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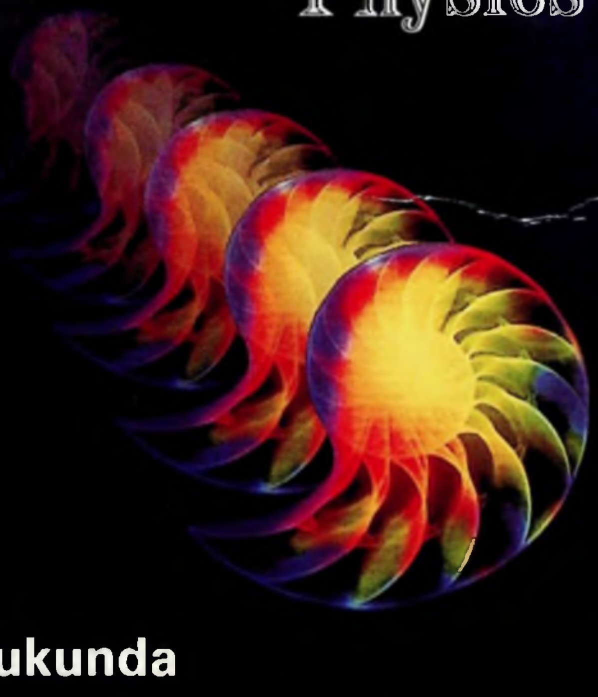

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Universities Press (India) Limited

Registered Office
3-5-819 Hyderguda, Hyderabad 500 029 (A.P.), India

Distributed by
Orient Longman Limited

Registered Office
3-6-272 Himayatnagar, Hyderabad 500 029 (A.P.), India

Other Offices
Bangalore / Bhopal / Bhubaneshwar / Calcutta / Chandigarh
Chennai / Ernakulam / Guwahati / Hyderabad / Jaipur
Lucknow / Mumbai / New Delhi / Patna

First published 2000
ISBN 81 7371 095 3

Typeset by
OSDATA
Hyderabad 500 029

Printed at
Orion Printers
Hyderabad 500 004

Published by
Universities Press (India) Limited
3-5-819 Hyderguda, Hyderabad 500 029

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Foreword

The Jawaharlal Nehru Centre for Advanced Scientific Research established by the Government of India in 1989 as part of the centenary celebrations of Pandit Jawaharlal Nehru, has completed the first decade of its existence. Located in Bangalore, it functions in close academic collaboration with the Indian Institute of Science.

The Centre is an autonomous institution devoted to advanced scientific research. It promotes programmes in chosen frontier areas of science and engineering and supports workshops and symposia in these areas. It also has programmes to encourage young talent.

In addition to the above activities, the Centre has a programme of publishing high quality Educational Monographs written by leading scientists and engineers in the country addressed to students at the graduate and postgraduate levels, and the general research community. These are short accounts introducing the reader to interesting areas in science and engineering in an easy manner so that later study in greater depth and detail is facilitated.

This monograph is one of the series being brought out as part of the publication activities of the Centre. The Centre pays due attention to the choice of authors and subjects and style of presentation, to make these monographs attractive, interesting and useful to students as well as teachers. It is our hope that these publications will be received well both within and outside India.

V. Krishnan.

V. KRISHNAN

Preface

The essays presented in this volume are the texts of lectures and articles prepared on special occasions over the past few years, some in honour of famous physicists and mathematicians of recent times. Anyone with at least a modest amount of formal education today is sure to be familiar with the name of Albert Einstein, and probably with some of his achievements. But there are other equally important figures in modern physics whose personalities and accomplishments ought to be known to more members of the general public, and definitely of course to students of science. Even for the latter, it is well to recall the words of James Clerk Maxwell:

> "It is when we take some interest in the great discoverers and their lives that science becomes endurable, and only when we begin to trace the developments of ideas that it becomes fascinating."

Of the six essays put together here, three are biographical in nature; and the other three, while partly woven around the lives of distinguished personalities, try to expose and develop some fundamental concepts in modern physics and sometimes overlap with biology. I have allowed a small amount of repetition to remain in the essays, in the hope that this will only help reinforce some of the points being made. The opening essay, "Paul Dirac—His Life and Work", is a tribute to Paul Adrien Maurice Dirac, one of the founders of quantum mechanics, written soon after his demise in 1984. For the student of physics, a brief description of each of his most important papers and the concepts he created, and for the general reader a picture of his unusual personality and an idea of the magnitude of his achievements, are given. This is followed, in "Bohr and Dirac", by a comparison of the contrasting personalities of Niels Bohr and Paul Dirac, presented at the time of the birth centenary of Bohr in 1985. It attempts, in a light-hearted vein, to trace the development of the quantum theory, the contributions of Planck and Einstein, Bohr's revolutionary insights, and then goes on to the birth of quantum mechanics and its philosophical repercussions. Bohr and Dirac differed in age by 17 years, and many aspects of their relationship are both warm and touchingly human.

The next three essays deal with developments of a conceptual nature, both within physics and in its relationship to biology. "The Mathematical Style of Modern Physics", traces the different levels at which the fundamental notion of symmetry has come into physics, both classical and quantum; and the senses in which unobservable quantities play a role in present-day physical theories. The paper was originally presented to a general scientific audience, not physicists alone, and so the explicit use of mathematics was kept to a minimum. The presentation of ideas is interspersed with quotations from many leading figures who contributed to the growth of physics, Dirac included, for through their words one gets closest to the heart of the matter. The next essay titled "The Mathematics and Physics of Quantum Mechanics", shows the way certain mathematical ideas, then new and unfamiliar to most physicists, were discovered to be essential for the formulation of quantum mechanics. These led to some speculative attempts, by and large unsuccessful, to extend the formal structure of quantum mechanics so as to take advantage of some purely mathematical developments. The strange features of the physical interpretation of the theory are also recounted.

The essay "Aspects of the interplay between Physics and Biology", is built around some very profound insights into the nature of scientific knowledge, recently elaborated by Max Delbruck, a theoretical physicist who turned to biology under the influence of Niels Bohr. It has to do with the way we perceive the world around us, how biological evolution by natural selection has equipped us to do so and, in the process, endowed us with ways of thinking and processing information that usually seem innate in us. The hope in this essay is to make both physicists and biologists aware that in certain aspects of epistemology — the theory of knowledge — their concerns come very close together, indeed.

The concluding essay, "Eugene Paul Wigner — A Tribute", gives a sketch of the life and varied accomplishments of one of the most talented physicists of the century. His contributions to quantum mechanics and its interpretation, and his elucidation of the special role of symmetry in modern physics are described.

It should be evident from these remarks that there are unifying threads running through all these essays, interweaving both personalities and ideas.

I would like finally to express my sincere thanks for the encouragement and support received from Prof C N R Rao in the preparation and putting together of this monograph.

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March 2000.

Acknowledgements

The essays in this volume have grown out of material presented as invited lectures on various occasions. Listed here, by way of acknowledgment, are the relevant original source details for each essay.

"Paul Dirac – His Life and Work" – presented at the Founder's Day celebrations, Saha Institute of Nuclear Physics, Calcutta, January 10, 1985, published in *Science Age*, February 1985; also in *Recent Developments in Theoretical Physics*, edited by E.C.G. Sudarshan, K. Srinivasa Rao and R. Sridhar, World Scientific Publishing Co. Pte. Ltd., Singapore, 1987.

"Bohr and Dirac" — presented at the Ordinary General Body Meeting of the Indian National Science Academy, held at the Indian Institute of Science, Bangalore, on August 2, 1985.

"The Mathematical Style of Modern Physics" — presented at the the 52nd Annual Meeting of the Indian Academy of Sciences, Varanasi, November 8, 1986; earlier versions at the Istituto Italiano per gli Studi Filosofici, Napoli, Italy, September 1983; the Mathematical Association of India (Delhi Chapter), Delhi, April 1986; also in *Recent Developments in Theoretical Physics*, edited by E.C.G. Sudarshan, K. Srinivasa Rao and R. Sridhar, World Scientific Publishing Co. Pte. Ltd., Singapore, 1987; reprinted in *Current Science*, Vol.56, No.4: 156–162, 1987.

"The Mathematics and Physics of Quantum Mechanics" — presented as the Inaugural P.L. Bhatnagar Memorial Lecture, the 53rd Annual Meeting of the Indian Mathematical Society, Gorakhpur, December 31, 1987.

"Aspects of the Interplay between Physics and Biology" — Lecture presented at the meeting of the Indian National Science Academy, North-Eastern Hill University, Shillong, May 17, 1989; first published in present form in *Journal of Genetics*, Vol.68, No.2 : 117–128, August 1989.

"Eugene Paul Wigner—A Tribute" — text of special colloquium given at Raman Research Institute, Bangalore, June 16, 1995; published in *Current Science*, Vol.69, No.4 : 375–385, 1995.

1 *Paul Dirac—His Life and Work*

An era in physics came to an end when Paul Adrien Maurice Dirac passed away on 20^{th} October, 1984, at the age of 82. Our last surviving link with the birth of quantum mechanics was also broken with his death.

Anyone acquainted with the development of modern physics would be well aware of the range, depth and profound beauty of Dirac's work and ideas which appeared in steady and staggering profusion over half a century and more. For those whose special field of interest may not be physics, however, and also for a more general audience, it is worthwhile describing the personality and the accomplishments of this genius of our times.

Dirac was born in Bristol in England on 8^{th} August, 1902. His father – Charles Adrien Ladislas Dirac – was Swiss, and mother – Florence Hannah Holten – was English. He went through school in Bristol, and in 1921 obtained a B Sc in electrical engineering from Bristol University. Unable to get a job in this field, he joined the University of Cambridge in 1923 as a research student in physics under the guidance of R H Fowler. After some work in the framework of the old quantum theory he published, in 1925, his first famous paper on the new quantum mechanics. This immediately established his reputation. In 1926 he earned the Ph D degree of Cambridge University – the title of his thesis was "Quantum Mechanics" – and was elected Fellow of St. John's College in 1927. In 1932, he succeeded Joseph Larmor – familiar to physicists through the Larmor precession – as Lucasian Professor of Mathematics, a position he held till 1969 when he became Professor Emeritus at the University of Cambridge. Dirac shared with Erwin Schrödinger, the Nobel Prize for physics in 1933 for his 'discovery of new fertile forms of the theory of atoms and for its applications.' In 1937 Dirac married Margit Wigner, sister of the well-known physicist Eugene Wigner. From 1971 onwards, till his passing away, he lived in Tallahassee, Florida, as Professor of Physics at Florida

Paul Adrien Maurice Dirac
1902-1984

Erwin Rudolf Josef Alexander Schrödinger
1887–1961

State University. He was a frequent participant at the Coral Gables physics conferences and remained scientifically active and involved with teaching until shortly before his death.

Apart from the Nobel Prize, some of the other honours that came his way were the Fellowship of the Royal Society of London at a very young age, the Order of Merit, and the Oppenheimer Prize.

1.1 Dirac's important papers and ideas

The number of scientific papers that Dirac wrote is not particularly great. A bibliography compiled at the time of his 70th birthday contained a little over one hundred publications; in all it may run to some 200 papers or so. But the number and variety of entirely original and trail-blazing ideas in these papers are truly stupendous. Here, in chronological sequence, are what most physicists would agree are his most important papers.

1925 "The Fundamental Equations of Quantum Mechanics", *Proceedings of the Royal Society of London*, A109 : 642. This work established Dirac's reputation. It was submitted on 7th November, 1925, and at R H Fowler's urging, it was in print in a few weeks' time before the end of the year. This paper has an interesting history. In June 1925, Heisenberg had taken the first decisive steps towards the new quantum mechanics; and on a visit to Cambridge in July he had given a seminar on some of his earlier work. Dirac was, however, unable to attend this seminar. Soon after, in September, Heisenberg sent Fowler the proofs of his paper, which the latter passed on to Dirac. At first reading, because his own ideas at the time were rather different from Heisenberg's methods, Dirac did not think much of the paper. But after a week he read it again, and then he suddenly saw "that it provided the key to the problem of quantum mechanics."

With this stimulus he proceeded to reformulate the theory in his *own* way, and in particular to expose the relationship of Heisenberg's quantum mechanics to the Hamiltonian form of classical mechanics, with which he was thoroughly familiar. Heisenberg had been uneasy about having introduced a non-commutative law of multiplication among physical quantities — in which the product *ab* may not be equal to *ba* — but for Dirac this became the most important feature. Much later, in a talk given in 1975, he recalled:

> When Heisenberg first noticed that his matrices did not satisfy commutative multiplication, he was very disturbed by it.

Werner Karl Heisenberg
1901–1976

> He felt that perhaps the whole theory would break down over that point. (From time immemorial, physicists had been working with dynamical variables for which we always have ordinary algebra; *a* times *b* equals *b* times *a*. And it was inconceivable to have dynamical variables for which this property fails). Heisenberg was naturally very disturbed by it, but still it was a fundamental point in his theory, and it turned out to be a most important point.

In a few weeks' time he got the idea that the commutation rules of Heisenberg approached in the classical limit the Poisson Bracket of classical dynamics, but he says:

> I did not know very well what a Poisson Bracket was then. I had just read a bit about it, and forgotten most of what I had read. I wanted to check up on this idea, but I could not do so because I did not have any book at home which gave Poisson Brackets, and all the libraries were closed. So, I just had to wait impatiently until Monday morning when the libraries were open and check on what the Poisson Bracket really was. Then I found that they would fit, but I had one impatient night of waiting.

Dirac has said that, of all his discoveries, this link between the commutator and the Poisson Bracket was his favourite. It is surely the most profound and the most precise formulation of the otherwise rather hard to grasp, Correspondence Principle of Bohr. One must also mention that Dirac's immediate and profound grasp of the structure of quantum mechanics was in an important sense deeper than that of the Heisenberg–Born school. Dirac's approach could handle bound states and scattering states, discrete and continuous spectra, all at the same time.

This first paper on quantum mechanics was followed by many others developing the entire formalism, all written in such a style that it led Einstein to write with admiration of

> Dirac to whom in my opinion we owe the most logically perfect presentation of quantum mechanics.

1926 "On the theory of Quantum Mechanics" — *Proceedings of the Royal Society of London*, A112 : 661. In this paper, Dirac derived the statistical distribution, subsequently called Fermi–Dirac statistics, starting from the symmetry laws of quantum mechanics. As is well known, this kind of statistics applies to particles such as

Max Born
1882–1970

Albert Einstein
1879–1955

electrons, protons and neutrons, in contrast to the Bose–Einstein statistics applicable to photons, π-mesons and the like. The statistical law itself had been derived a few months earlier by Fermi; Dirac had read this work but says he had then forgotten all about it. The sense in which Dirac went beyond Fermi was the derivation of the statistical law from the symmetry properties of the quantum mechanical wave function describing a collection of identical and indistinguishable particles.

1927 "The Quantum Theory of the Emission and Absorption of Radiation" — *Proceedings of the Royal Society of London*, **A114** : 243. This is a landmark paper in the development of our understanding of the nature of electromagnetic radiation and of its interaction with matter. It was written partly at Cambridge and partly at Copenhagen, and communicated to the Royal Society by Niels Bohr in February 1927. In this paper, Dirac applied the principles of quantum mechanics to the electromagnetic field and thus inaugurated quantum field theory. It is the culmination of the work of Planck, Einstein and Bose, from the discovery of the black body radiation law by Planck in 1900, through the photon concept of Einstein in 1905, then the famous A and B coefficients of Einstein of 1917, and finally the Bose derivation of the statistical behaviour of photons in 1924—until in Dirac's hands was achieved the complete elucidation of the quantum nature of light. Among other things, this paper succeeded in obtaining the Einstein coefficients for stimulated emission, absorption and also spontaneous emission of light from basic theory.

In his earlier papers, Dirac had already obtained the coefficients for absorption and stimulated emission by treating matter quantum-mechanically, but the electromagnetic field classically. It required the quantization of the Maxwell field itself to obtain the Einstein coefficient for spontaneous emission, also from first principles. In the language of creation and destruction operators for photons, this arises from the "one" on the right-hand side in the commutation relation

$$[a, a^\dagger] = aa^\dagger - a^\dagger a = 1.$$

G Wentzel, a well-known theoretical physicist, described Dirac's achievements in this paper in these words:

> Today, the novelty and boldness of Dirac's approach to the radiation problem may be hard to appreciate. Dirac's explanation came as a revelation.

Max Karl Ernst Ludwig Planck
1858–1947

1928 "The Quantum Theory of the Electron I, II" — *Proceedings of the Royal Society of London*, **A117** : 610; **A118** : 351. The foundations of modern physics may be summarized in a handful of basic equations, and Dirac's relativistic wave equation for the electron is one of them. Describing a conversation with Niels Bohr at the Solvay Conference of 1927, Dirac says:

> I remember in particular an incident in the Solvay Conference in 1927. During the interval before one of the lectures, Bohr came up to me and asked me: 'What are you working on now?' I tried to explain to him that I was working on the problem of trying to find a satisfactory relativistic quantum theory of the electron. And then Bohr answered that the problem had already been solved by Klein. I tried to explain to Bohr that I was not satisfied with the solution of Klein, and I wanted to give him reasons, but I was not able to do so because the lecture started just then and our discussion was cut short. But it rather opened my eyes to the fact that so many physicists were quite complacent with a theory which involved a radical departure from some of the basic laws of quantum mechanics, and they did not feel the necessity of keeping to these basic laws in the way that I felt.

What Dirac refers to here is the need for the equations of motion in quantum mechanics to be differential equations of the first order, with respect to time. This was of paramount importance to him since it had its roots in the Hamiltonian form of classical dynamics, and he insisted on it while trying to combine special relativity and quantum mechanics. The measure of his success is revealed by the number of things the equation explained—the spin of the electron, its magnetic moment, the fine structure of the spectrum of hydrogen, and finally the reinterpretation of the negative energy solutions in terms of the positron leading to the concept of antimatter.

Concerning the last, Dirac said much later that his first impulse was indeed to reinterpret the negative energy states in terms of positively charged particles with the same mass as the electron, but he lacked the boldness to do so. So instead he suggested they be identified with the proton, which does have positive charge but is almost two thousand times as heavy as the electron. However, Oppenheimer's calculation of the consequent proton–electron annihilation rate and Weyl's arguments based on symmetry made it clear that Dirac's proposal was untenable. So Dirac wrote in 1931:

Hermann Peter Weyl
1885–1955

> It thus appears that we must abandon the identification of the holes with protons and must find some other interpretation for them. A hole, if there were one, would be a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to an electron. We may call such a particle an antielectron.

In contrast to the case of the photon, this was the first ever *prediction* of a new particle, and it was found soon after, in August 1932, by C D Anderson. Thus, the feature of Dirac's equation—the negative energy states—which initially had seemed an embarrassment, turned into its greatest triumph. This reinterpretation—called the hole theory—has been described by Arthur Wightman thus:

> It is difficult for one who, like me, learned quantum electrodynamics in the mid-1940s to assess fairly the impact of Dirac's proposal. I have the impression that many in the profession were thunderstruck at the audacity of his ideas. This impression was received partly from listening to the old-timers talking about quantum electrodynamics a decade and a half after the creation of hole theory; they still seemed shell-shocked.

1930 *The Principles of Quantum Mechanics*—Clarendon Press, Oxford. This book is often compared for its spirit and style to the *Principia* of Isaac Newton. The mathematician, von Neumann, while critical of Dirac's approach in certain respects, could not but say that this book "is scarcely to be surpassed in brevity and elegance." If one reads the book carefully one finds that many sections of it are taken unchanged from Dirac's original papers. This illustrates so well the statement of Niels Bohr:

> Whenever Dirac sends me a manuscript, the writing is so neat and free of corrections that merely looking at it is an aesthetic pleasure. If I suggest even minor changes, Paul becomes terribly unhappy and generally changes nothing at all.

This book has passed through several editions, and if one is lucky to be able to go through all of them, one would learn a great deal even from following the changes Dirac made from edition to edition.

1931 "Quantized Singularities in the Electromagnetic Field" — *Proceedings of the Royal Society of London*, A133 : 60. The prediction of the positron, recapitulated earlier, was in fact made in this paper. Those familiar with the plays of Bernard Shaw are aware of the beautiful essays that appear at the beginnings of the plays — often rivalling the plays themselves for wit and insight. A similar statement could be made about the introductions to Dirac's papers. He developed the style of reviewing in his own way the most important recent developments in a particular area, expressing his opinions about problems and progress and putting things in perspective, before going on with a presentation of his own results in each paper.

A collection of the introductory sections of his papers would be most interesting; the section in the present paper is an outstanding example, wherein he traces the changing emphasis in the relationship between mathematics and physics in passing from the previous century to the present one. He then proceeds to investigate an extension of quantum mechanics as accepted at that time, wherein the complex-valued wave function of a particle is generalized to a mathematical quantity with a well-defined modulus but a non-integrable phase. This train of thought led him to the concept of the magnetic monopole. He was able to show that the existence of even a single magnetic monopole in nature would imply, because of the demands of consistency with quantum mechanics, that all electric charges must be quantized in terms of a basic unit. The mathematical ideas Dirac introduced here—essentially the concept of fibre bundles—were decades ahead of the rest of the world; they entered the vocabulary of physicists in a big way only in the 1960s and 70s.

1933 "The Lagrangian in Quantum Mechanics" — *Physikalische Zeitschrift der Sowjetunion*, Band 3, Heft 1. In our selection of papers, this is the first one not taken from the *Proceedings of the Royal Society*. The two forms of quantum mechanics known at this time, Heisenberg's and Schrödinger's, were both based on the Hamiltonian form of classical mechanics which in its turn was obtained from the Lagrangian version of classical mechanics. Thus, from the Hamiltonian one either went Heisenberg's way to get quantum equations of motion for dynamical variables, analogous to the classical Hamilton equations of motion, or one went Schrödinger's way to get an equation of motion for the wave function, analogous to the classical Hamilton–Jacobi theory.

Dirac examined, for the first time, the question whether the classical Lagrangian had any direct role to play in quantum mechanics. He was able to show that there were mathematical quantities in quantum mechanics that were analogous to the Lagrangian — or rather its time integral, the action — of classical theory. In trying to understand precisely what Dirac meant, and in particular whether the analogy could be sharpened to an identity, Feynman was led some fifteen years later to a third version of quantum mechanics — the so-called path integral formalism — which has become exceedingly important in recent times.

1937 "The Cosmological Constants" — *Nature*, **139** : 323. Each of the fundamental physical constants of nature has a corresponding dimension, and the numerical value depends on the system of units employed. However, by forming suitable combinations of these constants one can arrive at dimensionless quantities; this is qualitatively like measuring one force or velocity in terms of another force or velocity. Some of these dimensionless combinations have "reasonable" values, such as a hundred or a thousand. Examples are the inverse of the fine structure constant, $\hbar c/e^2$, with the value 137, or the proton to electron mass ratio, $m_p/m_e = 1840$.

Dirac said that it is conceivable that such numbers may be derived some day from basic theory, in terms of factors of 4π and the like. However, there are other dimensionless constants which are "unreasonably" large. For instance, the ratio of the electrostatic force to the gravitational force between an electron and a proton is 2×10^{39}. It is inconceivable that such a large number could ever be explained in terms of factors like 4π; rather, understanding must come by relating this large number to other similarly large numbers. It now turns out that there are others! For example, the ratio of the age of the universe to the time taken by light to cross a distance equal to the classical electron radius turns out to be 7×10^{39}. Surprisingly, it is of the same order as the previously quoted large quantity. Dirac suggested that such near equalities among such extremely large numbers could not be accidental, and he offered an explanation which has come to be called the Big Numbers Hypothesis. Among other things, it implies that this equality must be maintained in time, leading to the result that as the age of the universe increases, the strength of the Newtonian gravitational constant must decrease. Though his predictions have not yet been unambiguously verified, the issue is still an active one and the last word on the subject has not yet been said.

Richard Phillips Feynman
1918–1988

1938 "Classical Theory of Radiating Electrons" — *Proceedings of the Royal Society of London*, A167 : 148. The theory of quantum electrodynamics, which had its birth in Dirac's 1927 paper, soon ran into severe mathematical difficulties. This was a source of much worry throughout the 1930s and early 40s, until the problems were solved in a fashion by the techniques of renormalization theory of Tomonaga, Schwinger and Feynman around 1947. The problem was that when one went beyond the first non-trivial approximation in the calculation of physically observable quantities, the theory gave infinite, hence meaningless, answers.

Dirac tried two ways to solve this problem, and this classic 1938 paper is concerned with one of them. He decided to go back to the classical theory of electrons interacting with the electromagnetic field, with the intention of putting it in as satisfactory a shape as possible before attempting to quantize it. Though this particular attempt did not quite succeed, there are some outstanding ideas in this paper.

Instead of basing the theory on a Lagrangian, Dirac showed that the classical equations of motion for a relativistic point electron could be determined pretty much completely by insisting on the conservation laws of energy and momentum. In particular, he was able to obtain the Lorentz radiation reaction terms in a clean way; more important, the concept of (infinite) mass renormalisation appeared here for the first time, already in a classical context. This work served as inspiration for a considerable amount of further work by, among others, Bhabha and Harish–Chandra at Bangalore, and Wheeler and Feynman at Princeton.

1942 "The Physical Interpretation of Quantum Mechanics" — *Proceedings of the Royal Society of London*, A180 : 1. Still struggling with the problem of infinities in quantum electrodynamics, Dirac tried this time to abandon the positive definite metric of Hilbert space and to allow states with a negative norm. Probabilities are by definition non-negative quantities, and the probabilistic interpretation of quantum mechanics depends crucially on the positive definite metric in Hilbert space, which is used to describe states of physical systems and to compute probabilities. While the problem of infinities can readily be cured by relaxing the condition of positive definiteness, it then immediately leads to severe problems for interpretation, and one wonders how Dirac could have contemplated such a drastic step.

The explanation and his attitude are best expressed in his own words taken from the introduction to his paper:

Julian Schwinger
born-1918

Konrad Zacharias Lorenz
1903–1989

> This makes it an easier matter to discover the mathematical formalism needed for a fundamental physical theory than its interpretation, since the number of things one has to choose between in discovering the formalism is very limited, the number of fundamental ideas in pure mathematics being not very great, while with the interpretation most unexpected things may turn up.

This concept of the indefinite metric was used to great advantage by S N Gupta a few years later, not for solving the problem of infinities but for achieving a neat, relativistic quantisation of the electromagnetic field. Later, it was particularly used by both Heisenberg and Sudarshan in their attempts to solve the divergence problems of quantum field theory.

1945 "Unitary Representations of the Lorentz Group" — *Proceedings of the Royal Society of London*, A183 : 284. A major area of research activity in mathematics in recent times has been the representation theory of non-compact semi-simple Lie groups. It is quite fair to claim that the original stimulus for this work came from physics and, in fact, from Dirac.

Finite dimensional quantities transforming in a definite and consistent way under the Lorentz group are, of course, well known. Examples are spinors (rediscovered by Dirac in his electron wave equation, though known earlier to mathematicians), vectors, tensors like the electromagnetic field, and so on. The corresponding representations are all non-unitary because of the nature of the Lorentz group. But quantities transforming according to unitary representations necessarily have an infinite number of independent components.

In 1932, the Italian physicist Ettore Majorana had constructed two very special unitary representations of the Lorentz group — curiously enough, in connection with a relativistic wave equation designed to avoid the negative energy problems of the Dirac equation! (Presumably this was just before the experimental discovery of the positron). In Dirac's paper, a whole new family of infinite dimensional unitary representations was constructed. Generalizing the terms vector and tensor, he named quantities belonging to his new representations "expansors". They were composed entirely of integer spin quantities. Soon after this, Harish-Chandra extended Dirac's work by constructing the half integer spin unitary representations, naming them "expinors". Both Dirac and Harish-Chandra had been concerned with the physical Lorentz group in four-dimensional space–time. In 1947, Bargmann succeeded in constructing all the unitary representations for the three-dimensional Lorentz group, then Gelfand and Naimark did the same thing independently for the physical Lorentz group, and a whole new area of modern mathematics had emerged, all starting with the Dirac paper.

1949 "Forms of Relativistic Dynamics" — *Reviews of Modern Physics*, **21** : 392. In the relativity of Galileo and Newton, the concept of simultaneity is the same for all observers. Thus, the concept of the state of a physical system at a given time is essentially unique and the same for all frames of reference. This concept is important when one looks upon equations of motion as equations determining the evolution of a system with respect to time—the equations tell us how the state varies as time progresses.

In Einstein's special theory of relativity, the situation changes drastically. Two events which appear simultaneous to one observer need not appear so to another. By the same token, one has far more flexibility in defining what one means by the term "state of a physical system", in such a way that the equations of motion determine the way this state evolves.

One could of course follow the Galilean example, and define "state" to mean "all physical conditions at all points of space at a given time". Relativistic equations of motion can then be viewed as equations that give the evolution of states defined in this way. This particular form of relativistic dynamics, Dirac called the instant form. However, because of the altered meaning of simultaneity, other forms are possible, corresponding to other ways of setting up the notion of state. Dirac exploited this increased freedom and elaborated two other forms of relativistic dynamics, which he called the point and the front forms. As usual, these have come to be used in various contexts in particle physics much later; in particular the front form has become important in some problems of optics quite recently.

There is a remarkable sentence in this paper:

> I do not believe there is any need for physical laws to be invariant under these reflections, although all the exact laws of nature so far known do have this invariance.

Dirac is speaking here, in 1949, of space and time reflection symmetries, whose breakdown in the weak interactions was demonstrated in 1957 and 1964, respectively!

Valentine Bargmann
1908–1989

Galileo Galilei
1564–1642

1950 "Generalized Hamiltonian Dynamics" — *Canadian Journal of Mathematics*, **2** : 129. In the long line of development of the formalism of classical dynamics, beginning with Galileo and Newton and then involving Euler, Lagrange, Hamilton, Jacobi and Poincaré, the work in this paper is the most important formal development in our times. It is obviously the result of several years' effort, though Dirac's final account of it makes it all appear so effortless and natural. What has been developed here is a general formalism which is capable of and has been designed to handle a class of physical systems called constrained dynamical systems; and the special concepts and methods Dirac has introduced are as usual of profound depth and beauty. These ideas have turned out to play a fundamental role in a great deal of current work, notably in gauge theories.

1958 "The Theory of Gravitation in Hamiltonian Form" —*Proc. of the Royal Society of London*, **A246** : 333. Much of the motivation for Dirac to develop his generalized Hamiltonian dynamics came from the need to cast Einstein's general theory of relativity — a constrained system par excellence – into Hamiltonian form, as a first step towards its quantization. This programme was carried through in this 1958 paper. As is well known, an important property of Einstein's equations is that they retain their form under a very wide variety of transformations in space–time, and one refers to their four-dimensional form and symmetry. Putting such equations into Hamiltonian form automatically leads to an apparent reduction in the visible symmetry, because the Hamiltonian method has to deal with the concept of "state at a given time."

Only Dirac could say of Einstein's theory, at the end of this paper:

> I am inclined to believe (from this) that four-dimensional symmetry is not a fundamental property of the physical world.

1962 "The Conditions for a Quantum Field Theory to be Relativistic", *Reviews of Modern Physics*, **34** : 592. As has been recounted earlier, the relativistic electron equation was the result of Dirac's attempts to unite the principles of special relativity and quantum mechanics. He returned to this theme in the context of field theory this time. In this paper he showed by·elementary means, that if for a field theory invariance under the Euclidean group is ensured in a natural way, then all the remaining requirements of relativistic invariance are obeyed if the commutator of the energy density with itself has a special form. Thus, symmetry under the ten-parameter

Isaac Newton
1642–1727

Henri Poincaré
1854–1912

group of special relativity is essentially reduced to one single requirement, which has come to be known as the Dirac–Schwinger Energy Density Condition. While Schwinger obtained this condition by considering the limiting case of minimal coupling to a weak external gravitational field, Dirac's method is deceptively simple and uses special relativistic arguments alone.

He also took the occasion to remark that the concepts of equivalence in mathematics and in physics need not be the same, and a unitary transformation which may be regarded as trivial from a mathematical point of view may be very important for physics.

1.2 *Festschrift* articles by Dirac

Eminent scientists often write articles to congratulate one another on important birthdays. Quite often, these pieces are nostalgic in character and the principal intention is to say "Many Happy Returns." I would like to particularly mention some of Dirac's articles written on such occasions, because each of them had a new and novel idea chosen perfectly for the person and the occasion.

For Niels Bohr's 60th birthday in 1945, Dirac contributed a piece titled "On the Analogy between Classical and Quantum Mechanics" in the *Reviews of Modern Physics*, 17 : 195. Here, he gave a lucid description of ways of establishing correspondences between classical and quantum observables, and a general theory of what have come to be known later as phase–space quasi-probability distributions in quantum mechanics. And we remember that it was Niels Bohr who took the first steps from classical mechanics to the mechanics of the atom. For Einstein's 70th birthday, Dirac presented the paper of 1949 on "Forms of Relativistic Dynamics", described earlier. At the Lorentz memorial conference, Leiden, 1953, he showed how the concept of the ether could be reinstated in quantum theory. (The reference is "The Lorentz Transformation and Absolute Time", *Physica*, 19 (1953) : 888). In classical theory, ether was ruled out because its rest frame would single out a preferred frame (or family of frames) of reference. But Dirac pointed out that in quantum theory, we need not prescribe a specific velocity for ether in every inertial frame. All we need do is ascribe a wave function determining the probabilities for various velocities to occur. As long as this wave function remained invariant under Lorentz transformations, there would be no preferred frames of reference, so that ether could be consistent with relativistic quantum theory.

The 1962 paper written in celebration of Wigner's 60th birthday was again most appropriate because it was Wigner who, in a classic paper in 1939, had carried out the first systematic analysis of relativistic invariance and representation theory of the group of special relativity in quantum mechanics.

One wonders whether Dirac kept careful track of important birthdays to come, and whether he saw to it that the right ideas matured at the right time!

1.3 Students and collaboration

Dirac did not have many students. It seems "his reason was not at all related to the trouble involved, but was because his own interests were in fundamental problems and he did not think that these were suitable for many Ph D students." It would be no exaggeration to say that his most illustrious student has been Harish-Chandra of India. Dr Homi Bhabha was also with Dirac at Cambridge in the 1930s. Thanks to this connection, Dirac visited the Tata Institute of Fundamental Research for several months, sometime in the early 1950s, and gave a set of lectures on "Quantum Mechanics and Relativistic Field Theory", which were written up by K K Gupta and George Sudarshan.

Practically all his important work was done by him in isolation, there being very few important papers in collaboration with others. Probably the two most notable ones are "On Quantum Electrodynamics" with V A Fock and Boris Podolsky in the *Physikalische Zeitschrift der Sowjetunion*, Band 2, Heft 6 (1932); and "On Lorentz Invariance in the Quantum Theory" with R E Peierls and M H L Pryce in the *Proceedings of the Cambridge Philosophical Society*, **38** : 193 (1942). The former sets up the many-time formalism for a system of relativistic particles in interaction with the electromagnetic field, and is the inspiration behind the covariant space–time formalism developed by Tomonaga and Schwinger in the 1940s. The latter was written in reply to an especially critical paper of Eddington's, in which he had questioned the logical consistency of using the relativistic electron equation to solve the problem of the hydrogen spectrum.

1.4 Personality of a genius

Dirac was an extremely shy, selfless and sincerely modest person. It is recounted that he would introduce his wife to friends as

"Wigner's sister". (To which Wigner must have retaliated by calling Dirac "my famous brother-in-law".) As for modesty, he always acknowledged his debt to his contemporaries most generously. For instance, he once said that Heisenberg and he were working on the same problem at the same time, and Heisenberg succeeded where he failed. In a talk in 1975 he put it in these words:

> Well, from the initial idea of Heisenberg, one could make a fairly rapid development, and I was able to join in it. I was just a research student at that time. I was lucky enough to be born at the right time to make it possible for that to be so.

Elsewhere he described those times thus:

> It was very easy in those days for any second-rate physicist to do first-rate work. There has not been such a glorious time since then. It is very difficult now for a first-rate physicist to do second-rate work.

What he omitted to say was that his own work created those glorious conditions.

Einstein was Dirac's principal hero, and his wife said that the only time she saw Dirac in tears was at Einstein's death. In talking about his work on the electron wave equation and the prediction of the positron and antimatter, Dirac merely said that it was all a direct consequence of Einstein's special relativity!

Dirac had a deep sense of beauty of form and structure in physics, as well as a love for simplicity. He was as fond of his invention of the bra and ket formalism — and the names he chose for them — as of anything else he did. In his hands, even items of notation became acts of creation and things of beauty. An outstanding instance is his delta function. An oft-quoted statement of his is that it is more important to have beautiful equations than that they should fit experiments perfectly. Of course, such criteria lead to results only in the hands of the gifted.

The Hamiltonian point of view in dynamics was very close to Dirac's way of thinking, and a great deal of his work was inspired by it. He said he had a geometrical way of picturing and understanding equations rather than an algebraic one. But in addition he was unmatched in his ability to think in abstract, non-pictorial terms. At the age of 28, in his book on quantum mechanics, he had said that the fundamental laws of nature

> control a substratum of which we cannot form a mental picture without introducing irrelevancies.

Dirac made practically no statements on the political issues of his time — unlike Einstein, Wigner and others — and he was a totally non-controversial person. Even on the philosophical problems of quantum mechanics he said surprisingly little, since he seems to have felt that the present interpretation of quantum mechanics is provisional and is bound to change. In a talk in 1975, he remarked:

> ... the present form of quantum mechanics should not be considered as the final form. There are great difficulties ... with the present quantum mechanics. It is the best that one can do up till now. But one should not suppose that it will survive indefinitely into the future. And I think that it is quite likely that at some future time we may get an improved quantum mechanics in which there will be a return to determinism and which will, therefore, justify the Einstein point of view.

Einstein brought classical physics to its pinnacle of perfection with the relativity theories, and with Planck and Bohr he paved the way for quantum physics. Dirac, with Heisenberg and Schrödinger, established quantum mechanics, and then went on to create quantum field theory as well. By his writings and thoughts , Dirac has inspired hundreds of physicists over several generations. There is little doubt that in the times to come it will be Dirac who will be remembered as the physicist of this century.

2 *Bohr and Dirac*

In this essay I would like to convey to my readers something about the personalities and work of Niels Bohr and Paul Dirac, juxtaposed against one another. Let me hope that the portraits I will paint of these two great figures from the world of physics will be faithful to the originals. The year 1985 was celebrated as the centenary of Bohr's birth, while Dirac passed away in October of the previous year. [1] There was a gap of almost a generation between them. Let us also recall that Einstein's life spanned the period 1879 to 1955; so he was just six years older than Bohr.

For Bohr and Dirac, the most important work of their lives was bound up with the strange story of the quantum—the struggle to adapt and alter the fabric of classical physics to accommodate Planck's quantum of action. That this called for an overhauling of all three components of the classical scheme—matter, motion and radiation. Naturally Bohr appeared on the scene at an earlier phase of the struggle than did Dirac, and several others were also involved, but here our focus will be on these two.

Some of you may remember that Planck made his momentous discovery sometime in the evening of Sunday, October 7, 1900 (incidentally, Bohr's fifteenth birthday). The experimental physicist Heinrich Rubens and his wife had visited the Plancks for tea that afternoon. Rubens told Planck of his and Kurlbaum's measurements of the black body radiation spectrum in the far infrared limit, where he had found definite deviations from the Wien radiation law. This law was a theoretical one which had been proposed in 1896 by Wien, and which Planck had believed to be exactly valid. Soon after the Rubens left, Planck set to work to find an interpolation between Wien's Law, known to be valid at high frequencies, and the low frequency measurements just reported to him by Rubens, which incidentally agreed with the theoretical results of Rayleigh

[1]Niels Henrik David Bohr, b. October 7, 1885, d. November 18, 1962. Paul Adrien Maurice Dirac, b. August 8, 1902, d. October 20, 1984.

Niels Henrik David Bohr
1885–1962

and Jeans. It was thus that Planck arrived at his celebrated radiation law. It is somewhat staggering to realise that quantum theory was born or discovered in this way in the space of a few hours!

The quantum of action was thus first discovered via the thermodynamic properties of light, and in the succeeding years the first insights into its significance came largely through statistical arguments as well as the wave-particle duality of light. In all of this of course, Einstein played a leading role. However, the connection of Planck's discovery to the structure of matter, its stability and its mechanics had to wait for Bohr's magic touch in the years 1912–13.

During his doctoral work on the electron theory of metals, completed in 1911, Bohr had realised very clearly that there was a need for a radical departure from the laws of classical electrodynamics in the atomic domain. It was extremely fortunate for him that in March 1912 he went to work briefly with Rutherford at Manchester, after a disappointing stint with J J Thomson at Cambridge. At Manchester he came to know of Rutherford's model of the atom in which the positively charged core of the atom, the nucleus, containing practically all the mass, occupied a negligible volume at the centre of the atom. This was in contrast to Thomson's model, in which the positive charge was spread out uniformly over a finite volume of atomic dimensions. Many problems and possibilities immediately became clear to Bohr. On the one hand, in order to produce in this model a length scale of the order of the atomic size, and also to ensure stability of the electron orbits, it was essential to bring in Planck's constant. On the other hand, it now appeared that all the chemical properties of an element should depend only on one datum, namely the number of peripheral electrons, i.e. the atomic number rather than the mass number. In fact, Bohr saw that while chemistry was determined by the outermost electrons of the atom, all radioactive processes like α and β emission originated from the nucleus, deep inside the atom. It appears that at this stage Bohr took Rutherford's model more seriously than Rutherford himself did.

Turning to the structure of the atom, Bohr assumed that the electrons moved in concentric circular rings around the nucleus. Classical electrodynamics could never explain the stability of such an arrangement; but Bohr had already anticipated the need for a fundamental departure from classical ideas in this realm. He was familiar with Planck's method of quantizing the motion and the energy of simple harmonic motion, and he now adapted it to the motion of an electron in the Coulomb field of the nucleus. As much by inspiration as by deduction he was able to arrive at the right order of magnitude for atomic sizes, and at the expression $E_n = -A/n^2$ for the allowed energies of an electron bound in an atom. Here the integer n takes values 1,2,3, ... For all this of course, Planck's constant was essential, but at that time the exact form of the quantum condition was beyond him.

At this stage another important event occurred — he was called upon to investigate the passage of α-particles through matter and to analyze the processes by which they ionized the atoms of matter, losing energy and slowing down as they did so. This was a matter of practical importance in Rutherford's laboratory. The fact that he could give a satisfactory classical account of this process, whereas classical ideas failed completely within the atom, led him to the following truth: however deep the break with classical ideas might be, the new theory would have to agree with the old one in the limit of low frequencies or large quantum numbers. This was the origin of the famous Correspondence Principle, which played such a major part in subsequent developments.

At this point in his thinking, Bohr had dealt only with the structure and stability of the atom, and had not yet connected up with atomic spectroscopy or radiation phenomena. He returned from Manchester to Copenhagen in July 1912, married Margrethe Norlund in August 1912, and set about writing up the ideas conceived in Manchester. It was only in early 1913 that his mind suddenly turned to problems of atomic radiation. Atomic spectroscopy was a well-developed field with a lot of data on the characteristic spectral lines and frequencies associated with various elements. There also existed several empirical formulae, giving simple expressions for many series of spectral lines. H M Hansen, a colleague of Bohr's at the University of Copenhagen, asked him in early 1913 if he knew of Rydberg's formula which expressed every frequency as the difference of two terms, and which for hydrogen took the simple form

$$\nu_{mn} = R\left(\frac{1}{n^2} - \frac{1}{m^2}\right)$$

where both n and m were integers. Bohr had not known this even though it had been around since 1890, and Rydberg worked at the nearby University of Lund in southern Sweden. So this query and information from Hansen came as a complete surprise to Bohr. But at the same time he saw that it gave the missing clue to the problem of quantization in the atom. He compared his own formula $E_n = -A/n^2$ for quantized electron energies in an atom with individual terms in Rydberg's expression and immediately realized that each spectral line corresponded to a transition of an electron from one allowed state to another, accompanied by the emission of a quantum of radiation. In the Planck–Einstein spirit, it was Bohr who first saw the Rydberg law as an expression of the conservation of energy,

$$h\nu_{mn} = E_m - E_n,\ E_m = -hR/m^2$$

By demanding agreement with classical theory for large n, Bohr was able to completely pin down the quantization condition as well as to calculate the value of Rydberg's constant. The break with classical physics came with the fact that none of the spectral frequencies ν_{mn} coincided with any of the classical orbital frequencies, but such a break was essential to explain the stability of the atom, as anticipated by Bohr. In fact, he said that Rydberg's formula gave him such a transparent clue that he immediately saw the quantum picture of the emission of radiation. He was sure he was on the right track inspite of the total breakdown of classical physics; at the same time the Correspondence Principle was obeyed.

In 1913 he published his three famous papers on the constitution of atoms and molecules, where he stated his two fundamental postulates: (1) the electron could only be in one of a special set of stationary states which had to be chosen out of all possible classical motions by imposing quantum conditions; (2) the transition of the electron from one such state to another is a non-classical and non-visualizable process, during which a single quantum of radiation is emitted or absorbed according to the Rydberg–Bohr frequency condition.

Many predictions of Bohr's theory were checked in Rutherford's laboratory, but the English physicists, in particular Fowler and Jeans, were skeptical and accepted his ideas only reluctantly. It seems that in Göttingen there was a sense of scandal and bewilderment. But both Einstein and Sommerfeld saw immediately the significance of Bohr's ideas.

I have devoted a considerable amount of space to recounting this early phase of Bohr's work, because it was the foundation of all else that followed. Indeed, though the quantum of action was discovered in the properties of radiation, the route to the new quantum mechanics was via the mechanics of the atom. And the application of Planck's ideas to the dynamics of matter, which Dirac was to later describe as the most difficult first step, was taken by Bohr.

Bohr was fully aware of the limitations of his theory. It was necessary to generalize the quantum condition from the circular motions of a single particle to the motions of general mechanical systems; to analyze the relationship between classical and quantum aspects of atomic phenomena; and to explore the many applications of his theory. To do all this, he gradually built up a school around himself in Copenhagen. One of his earliest collaborators was Kramers from Holland, who joined him in 1916. By 1919, he had an Institute of his own. Meanwhile his programme had also been taken up by the groups at Göttingen and Munich, led respectively by Max Born and Sommerfeld. The three centers worked in an atmosphere of friendly cooperation with frequent exchanges of ideas, and sharing of successes, hopes and people. Pauli and Heisenberg, among others, travelled frequently from one of these centers to another. In 1915, Sommerfeld found the general form of the quantum conditions for any so-called multiply–periodic system, and soon Bohr adopted Sommerfeld's mathematical methods. Instead of a picture of electrons moving in concentric circular orbits in a plane, Bohr could now deal with shells of electron orbits, tackle complex atoms and their spectra, and go on to elucidate the structure of the periodic table. This was of course, a great shot in the arm for chemistry. One must remember that Bohr did all this even before the Pauli exclusion principle and the electron spin had been discovered. In all this work the Correspondence Principle was the constant guide, being used both brilliantly and judiciously. In 1921 the Correspondence Principle was extended to dispersion by Ladenburg, and Kramers followed this up in Copenhagen. In this work he was joined by Heisenberg. (Along the way, Bohr collected the Nobel Prize for 1922.) But not all the data could be satisfactorily explained by the theory. Bohr remained acutely aware how far he was from a logically consistent framework which was able to explain his two postulates and at the same time be in harmony with the Correspondence Principle. In fact, the period 1923–1925 witnessed a crisis in the old quantum theory. To this period belongs a famous paper of Bohr, Kramers and Slater. In this, Bohr tried to give an overall picture of radiative processes taking place in the atom, and the authors suggested that classical causality had to be replaced by a purely statistical description. This paper had a deep influence on Heisenberg, as it showed even more clearly the inadequacy of the classical picture of atomic processes.

As is well known, the resolution of the crisis came with Heisenberg's discovery of matrix mechanics in June–July 1925. This was a direct outgrowth of his work with Kramers in Copenhagen on dispersion, and of the influence on him of the Bohr–Kramers–Slater work. But all that is another story.

Meanwhile, back at the ranch in Cambridge, a young Paul Dirac had joined R H Fowler as a research student in 1923, after getting a degree in electrical engineering. For two years he worked on applying Hamiltonian methods to multiply–periodic systems in the framework of the Rutherford–Bohr model, but that did not lead to any significant success. Then in September 1925, his lucky break came when, by a somewhat roundabout route, he learnt of Heisenberg's discovery of matrix mechanics. This was the spark that ignited him. He soon elaborated, practically in isolation, his own version of quantum mechanics, giving it a particularly abstract and elegant structure. One might remember here that Heisenberg's achievement had been aided by continuous contact and exchange of ideas with Bohr, Born, Pauli, Kramers and Sommerfeld. In any case, once the key step had been taken by Heisenberg, progress towards the establishment of a mathematically satisfactory quantum mechanics was extremely rapid and was essentially finished by early 1927. Schrödinger's discovery of wave mechanics had come in early 1926, and its equivalence to Heisenberg's version soon after. One of Dirac's key contributions in this phase was the exposure of the link between classical and quantum mechanics. This was the most beautiful expression of the Correspondence Principle and, said Dirac, it had given him the most pleasure of all his discoveries.

From 1925 to 1927, the most important advances were being made by Dirac in Cambridge, Heisenberg, Born and Jordan in Göttingen, and Schrödinger in Zurich. During this period, Bohr was in a sense watching from a distance, with a critical but approving attitude. He had inspired and oriented the work of the others; and the new theory had attained the goals he had set himself all along. The departure from classical physics he had sensed and foreseen for so long was now explicitly expressed; relations among physical quantities could no longer be maintained in the classical numerical sense, but only in a more abstract algebraic sense. Every physical attribute of a system could not always be reduced to a number. When the stage was set to find the physical meaning of the mathematical structure, Bohr re-entered the scene. The deeper understanding of the situation needed Bohr and his philosophical bent of mind. Indeed Heisenberg said of him:

> Bohr was primarily a philosopher, not a physicist, but he understood that natural philosophy, in our day and age, carries weight only if its every detail can be subjected to the inexorable test of experiment.

In early 1927, between the two of them, Bohr and Heisenberg developed what we now call the 'Copenhagen interpretation of quantum mechanics'. In this, they were greatly aided by the transformation theory of quantum mechanics, which had just been developed by Dirac and Jordan. Heisenberg's contribution was the uncertainty relations. Bohr's was the complementarity idea. According to the latter, every classical concept retains its usefulness in quantum mechanics, but not necessarily simultaneously. According to Bohr, this was the greatest lesson of quantum mechanics — that the classical concepts, each individually valid, might be mutually exclusive. In later years he would say that physics had by its simplicity shown the way to this profound idea, but that the idea itself was applicable to much more complex situations, such as the relation between physics and life.

Einstein critically attacked the Copenhagen interpretation at the two Solvay Congresses of 1927 and 1930, and it was Bohr who answered him each time and proved the logical consistency of quantum mechanics. Finally Einstein had to concede, saying only that he still felt there was an unreasonableness about it all. Of Bohr himself he said:

> His is a first-rate mind, extremely critical and far-seeing, which never loses track of the grand design,

and

> He is truly a man of genius, it is fortunate to have someone like that.

Turning our attention now to Dirac for a while, I have already recounted how he burst on to the scene in late 1925. Thereafter, he kept going like a house on fire, with a steady and staggering profusion of fundamental ideas and discoveries. One of his most important papers, on the quantum theory of the emission and absorption of radiation, was written at Bohr's Institute in Copenhagen; so he too had been drawn into Bohr's circle. By applying the principles of quantum mechanics to the electromagnetic field, Dirac brought to a successful conclusion the work begun by Planck in 1900, and also inaugurated quantum field theory. Then there was the discovery of the new statistics named after him and Fermi, the relativistic theory of the electron, the prediction of the positron and the general concept of antimatter, the idea of the magnetic monopole, and many more. In the midst of all this, he wrote the classic book *The Principles of Quantum Mechanics*, often compared with Newton's *Principia*. It would take a great deal of space to do justice to all that Dirac accomplished in this period. Just as Bohr had made the preceding era a heroic one, Dirac turned this one into the 'Golden Age of Theoretical Physics'.

There is a charming anecdote from the Solvay Congress of 1927, which is worth recalling. In the interval between two sessions, Bohr asked Dirac what he was working on, to which Dirac replied that he was looking for a satisfactory relativistic wave equation for the electron, which would combine special relativity and quantum mechanics properly. Bohr then told him that such an equation had already been found by Klein and Gordon, but before Dirac could explain why he was not satisfied with it, the bell rang and they had to go back to the next session. Dirac later said:

> ... It rather opened my eyes to the fact that so many physicists were quite complacent with a theory which involved a radical departure from some of the basic laws of quantum mechanics, and they did not feel the necessity of keeping to these basic laws in the way that I felt.

Dirac's style is essentially mathematical, and he turned out to be a master craftsman in the art of theoretical physics. He created with ease the mathematical tools that he needed. Bohr on the other hand was somewhat like Faraday. As Heisenberg said,

> ... his insight into the structure of the theory was not a result of a mathematical analysis of the basic assumptions, but rather of an intense occupation with the actual phenomena, such that it was possible for him to sense the relationships intuitively rather than derive them formally.

For Dirac, considerations of mathematical beauty and symmetry were of the highest importance, and he was matchless in the art of manipulating and working with the abstract. Bohr, on the other hand, was much more concerned with the problems of interpretation and communication, the difficulties and ambiguities inherent in language, and other such philosophical questions.

Dirac's writings have a characteristic and unmistakeable directness, simplicity and beauty. Bohr, on the other hand, is much harder

Michael Faraday
1791–1867

to read because each long sentence of his contains a great deal of thought in a highly compressed form. He spent a lot of effort in the choice of each important word. Bohr's style of work was to have a junior collaborator sit at a desk and take down notes while he himself kept pacing up and down the room, forming and changing and reforming his phrases and sentences. Watching him at one such session, Dirac apparently said something to the following effect:

> Professor Bohr, when we were young we were taught never to start a sentence until we knew how to finish it.

Bohr's speech and handwriting were, respectively, inaudible and illegible. On both counts, Dirac was far superior. As Bohr himself said:

> Whenever Dirac sends me a manuscript, the writing is so neat and free of corrections that merely looking at it is an aesthetic pleasure. If I suggest even minor changes, Paul becomes unhappy and generally changes nothing at all.

As I recalled earlier, Bohr was very deeply interested in the problems of biology, which he saw as a fertile field of application for his Principle of Complementarity. In fact, for him physics was a far simpler problem. In Dirac's writings I have been able to find a reference to biology. In his paper of 1931 concerned with the magnetic monopole, he says,

> There are at present fundamental problems in theoretical physics awaiting solution, e.g., the relativistic formulation of quantum mechanics and the nature of atomic nuclei (to be followed by more difficult ones such as the problem of life) ...

At another time he is supposed to have said that his equation for the electron explained all of chemistry and most of physics. Presumably for him, the problem of life was just one more of the things that theoretical physics would deal with in good time!

Bohr created and inspired an international school of theoretical physics; and his influence upon others was as much by direct contact and involvement in their struggles as through his writings. Dirac, on the other hand, worked largely on his own. He did not create a school of any kind, although his influence on others through his writings and ideas has been enormous.

In the years following the creation and completion of quantum mechanics, Bohr turned to the problems of nuclear physics while

Dirac was more concerned with relativistic quantum field theory and later on with gravitation and cosmology as well. However, there is a classic contribution by Bohr along with Rosenfeld in 1933 to quantum field theory. They analyzed the consistency of applying the principles of quantization to the electromagnetic field — something which Dirac had done in 1927 — and demonstrated the logical necessity of doing this if the quantum mechanics of particles and, in particular, Heisenberg's uncertainty relations were to be maintained.

As human beings, there is a great deal worthy of admiration in both Bohr and Dirac, and a touching simplicity and sincere modesty in their dealings with others. Dirac was always most ready to acknowledge his debt to others. And in seminars, it seems that Bohr would always preface his questions with the statement that he only wished to better understand the speaker's point of view. Bohr concerned himself with political matters and spoke a great deal on philosophical issues as well, while Dirac seems to have avoided both these areas. Bohr was quite categorical that quantum mechanics was complete; and the most valuable lesson it had taught us was that of complementarity. He was anxious to extend its application to other fields such as reason and instinct, heredity and environment, physics and biology. His debate with Einstein, begun in the 1927 Solvay Congress, continued for more than two decades, and he adhered to his point of view. In the 70s however, Dirac had this to say,

> ... the present form of quantum mechanics should not be considered as the final form. It is the best that one can do up till now. But one should not suppose that it will survive indefinitely into the future. And I think that it is quite likely that at some future time we may get an improved quantum mechanics in which there will be a return to determinism and which will, therefore, justify the Einstein point of view.

One is left speculating on what Dirac actually had in mind.

Physicists are familiar with many lovely sayings and stories about and by Bohr and Dirac. And they are all really a reflection of their greatness as human beings. Bohr was always a synthesizer of conflicting points of view. On one occasion he said,

> The opposite of a correct statement is a false statement. But the opposite of a profound truth may well be another profound truth.

On another occasion he is quoted as saying,

> There are things that are so serious that you can only joke about them.

One of Dirac's most celebrated statements was about the value of mathematical beauty in physics. He said,

> ... it is more important to have beauty in one's equations than to have them fit experiment ... It seems that if one is working from the point of view of getting beauty in one's equations, and if one has really a sound insight, one is on a sure line of progress.

This reminds us of the poet John Keats saying, "What the imagination seizes as beauty must be truth — whether it existed before or not."

Bohr paved the way from the world of classical physics to the world of the quantum, guiding everybody through the most difficult period with his unerring instinct and intuition. And when the great victory had been won, it was he who most comprehensively assessed the impact it had for the nature and goals of science. Dirac was one of the chief architects of the victory, and he went on to raise theoretical physics to unparalleled heights of imagination and beauty. As much for their heroic labours as for their great human qualities, Bohr and Dirac will always rank among the greatest scientists of all time.

3 *The Mathematical Style of Modern Physics*

Two important ingredients in the mathematical style of modern physics are the many roles of symmetry, and the uses of unobservable quantities. Here, I would like to recount and review them, taking examples from pre-relativistic and relativistic physics, particle and field mechanics, classical and quantum theory as illustrations. In order to be accessible to a wide readership, what will be presented will not be the latest technical advances in this field, but instead some characteristic features it has acquired over the last few decades and which are, of course, shared by recent developments. And some well-chosen quotations which help illuminate our understanding of these concepts.

A certain well-known book on mechanics describes physics as the science of measurement and change. In physics, as in other natural sciences, particular phenomena are isolated far enough to make precise observations and measurements, then models and theories are constructed in our minds to explain them and predict new phenomena. This involves relying on refined instruments of observation to aid our limited human senses, especially as we explore phenomena far removed from the human scale. Such instruments are, of course, based on previously understood phenomena and can be regarded as extensions of ourselves. The important point is that as we look at processes taking place at the microscopic or the macroscopic level, far smaller or far larger than ourselves, intuition gathered from everyday experience often fails as a guide to understanding. In its place we have to develop and rely on mathematics as our guide and develop it into a sixth sense.

Mathematics is of course used, and most effectively, also to describe phenomena on our own scale, and it is easy to underestimate the difficulties faced in the past in the creation of new concepts. Be that as it may, it is generally agreed that with the developments of relativity and quantum theory, the texture of theoretical physics has become much more subtle and abstract than could have been anticipated. This situation was described by Dirac in 1931 in these words:

> The steady progress of physics requires for its mathematical formulation a mathematics that gets continually more advanced. This is only natural and to be expected. What, however, was not expected by the scientific workers of the last century was the particular form that the line of advancement mathematics would take, namely it was expected that the mathematics would get more and more complicated, but would rest on a permanent basis of axioms and definitions, while actually the modern physical developments have required a mathematics that continually shifts its foundations and gets more abstract. Non-Euclidean geometry and non-commutative 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 generalization of the axioms at the base of the mathematics rather than with a logical development of any one mathematical scheme on a fixed foundation.

This passage eloquently conveys the changing relationship between mathematics and physics at the fundamental level. It can well be contrasted with, say, the situation in fluid dynamics where the basic equations of Navier and Stokes have been known for a very long time and the problem lies in solving them under various conditions.

As part of this changing style, in which mathematical structures are used in physical theories, let us now look at two sets of ideas. One is the increasing importance of the ideas of symmetry and invariance; the other is the often unavoidable use of unobservable quantities in physical theories.

On the eve of his retirement from the Institute for Advanced Study, Hermann Weyl gave a set of lectures on Symmetry which have since become a classic. In it he says,

> Symmetry, as wide or as narrow as you may define its meaning, is one idea by which man through the ages has tried to comprehend and create order, beauty and perfection.

The subject of Weyl's discourse was symmetry in the static sense, the most immediate sense in which we all, at first, appreciate this notion. To say that an object is symmetric — such as a beautiful building or a well-grown crystal — is to say that it presents the same appearance before and after the application of certain transformations to it. These transformations are geometrical in character, being made up of rotations, reflections and translations; and the symmetry of an object is conveyed by the set of all transformations that leave it unchanged. The mathematical language to handle such static symmetry — static because time is not involved — is developed in Weyl's book and it is the theory of finite and of discrete groups. But the focus of the present discussion is not the static symmetries of objects in space; rather it is the symmetries of physical laws describing processes taking place in space and time, and to appreciate this requires some amount of abstraction. In Bargmann's words,

> ... those laws of physics which express a basic 'invariance' or 'symmetry' of physical phenomena seem to be our most fundamental ones.

Symmetry in this more fundamental sense operates at three levels which may be called the descriptive, the restrictive and the creative. To see this, let us first recall with Wigner that there are three ideas of equal importance when discussing any set of physical laws: these are the laws themselves, then the allowed choices of initial conditions, and finally the symmetries of the laws. Again, as Wigner says,

> The purpose ... of all equations of physics is to calculate, from the knowledge of the present, the state of affairs that will prevail in the future.

To begin with, let us consider such deterministic laws of motion alone. They tell us, given some observed initial condition of a physical system, how the system evolves and what its condition will be at all later times. Thus, each solution of the equations determines one possible sequence of states in time, one history, corresponding to one choice of initial condition. In this context, a symmetry is an operation that leads us from one solution of the equations of motion to another generally different one. Such a symmetry is not a property of the condition of a physical system at an initial stage or any other time; rather it consists in the unchanging relationship at each time between the physical conditions of two different histories or solutions of the equations of motion. As opposed to static symmetry, this is a dynamical concept describing a property of the concerned physical laws and not of this or that state or condition. It is the equations that are preserved under the symmetry operation; this makes it somewhat abstract, since the symmetry "cannot be seen by the eye but only by the mind."

In this sense, one says that the equations of mechanics of Galileo–Newton are symmetric or invariant under the transformations of the Galilei group. Similarly, the Maxwell equations of the Faraday–Maxwell theory of electromagnetism are symmetric under the Lorentz – or better still, the Poincaré – transformations. And these are the two prime instances of the descriptive role of symmetry, since in both cases the relevant equations were discovered well before the complete understanding of their respective symmetries.

Descriptive Role of Symmetry
Galilean–Newtonian Mechanics: Galilei Group and Transformations
Faraday–Maxwell Electromagnetism: Poincaré Group and Lorentz Transformations

However, early this century there came a shift in emphasis and a change to a new point of view, due principally to Poincaré and Einstein. It arose from the realization that the Lorentz transformations and Lorentz invariance, though first seen in the context of Maxwell's equations, actually described the general properties of space, time and measurement, and so, had a much wider significance. This led to the use of symmetry as a restrictive principle in the construction of new theories. In the words of Bargmann again, speaking of special relativity which governs space–time in the absence of gravitation,

> ... every physical theory is supposed to conform to the basic relativistic principles and any concrete physical problem involves a synthesis of relativity and some specific physical theory.

Many striking examples of the restrictive role of symmetry are concerned with special relativity; some are in the framework of classical physics, others in connection with quantum theory and quantum mechanics, and yet others with quantum field theory. It is well worth devoting some space to quickly recount them.

James Clerk Maxwell
1831–1879

Restrictive Role of Symmetry Mass Energy Equivalence $E = mc^2$ Ten Conservation Laws Dirac–Lorentz Equation Sommerfeld's Fine Structure Formula Photon Momentum $P = E/c$ Planck's $E = h\nu$ to de Broglie's $P = hk$ Dirac's Electron Equation Weyl's Neutrino Equation Wigner's Analysis of Elementary Systems Fermi's Weak Interaction Theory Pauli's Spin Statistics Theorem Tomonaga–Feynman–Schwinger's Renormalization Theory

The most famous classical result is perhaps the equivalence of mass and energy, $E = mc^2$; this came from amending the Galilean–Newtonian mechanics of material particles so that it would also share the Lorentz invariance of electromagnetism. Thus, the two separate pre-relativistic conservation laws of mass and energy were combined into one. More generally, special relativity or the Lorentz invariance of a theory (almost) automatically ensures the ten basic conservation laws of energy, momentum, angular momentum and moment of energy. One of the most impressive uses of this was Dirac's 1938 treatment of the classical relativistic point electron: using essentially only the energy-momentum conservation laws he was able to obtain equations of motion, now called the Lorentz–Dirac equations, including the radiation–reaction terms. In the period of the old quantum theory, one can recall the use of special relativity by Sommerfeld in deriving the fine structure of the hydrogen spectrum. To that same period also belongs the association of a momentum to a light quantum with the energy-momentum relation $E = Pc$, which can only be understood on the basis of special relativity. Slightly later, special relativity showed de Broglie the way to extend Planck's energy-frequency relation $E = h\nu$ to his own momentum wave number relation $P = hk$ for material particles. He thus associated a relativistic wave with a moving particle, the particle properties of energy-momentum being proportional to the wave properties of frequency and wave number through Planck's constant. Turning to quantum mechanics, we first have the amazing discovery of the relativistic wave equation for the electron by Dirac, in 1928. It came about by combining three elements — the general structure of quantum mechanics, the requirement of symmetry with respect to special relativity, and the genius of Dirac — and it ended up explaining more things than its discoverer could have hoped for — the spin of the electron, its magnetic moment, the hydrogen fine structure, and the existence of the positron and antimatter. This last was, of course, a prediction and not an explanation. After this inauguration of relativistic quantum mechanics, one can mention Weyl's discovery of the wave equation for the massless neutrino; and later Wigner's analysis of the quantum mechanical representations of the symmetry group of special relativity, which gave a systematic classification of all possible free relativistic systems. Finally, in this recounting of the restrictive role of symmetry, we have some instances from quantum field theory and elementary particle physics. Soon after Fermi constructed a theory of the weak interactions in 1934, it was seen on the basis of special relativity that there were five independent forms for this interaction. This was based on the assumption that space reflection was a symmetry of nature. After it was shown by Lee and Yang in 1956 that this was not a valid symmetry for weak processes, the number of forms of interaction allowed by relativity jumped to ten; but it was quickly reduced to one by the discovery, in 1957, of the universal V-A interaction by Sudarshan and Marshak. This incidentally then led to a new symmetry called Chirality. In quantum field theory itself the remarkable connection between spin and statistics — the fact for instance that photons obey Bose Statistics while electrons obey Fermi Statistics — was shown by Pauli to be a consequence of relativity. In fact, he concludes his paper on the subject with the words,

> ... we wish to state, that according to our opinion the connection between spin and statistics is one of the most important applications of the special relativity theory.

Later, in the 1940s, relativistic invariance was one of the crucial guiding principles that enabled Tomonaga, Feynman and Schwinger to develop a consistent way to handle divergences and infinities in quantum field theory calculations, the renormalization theory, and thus to make meaningful predictions that could be compared with experiment.

These illustrative examples of the restrictive function of symmetry show the power and the fruitfulness of the point of view introduced

Chen Ning Yang
born-1922

by Poincaré and Einstein in the early 1900s. It is by carrying these to a higher level of sophistication — by pursuing them to their logical conclusion in various contexts, so to speak — that one arrives at the creative role of symmetry.

Creative Role of Symmetry	
Abelian Gauge Invariance	Electrodynamics
General Coordinate Transformation Invariance	General Relativity
Non-Abelian Gauge Invariance	Yang–Mills Theory

This is, however, quite a subtle step which has delicate connections with the second main idea I wish to present, namely the use of unobservable quantities in physical theories. Maxwell's electromagnetism is a relatively simple instance, while the general theory of relativity and the more recent non-Abelian gauge theory are quite complex instances, of this situation. Before going on to a description of these inter-relationships, it may be helpful to recall the words of Dirac which so beautifully motivated the transition from the restrictive to the creative role of symmetry:

> 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.

Let me describe the use of unobservable quantities in physical theories, which occur at several levels, so that at a suitable level the interface with the creative function of symmetry can be brought in. The ideas are best conveyed through examples, the first of which is from the field of classical optics. If one takes a black and white photograph, say, one is making a record of the variation of the total intensity of light over the photographic film at a certain time. A colour photograph records the intensities of light at various frequencies. Now the fundamental theory of light at the classical level is given by the electromagnetic field equations of Maxwell. They tell us how, from given initial conditions, the electric and magnetic fields develop in the course of time. However, the intensity of light essentially involves the sum of the squares of the electric and magnetic fields; and it is not true that if we knew the initial distribution of light intensity, say in some region of space and time, we could predict it elsewhere or at a later time. If we had provisionally defined the intensity of light as the only observable quantity in optics, then in order to see how intensity changes with space and time, we would have been forced to introduce something called the two-point correlation function — an unobservable quantity at this level — and express the laws of evolution in terms of it. The two-point function is a measure of the correlation between the electromagnetic field at one point of space at one time and at another point of space at a possibly different time. It is of the same mathematical nature as, but physically distinct from, the light intensity. The Maxwell equations for the electric and magnetic fields lead to definite laws of propagation for the two-point function. But intensity, being a particular case of the two-point function, does not obey any propagation law on its own. Once one admits that the Maxwell fields are observable, then so is the correlation function. This example is in a sense rather elementary since what is initially regarded as unobservable becomes, in a wider framework and with better understanding, an observable quantity.

Our next and less trivial example concerns electromagnetism again, but now assuming that the electric and magnetic fields are—at least classically — observable. In the presence of classical charged particles the combined system of Maxwell's equations for the field strengths and Lorentz's equations for the particles involve observable quantities only — field strengths on the one hand, and particle positions on the other. The system is deterministic in the sense assumed earlier, and is also local. In practical calculations one finds it convenient to express the field strengths in terms of an auxiliary quantity called the vector potential. However, the potential is, in principle, unobservable because there are transformations or changes in the potential — gauge transformations as they were called by Weyl — which do not change the observable field strengths at all. Quite generally, even in other contexts, gauge transformations are transformations which vary continuously but arbitrarily, from point to point in space–time, staying of course within a given class; and those quantities which do change under a gauge transformation are unobservable. As a result, the equations for the potential cannot completely determine it since they must allow for an arbitrary gauge transformation; but this causes no problem as the potential was introduced only for convenience and can be dispensed with. But the situation changes when the charged particles are subject to the laws of quantum mechanics, assuming for the moment that the field is classical and externally given. The quantum equation of motion for the particles, Schrödinger's equation, uses the vector potential in an essential way. In quantum theory it is much more awkward to eliminate the unobservable vector potential than it is in the classical theory. One can do so and it has been done, not only for the case considered but also for the complete system of quantized matter and Maxwell fields, using a method due to Dirac and Mandelstam. But one then has to work with non-local quantities and equations—quantities depending not just on a point in space–time, but on an arbitrary path leading up to that point. If one is prejudiced in favour of locally defined quantities and equations, one has to use the unobservable vector potential with the associated freedom of gauge transformations.

The third example concerns general relativity. The original way in which the equations of this theory were derived and presented depended very heavily on the invariance requirements placed upon them. These requirements were strong enough to almost determine the equations—the creative role of symmetry. One considers events taking place in space and time and describes them with the help of space and time coordinates. The essential point now is that one allows a great deal of freedom in the assignment of coordinates to events and demands that the equations of the theory must retain their form under any changes of coordinates. This requirement of symmetry renders the coordinates really unobservable. In Wigner's words,

> The basic premise of this theory is that coordinates are only auxiliary quantities which can be given arbitrary values for every event ... coordinates are only labels to specify space–time points. Their values have no particular significance unless the coordinate system is somehow anchored to events in space–time.

Nowadays, relativists use the term "coordinate markers" to convey this quality of coordinates and compare the situation to a telephone directory, indeed one of the best-known books on the subject is a telephone book. As long as one retains the freedom to make arbitrary changes of coordinates, they cannot be anchored to space–time events in any way and so remain unobservable. Of course, in recent times more refined mathematical methods have been brought in to formulate the laws of general relativity in what is called an intrinsic coordinate-free description, thus eliminating the unobservable coordinates altogether. Nevertheless, the problem of deciding what mathematical quantities are observable remains tricky and has no easy answers.

The non-Abelian gauge theories discovered by Yang and Mills in 1954 — and which are basic to the unification of electromagnetism and the weak interactions and also to the currently accepted theory of nuclear forces — stand midway between electromagnetism and general relativity in complexity. The arbitrary space–time dependent transformations now do not act on the space–time coordinates but rather in an internal space describing properties which are a generalization of electric charge. Once again, there is a vector potential which changes under these transformations, but it is more intricate than in the case of electromagnetism since now even the analogues of electric and magnetic fields change when the potential changes. This makes both the potentials and the field strengths unobservable. Here again, the increased demands of symmetry are powerful enough to almost determine the basic equations; the difference is that now the analogues of the Maxwell equations involve the potential in an essential way. The problem of constructing observables is somewhat more easily solved here than in the case of relativity, while the non-locality involved in trying to express everything in terms of them is more severe than in the electromagnetic case.

At this stage, some general comments connecting the creative function of symmetry to the use of unobservable quantities can be made.

Gauge Symmetry	* Non-observables * Non-deterministic equations * Restriction on initial conditions

At any rate at a classical level, one can say that in a theory without any symmetry of the gauge type, such as Galilean–Newtonian mechanics or Maxwell–Lorentz electrodynamics not using the potential, all quantities in the theory are in principle observable and the basic laws can be expected to be deterministic. However, in the presence of a gauge type symmetry, three related things happen–those quantities which change under the transformations must be regarded as unobservable; because of the arbitrary elements in these transformations the equations of motion cannot be fully deterministic; and on the technical side, restrictions emerge on the allowed initial conditions. In terms of the three components involved in the discussion of any set of physical laws — the laws themselves, the possible initial data and the symmetries — it means that an increase of the third component to gauge type symmetries has important repercussions on the first two components. If the freedom to perform gauge transformations is maintained, one has local quantities obeying local but not completely deterministic equations; if one wants to work with observable quantities alone, some degree of non-locality is unavoidable. Conversely, the fully local description will involve some unobservable quantities.

The creative uses of symmetry in both general relativity and non-Abelian gauge theory give these theories a strongly geometric flavour. One is reminded of Klein's well-known Erlangen program and gets the feeling that physics is being geometrized or becoming geometry. What saves the situation is that, as Regge said, physics is not geometry but geometry plus an action principle. Hence, the statement made more than once earlier that gauge type symmetry almost completely determines the form of the basic equations, but not quite.

While unobservable quantities seem to be closely related to local symmetries at the classical level, this connection is weakened in quantum theory, which is the fourth and last of our examples. In some respects the situation is similar to that of classical optics, except that it is very likely not provisional. According to quantum mechanics, not all the physical quantities associated with an atomic system can be simultaneously measured or specified as numbers. In this sense, there are definite limitations on the amount of "information" we can have about an atomic system at one time. If by means of a measurement one has obtained the maximal permitted information at a certain time, it can be mathematically represented by something called a wave function. The basic laws of quantum mechanics then determine how the wave function varies with time, and at that level things are deterministic. However, the wave function itself is unobservable. At any given time, the wave function determines the probabilities for various outcomes of various experiments that may be performed at that time, and these probabilities are essentially quadratic in the wave function. Thus, the observable quantities are essentially these probabilities, but there is no way to directly calculate how they change and evolve in time. There is no way of avoiding the use of the unobservable wave function, or something essentially like it, so as to be able to express all the features of quantum phenomena.

This discussion of the uses of unobservable quantities in physical theories shows that the rule of three operates here just as it does in so many other contexts.

The Rule of Three

	* Fundamental equations
Physical laws	* Initial conditions
	* Symmetries
Symmetries	* Descriptive
	* Restrictive
	* Creative
Non-observables	* Provisional, Temporary
	* Convenience, Locality; Avoidable with effort
	* Essential, Unavoidable

Thus, such quantities may appear in a provisional and temporary sense alone; or they may be used as a matter of convenience, it being a matter of lesser or greater difficulty to dispense with them; or finally, they may be essential and unavoidable. If we have not come across any of these possibilities, we may feel that there is something strange or even alarming in non-observable quantities playing such an important role in physical theory. But we can take comfort in the words of Max Planck,

> It is absolutely untrue, although it is often asserted, that the world picture of physics contains, or may contain, directly observable magnitudes only,

and in Richard Feynman's reassurance,

> It is not true that we can pursue science completely by using only those concepts which are directly subject to experiment. In quantum mechanics itself there is a probability amplitude, there is a potential and there are many constructs that we cannot measure directly .... It is absolutely necessary to make constructs.

This suggests that these ideas have a wider range of relevance than just in physics, and one also recalls Einstein's advice to Heisenberg,

> It is never possible to introduce only observable quantities in a theory. It is the theory which decides what can be observed.

It has been said that each generation of physicists feels that the next generation is too mathematical. Why is this so and why does physical theory get more and more abstract as it develops? One can do no better than quote Dirac in answer,

> The methods of progress in theoretical physics have undergone a vast change during the present century. The classical tradition has been to consider the world to be an association of observable objects (particles, fluids, fields, etc) moving about according to definite laws of force, so that one could form a mental picture in space and time of the whole scheme. This led to a physics whose aim was to make assumptions about the mechanism and forces connecting these observable objects, to account for their behavior in the simplest possible way. It has become increasingly evident in recent times, however, that nature works on a different plan. Her fundamental laws do not govern the world as it appears in our mental picture in any very direct way, but instead they control a substratum of which we cannot form a mental picture without introducing irrelevancies.

What a contrast to Lord Kelvin's statement from the last century,

> It seems to me that the test of 'Do we or do we not understand a particular point in physics?' is 'Can we make a mechanical model of it?'

Far from this, it has become increasingly necessary to rely on our feeling for the abstract and on our mathematical sensibilities in trying to comprehend the developing physical picture of nature. And though I have quoted from many Masters, it seems that more than anyone else the writings of Dirac express beautifully the style, and his works have contributed a great part of the content, of the changing mathematics that underlies modern theoretical physics.

4 *The Mathematics and Physics of Quantum Mechanics*

In this chapter I would like to describe some mathematical and physical aspects of quantum mechanics, in the hope that this might be of general interest. The mathematical structure of quantum mechanics is quite rich and beautiful. However, even though the theory is more than seventy years old now and amazingly successful in its practical applications, arguments and debates about its physical interpretation still continue. Many of its predictions run counter to intuition developed from "common experience." I will try to show how the quantum concepts and views have developed, starting with the classical ones. Some of the important mathematical features of the quantum mechanical formalism will be highlighted, and then we shall see what it is that makes the conventional physical interpretation seem strange in several respects.

4.1 The classical picture of a physical system

The foundations of the dynamics of particles and material bodies were laid by Galileo and Newton. They created a conceptual framework for a mathematical account of space, time and motion; and then Newton's laws made quantitative description and prediction in dynamics possible. Their work was elaborated and given a beautiful and flexible mathematical form by Euler and Lagrange, and at the same time, the extension to elastic continua and fluid dynamics was also achieved. The culmination of the formal development came with the work of Hamilton and Jacobi, to whom we owe the phase-space formalism and transformation theory of classical dynamics. What is fundamental here is what is today called a symplectic manifold, an even-dimensional space with a particular kind of geometrical structure. The state of a physical system is pictured as a point in such a manifold, while its various physical properties are represented by corresponding functions on it. The representative point moves in time, obeying first order differential equations of motion, and the entire evolution in time can be pictured as a phase–space trajectory.

The most important features of this classical view of dynamics are that it allows one to visualize in complete detail the state of a system at each instant of time, and to observe in equal detail how things change in the course of time. All this is supposed to be possible without in any way affecting or interfering with the system, so the description is of things "as they really are." All physical properties of the system are in principle simultaneously measurable and expressible in numerical form, and they change continuously in a deterministic way, in any state of motion, obeying the dynamical laws. Of course, a general state would be an ensemble or statistical distribution over phase–space, but the most elementary or pure states are of the above type, with every physical quantity being dispersion-free. The discoveries of Faraday and Maxwell enlarged the scope of dynamics to include fields in addition to material bodies. But the features of complete visualizability and reduction of all physical properties to numerical values, were unchanged. This mechanical view of things was expressed by Lord Kelvin in these words,

> It seems to me that the test of 'Do we or do we not understand a particular point in physics?' is 'Can we make a mechanical model of it?'

A little later, special relativity changed our understanding of the geometric nature of space and time as compared to the Newtonian view; while general relativity made space–time geometry itself dynamical and subject to equations of motion. Nevertheless, in spite of all the subtleties involved, we can say that these were the grand finishing touches to the evolution of the classical view which has been well expressed by Dirac thus:

> The classical tradition has been to consider the world to be an association of observable objects (particles, fluids, fields, etc) moving about according to definite laws of force, so that one could form a mental picture in space and time of the whole scheme.

However, he went on to say:

> It has become increasingly evident... that nature works on a different plan. Her fundamental laws do not govern the world as it appears in our mental picture in any very direct way, but

> instead they control a substratum of which we cannot form a mental picture without introducing irrelevancies.

Let us see how this came about.

4.2 Evolution of the quantum concepts

Quantum theory was officially born on the early evening of Sunday, October 7, 1900, when Planck discovered the radiation law and the constant of nature that bear his name. His discoveries showed a completely non-classical graininess in nature. In the ensuing years, insights into the effects of the quantum of action, such as the existence of photons, came largely through statistical arguments. It was in 1912–1913, while working on the problem of the stability of matter, that Niels Bohr linked Planck's constant to the mechanics of the atom in a direct way.

The problem with the classical picture was that if an electron were orbiting the atomic nucleus obeying classical equations of motion, it would continually lose energy by radiating away electromagnetic waves with a continuum of frequencies, and finally collapse. In contrast, atoms were evidently stable; and experiment showed that each element emits (and absorbs) only radiation with a certain discrete set of frequencies, which acts as its "fingerprint." Bohr made two revolutionary non-classical postulates to explain these facts. Out of the continuum of possible states allowed by the classical equations of motion, the electron could exist only in one of a certain discrete set, and in this set it would not radiate away its energy. This set of permitted states was selected from the classical continuum by a quantum condition involving Planck's constant, and so the possible energies for the electron also formed a discrete set. The next postulate was that radiation was emitted or absorbed only when the electron made a transition from one allowed state to another; the energy difference determined the frequency of the emitted or absorbed photon, thereby explaining discrete spectral lines.

Emission and absorption of radiation were thus connected with pairs of allowed quantum states, not just a single classical one. In a classical periodic motion, say, the position coordinates for the orbit would be expanded in a Fourier series, and from the expansion coefficients one could calculate the intensity of radiation emitted. At all stages one could "see" what was going on. In Bohr' s model, however, visualizability was retained only for the electron in any one of its allowed states. namely, that it followed the corresponding classical orbit; but the transition or "jump" from one such state to another just occurred, and could not be pictured at all. Bohr's original quantum condition was given a neat form by Sommerfeld in 1915. And in 1917, Einstein gave a new derivation of Planck's law based on Bohr's postulates. Here again the basic quantities referred to pairs of states rather than only one, and by now it had become clear to Einstein that chance and probability had entered physics in a very fundamental way.

The Bohr–Sommerfeld form of the quantum theory, which was adequate for simple systems, increasingly ran into problems for complex systems, and by the early 1920s it was clear that there was a crisis. The expectations at that time were well described by Max Born,

> It slowly became clear that this was the main feature of the new mechanics: each physical quantity depends on two stationary states, not on one orbit as in classical mechanics. To find the laws for these 'transition quantities' was the problem.

The problem was solved by Heisenberg in the summer of 1925. He completely gave up the attempt to visualize, in any classical sense, the motion of an electron in the atom. Instead he argued that both position and momentum — classically understood as numbers — ought to be represented by arrays of numbers, each entry corresponding to one possible transition between the allowed quantum states. But these states were not visualized at all, not even in the limited way permitted in Bohr's theory. Heisenberg then posed the following problem: Classically, the square of a real number, such as its position, is another real number. If now position x is an array, and not a single number, what is the meaning of its square? Then, guided by the Ritz Combination Law of Spectroscopy, he showed that x^2 is another array, obtained from x by a row-into-column rule of multiplication! The situation for momentum was similar. While Heisenberg did not know it, Born soon realized that these arrays and their multiplication law were just matrices and matrix multiplication. (We see that even if the theory of matrices had been unknown to mathematics, Heisenberg's work and the Ritz Law would have led to it!) Finally Heisenberg, and then Born and Heisenberg together, showed that the Bohr–Sommerfeld quantization rule could be expressed in terms of the matrices for position and momentum as the commutation relation

$$xp - px = \frac{ih}{2\pi}$$

Born was the first to grasp that the classical numerical position and momentum had been replaced by non-commuting quantities.

Heisenberg retained the classical Hamiltonian equations of motion, but gave new meanings to the quantities appearing therein. The concept of the state of a system was present only to a very limited extent—only those of definite energy were present, and they were also merely enumerated. The physical quantities or dynamical variables were the important objects, and they too were abstract arrays. While Heisenberg's mechanics was indeed successful, one can understand why Schrödinger said,

> ... I was discouraged, if not repelled, by what appeared to me a rather difficult method of transcendental algebra, defying any visualization.

This situation was remedied by Schrödinger himself very soon, with his discovery of the wave equation bearing his name. We shall have more to say about this later on. He also went on to show the equivalence of Heisenberg's matrix form of quantum mechanics to his own wave form. More important is the fact that the general quantum mechanical concept of the state of a physical system became clear for the first time, and there was an equation of motion for it—the Schrödinger wave equation. As Dirac recalled in 1941, Heisenberg's form is difficult, in that "it does not provide any description of radiative transition processes", whereas Schrödinger's method "supplies, in a certain sense, a description of what is taking place in nature." Schrödinger's wave equation also made it far easier to apply quantum mechanics to practical problems than Heisenberg's mechanics.

While many persons — among them Dirac and Wigner — were apparently quite close to giving a physical interpretation to the Schrödinger wave function ψ, it was Max Born who stated most clearly the meaning conventionally accepted for it: ψ is a (complex) amplitude for a probability, its absolute square is a probability. Thus, ψ itself is not an objectively real thing, but it is required for the calculation of probabilities for processes in a completely non-classical way.

4.3 Some mathematical aspects of quantum mechanics

The distinction between physical properties or dynamical variables of a system, and its possible states — usually left implicit in classical physics — becomes explicit and more significant in quantum mechanics. Dynamical variables, understood classically in a purely numerical sense, now become elements of a non-commutative algebra, while the states can be described by the elements of a complex linear space. The non-commutativity on the one hand, and the linearity on the other, are the two most distinctive non-classical features of quantum mechanics. One sees that both algebraic and geometric structures go into making up the edifice of quantum mechanics. As already recounted, Heisenberg was the first to suggest that physical quantities be divested of their purely numerical nature, and be replaced by arrays obeying a new multiplication law. However it appears that when he saw, with Born's assistance, that this multiplication was non-commutative he was quite disturbed. On the other hand, for Dirac, this non-commutativity became the central feature of quantum mechanics. He gave such dynamical variables the name 'q-numbers' as against classical 'c-numbers'; conceived of them in a more general way than the discrete arrays of Heisenberg, and even calculated the limiting classical form of the commutator of any two q-numbers! We shall come later on to the question of relating q-numbers to numerical values via measurement. While commutativity is lost, associativity is however retained; this is why dynamical variables can be treated as matrices or linear operators on a linear space. At one time, quite early on, P Jordan toyed with the idea of a mechanics in which even associativity was sacrificed (as happens with octonions). But the idea and the attempt did not prove fertile or fruitful.

Turning to the description of states, the Schrödinger wave equation is a first order, linear differential equation in time for the wave function ψ, so constant linear combinations of solutions are again solutions. This superposition law is a key feature of quantum mechanics. The (pure) states of a quantum system form a complex Hilbert space — more precisely, they are the rays of such a space — and they are the most elementary states possible. What makes superposition non-classical is this: in classical mechanics the only way we can put together two pure states to get a third state produces a statistical mixture, not another pure state; while in quantum mechanics superposition combines two (or more) pure states to yield more pure states.

In the classical limit, the Schrödinger equation yields the Hamilton–Jacobi partial differential equation, which is known to describe specially constructed families or bundles of phase–space trajectories. Thus, the closest classical analogue to the idea of a pure state

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after classical statistical mechanics, known as the Weyl–Wigner–Moyal form; the non-classical feature that shows up is that the replacement for the classical phase–space probability distribution is not necessarily non-negative. The physical reason for this is that, after all, there is no joint probability distribution for position and momentum. And the superposition principle is not at all obvious in this version. In the form patterned after classical statistical optics, due to Sudarshan and Glauber, the replacement for the classical probability density is quite a singular distribution. The path integral formulation of Feynman shows very clearly the departures from classical motion along a well-defined trajectory, while both non-commutativity of observables and the concept of a general state are arrived at only with some effort. Finally, one can cast quantum mechanics into what is called a hidden-variable form, which highlights a peculiar quantum non-locality which is decidedly non-classical. The difficulty with this form is that the addition of dynamical variables (and of course also the superposition law) becomes rather awkward or unnatural.

The second point has to do with the very "existence" of the wave function. In classical theory, one deals with physical quantities as real-valued functions on phase–space, so they naturally commute under point-wise multiplication; and one also deals with transformations on phase–space, possibly representing the action of various relevant groups on the physical system. Quite often these groups are non-Abelian or non-commutative, then so also are the transformations realizing them. In the case of Lie groups, the infinitesimal generators may be non-commutative. Thus, classical dynamical variables and transformation generators are mathematically very different; technically, the former are functions and the latter are vector fields. But in quantum mechanics, the dynamical variables are themselves non-commutative, so one can ask if there is a form of quantum mechanics in which transformation generators can also be dynamical variables. This can indeed be so, and is realized in the Schrödinger form based on the wave functions ψ and with transformations acting on ψ. The physical importance of this will soon be seen.

4.4 Physical interpretation and strangeness of quantum mechanics

I have stressed that the physical properties of a quantum system have fundamentally a non-numerical and algebraic nature. Perhaps the hardest thing to get used to in the usual interpretation of quantum mechanics is the fact that each physical property or dynamical variable does not, at all times, possess some definite numerical value. This is what forces us to give up the degree of complete and detailed visualizability of systems and processes we had grown accustomed to in classical physics.

The fact that dynamical variables form a non-commutative algebra, has many consequences. We do expect that if an experiment is set up to measure some real physical quantity, the result must be some real number. It happens that possible results of such a measurement are the eigenvalues of the corresponding linear operator. We cannot generally say in advance which value will result, but depending on the state just prior to the experiment, we can calculate the probabilities for various outcomes. The possible results of the experiment — the spectrum of eigenvalues of the operator — can often be quite different from the set of real values, usually a continuum, accessible to this same quantity classically. This is the cause of quantization of possible values of energy, angular momentum, etc. But the important thing is that we cannot imagine that before the experiment the concerned variable had a definite value, which the experiment then revealed. In quantum mechanics, physical systems do not on their own possess definite physical properties at all, if this means numerical values for dynamical variables. It is only when an experiment is done that the relevant quantity descends from its lofty algebraic status to the level of a number; and then again after some time it may cease to have a definite value, as a later experiment may reveal! We can speak of a dynamical variable having a certain value only when an experiment has been done and has resulted in that value. We have to be very careful with words in quantum mechanics; in a sense, this is Wittgenstein in action: "Whereof one cannot speak, thereof one must be silent." Depending on your nature, this state of affairs may be comforting or exasperating; it leads to papers with titles like, "Is the moon there when nobody looks?"!

The non-commutativity of dynamical variables also has the consequence that all of them cannot be simultaneously measured and reduced to numbers. Specifically, if two variables do not commute at a given time, an experiment can be set up to measure one of them but then not the other. We have stated that individual vectors in Hilbert space corresponding to single wavefunctions ψ, describe pure states, states with maximum possible information. But even in a pure state not all physical quantities are dispersion-free; some may have definite values, but then a whole lot of others do not. In fact, for two non-commuting quantities, the two dispersions are related by Heisenberg's uncertainty principle. However, as if to compensate for this fact and the resulting reduction in visualizability, the superposition law has the effect that there are many more pure states for a quantum system — by any method of counting — than for the corresponding classical system. As a suggestion, we can say that there is a definite need for more pure states in quantum mechanics to give each dynamical variable a chance of sometime being measured and brought down to numerical status, even if only momentarily!

An important consequence of what has just been said, is the enhanced role of symmetry in quantum mechanics. As mentioned earlier, in quantum mechanics, transformation generators are also dynamical variables; and one can ask if there are pure states in which such a variable has a definite value. The vastly increased number of pure states ensures that one can indeed have such states, which are then physically invariant under the concerned transformation. For example, parity or the operation of space reflection is an observable in quantum mechanics but not in classical mechanics; and there are far more pure quantum states unchanged by parity than in the comparison classical system. All this also explains the great relevance of the theory of linear representations of groups for quantum mechanics.

What of the wave function or the Hilbert space vector? What about its nature? It turns out that the wave function, ψ, is not something with a real objective existence external to ourselves, such as an electric or a magnetic field. At least, this is the conventional interpretation. It is "nonmaterial", not measurable with the help of any instruments. It is something we invent to represent our knowledge of the physical system as revealed by preparation or past measurement; and we use it to calculate the probabilities for various processes or future experimental results. It is this kind of interpretation for ψ that makes many people uneasy about quantum mechanics.

The wave function, ψ, is called a probability amplitude. For any given process or experimental outcome we are interested in, such an amplitude can be calculated, and taking its absolute square we get a corresponding probability. The basic equation of motion of quantum mechanics, Schrödinger's equation, is linear in the probability amplitude ψ, not in the probabilities themselves. In fact, there is no equation of motion that can be written exclusively in terms of probabilities alone. For any given process, one must add the amplitudes for each possible way in which we can classically conceive the process to have occurred, then take the absolute square of the total amplitude. This is essentially Feynman's presentation. Basically, it is amplitudes and not probabilities that add linearly in quantum mechanics—a new non-classical way of computing probabilities. But we must also be careful and speak only of the complete process, and not look "inside the black box" to see which classically available route or possibility was taken. If we do any such thing, we are "asking Nature a different question" and contemplating a different experiment, so there will be a different answer! Once again, the need for care with words, a reminder that "the road is difficult, the crossing is as the sharp edge of a razor".

If all this appears strange, there is more to follow! What "happens" to a wave function or state vector when we do an experiment to measure some dynamical variable and obtain some definite result? The rule is: the wave function "suffers a collapse", so we must immediately replace it by a new one, an eigenfunction of the dynamical variable for the concerned eigenvalue. It is impossible to obtain this prescription for the behavior of ψ from the linear Schrödinger equation. In fact, when no measurements are being made, we must evolve ψ smoothly and continuously according to Schrödinger's differential equation; but when a measurement is made and a result recorded, it jumps or collapses—two very different prescriptions for change of the wave function.

One often asks the question: is the framework of classical probability theory adequate for quantum mechanics? In discussing the problem of joint probabilities, coupled with the collapse postulate, one sees clearly that it is not! Imagine that an observable A with eigenvalues a is measured at a time t; and another observable B with eigenvalues b is measured at a later time t'. The collapse rule must be applied immediately after the first experiment at time t. We can calculate a joint probability distribution $P(a \text{ at } t;\, b \text{ at } t')$ to get the result a at time t, and followed by the result b at time t'. Summing up over all values of b does give us back the marginal probability distribution $P(a \text{ at } t)$ for various outcomes of the first experiment:

$$t' > t : \sum_b P(a \text{ at } t;\; b \text{ at } t') = P(a \text{ at } t)$$

But summing up over all values of a does not give us the expected probability distribution $P(b \text{ at } t')$ for various outcomes of the experiment done at time t', under the assumption that no attempt was made to measure A at time t:

$$\sum_a P(a \text{ at } t;\ b \text{ at } t') \neq P(b \text{ at } t')$$

This is a graphic illustration of the effect of the collapse postulate, and of the way in which classical and quantum joint probabilities differ.

The absence of trajectories for quantum mechanical particles has important consequences. The wave function for two or more strictly identical particles must have a special property; it must be either symmetric or antisymmetric when we interchange the variables of the two particles. This is because there is no way we can mark them as particle number 1 and particle number 2 and watch them travel and keep track of which is which—they don't travel on trajectories! Thus even for particles which would have been classically viewed as "non-interacting", there is in quantum mechanics either an affinity or a mutual repulsion caused by identity alone. We see here a blurring of the classical distinction between kinematics and dynamics.

Many of the strange and non-intuitive features of quantum mechanics recounted above bothered some of the founders of quantum theory — notably Einstein, Schrödinger and de Broglie — from the very beginning. Einstein declared,

> The belief in an external world independent of the perceiving subject is the basis of all natural science.

No doubt it was such an attitude that lay behind the construction of the well-known Einstein–Podolsky–Rosen paradox. Einstein used to refer to the conventional Bohr–Heisenberg interpretation of quantum mechanics as a "tranquillizing philosophy". Schrödinger too was unhappy with the idea that it was only observation that endowed a system with definite properties; and in an eloquent sentence he asks if before the evolution of living organisms with brains,

> Should it all have been a performance to empty stalls?
> Nay, may we call a world that nobody contemplates even that?

In a famous example, Schrödinger brought out the extremely bizarre consequences of extending the superposition principle to macroscopic dimensions. In a certain experiment involving an unfortunate

Louis Victor de Broglie
1892–1987

cat, he contrived a situation where, if quantum mechanics were applicable, the state of the cat would have to be described by a wave function which is a linear superposition of a wave function for a dead cat and the one for a living cat. Only the observation of the cat at a suitable time would lead to a definite outcome — it would be found alive or dead — but quantum mechanics forbids declaring or assuming that before the observation the cat was already definitely alive or dead. The observation causes the collapse of its wave function to one or the other state. But what of the cat's own feelings in the matter?

Heisenberg seemed willing to accept quantum mechanics in its present form, for he felt,

> Almost every progress in science has been paid for by a sacrifice, for almost every new intellectual achievement previous positions and conceptions had to be given up. Thus, in a way, the increase of knowledge and insight diminishes continually the scientist's claim on 'understanding' nature.

Today these problems are being examined and discussed quite vigorously again, and many people express their unhappiness with the conventional interpretation of quantum mechanics. It even appears that we are on the threshold of experiments which may indicate the limits of applicability of the superposition principle, where macroscopic systems are involved.

I have tried to expose some of the mathematical features of quantum mechanics that go to make it a truly beautiful theory. But I have also wished to communicate the fact that the physical interpretation of this extremely successful theory is quite problematical. Is one the price of the other? Late in life, Heisenberg reputedly discovered a new uncertainty principle—if the mathematics is clear, the physics is not, and conversely. Be that as it may, I hope that your curiosity has been excited, and you will ponder over the problems of quantum mechanics, but not merely as a mathematician!

5 *Aspects of the Interplay Between Physics and Biology*[1]

The interplay and exchanges between physics and biology form a very fascinating field of study, to which many distinguished persons have contributed. My aim here is not so much to present new physics to the physicists or new biology to biologists, but rather to present to each some interesting thoughts and provocative points of view from the other's field. This may enhance an appreciation and understanding of the entire situation.

Right away, physics and biology can be compared in quite strong terms as follows. While the aim of physics is to find and describe universally valid laws, governing processes and phenomena everywhere in the universe, the biologist is concerned with the very singular and unusual single occurrence of life on earth and, for practical purposes, on earth alone. To a certain extent, he is like a detective or archaeologist searching for clues about past developments and events in building up a picture of life and evolution as a whole. This basic contrast should be kept in mind: the contrast between the universal and the particular, despite the fact that within life itself, in all its forms there are many universal features. Another interesting contrast is that in our understanding and interpretation of biological processes, the ideas of a suitably defined *value* for each process and its *consequences* both play extremely important roles, in ways that are quite inappropriate in the context of physics.

Let me begin by briefly recounting some of the major developments in physics, more especially classical physics. Physics in the modern sense started with the work of Galileo and Newton, and especially, their demonstration that carefully controlled experiments, with the results expressed in mathematical terms, could lead to a deep and dependable understanding of the laws underlying natural phenomena. In this way, one can pass from precise description to

[1]In the development of the ideas presented in this essay, I acknowledge with gratitude the assistance of H Sharat Chandra.

explanation, then to prediction and to verification. Galileo took the first steps in discovering the fundamental principles of motion, in the process overhauling the older, more naive Aristotelian views of mechanics. Galileo's results were synthesized, added to and set out in an axiomatic form by Newton. He also discovered the universal law of gravitation, the first example of a unification in physics, and gave a common explanation for celestial and terrestrial gravitational phenomena.

These achievements of Galileo and Newton set the programme for natural science for the succeeding centuries—quantification by measurement, and mathematical analysis. Newton's own work was a combination of inductive and deductive methods, in that, after the laws of inertia and motion had been gradually discovered through ingenious experiments and arguments, he set them up as an axiomatic system from which consequences could be derived using mathematics and logic alone. He also gave clear expression to definite views on the nature of space and time: the absoluteness of each, their mutual independence, the validity of Euclidean geometry of space, and the uniform flow of time. In all this, he was greatly influenced by the style of geometry which Euclid had crystallized in his *Elements*. Indeed, Newton preferred geometrical arguments to algebraic analytical ones in deducing consequences from the axioms.

During the eighteenth century, with the efforts of many mathematical physicists, the domain of validity of Galilean–Newtonian physics was extended, and many successes were achieved. Apart from the continuing applications to astronomy, the theories of continuous media, fluid dynamics, elasticity, etc, were initiated by Euler, Lagrange, Cauchy and others. In celestial mechanics itself, it culminated in the monumental works of Laplace and Lagrange on the subject. By the end of the eighteenth century, some understanding had been achieved in the twin fields of electricity and magnetism, and everything seemed accessible to and falling into the Galilean–Newtonian pattern. Against a fixed space–time background, all processes were described in a causal and deterministic way; and the universe ran itself like a giant machine.

To a certain extent, all these advances were made possible by separating natural science from philosophical preconceptions and allowing the phenomena to speak for themselves. In the words of Max Born, speaking of Galileo and Newton,

> The distinctive quality of these great thinkers was their ability to free themselves from the metaphysical traditions

of their time and to express the results of observations and experiments in a new mathematical language regardless of any philosophical preconceptions.

But one cannot deny the fact that, nevertheless, these developments became possible thanks to a liberating philosophical atmosphere of the times, to which Bacon, Leibnitz, Descartes, Spinoza and others all contributed in one way or another. There were in fact two main schools of thought, the continental rationalist school (to which Descartes, Leibnitz and Spinoza belonged, notwithstanding differences in their views) and the English empiricist school of Locke, Berkeley and Hume. Stated simply and in a single sentence, the rationalist philosophers clung to the idea, going back to Plato, that reason was superior to and controlled experience, while the empiricists believed, as a kind of reaction, that everything had to be learned from experience alone. A kind of compromise or reconciliation of the two, and at the same time an explanation of the tremendous successes of Galilean–Newtonian physics, was attempted by Immanuel Kant towards the end of the eighteenth century. He distinguished between analytic and synthetic statements on the one hand, and between *a priori* and *a posteriori* truths on the other. An analytic statement is a statement based on pure logic alone, the contrary to which cannot even be imagined, and in that sense it is empty as it cannot but be true. For example, definitions are analytic statements. A synthetic statement is one that has non-trivial content, a positive assertion, the opposite of which can in principle be at least imagined. Thus, a synthetic statement is not a purely logical statement. Knowledge drawn from experience is of the synthetic kind but in principle there could be other sources of synthetic knowledge. *A priori* truths are truths not based on experience, truths possibly logically derived from some basic starting points, which in their turn are not based on experience. Non-*a priori* truths, then, are truths derived from experience.

Kant tried to combine rationalist and empiricist points of view in a scheme ultimately intended to justify the successes of Galilean–Newtonian physics. He viewed knowledge as partly drawn from experience and partly *a priori*; what he sought was absolutely certain and dependable knowledge, which necessarily had to apply to the world of experience. He was thus seeking *synthetic*, non-empty truths or knowledge that had *necessarily* to apply to actual experience but could not be *derived* from it. This was the strategy to his explaining the success of Galilean–Newtonian physics. He called such knowledge, *synthetic a priori*, already existing non-empty

Immanuel Kant
1724–1804

truths which are independent of experience. To achieve his goal, Kant categorized some of the fundamental principles of Galilean–Newtonian science (which were actually the distilled statements of experiments and experience) synthetic *a priori*. These included the nature of space and time, the validity of Euclidean geometry, causality and determinism.

Kant's idea was that synthetic *a priori* statements are a presupposition or precondition for science, not a result of scientific discovery. Thus, all the sensations and experiences of the external world incident upon us are seen through the glasses of Newtonian absolute space and time, Euclidean geometry, strict causality, etc. On all that comes to us from the outside, the mind imposes these categories; we have no other way of handling experience. Since science presupposes all these, we explain why these principles work by saying that nature could not be otherwise. More properly, one might say we are incapable of viewing nature in any other way. In later formulations, Kant included some of the detailed features of Newton's dynamics, such as the law of equality of action and reaction, and the law of conservation of mass, under synthetic *a priori*. Basically, the empirical successes of the Galilean–Newtonian physics were made inevitable features of experience. Even if nature were different, we would never know because our minds would always interpose these synthetic *a prioris* immediately upon all incoming sensory experience, and meaning and interpretation would only come later. In a way, this whole process reminds one of the earlier elevation of geometry from empirical knowledge based on experience to a product of pure reason.

One question that naturally and immediately comes to mind is this: How is it that these synthetic *a prioris*, which are independent of experience, nevertheless fit experience so well and efficiently? In effect, the question is: If these synthetic truths are not results of experience, where do they really come from? How is it that our minds already possess this knowledge which corresponds with experience? To this there was no convincing answer, but we will come back to it in a moment.

During the nineteenth century, the developments in physics reinforced the world view established earlier. There were many new discoveries; electricity and magnetism were seen to be closely related phenomena and, after their unification by Maxwell, the science of light was seen to be part of electromagnetism. Prior to this, the wave theory of light had come into its own, as against the corpuscular theory advocated by Newton. Also, in this process the concept of the field as an essential ingredient of physics, in addition to material substance as particles, was created and understood. The electromagnetic field too carried energy and momentum, and could exchange them with matter. The unification of electricity, magnetism and optics into one scheme was the second great unification in physics, after Newton's universal gravity. All in all, Galilean–Newtonian dynamics on the one hand, and the Faraday–Maxwell electromagnetic theory on the other, produced a world picture in which natural phenomena took place along strictly deterministic and causal lines, obeying definite mathematical laws; and our minds were presented with a faithful picture of an independent and externally existing real world. Towards the end of the nineteenth century, however, serious flaws in the foundations began to show up, which led to the major developments of relativity and quantum theory in this century. On the one hand, an incompatibility between mechanics and electromagnetism was found; this was resolved by special relativity, by modifying mechanics to fall into line with electromagnetism. On the other hand, a serious discrepancy between electromagnetism and classical statistical physics cropped up, which required the development of quantum theory and pretty much of an overhauling of everything at the conceptual level in classical physics! But at this point let us return to the question raised earlier about Kant's philosophical system: How does synthetic *a priori* correspond so well with *a posteriori* experience ?

The answer comes essentially from biology and the Theory of Evolution, which makes possible a reinterpretation and revalidation of Kant's ideas. It also tells us why these ideas may be limited, and in a sense prepares us mentally for the surprising and abstract developments in physics. The essential point is a proper appreciation of the relative roles of phylogenesis and ontogenesis—the development of the species over many generations and long periods of time, controlled or directed by natural evolution, and the development of each individual organism, each human being, in his or her own lifetime. This argument, going back to the ideas of Konrad Lorenz in the 1940s, has been beautifully expressed in a recent book, *Mind from Matter?*, by Max Delbruck. Several distinct ideas are involved; how in the course of the evolution of the species — phylogenesis — new abilities of organisms arise, and those conducive to survival are retained, just because individuals with those abilities leave more progeny; how infants in their period of growth learn to absorb experience and to deal with their surroundings; and how the mature adult mind manipulates and processes sensory inputs from

Max Delbruck
1906–1981

the external world. Our own world of daily experience is called the 'world of middle dimensions'. From the phylogenetic point of view, organisms capable of dealing successfully with the most important features of this world are, of course, favoured. Among these features are those of identity and permanence of material objects, the ideas of causes of events and an orderly pattern to experience, and geometrical properties of space. Thus, the capacity to detect such features in the world of middle dimensions is useful for survival, and this has developed slowly over long periods of biological evolution. Conversely, and to the same extent, these are objectively real features of the world at this scale. But this only means that each individual member of the species is born with — or comes equipped with — the capacity to see such aspects of the world around him. The basic lesson is: the result of biological evolution, what is *a posteriori* for the species, **appears to be** *a priori* **for the individual.** *A posteriori* for phylogenesis can lead to a priori for ontogenesis. But even here, this '*a priori*' apparatus does not exist readymade in the infant; during infancy, the innate capabilities, provided by phylogenesis must, by experience and exposure to the external world, be made into a workable and reliable system. I can do no better than quote Delbruck *in extenso* at this point:

> It appears therefore that two kinds of learning are involved in our dealing with the world. One is *phylogenetic* learning, in the sense that during evolution we have evolved very sophisticated machinery for perceiving and making inferences about a real world ... In other words, whereas in the light of modern understanding of evolutionary processes we can say the individual approaches perception *a priori*, this is by no means true when we consider the history of mankind as a whole. What is a priori for individuals is *a posteriori* for the species. The second kind of learning involved in dealing with the world is *ontogenetic* learning, namely the lifelong acquisition of cultural, linguistic and scientific knowledge. Thus we see the world through multiple pairs of glasses: some of them are inherited as part of our physiological apparatus, others acquired from direct experiences as we proceed through life. In a sense, the discoveries of science help us to see what the world is like without some of these pairs of glasses.

Delbruck describes in some detail, referring to the research of Jean Piaget, how the basic notions of the world around us are developed in every child during early infancy through his or

Jean Piaget
1896–1980

her interaction with that world. Thus, the identity of an object, its permanence, the association of causes to events as well as the motivation to always look for them, are all slowly learned in the early years from birth onwards. These 'facts' have been obtained through studies in developmental psychology, and it is fascinating to realize that this is how we all grew up! For instance, within two years, infants construct the concepts of object, space and causality. Between two and five years, the capacity to use symbols to represent objects and events, and to reason from memory and analogy, all develop. From five to ten years, our minds learn to classify, build hierarchies, the concepts of continuous quantities like weight and volume, and their conservation, arise. It is between ten and fourteen years that the ability for abstract thinking, logical arguments, assertions and consideration of hypotheses that may or may not be true are built up. Phylogenesis endows us with the innate capacity to develop these attributes and abilities because if we do develop them, we are more likely to survive in the world of middle dimensions. This then is the origin of the Kantian *a priori* categories of thought — thoroughly intertwined with biology in a way Kant himself could not have foreseen. At the same time, we realize that many seemingly 'obvious' features of the world around us are features we have slowly learned to recognize. It is interesting to mention here the following sentence from the preface to the book *Principles of Quantum Mechanics* (1930) by Paul Dirac,

> Like the fundamental concepts (e.g. proximity, identity) which everyone must learn on his arrival into the world, the newer concepts of physics can be mastered only by long familiarity with their properties and uses.

Against this background let us quickly see how the modern developments in physics have led us very far indeed from the world of middle dimensions, to concepts and phenomena that can be accurately described only in mathematical language, and for which ordinary language, pictures and intuition often fail. We are concerned with special relativity, general relativity and the quantum theory. The first was essentially completed in 1905; the second was fashioned in the decade 1905–1915; the quantum theory took the entire quarter century from 1900 to 1925 to develop and required many minds to complete it. In Newton's physics, space and time were both absolute and mutually independent. In particular, the concept of simultaneity was an absolute one. If one observer declared that two events taking place at two different points in space were simultaneous in time, all others would agree. However, special relativity showed that the simultaneity of spatially separated events could not be absolute. There is no such thing as a universal present or 'now' with the same meaning for everyone. While for each (inertial) observer, space and time retained Newtonian properties, with the former obeying Euclidean geometry and the latter flowing uniformly, two events appearing simultaneous to one observer could very well not seem to be so to another observer. What all observers do share is a common space–time, but each one carves out his own separate space and separate time in his own way, not always coinciding with another observer's separation. On the one hand space and time become unified into a greater whole, which alone is the same for everyone; on the other hand, there is a refinement of the terms 'past' and 'future', and which events could be causes for which other events. With respect to mechanics, substance is regarded as a form of energy and, subject to well-understood restrictions, while matter and radiation are interconvertible.

General relativity takes us one step further away from the intuitive commonsense world of middle dimensions. While special relativity expressed electromagnetism in its proper form, gravity had been left out of the picture. This was resolved by general relativity. The attempt to reconcile relativity with gravitation led to the former being superseded and giving place to general relativity. The inclusion of gravitation was shown by Einstein to involve changing the geometry of space and of space–time from Euclidean to non-Euclidean types! Thus, Euclidean geometry is no longer an *a priori* product of pure reason which necessarily must be obeyed by nature. The actual geometry of space has physical origins and causes, which are to be experimentally and empirically determined. Along with the earlier particles and fields, geometry too becomes an ingredient of classical physics, participating in and subject to physical laws.

These movements away from the intuitive ideas of simultaneity, causality and Euclidean geometry are things one can get accustomed to with reasonable training and dependence on the appropriate kind of mathematics. However, when we come to quantum mechanics, the changes are considerably more drastic and startling, since now all the intuitive ideas of substance, permanence, identity of objects, determinism and objectivity get affected. To begin with, the two classically distinct categories of particles and fields get fused or amalgamated; particles have wave attributes and vice versa. Matter loses some of its substantiality, solidity and permanence. Since at the microscopic level material points no longer have precisely defined paths in space along which they move, the meaning of similarity or identity of particles acquires a new and much more refined meaning. It also leads to ways in which identical particles can influence one another, which cannot be encompassed in the classical concepts of potential and force. Added to all this, quantum–mechanical laws are statistical or probabilistic in nature and, furthermore, they do not allow us to picture an atomic system as existing in some precisely defined state of its own, independent of our observations and experiments on it. Thus, both determinism and objectivity are affected and have changed from the classical ideals. Even with as complete knowledge at a given time of an atomic system, as is in principle permitted by quantum mechanics, we are only able to make probabilistic predictions about what might happen when we measure some quantity or the other at a later time. One cannot consistently imagine that a microscopic physical system exists by itself with definite numerical properties of its own, which our observations then reveal to us. According to the conventional interpretation, an experiment to measure a physical property always causes some disturbance to the system, and the result of the measurement is brought about by the measurement and was not pre-existing. Things do not have values in advance of measurement, and all things cannot simultaneously be measured or have values. To borrow Heisenberg's expression, potentiality (not as probability but as probability amplitude) rather than actuality is the fundamental quantity in quantum mechanics and is subject to a definite law of evolution in time. Such conceptions are what make quantum mechanics so counter-intuitive and hard to swallow; one is forbidden to paint a mental picture of a system as existing on its own. To quote Paul Dirac at this point, the fundamental laws of nature "control a substratum of which we cannot form a mental picture without introducing irrelevancies". Here, of course, we relate what we mean by intuition, commonsense, and the desire to picture an external world independent of ourselves, all to our biological heritage, our phylogeny! We need such a model or picture at least of the world of middle dimensions, so that we can evolve strategies to deal with it and survive in it.

We thus see that each one of the intuitive features of the world around us that we have painstakingly grasped has been superseded or sacrificed by later developments in physics when we study the very fast, the very large or the very small. The commonsense notions of substance, identity, permanence, objects, causality, determinism and geometry so assiduously learnt in infancy — the capacity to learn having been inherited — and so suited to the world of middle dimensions, have to be altered in dealing with other dimensions. One may be struck by the fact that in so many essential respects we have had to go beyond commonsense understanding, but maybe if we had not, that too would have been a still to be explained riddle. This exploration of nature so far from our own scale of things is well described by Schwinger:

> It is remarkable how Nature aids mankind's groping toward an understanding of the universe. As we raise the level of our scientific skills and sharpen our artificial senses, fascinating new phenomena continue to appear, testing and challenging our growing comprehension of Nature's grand design.

One of the key ingredients in the conventional interpretation of quantum mechanics is the principle of complementarity, put forward by Niels Bohr. There are two aspects, both relevant here. The first is that for microscopic systems, every experimental arrangement and observation leading to some result cannot be dissociated from that result. As I have said earlier, we cannot take the attitude that the result represents something that the system had already possessed, and which our measurement merely revealed. In quantum mechanics, according to complementarity, the experimental apparatus and the result obtained must be kept together as a whole and not be split apart. But then the experimental set-ups needed to measure two different physical properties may very well be mutually exclusive, and get in each other's way! In that case, we say that these properties form a complementary pair—knowledge of one leads to renouncing the possibility of simultaneous knowledge of the other. Position and momentum of an electron are an example of complementary variables. So also are the phase and the number of photons in an electromagnetic wave.

This fundamental principle governing atomic phenomena, led Niels Bohr to suggest in 1932 that it may have implications for the understanding of life as well. If we want to understand the functioning of a cell at the atomic level, in terms of physics and chemistry, the experimental technique needed would be such as to kill the cell. Therefore, the property of life, and the understanding of cell functions in terms of quantum mechanics, may be mutually exclusive or complementary. This led to his suggestion that the understanding of life would require something beyond quantum mechanics and which is yet to be discovered and not within quantum mechanics itself.

These views of Niels Bohr had the effect of influencing Max Delbruck to turn away from theoretical physics to molecular biology. Delbruck attempted to see whether Bohr's idea was necessary to understand life processes. In his book, *Mind and Matter*, Delbruck describes this attempt and comes to the conclusion — like others before him — that the principle of complementarity is not necessary in this context, and the situation is conceptually much simpler. To quote him,

> It might be said that Watson and Crick's discovery of the DNA double helix in 1953 did for biology what many physicists had hoped in vain could be done for atomic physics: it solved all the mysteries in terms of classical models and theories, without forcing us to abandon our intuitive notions about truth and reality.

Now, let us return to the problems of interpreting quantum mechanics. As mentioned earlier, an important statement is that an atomic system has no numerical properties of its own unless and until it is subject to experiment and observation. This has led to the idea that an external consciousness — of the experimenter and observer — is an essential part of the whole scheme of quantum mechanics. Many leading physicists have refused to accept such a situation; others have taken it as unavoidable. To illustrate the situation, I would like to quote from several serious physicists who represent the various shades of opinion on this state of affairs. At one extreme we have John Wheeler, a close associate of Bohr, who says:

> We used to think of a universe where we could in effect look at stars and galaxies as if it were from behind the safeness of a foot-thick slab of plate glass without getting involved. Today in our own time we have learned that even if we study so miniscule an object as a photon or an electron, in effect we have to smash this slab of glass. We have to reach in and install some kind of measuring equipment, and according as we set that equipment to measure one aspect of the situation or another, we get different results. We simply cannot put both pieces of equipment in at the same time; we have to make the choice. And what's more, what choice we make has an irretrievable influence on what will happen from then on. We

> have been promoted from observers to participators. There is a strange sense in which this is a participatory universe.

But his hesitation is also evident in the words,

> I confess that sometimes I do take 100 percent seriously the idea that the world is a figment of the imagination and, other times, that the world does exist out there independent of us.

In contrast, at the other extreme, is Einstein's well-known statement, "The belief in an external world independent of the perceiving subject is the basis of all natural science".

A kind of in-between attitude is reflected by Heisenberg:

> To what extent, then, have we finally come to an objective description of the world, especially of the atomic world? In classical physics science started from the belief ... that we could describe the world or at least parts of the world without any reference to ourselves. This is actually possible to a large extent ... One may perhaps say that quantum theory corresponds to this ideal as far as possible ... We have to remember that what we observe is not nature in itself but nature exposed to our method of questioning. Our scientific work in physics consists in asking questions about nature in the language that we possess and trying to get an answer from experiment by the means that are at our disposal ... It is understandable that in our scientific relation to nature our own activity becomes very important when we have to deal with parts of nature into which we can penetrate only using the most elaborate tools.

This problem of consciousness concerned Erwin Schrödinger, too, a great deal. The striking fact is that through study of inanimate atomic systems one should have come to a stage where one has to commit oneself on such problems as the existence of consciousness prior to the understanding of atomic phenomena. Speaking on the evolution of consciousness in *Mind and Matter*, he asks:

> Are we prepared to believe that this very special turn in the development of the higher animals, a turn that might after all have failed to appear, was a necessary condition for the world to flash up to itself in the light of consciousness? Would it otherwise have remained a play before empty benches, not existing for anybody, thus, quite properly speaking, not

existing? This would seem to me the bankruptcy of a world picture.

Later in the same essay, speaking of the emergence of the brain in certain animals alone, he says: 'Only a small fraction of them (if you count by species) have embarked on "getting themselves a brain". And before that happened, should it all have been a performance to empty stalls ?' Actually, Schrödinger was never happy with the conventional interpretation of quantum mechanics. Nonetheless, from such passages it should at least be understandable to a biologist why a serious study of quantum mechanics would tempt one to make definite statements about the nature of consciousness; the need for its existence as viewed from physical science, practically amounting to an assertion that there are reasons from outside biology why consciousness should exist.

Nowadays, with the many startling discoveries about the way the brain (human or animal) functions, there is a great deal of caution in dealing with the mind versus brain, consciousness, etc. The brain is an incredibly complex piece of machinery; and unlike what might have been previously imagined, the nervous system does not 'present' to it a 'faithful image' of an objective external world. Studies of the visual system, for instance, have shown that while the eye lens and retina function optically pretty much like a camera, thereafter an enormous amount of processing of the visual input is performed by the brain. The optical information is broken up into bits and pieces and sent to different cells in that part of the brain concerned with vision. Some cells are sensitive to patterns of contrasting illumination in one direction, some in another; other cells react only if something on the scene moves; and so on and so forth. These different aspects or features of the external visual scene are 'picked up' by different, spatially separated cells in the visual cortex. Aside from asking the obvious question — when, how and by whom is it all put together again — one is definitely struck by the complexity of the entire operation. It is not even the case that one is equipped at birth with all these capacities, but that in a few critical periods in early infancy, the wiring of the machinery and testing it are completed while interacting with the environment. The deprivation of this interaction at crucial times can lead to drastic deficiencies in the adult individual. Thus, the way the brain perceives the world is by a complex series of filters and processing operations — not through a naive, faithful image, but a highly treated one. The idea of 'naive realism' is here replaced by a 'structuralist realism' to reflect this fact. To a physicist, this is a fine example of capacity being turned into actuality—the interplay of phylogenesis and ontogenesis. Thus, all in all, the picture of external reality that is ultimately available to the brain is a highly filtered and processed one, involving many intricate steps along the way.

Those who have made these discoveries (and others too) are naturally very greatly impressed by the complexity of brain functioning. Their reaction to any attempt to discuss consciousness from the physicist's perspective is generally to say, "Wait, we do not yet know even how to define the term properly. Let us go on with our studies on how the brain works and unravel all its details; and in good time the understanding of consciousness and mind may come automatically". To quote Delbruck:

> The point of view of the evolutionist forces us to view mind in the context of other aspects of evolution, to draw parallels with other, more mundane forms of adaptation, such as the organs of locomotion and of digestion. In the context of evolution, the mind of the adult human, the object of so many centuries of philosophical studies, ceases to be a mysterious phenomenon, a thing into itself. Rather, mind is seen to be an adaptive response to selective pressures, just as is nearly everything else in the living world.

One question that Delbruck does not seem to convincingly answer, though, is why the brain is capable of so much more than would seem necessary for survival. Anyway physicists are a bit impatient, being faced by problems of their own, and do not want to be deterred by such warnings. In Schrödinger's words,

> The urge to find a way out of this impasse ought not to be damped by the fear of incurring the wise rationalist's mockery.

Is biology then going to be a part of physics, just as chemistry is? Certainly in Delbruck's view, as quoted earlier, the understanding of life is going to be easier and less subtle than the mysteries of quantum mechanics; it will not require giving up our intuitive notions of reality. But reality in quantum mechanics is different from the classical conception, more subtle and complex than a naively objective view. Let me conclude by presenting the views of a very highly respected physicist, Rudolph Peierls. In discussing the question of the prior need of consciousness in setting up quantum measurement theory, he says:

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> The question seems to pose an insurmountable difficulty. But it is based on the assumption that living beings ... can be described by the existing laws of physics, in other words, that biology is ultimately a branch of physics in the sense in which chemistry is now known to be in principle, a branch of physics ... Many people take it for granted that the same must be true of the science of life. The difficulty about how to formulate the acquisition of information ... is a strong reason for doubting this assumption. This is not far from the question of how one would incorporate the concept of consciousness into a description of living beings in terms of the physical functioning of their brain cells. Consciousness is admittedly hard to define objectively but each of us has a clear, intuitive understanding of what he means by being conscious ... In claiming that biology is not likely to be (a) branch of the present physics, I do not wish to imply that life can in some mysterious way evade the laws of physics ... It is at least possible, and to me probable, that ... new concepts have to be added to our present physical ones before an adequate description of life is possible. Whether the thus enlarged discipline should still be called physics is a semantic question.

May I leave you, dear reader, with these thoughts, fully conscious that at best my puzzlement has been transmitted to you. The physics–biology relationship could have been discussed from other points of view too — non-equilibrium thermodynamics, microscopic molecular structure, etc, — but this particular one was chosen. And I hope that this facet of the problem has been clearly brought home to you.

6 *Eugene Paul Wigner—A Tribute*

One of our last surviving links with the period of the creation and development of quantum mechanics was broken with the death of Eugene Wigner on 1st January, 1995 at Princeton in the USA. Wigner was remarkably talented and wide-ranging in his interests, and his work touched innumerable aspects of modern physics. In every area that he turned to, he discovered new and profound insights and interesting viewpoints, often understood and developed further by others much later. He was as much at home in fundamental problems of physics and their mathematical analysis, as in engineering and technological matters. In this tribute, I shall first describe briefly his life and career, then turn to a sketch of his work, and conclude with an attempt to capture his personality and philosophy of science and life.

6.1 A brief life sketch

Eugene Paul (Jeno Pal in Hungarian) Wigner was born on 17th November, 1902 in Budapest, Hungary, to Elisabeth Einhorn and Anthony Wigner. He thus belonged to the same generation as Werner Heisenberg, Enrico Fermi and Paul Dirac. Leo Szilard and John von Neumann were Wigner's classmates at the Lutheran High School in Budapest—'at the time, perhaps the best high school of Hungary and probably also one of the best of the world'.[2] Wigner always had a great regard for his mathematics teacher L Ratz, who also recognized and encouraged von Neumann's unusual talents.

After a year spent at the Technical Institute in Budapest, in 1921 Wigner joined the Technische Hochschule in Berlin to train as a chemical engineer. He completed his doctorate in 1925 and then worked for a year and a half as a leather chemist. By this time he had become very much a part of the Berlin physics scene; his break came with an appointment as assistant to Richard Becker between 1926 and 1927. This was followed in 1927–28 by a position

Eugene Paul Wigner
born-1902

as assistant to David Hilbert in Göttingen, and then as Privatdozent at Göttingen during the period 1928–30. At this point he moved to the United States, where he spent the rest of his life.

Wigner's career in the US began as a lecturer in mathematical physics in 1930 at Princeton University; he was quickly elevated to a Professorship in 1936. The year 1937–38 was spent as a professor at the University of Wisconsin at Madison. On his return to Princeton, he became the Thomas D Jones Professor of Mathematical Physics in 1938, a position he held until 1971. The academic year 1957–58 was spent at Leiden in the Netherlands.

In 1937, Wigner became a naturalized citizen of the United States. He took his citizenship very seriously, and played a very active role in public affairs and matters of government policy. As his contribution to the war effort, he spent the years 1942–45 at the Metallurgical Laboratory of the University of Chicago, the last two years as the head of the theory group there. Earlier he had joined Szilard and Fermi in persuading Einstein to write the famous August 1939 letter to President Franklin Roosevelt, that led to the setting up of the Manhattan Project. He was present at the University of Chicago's Stagg Field Squash Courts on 2nd December, 1942 to witness the world's first controlled nuclear fission reaction set up under Fermi's leadership. During 1946–47, he served as Director of what later became the Oak Ridge National Laboratory in Tennessee. In 1952, he was full-time adviser to the Du Pont Company to design the Savannah River heavy-water plutonium production reactors. Soon afterwards, in 1954, he was appointed to the General Advisory Committee of the United States Atomic Energy Commission, and also served on many panels of the Science Advisory Committee to the President of the United States.

Of the many awards that came to Wigner, we must mention the Medal for Merit, the Franklin Medal for 1950, the Enrico Fermi Award of the USAEC for 1958, the Atoms for Peace Award for 1960, the Max Planck Medal for 1961, and the 1963 Nobel Prize in physics (shared with Maria Mayer and Hans Jensen) for his wide range of contributions to quantum mechanics.

Wigner's first marriage, to Amelia Franck in 1936, was followed by a second one in 1941 to Mary Annette Wheeler, a professor of physics. His sister Margit Balasz nee Wigner married Paul Dirac in 1937.[3] There is a charming account by Margit of her first meeting with Dirac in Wigner's company. At a meeting in Budapest, the von Neumanns had invited Margit to visit and stay with them in Princeton. And then[4]:

'Eugene insisted, "If you come to Princeton, you must stay with me. What would people say if you did not stay with your brother?" I was not terribly thrilled with the idea. The von Neumanns had a lovely home, . . ., while my brother liked to appear, and act, like a pauper. We sailed in the fall; Eugene had a two-bedroom apartment, proudly boasting that he furnished it at the cost of under $25. It looked like it . . . It was soon after my arrival; we were having lunch at one of these restaurants, when a tall, slender young man entered the dining room, looked at Eugene and greeted him. He looked lost, and sad. I asked who he was, still standing undecided and none too happy-looking. I was told, he was an English physicist, whom Eugene knew in Göttingen, where they used to have their meals together. "He does not like to eat alone". "So why don't you ask him to join us?" That was how I met Paul Dirac. That was the fall of 1934. The Institute for Advanced Study had no building of its own as yet. Its members, like Einstein, von Neumann and Dirac as a visiting member, had adjoining rooms in a large university building, called Fine Hall. I remember so well: to the left was Einstein's room, in the middle Eugene's and to the right of him, Dirac's.'

Wigner, Szilard and von Neumann formed the famous Hungarian trio who contributed so decisively to intellectual life in the United States in the 1930s and later. There is a story that during a meeting of scientists connected with the war effort, there was so much confusion due to many languages being used that someone got up and exclaimed: 'Gentlemen, let us use one language we can all understand–Hungarian!'

When Wigner died he left behind his third wife Eileen, a son and two daughters.

6.2 Contributions to science and engineering

Wigner's work in physics is characterized by hard mathematical analysis based on simple yet profound physical assumptions. While there is a down-to-earth practical quality to some of his work, in others he dealt with the most fundamental issues with great refinement—he was both an artist and an engineer, and quantum mechanics was his medium. To quote John A. Wheeler:[5]

> In the work of Eugene Wigner one sees the basic harmony between the conceptual framework of physics and the structure of the mathematics associated with that physics.

On the other hand, his grasp of technology is best conveyed by this passage from Lawrence Dresner and Alvin M. Weinberg:[6]

> ... the facility with which he could pass back and forth between engineering and physics — from a discussion of the probable distribution of energy levels in U^{235} to a critical examination of the blueprints of the concrete foundations for the Hanford reactors, or from a group theoretical argument in transport theory to the design of aluminium fuel elements!

Wigner's first important work in physics was a powerful treatment of quantum many-fermion systems. Around the time of the move to Göttingen and, following a suggestion by von Neumann, he undertook the major task of introducing group-theoretical methods into quantum mechanics. By 1928, he had published six landmark papers on the subject; he shares with Hermann Weyl the credit for making this an essential and characteristic component of quantum physics which pervades all its applications. During the 1930s he worked in solid state physics and at the frontiers of the developing subject of nuclear physics, making a major effort to understand the forces between nucleons, and developing the compound nucleus model to explain resonance phenomena in neutron-induced nuclear reactions. His development later of the R-matrix theory of nuclear reactions was a response to a comment by Fermi that the compound nucleus model needed a firm theoretical foundation. Probably his most remarkable work in mathematical physics — the study of the unitary representations of the inhomogeneous Lorentz group — grew out of a suggestion made to him by Dirac in 1928. This was completed in Madison in 1937, and subsequently became the basic framework for all relativistic quantum theories. He came back to the problems of nuclear structure in his supermultiplet theory of 1937, and later in his statistical treatment of nuclear spectroscopy based on random matrices.

In the midst of all this, in the 1940s, he worked on the theory of neutron chain reactors and the design of plutonium breeder reactors.

Wigner's concern with the structure of quantum mechanics has led to a series of incisive insights over many years. In the early 1960s, he turned to problems of interpretation and epistemology raised by the standard interpretation of quantum mechanics. At this point it is convenient to present, briefly and selectively, sketches of Wigner's work under several broad areas. This is admittedly an inadequate, incomplete and possibly even a superficial way to survey his work, yet it may succeed in conveying some idea of the range and magnitude of his achievements. Before embarking on this, let us recall some important books published by Wigner: (1) *Group Theory and its Application to the Quantum Mechanics of Atomic Spectra* (Academic Press, New York, 1959; the original German version published by Friedrich Vieweg, Braunschweig, 1931); (2) *Nuclear Structure*, with Leonard Eisenbud (Princeton University Press, 1958); (3) *The Physical Theory of Neutron Chain Reactors*, with Alvin M. Weinberg (University of Chicago Press, 1958); and (4) *Symmetries and Reflections—Scientific Essays* (Indiana University Press, 1967).

The October 1962 issue of the *Reviews of Modern Physics*, published on the occasion of his 60th birthday, contains many articles surveying Wigner's work in several areas.

6.2.1 Structure and content of quantum mechanics

Any serious user of quantum mechanics is sure to find herself employing repeatedly, either explicitly or implicitly, one or another of the many basic concepts and methods invented by Wigner. One of the earliest is the concept of parity.[7] In classical physics, space inversion is merely a geometrical operation or transformation, a rule to map each point in space to its image by inversion through a chosen origin. The time is left unaffected. A particle trajectory, for example, would be mapped onto another possible trajectory.

Classical space inversion

$$P : \mathbf{x} \rightarrow -\mathbf{x}, \quad t \rightarrow t,$$

$$\mathbf{x}(t) \rightarrow -\mathbf{x}(t)$$

Wigner showed that in quantum mechanics, parity is more than a transformation, it is a physical observable whose value can be experimentally measured. The possible results of measurement are $\pm$ 1, and the corresponding quantum states are said to possess even or odd parity, respectively.

Quantum space inversion

$$P\psi(\mathbf{x}, t) = \psi(-\mathbf{x}, t),$$

$$\psi(-\mathbf{x}, t) = \pm\psi(\mathbf{x}, t) \Rightarrow P = \pm 1,$$

even/odd states.

It was this role of parity in quantum mechanics that was shown by Wigner to be the explanation for Laporte's selection rule in atomic spectroscopy:[8] the matrix elements of the electric dipole moment operator, and hence the corresponding transitions, vanish unless the two concerned states have opposite parities.

The deep connection between the invariance principles and conservation laws, both in classical physics and in the quantum domain with specifically new and subtle features, remained a lifelong concern for Wigner, something he came back to time and again. In the particular case of rotational symmetry, the general programme of incorporating group–theoretical methods into quantum mechanics led to Wigner's impressive body of results concerning angular momentum in quantum mechanics.[9] The detailed representation theory of the rotation group SO(3) and its covering group SU(2), which is basic to quantum mechanics, was developed by him in a form suited to practical application. The angular momentum addition theorem, the concept of tensor operators, the Wigner–Eckart theorem for their matrix elements, the explicit expressions of the Clebsch–Gordan coupling coefficients (also called the Wigner $3j$ symbols), leading to the intricate Racah–Wigner calculus for the coupling of tensor operators and computing the resulting matrix elements, the generalizations to other symmetry groups—all these oh-so familiar tools of the trade in atomic, nuclear and particle physics originate from his work.

In his book on group theory, Wigner formulated and proved a fundamental theorem concerning the representation of symmetry operations in quantum mechanics.[10] This is a very deep and subtle result, and a brief explanation would not be out of place here. The relation between physical states and wave functions (or Hilbert space state vectors) in quantum mechanics is one-to-many. This is because a change in the overall phase of a wave function is physically unobservable:

$$\text{vectors } \psi,\ e^{i\alpha}\psi,\ e^{i\beta}\psi,\ \ldots \rightarrow \text{same physical state,}$$

$$\text{vectors } \xleftrightarrow{\text{many-to-one}} \text{physical states.}$$

Therefore, what the physical states correspond to in a one-to-one manner are not vectors but rays: a ray is an equivalence class of vectors, two vectors being declared equivalent if they differ only by a phase. The ray to which a vector ψ belongs can be unambiguously described by the corresponding projection operator or density matrix ρ_ψ:

$$\text{vector } \psi \rightarrow \text{ray } \rho_\psi = \psi\psi^+,$$

$$\text{rays} \xleftrightarrow{\text{one-to-one}} \text{physical states.}$$

Rays do not form a vector space, so their geometry is somewhat harder to visualize than that of vectors ψ. Wigner's theorem then shows that any mapping τ of rays (i.e. physical states) onto themselves, preserving quantum mechanical probabilities — and any symmetry operation must be of such a nature — can be 'lifted' to either a linear unitary or an antilinear unitary (anti-unitary) transformation T on vectors.

Unitary–anti-unitary theorem

Symmetry operation τ,

$$\rho_\psi = \psi\psi^\dagger \rightarrow \tau(\rho_\psi) = \rho_{\psi'} = \psi'\psi'^\dagger$$

$$\rho_\phi = \phi\phi^\dagger \rightarrow \tau(\rho_\phi) = \rho_{\phi'} = \phi'\phi'^\dagger$$

ψ', ϕ', ... determined up to phases,

$$|(\phi', \psi')| = |(\phi, \psi)| \Rightarrow$$

either

$$\psi' = T\psi, \ \phi' = T\phi, \ \ldots,$$

T linear unitary,

$$(\phi', \psi') = (\phi, \psi) \rightarrow \text{unitary alternative;}$$

or $\psi' = T\psi, \ \phi' = T\phi,$.

T antilinear unitary,

$$(\phi', \psi') = (\phi, \psi)^* = (\psi, \phi) \rightarrow \text{anti-unitary alternative.}$$

Here, the inner product of the Hilbert space vectors ϕ, ψ is denoted by (ϕ, ψ). This remarkable theorem has been extended and proved under different conditions by others over decades.

Most symmetries in quantum mechanics turn out to be of the unitary type; time reversal is one example where the anti-unitary alternative is realized. The analysis of this transformation in quantum mechanics was given by Wigner[11] in 1932. In Schrödinger's quantum mechanics, time reversal acts on wave functions thus:

$$T\psi(x, t) = \psi(x, -t)^*$$

Unlike parity, however, this operation does not have the status of a physical observable in quantum mechanics, and its eigenvalues are not invariantly defined nor experimentally measurable.

Continuing with the theme of symmetry in quantum mechanics, Wigner and von Neumann arrived at a very interesting result in 1929, which is of great importance, especially in molecular physics:[12] if the electronic states in a molecule are classified according to their symmetry, i.e., the representation of the full group of symmetry of the relevant molecule, and if we have two distinct eigenvalues and eigenstates sharing the same symmetry (two electron terms), these eigenvalues will not cross (become accidentally equal) as one varies the internuclear distances in the molecule. On the other hand, electron terms of distinct symmetry can cross. This is a general theorem of quantum mechanics, applicable to a generic Hamiltonian possessing some symmetries and which is dependent on a continuous parameter: as the parameter is varied, distinct eigenvalues 'of the same symmetry' will not accidentally cross but will repel each other.

Many years later, Wick, Wightman and Wigner[13] brought to light another aspect of symmetry in quantum mechanics, namely the existence of superselection rules. This amounts to a restriction on the applicability of the superposition principle in quantum mechanics. In general, the Hilbert space of the states of a quantum system breaks up into sectors, and the formation of complex linear combinations to produce new states from old is limited to one sector at a time, not cutting across sectors. This is the reason why the phase of a spinor field — a field with half odd integer spin — is non-observable. So, for instance a non-trivial linear combination of states with an integer and half an odd integer angular momenta cannot be prepared. As another example, one finds that the linear superpositions of states of distinct electric charges are unphysical. It is suspected that these results had long been known to Wigner, and he was persuaded by his co-authors to join them and say so in print.

In the preface to his book on group theory, Wigner relates a conversation which he had with von Laue on the use of group theory as the natural tool with which to tackle problems in quantum mechanics.[14] He says: "I like to recall his question as to which results ... 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."

The exponential decay law for unstable states has been well known since the days of Rutherford's experiments on radioactivity. The first proper quantum-mechanical discussion and derivation of this law is due to Weisskopf and Wigner.[15] They were able to provide the basic theory for the natural line-widths and lifetimes of atomic states decaying via transitions to other states with emission of radiation. Their use of the second-order perturbation theory along with judicious and delicate assumptions also revealed that the exponential decay law is only an approximate, not an exact, consequence of quantum mechanics; so departures from it, for both very short and very long times, are to be expected.

The linear superposition principle of quantum mechanics, already referred to above, finds its most natural expression at the level of state vectors in the Hilbert space. The ray space or density matrix description of physical states, which is closer to a classical description, obscures this principle somewhat — it is present, but not manifest. In 1932, while studying thermodynamic equilibrium in quantum mechanics, Wigner introduced another description of the states for quantum systems possessing classical canonical analogues.[16] Thus, each quantum state is describable by a certain real distribution or function on the classical phase–space. In one dimension with classical phase–space variables x and p, the construction is as follows:

$$\psi(x) \to W(x,p) = \frac{1}{h}\int_{-\infty}^{\infty} dx'$$

$$\psi\left(x - \frac{1}{2}x'\right)\psi\left(x + \frac{1}{2}x'\right)^{*} \exp(ix'p/\hbar)$$

These distributions — named after Wigner — are at the level of density matrices, not state vectors. They are suggestively like the classical probability distributions on phase–space, such as one uses in classical statistical mechanics. However, since in general $W(x,p)$ can become negative for some arguments, we do not have a classical statistical picture with well-defined probabilities. This is as it should be, since quantum features must be preserved. This description of states in quantum mechanics turned out to be the counter-part, or companion, to a rule or convention given by Weyl for associating a quantum-mechanical operator with every classical dynamical variable; and these ideas were further developed, particularly by Moyal.[17]

Wigner contributed a great deal to the formal description of scattering and reaction processes in quantum mechanics, especially in the context of nuclear physics. One of his results concerns the physical meaning of phase shifts. In general, scattering cross-sections are determined by the squared magnitudes of the S-matrix elements, and, in these, the phases get washed out. On the other hand, the spatio–temporal development of a scattering process described within the limits set by quantum mechanics, involves these phases. The beautiful connection found by Wigner is the expression for time delay caused by an interaction and its relation to the energy dependence of the scattering phase shift:[18]

$$\Delta T(E) = 2\frac{\mathrm{d}}{\mathrm{dE}}\delta(E)$$

Here $\delta(E)$ is the phase shift at energy E; thus, an attractive (repulsive) interaction leads to $\delta(E)$ increasing (decreasing) with energy, and hence to a delay (advance) in the appearance of the final-state products of a collision, after undergoing an interaction.

We conclude this account with a couple of 'curios'. Classically, one expects the possible states of a system of interacting particles — especially a two-body system — to separate into two types: unbounded or scattering states, having positive energy; and bound states, having negative energy. In quantum mechanics we expect the energy eigen–values to behave analogously: a continuum of unbound, positive-energy scattering states sitting on top of a set of discrete negative-energy, bound states. Only the latter have normalizable wave functions. In a remarkable paper in 1929, Wigner and von Neumann produced an example of a two-body potential which possesses a bound state embedded in the continuum![19] This is an unexpected and essentially quantum-mechanical result. The potential is 'artificial' in that it has to be carefully engineered in order to produce the desired result, and the state involved is unstable, even under small perturbations.

The passage from classical to quantum mechanics results, at the level of dynamical variables, in the loss of *commutativity* in multiplication. Thus, for two physical quantities represented by operators A and B, in general $AB \neq BA$. However, this departure from the classical is limited in the sense that associativity is maintained: for three (or more) quantities multiplied in a given sequence, the product is unambiguous: $(AB)C = A(BC) = ABC$. One can ask how quantum mechanics might be modified if one takes the non–classical path one step further and, along with commutativity, one gives up associativity as well. This was examined by Jordan, von Neumann and Wigner[20] in 1934. It did not, however, lead to any alternatives with sufficiently interesting and flexible properties which could give a further extension of quantum mechanics.

Going over this rich list of contributions, one is tempted to say that Wigner took his revenge for not having been involved in the discovery of quantum mechanics, and compensated for it accordingly!

6.2.2 Nuclear forces, structure and reactions

Following the discovery of the neutron by Chadwick in 1932, there was a great deal of work done exploring the nature of the strong nuclear forces between neutrons and protons. It was realized that these would be strikingly different from the familiar Coulomb forces between protons, of a very short range, and with complicated distance dependences. Further dependences on spin and space exchange were also anticipated. Wigner was one of the earliest contributors to this field, and his name is associated with one of the four basic types of terms in the potential energy expression:[21]

Potential energy between proton and neutron
= Purely distance-dependent Wigner term
+ Spin exchange Bartlett term
+ Space exchange Majorana term
+ Spin and space exchange Heisenberg term.

Thus, the Wigner force is the simplest of all; the others either distinguish between singlet and triplet spin states, or between even and odd orbital angular momenta, or both. Such phenomenological potentials are useful in analyzing low-energy nuclear bound states, scattering processes, etc.

The low-energy (in the KeV to few MeV range) scattering cross-sections of neutrons of various nuclei, were experimentally studied by Fermi and his collaborators, and many other groups, around 1936. They found striking resonance structures in these cross-sections with sharp maxima separated by very small values in between. Soon afterwards, a theoretical explanation was offered independently by Niels Bohr on the one hand, and by Gregory Breit and Wigner[22] on the other. This is the so-called compound nucleus model. It pictures the scattering and reaction processes as taking place in two steps. At first, the incoming low-energy projectile (which could be some light nucleus rather than a neutron) and the target combine to produce a compound nucleus in one of several possible metastable states. In this process, the projectile energy is quickly shared with all the nucleons in the compound structure, and then the mode of formation of this structure is 'forgotten'. In the second step, the decay of the compound nuclear state into various energetically allowed channels is governed by probability laws. It is the probability of occurrence of the first step that shows an extremely sensitive energy dependence and gives rise to the observed resonances. In their work, Breit and Wigner derived the famous bell-shaped single-level resonance formula, subsequently named after them.

The probability of the formation of the compound nucleus is proportional to

$$\Gamma_\lambda \Big/ \left\{(E - E_\lambda)^2 + \frac{1}{4}\Gamma_\lambda^2\right\},$$

E = total initial energy, E_λ, Γ_λ = average energy and width of a compound nuclear state λ.

The partial cross-sections, for subsequent decays into each of the several available final channels, retain this characteristic energy dependence.

Sometime after this, around 1944, Fermi remarked to Wigner (as was mentioned earlier) that a good theoretical basis for the compound nucleus model was lacking. Thereupon, Wigner set about formulating one. This was the starting point of the R-matrix theory of nuclear reactions, developed by him in collaboration with Eisenbud.[23] The basic idea was to separate the total multidimensional configuration space of all the nucleons in the compound nucleus (i.e., the projectile nucleons plus the target nucleons) into two parts: an interior region where they are *all* within the range of nuclear forces acting between every pair, and an exterior region where this is not so. In the latter region, one then defines or picks out essentially non-overlapping subregions, one for each possible (two-body) final channel into which the compound nucleus can decay. Instead of posing a multichannel Hamiltonian eigenfunction and eigenvalue problem, a series of matching conditions connecting the interior and exterior channel wave functions and their radial derivatives, across the borders, between the interior and each exterior region, are set up. The R-matrix elements are quantities that enter these relations, they are a multichannel generalization of the logarithmic derivative of a wave function, in a one-dimensional radial problem. The parameters entering the R-matrix are the energy values and the various partial decay widths of all possible compound nucleus levels. Thus, the R-matrix became, simultaneously, a convenient method for parametrization of the scattering and reaction amplitudes using phenomenologically accessible compound nuclear state energies and widths, and with further developments, a way to embody general physical principles, such as unitarity and causality, which govern the reaction processes. *Inter alia*, this led to a multilevel generalization of the Breit–Wigner resonance expression given above, and to a criterion for the validity of the single-level formula.

Returning to the problem of nuclear forces and structure, in 1937, Wigner came up with the SU(4) supermultiplet theory to systematize the low-lying energy levels of light nuclei[24]. The idea was that the interactions among protons and neutrons, regarded as nucleons possessing the isospin degree of freedom (introduced by Heisenberg[25] as early as 1932), might, to a good approximation, be both spin- and isospin-independent. More generally, it might be invariant under all four-dimensional unitary transformations mixing up the four independent spin-isospin states of a nucleon. (This assumption actually leads to specific spin and isospin dependences in the interaction.) It would then be possible to arrange the energy levels of 'neighbouring' nuclei which have a common mass number into various unitary irreducible representations (UIRs) of SU(4), to consider systematically the breaking of this symmetry, etc. Each UIR of SU(4) is made up of several spin–isospin multiplets in a definite pattern. While the idea was physically well motivated as a useful first approximation, it was pursued only to a limited extent. However, many years later, in 1964, Wigner's theory provided the inspiration for a similar SU(6) invariant theory of baryons and mesons in the framework of the quark model[26].

At the other end of the scale, from low-lying, well-separated energy levels of light nuclei, we have the relatively highly excited and closely spaced levels of heavy nuclei with many degrees of freedom. Here Wigner proposed a completely different physical approach, one which has stimulated work by many others and led to connections with several other problems[27]. The physical ideas may be traced follows. As the excitation energy (of a complicated nucleus) increases, one expects the energy levels to get closer and closer, and one also loses hope of being able to obtain them individually from a first-principles Hamiltonian. Instead, what would be more accessible and physically interesting are various statistical properties of the levels; the probability distributions for successive levels to occur at various energies, for the spacing between neighbouring levels to have different values, and so on. To obtain these statistical features, and at the same time to reflect the fact that one is dealing with a very complex system with many degrees of freedom, Wigner proposed that the basic Hamiltonian (after truncation to a large but finite dimension) be itself regarded as a random matrix, belonging to an ensemble with specified properties. Once one specifies the nature of this ensemble, regarded as a primary input, the statistical properties of the eigenvalues of the Hamiltonian, the spacing distribution, etc, can all be derived, in principle, as secondary consequences. It turns out that in using this approach, one must deal with one 'simple sequence' of nuclear levels at a time; this is a set of levels possessing the same exactly conserved quantum numbers – 'belonging to the same symmetry' – such as the total angular momentum and parity. Properties of different simple sequences are independent. Thus, Wigner's hypothesis was that the local statistical behavior of the levels in a simple sequence is given by the properties of the eigenvalue spectrum of a random matrix drawn from a suitable ensemble. The type of ensemble to be used depends on the integer or half-odd integer nature of the total angular momentum, its behaviour under time reversal, and the presence or absence of rotational symmetry. Later work has shown that there are three natural types of ensembles, in correspondence with the three great families of classical compact simple Lie groups: the Gaussian real orthogonal, the Gaussian complex unitary, and the Gaussian symplectic ensembles. These ensembles consist, respectively, of real symmetric, complex hermitian and real quaternionic matrices (of suitable dimensions, even in the last case). The probability distribution defining the ensemble is invariant under the real orthogonal, the complex unitary or unitary symplectic group of transformations applied to its elements; moreover, the matrix elements of the Hamiltonian are assumed to be independent random variables. It is the combination of these two properties that makes these ensembles Gaussian.

A great deal of sophisticated mathematical analysis has gone into these objects, and this activity continues[28]. One very interesting feature that was recognized early on, was that the spacing distribution vanishes as a power of the spacing as the spacing tends to zero. The rate of this vanishing and the power involved is characteristic for each of the three families of ensembles. The physical meaning of this result – borne out by experiments and reminding us of the no-crossing theorem of Wigner and von Neumann for electron terms of the same symmetry – is that, within a simple sequence, neighbouring levels do not like to come very close to one another. Had we imagined that the energy levels themselves were independently statistically distributed, there would have been no cause for such level repulsion. This only emphasizes Wigner's idea that the properties of the ensemble of Hamiltonians must be chosen first, and other properties then obtained as consequences.

6.2.3 Quantum field theory, relativistic classical and quantum mechanics

The rules for canonical quantization – creating a quantum theory from a classical one – were originally invented in the context of non-relativistic particle quantum mechanics. The first successful application of these rules to a classical field theory came with Dirac's quantization of the electromagnetic field. This led to his classic 1927 paper, in which he treated the processes of the emission and absorption of radiation by matter, using quantum principles and the photon concept[29]. The quantized field led to a synthesis of complementary classical particle and field languages, and could describe states with variable numbers of identical particles. The canonical quantization method led to commutation relations of the form

$$a_r a_s^\dagger - a_s^\dagger a_r = \delta_{rs},$$

$$a_r a_s - a_s a_r = a_r^\dagger a_s^\dagger - a_s^\dagger a_r^\dagger = 0.$$

Here, $a_r(a_r^\dagger)$ are the annihilation (creation) operators for photons in various states indexed by r. These states are an independent, orthogonal and a complete set of modes of the electromagnetic field. The operators, a_r, $a_r^\dagger$ are quantum analogues of the classical complex coefficients in an expansion of the classical field in these modes. In this case, the appearance of commutation relations led naturally to the Bose–Einstein statistics for photons. Soon after Dirac's paper, Jordan and Wigner showed that to describe fermions (such as electrons) obeying Pauli's exclusion principle and Fermi–Dirac statistics, the particle annihilation and creation operators must obey anticommutation relations[30]:

$$a_r a_s^\dagger + a_s^\dagger a_r = \delta_{rs},$$

$$a_r a_s + a_s a_r = a_r^\dagger a_s^\dagger + a_s^\dagger a_r^\dagger = 0.$$

For a finite number of modes, they proved that up to equivalence there is only one irreducible representation of these relations, and it is finite-dimensional. This uniqueness is similar to a corresponding result in the case of commutation relations. The major difference is that from a mathematical point of view systems of operators obeying the anticommutation relations are quite 'harmless', while in the case of commutation relations they are unbounded and the space is infinite-dimensional—even for a finite number of modes. Of course, in the Jordan–Wigner case there is no sensible classical limit.

It is interesting to note that Dirac's initial reaction to this work of Jordan and Wigner was decidedly negative[31]. Wigner later attributed this to Dirac's being very committed to the Hamiltonian point of view in dynamics—'a captive of the Hamiltonian formalism'. However, it became clear very soon that this was the correct way to set up quantum field theory for fermions, and it became part of the foundations of the subject.

The first attempts at uniting quantum mechanics and special relativity were due to Klein and Gordon which resulted in the wave equation being named after them. However, it faced problems of interpretation at the one-particle level. The next spectacularly successful attempt was Dirac's work in 1928, that led to his wave equation for the electron and its series of amazing consequences.[32] Soon after, probably in 1928 itself, Dirac suggested to Wigner that he undertake a comprehensive study of all the possible unitary irreducible representations of the inhomogeneous Lorentz group (IHLG), i.e., of the homogeneous Lorentz group (HLG) supplemented by spacebf -time translations. By about 1932, Majorana had constructed many of these UIRs, and later these were simplified by Dirac and Proca[33]. The solution to this problem suggested by Dirac to Wigner became a herculean effort, which was completed only in 1937. The result was an all-time classic paper in mathematical physics[34]. In it, Wigner acknowledges the help and guidance he received, not only from Dirac, but also on mathematical aspects from von Neumann. At some stage, Dirac advised Wigner to be careful, and the latter replied[35]:

> You point out that care is needed in the analysis of the representations of the Lorentz group; I promise you that I will be careful.

Wigner's paper contains a detailed analysis of the structure of the HLG and the IHLG and of general unitary representations (URs) of the IHLG, in the context of quantum mechanics; it then turns to a study of the UIRs. The result was that these could be classified into four broad types, depending upon the nature of the possible values of the energy–momentum p^μ occurring within the UIR, and the allowed 'states of polarization' for each energy-momentum. The helicity, λ, is defined as a component of angular momentum in the direction of momentum. For each kind of p^μ (provided it is not identically vanishing), the allowed values of λ are determined by some UIR of a corresponding subgroup of the HLG, the so-called 'little-group' for that p^μ; it consists of all the elements of the HLG which leave p^μ invariant. The pattern of UIRs of the IHLG is displayed in Table 6.1.

Table 6.1 The Pattern of UIRs of the IHLG

Nature of p^μ	Little group within HLG(SL(2, C))	Number of polarization states, spectrum of λ	Remarks
(a) Time-like (positive or negative)	SO(3)(SU(2))	$2s+1$ for $s = 0, 1/2, 1, \ldots$ $\lambda = s, s-1, \ldots, -s$	Massive particles with zero or finite spin $s = 0$ for π meson $s = 1/2$ for electron
(b) Light-like (positive or negative)	E(2), two-dimensional Euclidean group	One: $\lambda = 0$ Two: $\lambda = \pm s$, $s = 1/2, 1, \ldots$ Infinite: $\lambda = 0, \pm 1, \pm 2, \ldots$ or $\lambda = \pm 1/2, \pm 3/2, \ldots$	No known particles $s = 1$ for photons $s = 1/2, \lambda = -1/2$ for neutrinos. No known particles
(c) Space-like	SO(2,1) (SL(2, R))	One: $\lambda = 0$ Infinite: $\lambda = s, s+1, \ldots$ or $-s, -s-1, \ldots$ s= 1/2, 1, ... or $\lambda = 0, \pm 1, \pm 2, \ldots$ or $\lambda = \pm 1/2, \pm 3/2, \ldots$	Imaginary mass. unphysical
(d) Vanishing	HLG(SL(2, C))		

(Here, space inversion or parity has been included in the HLG, except that for neutrinos this operation is undefined)[36].

While many of these UIRs were known earlier to Majorana and Dirac, the so-called infinite-spin or continuous-spin representations in cases (b) and (c) were genuinely new. In his work, Wigner did not carry out the investigation of these, or of case (d), to completion. He mentioned their existence, and only remarked:

> ... the last case may be the most interesting from the mathematical point of view. I hope to return to it in another paper. I did not succeed so far in giving a complete discussion of the 3rd class.

'Wigner's 'last case' and '3rd class' correspond, respectively, to (c) and (d) in our table. We also see that not every mathematically acceptable UIR of the IHLG is acceptable on physical grounds.

Relativistic quantum systems described by any UIR of the IHLG are called 'elementary systems'. Truly elementary particles, able to exist in isolation, are described using them. Some examples are photons, neutrinos, electrons and muons. The phrase 'elementary systems' conveys the meaning that all their properties are revealed by studying their behaviour under all elements of the IHLG—there is no internal structure involved. In the above listing, only cases (a) and (b) for finite helicity are realized in nature.

The UIRs of case (d) are actually UIRs of the HLG SO(3,1) (or of the closely related group SL(2, C)). It remained for Harish-Chandra and for Gel'fand and Naimark to determine them independently[37]. The inputs needed to construct the UIRs of case (c) for infinite spin, were provided by Bargmann through his construction of the UIRs of SO(2,1) and SL (2, R)[38].

In his contribution to RMP, Dirac made the following comments on Wigner's work[39]:

> The problem of working out all unitary representations of the IHLG has been dealt with by Wigner, taking the mathematical point of view that two representations are equivalent if they are connected by a unitary transformation. He decomposes the representations into their irreducible constituents and finds that the irreducible constituents provide theories of elementary particles with various spins. This work does not lead to any interaction between particles. To bring in interaction, one must depart from the point of view of looking at two representations as equivalent if they are connected by a unitary transformation, a point of view which involves looking upon all unitary transformations as trivial. To a physicist, some unitary transformations are trivial, whereas others (for example, the

> S matrix) are far from trivial, so a physicist cannot look upon two representations connected by a unitary transformation as necessarily equivalent.

The point is that for any really interesting relativistic quantum system, such as a relativistic quantum field theory, it is not only important to know which UIRs of the IHLG are present, but also how they are put together.

However it must be pointed out that as early as 1949, Wigner himself had drawn attention to this situation:[40]

> The elementary systems correspond mathematically to irreducible representations of the Lorentz group and as such can be enumerated .... However, in the description, by irreducible states, the form of almost all physically important operators remains unknown and, in fact, depends on the system, the types of interactions, etc. This leads to a rather strange dilemma: in the customary description, the form of the physically important operators is known but the time dependence of the states is unpredictable or difficult to calculate. In the description just mentioned, the situation is the opposite: the time dependence of the states follows from the invariance properties, but the form of the physically important operators is hard to establish.

Wigner returned on many occasions to a description of the results of his classic work. He also constructed with Bargmann a unified set of wave equations whose solutions would lead to UIRs of types (a) and (b) in our table[41]. His work with Newton on the problem of position is particularly interesting, so I shall describe it in a little detail.[42]

The starting point of non-relativistic particle quantum mechanics is the set of positions and momenta as primary dynamical variables, out of which all the other variables are built up. (Later work has shown that these positions and momenta can be derived as secondary objects starting from suitable quantum–mechanical representations of the Galilei group). Now, from Wigner's point of view, in the relativistic context, the primary things are the UIRs of the IHLG. After setting them up, one must examine within which UIRs one can construct position operators with physically desirable properties. Such an analysis was first undertaken by Newton and Wigner. They were able to show that in every finite mass and finite spin UIR (case (a)), a unique set of position operators possessing several physically reasonable properties does indeed exist. However, contrary to naive expectation, they do not form the space components of a relativistic four-vector. This illustrates the fact that in quantum theory the unitary transformation law is more basic than the geometric one or manifest covariance. In the massless case with finite non–zero helicity, even this much cannot be done. Thus, neither photons nor neutrinos can be localized in space.

Among other related work, we must mention the study by Inonu and Wigner of the process of 'group contraction' by which the IHLG goes over in the non-relativistic limit to the Galilei group;[43] Salecker and Wigner's analysis of deep conceptual problems in bringing together quantum mechanics and general relativity, caused by quantum limitations on position measurements;[44] and van Dam and Wigner's construction of classical relativistic direct-interaction theories resting upon integrodifferential equations for particle trajectories.[45] One of Wigner's conclusions was that while special relativity and quantum mechanics could at least conceptually be combined, in the case of general relativity and quantum mechanics there was no common ground at all.

6.2.4 *Interpretation of quantum mechanics*

In the early 1960s, Wigner turned to a serious examination of the problems of interpretation of quantum mechanics and a clear expression of the orthodox position which essentially coincided with his own.[46]. As evidence for the latter, here is his own statement:

> The orthodox view is very specific in its epistemological implications ... A large group of physicists finds it difficult to accept these conclusions and, even though this does not apply to the present writer, he admits that the far-reaching nature of the epistemological conclusions makes one uneasy.

He also often said that he was adding hardly anything new to London and Bauer's classic 1939 exposition.[47] He accepted the treatment of the measurement theory that had been articulated by his friend von Neumann[48] in as early as 1932, and wanted to restate it for a new generation and extract its ultimate consequences for epistemology.

Wigner emphasized that the state vector of a quantum system changes in two mutually exclusive ways—continuous, deterministic Schrödinger evolution when not subject to observation; and discontinuous, probabilistic, collapse when measurements are made. He went to great lengths to show that the linear Schrödinger equation

– even including the apparatus and the system's coupling to it – can never produce the macroscopically desired collapse phenomenon, and he stressed repeatedly that pure states and mixtures have very different physical properties. He also presented a pragmatic answer to the question 'What is the state vector?'. It was that it codifies in a compact way all the past information about a system, on the basis of which we can state the probabilistic connections that quantum mechanics gives among a series of measurements carried out subsequently and sequentially in time; all the consequences of quantum mechanics are just such statements. So, as the orthodox view claims, 'the laws of quantum mechanics can be expressed only in terms of probability connections', and cannot be formulated in terms of objective reality.

Pursuing this analysis further, Wigner came to the conclusion that human consciousness is an essential external ingredient needed to make complete sense of quantum mechanics. The collapse of the state vector occurs when, and only when, an observation is registered in some individual consciousness:

> It is at this point that the consciousness enters the theory unavoidably and unalterably. If one speaks in terms of the wave function, its changes are coupled with the entering of impressions into our consciousness.

And again:

> ... it was not possible to formulate the laws of quantum mechanics in a fully consistent way without reference to the consciousness.

In support of this declaration, Wigner appeals to Heisenberg and says:

> W Heisenberg expressed this most poignantly (*Daedalus*, 1958, 87, 99): 'The laws of nature which we formulate mathematically in quantum theory deal no longer with the particles themselves but with our knowledge of the elementary particles ... The conception of objective reality ... evaporated into the ... mathematics that represents no longer the behaviour of elementary particles ... but rather our knowledge of this behaviour'.

As one can imagine, this line of thinking led Wigner inexorably to a kind of solipsism, and to the delineation of two kinds of reality—the content of one's own consciousness, the only absolutely real, and everything else external to oneself, including every other person's consciousness. To support the former he turned to Schrödinger:

> . . . the most eloquent statement of the prime nature of the consciousness with which this writer is familiar and which is of recent date is on page 2 of Schrödinger's *Mind and Matter*: 'Would it (the world) otherwise (without consciousness) have remained a play before empty benches, not existing for anybody, thus quite properly not existing?'

But there was a sign of asymmetry—the only absolutely real, one's own consciousness, does depend on food, air and water for its own survival and functioning, as we are painfully aware; so he made a case for devising experiments which might show the effects of consciousness on matter. In talking of the first kind of reality, Wigner also realized and stated its obvious limitations—its awakening with birth and infant growth, its extinction at death. So he argued for a deep study of the former phase to understand the nature of consciousness.

Wigner felt that the development of quantum mechanics had widened the outlook of most physicists, and also in a sense made them inward-looking:

> Until not many years ago, the 'existence' of a mind or soul would have been passionately denied by most physical scientists . . . Even today, there are adherents to this view though fewer among the physicists than —ironically enough— among biochemists.'

He also saw that quantum mechanics reinforces the circumstance that any observation and interpretation of measurement rests on previously constructed and understood theories. Thus, we are linked in a chain to the very beginnings of our acquisition of knowledge about our surroundings and its regularities—indeed to phylogenesis and ontogenesis.

Today, it may seem that these conclusions of Wigner's were premature. Certainly, efforts are afoot to find more 'acceptable' interpretations of quantum mechanics, without an appeal to ourselves as essential prerequisites. Was Wigner then 'a victim of his generation'? Should we smile at these conclusions which he found inescapable? Or was he only being ruthlessly honest and expressing clearly what others hesitated to put into words?

6.2.5 Solid state physics, reactor theory and technology

I will touch upon these areas only briefly. Wigner's interest in the problems of solid state physics and materials science stemmed from a very early date. There must have been links to his original training as a chemical engineer; later on, his detailed knowledge of properties of materials played a key role in his work on reactors. Among his gifted students in solid state science in the 1930s were John Bardeen, Gregory Wannier and Frederick Seitz. It was Wigner who suggested to Wannier[49]

> that there ought to be a way to reconcile the local and the band concept for electrons, and that such a reconciliation would probably be useful in understanding the spectra of insulators.

Wigner also worked on radiation damage in solids—the detailed microscopic picture of lattice defects occurring when materials are irradiated with neutrons, the resulting changes in heat and electrical conductivity and ductility, and also the ways in which the material seems to recover from the damage with the passage of time.[50]

Wigner was the source of much of the theory and the major technological developments connected with nuclear reactors. His contributions began in 1940. As briefly mentioned earlier, he was a leader at the University of Chicago Metallurgical Laboratory during 1943–45. He contributed to the development of research reactors, power reactors and plutonium production reactors. On the theoretical front, he made major contributions to the spectrum of the Boltzmann equation, neutron thermalization, thermal utilization and resonance absorption. All very practical contributions "which one would hardly, a priori, have associated with the same man who introduced group theory into quantum mechanics"[51].

6.3 Views on science, philosophy and life

Wigner was a gifted and articulate expositor of science and its principles to general audiences. However, he frequently indulged in a kind of mock humility—as his Princeton colleagues explained his language,[52] ' A piece of work is "amusing" if it is correct and beautiful; it is "interesting" if it is wrong and messy.' And in describing the epistemology of quantum mechanics to an audience of nonphysicists, he said of himself, the writer:[53]

> He realizes the profundity of his ignorance of the thinking

> of some of the greatest philosophers and is under no illusion that the views to be presented will be very novel. His hope is that they will appear sensible.

He could convey sharp ideas pithily:

> Someone once said that philosophy is the misuse of a terminology which was invented just for this purpose.

This apart, his grasp of and concern for the grand principles of science were very deep. The role of invariance principles and their associated conservation laws captivated him —he dwelt upon them at length on many occasions,[54] and said:

> A large part of my scientific work has been devoted to the study of symmetry principles in physics ...

He titled his Nobel lecture 'Events, laws of nature, and invariance principles'. He often described as a miracle the fact that human understanding could uncover laws of nature, and separate them from the accidents of initial conditions. The laws provide structure and coherence to events, and, in turn, the symmetry principles provide these qualities to laws; thus, one has the ascending progression: events to laws to symmetry principles.

Turning to the role of mathematics in natural science, he expressed wonder at the way in which mathematical concepts and connections show up in unexpected ways and places, and also at the fact that tentative theories turn out, upon further development to be far more accurate than could reasonably have been expected at the outset. This led him to conclude that, since we do not quite know why we succeed so well and so often, we must be cautious and not immediately regard a successful explanation as the truth!

Musing on the likely future of science, Wigner wondered whether it might not wind down under its own weight, and lose its appeal to the young. The increasing extent of science makes it go beyond the reach of any one individual. But the response to this cannot just be an increase in team efforts, because this can never capture true creative thinking in the individual subconsciousness. There is a need to find deeper ways of sharing information and insight, of harmonizing the collective consciousness with the subconscious in each individual.

Continuing on the theme of the growth of science and the emergence of large collaborative efforts, he argued for protecting the individual and giving value and esteem to little science:

> One does not have the satisfaction which creative work, as we know it today, provides, if one's activities are too closely directed by others.

About the emergence of deep insights,

> It is hard to imagine how they can be developed other than in comparative solitude.

And as for the pleasures of pursuing science:

> It has been said that the only occupations which bring true joy and satisfaction are those of poets, artists, and scientists, and, of these, the scientists are apparently the happiest.

Throughout my description of his work, I have tried to convey the fact that Wigner very graciously acknowledged his debt to some of his most gifted contemporaries. He was also generous in his assessment of them. Of von Neumann he wrote:

> ... whenever I talked with the sharpest intellect whom I have known – with von Neumann – I always had the impression that only he was fully awake, that I was halfway in a dream.

And about Richard Feynman:

> He is a second Dirac, only this time more human.

Two people that Wigner had been very close to – Enrico Fermi and von Neumann – both died in their fifties. Wigner described and contrasted their attitudes to the inevitable. With Fermi,

> On a heroic scale was his acceptance of death ... He was so completely composed that it appeared superhuman.

But with von Neumann it was very different:

> It was heartbreaking to watch the frustration of his mind, when all hope was gone, in its struggle with the fate which appeared to him unavoidable but unacceptable.

These experiences must have affected Wigner deeply; at a convocation address to an audience of young students soon after, he said:

> Our culture is committing a sin by covering our eyes against the realization that none of us will be here always.

And to a general audience some time later:

> The recognition that physical objects and spiritual values have a very similar kind of reality has contributed in some measure to my mental peace.

Wigner was a physicist who achieved an uncommon range and depth in his life and work. He was a product of the old world who flowered during the golden age of theoretical physics, and carried his subject into the new world. It will not be an easy task to find a worthy successor to him.

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We provide here, for the benefit of the interested reader, a selected list of references to the original literature, as a guide to further reading. Both books and papers in scientific journals have been listed, and the reader is urged to refer to some of them at least, in order to see further developments of the ideas dealt with in the text. The references are grouped in the same sequence as the first five essays. However, it should be clear that just as there are overlapping themes in the various essays, each reference may cover material relevant to more than one essay. Some of the books listed, while they have become classics of the literature, may be hard to come by. We have included them as a challenge to the more motivated reader.

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