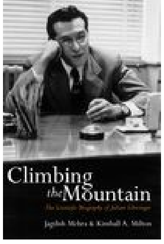

Climbing the Mountain: The Scientific Biography of Julian Schwinger

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CHAPTER

2 Julian Schwinger at Columbia University

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Abstract

Julian Schwinger became a frequent visitor to the excellent Columbia University Library in New York City. The Schwingers lived quite close to the University, and Julian often walked there, casually entering the library, picking up a book and finding a quiet place somewhere to read. Julian continued to use the library at will until he became a regular student at Columbia. This chapter chronicles Schwinger's education at Columbia University, his work on spin resonance and neutrons, his stay at the University of Wisconsin, and his final year in graduate school. The introduction of the tensor force was Schwinger's first significant and truly original contribution to nuclear physics.

Keywords: Columbia University, University of Wisconsin, spin resonance, neutrons, nuclear physics, tensor force

Subject: History of Science and Technology, History of Physics

Transfer to Columbia

Although Benjamin Schwinger was not able to fulfill his dream of sending his sons to Columbia University, Julian did not think much about it; in any case, he became a frequent visitor to the excellent Columbia University Library. The Schwingers lived quite close to the University, and Julian often walked there, casually entering the library, picking up a book and finding a quiet place somewhere to read. This normally unallowed procedure continued for several months until one day a librarian, puzzled by his young age, asked Julian whether he had library privileges. He lied to her that that he did, and she asked for his name, then looked up the list of library card-holders to verify. To the amazement of the young boy, who was already resigned to hear a reprimand, a strange thing happened: She indeed found a Schwinger on the list of registered users! From then on, mistaken for this unknown relative, Julian continued to use the library at will until he became a regular student at Columbia.[1]

In early 1935 Schwinger also began frequenting seminars and colloquia at Columbia in the company of Lloyd Motz. He found them exciting and became a regular visitor there. After he had been attending these events for several months, Isidor Rabi noticed him and, intrigued by his youth, asked Motz: 'Who is that sleepy-eyed kid you bring along with you?' Motz explained that 'he is a very brilliant, incredibly bright sophomore from the City College' and promised to bring him over one day and introduce him to Rabi.[2] The occasion presented itself soon, when one day Julian and Motz were talking in front of the library. The library and Rabi's office opened on to the same hallway on the eleventh floor of the Pupin Physics Laboratory. Suddenly the door opened and Rabi appeared; he invited Motz into his office to discuss 'a certain paper by Einstein in the *Physical Review*.' Motz introduced Julian and asked if he could bring his young friend along; Rabi did not object, and so it began.[1]

The Einstein article turned out to be the famous paper of Einstein, Podolsky, and Rosen,[3] with which young
p. 23 Julian was already familiar. He had studied ↳ quantum mechanics with Professor Wills at the City College,

and discussed with him the problem of the reduction of a wave packet after additional information about a quantum system is gained from a measurement. 'Then they [Rabi and Motz] began talking and I sat down in the corner. They talked about the details of Einstein's paper, and somehow the conversation hinged on some mathematical point which had to do with whether something was bigger or smaller, and they couldn't make any progress. Then I spoke up and said, "Oh, but that is easy. All you have to do is to use the completeness theorem." Rabi turned and stared at me. Then it followed from there. Motz had to explain that I knew these things. I recall only Rabi's mouth gaping, and he said, "Oh, I see. Well, come over and tell us about it." I told them about how the completeness theorem would settle the matter. From that moment I became Rabi's protege. He asked, "Where are you studying?" "Oh, at City College." "Do you like it there?" I said, "No, I'm very bored."'[1]

Watching young Julian demonstrate such 'deep understanding of things that were at the time at the frontier and not clearly understood,'[2] Rabi decided on the spot to talk to George Pegram, then chairman of the physics department and dean of the graduate faculty, to arrange Julian's immediate transfer to Columbia. He and Motz left Julian waiting and went to see Pegram who also had an office in the same building. Motz stayed behind and waited outside Pegram's office. Rabi emerged a few minutes later with the word that there might be a scholarship available and Pegram would help in carrying the transfer through. Motz hurried to bring the good news to Julian, but he was astonished to find the independently minded Schwinger hesitate. The unique intellectual atmosphere of the City College where he had made many friends and felt at home had worked very well for him so far; therefore he decided not to rush and first to seek a transfer to the honors program at the College, and only if this didn't work would he accept Rabi's offer.[2]

The honors program at the City College was generally available, with the approval of the physics department's chairman Charles Corcoran, to the best physics majors after they had completed the core curriculum physics courses, but Schwinger had finished only the basic first-year requirements and was at odds with Corcoran for not returning his laboratory reports. Therefore Motz felt that he had better chances with Corcoran than did Julian, and offered to bring up the subject with the boss himself. The chairman had already heard about Julian, but the City College at that time was a unique place, full of excellent students, brought up with the attitude of studying and passionate about learning, more brilliant than the faculty, who had come from backgrounds that did not emphasize intellectualism much.[2] The place abounded with talent and Corcoran did not see anything extraordinary in Schwinger, whose grades outside mathematics and physics were quite abominable because he always ↳ performed poorly if the nature of the course did not agree with his individualistic patterns of study. After Motz made an impolitic remark that Julian knew more about physics than did most people on the faculty, Corcoran bristled with anger and ruled that the proposition was out of the question. According to Bernard Feld, 'Corcoran is alleged to have said, "Over my dead body. As long as I'm chairman of this department, no smart-ass kid is going to be allowed to skip taking my course in elementary physics."'[4] Several days later he even criticized Motz in a department meeting for trying to ruin the fine-tuned process of educating the young man in the only natural way, that is gradually.[2],[*]

Rather upset, Schwinger returned to Rabi and asked him to set the process of transfer to Columbia in motion. To Rabi's astonishment it turned out to be more difficult than he had expected. The obstacle was Julian's terrible grades. An official who examined his transcripts from the City College declared that on their basis Julian could not even be admitted to Columbia University. Rabi felt a little insulted and asked: 'Suppose he were a football player?' and decided to override the administration with Pegram's assistance and the help of Hans Bethe. Bethe provided an enthusiastic letter of support after he read Julian's notes on electrodynamics.[6] Bethe's letter, dated 10 July 1935, reads as follows:

'Dear Rabi,

Thank you very much for giving me the opportunity to talk to Mr. Schwinger.

When discussing his problem with him, I entirely forgot that he was a sophomore 17 years of age. I spoke to him just as to any of the leading theoretical physicists. His knowledge of quantum electrodynamics is certainly equal to my own, and I can hardly understand how he could acquire that knowledge in less than two years and almost all by himself.

He is not the frequent type of man who just "knows" without being able to make his knowledge useful. On the contrary, his main interest consists in doing research, and in doing it exactly at the point where it is most needed at present. That is shown by his choice of his problem: When studying quantum

electrodynamics, he found that an important point had been left out in a paper of mine concerning the radiation emitted by fast electrons. That radiation is at present one of the most crucial points of quantum theory. It has been found to disagree with experiment. It is quite conceivable that the error which Mr. Schwinger found in my paper might bring about agreement between theory and experiment which would be of fundamental importance for the further development of quantum electrodynamics. I may add that the
p. 25 mistake has not ↳ only escaped my own detection but also that of all the other theoretical physicists although the problem has been in the centre of discussion last year.

The way in which Schwinger treated his problem is that of an accomplished theoretical physicist. He has the ability to arrange lengthy and complicated calculations in such a way that they appear simple and can be carried out without any great danger of errors. This gift is, I believe, the most essential requirement for a first-class theoretical physicist besides a thorough understanding of physics.

His handling of quantum theory is so perfect that I am sure he knows practically everything in physics. If there are points he does not know, he will certainly be able to acquire all the necessary knowledge in a very short time by reading. It would be just a waste of time if he continued listening to the ordinary physics course, 90% of whose subject he knows already while he could learn the remaining 10% in a few days. I feel that nobody could assume the responsibility of forcing him to hear any more undergraduate (and even the ordinary graduate) physics courses.

He needs, of course, some more courses in minor subjects, principally mathematics and chemistry and a small amount of physical laboratory work. In physics the only thing he has to learn is teaching physics, i.e., to explain himself very simply—an art which can be learnt only by experience. He will learn that art automatically if he works at a great institution with other students of similar caliber.

I do not need to emphasize that Schwinger's personality is very attractive.

I feel quite convinced that Schwinger will develop into one of the world's foremost theoretical physicists if properly guided, i.e., if his curriculum is largely left to his own free choice.'*

Eventually, Schwinger was admitted to Columbia as a junior, with a full tuition scholarship starting in September 1935, but in the preceding summer semester he had to take some required courses that he had missed out at the City College.[1]

Rabi laid down a contract to Julian. 'You're coming here and you are going to take all undergraduate courses and I want you to get As in all those classes.'[2] Julian obliged for a while, but soon returned to his own individualistic ways. He disliked writing themes or laboratory reports and treated them as a nuisance that distracted him from his real vocation, which was learning physics. He admitted unabashedly: 'I did not learn anything in my physics courses [other] than what I already knew from my own private studies.'[1] Therefore, to avoid any pressure to attend lectures he began to develop work patterns which gradually drifted into later
p. 26 and later working hours, extending deep into the night. He ↳ would sleep through the day and show up at Columbia around 6 o'clock in the evening. He spent most of his time reading advanced texts in physics and mathematics and journal articles in the library, and writing papers. It was a relatively simple matter for him to pass oral examinations by stunning his professors who watched him inventing on the spot his own proofs or nonstandard methods of approaching standard problems. Sometimes this did not work, and he flunked the chemistry course of Victor LaMer, who had the custom of introducing his own peculiar notation and demanding that his students make full use of it, and which was obscure to anybody who did not attend his class regularly.[7],[*]

Rabi recalled, 'LaMer was, for a chemist, awfully good. A great part of his lifework was testing the Debye–Hückel theory[9] rather brilliantly. But he was this rigid, reactionary type. He had this mean way about him. He said, "You have this Schwinger? He didn't pass my final exam." I said, "He didn't? I'll look into it." So I spoke to a number of people who'd taken the same course. And they had been greatly assisted in that subject by Julian. So I said, I'll fix that guy. We'll see what character he has. "Now Vicky, what sort of guy are you anyway, what are your principles? What're you going to do about this?" Well, he did flunk Julian, and I think it's quite a badge of distinction for him, and I for one am not sorry at this point, they have this black mark on Julian's rather elevated record. But he did get Phi Beta Kappa[†] as an undergraduate, something I never managed to do.'[6]

Norman Ramsey added an amusing footnote to this story. In 1948 Schwinger had to repeat his brilliant lecture on quantum electrodynamics three times at the American Physical Society meeting at Columbia, in successively larger rooms.[‡] 'It was a superb lecture. We were impressed. And as we walked back together—Rabi and I were sitting together during the lecture—Rabi invited me to the Columbia Faculty Club for lunch. We got in the elevator [in the Faculty Club] when who should happen to walk in the elevator with us but LaMer. And as soon as Rabi saw that, a mischievous gleam came into his eye and he began by saying that p. 27 was the most sensational thing that's ever happened in the American ↳ Physical Society. The first time there's been this three repeats—it's a marvelous revolution that's been done—LaMer got more and more interested and finally said, "Who did this marvelous thing?" And Rabi said, "Oh, you know him, you gave him an F, Julian Schwinger."'[7]

Somewhat later George Uhlenbeck came from Holland as a visiting professor to Columbia and taught a course in statistical mechanics in which he was a great expert. A large number of students signed up to take his class and many faculty members also attended. Schwinger registered, but never went to class, and did not bother to take the final exam. Uhlenbeck complained to Rabi that he was not even given a chance to see the invisible student. Rabi became infuriated. He knew that Schwinger had just begun dating and felt concerned that he might be getting distracted away from physics. He decided to see Julian in person and ordered him to take the oral examination (as was the Dutch custom) immediately. Schwinger bargained that he would do so but only at 10 o'clock in the evening. This request was beyond Uhlenbeck's limit of tolerance, yet a special examination date was arranged for 10 o'clock in the morning. With Schwinger's answers in the examination, Uhlenbeck was overwhelmed: 'I can say nothing. Not only did he hand in a perfect paper, but he did it in the way I did everything, as though he had sat through every lecture. This is amazing. So I have nothing to say,' he declared to Rabi.[2,6,10]

Schwinger's lack of attendance at lectures and completion of coursework caused other problems as well. Many years later Norman Ramsey recalled that when he proposed him for membership in Phi Beta Kappa, many people objected and cited his uncompleted courses and bad grades. Julian was eventually elected to Phi Beta Kappa, but only after a big argument in which Ramsey pointedly remarked that Schwinger had published more papers that year than anybody on the faculty.[7,8]

At Columbia, Julian had somewhat severed his relations with most of his peers in class. To a greater extent than at City College, when because of his young age he could develop by emulating his fellow students, at Columbia he benefited more from the faculty. He had good working relations with graduate students and professors and generally enjoyed interacting with people. He participated in seminars and was a good listener, since from his early articles it is evident that he had a detailed knowledge of the most recent experimental data and formal developments. He also offered himself as a lecturer in seminars and discussion groups and discussed matters in a mature manner.

Lloyd Motz vividly described one such seminar talk, given at a weekly tea meeting of the Astronomy Journal Club run by Jan Schilt and attended by astronomers from colleges and universities in the New York metropolitan area. Neutron stars were then the hot topic in their discussions, some two years before the p. 28 definitive paper of Oppenheimer and Volkov appeared.[11] Schilt looked for ↳ someone able to present at the Journal Club the rules of the new quantum statistics, the questions related to quantum degeneracy, properties of the degenerate electron gas, and similar topics. Motz suggested Schwinger as the most suitable person for the task. Julian quickly agreed: 'Sure, no problem. As soon as you want me!'[2] The lecture was a revelation. 'Everything was perfect. He would begin writing at one end of the blackboard, and then finish at the other. He would do things with his left hand. He was ambidextrous so he would write on the board with both hands. It was all so beautiful. It came out of him the way music came out of Mozart, as though he had been born with it. He never made a mistake. It didn't matter what question you would ask him; he always had a ready answer.'[2]

Julian sought isolation for his work. Of course, the habit of shifting to work late in the evening and sleeping through the day had to be in conflict with the basic responsibilities of the undergraduate's life. Julian ceased attending classes; he felt he did not need them. Very likely he shifted his life into the night pattern just to avoid being pressured to go to classes and waste his time listening to other people explaining what he already knew. He was indeed a very strange kind of undergraduate, whom Rabi often asked to be his substitute for teaching a graduate quantum mechanics class for him when he was away or had other engagements. According to Rabi, 'Whenever I had to go away, I'd ask Julian, who was an undergraduate, to take the class. I can assure you it was a great improvement. He's a much better teacher than I ever was.'[6]

Rabi praised Julian enormously for his willingness to offer help in any calculation; he would not stop at the final formula, but work with the phenomeno-logical data until he could produce a final number as an answer. Similarly, he was very friendly and helpful in his interactions with fellow students. Morton Hamermesh recalled Schwinger teaching him (!) group theory, and the intricacies of using Bessel functions in theoretical calculations: he coached Hamermesh for days, several hours at a time.[12]

Rabi had great confidence in his protégé, but it was not limitless. He was afraid that one day his genius would turn out to be a flash in the pan and he had to reassure himself periodically by introducing Schwinger to any physicist of consequence who visited Columbia. They all left impressed by his age and by the sheer volume of knowledge he had acquired. Wolfgang Pauli wrote a letter to Rabi saying how impressed he was and closed with the words, 'And give my love to this physicist in knee pants.' Recall that in the summer of 1935 while Rabi was trying to get Julian into Columbia, Hans Bethe arrived and Rabi asked him to assess Julian's progress and send a written evaluation. Bethe sent a letter full of superlatives including a strong endorsement. After receiving this remarkable assessment, a happy and beaming Rabi showed Bethe's letter to Motz with the words, 'Now, I am satisfied.'[2]

p. 29

Spin resonance

At Columbia University Schwinger, for a while, continued his research contacts with Halpern and Motz, but soon he gained so much confidence in himself that he did not need their support and encouragement. Besides, they were no match for him in the speed of doing calculations. By September 1936, at the age of 18, only one academic year and two summer sessions after his matriculation at Columbia, Julian received his undergraduate degree and, in passing, produced a quantity of research ordinarily considered sufficient for a doctoral dissertation. Rabi recalled that it was not altogether trivial to get Julian's undergraduate degree in such short time. Columbia required more than just completing a sufficient number of hours. You had to have 'a certain weight of ordinary credits and a certain weight of maturity credits. One Sunday morning I was called up by the dean, Dean Hawkes, and he said, what shall I do about Schwinger? I said, what's the problem. He said he has enough credits to graduate but he hasn't enough maturity credits. It seemed too absurd. How can you talk about things that way? So I said, well, you have your rules. I don't know what you can do about it. I wasn't going to make a great plea. See how the thing'd work. Well, he was a real man, and on Sunday, he was a religious person, he said, I'll be damned if I won't let Schwinger graduate because he doesn't have enough maturity credits. Of course, this gave me great faith.'[6] It seems that Edward Teller was the first person to deem Schwinger's work on neutron scattering as worthy of a PhD,[2,13] but the requirements of the graduate school at Columbia set a minimum two-year residence period for doctoral candidates. Considering Julian's young age, there seemed to be no compelling reason to depart from this rule. Schwinger himself did not see any point to the rule: 'Why they didn't let me out of Columbia two years earlier, I will never know.'[1] In the meantime he registered for more courses, and ever faithful to his custom, he seldom if ever attended classes and kept on working on problems of scattering theory and spin. He soon became a sought-after expert in this subject, a real catalyst (Motz called him a 'spark plug') for Rabi's spin resonance team and also J. R. Dunning's cyclotron experimental group, where he helped to interpret the influx of data produced with the use of this emerging (just five-year-old) technology.

Julian carried around ever thicker-growing notebooks, but never felt compelled to write articles, even though Motz and Rabi insisted that he finally write up at least a part of his results. Finally, during the year 1937, Schwinger published five papers in the *Physical Review*. They became his doctoral dissertation; he never sat down to write a doctoral thesis as such, but submitted a bound-up set of these papers as his dissertation.

The common trait of these articles was that they were all devoted to spin and magnetic moment-dependent
p. 30 aspects of neutron scattering. In 1936 and ↳ 1937 so little was known about the neutron that in his comprehensive review of nuclear physics, published then in the *Reviews of Modem Physics*,[14] Hans Bethe still had to invoke the argument of simplicity to justify a value of one-half for neutron's spin over an equally plausible magnitude of three-halves. Due to the fact that the neutron has no charge, nuclear physicists had to rely on indirect information from nuclear spins and from the data gathered in proton-neutron and proton–deuteron scattering experiments.

Rabi remained the strongest influence upon Julian at that time and it was therefore no accident that the first two articles Schwinger wrote at Columbia, begun while still an undergraduate, were related to his interests. In these articles, Schwinger improved upon or corrected the works of Rabi and Felix Bloch. The first of his Columbia papers was a full-length article 'On the Magnetic Scattering of Neutrons'[3], which included the work he had completed by himself, albeit with Rabi's blessing, in 1936. Earlier that year, Felix Bloch had proposed a technique for measuring the neutron's magnetic moment from the spatial distribution of neutrons scattered twice on targets magnetized in different directions.[15] Bloch argued that since the range of nuclear interactions is short and the neutrons carry no charge, the scattering of thermal neutrons from atomic targets is dominated by the magnetic interaction between the neutron's spin and the atomic electrons. Therefore an unpolarized stream of neutrons scattered by a magnetized target (ideally by saturated magnetized iron plates) becomes partially polarized. If it is then scattered for the second time, the angular distributions of emerging neutrons depend on the relative orientations of magnetization vectors of the targets and on the magnitude of the neutron's magnetic moment, which can therefore be determined from such data. Soon after the publication of Bloch's paper, a preliminary experimental trial of the double scattering method was performed by Bethe, Hoffman, and Livingston.[16] Their experiment had indeed registered an asymmetry in the scattered beam caused by rotating the magnetization vector of the analyzer, but the effect was too small to produce any reliable estimate for the magnitude of the magnetic moment.

Schwinger did not trust Bloch's calculations, which were based on the classical form of interaction between two magnetic dipoles. Julian had done somewhat similar work on Coulomb double scattering for his paper with Halpern [1], and now he decided also to recalculate the magnetic effect. He employed the techniques he had learned from Mott and Massey's *Theory of atomic collisions*,[17] and used an interaction Hamiltonian in which in addition to the term corresponding to the magnetic interaction between neutron's magnetic moment and electron spin, he also included the nucleonic potential at the position of the neutron. According to the current practice (which soon thereafter, also thanks to Schwinger's work, was found to be incorrect
p. 31 [13]), Julian considered it to be ↳ a potential of a central (spin-dependent) force of still unknown nature. Such a form of interaction was then known as Wigner's force, although Schwinger attributed it to Van Vleck.[18] The situation was difficult, because this unknown interaction was strong and had to be included exactly, while the better-known magnetic force was to be treated as a perturbation. However, Schwinger was still able to carry out the calculation because only the slowest thermal neutrons spend a long enough time in the proximity of the target atoms to experience any significant magnetic effects. He knew that thermal neutrons do not create metastable states with iron nuclei and, having zero orbital momentum, scatter on the nuclear potential in a spherically symmetric manner almost independently of the actual form of the Hamiltonian. For the cross-section for long-range magnetic scattering only the asymptotic forms of the wave-functions corresponding to the scattering by nuclear forces are important and, for the zero orbital momentum neutrons, the single most relevant parameter that characterizes that asymptotic behavior is the phase shift of the scattered S-wave with respect to the incident wave. It is linked to the overall strength of the nuclear force, and Schwinger was able to infer its magnitude from other neutron-scattering experiments.

This allowed Schwinger to proceed to the next step of the approximation, using the magnetic moment–spin interaction as a perturbation. This fully quantum-mechanical calculation produced an angular distribution and spin density of the elastically scattered neutron different from Bloch's. In addition to the classical term, which Bloch had correctly derived, it included a pure quantum term in the part dependent on the spin density of the incident radiation. Julian continued with a discussion of how to configure the experimental trials optimally. He applied his results to the scattering of polarized and unpolar-ized beams from ferromagnets. Then he analyzed the practical limitations of measuring the spatial distributions of neutrons double-scattered from two separate magnetized targets. The thoroughness of this analysis substantiated Rabi's opinion that Julian represented the ideal of a theorist from an experimentalist's viewpoint, one who was always willing and able to come up not only with a general analysis but also with 'a final number as an answer.'[2]

Schwinger found that 'the intensity of double scattering with parallel orientation of magnetizations [could be] 15 times that with antiparallel orientation. However, despite the large magnitude of the asymmetry, this effect will be difficult to detect with present methods because of the small intensity of the double-scattering neutrons.' Thus he proposed studying the induced polarization of the undeflected beam. If the transmitted beam was subsequently scattered, the experimenter could measure a polarization asymmetry defined as the 'difference in intensity between antiparallel and parallel orientations of magnetization divided by the
p. 32 average intensity.' The asymmetry, in certain ↳ configurations, could reach a value of more than 90%. He also proposed a double transmission experiment in which there was a compromise between having sufficient intensity transmitted and yet having a substantial polarization. Polarization asymmetries of about 40% were nevertheless achievable.

Although they were more feasible in yielding information than was double scattering, double transmission and transmission scattering were never successfully applied for the purpose of measuring the neutron's magnetic moment. Instead, subsequent experiments were based on resonance depolarization in neutron beams and were similar to the method used in Rabi's original molecular beam spin resonance apparatus, except that the neutrons passed not through Rabi's constant magnetic fields with opposite gradients but through ferromagnetic plates.[19] By then the more elaborate theory of magnetic interactions of neutrons had already come to exist, but Schwinger's calculation represented the first correct quantum-mechanical quantitative description of Bloch scattering.[20]

Hans Bethe was the referee of the paper, and, while praising it, suggested it be rewritten to emphasize the difference between the classical interaction between the dipoles, used by Bloch, and the correct Dirac treatment. He suggested that Schwinger was being too modest. The editors of the *Physical Review*, however, disregarded this advice, and the paper was published unchanged.[13]

Schwinger's next article in 1937 [4] appeared side-by-side with a related paper of Rabi on magnetically induced spin transitions in atomic beams.[21] Like the previous article on Bloch scattering, it contained a detailed and expanded calculation of an effect that had been previously analyzed semi-classically, this time by Rabi, who had studied the behavior of spin one-half atoms in a pre-cessing magnetic field.[22] A few attempts on the theory of this effect already existed, but they were very limited in scope, and Motz, who looked at Rabi's results, found a troublesome discrepancy between them and those obtained somewhat earlier by Guttinger[23] for the case of a rotating magnetic field. The discrepancy demanded immediate reconsideration with the help of rigorous quantum-mechanical procedures, and Rabi presented Julian with the task of performing it.

The underlying physics involved the classic problem of the evolution of a state coupled to a variable external field. If the transition between any two states of the field was rapid (as in the case of a sudden reversal of an external magnetic field), the dynamical state of the system would be unchanged. On the other hand, in the case of an adiabatic transition (such as the infinitesimally slow rotation of the same field by 180°), Ehrenfest's adiabatic theorem applies and the system follows the external changes continuously and gradually evolves from one energy eigenstate into another.

Being interested in the most general case of a time-dependent interaction, Schwinger considered the
p. 33 Schrödinger equation for a system with a ↳ Hamiltonian which involved no time-dependent variables except those associated with the external field. In such perturbative calculations one expands the wavefunction in a complete set of orthogonal energy eigenfunctions, treating the coefficients of expansion as time-dependent functions depending on the external field. Knowledge of these functions suffices for finding the transition amplitudes; however, the coefficients must be determined from the equations that expressly involve the energy eigenfunctions themselves. Therefore, in general, it is necessary first to solve the full dynamical problem and find the eigenfunctions, and only then proceed with the calculation of the lifetimes of individual energy levels.

In the simpler case of a magnetic field rotating with constant angular velocity, Guttinger had derived a set of equations for the coefficients that had the advantage of not involving the eigenfunctions, but only the energy eigenvalues, which in some cases could be inferred without the complete knowledge of the individual eigenfunctions. Starting from scratch in the general case, Schwinger recovered the Guttinger equation, but with an additional term which included the eigenfunction. Julian found a way of solving for this function in a general case and expressing this term by means of the angular momentum, magnetic quantum numbers, and the spherical components of the external magnetic field. This additional term happens to vanish for the transitions induced by a steadily rotating magnetic field; therefore Guttinger's results were correct for the case he considered, but not for the case of Rabi's precessing field. That is, only in the case when the magnetic field was perpendicular to the precession axis was Guttinger's result correct. This explained the discrepancy found by Motz, who had used the unmodified Guttinger equations outside the bounds of their applicability.

This paper was a precursor to Schwinger's later definitive work on the theory of angular momentum. As Schwinger noted, 'In fact, this was the origin of the work I did later about the general theory of angular momentum and so on [69]. But the whole interest in angular momentum goes back to these Rabi, molecular, atomic beam problems. And I'm sure this was done while I was still an undergraduate, or very soon thereafter.'[1] Norman Ramsey, then Rabi's graduate student, characterized the significance of the Rabi-Schwinger papers: 'They are the fundamental papers for nuclear magnetic resonance.'[8]

‘Because I, not my distinguished colleague, wrote it’

In the mid-1950s convincing evidence had emerged that nuclear forces were spin dependent. For example, neutron-proton binding forces were found to be much stronger than the forces between the neutrons or the
p. 34 protons themselves. Also, the exclusion principle ruled out any binding between pairs of neutrons or ↳ protons unless they had antiparallel spins so they could form only singlet bound states. No such restriction applied to neutron-proton pairs, yet no singlet states had been observed. In 1935, Gregory Breit and Eugene Wigner pointed out[24] that if one includes a singlet state in neutron-proton scattering processes then, in order to provide even crude agreement with experimental data, the singlet and triplet bound states must yield drastically different contributions to the total cross section. This could not be explained on the basis of existing models or small modifications of them.

As we have noted earlier, global effects of scattering of slow neutrons are well described by the phase shift of the scattered wave with respect to the incoming S-wave. The phase shift is a dynamical quantity dependent on the interaction potential. Since it is readily calculable and directly connected to the total cross section for S-wave scattering of neutrons it was used as a convenient tool in model testing, together with the Fermi scattering length, which is the radius of a sphere of surface area equal to the total cross section taken with a sign depending on that of the phase shift. At low energies, the relation between the phase shift δ, the energy E, and the scattering length a is

formula

(2.1)

m being the mass of the neutron.

The scattering length depends on the volume of the potential well, but is relatively insensitive to its shape. Therefore all initial attempts to adjust the form of the nuclear potential to achieve satisfactory accord with experimental data failed hopelessly. For example, Wigner’s calculation[25] with the use of rectangular well potentials produced low momentum neutron-proton cross sections of about two and a half barns,* while the experimental value was then thought to be about 13 barns.[26] Wigner suggested that there must also exist a singlet neutron-proton bound state, different from the triplet ground state of a deuteron.[27] It ought to have a very small binding energy but a very large scattering cross section at low energy. The total cross section would then be a sum of the cross sections due to the singlet state and those due to triplet states with statistical weights of one and three-quarters, respectively. However, the singlet binding energy revealed little about the nature of the binding potential. As a first step, the sign of the scattering length was needed because the sign of the ratio of the triplet to the singlet scattering lengths determined whether the singlet state was real or virtual, that is, whether the binding energy was positive or negative. No such information could be found from S-wave neutron scattering cross sections by protons in bulk matter.

p. 35 In 1936, Edward Teller remarked that, if nuclear forces are spin dependent, one should expect differences between the scattering cross sections in ortho-and parahydrogen,[28] which have parallel and antiparallel spins, respectively. He also noticed that since the waves scattered on two hydrogen nuclei in a molecule interfere, such scattering should provide information about things like the sign of the scattering length and the range of the n–p force.

Schwinger learned about Teller’s suggestion from Bethe’s review articles in the *Reviews of Modern Physics*.[14] He saw it as another opportunity to deploy his skill in calculations involving spins and started to compute the cross sections without hesitation. He progressed rapidly and soon he had some results to show to Rabi. Rabi suggested that he should go to Washington and discuss them directly with Teller, who was then at George Washington University. Teller was very interested in solving the problem of neutron scattering by molecular hydrogen, but apparently was not able to do the calculations by himself. He greatly welcomed help and invited Schwinger to come to Washington, and offered him a room to stay in his house.

Julian stayed with the Tellers for about two weeks, during which time he became timidly but intensely infatuated with the grace and enchanting accent of Teller’s Hungarian wife, Mitzi.[1] This unexpected relapse into adolescence did not take his mind away from the project, which he continued and completed, doing all the calculations by himself. Apparently Teller offered advice and critique, but did not contribute to the progress of the work. The preliminary results of this somewhat uneven cooperation soon appeared in a letter to the *Physical Review* [5], and a regular article on ‘The scattering of neutrons by ortho- and parahydrogen’ [8] followed shortly thereafter.* The Schwinger–Teller paper quickly inspired experiments and thus this was the first Schwinger article which became a standard textbook reference. Schwinger made no bones about whom the credit for this work should belong to. In 1979, a collection of his major articles was published[29] and in it he provided pithy, often one-line comments on these selected papers. The punch line included on the paper with Teller read: ‘Because I, not my distinguished colleague, wrote it.’

The article was written in the characteristic style of Schwinger’s early papers, in which the details of complex calculations were mixed with phenomeno-logical approximations based on generally scarce data, and which ended with the interpretation of possible results for future experiments. In the absence of an accepted theory of underlying forces, the calculation had to be essentially model-independent; thus, as in
p. 36 the case of Bloch scattering, it required neutrons ↳ of de Broglie wavelength large enough to be insensitive to the details of the spatial form of the nuclear potential. This restriction had an additional simplifying effect: Schwinger could calculate the coherent scattering cross-sections by simply summing up the scattering amplitudes from the two participating nuclei. Not having to worry about the radial dependence of the force, Schwinger treated the interaction potential as a contact interaction, vanishing unless the position of the proton and neutron, $\mathbf{r}_p$ and $\mathbf{r}_n$ coincide, and proportional to

formula

(2.2)

where Q is a spin operator constructed from the Pauli spin matrices of the proton and neutron and having the eigenvalue plus one in the triplet and minus one in the singlet state of proton-neutron system, and a_t and a_s are the scattering lengths for triplet and singlet spin states, respectively. The potential for coherent scattering was a sum of two terms of the type (2.2), one for each of the different hydrogen nuclei in a molecule. The final form of this sum turned out to contain two types of terms, one symmetric, the other antisymmetric in the proton spin. The antisymmetric part could induce transitions between the states of orbital quantum number differing by one unit, thus inducing conversions previously thought to be forbidden between the ortho- and parahydrogen. It was proportional to $a_t - a_s$ and even the very existence of such transitions, no matter how rare, would demonstrate a spin asymmetry of the nuclear interaction.

By treating the molecule as a quantum rigid rotator, and neutrons as plane waves normalized in a finite volume, Schwinger calculated the transition probabilities between the lowest energy levels of orbital angular momentum equal to zero or one, which were the only states that could significantly contribute to the total cross section at low temperatures. Experiments had to be performed at cryogenic temperatures so that neutrons had energies small compared with molecular rotational energy levels, which are different in ortho- and parahydrogen. With the rotational excitations eliminated, any difference in cross sections would have to be caused by the spin dependence of the interaction. The results confirmed Teller’s expectations: the cross sections for ortho- and parahydrogen were different for very slow neutrons; moreover, the difference between the two depended very strongly on the relative sign of a_t and a_s. The conclusion of the letter and the article was straight to the point. ‘(a) The orthoscattering cross section for liquid-air neutrons should be about 300 times the corresponding parascattering cross section. (b) The parascattering cross section for ordinary thermal neutrons should be roughly 100 times the parascattering cross section for liquid air neutrons. For a real singlet state, however, these ratios are of the order of one.’ Although not stated explicitly in the original article, but as may be easily inferred from the cross sections given there, in the
p. 37 limit of zero initial ↳ energy for the neutron there exists a simple relation between the cross sections (which are purely elastic in this limit)

formula

(2.3)

The approximate value of a_s, had already been found by Wigner, but now it became possible to determine its sign, since for a_t and a_s, having opposite signs, the difference $\sigma_{ortho} - \sigma_{para}$ would be much larger than in the case of identical signs. Naturally, the former alternative appeared to be more likely, as it implied that the as-yet unobserved singlet energy level of a deuteron was virtual.

The chances for successful experimental applications were excellent. Indeed, many experimenters rushed to do so, and the results of the first experiment by Otto Stern and his collaborators were even published before the appearance of the article of Schwinger and Teller.[30] They confirmed the suspected virtual nature of the singlet state.

Exploring the properties of neutrons

In 1937, having made the transition from being an undergraduate to a graduate student (a matter of pure technicality, since he had completed the entire graduate curriculum as an undergraduate), Julian Schwinger remained focused on the physics of neutrons, which was pursued aggressively by the research community at Columbia University. By then, Rabi began to realize that he had taught his protégé all he could. Therefore, he encouraged Julian to broaden his contacts and learn from new experiences by interacting with other physicists. Large numbers of interesting physicists came through Columbia, and Schwinger literally met all of them. With no more lectures to attend, he just did research; this was the goal he was aiming at and working for all along. He kept on lending his help to experimentalists and in the course of the next two years these collaborations proved to be fruitful; however, only a portion of all this work was ever published, often after a delay of several years. Some of it was presented as short communications at meetings of the American Physical Society, such as the one with Rabi on 'Depolarization by neutron-proton scattering' [6]. A conceptual descendant of the work with Teller, this paper discussed a method for determining the relative signs and magnitudes of singlet and triplet neutron–proton scattering lengths and described an alternative, viable, but not very practical, design of a suitable experiment in which the changes of the polarization vector of a neutron beam due to collisions with protons in a hydrogen-rich target would be used to determine the ratio of scattering lengths. The idea of such an experiment was Rabi's, while Schwinger derived the polarization formula which was the heart of this brief report: 'I just worked out the theory of it, which was two lines!'[1]

p. 38 At the same Spring 1937 Washington meeting of the American Physical Society, Hyman Goldsmith and John Manley, both from Columbia, spoke about their joint experiments on neutron absorption [7]. Manley was about to move to the University of Illinois, but at the same time he was still working with Rabi's group. He was a talented experimentalist with considerable experience in molecular beams, but his interests were just then turning to neutron physics. Manley teamed with Goldsmith, who had a good knowledge of virtually all the literature on this subject, and they enlisted Schwinger's help to carry out the computation and interpretation of the data. This work eventually grew into a longer article [10], which addressed the puzzling problem of selective energy absorption of slow neutrons which had been described about two years earlier in England and the United States.[31] The absorption of neutrons had all the characteristics of a resonance process. The absorption rates changed if the beam had been previously filtered through a thickness of the same material as the absorber; they seemed to be greater in a given element if the same element was also used as a detector of radiation. The discovery of these properties had led to the concept of neutron 'groups,' actually neutrons of separate bands of different kinetic energies, labelled by letters and characterized by the element which was their best absorber. However, the absorption process itself was poorly understood and the existing models involved large numbers of free parameters.

Manley, Goldsmith, and Schwinger explained the energy-selective absorption as a resonance capture in which a neutron and a nucleus create a virtual bound state. Quantum mechanics predicts that the cross section for such a capture by an individual nucleus is a bell-shaped (Lorentzian or Breit–Wigner) function of energy with width and height depending on the total width Γ of the bound state, the resonance energy E_0, and the energy E, formula

(2.4)

Γ, the full width of the cross section curve, is inversely related to the lifetime of the resonant state. The theory was to be tested on the known transmission curves for various thicknesses of rhodium, indium, and iridium, but the interpretation of these data was made complex by several factors such as the absorption taking place in bulk matter, the position of the resonance being affected by a Doppler shift due to recoil (which turned out to be negligible), and also because the angular distribution of resonance neutrons was unknown and had to be assumed.

A particular consequence of the resonance character of neutron absorption is the so-called 'self-reversal' of resonance lines. The energy-selective absorption process removes from the beam those neutrons whose
p. 39 energy is close to the resonance value, and most of them disappear from the beam within a ↳ small thickness of the absorber. A larger thickness is thus less effective in further reduction of the beam's intensity and the apparent absorption coefficient paradoxically appears as a rapidly decreasing function of the thickness of the absorber. The main achievement of Manley, Goldsmith, and Schwinger was the determination of the cross section for the capture at resonance and the width of the characteristic resonance (of rhodium) from such 'self-reversal' curves. A single resonance was sufficient to account for the activation of the 44 s half-life rhodium state, ^{104}Rh. In doing so, they arrived at the value of the scattering cross section (to be precise, at the effective absorption coefficient, which is proportional to the cross section at the resonance) that was 20 times larger than that obtained in 1936 by Fermi and Amaldi,[32] who had not yet recognized the importance of the self-reversal effects. However, since they defied the great authority of Fermi's school, the recognition of these correct results came only slowly.

On his own: a winter in Wisconsin

One day in 1937, Rabi had a conversation with Julian in which he said something like, 'Well, I have taught you everything I know. Why don't you go and study with other people?'[1] He suggested that Julian might go first to Madison, and work through the winter with Gregory Breit and Eugene Wigner at the University of Wisconsin. He arranged a traveling fellowship for Schwinger for one year: first to go to Wisconsin, and, maybe sometime in spring, he might go and work with J. Robert Oppenheimer's group at Berkeley. This was the Tyndall Fellowship, which Schwinger retained when he returned home to Columbia in the Fall of 1938. Before that trip, Schwinger, in the company of his college friend Joe Weinberg,* also went to Ann Arbor to attend the 1937 Summer School, which was then organized and run by Samuel Goudsmit and George Uhlenbeck at the University of Michigan. The activities at the school did not fully occupy Julian and left him enough time to learn to drive a car, courtesy of a friendly acquaintance.[1] Julian was fascinated by cars and eventually over the years even developed a strong affinity first for Cadillacs and then exotic sports cars, but
p. 40 for ↳ a while he had no opportunity to put this new interest into practice. Until he reached Berkeley he could not afford an automobile; before that, living in New York, he could comfortably get by without one.

Schwinger went to Madison for the fall semester and then stayed on there through the entire severe Wisconsin winter. As Van Vleck telegraphically noted later, 'Columbia is to be felicitated in giving Schwinger a traveling fellowship to Wisconsin in 1937 so that he could get a good education right after his doctorate [sic]. This was the golden year in theoretical physics in Madison with Schwinger, Wigner, and Breit all on campus at the same time.'[34] He had never before lived alone nor had to fend for himself for that long a period of time; he had always lived at home with only occasional excursions. In Madison, he settled in a small room in a boarding house which Gregory Breit found for him. He had arrived in Madison equipped with a trunk full of clothes and basic necessities which his concerned mother had chosen and packed for him. He still depended on his family for all his daily needs so completely that when the frigid winter weather set in he was freezing in his autumn clothes and suffered unnecessarily, totally unaware that there was a nice, warm winter coat waiting for him at the bottom of his only partially unpacked trunk.[1]

For Julian, the encounter with his new energetic hosts did not turn out to be as fertile as his interactions with Rabi. He had arrived in Madison with a specific research project in mind. 'During the fall of 1937 and all through 1938, I was thinking about tensor forces. I was certainly working on a field theory because the inspiration for the consideration of tensor forces came from field theory. I recall a paper written in 1937 by a fairly well known, but not famous, person who worked out a theory of spin-one particles. It could have been Nicholas Kemmer [certainly Schwinger had in mind Kemmer's articles on the "Nature of the nuclear field" and "Charge dependence of nuclear forces"[35]]; he wrote a paper in which he worked out his spin-one theory. So I read that paper and noticed the spin-orbit tensor forces, which I thought was very interesting. Why don't I incorporate them into the theory? I was a nuclear physicist fundamentally at that time, so I said to myself. "Why don't I see what effect the tensor forces have on nuclear physics?"'[1]

Thus Schwinger decided to try to incorporate non-central tensor and spin-orbit forces and see what their effect might be on the nuclear bound states and nucleon scattering. He found the atmosphere at the University of Wisconsin very pleasant, largely because he needed a temporary respite from analyzing data for experimental groups. Few people knew him at Madison and nobody expected anything in particular from him. Of course, it was anticipated that he would join his hosts in their research in some way. At that time, Breit and Wigner were completely absorbed in their work on the resonances in cross sections for the
p. 41 absorption of neutrons by nuclei. It was only a year since they had explained ↳ the shape of these resonances in cross sections by the famous Breit–Wigner formula (2.4).[36] Even though not particularly fascinated with the problems they were working on, after his experience with Manley and Goldsmith, Schwinger felt very confident in this area and was willing to join in. Unfortunately, to his horror, he found that the style of collaboration between Breit and Wigner relied on constant interaction, discussions, and excited conversations; in his shyness he perceived all this as 'constant giggling,'[1] which did not sit well with his own more private and concentrated method of working. He decided that he could not commit himself to their rules of engagement and, for fear of being controlled and pressured, he began to avoid encountering them. This was not at all difficult since both Breit and Wigner were day persons, while Julian worked best at night. He had already developed his favorite technique of avoiding unwanted interruptions by working late at night. Now Schwinger was free, with no obligations of student life, courses, examinations, and what at Columbia had merely been a preference in Wisconsin became a norm—he became a completely nocturnal person. He studied and worked in his room until dawn, then slept long, and did not interact with anybody until late afternoon. He still met some interesting people and learned from them a few things which broadened his horizons. The main influences upon him at that time were not Breit and Wigner but Julian Knipp, a theorist who later turned up at Purdue, and Robert Sachs, a young man just one year senior to him, and with whom Schwinger developed a lasting friendship. The two also collaborated in writing a joint paper on the magnetic moments of light nuclei immediately before the entry of the United States in World War II [32, 36].

Rabi later amusingly summarized Schwinger's year in Wisconsin. 'I thought that he had about had everything in Columbia that we could offer—by we, as theoretical physics is concerned, [I mean] me. So I got him this fellowship to go to Wisconsin, with the general idea that there were Breit and Wigner and they could carry on. It was a disastrous idea in one respect, because, before then, Julian was a regular guy. Present in the daytime. So I'd ask Julian (I'd see him from time to time) "How are you doing?" "Oh, fine, fine." "Getting anything out of Breit and Wigner?" "Oh yes, they're very good, very good." I asked them. They said, "We never see him." And this is my own theory—I've never checked it with Julian—that—there's one thing about Julian you all know—I think he's an even more quiet man than Dirac. He is not a fighter in any way. And I imagine his ideas and Wigner's and Breit's or their personalities did not agree. I don't fault him for this, but he's such a gentle soul, he avoided the battle by working at night. He got this idea of working nights—it's pure theory, it has nothing to do with the truth.'[6] But the theory seems validated.[1],[37]

Breit and Wigner let Rabi know that their contacts with Schwinger were minimal, although they could see
p. 42 that he was doing fine on his own. Schwinger was ↳ virtually invisible most of the time, but he gave up his plans to go to California, studied eagerly, showed up regularly at weekly seminars and himself gave four talks.[13] His first seminar, in October, was on neutron scattering in ortho- and parahydrogen; then in winter, he spoke twice on the magnetic scattering of electrons. Before returning home in May 1938, he gave one more talk, this time on deuteron reactions. Schwinger did not publish anything major during his stay at Madison. He studied field theory and made progress on several projects in nuclear physics, which he completed later on (sometimes with the help of others if extensive numerical computations were required).

Joseph Weinberg, it turned out, was also at Wisconsin that year, now as a graduate student. He was very unhappy working with Breit. Weinberg seldom saw Julian, although they occasionally double-dated, and recalled that Julian favored short girls, his own height. He noted that Julian was beginning to get interested in music, a passion of Weinberg, but exclusively in Mozart. Julian's interests were narrow, with no interest in history, or literature, or even biology.* He thought the work that Julian was doing in Madison on the deuteron was uninspiring. In any case, Julian was very reluctant to discuss what he was doing.[33]

Schwinger later recalled his work at Wisconsin leading to the prediction of a quadrupole moment for the deuteron, an outgrowth of his study of tensor forces. 'Well, I wasn't exactly inactive then. I was reading the literature. And there was a paper written by Kemmer which was on the then very primitive theory of the mesotron, explaining, looking into the kind of nuclear forces that would come out of that theory. This was in 1937. And among those forces was one that's quite familiar electrically, such as the force between two magnets, which depends on angles, and so I looked at this and I said, that's kind of interesting, nobody's thought about this in nuclear physics. What would it do? So I began in '37, kept on in '38, applied it to neutron-proton scattering, gradually got around to saying what would it do to the ground state of the deuteron and of course what it would do was produce a quadrupole moment. Now I came back to Columbia working on this, totally unaware that meanwhile at the same time they were busy discovering the quadrupole moment. So here in Columbia, independently, the theory, ready to receive the experiment, and experimental facts, and it all fitted together. In other words, things were just exploding.'[37] He presented his prediction of the quadrupole moment of the deuteron in a talk at the November 1938 meeting of the American Physical Society meeting in Chicago [13]. Rabi and Ramsey had already experimentally discovered that quadrupole moment, but let Schwinger present his result first. In an historic roundtable at which both
p. 43 Rabi and Ramsey were participants, Schwinger later ↳ stated, 'I went to give a paper at the November 1938 meeting in Chicago—the Physical Society—which was generally about the so-called tensor forces and I remember you came to me and said, are you going to talk about the quadrupole moment? I looked at you surprised. I didn't think you knew—and I said, yes, and then you didn't say anything, you walked away, and I didn't until later appreciate that in a way you were letting me scoop you—I didn't—because nobody paid any attention to it.'[37],*

The only paper bearing his Wisconsin address, and one in which he duly acknowledged his 'deep gratitude to Professors Breit and Wigner for the benefit of stimulating conversations on this and other subjects,' was a letter to the *Physical Review* 'On the spin of the neutron' [9]. This short paper contained the first quantitative analysis of the scattering data to support the hypothesis of neutrons being spin one-half particles. Previously this proposition could be supported only by arguments of simplicity because all data appeared to be equally consistent with the value of spin being one- or three-halves. This letter was an extension of Schwinger's earlier work on ortho- and parahydrogen [8] and followed it closely in all technical aspects. He recalculated the cross section for a transition between ortho- and para- states of molecular hydrogen assuming spin-$\frac{3}{2}$ neutrons. Such high-spin neutrons would produce quintet rather than singlet excited states with protons; the algebra of spin states would be different and would result in a different value for the ratio of cross sections, $\sigma_{\mathrm{ortho}}/\sigma_{\mathrm{para}}$, than in the case of spin-$\frac{1}{2}$ neutrons. The calculated value, of order unity, was in such discord with reality that it removed any doubts one might still have about the spin of the neutron.

The final year in graduate school

Julian Schwinger left Wisconsin and gladly returned home in the spring of 1938. The trip to Berkeley was postponed until after graduation and he could look forward to another year of complete freedom from outside pressure or obligations. Undistracted, he studied intensively and pursued a variety of fields and topics. He considered himself first and foremost a 'quantum mechanician' who completely devoured the works of Heisenberg, Pauli, and Dirac, all of whom he revered as gods and with whose creations he
p. 44 intellectually identified himself. He also developed a working interest in thermodynamics; the kinetic ↳ theory drew him into the study of relaxation phenomena of molecules and eventually to the propagation of sound and acoustic dispersion in gases.[1]

Schwinger pursued such interests only as sidelines of his main projects in nuclear physics. He always maintained a keen interest in experimental work and enjoyed the diversity of work on several concurrent projects. Rabi was stunned by Schwinger's surge of energy and was glad to see that after a year in seclusion at Madison, spent on purely theoretical studies, he again engaged himself in experimental collaboration. First of all, the article with Manley and Goldsmith on the width of nuclear energy levels had to be written up

[10]. The situation was somewhat complicated since the attempts to apply the method to another isotope, ^{115}In, were frustrated by inconclusive data [12], and Manley in the meantime had gone to the University of Illinois in Urbana. Goldsmith teamed up with Victor Cohen, another denizen of Columbia laboratories, who also worked on nuclear magnetic moments.

At the end of the decade of the 1930s, one of the most interesting experimental challenges was to devise techniques for measuring the magnitudes of neutron–proton scattering cross sections. The first attempts were undertaken as early as 1936 by Enrico Fermi. Fermi was the inventor of many practical methods which made it possible to analyze complex nuclear scattering data. The difficulty in measuring the scattering of neutrons off protons was that the latter were bound in a material such as paraffin. Fermi pointed out that in typical experiments with slow neutrons on paraffin targets, hydrogen nuclei in paraffin in general could not be treated as free protons unless neutrons have energies above the ground state vibrational level of the paraffin molecule.[39] This energy level is about 0.3 eV, which is roughly ten times the energy of thermal neutrons. Although it was possible to estimate the effects of binding, the results involved a high degree of uncertainty due to subtle factors, such as the effects of imperfect geometry of the beam and counters, the thickness of the scatterer, and the scattering of neutrons by carbon nuclei in paraffin. With still quite rudimentary experimental methods, which further added to the uncertainty, it was easier to use higher neutron energies, of the order of at least a few electronvolts. Although in this range of energies it was difficult to generate neutrons sufficiently homogeneous in energy and design selective detectors, the total cross section could be easily determined from the exponential drop of intensity of the beam as a function of the thickness of the scatterer.

Cohen, Goldsmith, and Schwinger achieved the equivalent of a monoen-ergetic source by utilizing the resonance levels for neutron absorption in the energy range of between one and ten electronvolts. Rhodium plates surrounded by cadmium that removed background thermal neutrons served both as absorbing filters and energy-selective detectors. Irradiated with neutrons, the detectors emitted secondary ionizing
p. 45 radiation which was subsequently ↳ measured with proportional Geiger-Müller counters. In this particular experiment, Schwinger's role was more than just providing theoretical support: he was truly dominant in all aspects of the work. The original suggestion to do the experiment came from him, and he designed it and fully participated in taking the data.[13] The experimental procedure was not very sophisticated; it consisted of essentially irradiating samples with neutrons from a radon-beryllium source and measuring secondary radiation, but it required much leg work since the experimenters did not have a laboratory and equipment of their own. The Geiger counters were located on the other side of the building from the neutron sources and rapidly decaying samples had to be rushed back and forth across the Pupin Laboratory building. (The separation was presumably necessary to avoid background radiation.) Most of this running took place from late evening until the middle of the night. Later, Schwinger's co-workers would retire but he, after taking a hearty meal, would return to his desk for several hours of quiet work and study. Hamermesh recalled that at the time 'I would work up at NYU or City College, come to Columbia around three o'clock, start doing calculations. Julian would appear sometime between four and six and we would have a meal which was my dinner and his breakfast, and then we would begin the evening's work, which was a strange combination of theoretical and experimental work. We were experimenters, if you can call us that. That is, we were capable of putting foils in front of a radon beryllium source and measuring transmissions through them and activations, like grabbing the foils, running down the hall of Pupin—it was on the top floor—running like crazy, putting the foil on a counter, and taking a reading. And then we would run back, put them up again, and start doing theoretical work. And we would work rather strange hours. It seemed to me that we would work usually to something like midnight or one a.m., and then go out and have a bite to eat. This would mean two or three hours during which I would get educated on some new subject. I learned group theory from Julian, and I must admit I forgot it all immediately, but as I recall, I had all of Wigner's book given to me, plus a lot more at the time and this was a regular process we went through and I think this must have gone on for a year or so and we started doing calculations of ortho-paradeuterium and on ortho-parahydrogen, scattering of neutrons, and this involved just an unbelievable amount of computation.'[12]

Feld was also involved in this experimental work. 'Probably when Morty and the other people working with him at Columbia had gotten pretty tired of running up and down the hall with the foils, I was recruited, as a sophomore then, to do the running. I guess Morty doesn't remember but I spent six months at Columbia doing the sprinting. I was a pretty good sprinter. I didn't know anything else but they were studying resonances in rhodium—I've forgotten what the mean life is now, but it's really very short [44s]. You had to
p. 46 take ↳ these foils and sprint the 40 yards from the irradiation to the Geiger counter and I was the fastest sprinter they could find. I was a real good sprinter then, so I made out real well. As a result of that I not only

got to hang around at Columbia at night but even when they went up to see Julian to consult on the theory or when something had gone wrong with the experiment or they got bored and just went up to talk with Julian, I was allowed to go with them, and so I got to listen.'[4] When all the measurements were done, Schwinger computed the proton-neutron cross section and obtained a magnitude of 20 barns, substantially larger than the then accepted value of 13 barns calculated earlier by Fermi and given in the Bethe 'bible.'[14] This result 'remained valid over the years'[29] and was cited as a benchmark value well into the 1950s [11]. The increase in the cross section affected the singlet neutron–proton interaction Schwinger had calculated with Teller [8]. The experiment was a diversion for Schwinger from several other undertakings, directed mostly towards a better understanding of the character of nuclear forces.

The contemporary theories of the neutron-proton interactions were based on the Schrödinger equation for a two-particle wavefunction depending on the coordinates and spins of participating nucleons. The potential energy was a function of the distance between the nucleons; in addition, there was an exchange operator, the action of which interchanged the variables within the wavefunction. There were four types of such operators, including the case of no exchange at all, known as the Wigner force. Another possibility was an operator that exchanged only the spins of the interacting nucleons, known as the Bartlett force. The Heisenberg force exchanged both spin and coordinate variables, and the fourth type of interaction, known as the Majorana force, took place by the exchange of coordinates alone. No single such exchange process was able to describe all the properties of the neutron-proton interaction. For example, under the coordinate-switching Majorana operator all eigenstates of odd angular momentum quantum number changed their signs, making the interaction angular-momentum dependent. On the other hand, the sign of the Bartlett potential alternates with increasing values of the total spin. These changes of sign make the force oscillate between being repulsive and attractive, which was against experimental evidence. Therefore it was believed that the interaction involved a mixture of all kinds of exchanges with the Heisenberg or Bartlett forces contributing to about one-quarter and the Wigner or Majorana forces to three-quarters of the total interaction potential.[40]

In order to find more about these forces, Schwinger decided to turn to more advanced applications of the interaction of neutrons on light nuclei. He still continued his friendship and collaboration with Lloyd Motz, and together they started to work on the interaction of thermal neutrons on deuterons. A letter and a
p. 47 *Physical Review* article [16, 17] appeared sometime later, in 1940, and was ↳ completed by an exchange of correspondence, for by then Schwinger had left Columbia for Berkeley. This work was interesting in certain respects: it again demonstrated that Schwinger had achieved maturity in handling extensive, complex calculations. It was mostly a computational piece of work, conceptually straightforward but very complex in execution. The point of departure was the interaction potential of the most general form which involved both position and spin exchange operators, assuming equal forces between both kinds of nucleons. The inclusion of polarizations would have been exceedingly difficult and cumbersome, so Schwinger and Motz decided to neglect them; they also replaced the deuteron's exact ground state wavefunction by a superposition of two Gaussian functions whose height and width were determined from graphical fitting. Despite these simplifications, the calculation was still a complex quantum three-body problem, the handling of which required considerable technical virtuosity. The challenge lay mostly in the ingenious mixing of approximations based on physical intuition with the mathematical methods of solving integral equations, so that the problem could be simplified enough to be reduced to a system of 20 linear algebraic equations solvable with the help of mechanical crank calculators. (They thank a Jerome Rothenstein for help on the numerical work.) Schwinger and Motz had access to the recent, still unpublished, accurate results of Dunning and his Columbia student Carroll. After comparing them with their own calculations they had no doubt that they agreed best with the mixture of the coordinate-exchanging Heisenberg and Majorana forces, without any admixture of Bartlett or Wigner interactions.

Schwinger also worked on similar subjects with Morton Hamermesh, his good friend with whom he had studied together and occasionally played chess in the past. As we have noted, they had also interspersed experimental work with their theoretical calculations. Together they generalized the Schwinger-Teller theory of scattering by ortho- and parahydrogen to the more complex case of deuterium and to a wider range of neutron energies. It was good phe-nomenological work, aimed at finding the cross sections for transitions from the ground state to other low-level states of ortho- and paradeuterium, which in conjunction with experiment could be useful for determination of the spin dependence of the nuclear force. This research was completed in 1939, and presented at the APS meeting at Columbia in February [14], but Schwinger's work on new projects delayed the publication of the detailed article. Hamermesh describes the agony of writing this paper vividly. 'Well, this work went on for a while and we got all these computations done, except that this was a period, as I recall it was around 1938, beginning of 1939, and I think Julian was getting ready to go off to Berkeley, and the paper was done and we were going to write it up and I looked upon this as my magnum opus. You know, I was going to be doing a thesis with Halpern, but who cared p. 48 about that. This was really great ↳ stuff. Then we started to write the paper.* The only trouble is that at this time Julian was already very much interested in the tensor forces and I remember very well helping him with some calculation involving the coupled differential equations that you get; [moreover,] I was a great reader of the literature and I was always telling him about interesting problems and unfortunately one day I mentioned the absorption of sound in gases and that started him off on an enormous amount of work which I don't think he ever published, as far as I can tell. But he did all sorts of calculations on this and there I was, trying to get him to write a paper and he's a rather finicky writer—maybe he isn't so finicky any more—but I can recall that there were only a few weeks before he was to leave and there was the paper and we were still in the first paragraph and every night we would start, we would write six or seven lines, and we wouldn't get it done, and here I could see the time slipping and I would go home and I would cuss hell out of him—to myself. And at one point I contemplated murdering him, but I didn't. He went off to Berkeley, paper not done.[12]

'The next time I saw Julian was at Cambridge. I came to the Harvard Radio Research Lab in '43 and Julian arrived there about the same time, at the radiation lab, and we saw each other and he said to me, well, you know, we really ought to write that paper. That's a great idea. It turned out, of course, he really had a point. He had found a very neat trick for reducing all this unbelievable amount of calculation that we had to do to what then amounted to four days of work, and so we did it all over again very, very quickly and the paper was finished in about two weeks, I think, of writing. He had improved his style by then and it was published I think in '46 [33] and another one in '47 [38]. Well, essentially what I'm trying to say is that I think I should claim that I'm Julian's first student. I believe I learned more from him than I learned from anybody else. In fact, I think he's the only one from whom I ever learned anything.'[12] Julian's incredible productivity always made it difficult for him to find time for polishing up the details and writing papers. On this occasion, the delay was extremely long because of the war; the paper, under the title 'The scattering of slow neutrons by ortho- and paradeuterium' [33], did not appear until the end of 1945, more than six years after the original calculations had been completed. Schwinger's attitude towards writing papers, to say it mildly, was rather hesitant. He was so full of ideas that he assigned low priority to putting finishing touches to essentially completed work. He also often felt he could improve the paper if he waited a bit to come up with a better idea p. 49 for doing the calculation more elegantly, which was certainly true in this case. Nothing illustrates Julian's ↳ attitude in these matters better than his work on the theory of nuclear tensor forces.

Recall that Schwinger developed the concept of tensor forces during his stay in Wisconsin. He was frustrated by the fact that the existing theory, while capable of providing reasonable agreement with the experimental data on nuclear binding energies or total cross sections, could do it only with persistent discrepancies. He hoped that the gap between the experiment and theory could be narrowed or eliminated with an admixture of yet another type of force. If, like all other fundamental forces, it were invariant under rotation and space inversion it could, in principle, be proportional to any even power of the product $(\sigma_i \cdot \mathbf{r})$ of spin and position operators. Here $\frac{1}{2}\sigma_i$ is the spin of the ith nucleon, and r is the relative position of the two nucleons. However, for spin one-half particles, all higher powers of this product reduce to the lowest order ones, leaving only two candidates [13, 22, 23, 24],

formula

(2.5)

where

formula

(2.6)

and where $\frac{1}{2}\tau$ is the isospin of the nucleon, with $\tau_z = \pm 1$ for the proton or neutron, respectively. Interactions not involving S_{12} were a linear combination of the conventional Majorana, Heisenberg, Wigner, and Bartlett forces described above.

Schwinger chose this particular linear combination in order to have zero spatial average over all directions in space. Despite its resemblance to the classical expression for the magnetic coupling between two magnetic dipoles of magnetic moment $\boldsymbol{\sigma}$, he expected that the strength of this new interaction must be characteristic of the other nuclear forces, submerging any corrections due to the electromagnetic spin coupling.

The introduction of the tensor force was Schwinger's first significant and truly original contribution to nuclear physics. It did not merely add yet another phe-nomenological term to obtain somewhat better agreement with experimental data; the tensor term had a profound effect on the symmetry properties of the distribution of nuclear matter inside neutron-proton bound states, and even changed the quantum number structure of nuclear energy levels.

Firstly, the ground state wavefunctions of two-particle bound states created by central forces are always spherically symmetric. This would preclude deuterons from having electric quadrupole moments. On the other hand, by their very nature tensor forces endow the deuteron with a non-zero quadrupole moment. In
p. 50 1938, Schwinger had not yet heard about any experimental indication in ↳ support of such a claim, in spite of the ongoing experiment in Rabi's group.[38] His prediction was made without any basis of quantitative information, and he could not yet even say whether the quadrupole moment was negative or positive. No wonder he was cautious and somewhat apprehensive about announcing the new idea publicly. When he went to the Chicago meeting in November 1938, he learned, to his astonishment, that Rabi's group was just at the same time discovering the quadrupole moment by using his molecular beam techniques, as described above. Soon afterwards Rabi's group indeed measured the quadrupole moment, consistent with the distribution of charge in the shape of a spheroid prolate 14% along the direction of the deuteron's spin axis.[42]

The second major departure from established theory was that while all central forces were invariant under rotations of space and spin coordinates separately, the Hamiltonian of the tensor force was not; it required a coupled rotation of space and spin reference frames. In other words, the Hamiltonian operator was invariant only under those rotations in which the observer's point of view turned simultaneously with the space coordinates.

Therefore, with central forces alone, the operators of orbital angular momentum and spin commute with the Hamiltonian and the quantum numbers of two nuclei comprise of the values L and 5 of the angular momentum and spin, and their respective projections m_L and m_S. Incidentally, these were the same quantum numbers as used in atomic spectroscopy, and the lives of early nuclear theorists were made easier because the language and many useful techniques of special functions developed for atomic physics were readily adaptable for new applications in nuclear physics.

Just as in atomic physics, the situation changes when spin-orbit forces are considered. With even the smallest admixture of a tensor interaction, the energy eigenstates of nuclei must be described by a different set of quantum numbers because the total angular momentum J = L + S, rather than L or S separately, commutes with the Hamiltonian. Although the eigenvalues J of the total angular momentum and its projection m_J still remain good quantum numbers, the total spin and its projection no longer do. In the case of deuterons, the Hamiltonian is symmetric in spin variables and the corresponding wavefunction is either symmetric or antisymmetric. This makes it possible to distinguish between the singlet and triplet states from the criterion of symmetry alone, which permits the use of total spin as a quantum number in this case. However, m_S is not available; in its place, the fourth variable necessary to provide a complete set of quantum numbers of a neutron-proton bound state proposed by Schwinger was parity, the eigenvalue of the space reflection operator. In consequence, the energy eigenstates were mixtures of wavefunctions corresponding to either even or odd values of L, since these transformed differently under reflections. One
p. 51 important consequence of that was that even the stable ground state of the ↳ deuteron was different; in the spectroscopic notation, it was a combination of the states $^3S_1$ and $^3D_1$, while in the absence of the tensor force it was a pure $^3S_1$ state.

In order to investigate the amount of the admixture of the tensor potential, Schwinger wanted to compute the ground state wavefunction of the deuteron, the cross sections for radiative capture of thermal neutrons, scattering of neutrons by protons, and an especially interesting process—the photodis-integration of deuterons—which would provide accurate information about the deuteron's binding energy. For this he needed precise solutions of the Schrödinger equation with the tensor potential method. Unfortunately, despite using the simple square well potential, he could not find analytical solutions even for the lowest energy states. He realized that the equations must be solved numerically by power series expansion. Schwinger had done numerical calculations before, but this time the task was overwhelming and it would take him away from fundamental research. Therefore he decided to wait and look around for someone more adept in this art than himself. He abandoned the largely finished work, made a preliminary announcement of it at the APS meeting in Chicago in November 1938 [13], as noted above, but eventually published the entire work only in 1941, sharing the credit with William Rarita, who had done the numerical calculations, while on leave (from Brooklyn College) at Berkeley. The articles became known as the famous Rarita-Schwinger papers [23, 24], which had considerable impact on the development of theoretical nuclear physics. (It is interesting to note that in [23] Schwinger again thanks Breit and Wigner for the benefit of stimulating discussions at Wisconsin, where he began the investigation. Of course, the presentation improved with the passage of time, and he thanks J. R. Oppenheimer and R. Serber as well.)

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Notes

* 'The rigidity of Corcoran's concerning the physics department's requirements was typical of the whole CCNY curriculum. There was an astoundingly large number of required courses outside the major, which just couldn't be avoided.'[5]

* We are grateful to Karl von Meyenn for bringing to our notice a complete photocopy of Bethe's letter to Rabi.

* It was a dull course with a dull exam. A question on the final exam was 'Prove that $d\varepsilon = d\xi + d\eta$,' where none of the variables ε, ξ, or η) were defined.[8]

† Phi Beta Kappa is the most honored academic fraternity of young American students, to which they are elected by their peers and seniors entirely on the basis of academic excellence.

‡ K. K. Darrow, secretary of the Physical Society, who apparently had little appreciation of theory, always scheduled the theoretical sessions in the smallest room. Schwinger's second lecture was given in the largest lecture hall in Pupin Lab, and the third in the largest theatre on campus.[8]

* A barn, an originally facetious term referring to its unexpected largeness, is 10^{24} cm^2.

* It is interesting to note that the abstract of the article was quite long, a habit Schwinger often cultivated, and is nearly identical to the entire letter submitted two months previously.

* Weinberg recalled that Julian received graduate credit at Columbia for attending the Summer Symposium at Ann Arbor, Michigan. So Weinberg, who was not yet a graduate student, approached Uhlenbeck to request graduate credit as well. To support his petition, he showed him a manuscript he had written on weak interactions. Uhlenbeck glanced at it, said it was 'impossible' because it violated parity—after all, Michigan was the home of Laporte, of Laporte's Rule fame—and unceremoniously discarded the paper in the wastebasket. Julian stayed in the AT house with the lecturers, for example, Fermi and Uhlenbeck,[33] while Weinberg, feeling a less exalted status, stayed in the graduate dormitory.

* In Berkeley he later asked Weinberg 'why Oppy was interested in so many things.'

* If Schwinger had remained at Columbia during the winter of 1937-38, he might have known of the discovery of the quadrupole moment of the deuteron earlier. But since Schwinger was back in Columbia during the fall and winter of 1938, it is surprising he did not receive a hint of the experimental result.[8] However, in the abstract for the November 1938 meeting Rabi's group only claimed an anomaly. The existence of a quadrupole moment was only asserted in print in early 1939.[38]

* Elsewhere Hamermesh recalled, 'I remember that at one point when we were trying to write up our result for publication we worked steadily for several days with little sleep. We went to a seminar of Fermi's and both fell asleep during the whole seminar.'[41]