la structure des crystaux appliquée à plusieurs genres de substances cristallisées.* In this work he presented the view that continued cleavage of crystals would ultimately lead to a basic unit, and that the whole crystal is built up of a repetition of this unit. This theory led him to formulate the law of rational indices on the basis of the simple stacking model he proposed. Vice versa, one can argue directly from the law of rational indices to exclude all angles or rotation other than $2\pi/k$ with $k = 1, 2, 3, 4$ and 6. This means that any group preserving a polyhedron which satisfies the law of rational indices can only have the above angles as its angles of rotation. The existence of discrete subunits implies the law of rational indices, and they imply the non-existence of other angles of rotation.

in fact with the classification of all possible crystallographic groups which appeared in the research works of the Russian crystallographer Eugraf Stepanovich Fëdorov (late 1890), and of the German mathematician Arthur Moritz Schönflies in 1891. The latter had been invited by Klein to attack the question, and had used the up-to-date language of group theory [Scholz 1994, 1269].

The possible symmetries of the internal structure of a crystal were thus enumerated some twenty years before there was any possibility of its physical determination by X-rays analysis. When Voigt's *Lehrbuch der Kristallphysik* appeared in 1910 there was no physical phenomenon known that required one to accept the hypothesis of space lattices, based only on crystal symmetry and its relation to crystal properties. The situation changed from 1912, when, according to an idea put forward by Max von Laue, experiments of diffraction of X-rays in crystalline matter proved the wave nature of X-rays and the atomic theory of crystals formulated by Ludwig August Seeber since 1824([23]).

The above researches are examples of the studies on the symmetry of physical states which developed during the XIX century; symmetry arguments played a role also in the work of André Marie Ampère [D'Agostino 1983] and Lazare Carnot [Drago 1990] and reached a climax with Pierre Curie's works in the 1880s. It seems that Curie, inspired by Bravais' and Jordan's work in cristallography, and by Pasteur's ideas, was one of the first physicists to use symmetry arguments for predicting phenomena or their absence([24]).

([23]) Seeber published his ideas in 1824, 32 years prior to the entry of atomistic into modern physics in the form of the kinetic theory of gases, which in turn was violently opposed till the beginning of the XX century. He combined Dalton's newly created concept of the chemical atom with René Just Haüy' s idea of a space lattice composed of tiny parallelepipedal "building stones", a kind of elementary crystal of minimal dimensions which he called a "molécule intégrante". Seeber assumed that crystal lattices are made up of "chemical atoms" and viewed the interatomic distances as being determined by the forces acting between the atoms relating elasticity and thermal expansion to this postulate. His ideas completely fell into oblivion and were rediscovered only in 1879 by Sohncke [von Laue 1950, 119].

([24]) "L'univers est un ensemble dyssimmetrique. Je suis porté a croire que la vie, telle qu'elle se manifeste à nous, doit être fonction de la dissymmetrie de l'univers ou des consequences q'elle entraine" [Curie M. 1923, 62] Here the term dissymmetry means the absence of one of the possible symmetries compatible with the situation. When Pasteur was 26 years old, working for his doctorate in chemistry, crystallography was just emerging as a branch of chemistry. His project was to crystallize a number of different compounds. Happily he started working with tartaric acid. Crystals of this organic acid are present in large amounts in the sediments of fermenting wine, where crystals of a second acid called paratartaric acid or "racemic acid" could also be found. A few years earlier, the chemical compositions of these two acids, tartaric and paratartaric, had been determined. They were identical. But in solution there was a striking difference. Whereas tartaric acid rotated a beam of polarized light passing through it to the right, according to an effect discovered by Arago in 1811, paratartaric acid did not rotate the plane of polarization of plane-polarized light. This puzzled the young Pasteur who precipitated a salt of the optically inactive tartaric acid. Superficially its crystals appeared the same as the salts of the active variety, but under closer examination he discovered that half of them were indeed the same, but the other half of the crystals were their mirror images. Pasteur then performed one of the simplest and yet most elegant experiments in the annals of chemistry. With a dissecting needle and his microscope, he separated the left and right crystal shapes from each other to form two piles of crystals, and dissolved them separately. Each solution was indeed optically active, but in opposite sense. The optically inactive tartaric acid was really a mixture of two active varieties which cancelled each other out. This simple experiment proved that organic molecules with the same chemical composition can exist in space in unique stereospecific forms. With this

In fact pure symmetry considerations on the nature of crystalline matter, were at the root of Pierre and Jacques Curie's discovery of piezoelectricity, the electric polarization produced by the compression or the expansion of crystals, such as quartz, in the direction of the axis of symmetry. With an experimental skill rare at their age, the young men succeeded in making a complete study of the new phenomenon, established the conditions of symmetry necessary to its production in crystals, and stated its remarkably simple quantitative laws, as well as its absolute magnitude for certain crystals([25]).

Thus, the discovery of this hitherto unknown phenomena in 1880 was by no means a chance discovery, and it was at the origin of Pierre Curie's extension of the group-theoretical approach from crystal to physical systems in general: "Je pense qu' il y aurait intérêt à introduire dans l'étude des phénomènes physiques les considérations sur la symétrie familières aux cristallographes" (I think it would be interesting to introduce into the study of physical phenomena, the ideas of symmetry familiar to crystallographers.) Deeply involved in considerations of theoretical nature on the relations between crystallography and physics, in 1884 Curie published a memoir on questions of the order and repetition that are at the base of the study of the symmetry of crystals. This was followed in the same year by a more general treatment of the same subject. Another article on symmetry and its repetitions appeared in 1885. In that year he published, too, a very important theoretical work on the formation of crystals, and the capillary constants of the different faces. Both his theoretical and his experimental research in this domain grouped itself around a very general principle, the principle of symmetry, that he had arrived at step by step, and which he only definitely enunciated in memoirs published between the years 1893 and 1895([26]). In his work *Sur la symétrie dans les phénomènes physiques—Symetrie d'un champ électrique et d'un champ magnétique* (1894), Curie classifies all space transformations which leave physical phenomena [Curie 1894] invariant. Guided by an exhaustive study of the groups of symmetry which might exist in nature, Pierre Curie generalized the procedure he had used for the discovery of piezoelectricity. He showed how one should use this revelation in character at once geometric and physical, in order to foresee whether particular phenomena are allowed to occur, or whether their

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work did Pasteur launch the new science of stereochemistry. To Pasteur this discovery had a deeper meaning. He proposed that asymmetrical molecules were indicative of living processes, and he used this symmetry law as an heuristic method for his later well known researches. In the broadest sense, he was correct. We know today that all of the proteins of higher animals are made up of only those amino acids that exist in the left-hand form. The mirror image right-hand amino acids are not used by human or animal cells. Likewise, our cells burn only the right-handed form of sugars, not the left-handed form that can be made in the test tube.

([25]) The second part of the work, and much more difficult to realize experimentally, concerned the compression resulting in piezo-electric crystals when they are exposed to the action of an electric field. During the course of their experiments on piezo-electricity the Curies were obliged to employ electrometric apparatus, and, not being able to use the quadrant electrometer known at that time, they developed a new form of that instrument, better adapted to their necessities. This became known in France as the Curie electrometer, and later was fundamental for Pierre and Marie Curie's researches in radioactivity.

([26]) The following is the form, already classic, in which he made his announcement: "When certain causes produce certain effects, the elements of symmetry in the causes ought to reappear in the effects produced"; "When certain effects reveal a certain dissymmetry, this dissymmetry should be apparent in the causes which have given birth to them", "The converse of these two statements does not hold, at least practically; that is to say, the effects produced can be more symmetrical than their causes".

reproduction is impossible under the given conditions. Once established the symmetry of electric and magnetic fields by means of the so-called *Curie Groups*, he showed how Hall and Wiedeman effects can be foreseen observing that the combination of an electric and a magnetic field is a chiral object, which is at the origin of the observed phenomena([27]).

Curie, like Louis Pasteur an heir of the great crystallographic French school, was the first to understand that the symmetry of space itself is modified through the action of an electromagnetic field and he well summarized a premonitory idea deriving from the consideration that symmetry means that something happened which is not observable, when he stated: "C'est la dissymétrie qui crée le phénomène", a concept announcing the utmost importance which would be attached to symmetry breaking in modern physics([28]). When Curie got deeply involved in the well-known researches on radioactivity phenomena, he abandoned his pioneering studies, which where soon going to be superseded by outstanding novelties intervening in the world of theoretical and experimental physics([29]).

Curie was analyzing the symmetry of physical states, whereas Sophus Lie was already talking about the symmetry of laws, whose study would dominate the physics of the XX century. In that same 1895 Lie wrote in a centenary volume of the École Normale supérieure under the title "Influence de Galois dans le développement des mathématiques": "Ayant vu combien les idées de Galois se sont peu à peu montrée fécondes dans tant de branches de l'analyse, de la géometrie et même de la mécanique, il est bien permis d'espérer que leur puissance se manifestera également en physique mathématique. Que nous représentent en effet les phénomènes naturels, si ce n'est une succession de transformations infinitésimales, dont les lois de l'univers sont les invariants?" ("Galois ideas have little by little enriched all branches of analysis, geometry and of mechanics itself, in the same way we can perhaps hope that their power will prove in mathematical physics. What do actually natural phenomena represent other than a succession of infinitesimal transformations, whose invariants are the laws of universe?) [Michel 1991, 63].

6. – Einstein *vs.* mathematicians: Minkowski and the special theory of relativity

In 1895 Felix Klein was successful in his aim of appointing Hilbert to the chair of mathematics at the University of Göttingen, where the latter continued to teach for the

([27]) The electric field has the symmetry of a truncated cone while the magnetic field has the symmetry of a rotating cylinder. Curie remarks that by representing electric and magnetic fields by the same vector symbol the different symmetry character is lost: today we just say that the magnetic field must be represented by an axial vector. Curie further underlines that symmetry reasoning only prove that one of the fields is polar while the other is axial, but we cannot say who is who without recoursing to phenomena such as piezo-electricity, Hall effect, etc. which reveal the vector character of electricity.

([28]) "In every phenomenon the elements of symmetry compatible with its existence may be determined. Certain elements can coexist with certain phenomena, but they are not necessary to them. What is necessary, is that certain ones among these elements shall not exist. It is dissymmetry that creates the phenomenon" [Curie 1908, 127].

([29]) It has been remarked how Curie's more phenomenological approach led him to emphasize the role of "non-symmetry", rather than that of symmetry, so that he was the first to appreciate the role of symmetry breaking as a necessary condition for the existence of phenomena. The same attitude probably prevented him from appreciating a more general group-theoretical reasoning [Radicati 1983].

rest of his career. The University of Göttingen had a flourishing tradition in mathematics, primarily as the result of the contributions of Carl Friedrich Gauss, Peter Gustav Lejeune Dirichlet, and Bernhard Riemann in the XIX century. During the first three decades of the XX century this mathematical tradition achieved even greater eminence, largely because of Hilbert. The Mathematical Institute at Göttingen drew students and visitors from all over the world, but Hilbert's intense interest in mathematical physics also contributed to the university's reputation in physics(30).

In 1902 Hilbert succeeded to have a new mathematics chair created at the University of Göttingen for his inseparable friend Minkowski; they were known as the Castor and Pollux of the mathemathical world. According to Hilbert, Minkowski had been attracted by physics when he was in Bonn, through Heinrich Hertz, who was then director of the Physics Institute. In December 1890 Minkowski wrote to Hilbert:

> "I think that this time you would have found me thoroughly infected with physics. Perhaps I even would have had to pass through a ten day quarantine period before you and Hurwitz would have admitted me again, mathematically pure and unapplied, to your joint walks... In order to have points in common with other mortals, I have surrendered myself to magic —that is to say, physics... At home I study Thomson, Helmholtz, and their consorts. I will even work several days a week in a blue smock in an institute for the production of physical instruments. Thus I am a practical man of the most shameful sort [Reid 1970, 35]".

In the introduction to a series of lectures on elasticity Minkowski remarked that analogies between many different mechanical problems was a "triumph of mathematics". Such mechanical analogies were "of the greatest interest" because they led to a deeper understanding of forces in motion on the nature of which "we are still doubtful" [Pyenson 1979, 65]. Hilbert himself gradually got "infected", and fell under the spell of physics. In his famous 1900 Paris lecture he proposed *The mathematical treatment of the axioms of physics*, the sixth among the twenty-three *Mathematical problems*(31). His recent work on the axiomatic foundation of geometry suggested him doing the same for those physical sciences in which mathematics plays an important part. Hilbert noted that physicists such as Mach, Hertz and Boltzmann had already devoted treatises to the foundations of mechanics, he felt that mathematicians, too, should take up the discussion of the foundations of mechanics, and he was very proud of the application to the kinetic theory of his theory of integral equations. Since 1898 Hilbert presented courses of lectures and seminars on various topics in mechanics, but now, instead of running seminars jointly with Klein, he ran them with his friend Minkowski.

In 1904 the young Max Born arrived in Göttingen from Breslau:

> "My scientific life was inspiring and fascinating right from the beginning... I concentrated on mathematics and physics, represented by Hilbert and Voigt... Hilbert's regular courses were always leading into new country. One of these was on advanced mechanics, built on the Hamilton-Jacobian methods and the idea of canonical transformations. What I learned there was later of the greatest help

(30) Starting from 1910 Hilbert had taught mechanics, statistical mechanics, kinetic theory of gases, and from 1912 he enrolled an assistant for physics who should keep him abreast of current developments in physics [Corry 1999].

(31) An English translation can be found in [Hilbert 1902].

in the development of the mechanics of the atom, in the period 1920-25 which preceded the birth of quantum mechanics" [Born 1978, 81 ff]([32]).

The unity of physical laws exerted a strong attraction on Minkowski and Hilbert, as a natural extension of the idea of the unification of apparently distant mathematical domains, which played a leading role throughout Hilbert's career. They also shared a strong belief in the appearance of a pre-established harmony between mathematics and the physical world, and in 1905 they both became interested in the work on electrodynamics of Hendrik Antoon Lorentz and Henri Poincaré. The result was that they held an advanced interdisciplinary seminar to solve the outstanding problems of electrodynamical theory, in particular on electron theory, and on the difficulties resulting from Michelson's celebrated experiment, according to which the laws of physics were unaffected by the Earth's motion relative to the Sun. According to Born and Weyl, who were among the students, together with Max von Laue, then a post-doctoral auditor, that seminar was the starting point for Minkowski's celebrated work on electrodynamics of moving bodies.

The writings of Poincaré and Lorentz, both acknowledged as world authorities on the mathematical treatment of electromagnetism, were especially discussed during the seminar [Corry 1997a and 1997b, Pyenson 1979]. In developing his electron theory of matter Lorentz had introduced in 1904 the transformations, as a purely mathematical variable change of no physical significance, which Poincaré named "Lorentz transformations"([33]). Poincaré observed that they form a group and found that these transformations can be characterized by those linear substitutions of the four variables x, y, z, t which leave the quadratic form $x^2+y^2+z^2-c^2t^2$ invariant. Poincaré was the first to note the invariance in form of Maxwell equations respect to the 10-parameter group of extended Lorentz transformations (the 6-parameter rotations of the Lorentz group together with the 4-parameter group of space-time translations). He also observed that the Lorentz group could be considered as the group of rotations in a four-dimensional space [Poincaré 1905, 1906].

In that same year, 1905, Einstein's "Zur Elektrodynamik bewegter Körper", on the electrodynamics of moving bodies, appeared: "Daß die Elektrodynamik Maxwells — wie dieselbe gegenwärtig aufgefaßt zu werden pflegt— in ihrer Anwendung auf bewegte Körper zu Asymmetrien führt, welche den Phänomenen nicht anzuhaften scheinen, ist bekannt." (It is known that Maxwell's electrodynamics —as usually understood at the present time— when applied to moving bodies, leads to asymmetries which do not appear to be inherent in the phenomena.) [Einstein 1905]. Einstein showed little interest in the electron theory problems and in the associated mathematical investigations that occupied Sommerfeld, Voigt, Abraham and many other physicists. He did not care about models of the electron and problems concerning the nature of aether and matter; he simply rejected the problem of ether as superfluous, and started from two simple postulates: the

([32]) Born, who descended from an academic family, entered the University of Breslau in 1901, where he attended lectures in physics, chemistry, zoology, philosophy, logic, mathematics and astronomy. He was very attracted by astronomy, but discouraged by the endless numerical calculations he concentrated on mathematics courses, where he made friends with Ernst Hellinger and Otto Toeplitz, from whom he "learned that the mecca of German mathematics was Göttingen and three prophets lived there: Felix Klein, David Hilbert and Hermann Minkowski" [Born 1968, 18].

([33]) In 1887 Woldemar Voigt had published his proof that a certain transformation in x, y, z and t (which was formally equivalent to the one used by Lorentz) did not alter the fundamental differential equation for a light wave propagating in the free ether with velocity c.

first, which is an extension of Galilei's principle of relativity, boldly declared that "the laws by which the states of physical systems undergo change are not affected, whether these changes of state be referred to the one or the other of two systems of coordinates in uniform translatory motion", and the second stated that the velocity of light in empty space is a constant independent of the velocity of the emitting body. As a consequence of these two principles he deduced the same transformation laws for the electromagnetic field —and the associated group property ("daß solche Paralleltransformationen —wie die sein muß— eine Gruppe bilden")— which Lorentz and Poincaré had posited *ad hoc* in order to obtain the invariance of the laws of electrodynamics in all uniformly moving frames of reference([34]).

Lorentz was reluctant to embrace Einstein's relativity, and thought that Einstein simply postulated what they deduced from the equations of the electromagnetic field. But Einstein's reasoning extended far beyond Lorentz's electrodynamics, as he later recalled: "The new feature was the realization that the bearing of the Lorentz transformation transcended its connection with Maxwell's equations and was concerned with the nature of space and time in general"([35]).

The key notion of invariant was at the heart of Minkowski's presentation of special relativity, where he introduced tensors for the first time. The simplest of these invariants, which remains the same no matter what the frame of reference in which they are measured, is the four-dimensional metric of space-time. A particle in uniform translational motion describes an invariant four-dimensional "*world-line*", an element of which at the "*world-point*" (x, y, z, t) in any reference frame is $\mathrm{d}\tau = \frac{1}{c}(c^2\mathrm{d}t^2 - \mathrm{d}x^2 - \mathrm{d}y^2 - \mathrm{d}z^2)^{1/2}$, τ being the "proper" time measured by a clock stationary at the particle itself. Whatever be the frame of reference, $\mathrm{d}\tau$ remains the same. Minkowski first publicly set forth his views in a talk on *The Principle of Relativity* presented to the Göttingen Mathematical Society on November 5, 1907([36]). He spoke of the "complete transformation of our representations of space and time which must be of exceptional interest to the mathematician... practically the greatest triumph applied mathematics has ever shown".

His original contribution concerned the requirement that all physical laws be invariants of the Lorentz group in four-dimensional space-time. According to Minkowski reasoning all 4-dimensional velocity vectors always lie on the surface $w_4^2 - w_1^2 - w_2^2 - w_3^2 = 1$ which he called a four-dimensional hyperboloid. At this point he explained: "The non-Euclidean geometry of which I have already vaguely spoken is developed now for these velocity vectors. The transformations about which I have already spoken become the

([34]) "One more consequence of the paper on electrodynamics has also occurred to me. The principle of relativity, in conjunction with Maxwell's equations, requires that mass be a direct measure of the energy contained in a body; light carries mass with it. A noticeable decrease of mass should occur in the case of radium [as it emits radiation]. The argument is amusing and seductive, but for all I know the Lord might be laughing over it and leading me around by the nose", as Einstein wrote to his close friend Conrad Habicht during the Summer 1905, while he was preparing the mass-energy paper. Indeed a notable implication of Einstein's analysis was that his theory challenged the classical view that distinguished sharply between mass and energy. Instead, Einstein showed the equivalence of mass and energy according to the celebrated equation $E = mc^2$. For Einstein's letters until 1919, see [Einstein 1987-1998].

([35]) For studies about how Einstein's thought differed from that of his contemporaries, see the bibliography cited in [Pyenson 1979, 55].

([36]) Six years after Minkowski's death, Sommerfeld transcribed his address for publication [Minkowski 1915].

real transformations of this four-dimensional hyperboloid...". As Thomas Hawkins has noted, "It is tempting to add to Minkowski's words the remark that in other words, the geometry of these vectors is the geometry in the sense of Klein's *Erlanger Programm*, defined on the manifold [the surface of the hyperboloid] by the group of Lorentz transformations"[Hawkins 2000, 340](37).

In fact Klein's idea was that special relativity could be thought of as the invariant theory of the group of Lorentz transformations(38). In the second volume of his well-known *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, Klein quoted the following sentence from his *Erlanger Programm* of 1872: "One should develop the invariant theory related to a group", and continued: "If we write instead 'the theory of the relations which are invariant *relative to the group*' then we are only a step away from the words *relativity theory* used by modern physicsts for the cases of the general goal [of invariant theory] that belong in their domain" [Klein 1927, 38]. Some historians of mathematics have claimed that, in believing that his work of 1872 had set the stage for the special theory of relativity, Klein failed to appreciate the revolutionary physical content of the theory and regarded it as a mere physical interpretation of essentially mathematical theories "that Riemann, Cayley, Sylvester, Klein himself, and Minkowski had developed much earlier" [Wussing 1984, 193]. However, "Relativity theory, according to Klein, should not be thought of in terms of the Lorentz group of special relativity and the group of continuous point transformations of general relativity —but rather should be broadly understood as the invariant theory *relative to some given group* that happens to be relevant to a particular physical theory". In fact Emmy Noether's work directly stemmed from Klein's general program [Rowe 1999, 215].

In Sommerfeld's view, Einstein's theory represented an intermediate step between Lorentz and Minkowski, who had rendered the work of both Lorentz and Einstein "irrelevant": "The troublesome calculations through with Lorentz (1895 and 1904) and Einstein (1905) prove their validity independent of the coordinate system and [for which they] had to establish the meaning of the transformed field vectors, become irrelevant in the system of the Minkowski 'world'" [Sommerfeld 1910]. As for Minkowski, he interpreted Einstein's principle of relativity as a prelude to his own interpretation of space and time, and thought that his own mathematization of special relativity clarified the essential physical features of Einstein's theory. Einstein, in turn, being skeptical about the physical consequences of Minkowski's approach, believed that the latter had introduced a useful mathematical formalism, but considered it only a technical improvement on his own work. Only in 1916, Einstein acknowledged his indebtedness to Minkowski for having greatly facilitated the transition from special to general relativity, and recognized that "He was

(37) In his autobiography Born told the following story he had heard from Minkowski himself: "He told me later that it came to him as a great shock when Einstein published his paper in which the equations of the different local times of observers moving relative to each other were pronounced, for he had reached the same conclusions independently but did not publish them because he wished first to work out the mathematical structure in all its splendour. He never made a priority claim and always gave Einstein his full share in the great discovery" [Born 1978, 231].

(38) In his efforts to highlight the connections between his *Erlanger Programm* and the work of Minkowski, Einstein, Hilbert and others, Klein exclusively lectured on relativity theory from the summer of 1916 through the summer of 1917. It was also characteristic of Klein's that he always opposed the tendency to disciplinary purism and thought that mathematics in Göttingen should represent in microcosm the full range of the discipline.

the first mathematician to clearly perceive the formal equivalence of the space and time coordinates; this made possible the construction of the [general] theory." [Einstein 1916].

Physicists on the other hand, at first did not completely realize that Einstein's radical approach, concerned with the principles of measuring physical quantities, was a new syntesis of classical mechanics and Maxwellian electrodynamics, and not merely a contribution to the "electrodynamics of moving bodies".

Minkowski believed that he himself was formulating a new theory of matter that revealed the true mathematical harmony of the physical world, *his* principle of relativity expressing the fact that the laws of mechanics are covariant under the Lorentz group([39]):

> "Es würde zum Ruhme der Mathematiker, zum grenzenlosen Erstaunen der übrigen Menschheit offenbar werden, dass die Mathematiker rein in ihrer Phantasie ein grosses Gebiet geschaffen haben, dem, ohne dass dieses je in der Absicht dieser so idealen Gesellen gelegen hätte, eines Tages die vollendetste reale Existenz zukommen sollte." (It will become apparent, to the glory of the mathematician and to the boundless astonishment of the rest of mankind, that the mathematicians have created purely within their imagination a grand domain that should have arrived at a real and most perfect existence, and this without any such intention on their part.) [Minkowski 1915].

These lines conveyed Minkowski's strong self-confidence —one would say nearly a feeling of omnipotence— and expressed his deep convinction that the mathematician is particularly predisposed to internalize the new intuitions, as they consist in adapting concepts with which he is long since conversant. He had an image of the physicist as someone who, to some extent, needs to invent these concepts from scratch, and must laboriously carve a path through obscurity while the mathematician "is able... with its senses sharpened by an unhampered outlook to far horizons, to grasp forthwith the far-reaching consequences of such a metamorphosis of our concept of nature" [Minkowski 1923, 79].

For the first time Minkowski had presented Maxwell-Lorentz equations in their modern tensor form, inventing new terms such as *spacelike* and *timelike* vectors, *light cone*, *world line*([40]). Minkowski, who had been Einsteins' former teacher in Zürich, also criticized the latter's mathematical presentation of special relativity, which he thought

([39]) Minkowski distinguished three possible different meanings of this principle. He called "theorem of relativity" the plain mathematical fact that the Maxwell equations are invariant under the Lorentz transformations. The "postulate of relativity" expresses the confidence that the domain of validity of the theorem may be extended to cover *all* laws governing ponderable bodies, including laws that are still unknown. Finally the "principle of relativity" expresses the assertion that the expected Lorentz covariance actually holds as a relation between purely observable magnitudes relating to a moving body. Minkowski's declared aim was to deduce the exact expression of the equations for moving bodies from the principle of relativity and believed that his axiomatic analysis of the principle and of the electrodynamical theories of moving bodies was the best approach for unequivocally obtaining the correct equations [Corry 1997b].

([40]) Minkowski used also 4-dimensional vectors with six components, such as electric and magnetic field, the so called *Traktoren*, which he expressed with matrices. From this generalized treatment he could point out that many results of electrodynamics simply fall out of the algebraic characteristics and manipulation of these matrices. The formulation of the fundamental equations of electrodynamics in matrix language in general was not accepted at the time, notably by Sommerfeld.

to be “mathematically inelegant” [Pyenson 1977, 72]. Half-jokingly Einstein remarked: “Since the mathematicians have now invaded the theory of relativity, I do not understand it myself anymore!” [Mehra 2001, 227]. Like Minkowski, Hilbert was deeply convinced that the new relativistic physics was so entangled with mathematics, that it was the mathematician who was perhaps best equipped to develop it, but on his side he acknowledged that, in spite of that, Einstein did the work and not the mathematicians.

On September 21, 1908, at a scientific congress in Cologne, Minkowski delivered his celebrated lecture *Raum und Zeit*, beginning with the following words: “Die Anschauungen über Raum und Zeit, die ich Ihnen entwickeln möchte, sind auf experimentell-physikalischem Boden erwachsen. Darin liegt ihre Stärke. Ihre Tendenz ist eine radikale. Von Stund’ an sollen Raum für sich und Zeit für sich völlig zu Schatten herabsinken und nur noch eine Art Union der beiden soll Selbständigkeit bewahren”. (The views on space and time which I wish to lay before you have sprung from the soil of experimental physics, and therein lies their strength. They are radical. From now on space by itself, and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality.) [Minkowski 1923, 75]. Having started with this premise, Minkowski ended the first session of his address by saying that “Three-dimensional geometry becomes a chapter in four-dimensional physics” [[Minkowski 1923, 80].

Born, who graduated during the summer 1906, went expressely to Cologne to meet Minkowski, who had asked him to collaborate in relativity theory: “After having heard Minkowski speak about his ideas, my mind was made up at once. I would go to Göttingen and help him in his work” [M. Born 1978, 131](41). Minkowski died suddenly only after few weeks, with him Hilbert lost a friend and a partner in the study of physics, but he still went ahead with his program of the axiomatization of physics, the same approach he had recently and successfully adopted for geometry: “Now I believe that the highest distinction for every science is to be assimilated by mathematics and that theoretical physics is now also on the verge of achieving this distinction. This is so above all for relativistic mechanics, or four-dimensional electrodynamics; I have been convinced for some time now that it belongs to mathematics” [Corry 1999a, 175]. He believed that the systematic mathematical foundation of physics theories required a mathematician: “Physics is much too hard for physicists”, he used to say.

(41) Born had become Hilbert’s assistant in 1905, continuing to attend lectures by Klein and Runge. However, he annoyed Klein by only making irregular attendances at his lectures, and what is worse he initially refused to compete for an academic prize of the Philosophical Faculty with a work on elasticity, as Klein had asked him, because, he claimed, he was not interested in elastic problems nor in applied mathematics in general. Born decided to substitute astronomy for geometry as one of his doctoral subjects. He attended Schwarzschild’s astronomy lectures and successfully obtained his doctorate in 1907: “I did not get the highest mark, *summa cum laude*, but the second one, *magna cum laude*; and I thought it was a fair decision since I never claimed to be a real mathematician, like Schmidt or Carathéodory or Toeplitz, but just a fellow with common sense who could learn obvious things like mathematical theorems from lectures or books, without claiming any originality or productivity. If later something like that appeared in my work, it was in a rather different field, in the quasi-magical process of theoretical physics, of abstracting natural laws from experimental evidence” [Born 1978, 105]. After taking his doctorate he visited Cambridge for a while, but made less of Larmor’s lectures than he might because he had difficulty with Larmor’s Irish accent. In England he bought the just appeared J. Willard Gibbs’s scientific papers, which fascinated him and helped to turn him into a physicist. For biographical notes on Born up to the early 1920s, rich of information drawn from personal conversations, see [Mehra 2001a].

Minkowski's work on the invariance of the fundamental equations of electrodynamics with respect to Lorentz tranformations, led him to the discovery of the mathematical structure behind the physical phenomena and to a new concept of space and time, which had to become vital for the generalization of relativity. What he saw as a triumph of mathematics confirmed his belief in the idea of a pre-established harmony between pure mathematics and physics.

In the same time, since 1907, Einstein began to pursue his heuristic goal to formulate a relativistic theory of gravitation attempting to combine mathematical strategies with a search for physical meaning. In the special theory the transformation properties of the electromagnetic field had been derived as a consequence of relativistic invariance, largely dictating the form of Maxwell's equations. In postulating the universality of the global continuous space-time symmetries Einstein's work on special relativity marked "the reversal of a trend: until then, the principles of invariance were derived from the laws of motion...It is now natural for us to derive the laws of nature and to test their validity by means of the laws of invariance, rather than to derive the laws of invariance from what we believe to be the laws of nature" [Wigner 1967, 28]. This profound change of attitude became far more radical in Einstein's search for a general theory. Starting from the familiar equivalence of gravitational and inertial masses, Einstein was led to advance his principle of equivalence —the equivalence between the effects of homogeneous gravitational fields and uniformly accelerated motion. The principle of general covariance, that is the form invariance of the laws of nature under arbitrary smooth coordinate transformations, must now dictate the dynamics of gravity, of space-time itself.

Einstein, who initially regarded the transcriptions of his special theory into tensor form a "überflussige Gelehrsamkeit" (a superfluous learnedness) [Pais 1982, 152], by 1912 had completely surrended to the necessity of handling what most mathematicians considered an esoteric branch of mathematics as the absolute differential calculus — almost a "formal trick"— to tackle the formidable mathematical problems posed by the formulation of the theory [Bottazzini 1999, 253].

7. – The Italian connection: Gregorio Ricci-Curbastro and Tullio Levi-Civita

On 29 October 1912, Einstein was "exclusively occupied with the problem of gravitation theory", as he himself explained in the oft-quoted letter to Sommerfeld: "with the aid of a local mathematician who is a friend of mine [Grossmann] I believe I will now be able to master all difficulties. But one thing is certain that in all my life I have never struggled as hard. A great respect for mathematics has been instilled within me, the subtler aspects of which, in my stupidity, I regarded pure luxury up to now! Against this problem the original theory of relativity is child's play". And yet, still around 1910, when he told his students at the University of Zürich that "with mathematics one can prove anything" and that all that mattered was the content, he repeated often that he knew "very little mathematics", it was a kind of trick of his. In 1911 he even complained in a half-joking way that he "could hardly understand" the first mathematical text on relativity theory published by von Laue that same year([42]). In fact, the mathematical apparatus of his

([42]) "There [at the Polytechnic Institute of Zürig] I had excellent teachers (for example, Hurwitz, Minkowski), so that I really could have gotten a sound mathematical education", Einstein wrote in his *Autobiographical Notes,*"However, I worked most of the time in the physical laboratory, fascinated by the direct contact with experience. The balance of the time I used in the main in first works was minimal, but a trace of his growing feeling in the physical relevance of certain of "the subtler parts" of mathematics can also be found in a letter written to Antoon Lorentz on August 14, 1913, where he remarked that mathematicians had not developed group theory sufficiently for his needs [Jungnickel and McCormmach 1986, Vol. 2, 339].

Many concepts of Riemaniann geometry, like that of curvature, coud be reformulated in tensor form. In his work of 1854 Riemann had discussed physical space as a particular manifold whose properties can only be deduced from experience. The idea of associating space and matter for inquiring into the real nature of space, was further developed around 1870 by William Kingdon Clifford, who translated Riemann's lecture into English. In proposing that the "variation of curvature of space is what really happens in that phenomenon which we term the motion of matter" he was suggesting to reduce physics to geometry; this curvature, he claimed was "passed on from one portion of space to another after the manner of a wave" [Clifford 1876]. This kind of radical questions directly led to Einstein theory of general relativity, whose mathematical machinery had been elaborated by Ricci-Curbastro and Levi-Civita.

The study of differential invariants set up by Riemann, Beltrami, Christoffel and Lipschitz, stemming also from Klein's work on transformation groups which leave the expression of the line element $\mathrm{d}s^2$ invariant, inspired the work of Gregorio Ricci-Curbastro. Both Luigi Bianchi and Ricci-Curbastro had spent some time with Klein in Göttingen after having studied in Pisa under Enrico Betti. The latter had made friends with Riemann during his stay in Göttingen in 1858, at the time of the famous European *tour* during which Betti, along with Francesco Brioschi and Casorati, visited Germany and France, getting in touch with leading European mathematicians.

Ricci-Curbastro undertook research in differential geometry beginning his investigations in 1884 on quadratic differential forms and was the inventor of the absolute differential calculus between 1884 and 1894: "For the sake of brevity I will denote by the name 'absolute differential calculus' the totality of methods which I named in other occasions covariant and contravariant derivatives, because they can be applied to any fundamental form independently of the choice of the independent variables and require instead that the latter are completely general and arbitrary" [Bottazzini 1999, 241]. This "special analytic method" eventually became known by the name "tensor analysis". If the foundation of this theory was established by Christoffel, heavily relying on the ideas of Riemann, Luigi

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order to study at home the works of Kirchhoff, Helmholtz, Hertz, etc. The fact that I neglected mathematics to a certain extent had its cause not merely in my stronger interest in the natural sciences than in mathematics but also in the following strange experience. I saw that mathematics was split up into numerous specialities, each of which could easily absorb the short lifetime granted to us. Consequently I saw myself in the position of Buridan's ass which was unable to decidee upon any specific bundle of hay. This was obviously due to the fact that my intuition was not strong enough in the field of mathematics in order to differentiate clearly the fundamentally important, that which is really basic, from the rest of the more or less dispensable erudition. Beyond this, however, my interest in the knowledge of nature was also unqualifiedly stronger; and it was not clear to me as a student that the approach to a more profound knowledge of the basic principles of physics is tied up with the most intricate mathematical methods. This dawned upon me only gradually after years of independent scientific work. True enough, physics also was divided into separate fields, each of which was capable of devouring a short lifetime of work without having satisfied the hunger for deeper knowledge. The mass of insufficiently connected experimental data was overwhelming here also. In this field, however, I soon learned to scent out that which was able to lead to fundamentals and to turn aside from everything else, from the multitude of things which clutter up the mind and divert it from the essential" [Einstein 1949,15].

Bianchi had built further on this work studying in particular the surfaces. These geometrical entities have an existence independent of any choice of coordinates, so that intrinsic quantities like curvature and geodesic curves should be left unchanged by any coordinate transformation. The description of these kinds of quantities is the goal of tensor analysis, which Gregorio Ricci-Curbastro developed over a period of some 15 years, from 1884 to the end of the century, when he began to work with his student Tullio Levi-Civita. Ricci Curbastro's approach emphasized rather the idea of transformation laws of tensors, than that of differential invariants, from which he had derived these objects([43]).

In the fundamental joint paper, *Méthodes de calcul différentiel absolu et leurs applications* (1901) [Ricci-Curbastro and Levi-Civita 1901,125], which had been requested years earlier by Klein who had urged him to make his methods more widely known, the new algorithm is presented and applications are given by Ricci-Curbastro and Levi-Civita to the classification of the quadratic differential forms and to a number of geometric and physico-mathematical problems([44]). They had concluded their article stating that a certain number of partial differential equations and physical laws could be formulated in tensor form, in order to make them independent of any frame of reference. Ricci-Curbastro's absolute differential calculus, which became the foundation of tensor analysis, was thus employed to express mathematical invariance of physical laws many years before Einstein used it in his general theory of relativity. In the opening lines the authors had quoted Poincaré's statement: "Dans les sciences mathématiques une bonne notation a la même importance philosophique qu'une bonne classification a dans les sciences naturelles". [In the mathematical sciences a good notation has the same philosophical importance as a good classification in the natural sciences]([45]). Still, as Bottazzini remarks, "for most mathematicians of the time the absolute differential calculus appeared to be little more than a

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([43]) Tensors, offsprings of vector calculus, were at first magnitudes related to stress and strain. The word "tensor" was first used by Woldemar Voigt in 1898, in connection with his mathematical treatment of crystals whose elastic, thermal, electric and magnetic properties he classified in magnitudes of three types: scalar, vector and tensor. In his treatment of crystals Voigt grouped the phenomena according to the kind of directed quantities they required for their representations. He thought that the properties of mathematical quantities were the expression of the symmetries of the physical world; his text *Lehrbuch der Kristallphysik,* where he stressed symmetry considerations and transformation properties in the methods of theoretical physics, had also the purpose to show teachers of physics the value of symmetry considerations; in 1910 Voigt sustained that lectures on symmetry should enter every "theoretical-physics course" [Jungnickel and McCormmach 1986, Vol. 2, 272]. Crystal physics at the time attracted very few physicists; in sending a letter with a copy of his text to a friend of his, Voigt said that it would appear to him "as a guest from a rather strange world" [*Ibid*., 273].

([44]) They give applications to geometry including the theory of surfaces and groups of motions; and mechanical applications including dynamics and solutions to Lagrange's equations.

([45]) Dirac, who regarded the matter of notation as an important part of physics, and invented new terms and symbols, wrote in 1939: "In mathematical theories the question of notation, while not of primary importance, is yet worthy of careful consideration, since a good notation can be of great value in helping the development of a theory..." [Dirac 1929]. Jungnickel and McCormmach underline how the sequence of Einstein's publication can be used to trace the increasing compactness of physical mathematics in the XX century. In 1905 Einstein wrote Maxwell's equations à la Hertz, 8 equations altoghether. In 1908 he could write four equations using a three-dimensional vector notation together with the vector operators of "curl" and "divergence", and in 1916 he wrote them as two four-dimensional tensor equations, where the $F_{\mu\nu}$ components corresponded to the electric and magnetic forces, and the J_μ to current and charge [Jungnickel and McCormmach 1986, Vol. 2, 343].

formal trick. There was no need to learn these complicated calculations to obtain results which were available with classical, well-known methods"([46]). According to Darboux, Ricci's work was "quelque peu formelle et insuffissamment géometrique, while Poincaré thougth that it allowed "une géometrisation globale des problèmes dynamiques".

With general relativity Ricci's calculus revealed itself to be not only useful but truly indispensable. From the Ricci–Levi-Civita paper Einstein learned the tools he might need to generalize the theory of relativity([47]). In presenting his first contribution at the Prussian Academy on 4 November 1915, about three years after becoming "lost in tensors", Einstein expressed his indebtedness to the mathematicians —and to "the subtler parts" of mathematics— who had prepared the way for his general theory of relativity, and remarked: "The fascination of this theory would hardly be denied to anyone who has really grasped it. It represents a real triumph of the method of the general differential calculus founded by Gauss, Riemann, Christoffel, Ricci and Levi-Civita" [Einstein 1915, 779]([48]).

8. – Einstein *vs.* mathematicians: Hilbert and the gravitation field equations

From 29 June to 7 July 1915 Einstein was invited by the Göttingen mathematicians to lecture on the current state of his research on gravitation and relativity. He delivered six lectures and his comment about that visit was that he "had the pleasurable experience of convincing the mathematicians there thoroughly", adding that "Berlin is no match

([46]) [Bottazzini 1999, 253]. Karin Reich has remarked that at the turn of the century mathematicians had the same "conservative" attitude towards vector analysis that they had towards Ricci's tensor calculus [Reich 1996, 199]; see also [Reich 1994].

([47]) In the so-called *Entwurf* theory of 1913 Einstein and Grossmann, in presenting the methods of the absolute differential calculus, indicated how these techniques were a natural generalization of the vector analysis methods developed by Minkowski, Sommerfeld and von Laue for special relativity. When the latter wrote his book on relativity of 1921 (*Die Relativitätstheorie, zweiter Band: Die allgemeine Relativitätstheorie und Einsteins Lehre von der Schwerkraft*) he heavily relied on the works of mathematicians, including Hilbert's unpublished lectures on the foundations of physics from 1916-1917 [Rowe 2001, 38].

([48]) Between 1914 and the spring of 1915 Einstein and Levi-Civita had an intense correspondence about the general theory, particularly involving the transformation properties of what in fact is the electromagnetic field tensor. "Eine so interessante Korrespondenz habe ich noch nie erlebt" (Such an interesting correspondence I have never experienced), wrote Einstein to Levi-Civita on April 2, 1915. The first, and perhaps the most important development of Einstein's gravitational theory, came in 1917, when Levi-Civita, inspired by what he called "la grandiosa concezione di Einstein" and the growing need to reduce somewhat the formal apparatus introduced a remarkable mathematical refinement, the concept of parallel transfer [Levi-Civita 1917]. In liberating Riemannian geometry from the metric he opened the way to a much more general concept of differential geometry, providing the inspiration for the non-Riemannian geometry which is at the heart of gauge theory [O'Raifeartaigh 1997, 38]. Physicist were introduced to these concepts —and to the term *gauge*— through Weyl's article "Gravitation and Electricity" of 1918 [Weyl 1918] and the third edition of *Raum, Zeit, Materie* published in 1919. In 1917 Einstein and Levi-Civita resumed their correspondence, interrupted by the 1st World War, when Einstein's attention was caught by the paper "Sulla espressione analitica spettante al tensore gravitazionale nella teoria di Einstein", where Levi-Civita discussed Einstein's choice for the analytical expression of the gravitational tensor and consequently for the connected conservation law, which he derived otherwise, using for the first time the contracted Bianchi identities [See Cattani and De Maria 1989].

for Göttingen, as far as the liveliness of academic interest is concerned, in this field at least" [Rowe 2001, 399]. In fact not all physicists had reacted enthusiastically to Einstein struggle towards a general relativity theory. Planck, for example, had explicitly discouraged him, so that immediately after he successfully demonstrated agreement of his theory with the perihelion motion of Mercury, Einstein wrote to Besso on December 21, 1915: "Read the papers! The final release from misery has been obtained... Even Planck now begins to take the thing seriously, although he still resists it a bit". After Einstein's visit Hilbert began to work hard on general relativity, boldly aiming from the beginning at an axiomatic foundation of physics and at a kind of world formula, unifying gravitation with electromagnetism, and very much relying on the power of his superior mathematical formalism. In November of the same year Emmy Noether wrote to the Erlanger mathematician Ernst Fischer: "Invariantentheorie ist hier Trumpf; sogar der Physiker Hertz studiert Gordan-Kerschensteiner; Hilbert will nächste Woche über seine Einsteinschen Differentialinvarianten vortragen, und da müssen die Göttinger doch etwas können." (The theory of invariant is very popular here; even Hertz, a physicist, is studying the Gordan-Kerschensteiner; next week Hilbert will give a seminar on his Einstein differential invariants, by then people in Göttingen will have to know something about all that.)([49]).

In a paper entitled simply "Die Grundlagen der Physik" (The Foundations of Physics) [Hilbert 1915], dated 20 November 1915, Hilbert set forth axiomatically a unified theory of gravitation and electromagnetism, based on Gustav Mie's electromagnetic theory of matter, which towards 1912 had interested Max Born, who lectured on Mie's work before the Göttingen Mathematical Society([50]); his reformulation of this theory was crucial in Hilbert's route to the problems of general relativity [Corry 1999b, 176]. At the end of his communication Hilbert praised the "axiomatic method", he had used "which here, as we see, employs the most powerful instruments of analysis, namely the calculus of variations and invariant theory". Five days later Einstein presented his derivation of the gravitational field equations based upon the assumption of general covariance [Einstein 1915]([51]). "Hopes in the Hilbert circle ran high at that time; the dream of a universal law accounting both for the structure of the cosmos as a whole, and of all the atomic nuclei, seemed near fulfillment", as Hermann Weyl, who was very much a part of that circle, later wrote [Weyl 1944, 171]. During 1916/1917 Hilbert lectured on the foundations of physics. His views, much on the tradition initiated by Minkowski, rested on the idea that the roles of geometers and physicists could merge onto one of a mutual exploration of a single science. Hilbert had reflected on the natural character of geometry and on the role of axioms in elucidating its foundation since 1894, when his *Grundlagen der Geometrie*

([49]) Here she is referring to Gordan's *Vorlesungen über Invariantentheorie,* edited by G. Kerschensteiner (1885 and 1887) [Kastrup 1983, 122].

([50]) In his electrical theory of matter Mie proposed to make gravitation a consequence of electrodynamics; his purpose was to develop a theory in which the electromagnetic field quantities "completely suffice to describe all the phenomena of the material world" [Mie 1912, 513]. In combining Mie's theory of matter with Einstein's theory of gravitation, Hilbert's aim was instead of making electrodynamics a consequence of gravitation.

([51]) Recent archival findings and careful analysis of Hilbert's papers have shown untenable the generally accepted view that Hilbert and Einstein independently discovered essentially the same gravitational field equations. Hilbert's statement that his field equations are in agreement with Einstein's is not contained in the initial page proofs; actually by the time Hilbert's paper was published in a revised form at the end of March 1916, he had been able to modify his field equations so that they were in agreement with Einstein's [Corry *et al.* 1997; Rowe 2001].

was published: "Nevertheless the origin [of geometrical knowledge] is in experience. The axioms are, as Hertz would say, images of symbols in our mind, such that consequents of the images are again images of the consequences ["Folgen der Bilder wieder Bilder der Folgen sind"], *i.e.*, what we can logically deduce from the images is itself valid in nature" [Corry 1999b, 151]([52]). Heinrich Hertz, who had been a great source of scientific inspiration for Minkowski, published the first edition of *Die Prinzipien der Mechanik in neuem Zusammenhange dargestellt* (The principles of mechanics presented in a new form) in the same year. From the very beginning Hilbert's conception had focused on geometry as a natural science; in this respect the advent of general relativity was perhaps the most striking demonstration of his early views on a pre-established harmony between mathematics and reality. From then on Hilbert became more and more enthusiastic about the significance of general covariance and considered Einstein's relativity theory "the highest achievement of human spirit".

Einstein sharply criticized Hilbert in a postcard to Ehrenfest (25 May 1916): "I do not like Hilbert's formulation. It is needlessly specialized and, as far as 'matter' is concerned, unnecessarily complicated. It is not honest (=Gaussian) in design, the pretension of a superman by a camouflage of techniques". On December 15, 1917, Einstein frankly wrote to Klein: "Es scheint mir doch, daß Sie den Wert formaler Gesichtspunkte sehr überschätzen. Dieselben sind wohl wertvoll, wenn es gilt, eine schon gefundene Wahrheit endgüldig zu formulieren, aber sie versagen fast stets als heuristische Hilfsmittel." (It seems to me that you highly overrate the value of formal points of view. These may be valuable when an *already found* truth needs to be formulated in a final form, but they almost always fail as heuristic aids.)([53]). In the same time he expressed his perplexities with his friends about Hilbert, Klein and their followers: "The people in Göttingen sometimes strike me, not as if they want to help one formulate someting clearly, but as if they only want to show us physicists how much brighter they are than we" [Reid 1970, 142]. Hilbert's approach to relativity, in particular his faith in the axiomatic method was judged by Einstein "childish, just like an infant who is unaware of the pitfalls of the real world", as he wrote to Hermann Weyl on November 23, 1916. Weyl himself thought that Hilbert's work in physics was of limited value, if compared to his work in pure mathematics. In an obituary of Hilbert he remarked that "Men like Einstein and Niels Bohr grope their way in the dark toward their conceptions of general relativity or atomic structure by another type of experience and imagination than those of the mathematician, although

([52]) Later Born clarified the nature of Hilbert's axiomatic treatment and why, in general, physicists tended not to appreciate it: "The physicists set out to explore how things are in nature; experiment and theory are thus for him only a means to attain an aim. Conscious of the infinite complexities of the phenomena with which he is confronted in every experiment, he resists the idea of considering a theory as something definitive. He therefore abhors the word 'axiom', which in its usual usage evokes the idea of definitive truth. The physicist is thus acting in accordance with his healthy instinct, that dogmatism is the worst enemy of natural science. The mathematician, on the contrary, has no business with factual phenomena, but rather with logic interrelations. In Hilbert's language the axiomatic treatment of a discipline implies in no sense a definitive formulation of specific axioms as eternal truths, but rather the following methodological demand: specify the assumptions at the beginning of your deliberation, stop for a moment and investigate whether or not these assumptions are partly superfluous or contradict each other" [Corry 1999b, 181].

([53]) Abraham Pais emphasized the irony of this remark recalling how Einstein later became devoted to mathematical formalisms in his search for a unified field theory [Pais 1982, 325].

no doubt mathematics is an essential ingredient" [Corry 1999b, 180]. By that time Weyl's active struggle in the arena of theoretical physics had dragged him far from the optimistic spell of Hilbert's views about the ability of mathematicians to contribute to theoretical physics on such a grandiose scale. Elizabeth Garber has observed how physicists "rudely reminded mathematicians that there was more to physics than the formulation of a generalized, mathematical coherence. Physical imagery distinguished the physically significant from the myriad of mathematical possibilities. Minkowski had chosen his group $\mathrm{G_c}$ rather than any other group of transformations because of its significance already indicated in mechanics and electrodynamics." [Garber 1999, 358]. In 1922 Élie Cartan proved *a posteriori* that Einstein had discovered the only possible set of equations consistent with his request of general covariance. The intuition of a physicist had outpaced the ratiocinations of mathematicians: "...physics... had the final say [von Neumann 1961, 3].

Was not the tense competition between Einstein and Hilbert during November 1915 the climax of long-lasting mutual interests of mathematicians and physicists in the relations between space, time, and gravitation? In spite of their different views Hilbert and Einstein certainly admired one another under some respect, and it was Hilbert who recommended the award of the third Bolyai Prize in 1915 to Einstein "for the high mathematical spirit behind all his achievements" [Reid 1970, 142](54).

Theoretical physics, whose methods were undergoing a radical epoch-making change, was finding its own way between *mathematics*, *physics* and *mathematical physics*.

9. – Symmetry of laws: Emmy Noether

Sophus Lie did not live enough to see his prophetical vision about the laws of universe being the invariants of infinitesimal transformations fulfilled in Emmy Noether's fundamental paper *Invariante Variationsprobleme* (Invariant variational problems) presented by Felix Klein to the July 26 meeting of the Königlische Gesellschaft der Wissenschaften in Göttingen in 1918. In this work group theory —especially Lie's theory for solving or reducing differential equations by means of their invariance groups— algebraic and differential invariant theory, Riemannian geometry and the calculus of variations shed a deep insight into the nature of the connection between symmetries and conservation laws in physics. The genesis of Noether's paper was closely related to David Hilbert's work on Albert Einstein's theory of gravitation and particularly to unresolved issues regarding the problem of energy-momentum conservation in the theory, which were the object of an extensive memoir communicated by Klein to the Göttingen Academy on 19 July 1918: "Einsteins's papers, upon which I shall comment here, indeed demonstrate that at times, although not systematically, he resorts to the same conceptual freedom which I have proposed in my *Erlanger Programm*" [Klein 1918]. Klein's initial interest in relativity theory had been awakened by Minkowski's approach, based on the invariance properties of the Lorentz group. Everything perfectly combined with his great admiration for Riemann, who had provided the mathematical apparatus where Einstein had found the frame to fit his physical ideas. In his famous "trial" lecture *Über die Hypothesen, welche der Geometrie zu Grunde liegen [On the hypotheses which lie at the basis of geometry]*, delivered at Göttingen in June 1854, Riemann approached the foundations of geometry through the general concept of space as a particular instance of the more general concept, that

(54) The first Bolyai prize had been awarded by the Hungarian Academy of Science to Henri Poincaré in 1905, and to Hilbert himself in 1910.

of manifold, a multiply extended magnitude. In his description of metrical spaces with variable curvature a metrical structure is defined by means of a quadratic differential form. Although Riemann's approach was exceedingly general, it was motivated by physical concerns; he concluded his lecture with the remark: "Die Frage über die Gültigkeit der Voraussetzungen der Geometrie im Unendlichkleinen hängt zusammen mit der Frage nach dem inneren Grunde der Massverhältnisse des Raumes… Die Entscheidung dieser Fragen kann nur gefunden werden indem man von der bisherigen durch die Erfahrung bewährten Auffassung der Erscheinungen, wozu Newton den Grund gelegt, ausgeht und diese durch Tatsachen, die sich aus ihr nicht erklären lassen, getrieben allmählich umarbeitet; …Es führt dies hinüber in das Gebiet einer andern Wissenschaft, in das Gebiet der Physik…" (The question of the validity of the hypotheses of geometry in the infinitely small is bound up to the the question of the ground of the metric relations of space… A decision on these questions can only be found if one starts from the present, empirically tested concepts of the phenomena, for which Newton laid the foundations, and compelled by facts which cannot be explained by them, gradually modifies these concepts; …This leads us into the domain of another science, into that of physics…)([55]).

Noether, who later would be called "the mother of abstract algebra"([56]), was a real expert in algebraic invariant theory, so that Einstein, quite impressed by a paper she had published in January of that same year [Noether 1918a] wrote enthusiastically to Hilbert on May 24: "Gestern erhielt ich von Frl. Noether eine sehr interessante Arbeit über Invariantenbildung. Es imponiert mir, dass man diese dinge von so allgemeinem Standpunkt übersehen kann. Es hätte den Göttinger Feldgrauen nichts geschadet, wenn sie zu Frl. Noether in die Schule geschickt worden wären. Sie scheint ihr Handwerk zu verstehen!" (Yesterday I received from Frl. Noether a very interesting paper on invariant forms. It impresses me that one can comprehend these matters from such a general standpoint. It would not have hurt the Göttingen troops in the field if they had been sent to Frl. Noether's school. She appears to know her business!)([57]).

Actually Noether started considering a specific case: the apparent failure of ordinary laws of energy-momentum conservation in Einstein's general theory which had concerned Klein and Hilbert since 1916; both had asked Emmy Noether's help to clarify the matter([58]). Lie algebras and Lie groups arise naturally where there is symmetry, so they have a significant role to play in the study of differential equations with invariants. Knowledge of these invariants, such as the total energy of the system and total angular momentum, which arise in problems involving bodies in motion, can be used to simplify the differential equations describing the motion. In her typical style Noether obtained the most general

([55]) At that time Riemann was preoccupied with the study of the "forces" of nature —gravity, light, heat, electricity, and magnetism— and there is some evidence to indicate that he believed that the mathematical analysis of such interrelations might require differential forms corresponding to manifolds of variable curvature [Hawkins 2000, 125].

([56]) Emmy Noether's unique contribution to mathematics is described by Hermann Weyl in his obituary article as "a new and epoch making style of thinking in algebra" [Weyl 1935].

([57]) In this paper Emmy Noether gave a general procedure for calculating all differential invariants of a differential form. She was the daughter of the mathematician Max Noether from Erlangen: Klein had gone to Göttingen from there and had kept up his contacts with him and with Paul Gordan, the " king of invariants", who had been Emmy Noether's thesis supervisor. In the spring of 1915 she was in Göttingen, invited by Hilbert and Klein, working on invariant theory.

([58]) For the connections between Emmy Noether's work and Göttingen mathematicians see [Rowe 1999].

result: every conservation law associated with a system arising from a variational principle (which is the usual case in physics) comes from a symmetry property. In dynamics, for example, if the variational principle leading to the so-called Euler-Lagrange equations is invariant under translations of the time variable, there is conservation of energy, and invariance under the group of translations and rotations in space yields conservation of linear and angular momentum. Notwithstanding the extraordinariness implied by these results, the physicists' community slowly became aware of the importance of Emmy Noether's remarkable work, which for nearly 40 years was seldom mentioned in the literature, even if implicitly used, and became fully appreciated only starting from 1960s, when Noether's theorem became a basic tool in the arsenal of theorists([59]).

In her paper two theorems are proved and their converses, both of which are derived from a variational problem; as she claimed, they are essential in order to clarify the distinction between those which Hilbert called "proper" conservation laws, that express the invariance of a physical quantity, and those (called "improper" by Hilbert) which can be derived without the field equations for the associated field being satisfied; for this reason they may be regarded as mathematical identities deriving from the Lagrangian formalism itself. In theories prior to general relativity, such as classical mechanics and electrodynamics, the conservation laws are consequences of the equations of motion of particles and fields. Hilbert pointed out that in general relativity the conservation of energy of the matter fields can be obtained without the matter field equations being satisfied. Conservation of energy in general relativity is an 'improper' conservation law, because it follows from satisfaction of the Einstein field equations, independently of the specific form of those equations and of the Euler-Lagrange equations associated with the matter fields([60]).

Theorem I applies when a physical system is characterized by a finite continuous group of symmetries (transformations that depend on constant parameters), and theorem II when the symmetry group is an infinite-dimensional Lie group (transformations determined by finitely many functions and their derivatives). In the first case such transformations are global transformations, and the theorem generalized the formalism underlying the standard results pertaining to first integrals in classical mechanics: for every continuous global symmetry there exists a conservation law. As already mentioned above, the familiar applications of Noether's first theorem are that spatial translation, spatial rotation, time translation, and boost symmetries are connected to conservation of linear momentum, angular momentum, energy, and centre-of-mass motion. This applies to theories that are Galilean or Poincaré invariant, as in the case of special relativity. If the Lagrangian is invariant under an m-parameter Lie transformation group then m independent conserved quantities exist, which generate the group according to the procedure of Sophus Lie. In quantum mechanics, Noether theorem is simply "the observation that the commutator relationship $[H, Q] = 0$ between the Hamiltonian and any dynamical variable Q has a dual interpretation: considered as the action of H on Q it implies

([59]) For an account of Emmy Noether's work in the context of general relativity see [Kastrup 1983, Byers 1996 and Rowe 1999]. Kastrup and Byers contain a discussion about the lack of references to Noether's theorem and the delayed impact of Noether's work within the scientific community.

([60]) The distinction between proper and improper conservation laws and the case of general relativity, is discussed in detail in [Brading and Brown 2003]. For a discussion of Noether's two theorems, exploring their relationships with Weyl's work of 1918 and 1929, and the modern connection between gauge symmetry and conservation of electric charge see [Brading 2002].

that Q is conserved and considered as the action of Q on H it implies that Q generates a symmetry group. Thus to every symmetry there corresponds a conserved quantity and conversely" [O'Raifeartaigh 1997, 20].

The older history of the 10 classical conservation laws in mechanics preceding Emmy Noether general derivation goes back to Lagrange, Hamilton and Jacobi, whose derivations —especially those of momentum and angular momentum conservation— from translation and rotation invariance, where much in the spirit, or even quite similar to, Noether's theorem([61]). The connection between the 10 classical conservation laws (energy, momenta, angular momenta and uniform centre of mass motion) and the corresponding space-time symmetries (time and space translations, rotations and Galileo or special Lorentz transformation) which had interested Klein for several years, received a most general treatment in Noether's first theorem, which generalized the formalism underlying the standard results regarding first integrals in classical mechanics.

Her second theorem, which she characterized as "the most general group-theoretic generalization of 'general relativity'" [Noether 1918b, 240], applies in case of invariance under an infinite-dimensional group, which, as already quoted, arise from symmetry transformations that depend on arbitrary functions of space and time and on their derivatives (local trasformations). Noether noted that the conservations laws of classical mechanics as well as those of special relativity theory are proper in the sense that one cannot deduce these as invariants of a suitably particularized subgroup of an infinite Lie group. In general relativity, on the other hand, Noether found that there are four identities, or "dependencies" as she called them, between Euler-Lagrange functions of the theory and their derivatives, as a consequence of the principle of general covariance. These "depen-

([61]) In his communication of 1868 entitled "On the facts which lie at the basis of geometry" Helmholtz related the constants of motion of a solid body in space to the geometrical properties of space and of the body itself. Jacobi had found a connection between the Euclidean invariance of the mechanical Lagrangian and the conservation laws for linear and angular momenta. He was the first person who derived the 10 integrals of the mechanical equations of motion partly by using the infinitesimal transformations contained in the Euclidean group. Later Ignaz Schütz (1897) derived momentum conservation from the Galileo-covariance of energy conservation. During the last decade of the XIX century several papers appeared related to the method of obtaining constants of motions by means of cyclic coordinates (among the authors were Gaston Darboux, Paul Painlevé, Otto Staude, Levi-Civita and Guido Fubini). Georg Hamel, who had Hilbert as thesis supervisor in 1901, demonstrated the connection between symmetry transformations and conserved quantities in mechanics using Lie's work on continuous groups (1904). Beginning in 1909 Hamel continued his attempts to establish the axiomatic foundations of mechanics along the lines of Hilbert [Mehra 1973a, 158]. In 1911 Gustav Herglotz generalized Hamel's work discussing the 10-parameter Poincaré's group of "motions" in the four-dimensional space, and related its symmetries to ten general integrals, among which the most familiar are the energy, momentum and angular momentum. The other three conserved quantities correspond to a generalization of the center-of-mass integrals in classical mechanics. The connection between the 10 classical conservation laws and the corresponding space-time simmetries had interested Klein for several years. Herglotz was well-known to Felix Klein for having worked in Göttingen from 1903 till 1908, so that when he saw his paper he realized that the connection between symmetry properties of a system and its conservation laws was related to Lie's work on group theory. On Klein's request Friedrich Engel, his former student and then collaborator of Sophus Lie, derived the 10 known integrals of the non relativistic mechanical n-body system "in the sense of Lie". In this work Engel derived the Lie algebra of the 10-parameter Galileo group, too (1916). Two years after Emmy Noether's paper Klein asked E. Bessel-Hagen to apply her results to Galileo's invariance of mechanics and conformal invariance of electrodynamics [Kastrup 1983].

dencies" are the four so-called Bianchi identities for the components of the curvature tensor which enters the equations of gravitation. It is remarkable that neither Hilbert nor Klein had recognized the connection between Hilbert's identities for the contracted curvature tensor and the equations of general geometry, the Bianchi identities, which express the energy conservation law similar to those found by Einstein and Lorentz, and by Weyl in his first edition of *Raum, Zeit, Materie*([62]).

10. – "Relativity Theory as a Stimulus in Mathematical Research": Hermann Weyl

Towards the end of 1913, the young Hermann Weyl, influenced by the works of Hilbert, Lorentz and Klein, as well as by Mie's field theory of matter, was particularly fascinated by the great intellectual achievement of Einstein's theory, to make it the basis of a new programme of his own, in the tradition of researches of very general theories of physics.

He had developed at the school of mathematics grown up around Hilbert: "I came to Göttingen as a country lad of eighteen... In the fullness of my innocence and ignorance... the doors of a new world swung open for me, and I had not sat long at Hilbert's feet before the resolution formed itself in my young heart that I must by all means read and study whatever this man had written" [Weyl 1944]. Weyl, who later recalled Hilbert as the Pied Piper whom the young mathematicians followed [Reid 1970, 134], had arrived there in 1904 and received his doctorate in 1908; his supervisor was Hilbert and his thesis dealt with singular integral equations. He remained in Göttingen until 1913, when he moved to Zürich where he would hold the chair of mathematics at Technische Hochschule until 1930, and where he made direct contact with Einstein, then a professor in the same

([62]) It is surprising that scarcely any reference has been successively made to the striking and definitive results found by Noether, even by Hilbert, Klein and Einstein. In Spring 1958 Pauli talked with Jagdish Mehra about the first edition of his influent report on relativity theory [Pauli 1921], which did not contain a single reference to Noether's "Invariante Variationsprobleme": "You know, it was not all that perfect. I had not even mentioned the Bianchi identities" [Mehra 2001b, 316]. And indeed he discussed the identities in detail in the English edition published that same year. Nevertheless Pauli again did not mention Noether's work. It has been rightly pointed out how references to Noether's theorem are quasi inexistent in the literature on quantum physics before the late 1950s. In the author's preface to the 1919 edition of his *Raum, Zeit, Materie*, in chapter IV, devoted to Einstein's theory of gravitation and containing the principle of energy-momentum, Weyl did not mention Noether's theorem and in the English edition of 1921 he mentions Noether's paper in footnote 5 to Ch. IV. On the other hand Weyl was well aware of the connection between invariance and conservation laws, so that in his *Gruppentheorie und Quantenmechanik* of 1928 he stated that as a general rule "every invariance property of the kind met in general relativity... gives rise to a differential conservation theorem". In his seminal paper of 1929 Weyl does not explicitly mention it, even if he gives a systematic derivation of the Noether conservation laws. He used her theorem II in his *Gruppentheorie und Quantenmechanik*, and refers to the theorems for conservation of energy and momentum again omitting referencing Noether's paper. Notwithstanding this, his contribution was to make these results familiar to physicists in the context of field theory. A short section devoted to Noether's theorem could be found in Courant and Hilbert's *Methods of Mathematical Physics*, published in Germany in 1924. This lack of initial references probably had a great part in losing the connection with Emmy Noether, so that others followed suit. For a detailed discussion see [Byers 1966 and Kastrup 1983].

city([63]). As he later recounted, "Einstein's memoir came into my hand and set me afire" [Hawkins 2000, 422].

In his definitive paper of 1916 [Einstein 1916] Einstein had looked to the possibility of combining gravitational and electromagnetic theories to gain insight into the structure of matter. In an address on "Ether and the Theory of Relativity" delivered on October 27, 1920, at Leiden University, he concluded saying that gravitation and electromagnetism are "two realities which are completely separated from each other conceptually". To view them as "one unified conformation" would be a great advance in physics: "Then for the first time the epoch of theoretical physics founded by Faraday and Maxwell would reach a satisfactory conclusion. The contrast between ether and matter would fade away, and, through the general theory of relativity, the whole of physics would become a complete system of thought" [Jungnickel and McCormmach 1986, Vol. 2, 332]. Weyl's *Gravitation und Elektrizität* of 1918 [Weyl 1918] was the first attempt of a mathematical generalization of Einstein's theory, where he suggested that this generalization might encompass not only gravitational but also electromagnetic phenomena.

His paper had the purpose of constructing what Weyl called a "true" infinitesimal geometry and then use it to construct a unified theory of gravitation and electromagnetism. In the beginning paragraphs Weyl said that according to special relativity "the stage for physical events, *the world*, is a *four-dimensional*, *metrical continuum*". It was on the basis of the idea that the metric is an intrinsic property of the space, dependent on the matter contained in it, that Einstein "erected the grandiose structure of general relativity" [O'Raifeartaigh 1997, 24]. Weyl went on remarking that "Whereas the gravitational potentials are the components of an invariant quadratic differential form, *electromagnetic phenomena* are controlled by a four-potential, whose components ϕ_i are the components of an invariant *linear* differential form $\sum \phi_i \mathrm{d}x_i$ [in today's notations $\sum A_\mu \mathrm{d}x^\mu$]. However, both phenomena, gravitation and electricity, have remained completely isolated from one another up to now".

Weyl moved by remarking that Riemannian geometry was not a consistently infinitesimal geometry, containing a last element of geometry "at a distance... due only to the accidental development of Riemannian geometry from Euclidean geometry". According to the work of Levi-Civita [Levi-Civita 1917], in Riemannian geometry if two parallel vectors located at some point are moved by "parallel transport" to a different point along two different paths, they are generally no longer parallel. The direction of the transported vector depends upon the path taken in proceeding from the first point to the second one. Weyl pointed out that metric $\mathrm{d}s^2 = \sum_{ik} g_{ik}\mathrm{d}x_i\mathrm{d}x_k$ "allows the magnitudes of two vectors to be compared, not only at the same point, but at any arbitrarily separated points. *A true infinitesimal geometry should, however, recognize only a principle for transferring the magnitude of a vector to an infinitesimally close point* and then, on transfer to an arbitrary distant point, the integrability of the magnitude of a vector is no more to be expected than the integrability of its direction. On the removal of this inconsistency there appears a geometry that, surprisingly, when applied to the world, *explains not only the gravitational phenomena but also the electrical.* According to the resultant theory both spring from the same source, indeed *in general one cannot separate gravitation and electromagnetism in an arbitrary manner.* In this theory *all physical quantities have a world-geometrical meaning; the action appears from the beginning as a pure number. It*

([63]) In 1930 Weyl returned to Göttingen to take up Hilbert's chair.

leads to an essentially unique universal law; it even allows us to understand in a certain sense why the world is four-dimensional".

Weyl proposed a more general affine geometry in which the lengths of two different vectors are not absolutely comparable, and introduced, along with the usual quadratic differential form, a linear differential form that made the length of a transported vector depend on the path. As a consequence, metric relations are determined locally only up to a positive calibration (*gauge*) factor λ, which varies from point to point in such a way that the comparison of the lengths of two different vectors in two different points is also in general a path-dependent process.

In order to make this generalized geometry the foundation of a physical theory ("I shall first sketch the construction of the corrected Riemannian geometry without any reference to physics; the physical application will then suggest itself"), Weyl required that physical laws should be independent of the choice of *gauge*: "Correspondingly each formula must have a double-invariance: 1. it must be *invariant with respect to arbitrary smooth coordinate transformations* 2. it must remain unchanged *when the g_{ik} are replaced by λg_{ik}* where λ is an arbitrary smooth function of position. Our theory is characterized by the appearance of this second invariance property".

The length of a vector (or the square of its length) varies under an infinitely small parallel displacement according to the law $\mathrm{d}l = -l\mathrm{d}\phi$, where $\mathrm{d}\phi$ is a linear differential form $\mathrm{d}\phi = \sum \phi_i \mathrm{d}x_i$ which supplements the quadratic fundamental form (Riemannian line element) in the new geometry: "*The metrical connection of the space depends not only on the quadratic form* [$\mathrm{d}s^2 = \sum_{ik} g_{ik}\mathrm{d}x_i\mathrm{d}x_k$] *... but on the linear form* [$\mathrm{d}\phi = \sum \phi_i \mathrm{d}x_i$]". Weyl observed that the transformation $g_{ik} \to \lambda g_{ik}$ implied for the linear form $\phi_i \mathrm{d}x_i$ an arbitrariness of "the form of an *additive total differential* rather than a proportionality factor that would be determined by a choice of scale". In his attempt to unify electromagnetism and gravity Weyl's argued that the addition of a gradient $\mathrm{d}(\ln\lambda)$ to $\mathrm{d}\phi = \sum \phi_\mu \mathrm{d}x_\mu$, should not change the physical content of the theory. The quantities ϕ_i turn out to be components of a four-vector that transforms according to the law $\phi_i \to \phi_i - \frac{\partial\lambda}{\partial x_i}$ under the scale transformation $g_{ik} \to \lambda g_{ik}$.

"For the analytic representation of the geometry the two forms $g_{ik}\mathrm{d}x_i\mathrm{d}x_k$, $\phi_i\mathrm{d}x_i$ are on the same footing as $\lambda\, g_{ik}\mathrm{d}x_i\mathrm{d}x_k$, $\phi_i\mathrm{d}x_i + \mathrm{d}(\ln\lambda)\dots$ *The invariant quantity is therefore the anti-symmetric tensor with components* $F_{ik} = \frac{\partial\phi_i}{\partial x_k} - \frac{\partial\phi_k}{\partial x_i}$" concluded Weyl.

"The special case for which the magnitude of a vector at an arbitrary initial point can be parallel-transferred throughout the space in a path-independent manner appears when the g_{ik} can be chosen in such a way that the ϕ_i vanish... The necessary and sufficient condition for this to be the case is the vanishing of the tensor F_{ik}" remarked Weyl, and encouraged by the formal resemblance according to which the metric tensor g_{ik} was identified with the gravitational potential in general relativity, he identified the metric vector ϕ_i with the potential of the electromagnetic field: "Accordingly, it is very suggestive to interpret ϕ_i as the electromagnetic potential and the tensor F as the *electromagnetic field*".

It was the first *gauge* theory in which the Maxwell electromagnetic field and the gravitational field appeared as geometrical properties of space-time([64]). Weyl's hope was that

([64]) Invariance of the theory with respect to the addition $\mathrm{d}\phi \to \mathrm{d}\phi + \mathrm{d}(\ln\lambda)$ led Weyl to the term *Maßstab-Invarianz*, (translated as "calibration invariance") which later became *Eich-invarianz*, while the English term became "gauge invariance", which was quite appropriate at first, since the scale factor attached to the metric changed the measurement of length. See [Straumann 2001] for a discussion on the rise of the gauge theory. For an analysis of Weyl's geometry and local scale invariance would be connected in a natural way to the electromagnetic field as the invariance under local coordinate transformations is related to the gravitational field.

The main interest of Weyl's paper for physicists was that for the first time conservation of electric charge was derived from an invariance principle: "The strongest argument for my theory seems to be this, that gauge-invariance corresponds to the conservation of electric charge in the same way that coordinate-invariance corresponds to the conservation of energy and momentum", commented Weyl in a postscript written in June 1955 [O'Raifeartaigh 1997, 37](65).

At first sight Weyl's theory was a really fascinating mathematical work, even if extraordinarily difficult, and it was enthusiastically received by many theoretical physicists of the period. But the following month, in receiving Weyl's paper for the communication to the Berlin Academy, Einstein began to show some perplexities about the real physical significance of the theory, and on April 8 he wrote to Weyl: "I must say frankly that it cannot possibly correspond in my opinion to the theory of Nature, that in itself it has no real meaning". Direct application to gravitational theory was unacceptable, Einstein pointed out in a note published as a postscript at the end of the paper(66). Nernst and Planck were of the same opinion; and even Pauli, who had started his research career with a work on Weyl's theory, had an exchange of letters with him objecting about it(67).

Weyl had hoped that his theory, which according to Planck could be characterized as an attempt to subsume all physical phenomena under a "single 'world formula'", would also get an insight about the interior of atoms, something about which Planck was doubtful, thinking that "there could be no question of an experimental test of the theory at present" [Jungnickel and McCormmach 1986, Vol. 2, 332]. Einstein's work of the future would be devoted to the effort of building a unified theory of gravitation and electromagnetism, in the realm of the theories which in his *Autobiographical Notes* he later characterized as "such theories whose object is the *totality* of all physical appearances" [Einstein 1949, 23]. Weyl's attempt was a failure from the point of view of combining electromag-

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an account of the evolution of his ideas on this theory see [Scholz 2001a].

(65) O' Raifeartaigh has pointed out how Weyl was convinced to be "on the right track" by the fact that electromagnetic current conservation $\partial^\mu \mathrm{j}_\mu(x) = 0$ would follow from scale invariance in the same way that energy-momentum conservation $\partial^\mu \mathrm{T}_{\mu\nu}(x) = 0$ follows from Poincaré invariance. Moreover Weyl could not help being impressed by the fact that "the conservation of momentum and charge, which are so different from the phenomenological point of view, would have a common geometrical basis" [O'Raifeartaigh 1997, 45].

(66) Transformation of the metric tensor with a "scale factor" that, in turn, transformed the line element, caused lengths of parallel transported vectors, as well as their orientations, to change. Einstein objected that the measure of time of two different clocks depended on their displacements along different closed loops bringing them back to the same point of departure, in disagreement with observation. According to Einstein the length of a standard measuring rod (as well as the time measurement of a "standard clock") would be rescaled by the non-integrable scale factor and would therefore depend "on its history. "If this were really so in Nature chemical elements with spectral-lines of definite frequency could not exist and the relative frequency of two neighbouring atoms of the same kind would be different in general. As this is not the case it seems to me that one cannot accept the basic hypothesis of this theory, whose depth and boldness every reader must nevertheless admire", concluded Einstein in his postcript to Weyl's paper [O'Raifeartaigh 1997, 34]. All this led to an intense exchange of letters between Einstein (in Berlin) and Weyl (at the ETH in Zürich), part of which has now been published in [Einstein 1987-1998,Vol. 8]. See also [Yang 1986, 13].

(67) See Weyl to Pauli: May 10 and December 9, 1919. For Pauli's letters, see [Pauli 1979].

netism and gravitation, but many theoreticians were impressed by its depth and its thoroughly mathematical character, as well as by its connection with the general theory of relativity, which had achieved great success at the beginning of the 1920s. Moreover an interesting debate was also aroused by Weyl's theory around what a physical theory could and should achieve. The creation of a unified field theory incorporating the electromagnetic field on the basis of one or another generalization of four-dimensional Riemannian geometry led immediately (1921-1922) to the appearance of a whole series of unified geometrical field theories (Eddington, Einstein, Kaluza, Cartan) [Vizgin 1989]. This line of investigation attracted also Schrödinger's attention, and in 1922 he attempted to connect the Bohr-Sommerfeld quantum conditions with the geometrical foundations of Weyl's theory [Schrödinger 1922](68). Kaluza's theory, which used a 5-dimensional version of Einstein's theory, was extended to cover the case of wave mechanics by Klein and Fock in 1926 [Kaluza 1921, Klein 1926, Fock 1927], and the Klein-Fock-Gordon-Schrödinger equation, the first relativistic scalar wave equation for particle with a nonzero rest mass, was set up.

In the meantime, when also Noether's theorem was being published and went practically unnoticed, appeared Weyl's *Raum, Zeit, Materie*, the outgrowth of a course of lectures on relativity which had a great resonance and influence on a whole generation, whose first lines were: "Einstein's theory of relativity has advanced our ideas of the structure of the cosmos a step further. It is as if a wall which separated us from thruth has collapsed"(69). He also declared that "geometry has not become physics; rather physics has become geometry". All this appeared much in the spirit of Hilbert; in that same year Weyl wrote: "According to this theory everything real that is in the world is a manifestation of the world metric; the physical concepts are no different from the mathematical ones. The only difference that exists between geometry and physics is that geometry establishes in general what is contained in the nature of the metrical concepts, whereas it is the task for physics to determine the law and explore its consequences, according to which the real world is characterized among all the geometrically possible four-dimensional metric spaces". Weyl's efforts of developing the mathematical and philosophical foundation of relativity theory culminated in the fifth edition of his *Raum, Zeit, Materie*, which had a great resonance and influence on a whole generation. Einstein's reaction was very positive since the beginning: "Dear Colleague, I am reading the proofs of your book with admiration. It is like a master-symphony. Each little word is related to the whole and the structure is grandiose" [O'Raifeartaigh 2000, 40].

In *Raum, Zeit, Materie* Weyl had devoted considerable attention to tensor algebra and particularly to the symmetry properties of tensors which arise in relativistic physics and geometry. Weyl's convinction of the superiority of tensor calculus for the physical geometry of space-time was strongly criticized by Eduard Study, who had become a well-known expert in invariant theory, and complained that those working on the theory of relativity were neglecting the tools of the symbolical method of the algebraic theory of

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(68) In his attempt Schrödinger obtained the resonance formulation of the quantum conditions, in de Broglie's sense, so that later he was able to recognize more easily the wave character of particles, and to establish wave mechanics. See [Raman and Forman 1969].
(69) Weyl, who quickly mastered the theory, gave a series of lectures on it in the summer of 1917 in the ETH in Zürich. These were the basis of his book which appeared in 1918 and whose fourth edition (1921) was translated into English as *Space, Time, Matter* (Methuen, London, 1922) and in French (*Temps, espace, matière*, Blanchard, Paris, 1922); already by 1923 it had attained its fifth edition.

invariants that went back to Aronhold and Clebsch, in favor of the formalism of tensor calculus. In the foreword of his book on invariant theory, he had attacked Weyl using the latter's own words from the first edition of *Raum, Zeit, Materie*: "Many will be appalled at the deluge of formulas and indices with which the leading ideas are inundated. It is certainly regrettable that we have to enter into the purely formal aspect in such detail and to give it so much space, but nevertheless, it cannot be avoided" [Weyl 1918a].

In the case of the space problem Weyl had utilized only the basic elements of Lie's theory. Further research made him discover the power of group theory in quite a different direction and during the years 1923-1924 he discovered in the representation theory of groups something that could give him insight in the foundations of tensor algebra. Weyl's sharp response to Study came years later, in an essay entitled *Relativity Theory as a Stimulus in Mathematical Research* [Weyl 1949, 541]: "In the last decades a quite elaborate theory of representations of countinuous groups has developed... Here those problems which according to Study's complaint the relativists had let go by the board are attacked on a much deeper level than the formalistically minded Study had ever dreamt of". In *Relativity Theory as a Stimulus in Mathematical Research* Weyl recalled his intense envolvement with Einstein's general theory of relativity and the extension of his interests to the representations of Lie groups: "For myself I can say that the wish to understand what really is the mathematical substance behind the formal apparatus of relativity theory led me to the study of representations and invariants of groups". In the context of the fundamental questions inspired by general relativity Weyl discovered group theory as a powerful tool for "deeply penetrating matematical questions". Inspired by Cartan's theory of representations of Lie algebras, and by Georg Frobenius's theory of representations of finite groups combined with the appearance of a paper of Schur in 1924, Weyl was led to the discovery of what he termed "the group-theoretic foundation of tensor calculus" and then to a deepening appreciation of the theory of group representations as a powerful instrument for dealing with mathematical questions inspired by physics. The fruits of this work are expounded in his monograph *The Classical Groups, their Invariants and Representations* appeared in 1939.

Weyl's capacity of feeling quite at ease in any field of mathematics or physics with incredible rapidity, enabled him to emulate Hilbert's mastery of many mathematical fields, which he had also extended to theoretical physics. Starting in 1923 from seemingly (at the time) disparate fields such as Frobenius' theory of group characters, tensor calculus, Riemann surfaces, and integral equations Weyl evolved a general theory of continuous groups producing in 1925-1926 his celebrated three part paper on the representation of semisimple groups, a major step for the global structure of Lie groups, which he himself later regarded as a peak in his mathematical achievements([70]).

Weyl's own intellectual attitude is clearly stated in the following passage in the preface to the first edition of his book *The Classical Groups* [Weyl 1939, viii]:

> "The stringent precision attainable for mathematical thought has led many authors to a mode of writing which must give the reader the impression of being shut up

([70]) See Part IV of Hawkins 2000 for a detailed account of Weyl's involvement with Einstein's general theory of relativity and its subsequent work on the representation of Lie groups culminating in his great papers of 1925 and 1926, an important landmark in the history of mathematics. The first had the title "Theory of the Representation of Continuous Semisimple Groups by Linear Transformations", the second was written with F. Peter, and launched what is now called harmonic analysis on groups. See also [Borel 1986 and 2001].

> in a brightly illuminated cell where every detail sticks out with the same dazzling clarity, but without relief. I prefer the open landscape under a clear sky with its depth of perspective, where the wealth of sharply defined nearby details gradually fades away towards the horizon."

In sharing Hilbert's and Minkowski's tendency toward an inner unity of mathematical sciences, Weyl contrasted its tendency toward internal specialization, and compared it to physics: "Whereas physics in its development since the turn of the century resembles a mighty stream rushing on in one direction, mathematics is more like the Nile delta, its waters fanning out in all directions".

11. – Einstein's lesson

After sending his first paper for publication to the *Annalen der Physik* in 1915 [Einstein 1915], Einstein well expressed physicists' largely shared opinion about the unifying goals of theoretical physics, as he wrote in a letter to his former polytechnic classmate Grossmann, on April 14: "It is a magnificent feeling to recognize the unity of a complex of phenomena which appear to be things quite apart from the direct visible truth".

In his efforts along the line of deriving the whole theory from a single symmetry principle, the equivalence principle, which found its formal expression in the principle of general covariance (equivalence of all reference frames for the description of physical laws), Einstein revolutioned the way of doing theoretical physics. In doing this he was following Minkowski's procedure, according to which one should start with Lorentz invariance and require that field equations be covariant with respect to the invariance, according to the schema: symmetry → field equations. What had long been an underlying theme, had now become a principle. It was a real break with the past, the process had been completely reversed with respect to the XIX century style when, in following the schema: experiment → field equations → symmetry (invariance), Lorentz invariance had been found as a pure mathematical property of Maxwell equations, which in turn had been formulated fundamentally on the base of the experimental laws of electromagnetism. "We might say", as it was well expressed by Chen Ning Yang, "that Einstein initiated the principle that *simmetry dictates interactions*" [Yang 1980, 42]. This principle much later played an essential role giving rise to various field theories starting from the Yang-Mill gauge theory of 1954. But the achievement of this profound change of attitude in constructing physical theories had to undergo a new big leap in the realm of microphysics [Radicati 1991].

When Dirac in 1928 proposed a relativistic wave equation for spin-1/2 particles, and gave the first solution to the problem of expressing quantum theory in a form which was invariant under the Lorentz group of transformations of special relativity, the concepts of symmetry and invariance which had been at the core of Einstein's theories of relativity had been completely assimilated by theoretical physicists.

Dirac himself attached great importance to the existence of groups of transformations, which in the preface of the first edition of his *Principles of Quantum Mechanics*, published in 1930 he had connected with his views about what should be the new methods of theoretical physics in dealing with fundamental laws of nature: "The formulation of these laws requires the use of the mathematics of transformations. The important things in the world appear as the invariants (or more generally the nearly invariants, or quantities with simple transformation properties) of these transformations... The growth of the use of transformation theory, as applied first to relativity and later to the quantum theory, is the essence of the new method in theoretical physics. Further progress lies in the direction of making our equations invariant under wider and still wider transformations. . . " [Dirac 1930, v].

When Dirac obtained his equation from symmetry arguments only, the old quantum theory had been completely superseded by the new quantum mechanics, which had successfully explained phenomena about which Poincaré wondered since 1904, in his book *La valeur de la science*: "Ce sont les mouvements des électrons qui produisent les raies des spectres d'emission. Mais pourquoi les raies du spectre sont-elles distribuée d'apres une loi régulière?" Poincaré also noted that this problem had nothing in common with the well-known equations of mathematical physics they were accustomed to, such as those related with vibrations of elastic bodies. "Les lois sont plus simples, mais elles sont de tout autre nature. Il y a là un des plus importants secrets de la nature".

This mystery was clarified by the quantum theory of the atom which was the basis for Niels Bohr's explanation of Mendeleeev's periodic system of elements. Bohr constructed his atoms one by one by adding one electron after the other to the elements. His force of intuition was so great that only from the properties of atomic spectra he divined the fact that not more than two electrons can be placed in one orbit, which almost automatically leads to the periods of Mendeleev. Thus, also the chemical ideas and concepts turned out to be contained in quantum physics and chemistry ceased to be a separate science. Everybody in the world was excited about the Bohr model, but Bohr himself, on the other hand, constantly remarked that this was temporary, and that one ought to be looking for a way to do it right.

The "New Quantum Mechanics" supplanted "old" quantum mechanics in 1925–26 as a result of an extraordinary outburst of creative imagination by Werner Heisenberg, Max Born, Pascual Jordan, Erwin Schrödinger, and Paul Adrian Maurice Dirac. In the meantime it became also clear that the puzzle of spectral lines could be completely explained only by considering unprecedented, most astonishing properties of the electrons, owing their origin to new kind of symmetries, which have no place in the classical context. The formidable problems posed by atoms with more than two electrons forced symmetry principles, other than the Lorentz group of transformations for special relativity, to play a more fundamental role in the realm of quantum mechanics, conveyed by group theory. Their first application was to the theory of atomic spectra.

PART II

12. – Physicists *vs.* physicists: the "New Quantum Mechanics"

In 1921 Max Born was called to the direction of the Institute for Theoretical Physics in Göttingen. His rigorous mathematical methods were in the spirit of David Hilbert and Hermann Minkowski. Under their influence he had grown up and spent the first years of his scientific career in that same University([71]). After two years the young Werner Heisenberg, a former pupil of Arnold Sommerfeld, had become his assistant. The two embarked upon a systematic study of complex atoms, but by 1925 Born concluded that all the advanced methods of celestial mechanics had failed in the helium problem, so he was convinced that new mathematical tools, completely different from those provided by

([71]) For biographical notes on Born up to the early 1920s, rich of information drawn from personal conversations, see [Mehra 2001a].

the differential calculus, had to be developed for atomic mechanics, and that the correct understanding of atoms called for a radical reformulation of the physical theory.

In the first days of July of that same year Heisenberg had submitted to Born a paper which deeply "fascinated" him, as he recalled years later: "I was soon so involved in it that I thought about it for the whole day and could hardly sleep at night. For I felt there was something fundamental behind it, the consummation of our endeavours of many years. And one morning, about the 10 July, I suddenly saw the light: Heisenberg's symbolic multiplication was nothing but the matrix calculus, well known to me since my student days from Rosanes' lectures in Breslau". [Born 1978, 217; Born *et al.* 1969](72). In reformulating Heisenberg's quantum condition into matrix notation Born could recognize at once its formal significance: It meant that the two matrix products pq and qp are not identical. He was familiar with the fact that matrix multiplication is not commutative; therefore he was not too much puzzled by this result. After seventy years, Heisenberg's discovery that in the description of atoms quantities which do not commute play a crucial role, was now evoking in the battleground of physical events the old multiplication rule for matrices of Arthur Cayley's abstract matrix calculus. Rosanes himself had observed in 1903 that the estrangement of mathematics and physics of the last several decades was past and that an "epoch of closer union" had opened [Jungnickel and McCormmach 1986, Vol. 2, 340]. And now all this was largely going beyond his expectations.

When Heisenberg visited Leyden in July 1925 and mentioned his new results in quantum mechanics, Ehrenfest remarked that "he was like Sir Isaac Newton; not only had he invented a new mechanics, but he also had to invent a new mathematics to go with it" [Mehra and Rechenberg 1982, Vol. 4, 232].

Pauli was enthusiastic about Heisenberg's work, which he knew from letters the two had exchanged in June, but it is well known how he rejected Born's tendence to "complicated formalisms" and feared he would spoil Heisenberg's physical ideas, as he wrote to Kramers on July 27, 1925: "Mit Freude habe ich auch wahrgenommen, daß Heisenberg in Kopenhagen bei Bohr ein bißchen das philosophische Denken gelernt hat und sich vom rein Formalen doch merklich abwendet" (I was also pleased to notice that Heisenberg has learned some philosophical thinking from Bohr in Copenhagen and takes a sharp turn away from purely formal methods).

As Jammer has duly recalled: "Obviously the abstract, almost philosophical, nature of Heisenberg's paper and its apparent lack of immediate practical applications made it repugnant to the more experimentally inclined physicists. But precisely the same qualities, viewed as initiating a new era of progress in theoretical physics, appealed to the more philosophically minded theoreticians —as far as they took notice at all" [Jammer 1966, 209]. Thus Bohr fully acknowledged the importance of the latest achievements in the atomic theory at the sixth Scandinavian Mathematical Congress, held in Copenhagen

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(72) As a young student at the university of Breslau Born had attended lectures of Jacob Rosanes, who was an expert in algebraic geometry and invariant theory: "He introduced us very early into the ideas of group theory and matrix calculus and I owe to him my knowledge of this powerful method which I later successfully applied to physical problems, first to the theory of crystal lattices, then to quantum mechanics. In those four Breslau semesters I laid a solid foundation for my mathematical education" [Born 1978, 53]. Actually Born had utilized matrices first in 1909, when he worked on electrodynamics after Minkowski's death, and in 1912, when he had used infinite matrices in the theory of crystal lattices developed with Theodore von Kármán [Born 1940, 617] For early applications of matrix methods in physics see [Mehra and Rechenberg 1982, Vol. 4, 34-44].

at the end of August, 1925, only a few weeks after the completion of Heisenberg's paper. In his remarks appended to the original address in November before publication, Bohr said: "It will interest mathematical circles that the mathematical instruments created by the higher algebra play an essential part in the rational formulation of the new quantum mechanics. Thus the general proofs of the conservation theorems in Heisenberg's theory carried out by Born and Jordan are based on the use of the theory of matrices, which go back to Cayley and were developed by Hermite. It is to be hoped that *a new era of mutual stimulation of mechanics and mathematics has commenced* [emphasis added]"([73]).

Jordan had worked at the preparation of Hilbert and Courant's *Methods of Mathematical Physics* (1924) whose content would be fundamental for the development of quantum mechanics. His collaboration with Born led to matrix formulation of quantum mechanics, completed on 27 September 1925 [Born and Jordan 1925]. Their paper contained an extended explanation of matrix methods, the interpretation of Heisenberg's symbolic multiplication as matrix multiplication, the proof of the formula for the product difference $\mathbf{pq} - \mathbf{qp}$ based upon the calculation of the time derivative of $\mathbf{pq} - \mathbf{qp}$, proof of energy conservation by the same method, and the proof of Bohr's frequency condition([74]). It also contained an attempt at the quantization of the electromagnetic field by regarding its components as matrices, as Born recalled later: "Where our suggested application of matrices to fields was concerned, we found not the slightest response; some distinguished physicists (such as the Russian Frenkel) considered our suggestion a mild form of madness" [Born 1978, 219-222]. It is now clear how Born's extraordinary mastery of mathematical subleties with which he had become familiar since the times he had been Hilbert's assistant, explains how Heisenberg's intuitions could be transformed within only three months into a full-fledged matrix mechanics.

Pauli, having overcome his initial disgust at Göttingen's abstract formalism, by the end of that year demonstrated that matrix mechanics gave the correct approach to atomic theory obtaining the (non-relativistic) spectrum of hydrogen. But on October 12 he had written in a very long letter to Kronig: "Die Heisenbergsche Mechanik hat mir wieder Lebensfreude und Hoffnung gegeben. Die Lösung des Rätsels bringt sie zwar nicht, aber ich glaube, daß es jetzt zieder möglich ist, vorwärts zu kommen. Man muß zunächst versuchen, die Heisenbergsche Mechanik noch etwas mehr im Göttinger formalen Gelehrsamkeitsschwall zu befreien und ihren physikalischen Kern noch besser bloßzulegen" (Heisenberg's mechanics has again given me hope and joy in living. It has not yet brought the solution of the riddle, but I believe that it is again possible to go

([73]) See [Mehra and Rechenberg 1982, Vol. 3, 3] for a very systematic inquiry about the rediscovery of infinite matrice as a mathematical tool for the formulation of quantum-mechanical relations. The survey includes historic developments of matrix calculus, an account of the early applications of matrix methods in physics, as well as a detailed description of the early applications in quantum theory by Born, Jordan and Heisenberg.
([74]) Heisenberg generalized the relation for $\mathbf{pq} - \mathbf{qp}$ to several variables, as he wrote in a letter to Jordan, dated September 21, 1925: $p_r q_s - q_s p_r = h/2\pi i$ for $s = r$, $p_r q_s - q_s p_r = 0$ for $s \neq r$; $p_r p_s - p_s p_r = 0$, $q_r q_s - q_s q_r = 0$. The same relations were proposed independently by Dirac, Pauli, but also by Weyl, as can be found in a letter by the latter to Born dated September 27, 1925: "Lieber Herr Born! Ihr Ansatz wur Quantentheorie hat auf mich gewaltigen Eindruck gemacht...". Next, Weyl considers the matrices $\mathbf{p}$ and $\mathbf{q}$ as infinitesimal generators of Lie groups $\{e^{\xi\mathbf{p}}\}$ and $\{e^{\eta\mathbf{q}}\}$ and writes Born's relation for $\mathbf{pq} - \mathbf{qp}$ as $e^{\xi\mathbf{p}}e^{\eta\mathbf{q}}{=}e^{h\eta\xi}e^{\eta\mathbf{q}}e^{\xi\mathbf{p}}$. By h Weyl means $h/2\pi$. Using this formula, he gives a proof of the conservation of energy and shows how the relation can be generalized to several variables. See [van der Waerden 1967, 52].

forward. One must first seek to liberate Heisenberg's mechanics from Göttingen's deluge of erudite formalism and better expose its physical essence).

Even Heisenberg was very puzzled: "Now the learned Göttingen mathematicians talk so much about Hermitian matrices, but I do not even know what a matrix is", he wrote to Jordan from Copenhagen. On November 16, 1925, he wrote Pauli about the final redaction of the historical paper with Born and Jordan, and told him how he had made every effort in keeping it on a "more physical" side. But he was not satisfied, and also feared he was "too stupid" to really understand mathematics. He was even thinking of "completely removing the word Matrix" from the work. He also described the general atmosphere:

> "Göttingen zerfällt in zwei Lager, die einen, die, wie Hilbert (oder auch Weyl in einem Brief an Jordan) von dem großen Erfolg reden, der durch die Einführung der Matrizenrechnung in die Physik errungen sei, der andere, der, wie Franck, sagt, daß man die Matrizen doch nie verstehen könne".
>
> (Göttingen is divided into two camps, those who, like Hilbert (or also Weyl, in a letter to Jordan), talk about the great success which has been scored by the introduction of matrix calculus into physics; the others, like Franck, who say that one will never be able to understand matrices.)

But Heisenberg learned very soon, and by the end of October the paper usually called the *Dreimännerarbeit*, a Born, Jordan and Heisenberg joint work which ushered the new era of quantum mechanics, was completed. It was the first comprehensive treatment of the foundations of matrix mechanics [Born *et al.* 1926]. As Born recalled, it contained

> "...the representation of physical quantities by non-commuting symbols, the generalization of Hamilton's mechanics for these, canonical transformations, perturbation theory, treatment of time-dependent perturbations with application to the optical theory of dispersion (derivation of Kramer's formula), the notion of degeneration, the connection with the eigenvalue theory of Hermitian forms, an outline of the treatment of continuous spectra, the theorems of momentum and angular momentum (quantization of direction, anomalous Zeeman effect), quantum mechanics of electromagnetic fields with derivation of Einstein's fluctuation formula... My main contribution was the relation of our theory to Hilbert's work on quadratic forms of an infinite number of variables with which I was acquainted from my student days. Here the matrices are used as operators acting on vectors (in a space of infinite number of dimensions, the so-called "Hilbert space"). These vectors form the bridge from our matrix mechanics to Schrödinger's wave mechanics..."[Born 1978, 219-222]

One can imagine Pauli's reaction to this torrent of *formalen Gelehrsamkeits* from Heisenberg's deeply irritated answer of October 1925 to one of Pauli's letters, presumably full of sarcasms:

> "Ihre ewigen Schimpfereien auf Kopenhagen und Göttingen sind einfach ein schreiender Skandal. Sie werden uns doch lassen müssen, da wir jedenfalls nicht mit bösem Willen die Physik zu ruinieren trachten; wenn Sie uns vorhalten, daß wir so große Esel seien, daß wir doch nie etwas physikalisch Neues fertig brächten, so mag das richtig sein. Aber dann sind Sie doch ein ebenso großer Esel, weil Sie's auch nicht fertig bringen."
>
> (Your eternal reviling of Copenhagen and Göttingen is a flagrant scandal. You will have to allow that, in any case, we are not seeking to ruin physics out of malicious

> intent. When you reproach us that we are such big donkeys that we have never produced anything new physically, it may well be true. But then, you are also an equally big jackass because you have not accomplished it either.)

In the months following the publication of the *Dreimännerarbeit*, Born and Norbert Wiener wrote an article introducing the concept of operators into quantum mechanics, thus emphasizing the usefulness of a branch of mathematics at that time still scarcely known to the majority of physicists [Born and Wiener 1925-1926, 1926](75): "The job was a highly technical one... I had the generalization of matrices already at hand in the form of what is known as operators. Born had a good many qualms about the soundness of my method and kept wondering if Hilbert would approve of my mathematics. Hilbert did, in fact, approve of it, and operators have since remained an essential part of quantum theory" [Wiener 1956, 108].

Born and Wiener generalized matrix mechanics into an operator calculus whereas Dirac recasted it into an algebraic algorithm. In the summer of 1926 Dirac presented q-number algebra as a purely mathematical theory, without referring to physics at all [Dirac 1926]. "Heisenberg broke away from the idea of Bohr orbits and was led to the formulation of a new dynamics. This new dynamics was based essentially on a new algebra... It was a big surprise to me... It showed me how completely wrong I was, sticking to Bohr orbits, when what one really wanted was a new mathematics which should be quite independent of the previous mathematics and should lead to some quite new equations" [Dirac 1983, 42]. The noncommutative character of the multiplicative rules he himself had found had indeed deeply puzzled Heisenberg, who at first had tried to avoid dealing with non commuting quantities, so foreign to all ideas of physicists at the time. Dirac, on the contrary, had no prejudice against noncommuting quantities for several reasons. In the development of his new calculus Dirac was influenced by his earlier study of Heaviside's calculus, by the symbolic calculus used in geometry(76), and by Grassmann's *Ausdehnungslehre*, where again he could find that the product of two quantities is not necessarily commutative. After introducing his own noncommuting quantities, the q-numbers, he recognized that quaternions, he had come across in his mathematical studies, provided the most general algebra for these quantities, and even if he did not use them as a tool for his work, the noncommutativity did not appear as something alien to him. He saw in quantum theory the emergence of a new type of algebra, the algebra of q-numbers —the dynamical variables which "satisfy all the ordinary laws of algebra, excluding the commutative law of multiplication". In those days he invented notations which are now part of our language: q-number, where "q stands for quantum or maybe queer", and c-numbers, where "c stands for classical or maybe commuting" [Dirac 1977, 109]. Like Hilbert and Minkowski, Dirac was feeling a sense of omnipotence: "It was a wonderful revelation to me to see the great power which physicists then acquired from using the noncommutative algebra which Heisenberg had been led to, or rather forced to adopt... It was found to be possible to develop this new mechanics in a way which was formally very close to the old classical mechanics

(75) For details on the history of the development of operator conception see [Jammer 1966, 221], and particularly [Mehra and Rechenberg 1982, Vol. 3, 220-246].

(76) In developing his ideas Dirac heavily relied on H. F. Baker's *Principles of Geometry*, following the analogy to geometry in detail, according to the idea that quantum variables might be thought of as symbols similar to the ones used in geometry [Mehra and Rechenberg 1982, Vol. 4].

expressed in Hamiltonian form. In this development, it was the mathematics which really dominated the progress that was made. The correct understanding of the physical relationships brought in by the new mathematics did not become clear until a few years later. The equations themselves came first" [Dirac 1983, 41].

Dirac responded to matrix mechanics finding his own way, while Erwin Schrödinger's reaction was quite a negative one: "I felt discouraged if not repelled by what appeared to me a rather difficult method of trascendental algebra, defying any visualization [*Anschaulichkeit*]" [Schrödinger 1926b, footnote 2, 735]. Schrödinger's decisive step was a speculative jump of mathematical imagination. His work rested on the formal mathematical similarity between the theory of light rays and the theory of particle orbits discovered some 90 years earlier by William Rowan Hamilton. Schrödinger observed that the theory of ligth rays is a special limiting case of the theory of light waves that had been established later by Maxwell and Hertz. So Schrödinger argued: "Why should there not be a theory of particle waves having the same relation to particle orbits as light waves have to light rays?" [Dyson 1964]. A purely mathematical argument, the optical-mechanical analogy developed by Hamilton between 1828 and 1837, provided an appropriate formalism for expressing what was immediately called the quantum-wave mechanics, which he developed in four papers entitled "Quantisierung als Eigenwertproblem" (Quantization as an eigenvalue problem), where he presented the partial differential equation which is called after his name [Schrödinger 1926]. At the same time he was writing to Planck on February 26, 1926: "I do have the very boldest hopes that one would now succeed in constructing a harmonious quantum theory, free of all roughness... the beautiful classical methods provide automatically all the integralness that is necessary; this is no mysticism of integral numbers but just the same integral numbers, which we are used to since long as occurring in surface harmonics, Hermitian and Laguerre polynomials".

In his first paper Schrödinger, who had established a very close relation with Hermann Weyl since when he had moved to Zürich in 1921, thanks him for assistance and for indicating the way to solve the eigenvalue differential equation of the hydrogen atom [Schrödinger 1926a, footnote 1, 363]. It is clear that Born, Weyl and Hilbert regarded the reduction of atomic dynamics to an eigenvalue problem as highly important, and at first it was a common feeling that matrix mechanics was a typical product of Born's formal, theoretical mind. As years later George Uhlenbeck told Abraham Pais, "The Schrödinger theory came as a great relief. Now we did not any longer have to learn the strange mathematics of matrices". At least physicists were well accustomed to manipulating partial differential equations, so that Wigner remarked that "People began making calculations but it was rather foggy" [Pais 1986, 255]. Actually Pais underlines that "Indeed, until the spring of 1926 quantum mechanics, whether in its matrix or its wave formulation, was high mathematical technology of a new kind, manifestly important because of the answers it produced, but without clearly stated underlying physical principles in the context of quantum mechanics."

So, at last, according to Edward Condon's account [Condon 1962, 46](77)

> "Hilbert was having a great laugh on Born and Heisenberg and the Göttingen theoretical physicists because ... they were having, of course, the same kind of trouble that everybody else had in trying to solve problems and to manipulate and

(77) Condon went to Göttingen to work with Born in the second half of 1926, and remembered that Hilbert was lecturing on quantum theory that fall, although he was in very poor health.

> really do things with matrices. So they went to Hilbert for help, and Hilbert said the only times that he had ever anything to do with matrices was when they came up as a sort of by-product of the eigenvalues of the boundary-value problem of a differential equation. So if you look for the differential equation which has these matrices you can probably do more with that. They had thought it was a goofy idea and that Hilbert did not know what he was talking about. So he was having a lot of fun pointing out to them that they could have discovered Schrödinger's wave mechanics six months earlier if they had paid a little more attention to him."

Schrödinger proved that his theory —expounded in terms of differential equations of functions of the coordinates of particles—, and Heisenberg's —expounded in terms of infinite-dimensional matrices acting on infinite vectors whose components represented allowed values of an observable— were completely equivalent [Schrödinger 1926b]. Notwithstanding the formal equivalence Schrödinger remained convinced that his theory had the advantage of visualizability as a guide to tackle problems such as collisions, which would be difficult to tackle as long as one felt obliged on epistemological grounds to suppress intuition in atomic dynamics, and to operate only with such abstract concepts like transition probabilities, energy levels, etc. "...he hoped", remarked Pais, "that wave mechanics would turn out to be a branch of classical physics— a new branch, to be sure, yet as classical as the theory of vibrating strings or drums or balls" [Pais 1986, 255-256].

13. – "Playing with equations and seeing what they give"

Dirac's strong commitment to mathematics was certainly different from more intuitive "physical" approaches: one for all Fermi's entirely pragmatic approach: "Quantum Mechanics is acceptable *because* its predictions agree with experiment" [Telegdi 1993, 82-83]. The unfamiliarity with matrix mechanics conceptual background seems also at the origin of his reluctance of accepting the unaccustomed mathematics involved, as Emilio Segrè has reported: "Heisenberg's great paper on matrix mechanics of 1925 did not appear sufficiently clear to Fermi, who reached a full understanding of quantum mechanics only later through Schrödinger's wave mechanics. I want to emphasize that this attitude of Fermi was certainly not due to the mathematical difficulties and novelty of matrix algebra; for him, such difficulties were minor obstacles; it was rather the physical ideas underlying these papers which were alien to him" [Segrè 1962, LV], and according to Valentine Telegdi "He was completely self-taught in quantum mechanics, an outsider to the Göttingen-Zürich-Copenhagen spirit circle of its founders. It may be supposed that Fermi always needed to draw a firm line between physics and 'philosophy'" [Telegdi 1993, 83].

It seems that the resistance to adopting new and unfamiliar ideas had also a strong root in mathematical conservatism, which, according to Dirac's extreme views, could be generally cured in the following way: "The most powerful method of advance that can be suggested at present is to employ all the resources of pure mathematics in attempts to perfect and generalize the mathematical formalism that forms the existing basis of theoretical physics, and *after* each success in this direction, to try to interpret the new mathematical features in terms of physical entities" [Dirac 1931]. One cannot help recalling Weyl's methods based on *a priori* mathematical conjecture, according to which he had identified the vector and tensor structures of his purely infinitesimal world geometry with those of gravitation and electromagnetism.

Dirac rediscovered Born's formula for the canonical transformation, upon which "transformation to principal axes, and "perturbation theory" were based.

The Principal Axes Transformation, and diagonalization of the matrix **H(p,q)** were the central ideas of the *Dreimännerarbeit*(78): if any pair of values $\mathbf{p}_0$, $\mathbf{q}_0$ be given which satisfy canonical equations "...then the problem of integrating the canonical equations for an energy function **H(pq)** can be reduced to the following: A function **S** is to be determined, such that when $\mathbf{p} = \mathbf{S}\mathbf{p}_0\mathbf{S}^{-1}$, $\mathbf{q} = \mathbf{S}\mathbf{q}_0\mathbf{S}^{-1}$ the function $\mathbf{H}(\mathbf{pq}) = \mathbf{S}\mathbf{H}(\mathbf{p}_0\mathbf{q}_0)\mathbf{S}^{-1} = \mathbf{W}$ becomes a diagonal matrix" [Born *et al.* 1926]. Solving of the quantum problems (the problem of integrating the canonical equations for the given **H(p,q)**) was thus reduced to determining a matrix **S** which diagonalizes the Hamiltonian(79). The calculation of energy values was thus reduced to the solution of the eigenvalue problem of Hermitian matrices in linear algebra. The problem, as the authors pointed out, was analogous to the Hamilton-Jacobi differential equation in classical physics and the matrix **S** corresponds to the action function.

The origin and development of this mathematical apparatus could be traced back to the the study of celestial mechanics; as Jammer has observed, "Ironically, matrix mechanics the outcome of Heisenberg's categorical rejection of orbits, had eventually to resort to the mathematics of orbital motion" [Jammer 1966, 215](80). The very name of the "secular" equation $|\mathbf{H} - \lambda\mathbf{I}| = 0$, whose roots are the eigenvalues of **W**, testify the ancient connection with astronomical problems. The study of the transformation to principal axes engaged the attention also of geometers and algebraists. The generalization to infinitely many variables of orthogonal transformations of quadratic forms culminated with Hilbert's recognition that the eigenvalues of quadratic forms and eigenvalues occurring in boundary problems of differential and integral equations have a common origin. In 1901 Hilbert's attention had been attracted by the theory of integral equations developed by the Swedish mathematician Ivar Fredholm, who had been inspired by the far-reaching work of the Italian Vito Volterra(81). During the period 1904-1910 Hilbert systematically developed a general theory of principal-axes transformations or orthogonal transformations of infinite bilinear forms riducing the problem of solving linear integral equations or partial differential equations to a problem of the theory of invariants. The "geometric" function-space approach was gradually developed in the work of his students, among which Erhardt Schmidt who gave a further move towards abstraction in 1908 introducing geometrical language into Hilbert's algebraic formulation, which was a generalization of the ideas of Hamilton, Grassmann and Peano. In 1908 Schmidt formulated what later was called the theory of "Hilbert spaces". The definition of a linear space (or vector space) became widely known in mathematics only

(78) Born was not only familiar with matrix calculus, which he had learnt from Rosanes at Breslau, but also with Hilbert's theory of integral equations and with pricipal-axes transformations of infinite quadratic forms.
(79) This formulation encountered the difficult problem of calculating the reciprocal matrix $\mathbf{S}^{-1}$. Only in the simple case of the infinitesimal canonical transformation, where $\mathbf{S} = \mathbf{1} + \lambda\mathbf{S}_1 + \lambda^2\mathbf{S}_2 + \dots$, could one easily obtain $\mathbf{S}^{-1} = \mathbf{1} - \lambda\mathbf{S}_1 + \lambda^2(\mathbf{S}_1^2 - \mathbf{S}_2) + \lambda^3 \dots$ and use the transformation.
(80) Jammer also gives a brief survey of the connection between astronomical problems and transformation of bilinear forms of n variables to sums of squares (transformation to principal axes) studied principally by Lagrange, Laplace and Cauchy and extended to Hermitian forms in 1855 by Charles Hermite.
(81) Fredholm showed that an integral equation was equivalent to a system of n linear equations in the limit of arbitrarily large n and recognized that the theory of infinite systems of linear equations could be used to solve the integral equation provided the determinant of the system and all its subdeterminants assumed finite values in the limit of infinite n.

around 1920, when Hermann Weyl and others published formal definitions([82]).

In his famous work on the foundations of a general theory of integral equations Hilbert showed that if subjected to an orthogonal transformation to principal axes, these forms give rise to an eigenvalue spectrum which contains, in addition to discrete values, or a "point spectrum", also a continuum of terms or a "line spectrum" [Hilbert 1904, 1905, 1906]([83]). So Born perfectly knew that the problem of diagonalization of a Hermitian form is equivalent to an "eigenvalue problem", however, as van der Waerden underlines "he did not realize that the eigenvectors determine stationary states of the atom; he used them only as a mathematical aid in performing the transformation to principal axes. The physical significance of the eigenvectors was not made clear before Schroedinger" [van der Waerden 1967, 52]. Hilbert on his side was enthusiastic about the fact that Schrödinger equation was a very simple example of an eigenvalue problem showing on the one hand, discrete eigenvalues, and on the other hand, a continuous interval of eigenvalues: "Hilbert himself had known from his fundamental researches that such occurrences must exist, but no mathematician had been able to find a simple example of this kind. Only Nature itself, as studied by the quantum physicists, held in store simple examples of this kind, at that time not yet detected by mathematical fantasy" [Jordan 1973, 298].

In the summer of 1930 Dirac published *The Principles of Quantum Mechanics*, a book which clearly expresses Dirac's peculiar abstract approach to physics. It was based on what he called "the symbolic method", which "deals directly in an abstract way with the quantities of fundamental importance (the invariants, & c., of the transformations)". This method, claimed Dirac, "seems to go more deeply into the nature of things". In accordance with this program he presented the general theory of quantum mechanics in a way that was free from physical interpretation: "One does not anywhere specify the exact nature of the symbols employed; nor is such specification at all necessary. They are used all the time in an abstract way; the algebraic axioms that they satisfy and the connexion between equations involving them and physical conditions being all that is required" [Dirac 1930, 18]. In reviewing it on *Physical Review* Oppenheimer warned that even being clear, its clarity was to be considered "dangerous for a beginner, deductive, and in its

([82]) In the first chapter of his *Raum, Zeit, Materie* Hermann Weyl gives an axiomatic definition of linear vector-manifolds which is quite similar to the modern definition of vector-spaces, referring to Grassmann's *Ausdehnungslehre* as an "epoch-making work".

([83]) A theory of bounded quadratic forms of infinitely many variables was also constructed independently by E. Hellinger in 1909. He showed that the bilinear Hermitian form $\sum H_{mn}x_m x_n^*$ can be transformed to the expression $\sum W_n y_n y_n^* + \int W(\phi)y(\phi)y^*(\phi)\mathrm{d}\phi$, where the discrete real numbers W_n (which Hilbert had called the "reciprocal eigenvalues") form the point spectrum and the real-valued functions $W(\phi)$ of the continuous parameter ϕ the line spectrum [Jammer 1966, 218]. Hellinger, one year Born's junior, had been recommended by Born to Hilbert as his successor as private assistant but Hilbert insisted for having Courant "since those fellows from Breslau were so useful". Later Hellinger and Otto Toeplitz wrote together the review article on integral equations and systems of linear equations for the *Encyklopädie der mathematischen Wissenschaften.* In the *Dreimännerarbeit* Born quotes Hilbert's work on *Integralgleichungen* and gives an account of Hellinger's theory assuming this result to be valid also for systems which are not bounded. Quantum mechanical matrices are in general not of finite order, nor are even finite bounded, but at the moment they assumed that the procedure remained valid also for infinite matrices. Later von Neumann succeeded in proving that this confidence was justified. The spectral resolution of self-adjoint operators was one of von Neumann's most brilliant mathematical achievements. Von Neumann told van der Waerden that "he had struggled with the problem for a long time" [van der Waerden 1967, 51].

foundations abstract; its argument is predominantly analytical; the virtual contact with experiment is made quite late in the book...physical ideas are seldom used to advance the argument, and occur chiefly as an aid to exposition. The book remains a difficult book, and one suited only to those who come to it with some familiarity with the theory". Coming to what he defined "Dirac's quasi-postulate-theory foundation" of quantum mechanics, Oppenheimer observed that "Dirac's axioms... have an extreme generality; but because the meaning of any equation, in physics, is ultimately to be derived from the numbers given by specific observations, they have also a higher grade of abstractness. This is the price, concluded Oppenheimer, that must be paid for the generality. And Dirac's book will be for many of us so satisfying, that we shall be glad to pay it" [Oppenheimer 1931].

Pauli, too, reviewed it on *Die Naturwissenschaften* [Pauli 1931], and in recommending it he also pointed out that Dirac's symbolic method might lead to "a certain danger that the theory will escape from reality". In saying this he was referring to the problem of measurement, a process which requires devices following the laws of classical physics, not merely involving mathematical formulae, a conception which Pauli shared with Bohr, who, according to Heisenberg "feared... that the formal mathematical structure would obscure the physical core of the problem, and in any case, he was convinced that a complete physical explanation should absolutely precede the mathematical formulation" [Kragh 1990, 79].

Dirac's attitude was on the opposite side: he distrusted all physical concepts as the basis for a theory, and put his trust in a mathematical scheme, even if the scheme did not appear at first sight to be connected with physics. He concentrated on getting interesting mathematics: "There are occasions when mathematical beauty should take priority over agreement with experiment". [Mehra 2001d, 703]. Dirac was even more radical when he said: "A great deal of my work is just playing with equations and seeing what they give" ([84]). Probably Minkowski, Dirac or Weyl or Einstein had a different opinion of what is beautiful mathematics, but certainly they had a common belief in the creative power of mathematical reasoning: "Experience remains, of course, the sole criterion of physical utility of a mathematical construction. But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed" [Einstein 1973, 267].

In spite of Dirac's attitude about the interplay between mathematics and physics, "Not every physicist loved Dirac's work. Some searched it in vain for a mathematical basis. Other complained that it lacked the plain, handsome rigor of fine scientific writing. One struggled to follow Dirac's work...", recalled Wigner, "So at first Dirac's work was less admired than Heisenberg's... Some people even whispered that he evaded the hard work or physics by clever tricks" [Mehra and Wightman 2001, Vol. 7, 31]. In fact the language barrier —German was at that time the language of physics— combined with "Dirac's novel viewpoint" made his papers even hard to read.

In the course of the semester 1926-1927 von Neumann, who was working on the spectral theory of self-adjoint operators, attended lectures by Hilbert, who had recovered from a severe illness of the previous year and had taken up once again his early interest in the new quantum mechanics. Hilbert and his assistants, Lothar Nordheim and von Neumann, provided a clear axiomatic formulation of the transformation theory [Hilbert *et al.* 1927]. In that same period von Neumann, like Weyl particularly concerned with

([84]) Interview with P. A. M. Dirac by T. Kuhn (May 7 and 14, 1963, Niels Bohr Library, American Institute of Physics).

foundational questions, published his interpretation of quantum mechanics, where he combined his insistence on mathematical rigour with a liking for mathematical generalization; his systematic approach put order in the semi-coherent assemblage of principles and rules for application characterizing theory by that time [von Neumann 1927].

Later von Neumann put quantum theory on a firm theoretical basis into the setting of operator algebra. Among the mathematicians he shared with Weyl the extraordinary ability of getting into a new subject and bringing important contribution to it within a few months. In the years 1926-1932 von Neumann worked out the Hilbert space formalism of quantum mechanics in a systematic and mathematically precise way, and summed it up in his *Mathematische Grundlagen der Quantenmechanik* [von Neumann 1932], where he translated Hilbert's geometrical ideas in a more analytical and formalistic language, particularly suited to quantum mechanical operations. In the introduction, after praising Dirac's exposition of quantum mechanics "hardly insuperable for its conciseness and elegance", von Neumann observes that "mathematical rigour is definitely lacking, even if it is reduced to the minimum generally requested by theoretical physics... it is not a problem of simply changing the mathematical tools basing on the same physical theory... we shall have to turn to a completely different theory from the very beginning, that is Hilbert's spectral theory". It was this abstract axiomatic foundation which was at the basis of the name "Hilbert space". Physicists learned from him to see in the mathematical conception of the Hilbert space the natural background of quantum mechanics.

The mathematical description of a state by a vector in "Hilbert space" determining probabilities for the results of any kind of experiments made a very big change towards a much more abstract definition than the original electronic orbit. In 1973 Heisenberg recalled the "unwillingness of Einstein, Planck, von Laue and Schrödinger" to give up this kind of objective description of natural phenomena:

> ... "it was indeed extremely difficult to give up this notion because all our language is bound up with this concept of objectivity. So all the words which we use in physics in describing this experiments, like the words measurement of position or energy or temperature and so on, are based on classical physics and its idea of objectivity. The statement that such an objective description is not possible in the world of the atoms, that we can only define a state by a direction in Hilbert space —such a statement was indeed very revolutionary; I think it is really not so strange that many physicists of that time simply were not willing to accept it" [Heisenberg 1973a, 270].

The problem of relationships between mathematics and physics, and between mathematicians and physicists, which had been at the core of debates about relativity theory and the new issues stemming from it, was now again on stage. The building of new quantum mechanics aroused heated and lively discussions about the construction of physical theories, and not only among physicists: mathematicians such as Bartel van der Waerden and John von Neumann got particularly involved also in the conceptual development of the new quantum mechanics; their mathematical rigour was certainly considered repulsive by many physicists, or worse uninteresting, but certainly they had an imporant role in helping with the problems of the mathematical foundations of the theory.

Heisenberg, in particular, remembered how discussions about axiomatic foundations in mathematics in Göttingen had someway influenced him in accustoming to a completely new scheme of thinking, differing from classical mechanics, but nevertheless a consistent one:

> "I remember that in being together with young mathematicians and listening to Hilbert's lectures, I heard about the difficulties of mathematics. There it came

> up for the first time that one could have axioms for a logic that was different from classical logic and still was consistent. That, I think, was just the essential step [in physics]. You could think just in this abstract manner of mathematicians, and you could think about a scheme which was different from the other logic, and still you could be convinced that you would always get consistent results... It was a consequence of this whole development —the theory of relativity, the axiomatization of mathematics, etc.— which gave the possibility of distinguishing between paradox and inconsistency. At that time it was rather new and not obvious to everybody. It just came out of all these discussions. I could not say that there was a definite moment at which I realized that one needed a consistent scheme which, however, might be different from the axiomatics of Newtonian physics. It was not as simple as that. Only gradually did the idea develop in the minds of many physicists that we can scarcely describe nature without having something consistent, but we may be forced to describe nature by means of an axiomatic system which was thoroughly different from the old classical physics and even using a logical system which was different from the old one" [Mehra and Rechenberg 1982, Vol. 4, 230].

The acceptance of the new theory became coupled with the question of whether one grasped the mathematical tools or not. People feared that "if atomic physics has to progress on the line of Born and Jordan, you will find very few people left in the atomic physics circle." [Mehra and Rechenberg 1982, Vol. 4, 233].

14. – The new role of symmetries in quantum mechanics

Heisenberg, whose physical intuition became soon perfectly combined with formal virtuosity, was indeed convinced that the intrinsic properties of subatomic particles could be revealed through the mathematics of the quantum theory. After a year from his first paper he tried "to give the foundation for the quantum-mechanical treatment of the many-body problem" in his "Mehrkörperproblem und Resonanz in der Quantenmechanik" [Heisenberg 1926], submitted on June 11, 1926, and published in *Zeitschrift für Physik*, issue of 10 August 1926. It was a watershed event in introducing the notion of intrinsic symmetry into quantum mechanics, stressing for the first time the implication of the exclusion principle for identical particles, and thus setting the stage for the helium atom calculation. Heisenberg treated the problem of two identical oscillators symmetrically coupled to each other with the matrix method and next with Schrödinger's method, finding identical results in both cases. He found "a decisive result": each term of the uncoupled systems splits under the influence of the coupling into two sets of states "which cannot be combined in any way" ("Dieses Termspektrum läßt sich wieder —und dies ist das entscheidende Resultat— einteilen in zwei Termreihen, die in keiner Weise miteinander kombinieren können" [Heisenberg 1926, 418]; he associated the set of higher energy levels with a wave function that is symmetric under exchange of the spatial coordinates of the two systems, and the lower set with an antisymmetric wave function. That these two sets of states were not connected then followed from the symmetry of the Hamiltonian of the system under a particle permutation. It was a decisive step towards establishing a relation between the symmetry of n-particle wave functions and statistics. Heisenberg incorrectly identified Bose-Einstein counting with Pauli's exclusion principle. By that time the difference between the two types of statistics was not completely clear even if Fermi in Spring had already published on the *Zeitschrift für Physik* his paper on the theory of an ideal gas of particles obeying Pauli's exclusion principle [Fermi 1926]. This work did not seem to produce any immediate effect on the international scientific community, the reason was probably that his formulation was also based on procedures from the old quantum physics, which were apparently distant from people's more recent interests.

In August 1926 the problem was stated correctly by Dirac, who set the two forms of statistics in their appropriate theoretical context: "The solution with symmetric eigenfunctions must be the correct one when applied to light-quanta, since it is known that the Einstein-Bose statistical mechanics leads to Planck's law for blackbody radiation. The solution with antisymmetrical eigenfunctions is... the correct one for electrons in an atom" [Dirac 1926a]. As Dirac commented later "Schrödinger's theory did provide new insights", and the main new development comes in fact from applying the theory to two or more particles of a similar nature: "It then becomes possible to think of a Schrödinger wave function which is symmetrical between the two particles, or, alternatively, a wave function which is antisymmetrical. These symmetry ideas, added Dirac, I doubt that one would ever have thought of if one had just kept to the Heisenberg picture... I soon saw the value of this development of symmetry/antisymmetry relations and studied them" [Dirac 1983, 44]. In fact, both investigations of Heisenberg and Dirac on the quantum theory of assemblies of identical particles, had established the self-consistent framework of quantum statistics. The quantum particles loss of individuality was considered to be "something much more fundamental than the space-time concept", as was later stressed by Pauli [Von Meyenn 1983, 339].

Physicist were well accustomed to symmetries from "old classical physics", but now they had been forced to recognize in the permutation symmetry of states of n identical particles the first example of new symmetries which quantum mechanics had in store for them. These symmetries hold the key to the exclusion principle and to quantum statistics. On June 8, 1926, Heisenberg, who regarded matrix mechanics as more fundamental than wave mechanics, had written to Pauli from Kopenhagen: "Übrigens noch eine inoffizielle Bemerkung über Physik: Je mehr ich über den physikalischen Teil der Schrödingerschen Theorie nachdenke, desto abscheulicher finde ich ihn." (By the way, one more unofficial comment on physics: The more I ponder about the physical part of Schrödinger's theory, the more disgusting it appears to me.) He did not agree at all with Schrödinger's claim, about the absolute square of the wave function describing the charge distribution of the electron in space ("Was Schrödinger über die Anschaulichkeit seiner Theorie schreibt... ich finde es Mist") —which was later replaced by Born's probabilistic interpretation—, still, in the same period he was the first to resolve successfully the problem of calculating the helium spectrum applying the abhorred wave mechanics for calculational purposes only([85]). In fact Schrödinger's theory offered an easy access to many-electron systems, so "Warum soll man nicht auch einmal 'Dampfwalze fahren'" (Why should one not once in a while "use the steamroller?"), as Heisenberg wrote to Pauli in the same letter, announcing his paper on two-electron systems "Über die Spektra von Atomssystemen mit zwei Elektronen" [Heisenberg 1926a]. In examining the concrete example of the two electrons in a helium atoms Heisenberg concluded that only those states whose eigenfunctions are anti-symmetric in their electron coordinates can arise in nature. This important advance could not but generate in many physicists the feeling that there must be something "physically" more profound in Schrödinger's approach than in the matrix approach. The separation of helium spectrum in ortho- and parastates was one of the

([85]) Interview with W. Heisenberg by T. S. Kuhn (February 25, 1963, Niels Bohr Library, Archive for the History of Quantum Physics).

earliest examples of how symmetry operations in quantum mechanics are implemented by linear operations in Hilbert spaces. Permutation symmetry of states of n identical particles holds the key to the exclusion principle and to quantum statistics: restricting the wave function to be antisymmetrical between the various particles is exactly what is needed for the electrons in order to get Pauli's exclusion principle. But indeed a more deep understanding of the intrinsic meaning of this and other kinds of symmetry requirements was on the verge to come from an unespected part of mathematics.

15. – Group theory and Quantum Mechanics

The quantum-mechanical treatment of the many-body problems attracted the attention of the Hungarian Jenö Pál Wigner, who had completed his degree in chemical engineering in 1924 in Berlin, but was fond of theoretical physics:

> "In '26 I officially became a physicist. Even when I was a chemical engineer, I subscribed to the German journal of physics, *Zeitschrift für Physik*. I read in it an article by M. Born and P. Jordan who contributed very significantly to the foundation of quantum mechanics. Reading their article overwhelmed me—I realized that physics would change tremendously. Until then, physics was largely overwhelmed by the belief that man is not bright enough to understand microscopic phenomena" ([86]).

Wigner knew matrices from the work on the crystal structure of rhombic sulphur for his *Diplomarbeit* at the Technische Hochschule in Berlin, so he could perfectly follow the article on matrix mechanics. After that he received an offer of a research assistantship from Richard Becker who had been newly appointed to a chair in theoretical physics at the University of Berlin. Becker arranged for Wigner to work also with Karl Weissenberg, an X-ray crystallographer at the Technische Hochschule. Since Wigner had a fine command in mathematics, his supervisor Weissenberg frequently posed him problems which had mathematical roots: "...it's a miracle that in a crystal the atoms are often arranged along the axis of plane of symmetry. Why?" [Mehra and Wightman 2001, Vol. 1, 5]. Wigner had used the classical space groups already in his chemical engineering days, and trying to produce a group-theoretical proof, he went back to his study of group theory, in particular to the classic *Lehrbuch der Algebra* by Heinrich Weber which contained elements of group theory [Mehra 2001c and 1997; Seitz *et al.* 1995].

In Heisenberg's paper on the helium spectrum the first quantum-mechanical application of the Pauli principle was given: two-electron wave functions are antisymmetric for simultaneous exchange of space and spin coordinates. Wigner took up the problem of the symmetry of atomic eigenfunctions under permutation of electrons, and on November 12, 1926, he submitted a paper to *Zeitschrift für Physik* where he extended Heisenberg's results calculating the wave functions and energies of the non-combining term systems constructed from 3 electrons [Wigner 1926]. In his paper Heisenberg had conjectured that non-combining sets should likewise exist if the number of n of identical particles is larger than two, but had not found a proof. Now Wigner had been able to handle the case $n = 3$ quite completely by elementary methods (however, he had to solve a reducible equation of degree six), and ended his article promising a second paper generalizing the treatment

([86]) Interview with E. P. Wigner by W. Aspray ("The Princeton Mathematics Community in the 1930s", 1985, transcript number 44, PMC44).

to many electrons. But he soon realized that this method would be prohibitively complicated for atoms with more than three electrons. "I realized that this was connected with group theory, because I knew group theory from much earlier. When I finished that article I realized that the result could be extended to more than three particles. I talked to Johnny von Neumann about it. This was at Berlin. He knew which article to give me to read: one by Frobenius and Schur. It had an enormous effect on me"([87]).

Wigner already having an expertise in matrices from Weber's text, found Schur's papers on the irreducible representations of symmetry groups easy to understand. In the first lines of his second paper on the case of n electrons, submitted to the *Zeitschrift für Physik* on 26 November 1926, "Über nicht kombinierende Terme in der neueren Quantentheorie. Zweiter Teil", he remarked: "Es ist klar, daß man diese elementaren Methoden schon bei vier Elektronen kaum anwenden kann, da die rechnerischen Schwierigkeiten zu groß werden. Es existiert aber eine wohl ausgebildete mathematische Theorie, die man hier verwenden kann... die am Ende des vorigen Jahrhunderts von Frobenius begründet und später unter anderem von W. Burnside und J. Schur ausgebildet worden ist". (It is clear that this elementary methods can be barely applied to the case of four electrons, as the computational difficulties get too hard. There exists a well-developed mathematical theory which one can use here: the theory of transformation groups which are isomorphic with the symmetric group [the group of permutations]... which at the end of the last century was built by Frobenius and later was developed by W. Burnside and J. Schur.)

In general, if an arbitrary physical system is given, for which we know a symmetry group G, in order to exploit this information another concept is needed, that of a (linear) *group representation*. A representation of the group (or Lie group or Lie algebra, depending on the type of symmetries considered) is the appropriate mathematical tool for achieving the goal of letting a given symmetry transformation act on other objects (*e.g.* the wave function describing an electron in quantum mechanics). An n-dimensional representation D of a group G is defined as follows : to each element $g \in \mathrm{G}$, one associates an $n \times n$-matrix $D(g)$ (*i.e.* a linear operator on an n-dimensional vector space V) in such a way that the operators $D(g) : g \in G$ obey essentially the same algebraic ralations as the group elements themselves, and the group structure is preserved: $D(gg') = D(g)D(g')$, $D(g^{-1}) = [D(g)]^{-1}$, $D(e) = 1$ (e is the neutral element of G, 1 is the identity operator). We say in this case that the map $D : g \to D(g)$ is an n-dimensional (linear) representation of G. If the correspondence $g \to D(g)$ is one-to-one, the set of all representatives $D(g)$ forms a group which is isomorphic to the original group G.

These representations were already familiar to physicists since they are the well-known transformation formulae for vectors, tensors, etc. The transformation matrices of vector or tensor components form a representation of the rotation group in the following terms: to each rotation there corresponds a linear unitary operator, and this correspondence is the representation of the rotation group obeying a composition law structurally identical

([87]) Interview with E. P. Wigner by W. Aspray (1985, "The Princeton Mathematics Community in the 1930s", transcript number 44, PMC44).Wigner and von Neumann had been close friends since when they were young students in Budapest, where the young Wigner had shown exceptional if not rare abilities in mathematics, and von Neumann soon was recognized to be a mathematical genius. Both their parents had insisted that they attend the Technische Hochschule in Berlin and focus on chemical engineering, so that they might be in a better position to earn a living in Hungary. In Berlin Wigner met Leo Szilard, whom he often referred to as "the general", since Szilard enjoyed making decisions, and renewed a friendship with Edward Teller, whom he had known since school times in Budapest, and who was then working with Heisenberg in Leipzig.

to the composition of rotations. Thus, the transformation matrices for vectors are the rotation matrices themselves, and they form a representation of their own group.

Quantum mechanics offers a particularly favourable framework for the application of symmetry principles. The states of quantum-mechanical systems are represented by vectors in an infinite-dimensional vector space, and observables are linear mappings of these states onto one another. As such, quantum states can be added to one another into subspaces invariant under the action of the associated transformations. The operations relating the states to each other, are generally the operators acting on the state space that correspond to the physical observables, and any state of a physical system can be described as a superposition of states of elementary systems, that is, of systems the states of which transform according to the "irreducible" representations of the symmetry group. Once the classification of groups and of their representations has been done independently of the physical situation to which the groups may be applied, the immense power of group theory in physics derives from the fact that the laws of quantum mechanics decree that whenever a physical object has a symmetry, there is a well-defined group of operations that preserve the symmetry, and the possible quantum states of the object transform into each other according to some "representation" of the symmetry group. The observables representing the action of the symmetries in the state space, and therefore commuting with the Hamiltonian of the system, play the role of the conserved quantities, which in fact generate the associated symmetry transformations. If the Hamiltonian of a quantum system is unchanged by some group of symmetries G, then the eigenspaces of H span representation spaces for G; furthermore, the eigenvalue spectra of the invariants of the symmetry group provide the labels for classifying the irreducible representations of the group, so that the values of the invariant properties characterizing physical systems can be associated with the labels of the irreducible representations of symmetry groups. Atomic states can then be characterized by irreducible representations of the various fundamental symmetry groups.

The investigation of symmetries allows also for an interpretation of the degeneracies which generally occur in the energy spectrum of quantum systems. By that time this was a well-established inherited concept since classical mechanics, whereas a great novelty came from dealing with new features typical of atomic systems: a system of identical particles is unchanged by permutation of the coordinates of its particles. It was the first non-spatiotemporal symmetry to be introduced into the realm of microphysics. This brand new problem triggered Wigner's interest, and he began his study of symmetry in quantum mechanics with the problem of classifying the behavior of eigenfunctions of the Schrödinger Hamiltonians for atoms under permutations of the electrons. The analysis of quantum statistics in terms of the permutation of indistinguishable particles had thrown light on the connection between the two forms of statistics and the symmetry characteristics of the relevant states of the particle systems. Thus, the physical assumptions of quantum mechanics entail that any state of a system of identical particles belongs to a one-dimensional representation of the symmetric group, or permutations of the n particles.

So far the very basis for the applicability of group theory appeared to lie in a new symmetry property, the non-classical indistinguishability of quantum particles. As von Neumann had suggested, the representation theory of the symmetric group due to Frobenius and Burnside was exactly what Wigner needed to develop a theory of the spectrum of atoms with n electrons.

Thanks to his earlier research in crystal physics Wigner realized the importance of group-theoretical methods for accomplishing more than just calculations of wave functions and energies —namely, for drawing out and then expressing the intrinsic symmetries of atomic systems according to which quantum particles are divided into two exclusive categories, bosons and fermions, and so only two possibilities arise: the identity representation and the antisymmetric representation. Wigner recognized that the separation of the states of the system into sets of dynamically non-combining states, was thus connected to the possibility of reducting the state space of a system of indistinguishable particles into invariant sub-spaces.

In the meantime, almost simultaneously, Heisenberg, too, realized that he could generalize his earlier results to systems of many electrons introducing methods of group theory into quantum mechanics. For two electrons the consequences of the correspondence between the ortho-para separation of the orbitals and the triplet-singlet separation of the spin states are still trivial. They cease to be so in the case of three or more particles, when parallel spins are unavoidable. Heisenberg noted that the theory of permutation groups could be used for constructing the wave functions for the noncombining terms systems that occur in an ensemble of n-interacting electrons, and calculated the non-combining terms systems and their energies for the three-electron system. The second part of his paper on many-body problems was submitted on December 22, 1926 [Heisenberg 1926c]. He only exploited the identity of electrons and the total antisymmetry of the system's wave function descending from it, and realized that "Vielleicht ist das hier versuchte Schema noch nicht genügend allgemein" (possibly the method investigated here is not yet sufficiently general). And indeed later he recalled how "There was first the helium problem where one saw the two term systems. And then I tried to do a similar thing for molecules with three equal nuclei. There I realized that I had to do with a new group property. But I couldn't do it well, and then the paper of Wigner appeared..."([88]).

The energy levels of an isolated atom do not depend on the direction from which the atom is observed, and this has the consequence that the Hamiltonian is invariant with respect to the rotation group in 3-dimensional space. Recent papers of Hermann Weyl and of Schur on continuous groups had enabled Wigner to enlarge his study to the consideration of the action on eigenfunctions of rotations R of the coordinates of n electrons. In applying the analysis of the representations of the three-dimensional rotation group to atomic spectra, Wigner recognized that the group of rotations in "actual" space induces a group of transformations in Hilbert space under which the latter decomposes into invariant subspaces. In the absence of external fields, permutations of the electrons and coordinate rotations in 3-dimensional space are symmetry groups of the Hamiltonian. If the Hamiltonian commutes with the action on wave functions of permutations of coordinates or with the action on wave functions of rotations of coordinates, then the linear subspace spanned by the eigenfunctions of a fixed eigenvalue is left invariant by these actions and, in the subspace, yields a unitary representation of the permutation and the rotation group.

The theory of group representation, Wigner remarked, could lead to exact results even for more complicated systems than the hydrogen atom. The decomposition of Hilbert space into irreducible subspaces with respect to a particular group corresponds to the separation of the various values which are possible for a physical quantity. The eigenfunctions of the Hamiltonian can be classified in terms of the unitary irreducible representations obtained combining the rotational and permutational symmetries of the Hamiltonian. The irreducible representations of such groups can be used to label the eigenstates, as Wigner

([88]) Interview with W. Heisenberg by T. S. Kuhn (February 19, 1963, Niels Bohr Library, Archive for the History of Quantum Physics).

recognized in the first sentence of a long paper published a few months later where he gave a systematic account of the theory of group representations [Wigner 1927]([89]), as well as references to Speiser's book *Theorie der Gruppen von endlicher Ordnung* (Berlin, 1923)([90]), to Weyl's *Zur Theorie der Darstellung der einfachen kontinuierlichen Gruppen* (Berlin, 1924), and obviously to Schur's papers, like "Neue Begründung der Theorie der Gruppencharaktere", which was certainly a primary reference. He also thanked von Neumann for having suggested those readings ("Für den Hinweis auf diese Arbeiten"). A detailed treatment followed of the extent to which the theory of the energy levels of an atom may be understood by applying this theory, even if assuming the simplified model in which the recently discovered "spin" of the electron was neglected.

Years later Heisenberg's comment was:

> "I could see that Wigner had, by his method, solved certain problems which I could not properly solve... in the Wigner paper I could see that all was much better mathematics and that it was clear and my paper was not clear at all. So it was definitely an improvement. Still, it did involve many new mathematical techniques, which so far no physicists had ever known. So we had to sit and learn group theory and read Schur's papers and that kind of thing. Everybody was a bit exhausted and felt "Well, it's just awful that we have to learn all that stuff... So actually, I took the book of Schur and studied it very carefully"([91]).

In Wigner's paper the classification of atomic states in terms of irreducible representations of the symmetries of the Hamiltonian contained also the inversion operation, which became associated with the quantum number "parity"; by that time nobody vaguely suspected the possibility that this symmetry could be violated in nature([92]).

([89]) For an accout on Wigner's scientific papers applying group theory between 1926-1935 see [Judd 2001], and [Mackey 2001]. If the atom is inserted in a crystal or forms a molecule, then the symmetry of the potential energy is reduced from spherical symmetry to the symmetry of the position that the atom occupies in the crystal or molecule. Wigner had indicated the way to deal with such a diminishing of the symmetry, but the actual working out of the details was left to H. Bethe. His very important paper [Bethe 1929] which introduced point group symmetry in the handling of the electronic structures of polyatomic molecules and ions was published in 1929, the year Bethe became 23 years of age.

([90]) The Swiss Andreas Speiser had written his Ph.D thesis under Hilbert's supervision in 1909 (*Zur Theorie der binären quadratischen Formen mit Koeffizienten und Unbestimmten in einem beliebigen Zahlkörper*).

([91]) Interview with Werner Heisenberg by Thomas S. Kuhn (July 5, 1963, Niels Bohr Library, Archive for the History of Quantum Physics).

([92]) In his seminal paper "On the conservation laws of quantum mechanics" [Wigner 1927a, 375] Wigner noted that these laws are associated with the existence of unitary operators P that commute with the Hamiltonian H. As a result states can be chosen such that P and H are simultaneously diagonal: P is conserved. Taking P to be the reflection operators, its eigenvalues, the parity quantum number, can be ± 1. Wigner remarked: "Only rarely will one be able to use [parity] since it has only two eigenvalues (± 1) and has therefore too little predictive power." In introducing for the second time a discrete symmetry which has no classical counterpart (the first was permutation symmetry for identical particles) Wigner also stressed that parity "has no analog in classical mechanics". Time reversal invariance appeared in the quantum context, again due to Wigner, in a 1932 paper [Wigner 1932].

16. – 1927: Group Theory becomes Practice

In 1927 Becker proposed that Wigner spend a period in Göttingen as an assistant to David Hilbert. One of the most remarkable and unique traits of the Göttingen school was the convinction that mathematicians could and should be engaged with theoretical physics. Sommerfeld had always arranged for a young physicist to help Hilbert keep abreast of current developments in physics, but Hilbert suffered from pernicious anemia and Wigner saw him only a few times. Nevertheless he got to know Max Born and Walter Heitler and came in close contact with Pascual Jordan, with whom he wrote a paper which dealt with a new technique for imposing the Pauli exclusion principle on an ensemble of electrons expressing it in the language of second quantization [Jordan and Wigner 1928]. Jordan had recently published with Oscar Klein a work extending to many-boson systems the work of Dirac on field quantization. Now the formalism was repeated for fermions using the Jordan-Wigner matrices, as in those years creation and annihilation operators for the case of Fermi-Dirac statistics were referred to: "...the mathematical proof that this was the only way to explain Pauli's exclusion principle was mine and it was based on the theory of group representations —but the principal idea was Jordan's" [Mehra 1997, 7]. The advantages of second quantization were appreciated only slowly. An important exception was Fermi's theory of β-decay (1933) in which the idea of "creating" an electron and a neutrino was very naturally adherent to a phenomenological description refusing the pre-existence of electrons in the nucleus. The idea of an analogy with the creation and annihilation of photon was in the air, but no one had put it in operational form. In fact Fermi was the first to use second quantized spin-1/2 fields in particle physics, even if we know from his collaborators, Rasetti and Amaldi, that at first Fermi did not like second quantization, which does not appear in his famous papers on quantum electrodynamics. He used it at the end of 1933, when he recognized that the formalism was a "useful" application to beta-decay theory, where particle creation was at the core of the process([93]).

As late as 1968 Slater remembered that he "read the papers on second quantization when they first came out, and decided at that time that they seemed to him like a more complicated method of treating matters which could be more easily taken up without their use" [Slater 1968, 69]. Dirac himself, who had been the first to apply the second quantization to bosons, did not find the method appealing when applied to a set of fermions [Dirac 1983]:

> "When I first heard about this work of Jordan and Wigner, I did not like it. The reason was that in the case of the bosons we had operators... that had classical analogues. In the case of the Jordan-Wigner operators, they had no classical analogues at all and were very strange from the classical point of view. The square of each of them was zero. I did not like that situation. But it was wrong of me

([93]) According to Amaldi, Fermi, at least at first, did not study the papers on quantum electrodynamics by Pauli and Heisenberg, nor was he very familiar at the time with the formalism of second quantization. In fact, in all his QED papers from 1929 to 1932 Fermi described matter as particles obeying either Schrödinger or Dirac equations rather than by quantized fields —à la Jordan-Klein, Jordan-Wigner or Heisenberg and Pauli. "Apparently he had some difficulty with the Dirac-Jordan-Klein method of the second quantization of fields, but eventually also mastered that technique and considered a beta-decay theory as a good exercise on the use of creation and destruction operators. He also made use of the isotopic spin formalism, recently invented by Heisenberg...", as Franco Rasetti recalled in his long introduction to the beta-decay papers in *Enrico Fermi Collected Papers* [Fermi 1962-1965]; see also [Amaldi 1982].

> not to like it, because, actually, the formalism for fermions was just as good as the formalism I had worked out for bosons. I had to adapt myself to a rather different way of thinking. It was not so important always to have classical analogues for everything... one can proceed to build up a reasonable quantum theory quite independently of whether or not there is a classical analogue".

In the first months of 1927 Dirac was in Göttingen, too, and followed the course on group theory given by Hermann Weyl. Obviously he was not frightened by abstract mathematics, but thought that its role should be strictly an operational one, restricted to physical applications:

> "Group theory is just a theory of certain quantities that do not satisfy the commutative law of multiplication, and should thus form a part of quantum mechanics, which is the general theory of all quantities that do not satisfy the commutative law of multiplication. It should therefore be possible to translate the methods and results of group theory into the language of quantum mechanics, and so obtain a treatment of the exchange phenomena which does not presuppose any knowledge of groups on the part of the reader" [Dirac 1929a, 716].

Like most physicists, Dirac undervalued the role of group theory, even if he himself had always boldly stated what should be "a rather general principle in the development of theoretical physics; namely, one should allow oneself to be led in the direction which the mathematics suggests". In affirming this, on the occasion of an International Symposium on the History of Particle Physics held at Fermilab in May 1980, Dirac was referring to the fact that only two kinds of particles, bosons and fermions, are known in nature, so that he had underlined how "The mathematics really push one into thinking of symmetrical and antisymmetrical states, and one must follow up this mathematical idea..." [Dirac 1983, 46]. This was exactly what Wigner had done: in connection with the symmetrical and antisymmetrical states he had considered other kinds of symmetry which the mathematics allows. "If one handles this question generally, one gets the idea of permutation operators" continued Dirac: in exploiting the identity of the electron symmetries and the concomitant total antisymmetry of the system's wave function Wigner had been led to the permutation group of the similar particles, and to the group properties of these operations. Thanks to von Neumann, he discovered that these could be tackled with the most sophisticated methods developed by mathematicians during the whole XIX and the beginning of the XX century, in the realm of what had become a well-established branch of mathematics, even if research was still in progress and its role more and more enlarging in importance.

The fundamental equations of quantum mechanics give rise to characteristic shapes and patterns which reflect the intrinsic symmetry of the atomic or molecular situation. Wigner had indeed shown how most of the empirical rules discovered by spectroscopists were independent of the details of the interactions between electrons but followed directly form the invariance with respect to the three-dimensional rotation group and to the permutation group. Physicists were forced to recognize that the properties of quantum states are to a large extent determined by their symmetries and by a set of quantum numbers determining these symmetries. Some of them enthusiastically thought that group theory could provide a tool to exploit these characteristic features, so that the group theory fashion inaugurated by Wigner and Heisenberg at the end of 1926, flourished in *Zeitschrift für Physik* all through 1927.

In late 1927 Walter Heitler had become Born's assistant at Göttingen and wrote to Fritz London saying how excited he was about the physics there, and about Born's course in quantum mechanics where everything was presented in the matrix formulation and then one derived "God knows how, Schrödinger's equation" [Gavroglu 1995, 54]. No understanding of the mechanisms and dynamics of chemical bond was indeed possible before the advent of Schrödinger's equation. Heitler and London's joint paper of June 1927 [Heitler and London 1927], immediately regarded as a classic work, had been the beginning of a new era in the study of chemical problems; quantum chemistry was born. It was a great contribution to the clarification of the chemist's conception of valence. Here was found the mathematical dynamic formulation of Lewis' covalent bond, the energy of the electron pair bond being given as a resonance energy due to the interexchange of two electrons [Ballhausen 1979].

The proposed exchange mechanism obliged them to answer questions posed by experimental physicists and chemists, like "What is really exchanged? Are the two electrons really exchanged?". In stressing the "non-visualizability" of the exchange mechanism, Heitler remembered [Gavroglu 1995, 48]:

> "It became gradually clear to me that it has to be taken as a fundamentally new phenomenon that has no proper analogy in older physics. But I think the only honest answer today is that the exchange is something typical for quantum mechanics, and should not be interpreted —or one should not try to interpret it— in terms of classical physics".

Immediately Linus Pauling and John van Vleck wrote two review articles placing great emphasis on the importance of spin for chemistry, and showing that the Pauli exclusion principle could provide a remarkably coherent explanation of the periodic table.

Quantum mechanics was essential also to understand why and how atoms combine. Heitler felt that the use of group theory was the only way to deal with the many-body problem and he wrote two long letters outlining his ideas about the meaning of the bond line used by chemists. He assumed that every bond line meant exchange of two electrons of opposite spin between two atoms. He examined the case with the nitrogen molecule and, by analogy with the hydrogen case, among all the possibilities, the term containing the outermost three electrons of each atom with spins in the same direction was picked out as signifying attraction. He felt that this could be proved in general only using group theory, and in particular the theory of representations: "Let us assume for the moment that the two atomic systems $\uparrow\uparrow\uparrow\uparrow\uparrow \ldots$ and $\downarrow\downarrow\downarrow\downarrow\downarrow \ldots$ are always attracted in a homopolar manner. We can, then eat Chemistry with a spoon". Wigner and Weyl were in Göttingen by that time and Wigner used to tease him, since he was sceptical that the whole of chemistry could be derived in such a way. Wigner would ask Heitler to tell him what chemical compounds between nitrogen and hydrogen his theory could predict, and "since he did not know any chemistry he couldn't tell me" [Gavroglu 1995, 54]. Both Heitler and London believed that chemistry must be within the reach of quantum mechanics. London agreed with Heitler that group theory might provide many clues for the generalization of the results derived by perturbation methods. While Heitler in Göttingen was studying group theory intensively, under the spell of Wigner's and Weyl's presence in Göttingen, London had some problems in familiarizing himself with group theory; as Heitler recalled later "He thought it was too complicated and wanted to get on in his own more intuitive way" [Gavroglu 1995, 54]. Convinced that it was impossible to continue his work in chemical valence by more analytic methods, London got persuaded that analytic methods were inadequate to tackle the problem of chemical valence and at last turned to group theory agreeing with Heitler that it might provide many clues for the generalization of the results derived by perturbation methods. The aim of their program was to prove that, from all the possible combinations of spins between atoms, only one term provides the necessary attraction for molecule formation.

In that same period Wigner's papers had aroused the feeling that the theory of the irreducible representations of the permutation group could be used for classifying the energy values in a many-body problem. But group theoretical techniques did not arouse general enthusiasm, even if physicist were beginning to feel that the future of theoretical physicis would more and more reside in the mastering of all this new "abstract" mathematics, "the subtler parts"of which were still considered stuff for mathematicians. The process of assimilation was still in progress, as the young Douglas Hartree's expressed in a letter to a friend of his, from Kopenhagen [Gavroglu 1995, 55]:

> "I am afraid that having studied physics, not mathematics, I find group theory very unfamiliar, and do not feel I understand properly what people are doing when they use it...I have been waiting to see if the applications of group theory are going to remain of importance, or whether they will be superseded, before trying to learn some of the theory, as I do not want to find it is going to be of no value as soon as I begin to understand something about it! Is it really going to be necessary for the physicist and chemist of the future to know group theory? I am beginning to think it may be".

Group theory could really seem something superfluous and bewildering, which could definitely cloud the issue. Moreover it appeared that something was happening which had many analogies with the recent discussions about the two different formulations of wave mechanics. In this respect one cannot help recalling a discussion between Ehrenfest and Born, during which Born mentioned how a certain physical argument could be more adequately expressed in the language of matrices. Ehrenfest insisted that it was clearer in wave mechanics, and at the time Born commented: "Es is eine Gewohnheitssache." (It's a question of habit.) Ehrenfest answered on his side: "Aber es gibt auch schlechte Gewohnheiten." (But there are also bad habits.) [Mehra and Rechenberg 1982, 233].

Could thus the application of group theory to quantum mechanics be regarded like a new example of a "bad habit"?

17. – Hermann Weyl: group theory and the conceptual foundations of Quantum Mechanics

There was someone whose ideas on the subject went well far ahead of simple applications to quantum mechanics. Hermann Weyl, a leading exponent of Hilbert's Göttingen style, had been in touch in his early career with Hilbert's interests such as invariant theory, integral equations, and mathematical physics, which the latter had shared with his friend Minkowski. Weyl was an expert in several aspects typical of the new quantum mechanics, such as the theory of Hilbert space, singular differential equations, eigenfunction expansions([94]), and now his commitment in the new intellectual challenge stemmed from his longstanding interests in group theory.

([94]) After learning from Born about matrix mechanics Weyl immediately got interested in the new theory and exchanged letters with Born and Jordan during 1925 (Weyl to Born 27 September

At the end of that same year 1927 Weyl's paper "Quantenmechanik und Gruppentheorie" appeared on *Zeitschrift für Physik* [Weyl 1927]. As Mackey points out, in the introduction Weyl had begun by distinguishing two questions in the foundations of quantum mechanics: 1) How does one arrive at the self-adjoint operators which correspond to various concrete physical observables? 2) What is the physical significance of these operators, *i.e.* how does one deduce physical statements?[Mackey 2001, 249]. Von Neumann formulation of quantum mechanics published earlier in that same year answered more or less satisfactorily to the second question in term of Hilbert spaces, so Weyl proposed to treat the first and deeper one with the help of group theory: "Hier glaube ich mit Hilfe der Gruppentheorie zu einer tieferen Einsicht in den wahren Sachverhalt gelangt zu sein." (Here with the help of group theory I believe I have succeeded in arriving at a deeper insight into the true nature of things.) It was an analysis of the foundations of quantum mechanics in which Weyl emphasized the fundamental role played in the new theory by the theory of Lie group. And indeed he managed to show that group theory was much more than a useful artifice for simplifying the problem of diagonalizing matrices, and that it was a poweful tool for penetrating the conceptual foundation of quantum theory. Weyl's work was perhaps the first step in the program of deriving fundamental relationship in quantum mechanics from group-theoretical symmetry principles; he tried, for example, to find a deeper reason via group theory, for assuming the Heisenberg commutation relations than that provided by analogy with the classical Poisson brackets [Mackey 2001, 249](95). As he later stressed "*The kinematical structure of a physical system is expressed by an irreducible Abelian group of unitary ray rotations in system space. The real elements of the algebra of this group are the physical quantities of the system; the representation of the abstract group by rotations of system space associates with each such quantity a definite Hermitian from which 'represents' it.* If the group is *continuous* this procedure automatically leads to *Heisenberg's* formulation" [Weyl 1949, 275].

On January 29 of 1928 Pauli wrote him from Hamburg, commenting Weyl's recently published paper and telling him how they would enjoy themselves talking "über Quanten- und Gruppentheorie" during Weyl's stay in Hamburg for some lectures. All this happened just before what Pauli had called in the above letter his "Quantensprung von Hamburg nach Zürich". At the beginning of April he was appointed full professor of theoretical physics at the Eidgenössische Technische Hochschule, where he funded an institute of his

1925; Weyl to Jordan 13, 23 and 25 November), especially about calculations connected with canonical transformations [Mehra J. and Rechenberg 1982, Vol. 4, 228, footnote 2].

(95) Weyl proposes that the spectral theorem should allow one to associate to an unbounded Hermitian operator A on a Hilbert space H a one-parameter group $\{\exp[itA]\}(t \in \mathbf{R})$ of unitary transformations of H having iA as an infinitesimal generator, as is well known to be the case in the finite-dimensional case. Then, given canonically conjugate Hermitian operators satisfying the Heisenberg commutation relations $[P_j, Q_k] = \frac{\hbar}{i}\delta_j^k \; [P_j, P_k] = [Q_j, Q_k] = 0 \quad (j \geqslant 1,\, k \leqslant n)$, Weyl views these relations as defining a Lie algebra (the commutation relation holds if and only if the two one-parameter subgroups $\tau \to e^{i\tau P_j}$, $\sigma \to e^{i\sigma Q_k}$ obey the commutation relation $e^{i\sigma P_k} e^{i\tau Q_j} = e^{i\sigma\tau\hbar\delta_j^k} e^{i\tau Q_j} e^{i\sigma P_k}$ for all σ and τ) and considers the associated group N of unitary transformations generated by the exp $[itU]$, where U runs through the real linear combination of the P_j and Q_k. The physical magnitudes of the physical system are thus characterized as the real elements of the algebra of the group of unitary transformations. Weyl also shows for the case of the group generated by one canonical pair Q, P that the irreducible ray representation is unique and is the representation used by Schrödinger for which $Q\psi(q) = q\psi(q)$ and $P\psi(q) = \frac{1}{i}\frac{\mathrm{d}}{\mathrm{d}q}\psi(q)$. These operators are thus a necessary consequence of Heisenberg's commutation relations.

own. During the semester 1927-1928 in Hamburg Pauli had attended lectures of Emil Artin about "Representation Theory of Semi-simple Systems" and in the following year, when he took up the professorship at the ETH in Zürich, he came into closer contact with Hermann Weyl, so that on 14 July 1928 he could write to Bohr from Zürich: "Von Weyl habe ich ferner viel gelehrte Gruppentheorie gelernt, so daß ich jetzt die Arbeiten von Wigner sowie von Heitler und London, die auch für die neue Heisenbergsche Theorie des Ferromagnetismus wichtig sind, wircklich verstehen kann" (from Weyl I have learned so much erudite group theory that really can understand the papers of Wigner, Heitler and London, which are also important for Heisenberg's new theory of ferromagnetism)([96]). In fact Pauli's treatment of the hydrogen atom of 1926 was in its essence entirely group theoretical([97]). He used the language of angular momenta, and considered a quantum-mechanical version of the Runge-Lenz vector, the conservation of which is specific to the Coulomb potential, to explain the degeneracy of the hydrogen spectrum. His formulation of the problem using the quantum extension of the Runge-Lenz vector to obtain the dynamical $SO(4)$ symmetry of the bound states, combined with the general theory of angular momentum, allowed him to obtain the energies of the hydrogen atom by an abstract approach which can be immediately transposed in strict group theoretical language [Pauli 1926]. In 1935 the spectrum of the H-atom was explicitly derived from the $SO(4)$ symmetry by Fock: "Die Transformationsgruppe der Wasserstoff-gleichung ist also die *vierdimensionale* Drehgruppe; dadurch wird die Entartung der Wasserstoffniveaus in bezug auf die Azimutalquantenzahl l erklärt." (The transformation group of the hydrogen equation is therefore the *four-dimensional* rotation group; for this reason the degeneration of hydrogen levels will be explained through the azimutal quantum number l) [Fock 1935].

During 1927-1929 group theory was intensively applied by Weyl, von Neumann and Wigner to investigate the invariance properties of a system's Hamiltonian and to seek non-combining term systems. The same methods were applied by Friedrich Hund, and also by Walter Heitler and Fritz London [Hund 1927; Heitler and London 1927; Heitler 1927]. In his paper Hund rediscovered in quantum mechanics Young's symmetry operators, the algebraic construction of the irreducible representations of the symmetric permutation group in an explicit and elementary manner, due to A. Young and G. Frobenius([98]).

At first Robert Mulliken gave due credit to Heitler and London's works saying that they offered "a suitable theoretical foundation for an understanding of the problems of valence and of the structure and stability of molecules" [Gavroglu 1995, 75]. On the other hand the chemical community did not show a real interest in the Heitler-London group-theoretical approach to the homopolar bond, which did not produce quantitative results, and which in any case constituted quite a different program from that developed by Linus Pauling and Robert Mulliken, who had a strong preference for semiempirical

([96]) In his very popular book *The Theory of Electric and Magnetic Susceptibilities* (Oxford University Press, London 1932) van Vleck gave a proof of two equations "derived by Heisenberg with the rather involved machinery of group theory" showing "that Dirac's kinematical interpretation of the exchange effect...frees us from the need of using this." [*ibid.*, 340]. Van Vleck was also convinced that the entire interaction problems could essentially be solved using Dirac's vector coupling of electron spins— equivalent to Heitler and London's exchange force— instead of "recondite" group theory methods.
([97]) About Pauli's attitude see also [von Meyenn 1983, 341].
([98]) At the turn of the XIX century A. Young applied the representation theory of the symmetric group to construct all of the irreducible tensor representations of the special linear groups SL (n, C) and the group GL (n, C) of invertible $n\times n$ matrices with complex elements.

methods, whose only criterion for acceptability was their pratical success. London's first major paper on the application of the group-theoretical methods to problems of chemical bonding appeared in 1928; the overall result was that the interpretation of the chemical facts was compatible with the conceptual framework of quantum mechanics [London 1928a, 1928b](99). In 1933 Van Vleck recognized the merit of symmetry arguments in molecular calculations on polyatomic molecules [Van Vleck 1933]. Mulliken surrendered, too, and made an extensive use of group theory in his paper of that same year, where he applied Bethe's results to molecular electron wave functions [Mulliken 1933].

By 1929 Dirac could state that "the underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known" [Dirac 1929a](100). However, the application of symmetry arguments was slow in penetrating the chemical community.

During the fifth Solvay Conference in October 1927 Born and Heisenberg mentioned Wigner's systematic treatment for the several-body systems using group theory [Mehra 1975](101). In those same days Dirac mentioned to Bohr his concern about a relativistic wave equation and within two months, on January 2, 1928, he had solved the whole

(99) London wanted to tackle what he thought was the most fundamental problem of atomic theory: "the mysterious order of clear lawfulness, which has been expressed symbolically in the language of chemical formulas". In his program he intended to deal with the problem of the mutual force interactions between the atoms, and he wanted to examine whether it was possible to decipher the meaning of the rules that the chemists had found in semiempirical ways and to place those on a sound theoretical basis. Thirdly, he would attempt to determine the limits of these rules and, if possible, to initiate a quantitative treatment of them. However, he was not at all certain that the principles considered so far in atomic theory could, in fact, be used for the realization of such a program, because of the characteristic deviation of chemical forces from other familiar forces. Group theory did provide a way out, and the interpretation of the mathematical results turned out to be formally equivalent to the chemical model, *i.e.* it produced the same valence numbers and it satisfied the same formal combination rules as those expressed in the symbolic representation of the structural formulas of chemistry. In particular the fact that the valences were "saturated" proved in this context to be an expression of the restriction of the Pauli exclusion principle. Also for further details see [Gavroglu 1995, 56-57]

(100) By 1933 the pioneering years were over, and the *The Journal of Chemical Physics* made its appearance. In the first issue, January 1933, the "Editorial" by Harold C. Urey said: "The Journal of Chemical Physics... is a natural result of the recent development of the chemical and physical sciences. At present the boundary between the sciences of physics and chemistry has been completely bridged ... mathematical physics has contributed to many chemical problems...Most important of all, the experimental and theoretical work associated with the quantum theory has made a profound contribution to our knowledge of chemistry and physics. Moreover, the history of these sciences in recent years teaches the effectiveness of applying the exact logic of mathematics to chemical as well as physical problems".

(101) Wigner had shown that a system could, in general, be split into sub-systems without interactions. One of its features was the separation between symmetric and anti-symmetric partial systems. Born and Heisenberg pointed out that the systems of the symmetric type were subject to Bose-Einstein statistics while the antisymmetric systems obeyed Fermi-Dirac statistics. On October 28 Wigner proved that considering a system (a molecule, a nucleus) which contains N particles each of which satisfies Fermi-Dirac statistics, then a gas of such systems follows Bose-Einstein (Fermi-Dirac) statistics if N is even (odd). At that time nobody noticed this result which he published in a Hungarian journal [E.P. Wigner, *Math. Und Naturwiss. Anzeiger der Ungar. Ak. Der Wiss.*, **46** (1929) 576]. In early 1929 Franco Rasetti had published experimental results for the molecules H_2 and N_2, which showed that nitrogen nuclei obey Bose-Einstein statistics, a fact extraordinaly surprising if applyed to the proton-electron model of the N^{14} nucleus.

matter guided by two invariance requirements: the space-time properties of the equation should transform according to the theory of relativity and the quantum properties should transform according to the transformation theory of quantum mechanics. He worked out the problem "playing around with mathematics" [Dirac 1928]. According to Dirac's philosophy, which completely excluded the idea of model-building, the physical problem must be reduced to a mathematical one, and mathematics forced him to accept a four-component wave function. As Darwin acknowledged, in comparing Dirac's equation with his own attempt: "Dirac's success in finding the accurate equations shows the great superiority of principles over the previous empirical method" [Kragh 1990, 56]. In a sense, this was the first example of a theory built after the rule of imposing certain symmetries (Lorentz invariance, in this case) independently of the phenomenological evidence. "Gauge theories" would be the next step.

In 1927 Pauli's paper on nonrelativistic electrons with spin 1/2 had appeared [Pauli 1927]([102]). It was the first concrete step toward the formulation of what two years earlier he had called "not classically describable two-valuedness" (Zweideutigkeit) [Pauli 1925]. In order to accommodate spin in a wave of two components for the electron, he introduced the famous matrices which bear his name assuming a function $\varphi(q, s_z)$ with two components corresponding to $s_z = \pm 1$. The step from one to two φ components was very large, as well as the step from vector algebra —the spin vector $\mathbf{s}$ introduced by Heisenberg and Jordan— to a two-valued representation of the rotation group. As van der Waerden remarked later [van der Waerden 1977]

> "In spite of these highly satisfactory results, Pauli regarded his theory as 'provisional and approximate' only because, as he says, his wave equation is not invariant under Lorentz transformations and does not allow the calculation of higher-order corrections to the fine structure of hydrogen terms. It seems that Pauli underestimated the importance and the final character of his methods and results. His description of the state of N electrons by a φ function having several components which transform linearly according to a two-valued representation of the rotation group, was fundamental and final. It enabled Wigner and von Neumann to deduce all empirical rules of atomic term zoology without introducing any new assumption or any approximation".

The paths of physicists and mathematicians got really entangled on the occasion of Pauli's introduction of the matrices which are now called after his name. He had put forward the idea that the wave function of an electron could be represented by a vector with two complex components, a spinor in 3-dimensional Euclidean space, and had realized that spin corresponded to a new kind of tensor, one not covered by the Ricci and Levi-Civita work of 1901. Physicists had rediscovered spinors, the mathematical entities

([102]) The relations $\mathbf{s}_i\mathbf{s}_j + \mathbf{s}_j\mathbf{s}_i = 2\delta_{ij}\mathbf{I}$ were pointed out to Pauli by Jordan who also drew his attention to the connection between these matrices and quaternions [Pauli 1927, 607, footnote 2]. Dirac later commented: "I believe I got these [matrices] independently of Pauli, and possibly Pauli also got them independently from me" [Dirac 1977, 109]. Dirac praised Pauli's theory: "It consists in abandoning from the beginning any attempt to follow the classical theory. One does not try to take over into the quantum theory the classical treatment of some model, which incorporates the empirical facts, but takes over the beautiful example of the general quantum theory, and shows that this quantum theory is no longer completely dependent on analogies with the classical theory, but can stand on its own feet"[Kragh 1990, 56].

created and studied by Cartan who had introduced spinor representations in 1913, as part his much more general investigation on the linear representations of groups, without giving them any special significance. It was Pauli's theory, and then Darwin's —which sought to introduce the electron's magnetism in a way conforming to the theory of relativity by defining four functions representing the components of a space-time vector— which inspired Dirac to invent his theory of the relativistic electron. In 1928 Dirac thus created for the needs of his relativistic equation a wave function for the electron represented by a vector with four complex components, which is a spinor of 4-dimensional pre-Euclidean space-time: mathematics had forced him to accept the use of the "unphysical" 4×4 matrices as coefficients in his equation, from which a four-component wave function had stemmed([103]). The vector spaces which have spinors as elements are connected with the general theory of Clifford spaces, introduced by Clifford in 1876. In 1935 Weyl wrote a paper on spinors jointly with R. Brauner giving a global definition based on the use of the Clifford algebra, itself suggested by Dirac's formulation of the equations for the electron. After spin representations were found important in quantum physics, Cartan published a detailed general study: "One of the principal goals of this work is to develop the theory of spinors systematically by giving these mathematical entities a purely geometric definition: thanks to their geometric origin, the matrices used in quantum mechanics by physicists are themselves presented" [Cartan 1938].

For the winter semester 1927–28 Weyl planned to give a set of lectures on group theory. However, before he started, Schrödinger and Debye left Zürich, leaving a temporary void which he decided to fill putting the role of group-theoretic principles in quantum mechanics at the center of his course, from which his second famous book on mathematical physics arose. The first German edition of *Gruppentheorie und Quantenmechanik* appeared in 1928 [Weyl 1928]. "Die Wichtigkeit gruppentheoretischer Gesichtspunkte für die Gewinnung der allgemein gültigen Gesetze in der Quantentheorie ist in der letzten Zeit immer mehr offenbar geworden" (the importance of the standpoint afforded by the theory of groups for the discovery of the general laws of quantum theory has of late become more and more apparent) wrote Weyl in the first lines of his preface. In the introduction he remarked that "Unsere Generation ist Zeuge eines Vordringens der physikalischen Naturerkentniss in die Tiefe, wie sie seit den Tagen von *Kepler, Galilei* und *Newton* nicht mehr erlebt worden ist. So stürmische Epochen kennt die Geschicte der Mathematik kaum." (Our generation is witness to a development of physical knowledge such as has not been seen since the days of Kepler, Galileo and Newton, and mathematics has scarcely ever experienced such a stormy epoch. Mathematical thought removes the spirit from its worldly haunts to solitude and renounces the unveiling of the secrets of Nature. But as recompense, mathematics is less bound to the course of worldly events than physics.)

In recalling that Felix Klein considered the group concept as most characteristic of the XIX century mathematics, Weyl stressed why group theory was of fundamental importance for quantum physics: "enthüllt hier die wesentlichsten Züge, welche nicht an eine spezielle Form des dynamischen Gesetzes, an einen bestimmten Ansatz für die wirkenden Kräfte gebunden sind" (here reveals the essential features which are not contingent on

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([103]) In his article, where von Neuman gave the five fundamental covariants of the Dirac theory, and thanked Jordan and Wigner for discussions, he also commented: "Daß eine Größe mit vier Komponenten kein Vierervektor ist, ist ein in der Relativitätstheorie nie vorgekommener Fall." (That a quantity with four components is not a four-vector has never yet happened in relativity theory.) See [von Neumann 1928] particularly footnote on p. 876.

a special form of the dynamical laws nor on special assumptions concerning the forces involved). And he continued: "Die Untersuchung der Gruppen wächst sich eigentlich erst in der Lehre von der *Darstellungen einer Gruppe durch lineare Transformationen* zu einer zusammenhängenden, tiefer eindringenden und einigermaßen geschlossenen Theorie aus. Und gerade dieser mathematisch wichtigste Teil ist es, dessen wir für eine adäquate Wiedergabe quantenmechanischer Beziehungen bedürfen." (The investigation of groups first becomes a connected and complete theory in *the theory of the representation of groups by linear transformations*, and it is exactly this mathematically most important part which is necessary for an adequate description of the quantum mechanical relations.) The reduction of the state space into irreducible subspaces on the quantum-mechanical side, and the reduction of representations on the group-theoretical side, had shown once more the deep relation of mathematics to physics, far beyond the simple possibility offered by a useful mathematical tool. In fact the "unreasonable effectiveness of mathematics", how Wigner later called it, generated a bridge between the theoretical structure of a *already matematized* physical theory, and the mathematical structure in which the theory was embedded([104]). The representation of the state of a quantum system in terms of Hilbert spaces allowed a group-theoretic formulation of quantum mechanics. Now Weyl went on triumphally stating what must have appeared to him as a pre-established harmony between mathematics and the physical world: "*Alle Quantenzahlen, mit Ausnahme der sog. Hauptquantenszahl, sind Kennzeichen von* Gruppendarstellungen." (All *quantum numbers, with the exception of the so-called principal quantum number, are indices characterizing representations of groups.*) No wonder if he could conclude: "Man wird erwarten dürfen, daß diese Teile der Quantenphysik zugleich die am meisten gesicherten sind." (We may well expect that it is just this part of quantum physics which is most certain of a lasting place.) [Weyl 1949, xxi].

Heisenberg immediately reviewed Weyl's book in entusiastic terms. He praised the "extraordinary elegant treatment" and the "masterly exposition", and in remarking its importance for physicists at the same time he hoped it could arise mathematicians' interest towards "the most abstract trends in modern physics" [Heisenberg 1928]. While Schrödinger immediately complained to Hermann Weyl on November 6, 1929, that he had to struggle with the most "basic difficulties" and that he was not willing "to consume such a limitless formalism".

People of the younger generation showed more willing towards what most physicists saw only as a highly developed branch of pure mathematics. Hendrik B. G. Casimir remembered his early years at Leiden [von Meyenn 1989, 108-114 and Casimir 1983, 75]:

> "During the autumn and winter of 1928-29 we struggled mainly with group theory... The idea that many qualitative conclusions from quantum mechanics could be reached by formalized considerations about the symmetry of the atom strongly appealed to Ehrenfest. Several specialists came to lecture to us about the subject, Wigner first of all... I also remember a brilliant lecture by Van der Waerden. But, for me, the great revelation was Hermann Weyl's *Gruppentheorie und Quantenmechanik*. I think it was a wonderful book. I had —and still have— the feeling that this is the right way to formulate mathematics, that the concepts Weyl formulates have just the right level of abstraction, yet correspond to something 'real',

([104]) For a discussion on the relationship between mathematics and physics represented in terms of an embedding of a scientific theory into a mathematical structure see [French 1999].

whatever that may mean in pure mathematics. If I have later been able to apply group theory with some measure of success to several problems, and if I have even been able to make a small but not entirely insignificant contribution to that field of pure mathematics, this is thanks to Weyl's book."

Casimir, still a student, met Pauli for the first time that same year on the occasion of his September 22 lecture in Utrecht, and he remembered how busy Pauli was, studying Weyl's demanding book published in August. In the summer of 1931 Casimir was at the Bohr Institute in Kopenhagen, writing his "academic thesis", a general formulation of the theory of the asymmetrical rotator, which was still in an unsatisfactory position because wave-mechanical methods led to extremely complicated relations. Only through the work of Oskar Klein and of Casimir could it be shown that these problems could be solved more rapidly and simply using the symbolic methods of the matrix representation. Using group theory methods Casimir could reduce the complex problem of calculating energy eigenvalues and transition probabilities to a procedure of the diagonalization of the relevant matrices. In his thesis he also developed the theory of the representation of the three-dimensional rotation group and of the continuous groups. Here the Casimir operators appear for the first time [Casimir 1931]. Casimir informed Weyl about his discoveries concerning the representation of continuous groups in a letter dated May 1, 1931. In 1936, together with van der Waerden, Casimir published a proof of the complete reducibility of the reducible representations of semisimple continuous groups dealt with in the letter to Weyl [Casimir 1983 and van der Waerden 1935]. All this originated by Pauli, as he himself recalled in 1955 writing to Weyl about Artin's lectures he had followed in Hamburg: "Artin said that he could not discuss the continuous groups, because no algebraic proof existed for the theorem of the complete reducibility of the representations of semisimple continuous groups... When Casimir was my assistant in Zürich I therefore drew his attention to this problem, for the case of the three-dimensional rotation group. He rapidly made a simple, purely algebraic, proof of the theorem. He sent it to van der Waerden in Leipzig who could easily generalize it to arbitrary semisimple groups"([105]). Years later Casimir met Weyl in Princeton: "Then I was happy to be able to tell him how grateful I was. (Weyl on that occasion paid me a probably wholly undeserved compliment: he introduced me to a colleague as a physicist with the soul of a mathematician)" [Casimir 1983, 76].

In the concluding lines of the first German edition of his *Gruppentheorie* Weyl synthethized his views about a new form of interaction between physics and "newer mathematics": "Im Gebiete der Physik behält zwar das Kontinuum der reellen Zahlen sein altes unerschütterliches Recht für die physikalischen Messungen. Aber das Wesen der neuen Heisenberg-Schrödingerschen Quantenkechanik kann man mit einigem Recht darin erblicken, daß zu jedem physikalischen Gebilde ein eigenes Größensystem gehört, eine nicht-kommutative Algebra im technisch-mathematischen Sinne, deren Individuen die physikalischen Größen selbst sind." (The continuum of real numbers has retained its ancient prerogative in physics for the expression of physical measurements, but it can justly be maintained that the essence of the new Heisenberg-Schrödinger-Dirac quantum mechanics is to be found in the fact that there is associated with each physical system a set

([105]) See [von Meyenn 1989] also for the full quotation and English translation of Casimir's letter to Weyl. Interest in the Casimir's operators drastically increased in the early 1960's, when the properties of irreducible representations of continuous semisimple groups acquired a considerable importance in the theories of strongly interacting elementary particles.

of quantities, costituting a non-commutative algebra in the technical mathematical sense, the elements of which are the phsical quantities themselves.) Therefore, it is little wonder that Weyl's book was considered difficult and obscure by most physicists. Weyl had tried to mediate trying to explain the mathematics to the physicists and the physics to the mathematicians ("bei den mathematischen Abschnitten ist mehr an einer physikalischen, bei den physikalischen mehr an einen mathematischen Leser gedacht" (the mathematical portions have been written with the physicist in mind, and vice versa) [Weyl 1949, vii]. Chen Ning Yang has recalled how Weyl "liked to deal with concepts and the connection between them. His book was very famous, and was recognized as profound. Almost every theoretical physicist born before 1935 has a copy of it on his bookshelves. But very few read it: Most are not accustomed to Weyl's concentration on the structural aspects of physics and feel uncomfortable with his emphasis on concepts. The book was just too abstract for most physicists" [Yang 1986, 10](106).

18. – "Gruppenpest"

Pauli viewed with favour how Wigner had showed that many selection rules were simply the consequences of symmetry. Laporte had found in his analysis of the iron spectrum in 1924 that in an electromagnetic transition in which one photon is emitted or absorbed, the levels always change from even to odd, or vice versa. Wigner's methods enlightened how the deep connection between the nature of physical systems and the necessity of the "existence" of the phenomenological rule was a consequence of parity invariance, the space-inversion symmetry, which Weyl had designated by the symbol i in his recently published book. Pauli had strongly criticized Bohr's attempts to interpret them using models: "Die Sache wird, glaube ich, viel klarer, wenn man auf die von Wigner (*Zeitschrift für Physik* 43, 624, 1927 und 45, 601, 1927) entwickelten gruppentheoretischen Methoden zurückgreift" (the matter is becoming much clearer, when one has recourse to the group theoretical methods developed by Wigner), he wrote from Hamburg on February 10, 1928, to Kronig, whose "Zur Deutung der Bandenspektren" had appeared that same day [Kronig 1927]. Pauli clearly stated that "Die Invarianz der Differentialgl[eichung] gegenüber Drehungen+Spiegelungen des Koordinatensystems (bei Fehlen äußerer Felder) hat nach Wigner unmittelbar die Laportesche Regel zur Folge." (The invariance of the differential equation as against the rotations and reflections of the coordinate system (in absence of external fields) leads immediately after Wigner to the Laporte rule.)

Paul Ehrenfest on the contrary was much troubled with the new field of the "group freaks", according to Pauli's colourful definition (Pauli to Ehrenfest, October 28, 1932), and in September 22 of that same year he complained to Pauli, who was on the way to Holland to give a lecture in Utrecht: "Hingegen habe ich Ihnen ein dickes Bündel Fragen über die schrecklich vielen neuen 'Gruppenpest-Arbeiten' vorzulegen, von denen ich keine einzige auch nur über die ersten Blattseiten hinaus kapieren kann, obwohl ich mich sehr darum plage." (On the contrary I have a thick pile of questions about the frightening number of new 'group-pest papers' of which I can't understand a single one

(106) In recalling Weyl's deep interest in art, J. A. Wheeler has also pointed out that "Art characterized also his own way of thinking, writing, and speaking: art in the sense of bringing disparate themes into harmonious unity. The chapters in *Theory of Groups and Quantum Mechanics* masterfully alternate between group theory and quantum mechanics, between mathematical methods and their applications, between the ideas basic for groups and the concepts fundamental for physics" [Wheeler 1986].

also not even beyond the first pages, although I trouble myself very much with them.) He also asked Pauli to help him at least in explaining him problems and principal results exposed in those works. He looked someway in dispair when he wrote at the beginning of the same month to Kramers: "Will I however ever understand the group maniacs?"

In his letter to Kronig of February 10, 1928, Pauli ended saying he had met Wigner in Göttingen: "...er hatte bereits selbst begonnen, Überlegungen zu den Bandenspektren anzustellen, aber seine Resultate sind mir im Einzelnen nicht bekannt" (...he is already workig to application to band spectra, but I don't know his results in detail).

In that period von Neumann and Wigner published the first of three papers generalizing everything that the latter had done to the case of spin-1/2 Pauli electrons, and where a more rigorous basis for aspects of atomic spectra having to do with electron spin was given [von Neumann and Wigner 1928]. In the course of this work they also argued that every symmetry of a quantum-mechanical system is implemented, either by a unitary operator of by an anti-unitary operator, in the Hilbert space of states. Wigner's and von Neumann's joint work was a big jump towards the full understanding of the spectroscopic consequences of Pauli exclusion principle. It provided the deep reason for spinors in nature, which had previously been introduced in an *ad hoc* fashion to describe a characteristic feature of the electron. They showed how permutation symmetry relates to the $SO(3)$ classification and introduced what are now usually called Clebsch-Gordan coefficients. The whole apparatus of group characters and representations was put into action and the complete system of term zoology, including selection rules, intensity formulae, Stark effect and Zeeman effect was developed([107]). All this showed how there is an elementary group-theoretical explanation for the ubiquitous appearance in atomic physics of degenerate "multiplets of multiplicity $2j+1$", a concept which is completely alien to classical theory.

In the introduction to the first part of their three part *opus* on group theory and quantum mechanics they claimed that: "This method rests on the exploitation of the elementary symmetry characteristics of every atomic system —namely, the equality of every electron and the equivalence of all directions in space... which has in the so-called representation theory its due mathematical tool" and had concluded that they connected the search for laws of nature with seeking "symmetry —(*i.e.* invariance) principles" likening this program to *"general relativity where an invariance principle permits the discovery of universal laws of nature"* (emphasis added) [von Neumann and Wigner 1928, 203]. On July 23, 1930 Wigner and Witmer submitted a paper covering all possible descriptions of the molecular states that derive from symmetry considerations by bringing up group theory into play. Wigner always stressed the importance of taking full advantage of the symmetry of a system before calculations are attempted [Wigner and Witmer 1928]. In the same spirit is one of the most remarkable of Wigner's early papers concerning the application of group theory to the normal modes of vibration of molecules, all of which he shows can be classified by the irreducible representations of the group whose operations send the equilibrium configuration of the molecule into itself [Wigner 1930].

([107]) The Hamiltonian of the electrons of an atom is invariant under the group of permutations of the electrons, and also under the group of space rotations and reflections. Hence, the eigenfunctions belonging to a given energy value are linearly transformed by these two groups. These linear transformations form representations of the groups. The problem is to determine these representations and to find the connection between their characteristic numbers and the quantum numbers L, S, J etc. of the corresponding terms.

All Wigner's work up to now can be regarded as precursors to his epoch making book on group theory and its applications to atomic spectroscopy *Gruppentheorie und ihre Anwendung auf die Quantenmechanik der Atomspektren* [Wigner 1931]. Wigner did not have a good opinion on Weyl's book, which had immediately become a standard text in the field, even if it turned out to be rather inaccessible for most physicists: "Those who understood it saw in it a rigorous beauty. But Weyl did not write clearly, and so most physicists did not understand his book. Young students especially found the book awfully dense. For all his brilliance and good intentions, Hermann Weyl had discouraged a number of physicists from studying group theory". He "felt deeply offended that Weyl did not mention my papers, which had preceded his book" [Mehra 2001c, 923].

Leo Szilard, Wigner's compatriot from Budapest, with whom he had became acquainted in Berlin during his chemical engineering studies, had convinced him of the importance of writing a book more accessible than Weyl's, and moreover Wigner, who at the time did not have any stable position, could establish a priority claim on the subject. Also Max von Laue encouraged him and persuaded Vieweg of Braunschweig to publish it. In the preface to the first English edition of 1959 Wigner remembered how "Von Laue's encouragement of both publisher and author contributed significantly to bringing this book into existence" and added "I like to recall his question as to which results derived in the present volume I considered most important. My answer was that the explanation of Laporte's rule (the concept of parity) and the quantum theory of the vector addition model appeared to me most significant. Since that time, I have come to agree with his answer that the recognition that almost all rules of spectroscopy follow from the symmetry of the problem is the most remarkable result" [Wigner 1959, v]. Already before the rise of the new quantum mechanics a semi-empirical description of the regularities observed in spectra had been given with the aid of the vector model. Wigner now stressed how the power of group theory had revealed itself in explaining the most important characteristic of spectra of n-electron atoms. In such cases the appropriate Schrödinger equation cannot be solved exactly, hence approximation was viewed as the only way forward. In reintroducing the effects of the electrons on one another as a perturbation the degeneracy is only partially removed, and most of the resulting levels are still degenerate and about them, "nothing is known on a purely theoretical basis (apart from a rough estimate of their positions) except their symmetry properties. These are manifested in the transformation properties of the corresponding eigenfunctions under permutations of the electrons, pure rotations, and inversion (reflection). Accordingly, each level corresponds to three representations —one of the symmetric group, one of the pure rotation group, and one of the reflection group" [Wigner 1959, 181]. In his book, which was mainly a detailed reworking of what he had previously published, Wigner also introduced and studied the concept of "tensor operator" and the theorem about such operators now known as the Wigner-Eckart theorem.

Mathematicians were getting more and more involved in the new physics. In 1932 also the mathematician Bartel van der Waerden entered the subject with *Die Gruppentheoretische Methode in der Quantenmechanik* [van der Waerden 1932], and in 1933 Bauer's *Introduction à la Théorie des Groupes et ses Application à la Physique Quantique*[Bauer 1933] appeared, an excellent, even if more elementary book.

Van der Waerden had studied with Emmy Noether in the years when she had made Göttingen the world's leading outpost for the study of modern algebra. In 1924-25, when he arrived in Göttingen as a graduating Amsterdam student, he was 22 years old, and one of the brightest young mathematical talents in Europe. He quickly mastered the theories of Emmy Noether, enlarged them with important new findings and promoted her ideas. In 1930 he had published *Moderne Algebra, unter Benutzung von Vorlesungen von E. Artin und E. Noether*, a reworking of Emmy Noether's lessons on group theory and hypercomplex numbers in which he incorporated a great deal of the important innovations accumulated over the last years at the level of the body of algebraic knowledge, and made this subject suddenly central in mathematics. In 1931 van der Waerden had been appointed professor of mathematics at the University of Leipzig: "Heisenberg and Hund held a seminar together, and I attended. It was on this occasion that I learned physics" [Dold-Samplonius 1994, 316]. According to Heisenberg van der Waerden's role at Leipzig was an outstanding one: "...he would, by a few sentences of explanation, clarify at once a complicated situation at our seminar... I feel that I have really learned a large part of my mathematical training from van der Waerden, just by discussing with him... when we spoke about the two-valued representation of the Lorentz group when the Dirac paper with spin came out, then van der Waerden at once could point out that the mathematicians know two-valued representations of the Lorentz group with such and such properties and that's apparently what the Dirac matrices are and so the understanding at Leipzig of the Dirac paper was very largely due to van der Waerden's help"([108]).

Van der Waerden, too, did not like Weyl's *Gruppentheorie und Quantenmechanik*: "...his book was so difficult that no one understood it. Hermann Weyl wanted to write mathematics for beauty's sake, but I did not find it very beautiful", and underlined how it "was of little help, being too difficult for most of us. Wigner's book was much easier" [Dold-Samplonius 1994, 316].

As John Slater wrote years later in his autobiography [Sternberg 1994, x]:

> ... "Wigner, Hund, Heitler and Weyl entered the picture with their "Gruppenpest": the pest of the group theory... The authors of the "Gruppenpest" wrote papers which were incomprehensible to those like me who had not studied group theory, in which they applied these theoretical results to the study of the many electron problem. The practical consequences appeared to be negligible, but everyone felt that to be in the mainstream one had to learn about it. Yet there were no good texts from which one could learn group theory. It was a frustrating experience, worthy of the name of a pest. I had what I can only describe as a feeling of outrage at the turn which the subject had taken."

At this stage group theory was suspected to be some kind of "black magic" which one had to be familiar with, as Heisenberg recalled later: "So for some time everybody learned group theory and representations —the papers of Schur, the books of Schur. But then there came the paper of Slater, who succeded, more or less, to reduce everything to the Pauli principle. So one didn't need too much this detailed work of Schur ..."([109]).

The "final solution of the difficulties came from Slater's paper", stressed van der Waerden.

([108]) Interview with W. Heisenberg by T. S. Kuhn (July 5, 1963, Niels Bohr Library, Archive for the History of Quantum Physics).
([109]) Interview with W. Heisenberg by T. S. Kuhn (February 19, 1963, Niels Bohr Library, Archive for the History of Quantum Physics).

19. – Slater kills the "Group Dragon"

In 1929 Slater published his influential paper "Theory of Complex Spectra" on *Physical Review*, showing that all results can be derived by using the simplest mathematics only [Slater 1929]. Slater discovered that by introducing the electronic spin at the very beginning of a calculation an enormous simplification could be achieved. He successfully obtained expressions for the energies of spectroscopic terms replacing considerations involving the group representation by considerations involving the three operators of angular momentum satisfying simple commutation relations.

"As soon as this paper became known, it was obvious that a great many other physicists were as disgusted as I had been with the group-theoretical approach to the problem. As I heard later, there were remarks made such as 'Slater has slain the Gruppenpest'. I believe that no other piece of work I have done was so universally popular" [Sternberg 1994, xi]. Here Slater is recalling a Goethe's *Faust* parody which was staged in April 1932 at Copenhagen, on the occasion of the tenth anniversary of Borh's Institute. In a short scene of this memorable play, the *Blegdamsvejen Faust*, Slater was seen to kill a dragon representing group symmetry: "Das indizbeschuppte Vieh (der Gruppendrache), er starb an Antisymmetrie." (The beast covered with indices scales (the group dragon) it died of antisymmetry.) Trace of this general rejection still remains in George Gamow's original drawings [Gamow 1966].

In 1967 Slater again recalled that during the years 1926-28, there had been a great deal of effort to adapt Heisenberg's simple ideas of symmetry in two-electron systems to atoms and molecules with more than two electrons [Ballhausen 1979]: "Wigner, Hund, Heitler, and others had been studying the general symmetry properties of many electron systems using group theory, but their results were so involved that it was difficult to see through them and arrive at practical results.... It seemed to me of great importance to see how to tie in the ideas of symmetry and anti-symmetry of wave functions, which Heisenberg and Dirac had formulated in 1926, with the practical study of complex spectra". In fact Slater's paper convinced most physicists for two decades that "they could be spared the trouble of learning such an 'unphysical' branch of mathematics as group theory" [Mackey 2001, Vol. 1, 249].

Still in 1935 Condon and Shortley explicitly rejected the group-theoretical approach as superfluous in their book "The Theory of Atomic Spectra", which for over a decade was the standard treatise on atomic spectroscopy. Toward the end of their introductory Chapter I they made the following statement, which is worthwhile reporting for illustrating the general feeling on group theory [Condon and Shortley 1935]:

> "The reader will have heard that this mathematical discipline is of great importance for the subject. We manage to get along without it. When Dirac visited Princeton in 1928 he gave a seminar report on his paper showing the connection of exchange energy with the spin variables of the electron. In the discussion following the report, Weyl protested that Dirac had said that he would derive the results without the use of group theory, but, as Weyl said, all of Dirac's arguments were really applications of group theory. Dirac replied, "I said I would obtain the results without previous knowledge of group theory". That incident serves to illustrate our attitude on this point. When a physicist is desiderous of learning of new theoretical developments in his subject, one of the greatest barriers is that it generally involves new mathematical techniques with which he is apt to be unfamiliar. Relativity theory brought the necessity of learning tensor calculus and Riemannian geometry.

> Quantum mechanics forces him to a more careful study of boundary value problems and matrix algebra. Hence if we can minimize the amount of new mathematics he must learn in order to penetrate a new field, we do him a real service...This does not mean that we understimate the value of group theory for atomic physics nor that we feel that physicist should omit the study of that branch of mathematics now that it has been shown to be an important tool in the new theory. It is simply that the new developments bring with them *so many new things to be learned that it seems inadvisable to add this additional burden to the load*" (emphasis added).

Actually, what Slater and Condon and Shortley did was to replace considerations involving the group representation of the group $SO(3)$ of all rotations about some point in three-dimensional Euclidean space, by considerations involving the three operators M_x, M_y, M_z, which occur explicitly throughout quantum mechanics as the operator counterparts of the x, y and z components of angular momentum, satisfying the well-known commutation relations $\frac{M_xM_y-M_yM_x}{i}=M_z$; $\frac{M_yM_z-M_zM_y}{i}=M_x$; $\frac{M_zM_x-M_xM_z}{i}=M_y$.

In the language of quantum mechanics the problem is to find the matrices that "represent" the three operators M_i for a particular base in Hilbert space. This base is built up of the eigenvectors common to one of the M_i and to M^2 (the operator representing the square of angular momentum). In the terminology now common in the literature such a base has become known as a "representation", a term which certainly must be distinguished from the group-theoretical meaning. Concepts like linear operators and matrices had very recently become familiar to physicists as integral part of the foundations of quantum mechanics; moreover, groups and group representations appeared difficult and unphysically abstract concepts: renaming Lie algebra of $SO(3)$ as "angular momenta" created the feeling that what they were doing was "physics and not esoteric mathematics", according to Mackey, who retrospectively remarked about Condon and Shortley's book: "Chapter III entitled 'Angular Momentum' is little more than a very complete account of the representation theory of $SU(2)$ (and hence of its quotient $SO(3)$) carried out from the point of view of a systematic study of triples of operators satisfying the commutation relations… it seems almost bizarre that the physicists should have made such a big thing of the difference between working with angular-momentum operators and working with representations of $SO(3)$" [Mackey 2001, 253]. In fact it must be noted that the mathematical similarity of the two problems, the group-theoretical and the quantum-mechanical, received at the time scant attention among physicists. They were probably too busy in "metabolizing" all the mathematical novelties stemming from the development of the new quantum mechanics, and also because, with a few notable exceptions, the majority were definitely not familiar with group-theoretical methods.

Even Max Born, who knew enough group theory not to be worried about its use, announced to Ehrenfest in September 29, 1930, that he had "removed an annoying point from physics, namely the unnecessary application of group theory. I find group theory a very beautiful mathematical tool but its application in atomic physics always seemed to me to be inappropriate (like shooting sparrows with artillery). I am therefore very pleased to observe that Slater and others (notably Fock) have mastered atomic structures most simply without group theory" [von Meyenn 1983, 341]. And indeed he had also criticized Heitler and London group-theoretical approach which, according to his views, implied unwarranted assumptions. His objection was not because group theory was not easy to use, but as he said: "in reality it is not in accord with the way things are" [Gavroglu 1995, 56]. He probably meant that from the fact that certain mathematical tools (such as group-theoretic techniques) are useful for solving a given problem (for example to establish convenient representation theorems in Quantum Mechanics), it does not necessarily follow that one has to believe that the structures generated by these techniques truly describe the world. Born's response to the use of group theory may appear surprisingly negative. It seems that Born, too, felt group theory as something not relevant, even alien to physics; maybe now it was his turn to consider such an abstract mathematical tool as something too far from physical reality.

Pauli, on the contrary, shared with Heisenberg a quite different opinion since the beginning; in 1933 he presented on page 176 a section on "Hamiltonfunktionen mit Transformationsgruppen" in the *Handbuch der Physik of 1933,* and in a footnote he mentioned Weyl's, Wigner's and van der Waerden's books as the relevant bibliography on the subject to which one should refer to investigate the whole matter in depth [Pauli 1933].

20. – Symmetry and Quantum Mechanics

In the first lines of Wigner's preface to the first German edition of his *Gruppentheorie*, after praising the group-theoretical methods by which a great part of results could be obtained "durch reine Symmetrieüberlegungen", Wigner continued "Man hat gegen die gruppentheorethische Behandlung der Schrödingergleichung oft den Einwand erhoben, daß sie 'nicht physikalisch' sei. Es scheint mir aber, daß die bewußte Ausnutzng elementarer Symmetrieeigenschaften dem physikalischen Gefühl eher entsprechen muß, als die mehr rechnerische Behandlung." (It was often objected that group-theoretical treatment of Schrödinger's equations is "unphysical". However, it seems to me that the deliberate utilization of elementary symmetry properties is bound to correspond more closely to physical intuition than the more computational treatment.) In the English edition published 28 years later, Wigner specified: "Of the older generation it was probably M. von Laue who first recognized the significance of group theory as the natural tool with which to obtain a first orientation in problems of quantum mechanics" [Wigner 1959, v].

In fact direct computation chosen by Shortley, Condon and Slater was profoundly different from more powerful reasoning by symmetry arguments which in quantum mechanics, thanks to linearity stemming from the superposition principle, can be spoiled to determine many properties of systems. On the occasion of Niels Bohr seventieth birthday, Wolfgang Pauli recalled the general attitude towards mental pictures and their sense of bewilderment during those years of transition: "After a brief period of spiritual and human confusion, caused by a provisional restriction to 'Anschaulichkeit', a general agreement was reached following the substitution of abstract mathematical symbols, as for instance "psi", for concrete pictures. Especially the concrete picture of rotation has been replaced by mathematical characteristics of the representations of the group of rotations in three-dimensional space" [Pauli 1955, 30]([110]).

Actually it was not until 1949, when Giulio Racah published his fourth article on complex spectra, that group theory became an accepted part of theoretical atomic spec-

([110]) Bohr's model of the hydrogen atom, which later was considered "anschaulisch", at first was not accepted as such. In a letter to Bohr, Richard Courant once wrote, "... how glad I was when I read of the Nobel Prize report in the newspapers. It reminded me vividly of that beautiful day in Cambridge in 1913 when you set forth your ideas for me in the quadrangle of Trinity. Thanks to prior suggestion by Harald [Bohr], who had so often told me wonderful things about his brother, I was at that point immediately ready to believe that you might be right. But when I then reported of these things here in Göttingen, they laughed at me that I should take such fantasies seriously" [Anderson 1982, 103].

troscopy. Racah was inspired by the 1929 paper of Slater and began with his anti–group-theoretical approach, developed and made standard by the book of Condon and Shortley. In 1942 he extended their algebraic methods, which led to prohibitively long and tedious calculations, finding short cuts, but only in Part IV he finally brought in group theory explicitly, thus opening a new chapter in theoretical atomic physics([111]). According to Abraham Pais, Racah, who was forced to leave Italy in 1938 due to racial laws, had "spent a full year studying Weyl's book during the isolation following his move from Florence to Jerusalem. That was all he needed to get started on his subsequent well-known work on complex atoms" [Pais 1986, 267]. Later Racah found that his work could be applied to nuclear structure, and the use of group theory among nuclear physicists culminated in the 1950s, when his methods were adapted to nuclear problems by H. A. Jahn, H. van Wieringen e B. H. Flowers in works published after 1950, but all this remained initially completely unknown to particle physicist, who learned it more than ten years later.

Wigner himself got deeply interested in nuclear interactions and structure of nuclei since 1932, when Chadwick annouced the discovery of the neutron. In 1932 he also gave a course on solid state physics at Princeton and in the ensuing six years he published nine papers on the subject([112]). In the same period he published over a dozen papers on nuclear physics. In both subjects he was one of the pioneers, so it was natural for Wigner to attempt to apply the theory of group representations even to these new fields.

By that time some physicist were beginning to feel that the future of theoretical physicis would more and more reside in the mastering of all this odd "abstract" mathematics, but "the subtler parts"of it were still considered stuff for mathematicians, as the young Douglas Hartree's expressed in a letter to a friend of his, from Kopenhagen [Gavroglu 1995, 55]:

> "I am afraid that having studied physics, not mathematics, I find group theory very unfamiliar, and do not feel I understand properly what people are doing when they use it. (In England, 'Physics' usally means 'Experimental Physics'; until the last few years 'Theoretical Physics' has hardly been recognized like it is here, and I understand, in your country. In Cambridge particularly the bias has been very much towards the experimental side, and most people now doing research in theoretical physics studied mathematics, not physics.) I have been waiting to see if the applications of group theory are going to remain of importance, or whether they will be superseded, before trying to learn some of the theory, as I do not want to find it is going to be of no value as soon as I begin to understand something about it! Is it really going to be necessary for the physicist and chemist of the future to know group theory? I am beginning to think it may be."

([111]) After Wigner's fundamental works of 1920s, the most important contribution to the group-theoretical treatment of n-electrons atoms came from Giulio Racah's revolutionary works published between 1942 and 1949. The fourth paper explains successfully hundreds of spectral lines from a few radial integrals. [Racah 1942, 1943, 1949]. During this period he wrote two letters to Weyl, one in 1947 and the other in 1949 [Borel 1986, 77]. In the first English edition of Wigner's book, *Group Theory and its application to the Quantum Mechanics of atomic Spectra*, a new chapter entitled "Racah coefficients" was added.

([112]) See for example [Bouckaert *et al.* 1936] Frederick Seitz was his first american Ph. D. student. Under Wigner's supervision he did a thesis devoted to what has been called the Wigner-Seitz method for constructing the wave function of metallic sodium.

In his paper "On the consequences of the symmetry of the nuclear Hamiltonian on the spectroscopy of nuclei" [Wigner 1937], submitted in January 1937, Wigner appears to have been the first to coin the term "isotopic spin" for a new variable, not related to space and time, assumed to constitute the difference between the neutron and proton states, and which had been invented by Heisenberg in his paper on the nuclear exchange force of 1932. The concept of isotopic spin was destined to play a key role in later developments in elementary particle physics. It was the first example of an internal or intrinsic variable characterizing the elementary particles and their interactions, starting from which, and using considerations of group theory, Wigner grouped nuclei into multiplets, thus extending this concept from spectral lines in atoms to nuclei. For the first time a general method was used to classify nuclear states: a multiplet structure reveals the presence of a certain symmetry or of some invariance property of a given observable with respect to transformations of the dynamical variables that serve to define it. This line of research would be explicitly introduced in elementary particle physics at the beginning of 1960s.

Still in 1960, Pais remembered, "Whatever the average well-educated theorist then knew about groups concerned rotations, Lorentz transformations, and various discrete symmetries. There were exceptions, of course, most notably Racah and his school, who already, in the 1940s, had successfully applied much more advanced group theoretical methods to atomic and nuclear spectroscopy. I well recall Racah's clear and interesting lectures in the spring of 1951 at the Institute for Advanced Study" [Pais 1986, 555]. But the mimeographed notes of these famous lectures (*Group Theory and Spectroscopy*, 1951) appeared in print only much later, in 1965. In fact S. Sternberg, who later became George Putnam professor of pure and applied mathematics at Harvard University, recalled that when he was a student in the early 1950s, "the basic facts of abstract group theory were part of the algebra course, but the theory of group representations was not included in the standard mathematics curriculum. My introduction to representation theory and its physical applications was at the hands of Prof. George W. Mackey. He gave a wonderful and justly famous course of lectures at the University of Chicago in the summer of 1955...". In 1962, when group theory was achieving a well-established status in theoretical physics, Sternberg was invited by Prof. Yuval Ne'eman to give a series of lectures on the topology of Lie groups at his seminar, then held at Nahal Soreq: "This was after the prediction of the existence of the Ω^- particle (by Gell-Mann and by Ne'eman on the basis of $SU(3)$ symmetry in 1961)", pointed out Sternberg, "but before its momentous discovery at Brookhaven National Laboratory in 1964." [Sternberg 1994, xi]. In his autobiography, Jeremy Bernstein reported that "Murray Gell-Mann in the fall of 1960 had to be told by a mathematician that the 'cute' objects he was playing with in the early development of current algebra were well known to mathematicians as Lie groups and had an elaborate theory" [Bernstein 1987, 163](113).

By that time symmetry principles, since their official appearance with Einstein's identification of the invariance group of space and time, had taken on a character in physicists'

(113) Already in 1958 Pauli gave lessons on continuous groups and representations in Berkeley and in Geneva. During the 1960s many review papers appeared devoted to group theory [Behrends *et al.* 1962; Biederharn 1963; De Franceschi and Maiani 1965; McVoy 1965]. Physicists of that generation could learn group theory also from new books such as Lyubarskii (1960), Heine (1960), Hamermesh (1962), Meijer and E. Bauer (1965), H. Bacry (1967). The book on group theory all chemists had to go through in the 60s, 70s and 80s was Cotton's *Chemical Applications of Group Theory* (1963).

minds as *a priori* principles of universal validity, expressions of the simplicity of nature at its deepest level(114).

21. – Weyl's revenge

It was only after the advent of quantum theory that the full significance of gauge theory, whose first source was Weyl's unsuccessful attempt to unify electromagnetism and gravity, emerged. In 1927 Fritz London pointed out in his article "Quantenmechanische Deutung der Theorie von Weyl" on the quantum-mechanical interpretation of Weyl's theory [London 1927](115) that although Weyl's use of his original idea was indeed incorrect, it "contained a much wider range of possibilities than was actually used by its author, and... contained nothing less than a logical path to wave-mechanics, and from this point of view had an immediate physical meaning": the correct application was a wave-mechanical phase factor. In fact London proved the non-integrable *scale* factor originally introduced by Weyl to be a non-integrable *phase* factor, if the identification $\phi_\mu = -\frac{ie}{\hbar c}A_\mu$ was made inserting a $-i$ ($i = \sqrt{-1}$), derived from quantum mechanics, in Weyl's original expression $\phi_\mu = (\text{constant})A_\mu$(116). But this insertion, although trivial formally, has profound physical consequences: the non-integrable scale factor which Weyl about ten years before had applied to the metric, should now change to a phase factor and be acquired by the wave function, in the presence of an electromagnetic field: $\psi \to \exp\left[-i\frac{e}{\hbar c}\int A_\mu \mathrm{d}x^\mu\right]\psi$.

London commented about Weyl's first paper of 1918: "In a logical extension of the geometrical interpretation of gravitation as the curvature of Riemannian space-time, Weyl sought to interpret the remaining physical interaction, the electromagnetic field, through the scale-relationships of the space-time, characterized by the variability of the gauge-measure." London was referring to the fact that in the case of "gravitational theory it was a physical fact, the equivalence of gravitational and inertial mass, that led Einstein to his geometrical interpretation. In the theory of electromagnetism no such fact was known... On the contrary, atomic clocks, for example, represent measuring-rods, whose independence of their history is shown by the sharpness of the spectral lines", in contradiction to the non-integrable measures assumed by Weyl, as Einstein had remarked in a postscript added to the paper. "In the face of such elementary experimental evidence", continued London, "it must have been an unusually strong *metaphysical conviction* [emphasis added] that prevented Weyl from abandoning the idea that Nature would have to make use of the beautiful geometrical possibility that was offered". London commented about Weyl's first paper of 1918: "One can only admire the colossal boldness which led Weyl, on the basis of this purely formal correspondence alone, to his gauge-geometrical interpretation of electromagnetism" [O'Raifeartaigh 1997, 95].

(114) The global space-time invariance principles are intended to be valid for all the laws of nature, for all the processes that unfold in the space-time. This universal character is not shared by the physical symmetries that were next introduced in physics. Most of these were of an entirely new kind, with no roots in the history of science, and in some cases expressly introduced to describe specific forms of interactions —whence the name "dynamical symmetries" due to Wigner [Wigner 1967].

(115) An English translation and a discussion of London's paper can be found in [O'Raifeartaigh 1997]. For a thorough analysis of Weyl's work see [Straumann 2001]; for a broad introduction to Weyl's work in the mathematical science and philosophy see [Coleman and Korté 2001].

(116) In quantum mechanics momentum becomes a differential operator changing from p_μ to $-i\hbar\partial_\mu$ so that Fock and London in 1927 pointed out that the quantity $p_\mu - \frac{e}{c}A_\mu$ should be replaced similarly by $-i\hbar\partial_\mu - \frac{e}{c}A_\mu = -i\hbar(\partial_\mu - \frac{ie}{\hbar c}A_\mu)$.

In 1983 Chen Ning Yang revisited the Weyl-Einstein controversy in the light of London's reinterpretation. He relooked at Einsteint's objection: "When the two clocks come back, because of the insertion of the factor $-i$, they would not have different scales but different phases. That would not influence their rates of time-keeping. Therefore, Einstein's original objection disappears". Changing the idea of a scale factor to a phase factor, the theory really became the theory of electromagnetism in quantum mechanics [Yang 1986, 18](117).

The reinterpretation of his original theory introduced by these developments induced Weyl in 1929 to propose, in the part of his paper which he considered the most fundamental one, that the *gauge* invariance be used as a *principle* to derive the electromagnetic theory [Weyl 1929 and 1929a]:

> "It is now very agreable to see that this principle [gauge-invariance] has an equivalent in the quantum-theoretical field equations which is exactly like it in formal respects; the laws are invariant under the simultaneous replacement of ψ by $e^{i\lambda}\psi$, ϕ_μ by $\phi_\mu - \frac{\partial\lambda}{\partial x_\mu}$, where λ is an arbitrary real function of position and time. Also the relation of this property of invariance to the law of conservation of electricity remains exactly as before..."

In doing so Weyl was inaugurating a way of thinking that would characterize the future style of theoretical physicists: deriving the form of interactions from the demand of invariance under a group of local transformations. What he had proposed for the already well-established electromagnetism (invariance under the group of $U(1)$ phase transformations) later turned out to be the common principle underlying all the known fundamental interactions, even if the full significance of his proposal that *gauge* invariance be elevated from the rank of a symmetry to that of a fundamental principle of physics would be really recognized after a long process during which these seminal ideas would have to become part of the theoretical physics culture. The right use of mathematical formalism had shown its power in giving a profound insight in the foundations of physical theories. Even Pauli, the most formidable wicked tongue of the physicists' community, was now obliged to admit, even with some touch of irony, that Weyl had definitely succeded in blurring the boundaries between mathematics and physics, and between mathematicians and physicists, much in the spirit of Hilbert and Minkowski, as he wrote on August 26: "...hier muß ich Ihrer Tätigkeit in der Physik Gerechtigkeit widerfahren lassen. Als Sie frührer die Theorie mit $g'_{ik} = \lambda g_{ik}$ machten, war dies reine Mathematik und unphysikalisch, Einstein konnte mit Recht kritisieren und schimpfen. Nun ist die Stunde der Rache für Sie gekommen!" (I must admit your ability in Physics. Your earlier theory [1918 paper] with $g'_{ik} = \lambda g_{ik}$ was pure mathematics and unphysical. Einstein was justified in criticizing and scolding. Now the hour of your revenge has arrived!)

According to Dirac's philosophy, which completely excluded the idea of model-building, the physical problem must be reduced to a mathematical one, and indeed Dirac had shown the great superiority of principle over the previous empirical method. In a sense, his relativistic theory of the electron was the first example of a theory built after the

(117) Y. Aharonov and D. Bohm in 1959 measured a phase difference through an interference experiment with electrons, which showed, independently of Weyl's theoretical demonstration, that electromagnetism has some meaning which had not been understood before [Aharonov and Bohm 1959]. For an analysis of its significance and its relationship with the identification of the non-integrable phase factor as the *essence* of electromagnetism see [Wu and Yang 1975].

rule of imposing certain symmetries (Lorentz invariance, in this case) independently of the phenomenological evidence. The next step along the way of using invariance prescriptions for constructing theories would consist in generalizing the gauge invariance used in electromagnetism to a form that could be used for the nuclear interactions: once understood that gauge invariance is phase invariance, the key idea is substituting the simple phase of complex numbers with a more complicated phase, namely an element of a Lie group.

The stage begun with Weyl's work culminated in non-Abelian gauge theories, such as Chen Ning Yang and Robert Lawrence Mills gauge theory, formulated in 1954, where they arrived to a correct structure for the nuclear interactions marrying local gauge symmetry with the isospin symmetry, the internal symmetry $SU(2)$ of which the proton-neutron system constitued a doublet [Yang and Mills 1954]([118]):

> "The conservation of isotopic spin points to the existence of a fundamental invariance law similar to the conservation of electric charge. In the latter case, the electric charge serves as a source of electromagnetic field; an important concept in this case is gauge invariance which is closely connected with (1) the equation of motion of the electro-magnetic field, (2) the existence of a current density, and (3) the possible interactions between a charge field and the electromagnetic field. We have tried to generalize this concept of gauge invariance to apply to isotopic spin conservation. It turns out that a very natural generalization is possible."

Together with symmetries such as parity and time reversal which took on a new significance after being rediscovered in the quantum context, new quantum symmetries emerged, such as charge conjugation [Heisenberg 1934]. In the meantime physicists had realized since the 1930's that there are internal symmetries, such as isospin conservation, having nothing to do with space and time, symmetries which are far from self-evident, and that only govern what are now called the strong interactions. The 1950's saw the discovery of another internal symmetry —the conservation of "strangeness"— which is not obeyed by the weak interactions, and even one of the supposedly sacred symmetries of space-time —parity— was finally found to be violated by weak interactions in 1956. Later, various internal symmetries grounded on invariances under phase changes of the quantum states, were described in terms of the unitary groups $SU(N)$.

In the second half of the 50s physicists pursued the idea that local gauge invariance of electromagnetic theory could be extended to the other fundamental interactions: not only strong interaction might well be a Yang-Mills theory, but also a deeper understanding of the nature of weak interactions appeared to point to the same Yang-Mills mechanism; moreover both interactions appeared to be connected through the Noether theorem. But the route to the solution of these problems was still long, and the seminal work of Yang and Mills of 1954, initially regarded as a formal curiosity, became a "dominant paradigm" only in the 1970s, when it was recognized that in fact gauge fields are connections on fiber bundles, a theory which mathematicians had developed out of Elie Cartan's work of the 1920s [Yang 1980]. As coordinate-transformation invariance gives rise to general rel-

([118]) Yang had thoroughly studied Pauli in *Handbuch der Physik* of 1933: "I was very much impressed with the idea that charge conservation was related to the invariance of the theory under phase changes and even more impressed with the fact that gauge-invariance determined all the electromagnetic interactions, ideas which I later found out were originally due to Weyl [Yang 1986]. In that same period Yang and Mill's line of thought was further generalized by Utiyama in an independent work [Utiyama 1956].

ativity, Abelian gauge symmetry gives rise to electromagnetism, and non-Abelian gauge symmetry gives rise to non-Abelian gauge fields, which in 1967 was at the base of Steven Weinberg and Abdus Salam independent models for a unified theory of of the weak and electromagnetic interactions, based on the gauge group $SU(2) \times SU(1)$, and appealing to the key concept of spontaneous *broken symmetry*([119]). The basic structure of these theories is no longer the four-dimensional space-time manifold, but a fiber bundle whose fiber is the $SU(2) \times SU(1)$ group acting on the isospin and hypercharge variables. Invariance under the non-Abelian gauge group expresses the arbitrariness in the choice of the space-tme as well as of the internal variables and represents the non-Abelian generalization of the arbitrariness in the choice of the phase of the charged-particle fields. Lie's group-algebras with Weyl's gauge field would be definitely syntetized later by quantum chromodynamics (QCD), a theory of strong interactions with gauge group $SU(3)$, the colour group relating quark colour multiplets([120]). Taken together these two models constitute what is now known as the *Standard Model* of particle physics, based on the gauge group $SU(3) \times SU(2) \times SU(1)$, and covering simultaneously all three types of interactions, but containing a large number of free parameters, notably the masses of the fundamental fermions (quarks and leptons). Important questions remain, in fact, that are not addressed in the Standard Model. These include the unification of the electroweak and strong interactions (possibly including gravity), the origin of quark and lepton masses, the source of the baryon asymmetry of the Universe, and the nature of its unseen matter and energy density. Some model extensions devoted to these problems have been proposed, and concrete evidence for physics beyond the Standard Model including neutrino masses, cosmic microwave background radiation, dark matter, and "dark energy" are now object of active experimental researches.

22. – Dirac and Weyl: antimatter enters the stage

In the first section of his 1929 seminal paper *Elektron und Gravitation* [Weyl 1929] Weyl had introduced the concept of two-component spinors: "Die Dirachsche Theorie, in welcher das Wellenfeld des Elektrons durch ein Potential ψ mit vier Komponenten beschrieben wird, gibt doppelt zu viel Energieniveaus; man sollte darum, ohne die relativistische Invarianz preiszugeben, zu den zwei Komponenten der Paulischen Theorie zurückkehren können. Daran hindert das die Masse m des Elektrons als Faktor enthaltende Glied der Diracschen Wirkungsgröße." (The Dirac theory, in which the wave-field of the electron is described by four components gives twice too many energy levels; one should therefore return to the two-component Pauli theory, without sacrificing relativis-

([119]) In this theory, what is originally a system of four massless vector bosons becomes, through the spontaneous breakdown of the ground state, a system of one massless particle (the photon), which corresponds to the unbroken $U(1)$ subgroup, and three massive particles (the Z^0 and the $W^{\pm}$) corresponding to the broken part of the group. Renormalizability of the electroweak was definitely secured in 1971 by 't Hooft and Veltman, and in the following decade the existence and properties of the weak neutral current and the Z^0 and $W^{\pm}$ bosons required by the gauge invariance were confirmed with amazing accuracy.
([120]) As F. Dyson wrote in 1983: "Quantum chromodynamics, the most fashionable theory of the particle physicists in 1981, is conceptually little more than a synthesis of Lie's group-algebras with Weyl's gauge fields... A fashionable idea, invented for a purpose which turns out to be ephemeral, survives a long period of obscurity and emerges finally as a corner-stone of physics" [Coleman and Korté 2001, 321].

tic invariance. But that is prevented by the fact that the electron mass appears in the Dirac action.) [Weyl 1929, 330]. Weyl discussed the question of parity and time-reversal invariance P and T for 2-spinors in Minkowski space —"Nur diese tatsächlich in der natur bestehende Symmetrie von rechts und links wird uns zwingen (Teil II), ein zweites Paar von ψ-Komponenten einzuführen." (It is only the fact that left-right symmetry actually appears in Nature that forces us (Part II) to introduce a second pair of ψ-components.) [Weyl 1929, 333]. He proposed the identification of the negative-energy states with protons: "Es is naheliegend, zu erwarten, daß von den beiden Komponentenpaaren der Diracschen Größe das eine dem Elektron, das andere dem Proton zugehört." (It is plausible to anticipate that, of the two pairs of components of the Dirac field, one should belong to the electron and the other to the proton.) [Weyl 1929, 332]. At that time physicists felt pretty sure that the electrons and the protons were the only elementary particles in Nature. The proton-electron model of the nucleus was far from being abandoned, notwithstanding the problems deriving from "nuclear electrons", so that the "neutron" about which Rutherford had speculated in his Bakerian Lecture of 1920 still existed only as an electron-proton composite. Weyl thought that the mass problem would eventually solved by gravitation, so he attributed the need for a Dirac 4-component theory to parity.

Weyl's two-component theory did not satisfy left-right symmetry, so later was explicitly rejected as "inapplicable to physical reality" (auf die physikalische Wirklichkeit nicht anwendbar) by Pauli [Pauli 1933, 226], whose strong belief in the strict validity of symmetry principles had in the meantime suggested him to propose the existence of a new particle, the neutrino, to save conservation laws in beta decay. According to Pauli, Weyl's equation was contrary to all the evidence from atomic spectra, which pointed to a universal symmetry in natural laws with respect to the operation of space-reflection([121]).

In 1956, when it became clear that parity conservation was violated in weak interactions, and CP used in substitution of P, interest in two-component theories revived, and Weyl's idea was immediately re-looked at [Salam 1957; Lee and Yang 1957; Landau 1957; Gatto 1957]([122]). Unfortunately Weyl did not live to witness the experimental demonstration that nature is actually not, after all, symmetrical under space-reflection. Since January 1957 we also know that charge conjugation is not an exact symmetry of the laws of nature.

The revised edition of *Gruppentheorie* [Weyl 1931] contained a lot of novelties people were not completely accustomed to. The second quantization of the Maxwell and Dirac fields, was first exposed for a larger audience with a particularly detailed analysis of the discrete symmetry transformations C, P, T and CPT applied to Dirac's relativistic theory of the electron [Dirac 1928]. A joking comment made by Valentine Telegdi in a

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([121]) "For Pauli, the invariants in physics were the symbols of ultimate truth which must be attained by penetrating through the accidental details of things", as de Laer Kronig and Weisskopf wrote in the Preface of Pauli's Collected Works [Kronig and Weisskopf 1964, viii]. For Pauli symmetry principles and conservation laws took precedence over everything else, and in fact he clearly expressed his views in favour of the neutrino in the following terms: "I believe that the analogy between the conservation laws of energy and electrical charge has a profound significance and can act as a reliable guide. If energy conservation is abandoned, the law of charge conservation can hardly be maintained" [Kronig and Weisskopf 1964, 439].

([122]) After parity violation was proposed in 1956, Salam was the first to use the 2-component theory for the neutrino. Like Weyl, Salam was not spared by Pauli's well-known sarcastic reaction: "Give my greetings to my friend Salam and tell him to think of something better" [Salam 1980, 527].

different context will help in illustrating the anticipatory character of Weyl's work: "Very few people knew in 1956 whether TCP was a symmetry or a gasoline additive" [Telegdi 1983, 444](123).

One of the great difficulties of Dirac's wave equation was, from the very beginning, the existence of states of negative energy, in addition to states of positive energy. The difficulty "is a general one appearing in all relativity theories, also in the classical theory", remarked Dirac in his "Theory of Electrons and Protons" [Dirac 1930a, 360]. Moreover, according to a quantum-mechanical relativistic theory one would expect all electrons to jump from states of positive energy into these negative states, contrary to experience. Dirac had the boldness to argue that these states, and the transitions to them, cannot be disregarded as nonphysical, but are to be assumed to exist, and to be interpreted in a way that agrees with experiment. The theory also predicted that the unoccupied negative-energy states, the so-called "holes" created by negative-energy electrons raised to a state of positive energy, would behave like a particle with a positive energy and a positive charge. In quoting Weyl's paper of 1929 [Weyl 1929, 332], Dirac commented: "This result has led people to suspect a connection between the negative-energy electron and the proton or hydrogen nucleus. One cannot, however, simply assert that a negative-energy electron *is* a proton", remarked Dirac, as this would lead to a series of paradoxes, and he concluded this discussion observing that "no particles of this nature have ever been observed". As an alternative Dirac assumed that all of the negative-energy levels were normally occupied by an infinite number of electrons, thus introducing what was later called the "Dirac sea". The Pauli exclusion principle prevented an electron with positive energy from falling into a negative-energy state: "We are therefore led to the assumption that *the holes in the distribution of negative-energy electrons are the protons.* When an electron of positive energy drops into a hole and fills it up, we have an electron and proton disappearing together with emission of radiation" [Dirac 1930a, 363]. It seemed that the hole should be identified with the proton, the only fundamental particle that was known to exist with a positive charge. "Can the present theory account for the great dissymmetry between electrons and protons, which manifests itself through their different masses and the power of protons to combine to form heavier atomic nuclei?" To this concern Dirac immediately added the following obvious observation: "It is evident that the theory gives, to a large extent, symmetry between electrons and protons. We may interchange their rôles..." [Dirac 1930a, 364]. "The symmetry is not, however", continued Dirac, "mathematically perfect when one takes interaction between the electrons into account... The consequences of this dissymmetry are not very easy to calculate on relativistic lines, but we may hope it will lead eventually to an explanation of the different masses of proton and electron." In his later recollections Dirac claimed that his first inclination was actually toward "particles having a positive charge instead of the negative charge of the electron, and also having the same mass as the electron" [Kragh 1990, 96]. On September 8 1930 Dirac gave a talk with the title "The proton" at the Bristol meeting of the *British Association*: "It has always been the dream of philosophers to have all matter built up from one fundamental kind of particle, so that it is not altogether satisfactory to have two in our theory. There are, however, reasons for believing that the electron and proton are really independent, but are just two manifestations of one elementary kind of particle. This connexion be-

(123) In 1964 also CP and T invariance were proved to be violated. It is well known that the theorem formulated by Schwinger, Lüders and Pauli showed that the invariance of a system under the combined CPT symmetry: right-left, particle-antiparticle, past-future, must hold.

tween the electron and proton is, in fact, rather forced upon us by general considerations about the symmetry between positive and negative electric charge" [Dirac 1930b, 605].

In the preface to the second German edition of his *Gruppentheorie* [Weyl 1931], written in November 1930, Weyl, too, was quite concerned about the dissimilar nature of positive and negative electricity, and the identification of the "hole" with the proton in the negative-energy sea: "Das Problem von Proton und Elektron wird im Zusammenhang mit den Symmetrieeigenschaften der Quantengesetze gegenüber den Vertauschungen von rechts und links, Vergangenheit und Zukunft, positiver und negativer Elektrizität augerollt. Im Augenblick ist keine annehmbare Lösung sichtber; ich fürchte, daß sich hier die Wolken zu einer neuen ernsten Krise der Quantenphysik zusammenballen" (The fundamental problem of the proton and the electron has been discussed in its relation to the simmetry properties of the quantum laws with respect to the interchange of right and left, past and future, and positive and negative electricity. At present no [acceptable] solution of the problems seems in sight; I fear, that the clouds hanging over this part of the subject will roll together to form a new [serious] crisis in quantum physics) [Weyl 1949, x](124). Weyl analyzed the invariance of the Maxwell-Dirac equations under the discrete symmetries that now correspond to the discrete symmetry transformations called C, P, T and CPT, both for the case of Relativistic Quantum Mechanics and for the case of Relativistic Quantum Field Theory(125). It is remarkable that he should even consider the possibility of time reversal and parity violation at a time when, as Chen Ning Yang has put it: "Nobody, absolutely nobody, was in any way suspecting that these symmetries were related in any manner. It was only in the 1950's that the deep connection between them was discovered. . . " and he added: "What had prompted Weyl in 1930 to write the above passage is a mystery to me" [Yang 1986, 10](126). Here Yang was referring to Weyl's cited discussion of the "symmetry properties of the quantum laws with respect to the interchange of right and left, past and future, and positive and negative electricity"(127). Actually, Weyl's analysis was indeed a very systematic one. In the section "Interchange of Past and Future" he discussed the Maxwell-Dirac theory under a transformation involving time reflection, space reflection and charge reflection, "similar in spirit to what is now called the CPT transformation"(128). In connection

(124) The words "acceptable" (annehmbare) and "serious" (ernst) are missing in the English translation.

(125) In 1932 Wigner wrote a paper [Wigner 1932] presenting the first correct analysis of time reversal in terms of an antilinear, antiunitary operator in the context of non-relativistic quantum mechanics, contrary to Weyl, who considered time reversal as linear and unitary.

(126) As is well known, T. D. Lee and C. N. Yang shared the 1957 Nobel prize for physics for their proposal of a parity breaking in weak interaction, experimentally verified by Madame C.S. Wu. It was the first demonstration that one of the fundamental interactions of nature displays an inherent asymmetry.

(127) In fact a discussion on the connection between past and future on the one side, and positive and negative electricity on the other, can be already found in a letter of reply to Pauli written by Weyl from Zürich on May 10, 1919. In the turn of a few days Pauli sent to the *Physikalische Zeitschrift* his work "Zur Theorie der Gravitation und der Elektrizität von Hermann Weyl". In his paper Pauli recalled this exchange of views which had continued in a second letter, sent by Weyl on December 9 of that same year [Pauli 1919]. These discussions stemmed from Weyl's famous attempt of a mathematical generalization of Einstein's theory in his paper *Gravitation und Elektrizität* of 1918.

(128) Weyl's discussion of the discrete symmetries that are now called C, P, T and CPT is thoroughly analyzed in [Coleman and Korté 2001].

with charge conjugation he could claim that [Weyl 1949, 225]:

> "We denote such a particle, whose mass is the same as that of the electron but whose charge is e instead of $-e$, as a "positive electron"; it is not observed in Nature! It follows from what has been said above that the energy levels of such a particle are $-h\nu$, where $h\nu$ are those of the negative electron. Disregarding this difference in sign, the two particles behave the same. *The electon will possess, in addition to its positive energy levels, negative ones as well*, the latter arising from the positive energy levels of the positive electron on changing signs as above. Obviously something is wrong here; we should be able to get rid of these negative energy levels of the electron. But that seems impossible, for under the influence of the radiation field transitions should occur between the positive and negative terms. That we have twice as many terms as we should is obviously related to the fact that our quantity ψ has *four* instead of *two* components (satisfying first-order differential equations). The solution of this difficulty would seem to lie in the direction of interpreting our four differential equations as including the proton in addition to the electron".

At the end of the section "Quantization of the Maxwell-Dirac Field Equations" [Weyl 1949, 262] Weyl definitely tackled the problem of interpreting "the presence or the absence of a *proton* in the state of positive energy μ as the absence or the presence, respectively, of an electron in the corresponding negative energy state $-\mu$" as Dirac had at last proposed in his hole theory. After introducing "the quantum number n_μ, which corresponds to the characteristic values μ and which may assume only the values 0 and 1, in addition on the numbers N_ν of photon of the various frequencies ν", Weyl recalled that,

> "The dynamical law allows only those quantum jumps of the particles in which *one* n_μ falls from 1 to 0 and another $n_{\mu'}$ jumps at the same time from 0 to 1. Consequently the total number of particles $\sum_\mu n_\mu$ and therefore the charge, remains fixed...Remembering that the numbers $n_\mu = 0, 1$ were at first introduced merely as an arbitrary index indicating the rows of a matrix, there is nothing to prevent us from replacing the numbers $n_{-\mu}$ for negative $-\mu$ by $n_\mu^- = 1 - n_{-\mu}$ keeping $n_\mu^+ = n_\mu$ for positive μ. The theorem of the conservation of charge is then $\sum n_\mu^+ - \sum n_\mu^- =$ const $(\mu > 0)$. But we thereby alter the content, as well as the notation, of the theory; we are now interested in that part of the dynamical equations in which *only a finite number of n_μ with negative μ are different from* 1! The quantum jump of an electron between positive and negative energy levels, which was so undesiderable in the Dirac theory as formulated in the previous section, now appears as a process in which an electron and a proton are simultaneously destroyed and as the inverse process."

The total charge remain fixed, even if the total number of particles does not, so that the simultaneous changes from 1 to 0 or from 0 to 1 implied by the above relations, as correctly interpreted by Weyl, are thus nothing else than pair creation and pair annihilation. He commented that

> "However attractive this idea may seem at first it is certainly impossible to hold without introducing other profound modifications to square our theory with the observed facts. *Indeed, according to it the mass of the proton should be the same as the mass of the electron* [emphasis added]; furthermore, no matter how the action

> is chosen (so long as it is invariant under interchange of right and left), this hypothesis leads to the essential equivalence of positive and negative electricity under all circumstances —even on taking the interaction between matter and radiation rigorously into account."

Weyl concluded the section discussing the charge-conjugation invariance([129]) for the quantized Maxwell-Dirac equations [Weyl 1949, 264]:

> "In view of Dirac's theory of the proton this means that positive and negative electricity have essentially the same properties in the sense that the laws governing them are invariant under a certain substitution which interchanges the quantum numbers of the electrons with those of the protons. The dissimilarity of the two kinds of electricity thus seems to hide a secret of Nature which lies yet deeper than the dissimilarity of past and future".

It is worthwhile noting that Weyl's thoroughly mathematical approach to physics, but may be also a lack of inhibition, which at last made him disregard the restrictions set by current empirical knowledge, was behind his recognition that the "holes" of Dirac's theory could not be protons as Dirac had at last proposed, maybe, one could say *a posteriori*, under the fear of introducing a new unobserved particle, as he recalled later [Dirac 1983]:

> "When I first got this idea [the holes as particles with positive energy and positive charge] it seemed to me that there ought to be symmetry between the holes and the ordinary electrons, but the only positively charged particles known at that time were the protons; so it seemed to me that these holes had to be protons. I lacked the courage to propose a new kind of particle... I was soon assailed by other physicists on the grounds that there could not be this difference between the mass of the new particles, the holes, and the mass of ordinary electrons. The person who most definitely came out against it was Hermann Weyl; he was essentially a mathematician and was not so much distrubed by physical realities but was very much dominated by mathematical symmetries. He said quite categorically that the new particles formed by these holes would have to have the same mass of the electrons."

It is remarkable that at that time the neutrino hypothesis had not yet officially appeared, and even when Pauli ventured the conjecture of this new particle, he considered it a real " act of despair"([130]).

Later Dirac repeatedly pointed out that he had not been sufficiently faithful to the power of his wave equation, perhaps lacking Weyl's boldness to rely only on pure mathematical reasoning:

> "Weyl was a mathematician. He was not a physicist at all. He was just concerned with the mathematical consequences of an idea, working out what can be deduced from the various symmetries. And this mathematical approach led directly to the conclusion that the holes would have to have the same mass as the electrons.

([129]) Actually the transformation considered by Weyl is slightly different from the transformation now called C [Coleman and Korté 2001, 306].

([130]) It appears that the earliest reference to the neutrino idea is Heisenberg's mention of "your neutrons" in a letter to Pauli dated 1 December 1930 [Pais 1986, 315]. The well-known letter to the "radioactive ladies and gentlemen" bears the date 4 December of that same year.

> Weyl ... did not make any comments on the physical implications of his assertion. Perhaps he did not really care what the physical implications were. He was just concerned with achieving consistent mathematics" [Dirac 1971, 55].

In reviewing the 1931 edition of Weyl's *Gruppentheorie* at the beginning of March Heisenberg concluded by remarking : "On the whole, the second edition is even more suitable than the first in teaching physicists the deepest mathematical connections of their theories, and in providing mathematicians with a vivid picture of their abstract formalisms" [Heisenberg 1931].

In May 1931 Dirac, convinced by Weyl's argument, explicitly withdrew the proton hypothesis: "It was shown that one of these holes would appear to us as a particle with a positive energy and a positive charge and it was suggested that this particle should be identified with a proton. Subsequent investigations, however have shown that this particle necessarily has the same mass as an electron" —here he referred to Weyl's book, particularly to page 234 of the 2$^{\text{nd}}$ German edition—, and introduced for the first time "a new kind of particle, unknown to experimental physics, having the same mass and opposite charge of the electron. We may call such a particle an anti-electron". Dirac also mentioned the possibility that an "encounter between two hard γ-rays (of energy at least half a million volts) could lead to the creation simultaneously of an electron and an anti-electron" [Dirac 1931, 61].

The following year, 1932, the April conference at Bohr's Institute in Copenhagen ended with the famous parody of *Faust*, celebrating the recent discovery of the neutron, with plenty of jokes about the many problems of Dirac's electron theory —donkey electrons, as they were called, because they did not behave properly. In August Anderson announced that he and Seth Neddermeyer had observed the tracks which seemed "to be produced by positive particles, but if so the masses of these particles must be small compared to the mass of the proton" [Anderson 1932]; but only in March 1933, abandoning what he later called "the spirit of scientific conservatism", Anderson suggested that he had discovered a positive-charged electron, which he named "positron".

In his book *Der Teil und das Ganze* Heisenberg reminded how Einstein had expressed his doubts about Heisenberg's opinion, which he had closely shared with Pauli, that a theory should contain only statements about facts that are observable in principle: "it may be heuristically useful to keep in mind what one has actually observed..., on principle, it is quite wrong to try founding a theory on observable magnitudes alone. In reality the very opposite happens. It is the theory which decides what we can observe" [Heisenberg 1973, 63]([131]). In fact the concept of antimatter originated from Dirac's equation received its sensational confirmation that same year with the announcement of Patrick Blackett and Giuseppe Occhialini. In quoting both Anderson and Dirac, they reported that "negative and positive electrons may be born in pairs during the disintegration of light nuclei" [Blackett and Occhialini 1933]. It was the first discovery of a new particle explicitly foreseen by theory. On October 9 Heisenberg wrote to Bohr: "What do you say about positive electrons? It seems indeed that Dirac is much more right than we thought so far"([132]).

Mathematics as a language of physics had been used for the first time to extrapolate

([131]) Actually Pauli had claimed something quite similar to Heisenberg's view in his 1921 survey of general relativity that "we should hold fast to the idea that in physics only quantities that are in principle observable should be introduced" [Pauli 1958, 206].

([132]) In fact this does not mean that the whole world acclaimed. Bohr, for example, was very beyond the confines of known experimental results to predict the results of specific, theoretically visualizable, yet still unrealized experimental conditions. Rutherford's characteristic remark was: "It seems to a certain degree regrettable that we had a theory of the positive electron before the beginning of experiments... I would be more pleased if the theory had appeared after the establishment of the experimental facts" [Skobeltzyn 1983, 116].

In his celebrated paper of 1931, "Quantised Singularities in the Electromagnetic Field", before introducing the anti-electron on pure theoretical grounds, Dirac expressed his views about the future work in theoretical physics [Dirac 1931]:

> "The steady progress of physics requires for its formulation a mathematics which is continuously more advanced... actually the modern physical developments have required a mathematics that continually shifts its foundations and gets more abstract. Non-Euclidean geometry and non-commmutative algebra, which were at one time considered to be purely fictions of the mind and pastimes for logical thinkers, have now been found to be very necessary for the description of general facts of the physical world. It seems likely that this process of increasing abstraction will continue in the future and that advance in physics is to be associated with a continual modification and generalisation of the axioms at the base of the mathematics rather than with a logical development of any one mathematical scheme on a fixed foundation."

On his side, in the introduction to the *Theory of groups and quantum mechanics,* Weyl expressed "once and for all, my deep respect for the work of the experimenter and for his fight to wring significant facts from an inflexible Nature, who says so distinctly "No" and so indistinctly "Yes" to our theories" [Weyl 1949, xx].

23. – On the heels of Majorana: Wigner and the unitary representations of the Poincaré group

In the first lines of the foreword to the second German edition [Weyl 1931] of *Gruppentheorie und Quantenmechanik* (Göttingen, November 1930) Weyl had also observed: "It has been rumoured that the "group pest is gradually being cut out of quantum physics. This is certainly not true in so far as the rotation and Lorentz groups are concerned...", and indeed one of Wigner's most celebrated papers is entitled "On unitary representations of the inhomogeneous Lorentz group" published in 1939 in the *Annals of Mathematics* [Wigner 1939]. It represented a great conceptual leap: from the classification of the symmetries of atomic states to the classifications of laws of quantum-mechanical evolution invariant under Einsteinian relativity. Wigner's seminal paper, extending Frobenius method to obtain the representations of the inhomogeneous Lorentz group, or Poincaré group (proper spatial rotations and translations, time translations

hesitant about the connection with Dirac's theory of holes. At the Solvay Conference of October 1933, during the discussion following Joliot-Curie's report on artificial radioactivity, he stressed that the best thing to do was "to draw as many conclusions as possible from the positive-electron experiments without relying on the Dirac theory". Even Anderson commented later "Yes, I knew about the Dirac theory... but I was not familiar in detail with Dirac's work. I was too busy operating this piece of equipment to have much time to read his papers... [Their] highly esoteric character was apparently not in tune with most of the scientific thinking of the day... The discovery of the positron was wholly accidental". (Interview with C. D. Anderson by C. Weiner (June 30, 1966, Niels Bohr Library, American Institute of Physics)).

and pure Lorentz transformations – boosts), is the culmination of the group-theoretical approach to relativity, where the physical caracteristics, *rest mass* and *intrinsic spin*, arise as parameters in the description of irreducible representations.

An elementary particle is an object whose nature does not change when it is translated in space or time, when it is rotated or seen from an observer in uniform motion relative to it. These considerations led Wigner to postulate that the quantum mechanical states of such a particle should belong to a Hilbert space carrying a certain representation of the Poincaré group, obtained by combining Lorentz transformations and translations, and whose elements represent isometries of Minkowski space. Thus, the very definition of an elementary particle is based on the geometric symmetries and so is the whole classification of particles according to their mass and spin.

The idea was to exploit the fact that the state of every relativistically invariant quantum-mechanical system is a unit vector in Hilbert space. Any symmetry group of the system manifests itself as a unitary (or anti-unitary) transformation of this Hilbert space, so that the state is intrinsically associated with a unitary representation (or projective representation) of the Poincaré group as in the simplest possible special case —that in which the system consists of a single particle moving freely in space. The Poincaré group is not compact and its unitary representations are infinite dimensional. Wigner posited that an elementary particle "is" an irreducible unitary representation of the Poincaré group; computing all the irreducible representations of the group Wigner found that those satisfying certain physically reasonable restrictions ($m^2 \geqslant 0$) could be parametrized by two parameters m and s, where m (a continuous parameter) is a non-negative real number and where s (a discrete parameter) is costrained by $s = 0, 1/2, 1, 3/2, \ldots$ if $m^2 > 0$ and $s = 0, \pm 1/2, \pm 1, \pm 3/2, \ldots$ if $m^2 = 0$. Thus rest mass and spin (or spin and helicity if $m = 0$) define the transformation properties under the Poincaré group, this means that the classification of such representations provides a classification of particles. Given any particular class, one can deduce a great deal about the quantum mechanics of particles in this class directly from the properties of the associated representation.

In high-energy physics the relevant group of space-time transformations is the Poincaré or inhomogenous Lorentz group of special theory. Wigner's work on the "Unitary representations of the inhomogeneous Lorentz group" which led to the possible form of equations and interactions in the elementary particle field not only provided a framework for the physical search for elementary particles, but at the same time constituted a key step in the purely mathematical program of developing a general theory of unitary group representations, much in the spirit of Weyl's 1927 work([133]). Wigner's scheme could not explain the observed values of the masses, and did not include electric charge, a

([133]) Much later his work stimulated I. M. Gel'fand and M. A. Naimark, as well as V. Bargman in their research aiming at determining all unitary irreducible representations of $SL(2, C)$ and $SL(2, R)$ etc., and this theory became highly developed. Wigner's later interest in nuclear physics, solid state, elementary particles *etc.*, was almost always under the banner of the role of symmetry in these problems. He devoted a considerable effort to generally comprehensible accounts of the significance of geometrical invariance principle, both in classical and quantum physics, in numerous well-known small and broad surveys, such as "Symmetry Principles in the Old and New Physics" [Wigner 1968]. His definitive pedagogical work on the general subject is contained in his review article with R. Houtappel and H. van Dam, "The conceptual Basis and Use of Geometric Invariance Principles" [Houtappel *et al.* 1965]. For Wigner's commitment to symmetry's role in quantum mechanics see [Wightman 1997]. For a lively account on the interactions between group theory and physics see [Coleman 1997].

conserved property of observed particles as well as mass and spin; several other invariant 'quantum numbers' were later necessarily introduced such as isotopic spin, strangeness, etc., and high-energy theorists effectively began to think of the elementary particles of the strong, weak, and electromagnetic forces as irreducible representations of the direct product of the Poincaré group and $SU(3) \times SU(2) \times U(1)$.

At the end of the introduction Wigner thanks Dirac, "his famous brother-in-law", for giving him the idea of writing this paper in 1929. In the same years a young Italian physicist was following with the deepest interest the applications of group theory to physics. Indeed, Wigner stated in §2 of his paper: "The representations of the Lorentz group have been investigated repeatedly. The first investigation is due to Majorana, who in fact found all representations of the class to be dealt with in the present work excepting two sets of representations". The first recognition, development and use of unitary infinite-dimensional representations of the Lorentz group —in fact the simplest of those representations as correctly claimed by the author— is contained in a paper published in 1932, but which appears to be written about two years before, by Ettore Majorana. Actually Majorana's pioneering paper "Teoria relativistica di particelle con momento intrinseco arbitrario" (Relativistic Theory of Particles with Arbitrary Intrinsic Angular Momentum) was the first attempt to develop a relativistically invariant linear theory for particles with arbitrary spin, both integer and semi-integer, in which all mass eigenvalues are positive [Majorana 1932]([134]). The importance of the achievements contained in this work were immediately recognized by van der Waerden as Majorana himself told in a letter written to his father from Leipzig (where he had been invited by Heisenberg) on February 18, 1933: "I will publish an extended version of my latest work appeared on 'Nuovo Cimento'; in this paper an important mathematical discovery is contained, as I could ascertain through a conversation with prof. Van der Waerden, a Dutchman teaching here, one of the greatest authorities in the field of group theory" [Recami 1991, 125]. In fact the paper Majorana had announced never appeared.

In order to avoid the embarrassing "negative energy" states of the Dirac's theory, Majorana was led to the infinite-dimensional representations of the Lorentz group. However, any interest in Majorana's theory was inhibited first of all by the discovery of the positron just shortly before its publication, which invested the negative mass eigenvalues of the Dirac electron theory with physical significance. His paper on the relativistic theory of arbitrary spin particles remained practically unknown after publication in 1932 also because it appeared only in Italian, and because it dealt with a problem that was premature with respect to the dominant interests of physicists at a time when even the neutron was a brand new discovery([135]). But in fact the Klein-Gordon equation, the Dirac equation, Weyl's neutrino equation (and Maxwell's equations) are all related to the appropriate irreducible representations of the Poincaré group, as Majorana had discovered, and Wigner later classified.

As Edoardo Amaldi, a former friend of Ettore Majorana since they were both Fermi's students in via Panisperna, later recalled: "In the early 30's in Rome, the person that was really very much interested in Group Theory was Majorana, who considered Weyl's book the best and deeper book on quantum mechanics. Once he mentioned to have started to

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([134]) For an accurate review of the Majorana paper see [Fradkin 1966].

([135]) The same problem was tackled by Dirac, Klein, Petiau and Proca in 1936, but none of these autors quoted Majorana's paper. Subsequent papers refer to Dirac and ignore Majorana's work completely, or almost completely. For Majorana's biography see [Amaldi 1966].

write a book on Group Theory, but after he disappeared nobody found any manuscript that could be considered as a draft or a part of such a book"([136]). Majorana's interest in group theory probably aroused at the beginning of 1928, during the first period of his deep friendship with Giovanni Gentile jr, a former pupil of the mathematician Luigi Bianchi in Pisa. Bianchi, a real expert in the subject, wrote an influential treatise, *Lectures on the theory of groups of substitution*([137]), which Majorana owned among his books, together with the first German edition of Weyl's *"Gruppentheorie"* and Andreas Speiser's *Theorie der Gruppen von endlicher Ordnung* (Berlin, 1923)([138]). From his personal notebooks we know that Majorana, as soon as he became Fermi's student, extensively studied Weyl's book, and certainly well knew Weyl's previous remarkable works published on the Zeitschrift für Physik([139]). Enrico Fermi owned the 1931 edition of Weyl's book([140]): "The only treatise that I know he read after he came to Rome was Weyl's *Gruppentheore und Quantenmechanik*; even if, according to Segrè, from as early as 1928 Fermi made little use of books [Segrè 1970, 56]. Fermi probably shared with Born the idea that the use of group theory was "like shooting sparrows with artillery", however, it is well known that "...although endowed with remarkable analytic powers, Fermi often affected an aversion to abstract mathematics", as Telegdi duly remarked [Telegdi 1992, 83]. The following anectode is also reported by Valentine Telegdi as an example of Fermi's humor about the matter: "At some point, Fermi decided to teach his private seminar Group Theory. He took out his index cards on that subject, and started to discuss, first *A*belian groups, then *B*urnside's theorem, then the *C*enter of the group, and only much later he got to the concept of group itself. Some of the students were a bit confused by this unorthodox approach. The master said, 'Group theory is merely a compilation of definitions'. Therefore he had simply followed the *index* at the end of Weyl's book" [Telegdi 1992, 85]([141]).

It is clear that, contrary to what Fermi and most physicists of the time thought, Majorana and Gentile were completely persuaded by the message contained in the introduction of Weyl's *Gruppentheorie*: "Group theory is of fundamental importance for quantum physics...it is just this part of quantum physics which is most certain of a lasting place" [Bonolis 2002a].

([136]) *Colloque International sur l'Histoire de la Physique des Particules*, *Journal de Physique* (Paris) (Suppl.) **43**, 2 (1982) 451.

([137]) Bianchi, who made important contributions to differential geometry, was also attracted by Lie's theory of continuous groups and the theory of groups of substitutions. He wrote *Lectures on differential geometry* (1894), *Lectures on the theory of groups of substitutions* (1900), *Lectures on the theory of continuous groups* (1918), *Lectures on the theory of functions of a complex variable* (1901).

([138]) Majorana had enrolled as engineer student in 1923, and followed these studies until the beginning of 1928, when he decided to change to physics urged by his fellow student Emilio Segrè, who had become the first of Fermi's students at the end of 1927. For Majorana's biography see also [Bonolis 2002].

([139]) Already starting from 1928/1929 Majorana's notebooks contain arguments such as Dirac matrices, Lorentz group, "characters of D_j and reduction of $D_j \times D'_j$", spinor transformations, and the infinite-dimensional unitary representations of the Lorentz group. To understand Majorana's deep interest in group theory, see also the English translation of his notebooks, the so-called "Volumetti" [Majorana 2003] and [Drago and Esposito 2004].

([140]) The book, signed by Fermi, can be found among the volumes of the Library of the Department of Physics of Rome University "La Sapienza".

([141]) The story was told Telegdi by T. D. Lee, a former Fermi's doctoral student in Chicago during late 1940's, who got his PhD in the Spring of 1950.

Gentile, too, owned Weyl's book together with van der Waerden's *Gruppentheoretische Methode in der Quantenmechanik.* In 1932 he had even prepared lecture notes for his course in theoretical physics in Pisa containing several paragraphs where group representation theory and its application to quantum mechanics were explained; indeed a complete novelty in the realm of teaching of modern physics, and certainly not only for Italy [Gentile 1933]. Majorana's manuscript lessons written for his course of quantum mechanics held in Naples in the first months of 1938, stopped at a chapter where the role of symmetries in classical and quantum mechanics is widely discussed. He disappeared in March of that same year.

Majorana's trend towards such cultural and scientific interests was to manifest itself fully in the paper "Symmetrical theory of the electron and the positron" [Majorana 1937]. In the first lines of the summary Majorana, in expressing his "philosophy" stated: "The possibility is demonstrated of reaching a full formal symmetrization of the quantum theory of the electron and the positron using a new quantization process. This is modifying somewhat the meaning of the Dirac equations in the sense that there are no more reasons either to talk about negative energy states or to presume the existence of 'antiparticles' corresponding to negative energy 'holes' for new types of particles, especially neutral ones". In stating this, Majorana raised a most important general problem in elementary-particle physics: the question about the true neutrality of electrically neutral fermions. A Dirac particle is one which is distinct from its antiparticle, in contrast Majorana formally introduced in 1937 the concept of a particle which is identical to its antiparticle.In his "Symmetrical theory of the electron and the positron", Majorana chose a particular representation, distinguished by the property that the four γ-matrices are pure imaginary, to manifest the complete symmetry of the quantized Dirac field theory between particles and antiparticles, and he suggested to apply it to "neutrons and hypothetical neutrinos". Actually, Majorana's two-component theory of the neutrino was a quantized field theory, while Weyl's original version of 1929 [Weyl 1929] was treated before Dirac's theory of holes and therefore as unquantized Dirac fields. According to Gian Carlo Wick, Majorana was already familiar with the second-quantization method at least since 1931([142]), and it looks like he had already formulated the core of his theory already by 1933, at the time of his stay in Leipzig, as Heisenberg recalled: "The Dirac spinor was the thing which everybody discussed, but then there was Weyl spinor business which van der Waerden knew. The others did not know about it, but then there was Majorana. He was in Leipzig and Majorana found his Majorana particle, which has no charge but still had the spin 1/2"([143]). Dirac's "holes", the antiparticles, seemed to be of quite different nature from

([142]) The following story was told by Gian Carlo Wick to Valentine Telegdi: "In 1931, he was together with Majorana and a couple of other people and Majorana said: To eliminate all these objections that people have against the Klein-Gordon equation, all one has to do is to consider it as field equation and go to second quantization and then you have positive and negative charged particles". Telegdi duly commented: "In other words, in 1931 Majorana already understood the whole of this business" [Telegdi 1983, 406].

([143])Interview with Werner Heisenberg by T. S. Kuhn (July 5, 1963, Niels Bohr Library, Archive for the History of Quantum Physics). It is quite reasonable that Majorana had worked on these problems also as a natural extension of his previous work about particles with arbitrary spin, and, more probably, as a parallel work in the same context. As I already conjectured [Bonolis 2002, 2002a], this can be deduced, in the initial analysis, from his correspondence, from what Wick told Telegdi, and particularly from Heisenberg's interview, which is undoubtedly very clear about this point.

the particles. In a more sophisticated quantum field theory approach this is not so; there is considerable symmetry between particle and antiparticle. By that time very few people appreciated the advantages of second quantization, while Majorana had fully grasped its necessity for the physical interpretation of the Dirac equation. Actually, it is still a fundamental and unresolved question whether the neutrino is a "Dirac" or "Majorana" particle.

In 1939 and 1940 Gentile returned on the same arguments treated by Majorana writing two very elegant works about the representations of the inhomogeneous Lorentz group, and a relativistic theory for particles with arbitrary spin, *à la* Majorana [Gentile 1939, 1940]. By that time representation theory of such groups was still unknown territory, and in fact Wigner's paper, one of the most quoted of the XX century, was refused for publication by several physics journals and even by one of the prestigious mathematics journals. Finally it was published in 1939 thanks to John von Neumann, then the editor of *Annals of Mathematics* [Kim 1995].

At the beginning of that same year, physicists were abruptly obliged to follow the chain of events which had started with the discovery of uranium fission, and Wigner himself got deeply involved in these researches in the United States.

Times were not ripe enough for group theory to become a tool for constructing theories.

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