# Rigden, John S.: Rabi's Resonance Method

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The magnetic resonance method was born in the fall of 1937; however, it was in the making for many years. We shall begin in 1931.

How is the feeble glimmer of a star measured against the lustrous brilliance of the Sun? How is the Sun's brightness kept from swamping the faint light of a star? These questions are analogous to the one that faced Rabi early in 1931. He wanted to measure the magnetic moment of a nucleus in the way that Stern had measured the magnetic moment of a silver atom. The problem was that an atom has a magnetic moment about 2000-times larger than that of a nucleus. The question, How can the minute nuclear moment be measured in the face of the swamping effect of the large atomic moment? was answered by the Breit–Rabi theory in 1931.

In the Stern–Gerlach experiment, a strong magnetic field is used to deflect the moving beam particles. This *strong* field acts directly on both atomic and nuclear magnetic moments, and 99.95% of the resulting deflection is due to the large atomic moment. There is virtually no chance of identifying that 0.05% contributed by the nucleus. However, Breit and Rabi showed that if a *weak* magnetic field is used, the tiny nuclear moment is effectively joined with the large atomic moment so as to affect distinctively the pattern of the deflected beam. For example, a strong magnetic field, acting on the large atomic moment, would split a beam of hydrogen atoms into *two* subbeamlets; on the other hand, a weak magnetic field, acting on the atomic moment coupled with the nuclear moment, would split the beam into *four* subbeamlets. Thus, in a weak deflecting field, the nuclear magnetic moment exhibits itself in a definite way. It was on the basis of this theory that Rabi and Cohen began to design and to build a state-of-the-art molecular-beam system in 1931.

The physics department at Columbia was content with Rabi's return to molecular beams. No one hindered him, but neither was there robust support. Financial backing was meager. Anyone, however, interested in physical results rather than splashy equipment, and willing to work at taking data rather than demanding the latest piece of hardware to ease the burden, could do a lot of physics—especially in those days—on a shoestring. As in his high-school days when he built a radio station out of parts scavenged from the streets, alleys, and back lots of Brooklyn, Rabi and Cohen assembled a simple Stern–Gerlach deflection system. This was the first step in the evolution of the magnetic resonance method.

In the simple deflection method, a beam of atoms or molecules passes between the poles of one magnet. These poles are shaped in such a way as to produce a magnetic field stronger at one pole than at the other. This varying, or inhomogeneous, field exerts a force on the magnetic moment of a beam particle as it passes between the pole faces. The beam is long and narrow—a fraction of a millimeter in thickness. The magnetic deflecting force acts at right angles to the beam particle's velocity so as to add to its straight-ahead velocity a small sideways velocity. As a result, the beam particles strike the detector at a position that is slightly shifted, either to one side or the other, from the center position of the undeflected beam.

The principal difficulty with the simple deflection method is that the particles in the beam move with a whole range of velocities. The average velocity of the beam particles depends on the temperature of the source from which they emanate. Most particles have velocities that are near the average velocity; like any quantity of a statistical nature, however, other velocities can be smaller or larger than the average value. Since slow particles are deflected more than fast ones, the deflected beam is not a sharp replica of the undeflected beam, but is smeared out. To interpret the smeared-out deflection pattern, one must make theoretical and experimental assumptions and do extensive analysis—tasks that suited neither Rabi's disposition nor his taste.

A second difficulty with the simple deflection method is that the magnetic-field inhomogeneity must be known: that is, the magnetic-field strength must be calibrated point-by-point. Such calibrations are not only irksome but also impossible to do with great precision. Thus, there is an inherent uncertainty in the strength of the deflecting field, resulting in an inherent uncertainty in any deflection that is measured.

And, most important, the simple deflection method did not allow Rabi to have his answer 'by the end of the day'.

Rabi wondered what combination of forces could be exerted on the beam atoms so that they would reveal their secrets more directly. How could the featureless smear of the deflection pattern be transformed into a pattern with features that conveyed the nuclear spin of sodium just by looking? 'My attitude towards physics ... has always been profound faith and profound skepticism. Profound faith and skepticism. I really felt I wanted to be convinced of nuclear spin. It sounded all right, the theory [of quantum mechanics] explained a whole lot, but I knew enough about the history of physics to know that a good explanation is not necessarily so.'

Rabi relied strongly on his intuition, which allowed him, in a manner of speaking, to put himself into the beam and, along with the other beam particles, experience the sudden jolts and subtle nudges as he streamed through the apparatus. As Polykarp Kusch has said, 'He [Rabi] appears to ride around on the electrons within an atom or asks the question, "If I were an electron, what would I do?" Possibly, through sheer force of character he gets the electron to do precisely that.'[1]

So, Rabi rode the sodium atom, first by clinging to an electron, then by sitting on its nucleus. He could feel the beam split decisively into two beamlets by the force of a strong magnetic field acting directly on the large magnetic moment of the sodium atom's outermost electron. If he could now block out one of these beamlets and thus allow only one to continue on its way into a *weak* and *reversed* magnetic field—weak, so that the field would gently separate the beamlet into still smaller beamlets by acting on the combination of atomic and nuclear magnetic moments, and reversed so that the separated beamlets would be sharpened by collapsing the spread resulting from the difference in deflections between the fast and the slow atoms as they passed through the first field—then he would need another strong (and also reversed) field to bring the whole pattern of separated and sharpened beamlets back toward the center of the apparatus where the individual beamlets could be detected. If the nuclear spin were $I$, $(2I + 1)$ beamlets should be detected.

Rabi discussed his ideas with Cohen, and the two men started to work on them. Additional deflection fields were added to the system: the first split the beam into two parts; the second subdivided the beam still further, depending on the value of the nuclear spin; the third shifted the spread-out beamlets back toward the center of the apparatus for detection. Between the first and second deflecting fields, a slit was added which allowed only one beamlet to continue through the apparatus.

Rabi's ideas were sound, but their implementation raised new problems. He and Cohen started with a beam containing a relatively few atoms, half of which were thrown away, and the remaining half further subdivided. How does one detect the rarefied presence of each little beamlet? A new detection system would have to be developed. One by one, solutions to other problems were found.

One morning, Victor Cohen, unable to wait for the elevator to take him to the fifth floor of Pupin Hall, bounded up the steps two or three at a time. Earlier that morning, he had found the nuclear spin of sodium, and now he wanted to show the world. He went straight to the laboratory and threw the switches that brought life to his apparatus. As the source oven slowly heated to its operating temperature, Cohen checked the pressure gauges. The pressure readings were low, indicating a good working vacuum. By this time, a few other laboratory workers had wandered in, and soon Rabi made his appearance. The timing was perfect, and Cohen called them all over. A steady flow of sodium atoms left the hot oven; half of them wended their way through the three deflecting fields. Systematically, Bill Cohen slowly moved the detector wire across the beam: the signal first increased and then decreased—a signal peak—one beamlet. Cohen continued the slow movement of the detector wire. The signal increased again and fell again—a second beamlet. A third time, the detector signal increased and fell—evidence of a third beamlet. Still Cohen moved the detector wire, and a fourth time the signal increased and fell—a fourth beamlet. How long would this go on? Cohen steadily moved the detector wire as Rabi watched the signal monitor. Would there be another beamlet? Rabi watched. The monitor registered nothing—nothing—nothing. Cohen stopped. The drama was over, but the excitement was about to begin. There were four signal peaks, four beamlets, which meant that the nuclear spin of sodium is $\frac{3}{2}$.

To this day, the sodium result obtained with Cohen stands as one of Rabi's most satisfying experiments:

> The world was young and I was young and the experiment was beautiful. It satisfied everything I wanted to see. There was an artistry in it or whatever it is called ... it just charmed me. These atoms in spatially quantized states, analyze them in one field, turn your focus back, and there it is. Count them! It was wonderful. There I really, I really believed in the spin, there are the states, count them! Each one, I suppose, seeks God in his own way.

To manipulate a beam of atoms in such a finely controlled fashion as to bring the atoms in one distinct quantum state onto the detector was the first big step toward the resonance method. The importance of this step was twofold: first, it infused the members of Rabi's team with the confidence that they could control the motion of beam particles in subtle ways; second, this first step, though spectacular, was limited. Counting the peaks gave the size of the nuclear spin, but they wanted to measure other nuclear properties as well, properties such as magnetic moments.

One Saturday afternoon in May 1933, Rabi called into his office Sidney Millman, a graduate student who had just passed the departmental qualifying examination and was therefore about to begin his own dissertation research. Rabi invited Millman to join his molecular-beam group and suggested a thesis topic: the determination of the nuclear spin and the magnetic moment of potassium. Rabi talked to Millman about Cohen's work then in progress and suggested that the 'zero-moment' method that Cohen was going to use for the study of cesium might also be applied to potassium. 'This was all new to me and sounded quite exciting,' Millman recalled later. 'I agreed right there and then to start my research.'[2] He was shown an area in Cohen's laboratory on the fifth floor of Pupin Hall and there began to build his own molecular-beam system.

By the time Millman started his dissertation research, the Rabi laboratory was a pulsating hive of activity. A second laboratory on the 10th floor of Pupin had been started with its own cast of young physicists—mostly postdoctoral fellows. At any time of the day or night, on any day of the week, the lights in the Rabi labs would be aglow and someone would be there working.

The zero-moment method was another experimental technique motivated by Rabi's need for simplicity, his desire for an immediate answer. It had style. The zero-moment method revealed properties of the atomic nucleus with an immediacy whose impressiveness was challenged only by its clarity. The method is based directly on Breit–Rabi theory which shows that, for specific values of the deflecting magnetic field, the *effective* magnetic moment is zero (the *actual* magnetic moment, a property of the nucleus itself, is not zero). This means that, for specific sizes of the deflecting field, all beam particles, regardless of their velocities, will be undeflected (because their effective moment is zero).

Millman could begin an experiment with the deflecting field turned off entirely and line up the detector to receive the full strength of the undeflected beam. The detector signal would, of course, be strong. Then he would turn on the deflecting field and slowly increase its strength. As beam particles were deflected, the detector signal decreased. Then, as the strength of the deflecting field was slowly increased, the detector signal would rise and fall—a peak—indicating that some particles were passing straight through the magnetic field without being deflected. The process was continued until no more peaks appeared. From the number of peaks and from their relative spacing, the nuclear spin could be determined. For his dissertation, Millman studied the alkali metal lithium.

The zero-moment method provided not only nuclear spins but also nuclear magnetic moments. The rendering of these data, however, was neither experimentally nor theoretically straightforward. To begin with, one particular magnetic field strength that allowed particles to pass through undeflected had to be known. In other words, one calibration was necessary (the field inhomogeneity was *not* needed). Even this one calibration, however, proved unnecessary when a new method was developed to generate magnetic fields.

Jerrold Zacharias, a Rabi postdoctoral fellow, doubted that the field between the iron pole faces of a magnet could ever be accurately calibrated. The outcome of his skepticism was a different method for generating a deflecting magnetic field—namely, by electrical currents, which produce magnetic fields. From the geometry of a current, the magnetic field can be calculated directly, and the error-riddled calibration avoided altogether. The trick was to know the geometry accurately.

With Sam Cooey, Rabi's master machinist, the ideas and sketches were translated into hardware. With the smallest of tolerances, a jig was machined to hold two wires in a parallel configuration. The separation of the two wires was known precisely. When these two wires carried an electrical current of known magnitude, the properties of the deflecting field could be calculated directly. Millman, and other 5th floor students, used the two-wire deflecting field and the zero-moment method to determine both the spins and the magnetic moments of several alkali metal nuclei.

The finances of most universities, including Columbia, were strained during the Depression years of the early 1930s, and Rabi got little financial support from the university for his research. When he really needed a piece of equipment, he would go to Dean Pegram and present a carefully reasoned explanation for what he wanted. About this time, however, Harold Urey received a grant of $7600 from the Carnegie Foundation in honor of his discovery of deuterium. (Urey, a faculty colleague of Rabi's from the chemistry department, would win the 1934 Nobel Prize in Chemistry.)

> Well [Rabi later recalled], Urey did one of the most extraordinary things imaginable. He gave me half of it. I had nothing to do with his discovery. What a greatness in Harold Urey—what a tremendous magnanimity to do something like that!
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> He had a deep faith in me ... He told somebody, referring to me, "That man is going to win the Nobel Prize." I don't know what he saw in me ... but what a tremendous magnanimity! That money set me free. It made me independent of the physics department.[3]

While Rabi could 'see' specific results or 'feel' the outcome of an experiment, to explain in detail to Dean Pegram was very difficult: 'Besides, Pegram was a wonderful guy, extremely intelligent, and he'd have ideas, too. Next thing I knew we were collaborating, and I wasn't interested in collaboration. All I wanted out of him was money, admiration, and encouragement. With Urey's money I was free. I was able to do things on my own.'

And Rabi needed to be free. His laboratory was growing in people, flourishing in ideas, and expanding in experimental methods. A new method, however, did not necessarily render obsolete an older method. A case in point is the refocusing method developed during 1935. The zero-moment method had one major drawback: it could not be applied to atoms whose nuclei had a spin of $\frac{1}{2}$. The refocusing method could be applied to all atoms. There were advantages with both methods, and both methods continued to be used concurrently. As a consequence, it was not possible to cannibalize old equipment and use the parts in the new apparatus. For the refocusing method, new hardware was required, and new hardware required money. With Urey's money Rabi was free to expand his experimental base and to do so on his own.

Whether with sledgehammer techniques or with graceful, teasing subtlety, the experimentalist cannot get something from Nature without a price. The refocusing method allowed the vexing problem of the beam-particle velocity distribution to be artfully sidestepped. The outcome of this method, as with the zero-moment method, did not depend on the speeds of the beam particles. The cost of this independence was that the researcher did need to know the magnetic field inhomogeneity.

In the refocusing method, two deflecting magnets were used. Atoms were deflected once, then once again in the opposite direction, so that in the end they arrived where they would have arrived had there been no deflection at all. Slow beam atoms were deflected more than fast ones by each magnet; but in the end, all were refocused into the detector.

The experimental procedure was beautifully simple. Beam particles went through two deflecting fields in sequence—the first called the A-field which pushed them one way, then the B-field which pulled them the opposite way. With both deflecting fields turned off, the detector's location was adjusted so as to register a strong signal and thereby indicate that it was receiving the undeflected beam. Then the B-field was activated. The B-field split the beam into two major subbeams. One subbeam missed the detector to the right, while the other missed it to the left. The detector signal was essentially zero.

Next, the A-field was activated. Slowly the deflecting field was increased in strength from zero to ever larger values. Atoms in the quantum state identified with the largest effective magnetic moment were the first to be refocused into the detector. The detector signal rose, then fell. Quantum state by quantum state, beamlets of atoms were slowly swept over the detector as the attendant rise and fall of the detector level signaled their passing. From the number of peaks detected and from a knowledge of the magnetic field characteristics, both the nuclear spin and the magnetic moment of an atom could be determined.

Nuclei have intrinsic spins (angular momentum) and magnetic moments. The magnetic moment has both a magnitude and a sign. In 1935, the sign was missing. The sign of a magnetic moment can be either plus or minus: if the spin and the magnetic moment have the same space-quantized direction, the sign of the moment is plus; if these directions are opposed, the sign is minus. The signs of nuclear magnetic moments became important in the mid-1930s as a result of the work on the hydrogens being carried out in the 10th floor laboratory by Rabi, Zacharias, and Jerome (Jerry) M. B. Kellogg, another postdoctoral fellow.

The effect of the signs on their data was subtle—so subtle, in fact, that there was no effect at all. As beamlets of a particular atom were refocused into the detector, the same pattern was observed regardless of whether the sign of the atom's moment was plus or minus. The problem of determining the signs was something like trying to determine whether someone's right hand or left hand is pushing the front-door buzzer.

In 1936, Rabi wrote a theoretical paper (one of the few he ever wrote by himself) in which he analyzed experiments that had been done in Stern's Hamburg laboratory in 1932 and 1933.[4] The purpose of Stern's experiment had been to answer a question that went back to the days of the old quantum theory, the days when the idea of space quantization strained credulity. The question was, can an atom that is 'clinging' to a magnetic field with some particular space-quantized orientation be shaken loose? Can an atom be made to change its orientation?

Putting the question to the potassium atom, Stern and his associates had sent atoms streaming through a magnetic field in which they would take on a specific spatial orientation. Then Stern played a trick: he arranged for the atoms to leave the magnetic field they were in, and suddenly enter another field—a field whose direction changed from the one the atoms were used to, to a new direction. The question could be asked, would the atoms, upon entering the field with the new spatial direction, stubbornly maintain their original orientation, or would they follow the direction of the changing field?

The Stern group determined that, when the direction of the magnetic field is changed quickly enough, the atoms on passing from one field to another will reorient. It was in this reorientation process that Rabi saw the possibility of determining the signs of nuclear magnetic moments. He described this idea in his 1936 paper. The idea was Rabi's; the work to implement it fell to his students.

In their 10th floor laboratory, Kellogg and Zacharias dismantled the molecular-beam apparatus, stretched it out a bit so that a new magnetic field—the 'T-field'—could be inserted between the two deflecting fields. The T-field was a strange one, its configuration giving it a treelike appearance. The direction of the field went up the trunk and then fanned out to right and left like tree limbs. A beam particle entered the T-field moving *against* the field (from the tips of the limbs in toward the trunk of the tree); and, on departure, the particle moved *with* the field (from the trunk out toward the ends of the limbs). As a particle moved rapidly through the T-field, it 'saw' a magnetic field change quickly from one direction to another; it 'saw' a field that appeared to rotate.

Rabi recognized that if this apparent rotation was synchronized with the precession rate of the magnetic moment (the Larmor frequency), the T-field would exert a tipping force on the magnetic moment and make it flop from one orientation to another. These reorientations would open the way for determining the signs of nuclear magnetic moments.

Kellogg and Zacharias made another addition to the apparatus. They added a barrier between the first deflecting field and the T-field. With this barrier, one of the subbeams coming out of the first deflecting field was stopped before it entered the T-field. By observing the changes in the signal level of the detector as *preselected* beamlets were allowed to pass through the T-field, one could infer the signs of the magnetic moments. It all worked: the T-field did its job.

What Kellogg and Zacharias did on the 10th floor, Henry Torrey, another Rabi graduate student, did on the 5th: he built a refocusing system complete with T-field. There he, along with Millman and Zacharias, determined the signs of the nuclear magnetic moments for the alkali metals.

The T-field was the last significant modification made to Rabi's apparatus before the advent of the magnetic resonance method. The refocusing method together with the T-field provided a provocative experimental milieu. With deflecting magnets laid out along the flight paths of the beam atoms, Rabi and his crew learned how to exert a fine sense of control over beams of particles. They learned where and how barriers and gates could be set up to allow only select beamlets to pass on to the second deflection stage and, from there, to the detector. They learned detection techniques and the idiosyncrasies of various deflection systems. They learned how to make atoms flip from one space-quantized orientation to another and to detect a reorientation. The fingers playing over the controls of the apparatus had a feel for beams. Everything was poised for the next step.

Concern with experimental accuracy and precision occupied the thoughts of Rabi and his students during the 1930s. They had started with the simple Stern–Gerlach experiment with uncertainties of at least 10%. The zero-moment method was the best of all and, when it worked, it worked well. The refocusing method produced results with uncertainties of about 5%. The best of their experimental results prior to the resonance method carried uncertainties of about 3%.

The uncertainties associated with these results were not altogether due to the design of either the apparatus or the experiments. Limits were also established by physical theory. Neither the zero-moment method nor the refocusing method yielded the value of the magnetic moment directly; rather, the quantity directly measured is the energy difference between two quantum states. To get from this number to the value for the magnetic moment, one had to use theory—theory that was only approximate. The magnetic resonance method inaugurated a new era of precision: the experimental and theoretical difficulties that limited the precision of earlier results no longer existed.

The idea in Rabi's 1936 paper, the one that led to the determination of the signs of magnetic moments, came to him as he walked up the hill from his home on Riverside Drive toward the campus:

> One day I was walking up the hill on Claremont Avenue and I was thinking about it [the sign of the nuclear magnetic moment] kinesthetically with my body. Now, yes, I was thinking about this as follows: here's the moment and it's wobbling around in the direction of the field and [to find] the sign was to find out in which sense it was wobbling. To do this, I have to add another field which goes with it or against it. This is the idea, just concretely. The whole resonance method goes back to this.

His intuition was sound, and atoms did reorient in such a way that the signs of their magnetic moment could be determined. But Rabi's 1936 paper was essentially qualitative, and later that year he set out to extend his earlier ideas and to develop them into a quantitative theory of atomic reorientations. His theory was published in 1937.

This paper, entitled 'Space Quantization in a Gyrating Magnetic Field,' presented a theory that became the basis for the magnetic resonance method.[5] After World War II, this was the paper cited independently by both the Harvard physicist Edward Purcell and the Stanford physicist Felix Bloch in their papers announcing the discovery of nuclear magnetic resonance (NMR) in bulk matter. Today, more than 50 years later, this paper is cited by laser physicists who use Rabi's 'flopping formula', derived in the 1937 paper, thus showing how a great paper can be applicable far beyond the immediate intentions of its author.

In this paper, Rabi derived an expression that gives the probability that an atom will be reoriented by a magnetic field that changes direction (gyrates). By using the T-field, Kellogg, Millman, Zacharias, and other physicists had been observing these reorientations. Rabi wanted both to know more exactly the conditions conducive for reorientations and also to extend the range of applicability of the general method.

As an atom moves through the T-field, it experiences a changing magnetic field. More specifically, the atom 'sees' a magnetic field—first in one direction, then in another. From the atom's point of view, the magnetic field appears to rotate. Rabi's 1937 paper showed that the critical factor is the frequency of this apparent rotation.

The frequency of the apparent rotation is significant because of a second frequency. When an atom with a magnetic moment is placed in a magnetic field, its motion is like that of a spinning toy top. The axis of a spinning top precesses (rotates) around the vertical direction. As the spinning top slows, the precessional circles get bigger and bigger until the top topples to the floor. Here the analogy breaks down because the spin of atoms is intrinsic and 'never slows down'. But the spin axis of an atom does precess around the direction defined by the magnetic field. Furthermore, this precession occurs with a specific frequency—the 'Larmor frequency'—that depends on the magnetic moment of the particular atom and on the strength of the magnetic field. Hence, Rabi's result. As an atom goes through the T-field, it 'sees' a rotating field—a field rotating with a specific frequency. If that frequency is equal to the atom's Larmor frequency, then the probability for reorientation is relatively large. On the other hand, if that frequency is either much below or much above the atom's Larmor frequency, the probability for reorientation is small.

Rabi's 1937 paper, completed in February and published in April, is the theoretical basis for the magnetic resonance method. The steps needed to implement the method were simple: the T-field had to be replaced with an oscillating magnetic field embedded in a uniform magnetic field. Zacharias remembers sitting in Rabi's office discussing the fact that 'if you apply a radiofrequency [that is, a magnetic field oscillating with a radio frequency], you can make it flop.' [6] After the basic ideas for this method were in place, however, implementation of the resonance method did not begin for over seven months.

Several factors contributed to the delay. First, there was, in a sense, a backlog of work to finish: namely, determining the signs of the magnetic moments that had been determined earlier; for this task the T-field method was adequate. Second, the T-field method appealed to Rabi: 'It had things in it—splitting the beam, counting peaks, getting the signs of moments.' Third, there was no pressing reason to rush: 'There was this very happy condition that nobody was competing with us. It was such a wonderful period.'

Rabi's happy condition was challenged in September—a challenge answered by the implementation of the resonance method. The Columbia Physics Department and Rabi's laboratory were a natural stopping-off place for European physicists visiting the United States. In September 1937, C. J. Gorter from the University of Groningen in Holland was such a visitor. The previous year, he had unsuccessfully attempted to observe a magnetic resonance effect in a solid sample of lithium fluoride. When visiting Columbia, Gorter, according to Rabi, said, 'Why aren't you doing it this way?' meaning, 'Why aren't you using an oscillating field?' Rabi acknowledges that, 'Gorter's visit was a stimulus. I knew about his work; in fact, he didn't tell us anything we didn't know. But he asked me, "Why aren't you doing it this way?" Well, I liked what we were doing, but I saw that he might go after it and we might get some competition. So I said, "Let's do it." Gorter's visit stimulated me into saying, "It's time to do it the other way."'

Gorter visited on a Saturday. On Monday morning, two days later, the pulsating vacuum pumps connected to Millman's apparatus were shut down, and modifications were started.

The ranks of Rabi's corps of postdoctoral fellows was expanded in September of 1937 by the arrival of Polykarp Kusch and, on that Monday morning, he joined Millman (who by that time had received his Ph.D. degree) and Zacharias in an all-out effort to modify Millman's apparatus so that it could be used for the resonance method. They started with a basic refocusing molecular-beam system. Between the two deflecting magnets that provided the A- and B-deflecting fields, a third magnet was placed. The pole faces of this magnet were flat so that a uniform field, the C-field, was established between them. Embedded in the C-field was a wire loop shaped like a bent hairpin. The loop was connected to a radiofrequency oscillator that sent a radiofrequency current through it. As particles of the beam passed between the sides of the hairpin loop, they were subjected to an oscillating magnetic field—a field that could change the orientation of precessing atoms.

The modifications did not take a long time. With Rabi, Kusch, Zacharias, Kellogg, and other physicists looking on, Sidney Millman made a final adjustment to the collimating slits so that the detector registered the presence of an intense beam. Molecules of lithium chloride were streaming unmolested through the apparatus. Millman turned on the B-field which deflected the beam molecules out of the detector. The detector signal vanished. Millman turned on the A-field and slowly increased its deflecting strength. Everyone watched the detector, waiting for its signal to rise as molecules were refocused back into the mouth of the detector. Molecules left the first deflecting field with a particular spatial orientation. They left the second deflecting field with the *same* spatial orientation and were refocused into the detector. When the detector signal reached full-beam level, everything was ready.

This was the moment. There was no sound except for the pounding vacuum pumps. Millman set the frequency of the oscillator at $3.518 \times 10^6$ c/s. His hand moved to the rheostat controlling the current in the electromagnet—the magnet that produced the C-field. Millman steadily turned the rheostat and watched the ammeter that registered the current in the windings of the electromagnet. The current read 110 A. The beam detector was recording a full-beam intensity.

Millman continued to increase the current—111 A—112 A. Rabi's eyes darted back and forth between the ammeter and the beam detector. The full beam was still arriving at the detector: 113 A—full beam. No one spoke. They no longer heard the rhythmic throb of the pumps: 114 A—the level dropped a lot; 116 A—the bottom fell out of the signal level. Much of the beam was no longer being refocused into the detector. There were glints of anticipated excitement in all the eyes fixed on the detector. Millman, with steady hand, continued turning the rheostat: 117 A—the beam was returning to the detector, and the signal level jumped up dramatically; 118 A—jumped up some more; 119 A—back to full-beam strength; 120 A—no change. The watchers stirred restlessly, breaking the tension; 121 A—still no change. The tension broke entirely as cheers filled the lab and reverberated down the corridor.

'Rabi was beside himself,' Kusch said later.[7] Backs were thumped and hands were shaken. For the first time ever, a nuclear magnetic resonance absorption had been recorded. The nucleus was lithium.

What happened as Millman increased the strength of the C-field? Recall that molecules left the first deflecting field with a particular space-quantized orientation. As they moved through the oscillating field, nothing happened until the current reached

115 A. This current produced a C-field in which the Larmor precession frequency of the lithium atoms almost matched the frequency of the oscillating field ($3.518 \times 10^6$ c/s); thus, some of the lithium atoms flopped to a new orientation and were not refocused into the detector by the second deflecting magnet. At a current of 116 A, the match was almost exact, and many, many lithium atoms flopped from one orientation to another and missed the detector. It was at this point that the bottom fell out of the signal level. At 119 and 120 A, the Larmor frequency was no longer in resonance with the frequency of the oscillating field, and the probability was slight for a reorientation; hence, they were all refocused into the detector, and the signal was once again large.

Helen and Rabi threw a party that night, and graduate students and postdoctoral fellows came to celebrate. Everyone was exuberant, especially Rabi who circulated in high excitement among the members of his team. Rising above the noise of many simultaneous conversations, a frequent sound could be heard: Rabi's high-pitched laugh.

Although even at that time, January 1938, it was likely that the events of the day might be crowned with a Nobel Prize, no one could have foreseen how important the magnetic resonance method would prove to be—for not only physics but also chemistry, biology, medicine, and the whole of science.

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## Biographical Sketch

John S. Rigden. *b* 1934. B.S., 1956, Eastern Nazarene College, Ph.D., 1960, Johns Hopkins University, USA. Postdoctoral research, Harvard University where he published an NMR paper with John D. Baldeschwieler. Faculty member in physics at Eastern Nazarene College, Middlebury College, and the University of Missouri–St. Louis; Director of Physics Programs, American Institute of Physics, 1987–present. Approximately 170 publications. Research specialties: molecular physics, history of physics, physics education.