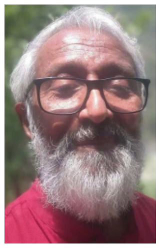
Friedrich Hermann Hund: Humanist, Physicist, Teacher, Activist, Science Historian*

K. D. Sen

"One may characterize physics as the doctrine of the repeatable, be it a succession in time or the co-existence in space. The validity of physical theorems is founded on this repeatability."

– Friedrich Hund



K. D. Sen, F.A.Sc., F.N.A, is INSA Senior Scientist at the School of Chemistry, University of Hyderabad. He is interested in confined electronic systems, atoms and molecules under the screened Coulomb potentials and the external magnetic fields, application of information theoretical concepts in chemistry, and density functional theory.

1. Introduction

The world of physics got twice lucky with two major scientific revolutions during the period 1900–1928, namely the special and the general theory of relativity and the quantum/wave mechanics. During the 2010s, the world celebrated hundred years of the great scientific contributions of Albert Einstein. The current decade of the 2020s calls for the much-deserved celebrations of the contributions of Werner Heisenberg, Max Born, Pascual Jordan, Paul Dirac, Erwin Schrödinger, and others. During 1925–1926, the fundamental laws of quantum mechanics were discovered, and it became established unusually rapidly as the fundamental branch of theoretical physics. Dirac, in early 1928, was able to develop a relativistic quantum theory of the electron. The existence of electron spin and of spin-orbit interaction arose naturally from the relativistically invariant Dirac equation. The atomic and molecular spectroscopic measurements provided the ever-ready test bed for exploration and interpretation. The glorious success stories of quantum mechanics in its ability to explain the electronic structure, bonding, and other properties of atoms and molecules continue to date.

Keywords
Hund's maximum spin multiplicity rule, atomic and molecular orbital theory, golden era of physics in Göttingen.

The present article is organized into two main parts. In Part A,

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we present a short historiographical account of the golden period of physics research in Europe during the first quarter of the 20th Century, with a special focus on the Institute of Theoretical Physics, Göttingen University, led by Max Born [1], where, during 1925–1927, the young theoretical physicist, Friedrich Hermann Hund (DoB: 04 February 1896, Karlsruhe — † 31 March 1997, Göttingen), produced several independent original research papers. This account gives the necessary background under which the multifaceted personality of Friedrich Hund took shape. In his 1975 book on the history of quantum mechanics [2], Friedrich Hund states that "the principles of quantum mechanics, by the end of 1926 . . . had been by and large identified, and that this produced a flood of applications starting in 1927". Three among those early applications due to Hund are of great significance to chemistry and physics, in general, and to the theory of the electronic structure of atoms and molecules, in particular. These contributions of Hund have been reviewed by several eminent scientists [3–8] besides being routinely covered in several standard textbooks of physical/quantum chemistry. We shall conclude Part A by (i) presenting a brief discussion on the current interpretation of the famous Hund's rule of maximum spin multiplicity, where a number of the existing quantum chemistry textbook descriptions need to be updated, and (ii) a very short description of Hund's main ideas associated with the remaining two applications. Finally, in Part B, we present the first-ever English translation of the most unique interview of the legendary Friedrich Hund conducted by Klaus Hentschel and Renate Tobies on the occasion of Hund's 100th Birthday [9]. This interview places the life and works of Friedrich Hund in a historical perspective and presents Hund as a great humanist, teacher, scientist, activist, and science historian.

During the first quarter of the 20th century, Europe had the world's four leading schools of research in modern physics.

PART A:

1. Theoretical Physics in Europe in Early 20th Century

During the first quarter of the 20th century, Europe had the world's four leading schools of research in modern physics. They were located in Göttingen, Copenhagen, Munich, and Leiden and developed under the mentorship of Max Born, Niels Bohr, Arnold Sommerfeld, and Hendrik Lorentz (from 1912 Paul Ehrenfest), respectively. An integrated experimental and theoretical research was pursued uniformly at all these centres. The continuous exchange of ideas in conjunction with the regular sharing/transfer of young scientists existed between all these centres of learning. For example, Werner Heisenberg and Wolfgang Pauli studied under Sommerfeld and subsequently became Born's assistant and later stayed at the Niels Bohr Institute. Indeed a synergetic nurturing of the nursery of outstanding physicists took place at these European centres of excellence. A very large number of U.S. physicists also visited here regularly. During this golden period, Göttingen became the centre which defined the margins of the entire active landscape of quantum science. The Institute of Theoretical Physics, Georg-August University of Göttingen, had a unique additional charm, i.e. the world-famous Mathematical Institute was located just in the adjacent building, collaborating closely together in teaching and research. During the first quarter of the 20th Century, Felix Klein, David Hilbert, Hermann Minkowski, Edmund Landau, Richard Courant, J. von Neumann, H. Weyl (who succeeded Hilbert after his retirement in 1930) and others working here contributed very significantly to the foundational development of theoretical physics. In 1926, von Neumann showed that if quantum states were understood as vectors in Hilbert space, they would correspond with both Schrödinger's wave function theory and Heisenberg's matrices. A special mention must be made of the sustained efforts made by Felix Klein and David Hilbert in reforming the German University system, which finally led to allowing women to pursue higher studies in mathematics [10].

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2. Bringing Born Back: Institute of Physics (Göttingen:1921)

Peter Debye stayed as the director of the Göttingen Institute of physics during 1914–1920 and then left for Zurich. Erich Hückel (DoB 09 August 1896, † Berlin – 16 February 1980, Marburg) obtained PhD under his guidance using the Debye–Sherrer method of diffraction to study liquid crystalline materials. Hückel continued in Göttingen until the fall of 1921 before joining Debye in Zurich in 1922. Hückel recalls while working in 1921 as the physics assistant under David Hilbert:

"My task with Hilbert was to converse with him in preparation for the lectures [on special and general relativity]. These conversations took place in the morning in his office room or in the backyard when the weather was nice. There he had a blackboard with a roof over it, and his small dog "Peter" was always there to be found. At first, Hilbert usually began by talking about the political situation, about which he always expressed original ideas and opinions." In 1929, Erich Hückel joined Hund, Heisenberg, Debye and Edward Teller at the University of Leipzig, where he formulated the famous Hückel Molecular Orbital (HMO) model for the planar conjugated molecules. Hückel acknowledges the help he received from Heisenberg and Hund in carrying out this work, which was originally suggested to him by Niels Bohr during his stay in Copenhagen in 1928 before moving to Leipzig.

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Soon after the departure of Debye, the search began to identify Debye's successor. Professor D. Hilbert, Institute of Mathematics, Göttingen, was instrumental in attracting Born from Frankfurt am Main to Göttingen. The first panel included Arnold Sommerfeld first, Born second, and Gustav Mie, third. Sommerfeld refused the offer, and finally, Born was offered the position. For Born, the position in Göttingen was "a real honor"—and Göttingen, one of the major mathematical centres of the world, was, after all, his alma mater. Now he was being invited back to be the head of the family, an offer he could not refuse. Born travelled to Berlin before taking up the offer in Göttingen and met with Herr G. Wendt, the German Ministerial Councillor for Art, Science, and Education. He explained to Wendt that he could not and would not teach experimental physics as the position offered to him actually required. Thus, Born succeeded in getting an additional chair professorship for the experimental spectroscopist James Franck. Born and Frank joined Göttingen in 1921. David Hilbert, extremely pleased with the final outcome, wrote to Richard Courant, the then head of the mathematics faculty, "Franck + Born are the best imaginable replacement for Debye! We have Born's energy to thank for it!" Interestingly, Hilbert was also instrumental in bringing Peter Debye to Göttingen in 1914.

In 1922, a set of four departments of physics in Göttingen were given new institute names : I Physikalisches Institut (Robert Pohl, Regarded by Nevill Mott as the father of Solid State Physics), II Physikalisches Institut (James Franck), Institut für Theoretische Physik (Max Born), and Institut für Angewandte Elektrizität (Max Reich). In the 1920's, Born's Institute rose to the pinnacle of its glory with the presence of W. Pauli, F. London, W. Heisenberg, E. Fermi, P. Jordan, F. Hund, L. Nordheim, W. Heitler, E.A. Hylleraas, J. Frenkel, R. Kronig,V. Fock, W. Elsasser, P. Dirac, E.U. Condon, J. Oppenheimer, E. Wigner, L. Rosenfeld, M. Göppert, M. Delbrűck, V. Weisskopf, E. Teller and others. Closely interacting with Born's group was D. Hilbert in the adjacent building hosting the likes of R. Courant and J. V. Neumann.

3. A Brief Academic Profile of Friedrich Hund

After the school education in Naumberg, Hund cleared the examination to enter the University (Abitur) in 1915. Hund was actually planning to become a school teacher. His father's business was not also doing particularly well. Destiny, however, had a different plan for him. His school teacher in physics, Paul Schönhals, was instrumental in encouraging Hund to study at Gottingen university and become a physicist. Hund received his University education in Marburg and Göttingen, with a brief interruption for military service in World War I. In Göttingen, he took a course from Peter Debye on quantum theory. Richard Courant, who taught Hund a course on partial differential equations, also recommended him to Max Born, who readily accepted Hund as his research student. Hund initially started working on the physics of crystals, a popular field of interest in Born's group. In 1922, Hund finally obtained a PhD degree working on the Ramsauer effect. By this time, Hund became one of the favourite collaborators of Born, who appointed Hund as a senior teaching assistant. Hund helped Born in the editing of the *Vorlesungen über Atommechanik*, a lecture course book, Born offered in the winter of

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1923/24.

The scientific community during this time became greatly excited with the announcement of the Nobel Prize in Physics to Albert Einstein (1921). During June 1922, as the part of the "Bohr's Festival", Niels Bohr delivered a series of lectures in Göttingen on quantum theory and atomic physics, which motivated Hund to shift his interest to spectroscopy. Hund intensely discussed spectroscopy with James Franck, Hertha Sponer, Jordan and Heisenberg and began working on the interpretation of the complex atomic spectra using the Russell–Saunders vector coupling model. The 1922 Nobel Prize in Physics went to Neils Bohr! During the following three years, Hund worked tirelessly and successfully defended his habilitation in theoretical physics in 1925. That year was special for Hund. He became a privatdozent under Born (Heisenberg and Jordan were the other privatdozents) and also published a total of 7 papers. Two of them led to the formulation of the famous empirical Hund's spin multiplicity rules explaining the complex atomic spectra. In 1926, Hund visited Niels Bohr in Copenhagen for six months on the recommendation of Max Born [3], written on 10 October 1925: "Now I still have [a] request for you. My assistant Dr Hund has a great desire to spend some time at your Institute, and I think nothing could be more beneficial for his training as a period spent under your influence. [...] So I thought I'd send Hund to Copenhagen for six months next summer, with a Rockefeller grant, if you are willing to receive him at the Institute. As to Hund's character, Heisenberg can inform you better than I can, in fact the two are close friends. Let me just tell you that Hund is the perfect assistant, always available, able, energetic, of ready intelligence and remarkable talent. On the scientific level he is not up to Heisenberg's-that would be unthinkable-but he is a person of great acumen and critical thinking, and a vast knowledge".

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During the last year of his stay as a privatdozent in Göttingen, Hund got interested in molecular spectroscopy. He began incorporating the electron spin into the molecular vector model to assign symmetry labels to electrons in a molecule and explain the observed band structure. Shortly thereafter, with the advent of new quantum mechanics, Hund proposed the well-known "symmetry-correlated united and separated atom limits" of the chemical bonding in the diatomic molecule with arbitrary bond length. Years later, this idea of multinuclear electron bonding blossomed into the Hund–Mulliken Molecular Orbital theory.

In 1929, Friedrich Hund met Dr Ingrid Seynsche (Born 21. 10. 1905, † 1994), already a teaching assistant in mathematics, at the inauguration of the new building of the Institute of Mathematics, Göttingen University. She was one of the first women recipients of a PhD degree in mathematics with distinction from the University of Göttingen. She worked officially under the supervision of Richard Courant (With Harold Bohr and A. Walther as stimulators). The title of her PhD work was "The theory of almost periodic sequences of numbers". They married on 17th March 1931 and raised a family of six children together.

During his lifetime of just over 101 years, Hund devoted the major part to teaching and research at several Universities and wrote over 250 research papers, several books and monographs. He travelled widely within Europe, U.K. and U.S.A. to present his research findings and interact closely with top physicists. Hund interacted with the likes of Debye, Bohr, Herzberg, Heisenberg, Dirac, Pauli, Schrödinger, Onsager, Bloch, Hűckel, Mulliken, and Wigner. Notably, Heisenberg, who joined Max Born's group in Göttingen to work on his habilitation thesis in 1924, became very good friends with Hund, and their friendship lasted lifelong. In Göttingen, they were frequently together, also enjoying outdoor activities like long walks and hikes on Sundays, over which they used to discuss physics. Thus, Hund became privy to Heisenberg's works prior to their publication. They both were active members of the youth movement in Germany. A significant number of Hund's publications during the later years present an authentic account of the history of the evolution of quantum mechanics, which made him a highly respected philosopher/historian of science in addition to a renowned theoretical physicist of his time. He regularly maintained a diary all throughout his academic

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career. Hund gave a copy of the diary to Helmut Rechenberg, the last doctoral student of his friend Heisenberg. The set of six magnificent volumes written by Jagdish Mehra and Helmut Rechenberg [3] is regarded as the standard chronological resource on the historical development of quantum mechanics. The wonderful Professor Dr Friedrich Hund, at 95 years, delivered his farewell lecture at the Institute of Physics, Göttingen, on the topic "What I Have Not Found"! A detailed academic profile [11] of Friedrich Hund is presented in *Box* 1.

4. Research Contributions of Hund

Friedrich Hund worked on several research topics [11], which are listed inside *Box* 2. His admirers used to teasingly say that Hund's idea of getting into a new research problem started with writing a manuscript. Three of Hund's research contributions of great importance in chemistry are:

(i) Hund's spin multiplicity rules.

(ii) First application of quantum tunneling.

(iii) Hund–Mulliken molecular orbital theory.

In what follows, we shall briefly discuss the recent work on the interpretation of Hund's rule of maximum spin multiplicity. The applications (ii)–(iii) listed above will be covered in passing, just highlighting the underlying basic idea.

(i) Hund's Spin Multiplicity Rules

Using the Russell–Saunders [12] scheme of the coupling of angular momentum, Wolfgang Pauli's exclusion principle [13] and Werner Heisenberg's work on the anomalous Zeeman effect [14] based on the correspondence principle of the old quantum theory, Hund published two pioneering papers [15] which accounted for the way the electron states of many-electron atoms are organized. He introduced, on empirical grounds, what became known as Hund's spin multiplicity rule, which allots the lowest energy to the states of highest total spin angular momentum.

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The term symbols resulting in the description of the complex spectrum of atoms were nicknamed by Max Born as the "term-zoology". In a letter to Einstein on 14 July 1925 in German, Born expressed appreciation of his three young teaching assistants as follows: "On the whole, my young people, Heisenberg, Jordan, and Hund, are brilliant. I find that to merely keep up with their thoughts demands at times a considerable effort on my part. Their mastery over the so-called "term-zoology" is marvelous. Heisenberg's latest paper, soon to be published, appears rather mystifying but is certainly true and profound. It enabled Hund to bring into order the whole of the periodic system with all its complicated multiplets. This paper, too, is soon to be published".

For the ground electronic states, the first of the three Hund's Spin Multiplicity Rules is generally known as the Hund's Maximum Spin Multiplicity Rule, which is stated as follows: "For a given electron configuration, the term with maximum multiplicity has the lowest energy. The multiplicity is equal to 2S+1, where S is the total spin angular momentum for all electrons. The multiplicity is also equal to the number of unpaired electrons plus one. Therefore, the term with the lowest energy is also the term with maximum S and the maximum number of unpaired electrons."

The remarkable success of Hund's spin multiplicity rule provided a strong empirical foundation for quantum mechanics. We shall limit our discussion to the atomic ground states and briefly discuss the relatively more recent interesting new development on the interpretation of the first of the set of three Hund's Rules stated above. While the applicability of the first rule is experimentally validated, its quantitative interpretation is an area of current research interest [16].

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Consider an electronic configuration of two electrons with antiparallel spins in two different orbitals ($1s^1 2s^1$) and change the spin state of one electron from the antiparallel state to the parallel state, i.e. a switch the two electron atom from the singlet state (2**S**+1=1) to the triplet state (2**S**+1=3). The presence of extra exchange energy (negative sign) contribution between the like- spins pair of electrons decreases the total electron-electron repulsion energy (positive sign) in the triplet spin state relative to the singlet spin state where the electron pair has unlike-spins. This is the original interpretation proposed by Slater [17]. Slater's interpretation is based on the incorrect assumption of choosing the same orbitals for the atomic states with different spin multiplicity. This would give rise to the same kinetic and electron-nuclear attraction energy for both the spin states and thus violate the virial theorem. More recently, numerical results were accumulated at various levels of wave functional calculations, which establish that the higher spin state with the lowest total energy often has the higher electron-electron repulsion [18, 19]. This is counter-intuitive. A reasonable explanation due to Boyd [20] can be stated as follows. The like-spin electrons in the higher spin state avoid each other due to the antisymmetric nature of the spatial wave function, and they feel an unscreened nuclear charge. As a result, a relatively more effective nuclear charge is experienced, and the total electron density gets more compact in the highest spin state. Due to this, the energy contributions from average electron-electron repulsion, as well as the nuclear electron attraction, increase. The total electronic energy in the state with higher spin multiplicity is a consequence of its dominating electron-nuclear attraction energy, which more than compensates for the increase in electron-electron repulsion energy. A more detailed interpretation is presented by Moiseyev and coworkers [21].

We discuss below very briefly the main ideas of Hund in the next applications (ii)–(iii).

(ii) First Application of Quantum Tunneling

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Hund was the first to make use of quantum mechanical barrier penetration in discussing the theory of molecular spectra in two papers in 1927. The first paper [22] was submitted from Copenhagen in November 1926, acknowledging encouragement from Niels Bohr and Werner Heisenberg and support from the Rockefeller grant, which supported his visit from Göttingen. In the second paper [23], Hund assumed the separability of the electronic motion from the vibration and rotation of the atoms, an approximation later made quantitative by Born and Robert Oppenheimer. Hund showed that the superposition of the even ground state and the odd first excited state of a symmetric double well with a finite barrier yields a nonstationary state that shuttles back and forth, or tunnels, from one classical equilibrium position to the other. In an insightful presentation of Hund's work on quantum tunnelling, Merzbacher [24] notes "Hund worked out the tunnel effect for a system with only bound states. He was the first to recognize its relevance for chemical binding and molecular dynamics."

(iii) Hund–Mulliken Molecular Orbital Theory

We begin with the remarks made by Hund [1, pp.183] "One of the greatest successes of quantum theory was the inclusion of theoretical chemistry in the thinking of physicists. The theoretical-physical treatment of the chemical bond naturally pre-supposes the theory of the molecules, and was therefore not possible without the new quantum mechanics (Heisenberg's) and without the Schrödinger equation (wave mechanics). The one-electron approximation, initially found useful for describing molecular states, was not so suitable for chemical bonding. For these, it was the relation of the state of the molecule to the state of separated atoms that mattered,...."

Hund published two landmark papers [25–26] describing the electron configuration of diatomic molecules using spectroscopic data. These ideas and their later developments have been widely discussed earlier [2–8].

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Hund first built up the states of diatomic molecules in which electrons are assumed to circle around the multinuclear framework. The molecule is built up from the corresponding atomic states by considering an adiabatic invariance starting from the infinitely separated atoms to the bonded two-centre diatomic molecule at an arbitrary bond length, R, which is then gradually decreased to give a fictitious united atom state. [*Adiabatic invariance follows a theorem in classical mechanics that if the energy function depends on a parameter x, if x is changed slowly, the system proceeds through those states having the same values of the action variables as when x has a series of different constant values. Born showed [27] that this theorem holds good in quantum mechanics. Hund drew diagrams connecting the symmetry adapted electron levels as a function of R for a homonuclear diatomic molecule, starting from the separated atoms $R = \infty$ and judged that levels going to the unoccupied levels of the united atom would be unfavourable to bonding and those remaining occupied for all R would be favourable. Using the experimental information from spectra, he could assign the electron configurations of some diatomic molecules. It emerged from his work that the conservation of the number of nodes in each orbital as R changed was very significant. Later on, with independent contributions from Mulliken and Herzberg, Hund's work took the form of the bonding, antibonding orbitals of the MO theory. We conclude this part by noting that the trio born in 1896, Hund (4 February), Mulliken (7 June) and Hückel (9 August), made fundamental contributions to the development of the Molecular Orbital Theory, which has become an integral part of computational quantum chemistry today. The impact of their contributions can be gauged from the recent statistics [28], according to which nearly 40% of the global supercomputing time is spent on quantum chemical and classical molecular dynamics simulations. In the coming decades, with the application of the rapidly advancing disciplines of artificial intelligence and machine learning, along with the real possibilities of quantum computers on the horizon, it may be possible to run computer simulations of cellular processes in real time!

Box 1. Hund's Academic Profile

1896 Born on February 4th in Karlsruhe (Baden)
Attended school in Karlsruhe, Erfurt, Naumburg an der Saale
1915 Abitur in Naumburg an der Saale
Two years with the Goods Train Station Gűterbahnh of Erfurt and the Naval Aviation Weather Service
Studied mathematics, physics and geography in Marburg and Göttingen
Received training under Richard Courant, David Hilbert, Carl Runge, Max Born, James Franck in Göttingen.
1921 1st state examination for teaching
1922 Pedagogical examination (2nd state examination)
1922 PhD in Göttingen (Max Born). In Fall, joins as official assistant of Born.
1922-27 assistant to Max Born
1925 Habilitation in Göttingen
1925-27 Lecturer (Privatdozent) in theoretical physics in Göttingen
1926 Discovery of the quantum tunnel effect
1926 Studies with Niels Bohr in Copenhagen
1927 Professor for theoretical physics in Rostock
1929 Guest lecturer at Harvard University in the USA
1929 Professor for mathematical physics in Leipzig
1931 Marriage to Dr. Ingeborg Seynsche (B.1905-†1994)
6 children: Gerhard, Dietrich, Irmgard, Martin, Andreas, Erwin
1933 Member of the Saxon Academy of Sciences
1943 Member of the German Academy of Natural Scientists Leopoldina
1943 Gold Max Planck Medal from the German Physical Society
1945 Pro-Rector of the University of Leipzig *)
1945 Adjusts to Change from American to Russian occupation in Leipzig **)
1946 Professor for theoretical physics in Jena
1948 Rector of the University of Jena ***)
1949 Member of the German Academy of Sciences (Leopoldina) in Berlin
1949 National Prize
1951 Professor for theoretical physics in Frankfurt am Main
1956 Guest professor at the University of Maryland in the USA
1957 Professor for theoretical physics in Goettingen
1958 Member of the Göttingen Academy of Sciences
1964 Superannuation
1965 The Great Cross of Merit of the Federal Republic of Germany
1968 Visiting professor at the University of Cologne
1969 Visiting professor at the University of Heidelberg
1971 Cothenius Medal of the Leopoldina
1970 Guest professor at the University of Frankfurt am Main
1974 Otto Hahn Prize for Chemistry and Physics
1993 panel discussion in Göttingen, one of the last public appearances
1997 Death on March 31 in Göttingen
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`https://www.teleschach.de/archiv/leben_f_hund.htm`

Box 2. Hund's Scientific Work from the Twenties

Applications of quantum theory
Structure of molecules and crystal lattices from ions
Structure of complex atomic spectra and magnetism of the rare earths
Symmetries in atomic and molecular structure
Quantum mechanical process (1926), the later so-called tunnel effect
Understanding of chemical bonding
Electronic structure of solid bodies
Construction of atomic nuclei
Line spectra and periodic table of the elements.
Hund's spin multiplicity rules
Description of the molecules
Structure of the molecular spectra
Matter under very high pressures and temperatures
Star structure issues
Elementary particle processes occurring at the highest energies
The theory of the structure of matter
History of quantum theory
History of physical concepts

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Box 3.

Figure A. Parents (Mother) Anna Caroline Hund and (Father) Karl Friedrich Hund: Infront of their shop in Naumberg Market Place.
(Reproduced with permission from Gerhard Hund)

Box 4.

Figure B. Dr.Ingeborg Seynsche (left) Dr. Friedrich Hund (right).
(Reproduced with permission from Gerhard Hund)

Box 5.

Figure C. Part of the group photograph at the Bohr Memorial Meeting, Copenhagen July 1963. (Reproduced with permission from Gerhard Hund.)
Seated 1st Row L-R: Frisch, Mrs Pauli, Ms. L. Meitner
Seated 2nd Row L-R: Courant, Mrs. Hund, Rosenfelt
Standing Row L-R: Blackett, Hund

PART B: English Translation of the Interview [9]

Friedrich Hund on his 100th birthday. Interviewed by Klaus Hentschel and Renate Tobies[1]

KH: Why did you come to Göttingen as a young student?

1 The interview took place on December 15, 1994 in the private apartment of Prof. Dr. Friedrich Hund (born February 4, 1896) on Charlottenburger Strasse, Göttingen. So far there have been at least three interviews with Friedrich Hund by science historians. T. S. Kuhn interviewed him in June 1963 for the *Archives of Sources for History of Quantum Mechanics*. Helmut Rechenberg held a 42-minute conversation with F. Hund on the topic: The rise of quantum mechanics. The recording bore the shelf number G 239 from the Institute for Scientific Film (IWF), Göttingen [which has meanwhile been dissolved]. You now find the film online at `https://av.tib.eu/media/11594?hl=keyword:%22physics%22`. Home pages of Dr Renate Tobies and Dr Klaus Hentschel can be found at: `https://renate.tobies.org` and `https://www.hi.uni-stuttgart.de/institut/team/Hentschel/`

KH: Why did you come to Göttingen as a young student?

FH: Oh, that was a lucky coincidence. My physics teacher at school, to whom I personally owe a great deal and without whom I probably would not have become a physicist, offered a holiday course. The University of Göttingen organized holiday courses for secondary school teachers, in this case, physicists and also mathematicians—it could have been in 1913 or 1914. My physics teacher took part and came back delighted. The Göttingers understood how to give even the less scientifically trained upper schoolteachers - they were called Oberlehrer at the time - an idea of how a university really operates. Well, he came back most enthusiastic about it and recommended to me: You must study in Göttingen.

RT: What was the name of the physics teacher?

FH: Paul Schönhals. That's not a very good portrait of him hanging on the wall by the painter Georg Kotschau. So I followed the recommendation. I graduated from high school in 1915. Then, of course, I went to Göttingen. It was war, and many things were cancelled. I was arrogant and skipped certain areas, but I don't [really] regret it. But I only attended beginner's lectures by and large. Then I was a soldier until the end of the war (essentially in a scientific position, namely in the navy's weather service.) I only studied briefly in Marburg and then returned to Göttingen. In 1921 I took the state examination for teachers at secondary schools. The circumstances in science were not particularly good. The representative of theoretical physics had just left; a new one hadn't arrived yet. Physics wasn't really in the foreground during my studies, but mathematics (was). One needed three subjects; I chose mathematics, physics and geography, then apparently passed the exam well, completed my probationary year at a high school in Göttingen. Lucky circumstances made it possible to return to a position in Göttingen. And the fact that I went to university and became a professor was again due to chance which I was partly able to influence.

K.H.: Apart from your teacher Schönhals, were there any other formative influences that made you decide on mathematics and physics?

FH: No, but only the fact that I could grasp mathematics, and my classmates couldn't. I had to teach them some of the math that the teacher was supposed to have been teaching them.

R.T.: Wasn't the math teacher good?

F.H.: No, I said at the time that he was an old idiot. Today the term "old" would be rendered differently; by today's standards, he was still a relatively young man. No, I also treated him badly by telling him whenever he made a mistake.

K.H.: There was no one in your family who aroused your interest in physics?

F.H: No, no.

R.T.: What was your father's job?

F.H.: My father had a small, struggling hardware, home and kitchen appliance store. The decision had not been reached for a shop like that to support a son in studying. This happened differently. Of course, I wanted to study; that was my wish as a schoolboy. I gave quite a few private lessons to make some money. Quite much was put aside, which of course, was swallowed up by the (run-away) inflation [of 1921/22], but it was just barely sufficient.

I passed my second exam at school in 1922, and I went at the same time to Born's[2] lectures for proper study, where I was soon working as an assistant. In fact, I really studied physics, and especially theoretical physics, only after my state examination, partly during my traineeship. I experienced the great inflation (1923) already as a wage earner. I always personally felt the economic difficulties that existed. My parents' homes had no academic tra-

2 Max Born (1882–1970) studied in Gottingen from 1903–1906, especially under David Hilbert and Hermann Minkowski and received his doctorate there in 1906. From 1921 to 1933 he was professor for theoretical physics in Göttingen.

dition, and I realized that the small business could not generate enough. We were three children; I had two sisters.

R.T.: Did your sisters also study?

F.H.: No, no, of course not, that wasn't very common back then. That was beyond the family's horizon.

K.H.: Why did you turn to Max Born in about 1922, was that from a personal affinity or was it his style?

F.H.: No, he was the real thing, he was the professor of theoretical physics.

R.T.: There was a seminar held by Born and James Franck[3] that you attended. Do you remember?

F.H.: Well, the trend toward theoretical physics was the essential thing for me at the time.

Physics was one of the three subjects on my state exam, but that was simple, classical physics. There wasn't a word about quantum theory[4]. But in 1922, I had to decide whether I wanted to stop at school teaching—I had tempting offers (from private schools), and unemployment among teachers was high. The decision reached was that I didn't want to leave Göttingen, and then, of course, I had to go officially to the Göttingen professors. I knew Born; I didn't know Franck at all. I had worked with

3 The experimental physicist James Franck (1882–1964) was called to Göttingen in 1921 at the same time as Born. Until they were expelled by the National Socialists, Göttingen developed into the "Mecca of atomic physics". See e.g. Hund's paper on "Highlights of Göttinger Physics II" in *Physikalische Blätter*, 25, pp.210–215, 1969, as well as Jost Lemmerich's publication: Max Born–James Franck: The Luxury of Conscience, Frankfurt/M, 1982.

4 The three subjects, physics, mathematics and geography, chosen for the state examination (1921) were also the examination subjects for the doctoral examination. In physics, Born examined on 15 November 1922 about: "Experimental justification of Maxwell's equations, para- and diamagnetism, dispersion theory, Van der Waal's equation of state, definition of entropy, Nernst's theorem of heat." The verdict was: "Excellent". [University Archive (henceforth: UA) Göttingen, Phil. Fak., Prom. H, 1922/23, No.5].

Courant[5], the mathematician, and he was kind enough to write a recommendation for me, so Born accepted me as a doctoral student. I never did write the PhD thesis that Born suggested for me. I later looked for a completely different topic myself. On my first visit in 1921, Born was just turning away from the physics of crystal lattices, which was his first area of research[6], to the general questions of quantum theory. He gave me a topic on certain applications of the then rudimentarily known quantum theory of atoms.

K.H.: That was scattering theory, right?

F.H.: No, it was actually Doppler effects when radiation hits crystals—that was too complicated at the time—those were matters that, of course, couldn't be solved in any other way at the time; I couldn't have done it. Thirty years later, a similar topic was solved by one of my students, Heinz Bieltz.

K.H.: How did you get along with Born? Were you satisfied with him as a supervisor?

F.H.: Born was a bit reserved as a person. It was not easy to come into close contact with him; but of course, his assistants succeeded; but both Born and Franck had a great influence on me. Franck actually led me to the topic that I actually treated in my doctoral thesis later on. Of course, I formally wrote the doctoral thesis under Born; and Franck was the expert on the faculty. In this respect, I always have to name Born and Franck as having

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5 Richard Courant (1888–1972) had received his doctorate with David Hilbert in Göttingen in 1910 and was appointed to the Felix Klein Chair in 1920, which he held until he emigrated in 1933. His scientific achievements were primarily in the field of analysis and its applications in mathematical physics. See especially Constance Reid, *Richard Courant 1888–1972*, Springer: Berlin, Heidelberg, New York, 1979.

6 From 1912 Born published on crystal physics, among other things, in 1915 his book on the dynamics of crystal lattices and in 1923, an article on "Atomic theory of the solid state (Dynamics of crystal lattices)" in the *Encyclopädie der Mathematischen Wissenschaften mit Einschluß ihrer Anwendungen*, Vol.V. Physics, part 3. Teubner: Leipzig 1909–1926, pp.527–781.

exerted an influence on me[7].

R.T.: Can you tell us something about Hertha Sponer[8], James Franck's assistant?

F.H.: Hertha was a cheerful co-worker with a certain self-confidence. She knew that it was something extraordinary for her as a woman to be doing physics. Around 1920, she became an employee at the Kaiser Wilhelm Institute for Physical Chemistry, where Franck also was. Franck took her with him as he appreciated her qualities. Franck had three assistants. One of them was Hertha Sponer. She admired him; you could see that clearly, yes, whether you want to grant it an erotic element or not, it had limits. Well, I remember that they liked to do some mischief on

7 Max Born appraised F. Hund's dissertation on November 4, 1922 as follows: "The present work emerged from the pro-seminar that colleague Franck and I led jointly. There Ramsauer's striking experimental results were discussed, according to which certain noble gas atoms, especially argon, are almost completely permeable to slow electrons. In order to explain this fact, which was also confirmed by experiments by G. Hertz and which seemed to contradict all valid theories, Franck gave two possibilities: (1) Perhaps it is not a matter of straight passage of the electrons, but of orbits, in which the electron orbits the nucleus once and then continues along the extension of the original direction of motion. (2) Perhaps for very slow electrons, which according to the classical calculation would fall into the nucleus due to their radiation deceleration, one must disregard the validity of electrostatics and instead introduce quantum theoretical deflections according to probability laws. These two questions have been examined theoretically in the present dissertation, the first exhaustively, the second at least offering a preliminary overview. The author tackled and solved the physically and mathematically difficult problems quite independently. The work conveys confidence in applying theoretical methods that go beyond the usual measure and good physical judgement. I apply for the rating: very good." [UA Gottingen, Math.-naturw. Fak., Prom. H. 1922/1923, No. 5]. See also Hund's paper on "Theoretical considerations on the deflection of free slow electrons in atoms", in *Zeitschrift für Physik*, 13 (1923), pp.241–263 and Gyeong Soon Im: "The formation and development of the Ramsauer effect", *Historical Studies in the Physical and Biological Sciences*, 25.2 (1995) pp.269–300, especially p.278.

8 Hertha Sponer (1895–1968) received her PhD under Debye in Göttingen in 1920. After a stay in Berlin with Franck, she returned to Göttingen in 1921 as his assistant, where she habilitated in 1925 and became a professor in 1932. See also F. Hund, Hertha Sponer–Franck, September 1, 1895–February 17, 1968, *Physikalische Blätter*, 24, p.166, 1968.

Theaterplatz with their bicycles—Franck was still very youthful at the time—whereupon Franck fell and broke something. She remained Franck's assistant until he left (1933). She stayed on over here for a while, but saw that she had no chance as a woman in the Third Reich and emigrated first to Norway, after which their lives parted ways.

After the death of Franck's first wife—a very nice woman, a Swede—they found each other again and got married; it was actually a romantic affair. He was a professor in Chicago, she was a professor at a university in North Carolina[9]. She had a cottage where they lived during vacations. They were too old for children. Before that, she had often made jokes about whether or not to marry. When an American couple was once staying here, the man employed at one university and the woman at a fairly distant university, she quipped: "Oh, if that's the way it is, then I'll get married too." So you can already see clearly here that marrying and having children were not so important to her.

R.T.: How did you meet your wife?

F.H.: She was a student assistant in mathematics. We saw each other occasionally, but we didn't really have any close contact. Only at the inauguration of the new (mathematical) institute in Bunsenstrasse (1929) did we meet again by chance. It occurred to me then that she actually is a very nice girl, and then I didn't let her go away again.

R.T.: Did she complete her math degree?

F.H.: She took her doctorate under Harald Bohr[10], formally of course under Courant.

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9 H. Sponer went to Oslo in 1933 ; from 1936, she was a member of the physics faculty at Duke University in Durham, North Carolina.

10 Harald Bohr (1887–1951), brother of the physicist Niels Bohr (1885–1962), had been a professor of mathematics at the polytechnic in Copenhagen from 1915 and from 1930 to 1951 there at the university. In the 1920s, he spent a lot of time at the mathematical institute in Göttingen.

R.T.: She has a doctorate? What's her maiden name?

F.H.: Seynsche-Hund[11], but the double name never occurs.

K.H.: Did you work together later?

F.H: No, hardly.

R.T.: Was she still working after her doctorate?

F.H: No, she had a doctorate, had her state exam and a preparatory year. When she was supposed to take the 2nd state exam—that was normal schooling, of course—we were married. She said: "Oh no, I want to have healthy children". She was very interested in science, read science magazines, etc., but didn't do active research.

K.H.: From 1922 Pascual Jordan[12] was also a student in Göttingen. When did you first consciously meet him?

F.H: Yes, back then when he came to join Born.

K.H.: And how close were you in contact with each other?

F.H.: Oh, we had little contact with Jordan. Well, it was the time of the youth movements back then, and I would say that any self-respecting person was part of the youth movement, so Heisenberg was, of course, and so was I. But Jordan always kept himself a

11 Ingeborg Seynsche(1905–1994), married F. Hund, received her doctorate in 1929 in Göttingen on the topic "On the theory of near periodic number sequences" (published in: *Rendiconti del Circolo Matem. di Palermo*, 55, 1931.). This dissertation topic had been proposed by Alwin Walther (1898–1967), from 1928, professor at the Darmstadt polytechnic, and was rated "very good" by Harald Bohr and Richard Courant. In the oral examination (on July 31, 1929) in mathematical analysis (Courant), physics (Franck) and applied mathematics (Gustav Herglotz), she received the grade "very good" [UAGöttingen, *Math.-Nat.Fak., Prom. S.*, Vol. IV, 1930–1934, No. 81.].

12 Pascual Jordan (1902–1980) had studied in Göttingen from 1922 and received his doctorate there in 1924 under Max Born. He then provided the mathematical proof for the correctness of the commutation relations between position and momentum operators found by Born and made important contributions to the development of Heisenberg's matrix mechanics and later to quantum electrodynamics.

little apart.

K.H.: Did the great proximity between mathematicians and physicists in Göttingen mean that people proceeded more cautiously than elsewhere, saying only what could be explicitly proven?

F.H: I wouldn't define it that way. Our most important seminar was announced under "Born and Hilbert". Hilbert had offered it before with Debye. The lecture was held by Kratzer[13] in the name of Hilbert, so to speak. Hilbert started getting sick at that time and wasn't very active.

K.H.: How would you describe your own work in the field of interpreting complicated spectra and in the area of explaining molecular bonds and molecular spectra in contrast to Mulliken's and to Slater's work?[14]

F.H.: Mulliken was tough and determined and solved many problems. Actually, once I understood the principles, I always first broached something new superficially and qualitatively. I remember the teasing about me. Kramers[15] would joke around, saying: When the dog (F. Hund, Hund means dog in German) wants to

13 Adolf Kratzer (1893–1983) had received his doctorate in Munich in 1920, and in the same year, he prepared the lecture on "Mechanics and New Gravitation Theory", on Hilbert's behalf. In 1921, he habilitated in Munich and in 1922 became a full professor for theoretical physics at the University of Münster.

14 Robert Sandersen Mulliken (1896–1986), professor of physics at the University of Chicago since 1928, published a large number of articles on molecular spectra since 1924. See also: F. Hund, Reminiscences of Robert S. Mulliken (a 12 min. talk in English, Gottingen 1988), *IWF Göttingen*, No. G 232, now available online at `https://av.tib.eu/media/14309`. John Clarke Slater (1900–1976) had studied in England in 1922 and from 1924 under Niels Bohr. From 1926 he published on the quantum theory of molecules and solids and developed new techniques for calculating the many-electron states (Slater determinant or Slater type orbitals).

15 Hendrik Anthony Kramers (1894–1952), professor of physics in Utrecht and Leiden (nuclear physics, quantum mechanics, solids, etc.). For his biography, see Max Dresden, *H. A. Kramers – Between Tradition and Revolution*, Heidelberg: Springer 1987.

sniff out a new area, it first sets about writing a paper on it.

K.H.: I would like to ask you something about the time after 1925, when you interpreted the spectra, for which you often used perturbation theory. What was your attitude towards the perturbation theory, which of course had been applied in celestial mechanics, but less in basic physics?[16]

F.H.: We learned about the theory of perturbations from Born; you must not attribute that to me. At the time, that seemed to be the only mathematical approach. Of course, he knew that classical physics and hence also the astronomical perturbation theory also had to be changed. He followed this path quite consistently. He wrote a successful procedure with the frequencies and then made the changes that were required by the integral nature of the quantum numbers. I also wrote this at least once in obituaries or other statements about Born[17].

K.H.: Did Born discuss the probability interpretation of quantum mechanics with you, which he developed using scattering processes?

F.H.: He invented them and his quantum theoretical discussion of scattering, for example of a particle on a cube, which he published in the summer of 1926[18]. He was probably the first to see that quantum theory does not describe the development or the motion of particles, but the motion of probabilities. Later this could be expressed in such a way that quantum mechanics deals with completely different objects than former (classical) mechanics, the motion of particles, e.g. of bodies under the influence of the

16 See e.g. Poincaré, Henri: *Les methodes nouvelles de la mecanique celeste*, Gauthier-Villars: Paris 1892.

17 Hund, Friedrich, Max Born, Göttingen und die Quantenmechanik, *Physikalische Blätter*, 38, I 1, pp.349–351, 1982, and likewise: Göttingen und die Atommechanik, *Bild der Wissenschaft*, 12, p.176, 1982.

18 See Max Born's paper "On the quantum mechanics of collision processes" in *Zeitschrift für Physik*, 37, pp.863–867, 1926 and 38 (1926) pp.803–827.

sun or electrons under the influence of a nucleus. But in quantum mechanics, these motions are not actually [considered] the goal. These motions were not what was discussed because they don't exist at all, but probabilities [because one recognized] that strict causality applies to probabilities as a consequence of the memory of the given state

K.H.: Did Born talk to you about it when he was here in Göttingen in 1926, or did he work more by himself?

F.H: He worked more on his own—he was smarter than me. (laughs) Not directly as it was being created, but (he discussed it) occasionally in seminars, obviously. Of course, he probably presented it at the colloquium or a seminar when his publications appeared.

K.H.: What did you think when, starting in 1927, Bohr and Heisenberg went public with interpretations of quantum theory?

F.H.: I learned about quantum mechanics from Heisenberg—now, that's something like a bond, but I saw that there was something to it, that the probability interpretation that finally became known in 1927 is a kind of a field. Of course, the Schrödinger function (equation) first described the movement of the amplitude of a probability field; and later, one got to know the meaning of dualism as a description of a real field. That was a long process, which interested me very much at the time. What is the function that was called Psi (Ψ)? What does it describe? Bohr's dualism made perfect sense to me. Around the end of 1927 or the beginning of 1928, one could describe quantum mechanics—one did not do it, but one could have—like this: (according to the interpretation given to it in Copenhagen) Quantum mechanics is a modification of classical mechanics that cannot be described graphically, and the modification goes just far enough to accommodate the quantum of action (Planck's constant). But now enters complementarity. The same quantum mechanics is also unintuitive, i.e. not at all intuitively comprehensible change of a classical field or wave theory, and it is changed just far enough to make particles possible. In the history of conditions, one cannot say exactly where something similar had stood. It was gradual hatching, and Bohr[19] certainly came the closest one to this idea. One can describe the atomic processes by a theory based on particles, but one has to consider the Planck constant. However, the processes can also be described by a modification of a field theory which is developed from the classical field theory. I wish I had figured this out from the start, but I figured it out afterwards. One can make a particle theory of matter, but one has to take into account the quantum of action (Planck‘s constant), but one can also make a field theory of matter, then one must also quantify field values. It had probably been a slow process for me toward understanding quantum mechanics. So I believe that one should properly [speak] of the Copenhagen dualism (i.e. complementarity); interpretation is a difficult concept. Today we no longer talk about the interpretation of Maxwell's equations, and so I don't like to talk about the interpretation of any quantum mechanical equations. It is a kind of natural reality. Nature is designed to allow this dualism.

K.H.: How was your relationship with Niels Bohr as a person?

F.H.: Bohr was a very engaging personality. We also often socialized with him at his country house in Tisvilde and got to know his family and very nice Mrs. Margrethe Bohr[20]. Bohr became something of an idol.

K.H.: Also for you?

F.H: Yes, yes, Bohr was always difficult to understand, but the formulations were not linguistically perfected. We called it the

19 After studying physics in Copenhagen, Cambridge and Manchester, Niels Bohr had developed a new theory of the atom in 1913, which for the first time contained a reasonably complete and empirically precisely confirmed theory of spectra and atomic scattering processes. From 1927 he developed in Copenhagen, together with his numerous students, the "Copenhagen interpretation" of quantum mechanics on the basis of his concept of complementarity

20 Margrethe Bohr (1890–1984), née Nörlund, was engaged to Bohr from 1909 and married to him in 1912.

"Bohrian" idiom. There were different dialects. There was an English dialect of Bohrian; there was a German dialect. There was a Danish dialect; the Danish one was also difficult to understand. If only because the Danish language is very difficult to understand acoustically.

K.H.: You were never bothered by the obscurity of the language and the obscurity of Niels Bohr's statements?

F.H.: Of course, it bothered us, but Bohr was just trying to say the unspeakable. Quantum mechanics treats processes that are different, that up until now could not be expressed in language as ordinary processes. The goal of this science was new, not to describe movements of bodies but movements of expected values. It required a new language. Bohr took this very seriously; this change in social language, so to speak, became clearer from this new fact.

There was something of that in his famous Göttingen lectures of 1922 when I saw him for the first time. We sat there, struggling to understand him acoustically. As students, of course, we weren't allowed to sit in the front rows; we sat further back with ears perked. That's how we gradually learned how to understand "Bohrian".

K.H.: The years after 1925 were a time of rapidly changing events in quantum mechanics. Did you feel competitive pressure from Heisenberg, Pauli, Jordan, etc.?

F.H.: Oh yes, you know, the professorship in Halle was once advertised. The list of candidates was Heisenberg, Pauli, Wentzel, Hund, etc. I knew that one day it would be my turn.

R.T.: You received a professorship in Rostock. Did you try to work with other mathematicians and physicists there?

F.H.: We didn't work together, but I already knew Robert Otto Furch[21] at least from the mathematics department, with whom I

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21 Robert Otto Furch (1894–1967), after receiving his doctorate in 1920 in Tübingen, continued his studies in Göttingen until 1921. From 1926, he

had already worked and studied a little in Göttingen. He had come to Göttingen on a kind of scholarship. And he was interested in physics. This acquaintance is what made my appointment [to Rostock] possible.

K.H.: What did you think when, in the late 1920s, Wigner[22] and some other mathematical physicists developed quantum mechanics and its symmetry principles in a very abstract way?

F.H: Well, that actually didn't interest us much; we were convinced of quantum mechanics and were able to formulate this quantum mechanics in proper language and weren't really looking for this abstract level—at least I wasn't.

K.H.: Can you describe the way in which you worked together with Heisenberg, and also independently of him in Leipzig?[23]

F.H.: We weren't equal in research; Heisenberg was done with quantum mechanics and worked, for example, on a field theory. Of course, I could only take part in that on a receptive level. He used to say that in the seminars, we held the seminar together[24]. So, on the research side, of course, everybody had to go his own way.

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was an associate professor and from 1928 to 1946, a full professor of mathematics in Rostock, then in Mainz until retirement.

22 Eugene Paul Wigner (1902–1995) wrote during his time as a private lecturer at the Berlin Institute of Technology his book on *Group Theory and Its Application to the Quantum Mechanics of Atomic Spectra* (1931). See also Hermann Weyl's book on *Group Theory and Quantum Mechanics* (1931) and B.L. van der Waerden's book on *Group Theoretical Methods in Quantum Mechanics* (Berlin 1931).

23 In 1929 Hund succeeded Gregor Wentzel as Professor of Mathematical Physics at the University of Leipzig.

24 The joint seminar "On the Structure of Matter" became famous. See also Hund's book on years of work with Werner Heisenberg in Leipzig. *Werner Heisenberg in Leipzig 1927–1942*, (Akademie Verlag: Berlin 1993), pp.94–97 and Carl Friedrich v. Weizsäcker's memoirs about his studies in Leipzig (ibid, p. 125) and in the periodical *NTM new ser.,* 1 (1993) 1, pp.3–18.

At that time I was occupied with molecules and the chemical bond. Of course we shared the teaching, the usual lectures, of which a large part was still classical physics.

R.T.: Was there any collaboration with chemists in this research on chemical molecules?

F.H.: Actually, quite little; the physicist Erich Hückel had a brother: Walter Hückel[25]. Through this connection, physics actually became known in chemistry. Of course, physical chemistry was always the mediator. When Karl Friedrich Bonhoeffer was given the chair for physical chemistry in Leipzig[26], the collaboration naturally became very close.

K.H.: During your time in Leipzig you certainly wanted to retain your independence from Heisenberg. When selecting your research topics, were you careful to investigate something that Heisenberg was not doing? Was that a criterion for you?

F.H: Yes, of course, but the difference in level was clear.

K.H.: I saw a citation statistics which shows that your work was cited more than Heisenberg's work in the late 1920s[27].

F.H.: Oh, I wrote a little more and was less careful.

K.H.: Presumably, some of your work was also applied more.

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25 Erich Hückel (1896–1980) had received his doctorate with Debye (1921) and stayed as a private lecturer in 1928/30 with a scholarship in London, Copenhagen and Leipzig. See esp. the book on *Dipole moment and Chemical Structure*, ed. by Peter Debye (1929) with contributions by F. Hund, W. Hückel a.o. as a result of the "Leipzig Weeks" 1929 on problems in atomic physics. On Hückel see Andreas Karachalios, *Erich Hückel (1896–1980): From Physics to Quantum Chemistry*, transl. by Ann M. Hentschel, Springer: Berlin, 2010.

26 Karl Friedrich Bonhoeffer (1899–1957) received this chair in 1934 at Debye's instigation.

27 See Fischer, Klaus, *Changing Landscapes of Nuclear Physics. A Scientometric Study*, Springer: Berlin 1993, p.63. In the period from 1926 to 1930, the following order of citation frequency was given: Rutherford, Chadwick, Bothe, Born, Hund, Heisenberg.

F.H.: This method of comparison should not be used here.

K.H.: What happened in the 1930s when the first steps towards nuclear physics were taken? Were you interested in that too?

F.H.: Heisenberg noticed right away that the experiments indicated that there is a neutron and that the nuclei consist of neutrons and protons and not just—as had previously been believed—of protons and electrons. Heisenberg saw this very early on, but one cannot say that he was the only one. That's a small part of it! In 1932 a lot happened: the discovery of the neutron, also the discovery of the positron; these things were done mostly in England. Heisenberg accepted this immediately and actually immediately came up with a theory of lighter nuclei.

K.H.: But you weren't interested in this topic that early?

F.H.: Oh sure, I was very slow, so I wrote an overview of the states of the lighter nuclei. In some analogy to the structure of the atomic spectra one could design nuclear spectra. But it was easy to see that it was much more difficult and yielded less. Of course, I thought about that. One could now design a system of nucleic states (ground and excited states), which at that time were known only very imperfectly experimentally, as was underway in atomic physics.

R.T.: Did you suggest dissertations on this topic?

F.H.: Yes, a few smaller ones, especially later in Frankfurt, e.g. Dieter Pfirsch, about qualitative systematics of the quadrupole moments. But what I didn't know at first was that Rainwater[28] had already explained this. The thirties were a time when not much was happening; during the Nazi era, one had to be careful to stay alive. This is actually a blank page in the history of physics for most physicists.

28 Leo James Rainwater (b. 1917) proposed the concept of "deformable nuclei" in the early 1950s, on which Aage Bohr and Ben Mattelson based their theory of the atomic nucleus. In 1975 the three received the Nobel Prize in Physics for it.

R.T.: Were there any women who defended their doctorates with you?

F.H.: Eleonore Trefftz[29] officially took her doctorate under van der Waerden, but actually on a physical topic, namely the crystal lattice in a quantum theoretical description. Of course, her father sent her to Göttingen when she had grown up. She worked mainly with van der Waerden. He regularly came to our seminars. Later she had a kind of assistant position with me.

R.T: Where?

FH: In Leipzig. Heisenberg went to Berlin. I was actually in charge of the Leipzig affair and kept it as small as possible.

K.H.: During your time in Leipzig, did you also have to fear and ward off interventions in the institute by the (National Socialist) party?

F.H.: Oh, we were careful; we didn't exactly announce any lecture on the theory of relativity, but rather one on the electrodynamics of moving bodies[30]. Science was very unimportant to the party for years. They didn't think it could have any significance, and that's how we were all spared.

K.H.: Did you have party members with firm NS-convictions in the institute who were known to be spies?

29 Eleonore Trefftz, born 1920 (daughter of the mathematician Erich Trefftz (1888–1937), professor for technical mechanics at the Technical University of Dresden since 1922), studied mathematics, theoretical physics and philosophy in Leipzig from 1941–44, among others, with Ernst Hölder, Friedrich Hund, Werner Heisenberg and Hans Georg Gadamer. She received her doctorate in 1945 at the Dresden Institute of Technology on the subject "Curie transformations of mixed crystals based on classical statistics".

30 According to Kleint, Christian; Wiemers, Gerald (ed.), *Werner Heisenberg in Leipzig, 1927–1942*, Akademie-Verlag: Berlin 1993, pp.175–179, Heisenberg renamed the lecture in the course on "Theoretical Physics III", which before 1933 had always been called "Electrodynamics" into "Electricity Theory". In the trimester Jan–March 1940, he organized an event, "History of the Electrodynamics of Moving Bodies" to Otto Heckmann and Fritz Sauter likewise in Göttingen in the summer term of 1936.

F.H.: Probably not, that can't be put so bluntly; we knew about some who were closer to the party, but on the other hand, they also respected our convictions, so I don't think that anyone was denounced within the framework of the institute; the attacks against Heisenberg came from a completely different quarter[31].

K.H.: Were you ever asked to take part in the uranium project[32]?

F.H: Certainly not, I could have got in there. I knew that these things had to be kept secret somehow and of course also kept secret from others in Leipzig. That was guided by other aspects.

K.H.: According to which aspects? Did you decide you didn't want to do it?

F.H.: There was, for one, an official Army Ordnance Office (Heereswaffenamt) that was also in charge of progress in applied science and technology. Then there were people who were trying to make a name for themselves. Various statements were made about how much energy can be released in fission, but one usually remained silent in public.

K.H.: Have you ever been a guest or speaker at the Kaiser Wilhelm Institute for Physics in Berlin?

F.H: No, not in this matter.

K.H.: But in other matters?

F.H.: Well, of course we visited occasionally and talked about something.

K.H.: But there wasn't any intensive exchange?

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31 See Hentschel, Klaus (ed.), *Physics and National Socialism. An Anthology of Primary Sources*, Birkhäuser 1996, Doc. 57, for Hund's hitherto unpublished letter of protest to Reich Minister of Education Bernhard Rust, dated July 20, 1937, and Doc. 74 for Siegfried Flügge's August 1939 newspaper article on "Exploiting atomic energy and the uranium machine" with estimates on the energy released in atomic fission.

32 See e.g. Mark Walker's book on the uranium machine, (Siedler-Verlag: Berlin 1990 German translation) and Hoffmann, Dieter (ed.): Operation Epsilon, (Rowohlt-Verlag: Berlin 1993).

F.H: Of course not in this matter, it was classified as secret.

R.T.: You mentioned van der Waerden[33]. Was there any regular contact between him and other mathematicians in Leipzig?

F.H.: We knew Van der Waerden personally from Göttingen. This also resulted in personal relationships with the family. The physicists weren't so uninvolved in van der Waerden's appointment.

K.H.: In 1946 you went to the University of Jena. Weren't you able to continue in Leipzig? Was it your own decision to go to Jena?

F.H.: It was my decision, I was something of a rarity because I had stayed in the GDR and I could actually choose the university where I wanted to work. In Leipzig I would have been involved in designing building plans for a physics institute for a whole decade, but I certainly wouldn't have been able to do any physics.

K.H.: And Jena was not so devastated.

F.H: Jena was less destroyed.

K.H.: Was that the main reason for you to move to Jena?

F.H.: Well, the main reasons, who knows the motives behind the decision. My wife wanted to go to Jena. The big city of Leipzig was losing its charm. When we arrived in Jena, she said: "We won't be leaving here again willingly." [laugh]

K.H.: You had to do a lot in Jena rebuilding, I mean, recruiting the right people?

F.H.: Yes, there was quite much to do and see that the lecture system, the seminars, etc., got going. But as long as one didn't get in the way of the politicians, one actually had great freedom in

33 Bartel Leendert van der Waerden (b. 1903), whose fields of work were algebra, algebraic geometry, number theory, topology, also probability calculation and statistics and, especially since his retirement, history of mathematics and astronomy, worked from 1931 to 1945 as a professor of mathematics at the University of Leipzig. See the interview with him in *NTM new ser.*, 2 (1994) 3, pp.129–147.

designing timetables and lecture programs, including the individual courses. I never was as independent as I was then. Of course, Koebe[34] had died at that time, who had always been a bit of an obstacle in joint decision-making.

R.T.: Why was he against joint decisions?

F.H.: He didn't want to give lectures on Mondays. It was not so easy to work with him to create a reasonable timetable for the entire faculty.

K.H.: In Jena, you wrote a multi-volume work as an introduction to theoretical physics.

F.H.: Yes, somewhat for lack of anything better. What else should I do? You couldn't conduct research; you didn't hear about the latest findings. Its beginnings were earlier in Leipzig. I knew the publisher Otto Mittelstädt quite well, and he talked me into it. That was a bit of a last resort. What else should one do during those terrible times, simply write books.

R.T.: Did theory of relativity and quantum physics play an important role in the lecture program?

F.H.: Oh yes, that happened naturally. We had the traditional sequence of lectures mechanics, electricity, optics perhaps, but a bit shorter, thermodynamics, atomic and quantum theory, as we first called it. We naturally started from the problems of the atom. In the beginning, that was more of a priority. Then the quantum of action (Planck constant) gradually moved into the main focus.

K.H.: What was the overall importance of teaching for you? Did you like teaching?

F.H.: Oh yes, both research and teaching, both of which I welcomed as an advantage of this profession.

K.H.: Did you have many PhD students?

F.H.: Of course there weren't that many, I only had a few (proba-

34 Paul Koebe (1882–1945) was full professor of mathematics from 1914 to 1926 in Jena, from 1926 to 1945 in Leipzig.

bly a one-digit number), later there were a little more in Frankfurt and Göttingen.

R.T.: You said your wife didn't really want to leave Jena. Why did you decide to leave?

F.H.: For political reasons, I saw that things could be achieved in the West; after all, got students of better quality than the ones we got in the East; had access to literature that one couldn't get in the East. It was quite clear in terms of research quality and teaching that the gap between the Federal Republic (of Germany) and the GDR was quite large. If you still wanted to accomplish something, you probably had to go to the Federal Republic. Of course, it took a while before you felt at home there again.

K.H.: Did you like the scientific environment in Frankfurt or did you prefer coming to Göttingen?

F.H.: I was a bit overloaded with teaching. I had a little too much, I was the only professor in the subject, I also had a lecturer, but he wasn't very involved. I was actually overburdened with the task. That was the main reason I went to Göttingen.

R.T.: You received two prizes: a Max Planck Medal and a National Prize. With what feelings did you receive that?

F.H.: Well, the National Prize was of course political.

R.T.: Just political?

F.H: No, but they didn't have anyone else, the others had gone away. I was a rarity, and they had to have a physicist somehow, I didn't take that scientifically seriously.

K.H.: But the Max Planck Medal from 1943?

F.H.: I took that as recognition of the work I had done before.

K.H.: Do you remember the circumstances? It was 1943, during the Second World War, when no physicists' convention took place.

F.H.: Yes, it was awarded in a smaller gathering.

K.H.: Was Max Planck present?

F.H.: Oh, I don't remember for sure, probably.

K.H.: You don't know who handed the medal to you? Who held the speech, the eulogy?

F.H.: That was kept short, and my lecture did not concern the quantum theory of the atom at all but was about that dualism, that one can regard the chemical force as a real force. It is electrodynamics if you start from particle theory, but within the framework of field theory, it is a force in its own right. What I said was already well known. Uranium fission was before that. That the strong coupling is described as a special force next to the magnetic force and next to gravitation[35].

K.H.: When you came to Göttingen in 1956, you were soon elected to the Academy of Sciences. What did that mean to you?

F.H.: I was a member of the Leipzig Academy, and so that was actually customary.

K.H.: Did you benefit from the discussions in these two academies?

F.H.: Oh, [you] sometimes got less money, sometimes more money.

R.T.: Money for research?

F.H.: Yes, for the assistants, I had two assistant positions and four good people that I wanted to keep. Then I had one paid for by the Academy in Leipzig and the other by the German Research Foundation. You looked for some funding source, and afterwards the people got their appointments.

K.H.: When did you start taking increased interest in the history of science?

F.H.: After retirement; well, I realized my research methods were becoming obsolete. I'd leave that to younger ones and start something new. That was the real motive.

R.T.: Was there any contact with the history of physics in Leipzig?

35 Cf. e.g. Hund's papers on Forces and their conceptual version, *Verhandlungen der Deutschen Physikalischen Gesellschaft*, 24, S.12-20, 1943, and on Chemical Forces as an effect of material field, in 36, pp.319–327, 1939.

F.H.: I was a member of the Leopoldina in Halle[36]. It is an organization in its own right that has general interests. Medical historians were in the lead. They [medical historian] may have had some influence in getting me elected. That's how I had a bit of contact with the history of science. But I didn't think I'd get this old, otherwise I'd have done it more thoroughly.

K.H.: But you did it very thoroughly. You wrote several publications on the history of concepts in physics and the book on quantum theory[37].

F.H.: I did what I thought was necessary for the students at the time. A gap had formed. Besides, as an emeritus, I couldn't take the lectures away from the real professors. So I had to do something else. I just filled that gap in the market.

K.H.: Were you always very interested in university politics?

F.H.: Yes, I also had to give more general lectures, where I mentioned these things. As rector in Jena in 1948, I had to give a lecture at the anniversary celebration, and I did so. It's also published somewhere[38].

K.H.: What discovery was the most exciting for you to have experienced so far?

F.H.: Discovery—well of course in a certain sense the Heisenbergian approach to quantum mechanics. Others would say the discovery of the neutron or something like that[39].

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36 The German Academy of Natural Scientists Leopoldina was founded in 1652 in Schweinfurt.

37 See Hund's books on the history of physical terms, (Mannheim 1972) and on the history of quantum theory, (3rd ed., Bibliographisches Institut: Mannheim 1984).

38 See Hund's book on *Physics and General Education*, (Jena 1949).

39 The neutron as the second, electrically neutral nuclear building block—in addition to the positively charged proton—was discovered in 1932 by James Chadwick (1891–1974).

K.H.: Thank you very much for the information we received today.

F.H: But don't forget me!

Prof. Dr Klaus Hentschel, Head of the Section for the History of Science and Technology Keplerstr. 17, D-70184 Stuttgart, Germany.
Prof. Dr Renate Tobies, Gastwissenschaftlerin am Ernst-Haeckel-Haus, Berggasse 7, D-07745 Jena, Germany.

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[11] Curriculum vitae, papers, diploma, honours, medals and decorations, photography, films and cassettes of Friedrich Hund compiled and maintained by son Gerhard Hund are available on: https://www.teleschach.de/teleschach/f_hund_en.htm. The material in the various Boxes in this article have been taken from this website with due permission from Gerhard Hund which is gratefully acknowledged
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Address for Correspondence
Kalidas Sen
School of Chemistry
University of Hyderabad
Central University P.O.
Hyderabad 500 046
Telangana State
Email:
kalidas.sen@gmail.com