Instrument Makers and Discipline Builders: The Case of Nuclear Magnetic Resonance

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Crucial to the establishment of a scientific discipline is a body of knowledge organized around a set of instruments, interpretive techniques, and regimes of training in their application. In this paper, we trace the involvement of scientists and engineers at Varian Associates in the development of nuclear magnetic resonance (NMR) spectrometers from the first demonstrations of the NMR phenomenon in 1946 to the definitive takeoff of NMR as a chemical discipline by the mid-1960s. We examine the role of Varian scientists in constructing several models of NMR instruments for research scientists in the 1950s and the Varian efforts to influence the adoption of NMR as a standard tool for chemical analysis through Varian-supported publications, participation in scientific meetings, collaborations with academic chemists, workshops, and postdoctoral fellowships. Special attention is devoted to the development of the Varian A-60, the first commercial NMR instrument intended for the broadly trained chemist rather than a custom-built tool for the research specialist. Drawing on an examination of the use-rate of NMR instruments in chemical literature, the assessment of NMR in the chemical review literature by practitioners, and indicators of the establishment of a suitable funding environment for the growth of physics-based scientific instrumentation in the post-Sputnik era, we argue that the establishment of NMR as a discipline coincided with the adoption of the A-60 in the mid-1960s. During the period of our study, Varian Associates was a

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primary military contractor. This paper is intended to contribute to recent interest in the relation of military funding to scientific enterprise during the Cold War and to the larger question of university-industry interactions in the growth of knowledge.

Introduction: Disciplines and Networks of Innovation

Historians of industrial research have provided ample evidence for rewriting the standard narrative about science as produced solely by persons in academic institutions and consumed by industry scientists and engineers as applied academic knowledge. Numerous studies of Bell Labs, General Electric, and the various Kaiser-Wilhelm-Institutes have emphasized the contributions to basic science by scientists working in industry (Hoddeson 1977, 1980, 1981; Russo 1981; LaPorte 1983; Reich 1983, 1985, 1987; Wise 1983; Feldman 1990; Johnson, Jeffrey A., 1990; Kline 1992). The history of chemistry and pharmaceuticals is rife with examples of the mutual stimulation of basic scientific research by academic and industrial laboratories as well as of the ambitions of industrial chemists to contribute to the growth of fundamental scientific knowledge and even the construction of new scientific disciplines (Rossiter 1975; Servos 1976, 1980; Hounshell 1980; Latour [1984] 1988; Liebenau 1984, 1985, 1987; Hounshell and Smith 1985, 1988; Schling-Brodersen 1989, 1992; Munday 1991; Jones, Paul R., 1993). Recent work in several areas of concern to social and economic studies of science and technology adds further support to the notion that we reconsider the entire spectrum of traditional assumptions regarding university-industry relations in the production of knowledge and new technology. Gone from recent studies, for example, is the traditional distinction between "pure," basic or fundamental, and applied science. In light of the multidisciplinary character of most contemporary scientific work and the highly instrumentalized character of most fundamental science, distinctions between basic and applied are increasingly difficult to draw in many fields (Rosenberg 1991; Rosenberg and Nelson 1993). Gone too is the linear model of innovation and development that characterized earlier accounts, replaced by the notion of innovation as a distributed process, which incorporates the user as adaptor and modifier of technology, in short as coparticipant in the process of innovation rather than as passive recipient of "black-boxed" technology (Latour 1987; Hippel 1988; Lundvall 1988; Imai 1992; Trajtenberg 1990). This notion of innovation as a distributed social process is further supported by recent social studies and economic studies of the organizational structures required to capture, support, and manage innovation at both the firm and industry level. One of the prominent features of recent studies of innovation is an emphasis on the role of local cultures, communities of practice, and situated knowledge in the generation and accumulation of innovations within organizations (Goodman and Associates 1988; Brown, Collins, and Duguid 1989; Brown and Duguid 1991). Rather than thinking of groups as bounded entities that lie within an organization and its view of tasks, this perspective, more productively, takes groups to be fluid and interpenetrative, often crossing the restrictive boundaries of the organization to incorporate people from outside. The focus in these innovation studies on interactive distributed networks of exchange meshes well with recent work on science regions, particularly the importance of horizontal, open yet highly competitive networks of information exchange and knowledge spillover in high-tech regions (Blume 1992, pp. 38–73; Kargon, Leslie, and Schoenberger 1992; Jaffe, Henderson, and Trajtenberg 1993; Scott 1993; Saxenian 1994). Several recent studies have noted the importance of shared culture and values in successful examples of innovative knowledge and technology transfer. High-technology innovation requires experimentation and learning and the rapid combination and recombination of local skill and knowledge. Saxenian (1994) has argued that flexible forms of organization that encourage the interdependent linking of firms and public and private educational and training institutions in active social networks rather than the vertically integrated corporate forms of an earlier industrial era are particularly conducive to the growth of high-tech industries that are dependent on rapid, continuous innovation. The mere agglomeration of self-sufficient independent units in the same area is insufficient to encourage innovation and adaptation in situations of change; geographical proximity must be supplemented by social structures and institutions that encourage the sharing of information, mutual trust, and collaboration.

This diverse array of work suggests we consider the production of knowledge relevant to technoscience networks in terms of a model of university-industry relations that extends knowledge production well beyond the walls of the university. Rather than treating university-industry relations in terms of bounded, sharply delimited organizations, it may be fruitful to think of universities as participating in a situated knowledge community and, in effect, to treat the disciplinary structure of the university as part of a regional knowledge economy. Indeed, Bruno Latour and Michel Callon have raised this principle to constitutive status in their "actor-network" theory (Callon 1987; Latour 1987), an approach that gains empirical support from—among other studies—Erich von Hippel's work on innovation. Von Hipple points out, for instance, that innovations in industrial products frequently lie outside the organization and among its suppliers or customers (von Hippel 1988). We might apply this insight more generally to knowledge communities: sources of innovation for knowledge work within the university might profitably be sought among its customers or suppliers. Focusing on the specific case of nuclear magnetic resonance (NMR), a field of physics research that came into existence after World War II, this paper traces its transformation by the mid-1960s into a discipline of analytical chemistry. Our goal with this example is to analyze the role of university-industry relationships in the generation of new technology and the role of the industrial partner in the creation of new regimes of practice at the core of young scientific disciplines. The emergence of a new discipline is typically associated with a new body of scientific theory, together with a collection of applications, research tools, and problem-solving methods. While acknowledging that disciplines serve and are composed of diverse practitioner communities, including academic, clinical, industrial, and governmental clienteles, studies of scientific disciplines focus mostly on aspects of institutionalization rather than on the construction of scientific and technical practice. In typical analyses topics such as the construction of a scientific society, a journal, and the creation of academic institutions for career training take precedence, which may result in an overrepresentation of academics in the processes of knowledge production and discipline building. But if we consider that most crucially, a new discipline is invariably linked with a new discipline-specific instrumentarium and training in its use and interpretation, then we might do well to consider the role of the industrial partner, particularly the manufacturer of instruments, in this process. The focus on the role of instrument makers in the process of discipline formation was explored in 1987 in a pioneering article by Yakov Rabkin on the development of infrared (IR) spectrometry, but Rabkin's call for more studies of instruments in discipline formation has not been followed (Rabkin 1987; Bud and Cozzens 1992; Hankins and van Helden 1994) Indeed, while departing from some of his theses, our own study supports many of Rabkin's claims, particularly his central claim concerning the role of instrument companies like Perkin-Elmer and Beckman Instruments in disseminating IR techniques to university chemists (Sturchio and Thackray 1988*a*, 1988*b*).

In this paper, we examine the early history of the development of NMR in Stanford University laboratories and at Varian Associates of Palo Alto. Our findings suggest a more active role on the side of the commercial company in constituting the object of scientific research as well as the techniques, skills, and standards of interpretive practice that come to constitute the core of the discipline as practiced by academics. Since its primary business is supplying an academic market, Varian, or any scientific instrument company, necessarily engages in close interactions with that community as part of its business. This case highlights the diverse range of creative science carried out by scientists working in industry and their participation in the construction of a public good at the same time they seek to generate proprietary knowledge and market advantage. Insights from such cases may suggest ways to illuminate the relations more generally between the university and other industry sectors.

The Culture of Innovation: The Stanford Experience and the Formation of Varian Associates

Varian Associates was founded in 1948 by, among others, the Varian brothers Russell and Sigurd, William Hansen, Edward Ginzton, and Leonard Schiff, who was then head of the Stanford Physics Department (Lowood 1987). These men came together to form a physics and engineering research company on the borders of Stanford partly as a response to impediments to their research agendas in the postwar environment at the university. In launching the commercial venture of Varian Associates, they sought to create a working environment offering both reliable funding and opportunity to pursue research and development interests in classified military technologies that could not be conducted in an open university environment.

The idea of encouraging startup companies to develop in the vicinity of Stanford had been pursued by Frederick Terman before the war, his encouragement of William Hewlett and David Packard being the most celebrated example. But the wartime experience of Frederick Terman, dean of engineering at Stanford from 1946–55 and provost from 1955–65, figures importantly here. Terman was not a founding member of Varian Associates, but, as Edward Ginzton recalled, "[Terman] was a mentor of a number of his former students, a partner in the formation of the company and a very able and wise counsel" (Ginzton 1990, p. 27). Terman eventually replaced Dorothy Varian as a member of Varian's board of directors. As director of the Radio Research Lab devoted to developing radar counter measures, located at Harvard during World War II, Terman, himself an engineer, coordinated a large staff of research physicists and had responsibility for turning the research of the lab into devices, overseeing their manufacture at Bell Labs, GE, RCA, and Westinghouse, and seeing that military personnel were instructed in their use. This experience convinced Terman of the importance and desirability of research eventually leading to a product. In writing of his time in the Radio Research Lab, he noted that he had been impressed by the amount of work required to take a device from working model to one ready for manufacture (Leslie 1993, p. 54). As Terman prepared to return to Stanford, he was convinced that in order to continue the productive cooperation of industries, universities and government after the war it would be necessary to give engineers educations more closely matching those of physicists. "Most major advances," he said, "were made by physicists and people of that type of training rather than the engineers"(Leslie 1993, p. 54). As one means to this end, Terman envisioned a new microwave laboratory on the campus that would bring physicists and engineers together.

The wartime experience was definitive in several respects for establishing collaborations between engineers and physicists at Stanford after the war. A persistent concern both before and after the war was the search for financial means to support an increasingly expensive and expanding physics laboratory. Indeed, even alliances between physicists and engineers at Stanford were driven in part by financial and equipment needs, a point that became clear during the war to Felix Bloch, a Stanford nuclear physicist with a theoretical bent, and William Hansen, Bloch's colleague at Stanford who was more strongly oriented toward applied physics than was Bloch. As the following letter from Hansen (working during the war at Sperry Gyroscope in Garden City, New Jersey) to Bloch indicates, some Stanford physicists saw value in Terman's vision of a future union of engineers and physicists in a microwave lab, though for different reasons. In contemplating how to organize their work after the war, Hansen wrote:

> There are obvious reasons for the close connections between any microwave laboratory and the E.E. Dep't. . . . An additional reason for Terman's entering the picture that you might not be aware of, is that he got the University to set aside money for a new radio lab, and it may be we could usefully tie onto this non-existent but promised building. This is especially so since the scheme outlined in [another physics colleague, Paul Kirkpatrick's] letter seems feasible and definite except for one thing—where do we put the stuff? I would like it in or near the Physics Building, but if this is impossible, we may have to look elsewhere. (Hansen 1942)

Hansen turned to the Stanford microwave group's equipment needs again at the end of the letter:

> At M.I.T., the government is accumulating a fantastic collection of micro-wave equipment. No one knows what will become of it, but as a guess I would say that a university with (a) a "Microwave

> Laboratory" and (b) if possible, some spare cash, would have a good chance of getting a lot of good stuff almost free. This is an argument for starting such a lab, and if possible, saving some money along about two or three years from now—whenever one guesses the war will end. (Hansen 1942)

Another persistent concern of the physicists, implicit in their ruminations about future collaborations with engineers, was the construction of a compatible research environment in which they were free to define and pursue their own interests. Since the very beginnings of their work in microwaves, matching these two criteria—funding and autonomy—proved to be complicated, in some ways impossible (Galison, Hevly, and Lowen 1992). From the late 1930s on, the group had experimented with several support arrangements. Pursuing Hansen's earlier work on the rhumbatron, Hansen and Russell Varian, a research associate in the Physics Department, had invented the klystron tube in 1937, and Sigurd Varian, who lacked a college degree but boasted considerable skill in electronics, turned their designs into a working model. This new device was potentially patentable, and Hansen, in the interest of securing more research money for his laboratory, had indicated as much to the university. During the years before the war, a collection of superb physicists and engineers in microwave physics had been assembled at Stanford, including Hansen, Ginzton, Bloch, Russell Varian, and David Webster, then chair of the Physics Department. To support the research of the microwave group, a bargain was struck with Sperry Gyroscope for the development and manufacture of the klystron. This arrangement proved only semiworkable: when Hansen wished to pursue a novel, more elegant klystron model, Sperry management insisted that he stick to a model that would lead to speedier development. The alliance with a commercial company interested in establishing its patent situation had the uncomfortable outcome of directing the physicists' work away from potentially interesting theoretical directions they wished to pursue.

For some members of the group, the wartime experience with Sperry Gyroscope proved to be more productive than their prewar experiences had led them to expect it would be. Edward Ginzton, for instance, recalled the time at Sperry as very productive. The laboratories were beautiful and well equipped. "Nobody told us what to do. . . . We were fully aware of military needs, and as a result of this participation in the military program, we made proposals to the government, and the government would grant us money with which we could do whatever was agreed upon. Sperry as a corporation never participated in guiding this work at all. We were on our own. As the war progressed, it was obvious that Sperry would be a fine place to work at for the rest of our lives " (Ginzton 1990, pp. 26–27). At the close of the war Ginzton visited Sperry president Preston Bassett to inquire about the possibility of staying on with the company. Ginzton, like his other Stanford colleagues, was not interested in staying in Long Island, however, but in returning to California. Would Sperry consider building a plant in Palo Alto? The answer turned out to be negative. Determined to return to the San Francisco Bay area, Ginzton, Hansen, and the Varians in 1943 formed a plan to construct a company that would be a small laboratory providing a source of employment for the microwave group. Initially their strategy was to stay clear of klystron work, for which, in spite of their own expertise, their combined capital of $22,000 was insufficient to compete with Sperry and other firms with immense resources poised to manufacture klystrons in high volume. "All we felt we would do was to get a laboratory started, and since we were smart enough to invent the klystron before, we would be smart enough to do something else again. Anything would do as long as it provided a source of a living" (Ginzton 1990, p. 27).

Everyone returned to Stanford desiring to recreate the institutional arrangements they had experienced during the war. Ginzton, Hansen, and the Varians wanted a well-equipped microwave measurement and instrumentation lab similar to what they had constructed at Sperry in Long Island, in which a combination of government and industry funding would support basic unrestricted research aimed at technologies with military and commercial application; Terman, for his part, returned to Stanford impressed by the "high class operation" he had observed at MIT (Terman n.d., p. 138). A common pattern emerges in the ideas about the postwar organization of scientific and engineering work of individuals we have studied who participated in the Manhattan Project or in the Radio Research Lab. Although we have no evidence that they were directly influenced by Vannevar Bush's vision for the postwar organization of science, the views of Terman, Ginzton, Varian, and others are remarkably consonant with positions outlined by Bush in his *Science—the Endless Frontier* and in the appendices to his report of July 25, 1945, to Franklin D. Roosevelt. Bush was insistent on the point that war related research had produced new scientific and technical knowledge that should be transferred to universities and industry for generating new jobs and stimulating industrial growth (Bush [1945] 1990, p. 8; Kevles 1977; Reingold 1987). Bush's report also advocated the establishment of contracts and fellowships for long-range military research to be conducted by civilians working in industry and in universities. The report emphasized that it was "essential that both kinds of research go forward and that there be the closest liaison between the two groups"(Bush [1945] 1990, p. 34). The Radio Research Lab and the Sperry labs were exemplars of Bush's vision of a cooperative flow of information between university and industry researchers working on projects of both military and commercial significance. In many ways the company envisioned by Ginzton, Hansen, and Varian embodied Bush's ideas, as did Terman's vision of the microelectronics lab linked to a proto-"industrial homestead" just off campus at Stanford.

Although they had been unhappy with the Sperry arrangement for supporting their early klystron work, in the immediate postwar period, before clear channels for government funding became available from the Office of Naval Research (ONR), and after 1951, the National Science Foundation, Stanford physicists continued to explore the prospects of generating research funds through the licensing of patents on their work through the university. Felix Bloch seems not to have been initially interested in the commercial possibilities of his work in NMR, for instance, but he did explore the possibilities of patenting it with university president Donald Tresidder. Perceiving the potential of the patent for generating revenues for his laboratory, Bloch wrote to Tresidder in February 1946, two months after the initial discovery of NMR:

> Our recent discovery of the nuclear induction effect has brought to mind two problems which I should like to discuss with you.
>
> The first problem is that of insuring ample funds so that our work can be planned for and proceed as efficiently as possible. Through personal acquaintance with some of the wealthy Jewish families of the Peninsula it would be easy for me to approach the Chairman of the Rosenberg and the Columbia Foundation in San Francisco. I have had a preliminary discussion with Mr. Daniel Koshland from San Mateo who has promised to pave the way for me. I should like to talk with you about the advisability of my taking these or similar steps.
>
> The second problem is that of patenting our discovery. The great simplicity of the technique makes it quite possible to be applicable in various fields and it might be in the University's interest to take a patent. (Bloch 1946*a*)

In the case of the nuclear induction patent, unlike the klystron previously, university officials apparently did not feel NMR would be potentially profitable, for they did not pursue Bloch's suggestion, nor is there evidence that Bloch continued to persuade them otherwise. Instead, Bloch followed Tresidder's suggestion of applying to the Research Corporation for support (Bloch 1945; Seidel 1992, pp. 26–27, p. 39; Leslie 1993, p. 139, p. 146). An additional—and ultimately primary—source of support for Bloch's lab was the ONR. As soon as the ONR got into the support of postwar research in 1947, Frederick Terman worked out proposals with Ralph Krause, a Navy liaison Terman had known from the Radio Research Lab, to support Bloch's research and work in electronics at Stanford (Terman n.d., p. 139).[1]

The prospect of government funding for postwar physics seemed to resolve some of these difficulties, particularly for Hansen, who until his death in 1949 was able to pursue his interests in klystrons and accelerator design. Not everyone in the physics department, however, had research interests that bridged microwaves, accelerators, and nuclear physics so smoothly. To insure its investment in the linear accelerator would be used in support of nuclear physics, the ONR imposed the requirement after Hansen's death that the lab make greater commitments of personnel to nuclear physics, a move that threatened the microwave group's ability to choose its own research problems. The physicists and engineers in the lab working on microwaves wanted a relatively autonomous mandate, to pursue a spectrum of problems in microwave research much in the spirit of the exciting concentration of physics and engineering effort at the Radio Lab during the war. Similar to Terman's model of the relations between the microwave lab and its clientele, some of the group's research would be of military significance, other research would contribute to building larger linear accelerators for high-energy nuclear-physics work, and still other research interests would be in areas irrelevant to military concerns but of potential importance for medicine and industry (Hansen 1943). Moreover, they wanted the flow of information and exchange of ideas among these different orientations to be as unrestricted as possible, in the spirit of the traditional, open academic institutional structure. The group basically wanted to create a sort of disciplinary hybrid between engineering and physics, working on accelerators and klystrons for military radars to generate funds to support their microwave research generally. But the ONR's requirement for its continued support of the linear accelerator was that the lab and the Physics Department devote

1. Terman recalled that Bloch "did not like it. I had to coax him to take the money from the Navy."

themselves to research that would ensure the use of the government-sponsored device, and this meant essentially committing the department to high-energy physics research. With the construction of the Mark III accelerator completed in 1952, physics careers in the lab would have to be devoted to nuclear physics. Microwave physicists, such as Ginzton and Marvin Chodorow, whose work by 1952 on the linear-accelerator project was more or less complete, could either direct their research more toward nuclear physics or go on and develop more powerful klystrons for future accelerators (Hansen 1943, p. 63). But in order to pursue broad ranging research across the entire field of microwave devices as they wished to do required departing from the model of unrestricted flow of information envisioned by Terman and others and instead creating a laboratory environment with access limited to persons with military security clearance. Essentially this meant moving that part of their research off campus.

This context is directly relevant to the initial foundation of Varian Associates in 1948 and to the early recruitment of scientists and engineers to the company in the formative years between 1948–53. It is also, as we shall argue, indirectly relevant for the formation of NMR as a chemical discipline. As we see it, the founding imperative was not simply to follow Terman's suggestions and possibly his encouragement to pursue commercial development of laboratory discoveries; the goals of Varian's first generation, including the original founders and those who joined them shortly as "associates"—men such as Martin Packard, Elliott Levinthal, Emery Rogers, James Shoolery, and others—were equally, perhaps more importantly, connected to lifestyle choices and to concerns about constructing an environment for pursuing their own scientific and technological interests. Crucial as their science was to a number of areas, the field they were developing was caught somewhere between the "big physics" of the postwar era and the small-scale laboratory physics of the pre-war period. The areas of science and engineering Hansen, Ginzton, Varian, and Chodorow were interested in developing were too expensive to be pursued in a university context without major external governmental funding support; but in the postwar period funding for their area of physics research was becoming tied to high-energy physics and physics research with military importance. They were interested in pursuing the development of technologies of military significance, such as new generations of more powerful klystrons and traveling-wave tubes, but they were also intent on developing technologies of industrial import that did not fall within the purview of a lab devoted to nuclear physics and accelerators alone.

Ginzton, for instance, was interested in pursuing the application of klystrons to the construction of a medical accelerator. At a time when they were seeking autonomy for their own microwave research, the funding opportunities available through government-sponsored research in a university setting were forcing the microwave physicists at Stanford into ancillary, almost service, roles where they did not compose their own agendas. In a surprising reversal of what we normally mean by "autonomous research," the formation of Varian Associates on April 26, 1948, and the successful conclusion to negotiations for security clearances enabling members of the company to develop the R-1 klystron for a missile guidance system in September 1948 provided the solution to the constraints these microwave physicists and engineers encountered in the university environment (Ginzton 1990, p. 32).

Essential to a fruitful interaction between a university laboratory and a commercial lab is a shared culture. As we shall see, it was precisely the fact that Varian Associates cultivated a research culture that was in many ways an extension of the university that they were able to achieve such success in the development and rapid adoption of NMR instrumentation as a standard tool for structural chemistry. Ironically, the founders of Varian Associates sought to create a new form of academic culture, one that would avoid the drawbacks of the academic culture in which they worked, where their autonomy was increasingly limited by the absorption of klystron work in an area of fundamental research that was primarily supportive of nuclear physics.

The idea of forming a company had been discussed by Hansen, Ginzton, and Russell Varian during the war (Varian, Dorothy, 1983, pp. 230–33), and the developments after the war reinforced their resolve. Of course, the individual founders and early members of Varian had diverse interests, and we have not succeeded in unearthing all their motives, nor is it our concern to reduce them to a single formula. Few persons connected with the development of NMR at Varian were clearly motivated by an opportunity for financial gain. Until the early 1950s, there was little reason to believe the technique would be of use to anyone other than nuclear physicists interested in determining magnetic moments of nuclei. Felix Bloch was not a founding member of the company, but he clearly supported the new venture. As the company grew, Bloch became more closely involved, eventually becoming a paid consultant to the company. Bloch was not demonstrably interested in financial gain in the beginnings of his work with NMR, but he became increasingly so in the following years. By the mid-1950s he regarded the research division of Varian as in many ways a necessary extension of his own domain (Bloch 1955*a*).[2] Many of his students were among the original scientific staff and managers of the company. For these individuals, work on NMR at Varian Associates represented an unprecedented opportunity. Academic appointments to continue work in this field beyond the stage of doctoral studies and research assistantships were virtually nonexistent. The Varian venture presented the opportunity to continue to do basic research in an as yet unrecognized area with a group of talented and similarly motivated colleagues, in a laboratory outfitted with the most sophisticated instrumentation available. Moreover, most of the Bloch students were native Californians finishing student careers and interested in starting families (Anderson 1994, p. 3).[3] The Varian venture offered the chance to remain in attractive and familiar surroundings. The weather mattered.

Russell Varian's idealism was undoubtedly the strongest source of

2. When Bloch returned to Stanford from his brief sojourn as director of CERN in 1955, he was particularly concerned to have positions for both Weston Anderson and James Arnold in order to continue the work they had been doing with him in Geneva. Both men had received offers from industry, and Arnold was a candidate for several teaching positions. After one-year appointments as research associates in Bloch's lab, both men moved to Varian. In a letter of March 8, 1955, to Leonard Schiff, Bloch explained why he needed to keep his team together:

> . . . there are strong positive reasons why the high-resolution work must go with me back to Stanford. We have in the last few months developed several new ideas which should allow to further improve the method to the point where one can study the different natural line-widths occurring in a spectrum. I am greatly interested in these questions, both from the experimental and from the theoretical point of view. . . . A separation [from Arnold and Anderson] would be a severe loss to me, to the research and to Stanford where it has originated and where it ought to be continued as one of the most important and original recent developments in the filed of nuclear magnetism. It is an absolute necessity, for this purpose, that at least one of the two men who are in sole possession of the experimental skill should set the apparatus up again and to operate it for a while until it can be continued in the hands of graduate students.
>
> You seem to be concerned whether this would be good for Anderson and/or Arnold. Actually I think it is by far the best if not the only thing for them to do. With my and their being outside of the country it would be most difficult to find even a half-way acceptable position for them in the U.S. It would seem worse than senseless if I tried now desperately to place them elsewhere when I need them badly myself for just about that time which it will take them to re-establish contacts in America and thus to obtain a really suitable position. Against this good, I fail to see the great harm which it would do them to spend about another year at Stanford. (Bloch 1955*a*)

3. The importance of Varian Associates California location to the career choices of early employees is a theme mentioned frequently by members of the founder generation, such as Martin Packard, Elliott Levinthal, and James Shoolery.

inspiration for the company. Russell Varian had dreamed of building his own company since his early college days. Born into a community of theosophists in Halcyon, California, Varian revealed in letters to family and friends his concerns about social justice, the labor movement in the 1930s, and socialism. Varian longed for a career that would support his interests in invention and scientific research, but early experiences with both industry and academe were less than satisfactory. In 1929 he joined Humble Oil in Houston, Texas, as a research scientist, but he felt constrained to work on conventional problems and discouraged from pursuing any ideas "off the beaten path." Varian spent time trying to get to know physicists at nearby Rice University, some of whom he had met previously at scientific meetings, but he was not invited to participate in the sort of departmental activities at Rice that would later be routinely open to him as a research associate at Stanford (Varian, Dorothy, 1983, pp. 89–95). After successfully completing work on improving the design of a vibrating magnetometer for oil exploration, work which Varian patented, he was dismissed from his position in January 1930 by the lab director Orley Truman, apparently as the result of a personality conflict. Varian returned to the San Francisco Bay area, where he worked briefly for Farnsworth Television and did additional physics research that he hoped would get him accepted into the Ph.D. program at Stanford. By this time, he had published three articles in physics journals, including a paper in the *Physical Review*, and held twelve patents. Unfortunately, his application to enter graduate studies in 1934, at the age of 37, was rejected, with the explanation that he was insufficiently sophisticated in mathematics, he had little German and no French, and his spelling was bad (Varian, Dorothy, 1983, p. 153). Varian did stay on at Stanford as a research associate, however, and, as mentioned above, in 1937 coinvented the klystron along with his brother Sigurd and William Hansen.

Russell Varian may have been frustrated in his pursuit of an academic career, but he and his colleagues succeeded in creating their own research laboratory on the edge of campus. For Varian, whose academic career had been blocked and who found work in an industrial lab unsatisfying, constructing the company was more than a matter of financial need. The Varian brothers, though not wealthy, enjoyed a substantial income from their klystron and various other patents. Nor did the academic players in the Varian venture have pressing monetary interests. Secure both financially and professionally, they were attracted by the opportunity to exploit their expertise in microwave physics and develop dimensions of their research in a commercial setting that was not encompassed by the agenda of "Big Physics" accelerator designs and nuclear physics being organized at Stanford. Their goal required a delicate balancing act: to pursue research as much as possible on their own terms, working with the generous funding of federally supported military research while avoiding restrictions of the sort placed on them by Sperry. In his 1975 article, "The $100 Idea," which discussed the development of the klystron, Ginzton pointedly critiqued the notion of "merely advancing technology for its own sake." Instead, Ginzton stressed the importance of being coupled to the marketplace, of identifying societal and market needs in the production of technology, and of the benefits of working in a creative research community rather than in small groups or in isolation (Ginzton 1975, p. 30).

As much as the early Varian documents witness to interest in developing profitable research discoveries and inventions, their language testifies to a strong desire to construct, beyond the walls of the university, a research community defined by the values, goals, and organizational and management style usually associated with a university environment. The recollections of Craig Nunnan, a Berkeley-trained microwave physicist who joined the company in 1955, are typical of descriptions of the environment at Varian. Nunnan recalled that "The strengths of Varian were ideas. The weaknesses were control. They did not have control from my point of view, . . . nothing. You'd just create, create. So it was very free, and we went on that way, just competing through ideas, getting contracts from customers because they liked what we were proposing, and trying to build the stuff. We always succeeded in building them, but we did not succeed in making a lot of money. We lost money" (Nunnan 1989). A document from March 1953, entitled "Company Philosophy, Company Objectives, Management Functions," written by Russell Varian, provides evidence of the spirit in which Varian Associates was conceived. In this document, Varian distinguished tangible factors in a working environment, such as remuneration, from intangibles, such as "human values of motivation, attitudes, and sense of security which determine the level and quality of morale" (Varian, Russell, n.d.). He noted that the company founders believed "the satisfactions to be derived from working, from associations, and from surroundings, were extremely important, and that these satisfactions could be achieved—to economic advantage." Under "Company Objectives," Varian outlined the group's goals, in language more suited to a university convocation than to a business charter: "to carry a large burden of pioneering in new fields in the application of science" and to employ "the very best of scientific and engineering talent . . . in an environment facilitating the proper exploitation of knowledge and intellect." He declared, "The rarest commodity in the world is human intelligence—and this must not be wasted, neglected, or allowed to stagnate." Varian went on to state the premise of the new company's objectives: he stressed that "the principal sales commodity [in a highly specialized field of technoscientific research] is human intelligence, associated with practical ingenuity." Accordingly, "Varian Associates [could] derive its income by selling research time and manufactured products"; the highly specialized nature of the field would necessitate the maintenance of "a research and development staff completely out of proportion, in size, to the support derived from contract sale of manufactured products"; but, he pointed out, "extensive research" conducted by "first-class talent" could be expected both to garner government support and to produce practical developments yielding numerous items "to be manufactured for the common good."

In the same document, Varian addressed implicitly the frustrations he and his Physics Department colleagues had encountered in the university, caught between the demands of big physics on the one side and the potential constraints imposed by research patronage from a corporate sponsor on the other. He pledged to provide an idealized, truly university-like working environment, in which corporate managers would be attuned to the needs, goals, and rhythms of laboratory researchers. He wrote:

> The control of Varian Associates must rest with those who understand the nature of the every-day work. . . . This premise is based upon the conviction that control of an operation of such extreme complexity, employing a large number of scientific personnel, must be in the hands of the technical staff.

Under "Management Functions" he noted, "Management realizes that the cornerstone of the Company activities is research and development. Therefore it will . . . establish a proper balance between research and development on one hand, and production on the other. The research activities will have stated budgets, which, once established, must be held inviolate."

The articles of incorporation of Varian Associates filed in 1949 state the purposes of the new company were to

> conduct general research in the fields of physical science of every kind or nature, including . . . heat, sound, light, optics, x-rays, charged particles, ionizing radiation, electricity, magnetism, properties of solids, liquids and gases, vacuum technology and applications thereof, chemistry including physical chemistry, electro-chemistry and metallurgy, to engage in the evaporation of

> substances in an evacuated chamber, to accelerate charged particles to high kinetic energies, to measure the gyro-magnetic ratio of nuclei of atoms, to use the gyro-magnetic properties of atoms to measure magnetic fields or for other purposes.

In short, they were in the business of research organized around microwave physics with the ultimate aim of developing vacuum products, NMR and electron paramagnetic resonance (EPR) spectrometers, linear accelerators, and optical spectrometers (Varian Associates 1958*c*, p. 1).

Obviously, for a company oriented primarily toward research, it was essential to attract excellent personnel and retain them. Russell Varian emphasized the importance of this issue:

> The foundation of Varian Associates is the quality of its research and development personnel. Such personnel are not easy to find, nor to keep. The company, therefore, will attempt to maintain the necessary environment to fully exploit such talents, and to augment its staff by attracting the very best talent available in the country.

Varian indicated how the company would pursue recruitment and retention of the very best talent. Besides maintaining "an efficient supporting facility" and encouraging "acceptance of new and challenging projects," Varian managers were to encourage "contacts between . . . staff and the outside scientific world by travel, publications, and close association with nearby universities." They were also to insure a company structure "in which new ideas [would be] easily transferred to the product development group." Varian Associates had to build a core of competent personnel working in a stimulating academic and technical environment. For this purpose patents were crucial. Through patents Varian would secure the core competence of the company. Through licensing agreements and development into commercial products, patents served both to generate revenues and, as a social mechanism, to keep the core intact. The nuclear-induction patent was deemed central to the Varian research enterprise. Although Bloch and Stanford University officials may have been uninterested in the commercial prospects of NMR, Russell Varian believed otherwise, for he convinced Bloch and Hansen to apply for their patent in the first place and, when Stanford did not pursue a patent, offered to prepare the patent application in return for an exclusive license to be transferred to Varian Associates once the company was established. It is interesting to note that he titled the patent application "Method and Means for Chemical Analysis by Nuclear Induction." As his notebooks from 1946 reveal, Varian's discussions with Hansen led him to perceive the broad potential of nuclear induction as a tool for detecting small quantities of an isotope in a sample. Following up on these discussions, Varian conducted his own study to determine whether, if suitably developed, nuclear induction could compete with other established chemical methods for identifying trace elements (Varian, Russell, 1946).[4] The results led Varian to urge Bloch and Hansen to file a patent on the induction principle, which they did in December of 1946 (Bloch and Hansen 1946). This was a visionary move, for the entire phenomenon of the chemical shift and spin-spin coupling (discussed below) had not been demonstrated in 1946, when the patent was filed. It was these phenomena, pursued by Martin Packard, James Arnold, Weston Anderson, and others in Bloch's laboratory, that made NMR particularly salient as an instrument for analytical chemistry. The creation of NMR as a field of chemical research was intimately connected to the hiring of those scientists as members of Varian Associates' scientific staff. When Russell Varian identified the scientific personnel as the backbone of the company, he undoubtedly had in mind the persons who formed the NMR research group and, later, the Applications Lab.

This was just the beginning of Varian's interest in patentability. As consultants to Varian Associates, Stanford physicists Chodorow and Ginzton not only remained attentive to prospects for patenting new inventions but also for extending the claims on their existing patents. Chodorow, for example, coming across a research paper by Les Hogan from Bell Labs, realized that the original Bloch-Hansen patent needed to be broadened to include resonance of electrons as well as nuclei of atoms. These revisions were added to the Bloch-Hansen patent reissued in 1955 and were crucial to securing Varian's position in the field of electron-proton magnetic resonance (Anderson 1994, p. 4). By 1955 Bloch was both a stockholder and a regular paid consultant for Varian, whose contract to the company committed him to licensing exclusively to Varian Associates patentable inventions emerging from his research (Stearns 1954). Moreover, Bloch, a few years earlier unin-

4. Varian constructed tables of moments for various isotopes and attempted to work out the components of an arbitrarily given small sample. Finding no chemists at Stanford with whom he could discuss his ideas, Varian went to Berkeley on August 9, 1946, to talk with Dr. Edwin F. Orlemann, who had set up many of the chemical processes at Oak Ridge. His goal was to determine whether NMR would be superior to other analytical techniques for detecting particular trace elements. Orlemann was impressed with the possibilities NMR offered for detecting fluorine, nitrogen, silicon, and various other elements. "Dr. Orlemann thinks the possibilities are very interesting, and well worth persewing [*sic*]. He says that if industrial concerns can speed up or improve their analysis, they won't hesitate at all to spend money on apparatus." Entry for August 10, 1946, p. 20.

terested in the commercial possibilities of his work, became dogged in his pursuit of licensing and royalty agreements covering aspects of inventions related to his NMR patent, particularly the magnets required by instruments for conducting NMR work (Bloch 1953).[5] Scientific research and profitability were mutually supportive and unproblematic goals.

The Discovery of NMR, the Bloch Lab, and NMR Development at Varian

We turn now to an examination of NMR and the experimental work that generated the patents central to Varian's business in the field of NMR. All of the physicists who founded Varian had rich backgrounds in microwave and radio research. While the company was originally formed with the idea of exploiting klystron designs, the primary objective of the founding partners was to develop businesses other than klystrons. As Russell Varian saw it, since the laboratory would be initially small and have limited capital, klystrons were more or less eliminated. Varian thought that in order to compete any company would have to have a considerable number of klystrons, and since they were expensive to develop, the prospects of entering the klystron business seemed remote. NMR seemed a more promising phenomenon to fol-

5. Bloch insisted that the base for determining his royalties should include the following:

> Nuclear magnetic resonance spectrometers; nuclear induction fluxmeter equipment; nuclear induction stabilizers; spin echo equipment; electromagnets; permanent magnets; power supplies; voltage regulators; probe holders; probes; radiofrequency units; extension cables; motor drives; oscillators; crystal calibrators; recorders; shims; control panels.

Obviously, Bloch was not a man to disregard possible personal financial rewards deriving from his research. He was prepared to press these claims legally, as is evident in the discussion recorded two years later on October 10, 1955, among Bloch, Myrl Stearns, and Paul Hunter, of Varian Associates. The discussion focused on limitations on the extremely broad list of items above. At issue was whether Bloch had a right to returns on all Varian magnets, including magnets not sold as part of NMR instruments. Bloch's response was that they (Varian) "would not be in the magnet business at all if it were not for his invention." Another issue to which Bloch was attentive was the development of NMR devices by other competitor firms and experimental work using NMR techniques by individual researchers who built their own equipment and did not pay licensing fees. At issue was the enforceability of the patent and license. (See Bloch 1955*b*.) Over the years Bloch, made a considerable sum of money from his NMR patent. His royalty was 4% of all sales of magnets and spectrometers in which magnets based on his original patent were a component. In 1956, for example, his royalties exceeded $52,000. (See Bloch 1956.) Extrapolating from these figures, by 1965 Bloch would have made at least $740,000 from sales of magnets alone and, if spectrometers are included, several million dollars.

low for initial small-scale commercial development (Varian, Russell, 1958, p. 6).

The phenomenon of NMR had first been discussed in 1937 by physicist I. I. Rabi in his proposal to use a static magnetic field that changed direction from point to point in order to measure the magnetic moment of a neutron, atom, or molecule. In a 1939 paper Rabi described the magnetic resonance beam method for measuring magnetic moments. He discussed applying a radio-frequency field in combination with a static magnetic field to produce magnetic resonance. In a later paper in 1939 he used this method to measure the moments of the proton and deuteron. Felix Bloch and Luis Alvarez used a variation of the beam method to measure the neutron moment in 1940. In 1945 Felix Bloch explored the question of whether the nuclear transitions could be detected by simpler electromagnetic methods applied to matter of ordinary density as opposed to molecular beams. He called the phenomenon of interest to him "nuclear induction," because of the similarity of the Faraday effect of rotation of the plane of polarization of light around a magnetic field, with the radio-frequency (RF) field taking the place of the field vectors in the light wave and the observed perpendicular nuclear induction indicating a rotation of the total oscillating field around the constant magnetic field. The intuitive picture is that nuclei of atoms have a magnetic moment and angular spin, like an iron barmagnet spinning (Bloch and Hansen 1946; Bloch 1946*b*; Varian, Russell 1951; Anderson 1992; Feeney 1992). In the presence of a constant magnetic field, the nuclear moments of a substance would be expected to polarize. By superimposing on the constant field an oscillating magnetic field at right angles, the polarization, originally parallel to the constant field, will be made to precess, like a rotating gyroscope. According to the laws of quantum mechanics, the interaction energy of the magnetic moments in the constant field can take one of several values, so-called energy levels. The application of the oscillating magnetic field of appropriate frequency can induce transitions in energy levels. The energy absorbed or emitted in this process can be measured, enabling one to determine the energy-level spacings. The energy-level spacings are proportional to the magnetic field and the magnetic moment of the atomic nucleus. By measuring the magnetic-field strength and the corresponding frequency, the physicists could determine the magnetic moments of the atomic nuclei. Felix Bloch developed and published the theoretical calculations connected with nuclear induction in 1946. He received the Nobel Prize for this work in 1952.

Bloch and William Hansen worked closely with one of their graduate students, Martin Packard, in devising an instrument and experimental arrangement that would detect the signal emitted from precessing nuclei and display it on an oscilloscope (Bloch, Hansen, and Packard 1946). For this part of the collaboration, Bloch contributed the theoretical calculations, the design of the magnet, and the measurements of the d.c. field in which resonance would be predicted to occur. Packard and Hansen, who had provided the original design of the circuitry, were concerned with the RF units, i.e., with the transmitter circuit, the receiver amplifier and rectifier, and the corresponding power supplies (Bloch 1959). Hansen designed the cross-coil configuration and "the probe," a brass box with an appendage that allowed the transmitter, receiver coils, and the sample to be placed between the pole pieces of the magnet. Packard designed and constructed the radio frequency source, the audio amplifier, and the oscilloscope display. The design of the instrumentation was beautifully simple, an attribute which may have contributed to the ultimate success of the NMR principle and its acceptance by other scientists.

According to the nuclear induction principle derived by Bloch, NMR depends on the fact that each isotope of elements with non-zero spin possesses a coupled magnetic moment that is both immutable and distinct in value from that possessed by all other non-zero spin isotopes. Packard, with Bloch's and Hansen's direction and assistance, set out to elicit this identifying attribute of the isotope from a sample of material containing that isotope. The instrument he designed involved a three-step process. First, the sample was placed in a powerful and uniform magnetic field, causing a certain portion (random thermal motion of molecules prevents all of them from lining up) of the nuclear moments to point in the direction of the field. This population of moments was considered the macroscopic spin vector of the moments. The second step was to tip the macroscopic spin vector by irradiating the sample with alternating radio frequency energy of the appropriate value. The alternating RF field was oriented at right angles to the main magnetic field. By alternating the frequency at the appropriate time (one-half cycle) the precessing macroscopic moment would continue to precess, the precession remaining always in step with the RF field reversal: hence the term "resonance." The result of this is a radiofrequency response by the isotope in the sample, which was detected by a receiver coil oriented at right angles to both the direction of the constant magnetic field and the field of the applied RF. The precessing magnetic moment induces a voltage in the receiver coil, which is then amplified and displayed on an oscillograph and (a later addition) graphic recorder. Packard conducted the experiments in the Stanford

Physics Department, using equipment that cost about $450 (Bloch n.d., p. 42). This was important work, and, as Bloch indicated in an interview, it depended essentially on an intense interaction among researchers with experience in wartime work on nuclear physics, radio and microwave work at the Radio Research Lab during the war and, after 1945, back at Stanford, work on traveling wave tubes and klystrons being done in the Physics Department by Hansen, Ginzton, Russell Varian, Chodorow, and others (Bloch n.d., pp. 40–44).

We have merely touched on what was certainly a deep network of experimental expertise and theoretical competence at Stanford and a tradition of patenting research. NMR at this point, however, was of relevance to measurements of magnetic moments of the nucleus and as such was certainly valuable for experimental physics, but not yet a more broadly useful chemical tool. However, developments in Bloch's lab over the next several years (1946–51) bore out Varian's optimism. As Bloch continued to learn more about the measurement of nuclear magnetic moments, it became clear that a limitation was the homogeneity of the magnetic field. He and his students and lab assistants devoted considerable efforts to improving magnets. In his reports to the Research Corporation, Bloch explained that the major part of the nuclear-induction apparatus consisted of three large magnets, two to magnetize the polarizer and analyzer plates and the third to procure the strong homogeneous field in which protons, neutrons, and deuterons were brought to resonance.[6] The polarizer and analyzer magnet were of double-yoke type with pointed pole pieces. They were each energized by a pair of coils, wound of copper pipe, and cooled by water running through the pipe. Each of these weighed about half a ton and produced a field of over 12,000 G in a gap of 1-1/2 inches. The resonance magnet was of single-yoke type with cylindrical pole pieces of 8-inch diameter, energized by two coils of fine wire, each having 17,000 turns and intermittent layers of pipe, cooled by running water. A current of two amperes through each turn and corresponding voltage of 2,500 V generated a field of 11,000 G over a gap of 1-1/2 inches. This magnet weighed about 1 ton, bringing the total weight of the instrument to over 2 tons—not the sort of instrument to fit neatly into a small lab. A number of design improvements in the magnets and frame of the instrument followed over the next two years, mostly undertaken by Martin Packard. By December 1947, for example, Packard

6. Bloch's reports on the personnel, experiments, and results of his laboratory on the Research Corporation grant, as well as the Status Reports on ONR grants, are contained in Felix Bloch Papers, SC 330, Box 30.

had designed a simplified and compact form of the device which delivered great constancy of field and ease of manipulation (Bloch 1948). In the following year Packard succeeded in housing the device in a single chassis. Warren Proctor, another graduate student in the lab, improved on Hansen's bridge-type design of the original nuclear-induction magnet, incorporating into it a balanced modulator to obtain extremely narrow bandwidth and thereby high sensitivity. Other improvements to the induction magnet were made by Emery Rogers. A further improvement, due to Packard, was crucial to the use of the instrument: he developed a method for obtaining nuclear-induction signals below the noise level, consisting of the superposition of many traces on a photographic film. The result was a blackening ribbon that became continuous the longer the exposure. This technique permitted the recording of small induction signals. Further improvements in the photographic method the next year enabled them to "take pictures of proton signals" in gases, particularly propane, hydrogen, and within a few months even a small sample of fluorine (Bloch 1948, p. 2).

With the improved magnets and sensitive photographic registration method, Bloch's lab discovered that the value for the magnetic field at the nucleus depended to some extent on the chemical environment, the so-called chemical shift (Proctor and Yu 1949, 1950*a*, 1950*b*). This major finding resulted from contributions to the group of two new members, E. L. Hahn and S. S. Dharmatti, who came to Stanford in 1950. Hahn had just received his Ph.D. from the University of Illinois and came to work as a postdoctoral fellow in Bloch's laboratory as a National Research Council (NRC) Fellow. Dharmatti, who came from the University of Southern California, had worked on magnetic susceptibilities and joined the group as a research associate. Hahn's work was crucial. In his original (Bloch 1946*b*) paper on nuclear magnetic induction, Bloch had mentioned using a *pulsed* radio frequency to induce the transient signal known as the free induction decay, but he had not gone on to elaborate this idea. Hahn pursued the use of pulsed radio frequencies in his dissertation, demonstrating with the technique of envelope modulation that following a short pulse, some substances exhibit "spin echoes," multiple closely spaced resonance lines. A new 12-inch electromagnet was introduced to the group's instrument for studying spin echo effects produced by pulsed RF (Bloch 1950). Bloch students Proctor and Yu investigated the resonance signature for the same nucleus in different compounds, observing a different resonance frequency due to the molecular contribution of the valency electrons to the effective field at the nucleus. They pointed out that the discovery of chemical effects on the resonance frequency introduced an element of uncertainty into the determination of the values of the magnetic moments, and it was difficult to say to what extent this affected the experimental accuracy (Proctor and Yu 1951, p. 29). From Bloch's point of view this was an obstacle to his original purpose, but for his students, it turned out to be the beginning of a new era. The first to pursue this further were Martin Packard, James T. Arnold, and Dharmatti, in a paper demonstrating different peaks for different groups in the alcohols. For ethyl alcohol, CH_3-CH_2-OH, for instance, the NMR spectrum was a three-line spectrum with intensities in the ratio 3:2:1 (Arnold, Dharmatti, and Packard 1951). Further explorations of phenomena connected with the chemical shift were contributed by Arnold in his dissertation, in which he demonstrated that spin-spin couplings occur between protons of the groups splitting each group into a further multiplet. The suggestion was strong that continued research directed at increasing field strengths of magnets, stabilizing them and rendering them homogeneous, would produce significant advances in developing an analytic tool for unlocking organic chemical structure.

Varian Associates opened its doors in April of 1948. In addition to the founders (the Varian brothers, Hansen, Ginzton, and Leonard Schiff were joined by Paul Hunter, a former Sperry patent attorney, former Sperry electrical engineer H. Myrl Stearns, Dorothy Varian, and Richard Leonard), a group of former Bloch students soon joined the company. Elliott Levinthal, who had measured the magnetic moment of deuterium for his dissertation, came to the company in 1949 as director of research. Between 1949 and 1956, Martin Packard, Harry E. Weaver, Warren Proctor, Emery Rogers, James Arnold, Weston Anderson—all Bloch students who had been involved in the design and improvement of NMR instrumentation and the extension of NMR techniques—became members of Varian Associates. We might be tempted to consider this an example of the paradigm of technology transfer: a group of academic scientists generated a host of novel, interconnected core ideas; these ideas were patented and turned into prototype devices, such as NMR spectrometers, linear accelerators, and klystrons; the patents were licensed to a start-up company; and the technology was transferred by hiring graduate students and junior faculty experienced in the experimental devices and techniques. However, this fundamentally unidirectional description does not capture the give-and-take between university and industry that emerges as we continue to trace the development of NMR.

In spite of this collection of core competence at Varian, NMR as the basis for an analytical tool in chemistry was still just a dream in the early 1950s. Varian Associates' business was based on the importance of klystron tubes for aerial navigation and missile guidance. Their goal was to expand the field of applications of microwave technologies to commercial technologies. NMR looked like a possible candidate for such development, but in the early 1950s no one was confident of its commercial future. Managers at Hewlett-Packard, for instance, questioned whether microwave was primarily of military interest and whether, because of the variable nature of military contracts, they should remain in the microwave business after the war. Similar qualms led General Radio Company to drop out of the microwave business after the war (Egan and Hackley 1984, p. 21). Several difficulties confronted the effort to produce a commercially viable instrument useful to scientific research and process control in industry. First was the problem of producing a high-quality NMR spectrometer capable of making reliable structure determinations in chemistry. Closely connected with this was the challenge of recruiting scientists to develop and refine the techniques of NMR and thereby of gaining acceptance of NMR as a chemical tool in the scientific community.

The possibilities here seemed promising but not easy. Rapid changes in physical instrumentation due to improved electronics during the war had brought about an explosive development of IR spectroscopy as a structure-determining tool. While IR had been introduced around 1900 as a technique capable of fingerprinting certain organic compounds, the field had not progressed much before 1940. Early work had shown that each compound has its own unique IR absorption pattern and that certain groups, even in different molecules, give absorption bands that are found at approximately the same wavelength. The difficulty of measuring the spectra, however, led the technique to be virtually ignored by chemists until the 1940s, when it became possible to amplify electronically the very small signals obtained from the tiny thermocouple in an IR spectrometer and to record them on a strip chart. Improvements in the responsiveness in thermocouples during the late 1940s led to the development of the enormously successful Perkins-Elmer models, 12-B and 21, and the Baird double-beam IR spectrometers. With these new tools chemists began to complement gravimetric combustion and volumetric titration methods with IR spectral data in determining the structure of compounds. Thus, by 1953, no less than 216 articles in the *Journal of the American Chemical Society,* roughly 10% of all articles in the journal that year, were either direct contributions to IR spectroscopy or employed IR spectroscopy as a tool for the determination of structure or the identification of a compound.[7] The Perkins-Elmer and Baird IR spectrometers thus created a potential market for other instruments capable of exploring regions of the electromagnetic spectrum below the IR into the range of microwaves (EPR) and radiowave frequencies (NMR). It took another decade to create this potential market and turn it into a source of commercial profit by the development of a high quality NMR spectrometer. In the remainder of this paper we concentrate on the recognition of this potential at Varian and the commitment to creating NMR spectrometers capable of becoming mainline analytical instruments in chemistry, a move that ultimately created new forms of practice and transformed the academic discipline of chemistry. Varian was not just reacting to developments in academic chemistry, providing scientists with the tools they thought they needed. Industry scientists and engineers at Varian actively participated in changing the disciplinary landscape of chemistry by replacing traditional methods with new physical techniques, concepts, and instruments. Major conceptual developments in chemistry, particularly in protein chemistry, resulted from these and related efforts by other physics instruments start-up firms.

An early policy statement by Elliott Levinthal gives a sense of the mission and the stakes. Levinthal outlined the possibilities presenting themselves to Varian Associates in the field of nuclear magnetic induction and paramagnetic resonance and made suggestions for future courses of action. He presented five areas for possible exploration: (1) nuclear fluxmeters and related field measuring equipment; (2) nuclear induction as a circuit element; (3) nuclear induction techniques for field measurements of the earth; (4) paramagnetic resonance as a method for measuring the earth's field and extending the range of fluxmeters to fields between 5–500 gauss. Areas 3 and 4 were crucial to ONR interests, and Levinthal recommended pursuing them in order to get military support for Varian work. It is interesting to see the necessity of getting military contracts to provide the funding needed to keep the team of scientists and engineers they were assembling together. At the same time, it was clear that pursuit of NMR for (5) chemical experimentation, would be of vast importance. Levinthal gave chemical analysis lengthy consideration:

> The original reason for our interest in Nuclear Induction was the tremendous commercial possibilities of its application to the field of chemical analysis and control. Since that time, with the

7. Determination made by content search of the articles in the *Journal of American Chemistry* for 1953. The total number of articles in the journal for 1953 was 2,238.

exception of some work on moisture measurements, no work has been done by Varian Associates in this field. The reason for this is that the applications were difficult and it required costly experiments and development to complete a working model which could clearly demonstrate the possibilities. . . .

Recent experiments at Stanford performed by Dr. Packard have opened up now an important chemical possibility. He has found that there are observable displacements of the hydrogen signal which are different for different organic compounds and are different for different hydrogen groups in the same compound. This means that this could be a tool for uniquely identifying organic compounds, for determining the arrangement of the atom in some organic molecules and observing for example hydrogen signals due to moisture in the presence of hydrogen signals due to other compounds. With of course reduced sensitivity these same effects could be observed for other atoms in compounds.

The general implication is that this is not only a tool for identification of isotopes but is also useful for the study of chemical compounds. I feel that these chemical aspects of nuclear resonance are of much greater commercial importance than any of the other previously discussed applications. While no application directly concerned with military weapons has been suggested it is felt that any technique such as this is of military importance equal to that of its commercial possibilities because of its contribution to our industrial potential and techniques in addition to applications such as the detection of moisture in gasoline. It is recommended very strongly that we get a contract to develop this technique even though it should mean sacrificing some of the other objectives outlined above. For reasons given below it is not suggested that we either finance this development completely ourselves or seek outside private subsidization.

Financial, Space and Personnel Requirements for the Above Program

I think it is certain that no program can be continued during the coming years unless it receives high priorities because of its military importance. The only concrete basis for judging, both on our part and the government's the priority which will be received is the extent of government financial support. This fact makes it almost imperative to seek this method of support and to engage only in projects which receive it. Thus, regardless of what other

> support we can or wish to get it is essential to get a government contract. (Levinthal 1951)

In the early days of the company, Varian Associates was primarily a research and engineering services company. The sale of manufactured items did not exceed the earnings from research and engineering work until 1955 (Varian Associates 1955, p. 3). As Levinthal's memo suggests, a constant concern for the company was the development of new commercial markets for its research and manufactured products. Varian accounting made a distinction between "Varian-sponsored" research and development and "contract-sponsored" research and development. NMR was one of the areas, identified by Levinthal above, that Varian Associates regarded as a target for its own internally supported research and development investment. Persons close to the developments at Varian Associates during those years attribute the company's commitment to NMR to Russell Varian's own visionary enthusiasm for the field.[8] Table 1 lists all areas of Varian-sponsored research and development, including NMR, for the years 1952–65, the period we identify as relevant to the establishment of NMR as a chemical discipline. It is remarkable that Varian Associates invested so heavily in a research field that did not generate a commercially profitable business for about a decade. The data on the relation of Varian-sponsored research to its total sales show a continuously increasing commitment to the strategy of expanding the company through research. For our purposes the period between 1956 and 1961 is of specific interest. It was in this period, so we argue below, that Varian spectrometers established NMR as a highly successful tool for structure determination in chemistry, and a commitment was formed (in 1957) at Varian to engineer and manufacture a successful commercial scientific instrument, the Varian A-60.

Data furnished by Varian Associates on the activities of its various departments do not permit us to chart the exact expenditures on research and development for NMR alone, but occasional revelations provided by internal company memoranda offer insight into the company's strategy. A decade after Levinthal's memo, the fiscal budget of instrument division for 1964 suggests that the themes outlined as priorities by Levinthal were still evident in the allocation of company effort. As the pie-chart from that memo shows (figs. 1 and 2), in the highest category of effort, 47% of all research and development went into tubes—klystrons, traveling-wave tubes, and other microwave tubes—while 12%, the second highest category, was devoted to re-

8. Personal communication from James Shoolery and Martin Packard.

Table 1. Varian-Sponsored Research

Year	Company R&D ($)	Increase ($)	Increase (%)	Total Sales ($)	R&D as % Sales
1952[a]	23,541[b]			3,826,702	.6
1953	158,728	135,187	174	5,023,272	3.2
1954	190,504	31,776	20	5,902,640	3.2
1955	193,193	2,689	1.5	7,162,350	2.7
1956	433,047	239,854	124	11,000,116	3.9
1957	714,382	281,335	65	16,836,086	4.2
1958	1,062,264	347,882	49	19,543,232	5.4
1959	2,045,303	983,039	93	38,130,311	5.4
1960	2,500,000[c]	454,697	22	49,000,000	5.1
1961	3,950,000	1,450,000	58	57,987,000	6.6
1962	4,480,000	550,000	14	70,825,000	6.4
1963	5,800,000	1,450,000	32	64,626,000	8.8
1964	5,100,000	−936,000	−16	52,606,000	8.6
1965	7,300,000	2,236,000	45	100,410,000	7.3
1966	9,554,000	...	...	145,182,000	6.6
1967	12,582,000	...	...	160,508,000	7.8
1968	14,572,000	...	...	170,755,000	8.5

NOTE.—R&D = research and development.
[a]Source is Dean Witter & Co. (1957), p. 9.
[b]Source for data for company R&D, 1952–58 is Varian Associates (1958*b*).
[c]Source for data on years 1959–65 on Varian-supported R&D is Varian Associates (1966), p. 14, Chart on R&D Expense (approximate figures). The figure for 1964 was confirmed against Varian Associates (1964), pp. 3–4.

search and development of analytical instruments, primarily NMR, EPR, and analytical methods supporting NMR. From the breakdown of research and development for tubes and analytical instruments in this budget, we see that most of the research dollars spent on tubes was generated from contracts (in the ratio of almost 80%:20%). The breakdown of development dollars was about the same, 80% contract versus 20% Varian. The situation was completely reversed with respect to research and development of NMR devices. Varian contributed 87% of the funds for research on NMR devices. Similarly on the development side: Varian contributed 90% of the funds toward development. As a percentage of their total research budget, Varian sponsored 10.3% of its research in NMR versus 8.5% for tubes. Contracts amounted to 30.9% of Varian's tube research, whereas only 1.5% of NMR research was supported by contracts. The conclusion from this seems clear: Var-

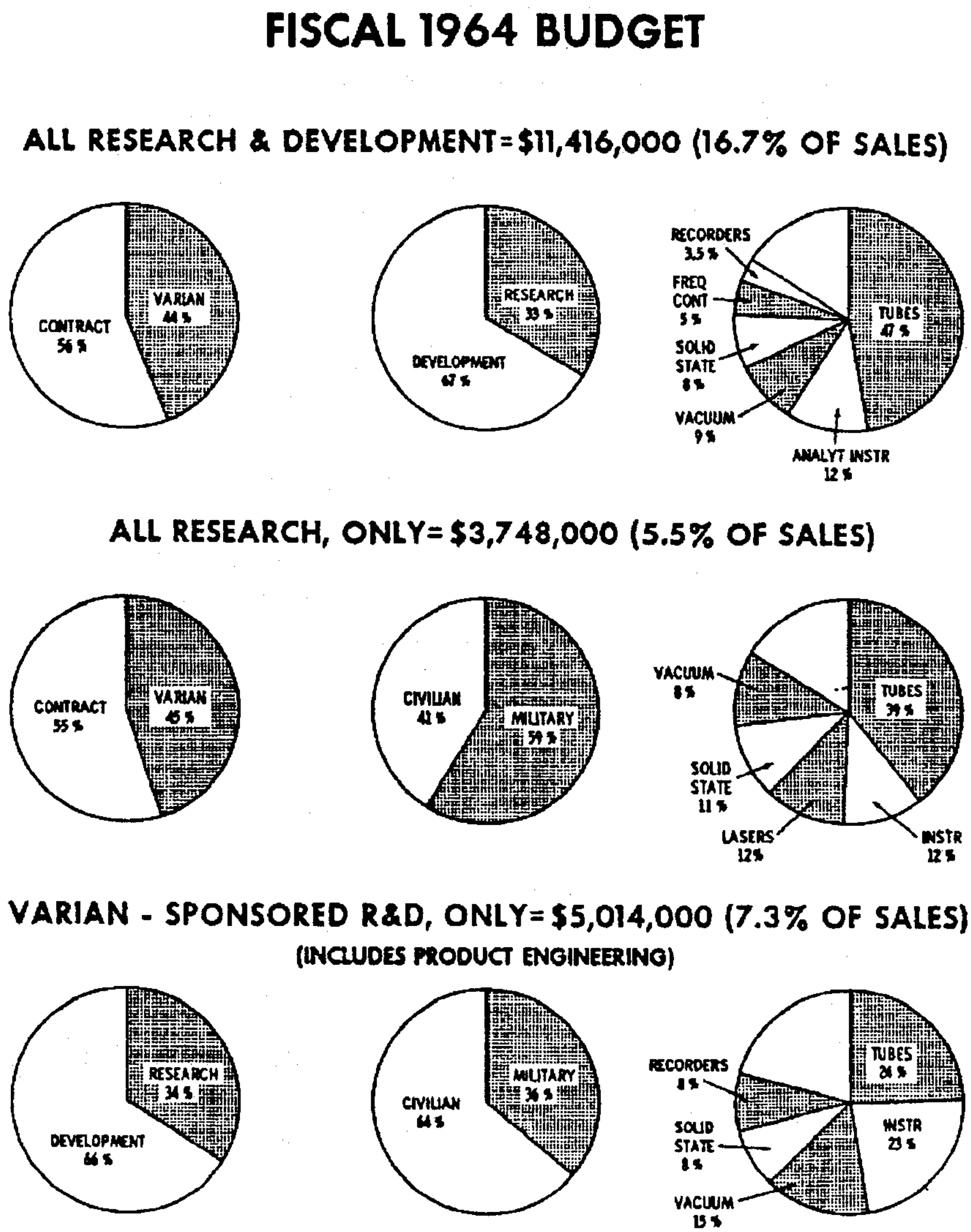
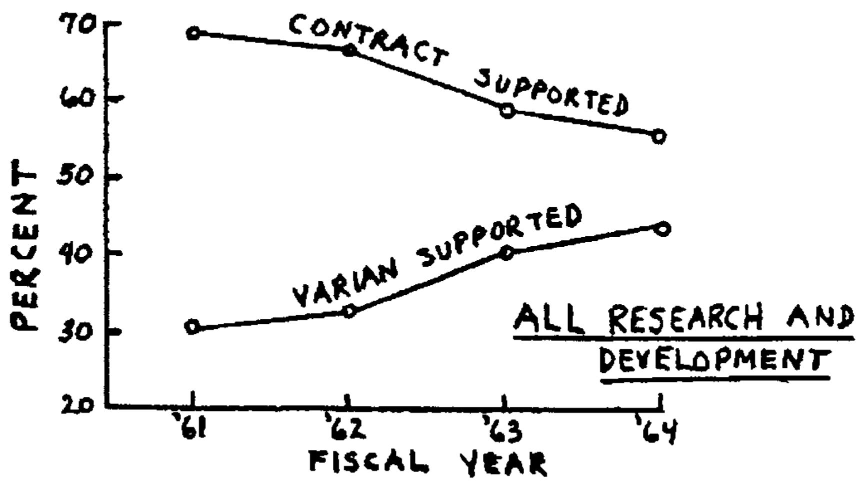


Figure 1. Varian-sponsored research budget memo from 1964. Source: Edward Ginzton papers, SC 330, Box 5, Folder "Central Records." Varian Interoffice Memo, January 15, 1964, from E. W. Herold, pp. 2, 8.

ANNUAL TRENDS

This study of the R & D budget is now the fourth one. In comparing the pie charts on the first page over the four years, there are only two which show a significant trend. For all the others, if they show a change, it is explained by the changes in the way the data were used (for example, for FY-1964, Product Engineering was included for the first time). The two significant trends are shown below.



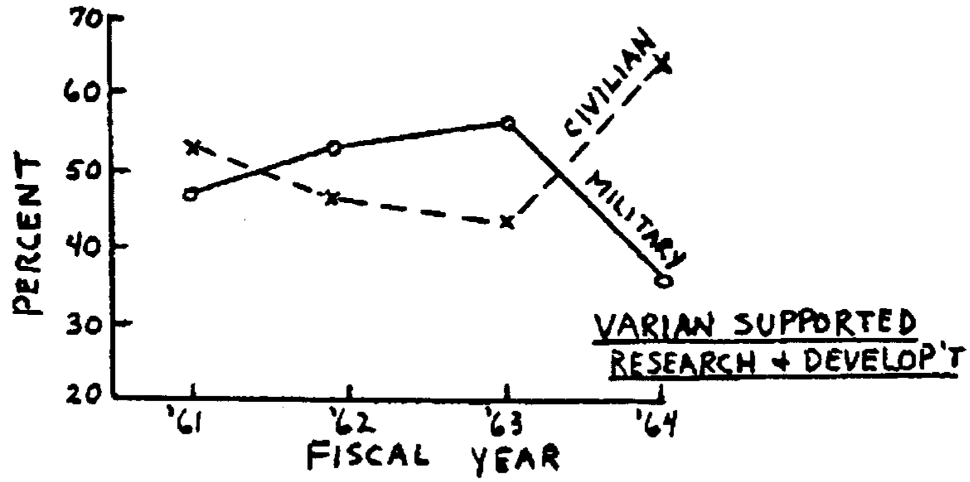


Figure 2. Projected military and civilian research contracts, 1964

ian was committed to the creation of NMR as a discipline of instrumentation. Indeed, funds generated by the tube business—primarily military contracts—paid for this NMR research.[9]

This commitment to turning profits from military and government contract research into research lines of future commercial importance is reflected in the annual reports to Varian stockholders beginning in 1955. Emphasizing the importance of research in occupying "the very heart of an industry which promises to be one of the fastest growing and most important in modern industrial history," Russell Varian noted that beginning from six employees in 1949 the company had grown to 844 employees in September of 1955, with a staff of 125 engineers, technicians and consultants, "including some of America's most prominent research scientists and engineers."(Varian Associates 1955, p. 3) The revenues generated by research and engineering services amounted to $2,203,960 (a decline from the previous year due to the completion of a major research contract), which amounted to 31% of the total sales for the entire company (down from 49% the previous year). Varian observed:

> The Company anticipates that research and engineering services under contract and billable to customers will continue to provide a steady source of revenue. However, increasing emphasis is being placed on basic research and development of the Company's own products. As a result, research activities will continue to expand, although direct income from these services may not be substantially increased. (Varian Associates 1955, p. 4)

Noting a 55% increase over 1954 in sales of electronic instruments, Russell Varian announced that "the Company is the largest producer of high-homogeneity, high-stability electromagnets in the world. Improved models of the Company's NMR spectrometer were developed during 1955, and have established NMR spectroscopy as a valuable tool in the field of physical and chemical research." A further reason for the expected future growth of the company's instrument business was the move into the manufacture of new graphic recording instruments, such as the Varian G-10 Graphic Recorder, and other microwave instruments with wide applications in industry, science, medicine, and

9. The budget survey concluded with a graph with an implicit note of warning, at least so we read it. Two trends were graphed over the previous four years. They showed a percent decline in contract supported research and development and an increase in Varian supported research and development. The second trend showed a precipitous fluctuation and decline in military versus civilian-sector research and development. Could they continue to sustain the NMR program in an era of falling military support?

other technical fields: "New products, such as electron paramagnetic resonance (EPR) and microwave absorption spectrometers, are now in various stages of research and development, and will be added to the Varian line of electronic instruments in the near future" (Varian Associates 1955, p. 4). The sale of its scientific instruments amounted to $940,540, or 13% of total sales for 1955. Varian was committed to expanding this business through the construction of manufacturing facilities combined with further research and improvement of existing instrument types:

> Production of instruments has increased to the point where manufacturing economies can be realized through standardizing many of the procedures in the complicated process of parts and subassembly fabrication, assembly and test of these extremely precise instruments. New facilities have been provided to permit the fabrication of components and assemblies in the Company's own plant, thus allowing the application of the most modern techniques and controls, resulting in improved product quality.
>
> Through a continuing product improvement program, a very dramatic increase in the resolving power of the high resolution spectrometer was recently achieved, enhancing its value in present applications and enlarging its potential for many new uses. This instrument, already widely used in leading research organizations, is of major importance in the spectroscopy field with the petroleum and chemical industries as principal users. (Varian Associates 1955, p. 7)

The introduction in December 1955 of the superstabilizer, which permitted an accuracy of field stabilization of one part per million, increased the resolution of the NMR instrument; further improvements in the homogeneity of the magnetic field had also been made. Such improvements, along with the introduction of the first line of EPR instruments and an improved model of the graphic recorder, led to a dramatic 106% increase in the sales of instruments (from $1,307,569 to $2,697,608). By the end of 1956 Varian had sold ten of the new EPR instruments, bringing the total number of all models of Varian spectrometers sold since 1953 to over 100 (Varian Associates 1956, p. 8).[10] Export sales of Varian instruments were increasing so rapidly that it

10. "Over one hundred NMR and EPR spectrometers are now being utilized in research establishments throughout the world. These research laboratories are operated by industrial enterprises, government agencies, and universities and are devoted to basic research in the fields of chemistry, physics, biology, and medicine. A number of these units have been installed in research laboratories outside the United States to meet the

became necessary to set up a direct system of foreign sales and service representation for this division of the company. By 1958 the company was represented in 20 countries by an organization of 32 representatives with a total of 175 employees (Varian Associates 1958*a*, p. 8). To further stimulate this increasing demand for spectrometers, Varian Associates opened a second laboratory devoted to EPR research in 1956, and by 1958 the Company operated four laboratories for spectroscopy research and development (Varian Associates 1956, 1958*a*, p. 15). The combined number of research scientists and engineers engaged in all areas of research and development in the fields of microwave tubes and electronic instruments in 1956 was reported to be over 150; in 1957 a special study done by Dean Witter & Co. reported a research and development organization totaling over 170 scientists and engineers, including 25 with Ph.D.s (Varian Associates 1956, pp. 4–5; Dean Witter & Co., 1957, p. 13), and in 1958 the reported number of scientists and engineers working in the Instrument Division's research and development labs was 200 (Varian Associates 1958*a*, p. 15). While numbers of scientists and engineers working specifically on scientific instruments as opposed to microwave tube research are not available for all years, the annual report for 1957 indicated a staff size of 61 for scientific instruments (Varian Associates 1957*a*, p. 8). The acceptance of Varian instruments and the growth of the field of radio-frequency spectroscopy was evident by the 184% increase in the 1957 sales of Varian spectrometers over instrument sales in 1956 (Varian Associates 1957*a*, p. 7).[11] While a pale comparison to the robust sales of 1957, the sales of laboratory instruments for 1958 showed a substantial 28% increase over sales for 1957 (Varian Associates 1958*a*, p. 8).

The commitment to assembling a core group of competent scientists and engineers and to improving and refining NMR at Varian resulted in its first substantial reward in 1956, with the Varian 4300 B series, a spectrometer capable of operating at 60 megacycles/second and equipped with a superstabilizer. The next major milestone came in 1961 with the production of the A-60, the first truly successful high-resolution NMR spectrometer, which brought NMR into the chemical lab as a routine instrument. Martin Packard is reported to have quipped that before 1961 "You could smell the cork on the bottle and

growing demand for these instruments in foreign markets" (Varian Associates 1956, p. 8).

11. Unfortunately, numbers and prices of spectrometers sold were not recorded in these reports. Efforts to gather this information from company records have been unsuccessful, to date.

Table 2. Introduction Dates of Varian Spectrometers

Year	Model
1953	HR-30
1955	HR-40
1956	HR-60, DP-60
1959	HR-100
1961	A-60
1962	PA-7
1963	HA-100, HA-60, DA-60, A56/60
1964	HA-60 EL, DA-60 EL
1965	HR-220

almost make the same analysis as those early systems"(Anderson 1994, p. 2). Table 2 shows the introduction dates of Varian spectrometers, from 1953.

The study done by Dean Witter & Company in 1957 of the potential of Varian for future growth pointed to several factors supporting its conclusion that the company would be an attractive stock investment. First, the Dean Witter report praised the company's commitment to research and the high level of competence the firm had assembled in its research organization. In what the report described as a "seller's market," Varian had been able to recruit and retain top researchers in the face of formidable competition. Relying on a projection of *Fortune Magazine* that the electronics industry would more than double its growth in the next five years (1957–62), Dean Witter felt that a company such as Varian that had developed a research and engineering organization with high degrees of technical skill would make it difficult for new competition to enter the field. The shortage of skilled scientists and technicians would compound the difficulties for a new organization, leaving a company like Varian with virtually assured dominance of the field (Dean Witter & Co. 1957, p. 17). The report also praised the company's strategy of moving into radiospectroscopy, a field of scientific instrumentation that had already been prepared for growth by industry giants such as Perkin-Elmer, Consolidated Electrodynamics, and Beckman Instruments. The report noted:

> In the field of scientific instruments, demand for Varian's spectrometers promises to grow at least as rapidly as that for other types of spectrometers now in existence and produced by such companies as Consolidated Electrodynamics, Perkin-Elmer, and

> Beckman Instruments. Indeed, it may be even more rapid since Varian's devices are complementary to these other types and are presently less widely used in the field. Consequently, Varian has considerable prospects for expansion into industrial areas already familiar with the advantages of using the spectroscopic analysis techniques for laboratory and process applications. (Dean Witter & Co. 1957, p. 16)

This series of NMR models represents 12 years of continuous improvements in a number of different components, including improvements in magnet technology, advances in frequency stabilization and locking, the introduction of solid-state devices, application of information theory and use of computers in NMR, and advances in the theory of NMR. One area of crucial significance is enhanced and refined magnet technology. Since chemical shift depends linearly on field strength, the history of NMR is in part a history of increasing field strengths employed for experiment. Figure 3 gives field strengths for Varian magnets (traditionally measured in megahertz rather than gauss, in light of the gyromagnetic relation $w = yH$). A full order of magnitude of dispersion in the resulting spectra was gained in the first 20 years.

These improvements were made not only through close interaction among Varian scientists, but also through careful monitoring of information in other domains, attending scientific meetings, etc. For example, one improvement crucial for early developments was the idea suggested to James Arnold and Weston Anderson of spinning the sample in the magnet, the effect of which was effectively to average the magnetic field variations over the sample volume. This was a crucial technique for improving line-width resolution (Anderson and Arnold 1954; Bloch 1954). This contribution to the dissertation work of these two researchers was incorporated by them into the Varian NMR devices they helped design. An equally important improvement to early instrumentation was the addition of an automatic feedback loop linking magnetic field and frequency. This idea was developed by Arnold, Anderson, and Bloch while they were working at CERN and brought back to Stanford in 1955. We get a sense of the ways these networks contributed to the developing "art of NMR" ultimately incorporated into Varian instruments from a letter of Bloch:

> Before I left Geneva Jim Arnold had his pulsing system working satisfactorily and was just about to apply it to actual observations. The mechanical part of the spinning system, with reversal of rotation, was almost finished, and I think he wants to try that out, too, before the equipment comes back here. Jim plans to start

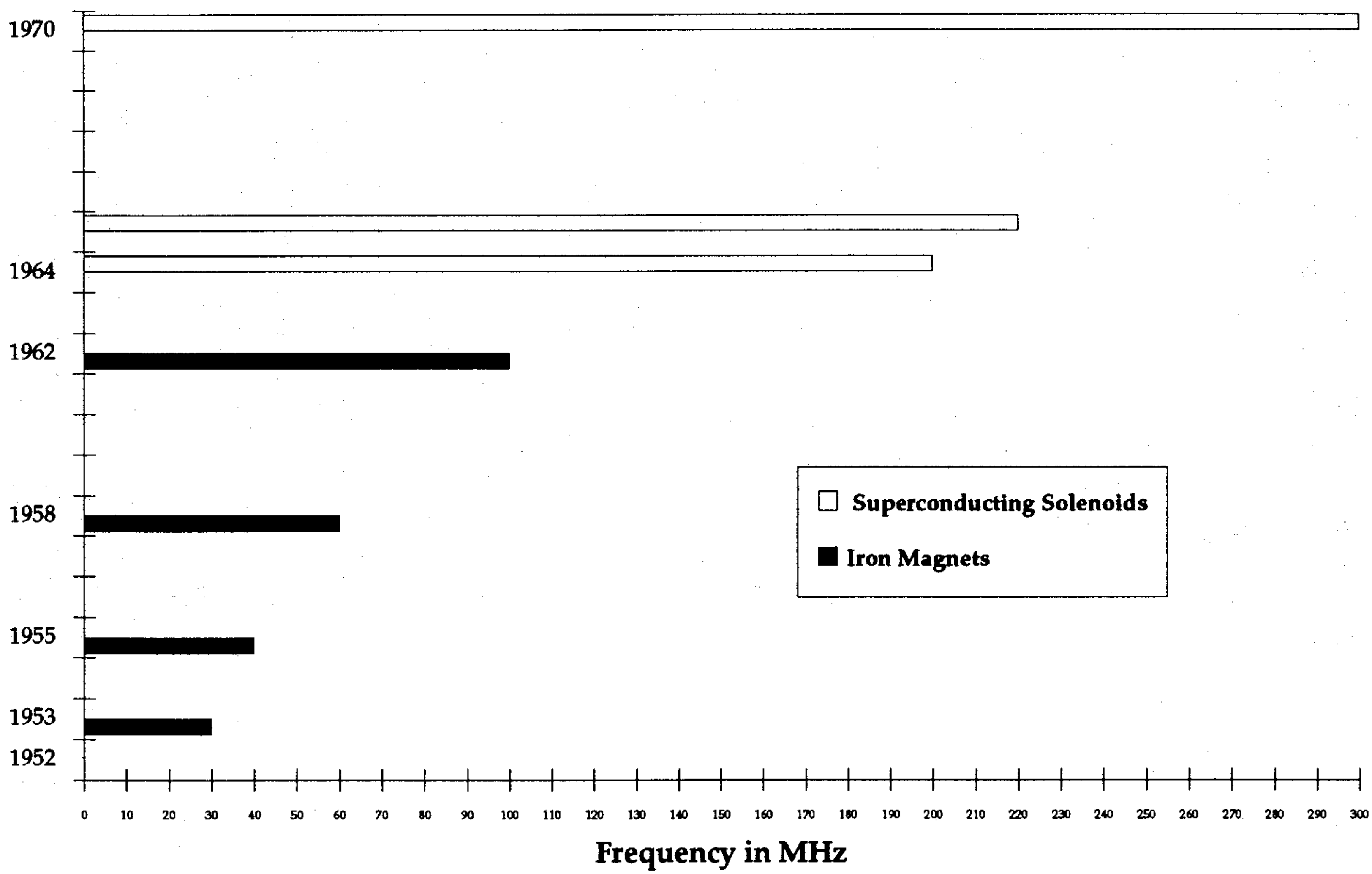


Figure 3. Varian spectrometer frequencies

> here again around the beginning of November, but it will evidently take some time after that until we are in full operation again. A third trick that has turned up between Anderson, Arnold, and me, while we were still in Geneva, is a kind of "magnetic clock," with which we hope to tie field and frequency together, so that the cumbersome problem of keeping them separately extremely constant can be bypassed. I am afraid it would take too long to describe to you in detail what we have in mind, and so far, the thing exists anyway only on paper. The combination of this device with improved spinning and pulsing of the rf should allow a major progress in the art of resolution. As soon as things begin to work, we shall communicate all details, which I am sure will be of interest to you. (Bloch 1955*c*)

Such exchange between physicists and Varian scientists was stimulated during the early 1950s and after his return from CERN through Bloch's seminar, to which Varian scientists were invited. The seminar alternated the site of its weekly meetings between Stanford and Varian Associates. Participants in that seminar recall lively exchange on issues related to NMR.[12]

But the immediate community was not the only source of stimulation in the early days of NMR at Varian. Their customers, too, suggested equipment modifications to Varian engineers. An example is the superstabilizer. Introduced in December 1956, this addition became a standard feature of NMR spectrometers after the HR 60 (V-4300-B), using iron magnets until superceded by the introduction of superconducting magnets in the 1960s. Researchers at Shell Oil's Houston laboratory suggested the superstabilizer concept to Varian. The idea had originated in discussions with Princeton physicist, R. H. Dicky (Packard 1980). In this way, networks of users contributed to the steady flow of innovations in the instrumentation.

NMR and Chemical Practice

From the perspective we are emphasizing in this paper, perhaps the most decisive set of contributions to the establishment of NMR as a new technique transforming the intellectual and technical practices of chemists was connected with the development of the Applications Laboratory at Varian under the direction of James Shoolery. While Varian researchers were making great improvements in NMR instrumentation, it was crucial to convince chemists of the indispensability of the

12. Personal communication from James Shoolery, Elliott Levinthal, and Martin Packard.

technique before a market of research and academic scientists could be created. Shoolery, a recent Ph.D. in chemistry from the California Institute of Technology, joined Varian in 1953. The lab he directed, oriented toward problem-solving methods as well as toward improvements in equipment design, was effectively a component of the marketing department. The goals were to make the results of NMR work interpretable to chemists, to devise new approaches for eliciting information from nuclear radiospectra, and above all to communicate this information to chemists. In the early days of NMR, chemists simply did not know about the techniques, and the amount of physics required to understand the methods was a forbidding obstacle. Chemists, always interested in new techniques for better solving their problems, needed to be shown that the investment required to learn how to use NMR instrumentation was worth it both intellectually and financially. At stake was the transformation of chemical practice. In addition to demonstrating convincingly the importance of NMR as a chemical tool, immediate obstacles to gaining entry into the chemists' labs were the cost, size, and unreliability of the instrument. The early NMR devices were indeed expensive. The first Varian spectrometer, for example, the HR-30, was purchased by Humble Oil for $30,000. Records of prices for all Varian spectrometers are no longer maintained by the company, but the price of the larger Varian spectrometers such as the HR-100 was approximately $40,000. Only very large research organizations, which included a few commercial firms (mostly oil companies), universities, and military labs, could afford one of these instruments. Moreover, as we have seen, the early NMR devices weighed several tons and could not easily fit into a standard chemical lab. The magnet for the Varian HR-30 weighed 5,400 pounds and required cooling water and heavy-duty (three-phase 220 volt) electrical service. The magnet of the HR-100 weighed nearly 8,000 pounds, and the floor of the typical laboratory had to be reinforced to accommodate it. For the instrument to gain acceptance Shoolery figured that it would have to compete with the new IR spectrometers just beginning to make their way into chemists' labs. Indeed, the concern to associate the new device with the IR spectrometer led Varian Associates to rename their instrument the "nuclear magnetic resonance spectrometer" rather than the "magnetic induction device," as Bloch and Hansen had called it in their patent on the induction principle. Varian designed its NMR instruments in order to ride the coattails of successful IR instruments, and they wanted their devices to have the same user-friendly character in the areas of display printouts, size, and general appearance. Furthermore, the instrument needed to have a footprint compatible with the projected chemist's lab of the 1960s.[13] In shaping the NMR spectrometer into a standard laboratory tool, Shoolery and his colleagues in the marketing division set their sights on producing an instrument that would cost $23,000, only slightly more than a high-end IR spectrometer, which in 1961 could be purchased for about $20,000. It took over a decade to design and manufacture a reliable instrument competitive with the IR spectrometers on the market (Shoolery 1993).[14]

Shoolery's foremost concern was to improve the instrument's sensitivity, its reliability, and the reproducibility of its results. As Shoolery indicates, the HR-30 was an operator's nightmare, requiring a considerable amount of "tweaking" on the part of a skillful operator. A lab assistant adjusted the field strength to locate the signals from the sample, while Shoolery acted as recorder, hurriedly writing down observations of the data as the beam swept across the oscilloscope. Here is his account of a typical procedure:

> After locating the signal from a sample of water, we had to place the probe (which contained the rf coils and sample) in the homogeneous spot in the magnet. That spot changed its shape, size, and location with changes in temperature and with the effects of hysteresis caused by turning the magnet on and off. The search was a hit-and-miss affair; there were many misses before scoring a hit.
>
> When the oscilloscope finally displayed a satisfactory pattern of decaying wiggles, denoting a narrow linewidth, we calibrated the sweep width by generating audiofrequency sidebands of the water signal. If we needed a permanent record, we took a polaroid photograph of the oscilloscope screen to establish the reference point of the chemical shift of water, and then quickly before the field could drift very far—we replaced the water sample with the sample being studied and took a second photograph. (Shoolery 1993, p. 734A)

One of the first improvements Shoolery introduced to the instrument was to replace the photographic technique with a high-speed chart recording printout that presented the NMR spectrum in a format close

13. For an example of the typical chemist's lab in 1965, see the excellent exhibit designed by Robert Bud at the Science Museum in London. The Varian A-60 is a prominent part of that display.

14. Personal communication from James Shoolery.

to those generated by IR instruments.[15] A number of other refinements followed in this period: higher field strengths; sample spinning to simplify the process of locating the point of field homogeneity as well as resolution improvement; field-gradient shimming with electric currents, which permitted the adjustment of field homogeneity at the sample; and use of degenerative feedback to stabilize magnetic flux (Shoolery 1993, p. 737A).[16] A further enhancement Shoolery made to the instrument was the introduction of a variable-frequency audio oscillator, which enabled the instrument to take advantage of spin decoupling. In 1955 Shoolery showed that the long life of nuclear spin states allows the chemist to operate on coupled systems and deduce which nuclei share common energy levels (Bloom and Shoolery 1955). The variable-frequency audio oscillator made it possible to irradiate a proton or group of protons strongly at any location in the spectrum while detection occurred for some other selected multiplet in the spectrum.

Shoolery initiated a number of programs to educate chemists about the power of NMR as an analytical tool and convince them to use it in their own research. In July 1953, Varian published the first of a series of Technical Information Bulletins. The Bulletin contained short, understandable technical articles on NMR and reproductions of interesting NMR spectra. These technical articles differed little from accessible technical articles in scientific journals. They were distributed to persons and organizations on an extensive mailing list that Varian shared with Hewlett-Packard. In his second year at Varian, Shoolery introduced the "NMR at Work" series, which appeared as a regular advertisement and information sheet on the back cover of the *Journal of the American Chemical Society.* These one-page notices described exemplary solutions of chemical problems using NMR, typically including an NMR spectrum and structural analysis of a chemical compound. The series eventually numbered over 100 solutions and were gathered to-

15. Marketing and machine esthetics played a significant role here as well. Shoolery notes:

> While highlighting the similarities between NMR and IR techniques, we still wanted to differentiate them. To that end, we established the practice of displaying NMR spectra as peaks with a positive deflection rather than as minima like IR spectra. The choice, we claimed, symbolized the vitality of the new technique. We referred to the fine structure arising from spin-spin coupling as a multiplet rather than an absorption band. It was years, however, before some organic chemists abandoned the term "NMR absorption band." (Shoolery 1993, p. 734A).

16. Shoolery notes that the magnetic flux stabilizer, which they called the Super Stabilizer, had the same effect on the acceptance of high-resolution NMR spectroscopy as the double-beam principle had on IR spectroscopy (Shoolery 1993, p. 737A).

gether and published as a separate catalog of spectra (Varian Associates 1960*a*). In 1962 Varian initiated a further series of NMR updates in the form of a newsletter directed at technicians and lab directors rather than research chemists.

In October 1957, the Instrument Division of Varian began its annual workshop series on NMR and EPR spectroscopy. More than 100 American and foreign scientists from industry, government agencies, universities, and research foundations attended the first four-day workshop. Convinced of the value of this workshop for disseminating the techniques of NMR, Varian Associates the next year extended the proceedings over an entire week and expanded the number of participants to 125, increasing it again to 160 the third year. The proceedings of the third annual workshop were published (Varian Associates 1960*b*).[17] To encourage European adoption of the instrument, a similar workshop was organized in Zurich during the 1960s and continued for a decade. Conducted like an academic course, with lectures and laboratory sessions, this effort became a major vehicle for informing chemists about NMR instrumentation. The workshop was divided into three parts. The first part was an introductory day of lectures on NMR and EPR for newcomers to the field. Forrest Nelson, Wayne Lockhart, and Robert C. Jones, from Varian, offered detailed explanations of NMR and EPR instrumentation. Hands-on laboratory instruction was provided. The second and third parts of the course were devoted to more advanced topics. In one section, James Shoolery offered step-by-step instruction in the use of NMR as a tool for structural analysis, the difficult business of translating spectral lines into chemical structures (Shoolery 1960*a*). He also schooled his colleagues in the use of "Shoolery's Rules" for calculating the alkyl proton frequencies in acyclic systems.[18]

17. In order to promote IR spectroscopy Van Zandt Williams at Perkin Elmer and the management at Baird had asked Richard Lord of MIT to organize a series of short courses in the 1950s. These highly successful courses were offered every summer at Bowdoin College. The Varian courses differed in being organized by company research scientists rather than by academics and in being offered at the company rather than at a college or university campus (Rabkin 1987, pp. 47–49; Lord 1989; Miller 1992, p. 827A).

18. In the absence of deshielding effects induced by neighboring atoms or groups, the protons of methyl groups are usually the most highly shielded of all organic types. Acyclic methylene groups are less shielded than methyl groups. Frequently it is necessary to consider shielding by more than one substitutent. In 1955, Shoolery and B. P. Dailey collaborated on a paper in which they established a close relation between the electronegativity of a substitutent X and the shielding of neighboring protons. Shoolery used this to develop a table of effective shielding constants to be used in the determination of the expected positions of methylene and methane protons (Dailey and Shoolery 1955). Shoolery's Rules are discussed in Jackman (1959, pp. 59–60).

In a further session of the workshop, Shoolery addressed the more advanced topic of using NMR as a quantitative analytical tool. Early achievements of the NMR field had provided convincing proof that the NMR spectrum of a compound is unique and can therefore indicate the presence of a particular compound in a sample. But for quantitative analytical work numerous factors determining the peaks in a spectrum had to be considered. Noting that application of the NMR technique had not been extensively investigated, the first textbook on NMR, published in 1959, offered one brief, seven-page chapter on quantitative analysis in a book of 500 pages (Pople, Schneider, and Bernstein 1959, pp. 458–65). An article published in the *Journal of the American Chemical Society* just prior to the 1959 workshop illustrates the issues Shoolery and his Varian colleagues faced in gaining acceptance for NMR. The article calls NMR spectroscopy "a powerful tool in the solution of qualitative analytical problems in organic chemistry, as in diverse other fields," but it goes on to register this qualification:

> One of the pressing problems in the application of this technique is the elaboration of a method for dissecting the intramolecular chemical shielding (as it would be measured in a single molecule) from the other influences affecting nmr line position, such as volume susceptibility, sample shape and orientation, solvent magnetic anisotropy and chemical association. A theory permitting the complete separation of these effects would be of great value in the application of nmr as an analytical tool. It would also facilitate further development of the theory of proton shielding in organic molecules. (Bothner-By and Naar-Colin 1958)

In his lecture and accompanying laboratory session, Shoolery addressed this issue by offering several examples of NMR in analytical work. In one example, Shoolery analyzed the total hydrogen content of samples of ethyl benzene, chloroform, toluene, p-dioxane, 17α-hydroxy-progesterone, and 3α-acetoxy pregnane. In a further case, he analyzed the tautomeric mixture of keto and enol forms of acetyl acetone. Through such examples, Shoolery sought to convince his audience that, "when quantitative work of high accuracy is desired, it can now also be regarded as within the scope of the high resolution NMR method. As a result, it seems likely that intensified efforts to make use of these capabilities will rapidly develop and progress in this area may surpass all expectations"(Shoolery 1960*a*, p. 139). His efforts went beyond demonstrating that it was possible in principle to solve such quantitative problems, however, materializing in a new accessory to the NMR spectrometer, the V-3521 NMR Integrator (Shoolery 1960*b*).

The underlying principle behind the integrator is that the strength of a NMR signal is proportional to the number of magnetic nuclei in the sample. Since each peak or multiplet resulting from chemical shifts and spin couplings in a high-resolution NMR spectrum represents one type of organic group, such as a benzene ring or methyl group, the area under the NMR peaks is directly proportional to the number of nuclei involved. By counting protons in the groups, a total number of protons in the molecule and an accurate picture of the structure of the molecule can be determined. Shoolery explained the theory behind the construction of the integrator, which used an audio-frequency phase-sensitive detector to select the absorption mode of the NMR signal and accumulated them in a RC integrator (Miller integrator). LeRoy Johnson explained the construction, including the circuit diagram, and operation of the integrator in one of the lab sessions of the NMR workshop (Johnson, LeRoy F., 1960).

Such examples show that Varian scientists actively engaged in scientific debates to gain acceptance of their new analytical tools. To be persuasive, Varian scientists had to have the respect of the scientific community, and indeed, as we have already seen, the founder generation of the company regarded as vital to its purposes the maintenance of close ties between Varian Associates and the academic community. The marketing of their instruments depended on contact persons who were not sales representatives lacking technical expertise. The idea was that through close ties with the academic community, Varian scientists could get their instruments adopted and positively affect the direction of scientific practice. Crucial in this was the notion that Varian scientists should publish their work. Shoolery and his colleagues discussed NMR at scientific meetings. In the early days of NMR a central question was whether to employ high-powered electromagnets or permanent magnets, which had limited field strength but greater homogeneity. While some competitors, such as H. S. Gutowsky and his coworkers, campaigned for permanent magnets, Varian scientists were committed to electromagnets. Scientific meetings, such as the meeting of the Faraday Society organized at Cambridge University on April 4–6, 1955, to discuss microwave and radio-frequency spectroscopy, provided occasions to debate the merits of various instrumental approaches. That meeting was particularly notable in that Shoolery, Anderson, and Arnold, all from Varian, gave papers on NMR, while Richard A. Ogg, a Stanford chemist, used the Varian spectrometer in his own contribution to the meeting. The meeting turned seriously around the issue of permanent magnets versus electromagnets, as the "General Discussion" shows: in an opening volley, Gutowsky maintained that the choice to employ a permanent or an electromagnet was simply a matter of personal preference (Anderson and Arnold 1955; [especially] Gutowsky 1955; Ogg and Ray 1955; Shoolery 1955). Between 1953 and 1956, Shoolery coauthored 17 papers discussing aspects of NMR. Shoolery collaborated on more than 150 such publications during his career. Between 1954 and 1957, Varian staff members published 33 articles on different aspects of NMR (Varian Associates 1957*b*). By 1960, Shoolery had made presentations on NMR to more than 20,000 persons, and Shoolery and members of the Varian applications lab had published more than 65 articles and two books on NMR (Shoolery 1993, p. 738A) Perhaps the most crucial of these was an extensive catalogue of 700 spectra, a reference source for initiating chemical work in the lab (Bhacca et al. 1962).

The years 1953–59, then, encompassed an intense period of research and development of NMR techniques and improvement in instrumentation. During this phase of their development, Varian spectrometers were instruments intended primarily for research scientists. But, as we have also seen, from the beginning Varian Associates was interested in the possibilities of developing a large commercial market for its instruments. In 1957, Shoolery and Emery Rogers, the head of the marketing department of Varian's Instrument Division, made a proposal for multiyear support for a four-year program to construct a commercial instrument, smaller and more affordable than the research instruments they had been developing. The goal of this effort, which culminated in the design of the A-60, introduced in 1961, was to create an instrument simple enough for any organic chemist or graduate student to operate with the aid of a manual, which would cost only slightly more than the high-end IR instruments. By 1958, with the introduction of the HR-60 (also known as the Varian Model V-4300B), the Varian NMR spectrometer was an extremely complex device. To this point, the NMR spectrometer had followed the path of development of most scientific instruments: namely, incremental improvements on an existing design. By 1958, a Varian spectrometer had an array of accessory items attached to it as performance-enhancing elements, including a superstabilizer, a rotating magnet, an NMR spin decoupler, an integrator, a strip-chart graphic recorder, a variety of shims and pole caps, and several different types of probes designed for eliciting different NMR signals. These components affected each other's operation. Moreover, to get reliable measurements one still had to recalibrate the machine on each run. One had to know a great deal about making the appropriate settings to achieve stability and control field homogeneity, temperature, the sweep unit, etc. In short, the instrument was a device for the research chemist with considerable knowledge of physics. A commercially viable NMR device would be an instrument that generated an identical result for a given sample each time on a precalibrated chart of values. To accomplish this goal the Varian engineers embarked on a new strategy of instrument design based on a team concept employing the notions of systems engineering (Varian Associates 1960*c*). A team of engineers, physicists, artists, and applications lab persons, under the leadership of John Moran, followed critical path analysis and Program Evaluation Review Techniques, or PERT, charts in coordinating the design and production of all the features, components, and constraints of the first commercial NMR spectrometer, the A-60. The resulting instrument was a streamlined device consisting of two components, the magnet and an operator console, that easily fit into an ordinary chemistry lab. The instrument was accompanied by the two-volume catalog of 700 spectra—produced by the A-60 itself—which served as a reference guide to begin the lab's own collection. The production run of the original A-60 was 125 instruments. By the end of the decade, more than 1,000 had been sold worldwide (see table 3). As Edwin Becker notes, "The A-60 spectrometer really brought NMR spectroscopy to the masses (of chemists)"(Becker 1993, p. 298A).

A further crucial strategy for improving instrumentation, maintaining contact with the academic world, and contributing to the dissemination of ideas about Varian work was a postdoctoral fellowship program and visiting fellow program (Packard 1988). Perhaps the most striking example of the way this program affected the practice of academic science is the collaboration that developed between Swiss postdoctoral fellow Richard Ernst and Varian's Weston Anderson. Varian scientists looking for ways to improve NMR for molecular structure determinations became aware of Ernst in 1963 through Warren Proctor, who was running the Varian applications lab at the Eidgenossische Technische Hochshule in Zurich. Ernst had done some work on NMR for his doctoral dissertation, and he was encouraged to come to Varian Palo Alto to study improvements of the A-60 NMR spectrometer. Ernst began working with an idea that had been patented by Russell Varian in the mid-1950s, which was to excite all the nuclei in a spectrum simultaneously, to perform a broadband spectrum analysis on all the nuclei, and to analyze the recorded response by a Fourier transform. Anderson had been trying to extend and develop this idea when Ernst arrived. The result of this collaboration was the introduction of standalone dedicated computers into the analysis of NMR data—the first

Table 3. Sales of Varian Spectrometers

Year	Instrument Sales ($)	Net Profit (%)	Sales of Specific Instruments ($)				
			Magnets	Spectrometers	EPR	A-60/56	Other NMR
1954	467,000	...	370,000	97,000	...	...	...
1955	756,000	...	553,000	203,000 [15]	...	...	...
1956	1,520,000	...	1,110,000	410,000 [33]	...	...	...
1957	3,000,000	...	1,837,000	1,163,000 [65]	...	...	...
1958	3,700,000	10.1	2,419,000	1,297,000 [65]	...	...	...
1959	4,600,000	10.6	2,872,000	1,518,000	...	...	...
1960	5,500,000	9.4	2,568,000	2,587,000	...	...	...
1961	6,600,000	6.9	NA	NA	...	... [120]	...
1962	10,100,000	6.5	1,396,000	8,381,000	...	...	...
1963	9,700,000	5.0	1,791,000	7,734,000	2,105,000	3,224,000 [137]	1,872,000
1964	13,342,000	5.5	2,422,000	9,033,000	2,782,000	3,947,000 [168]	2,304,000
1965	16,178,000	7.2	2,066,000	11,045,000	2,278,000	4,197,000 [178]	4,580,000
1966	20,400,000	8.0	...	...	...	...	...
1967	24,000,000	8.1	...	...	...	...	...
1968	20,600,000	...	...	...	...	...	...

SOURCES—M. Stearns, "Long Range Forecast," February 20, 1959, Minutes, Board of Directors, 1959; Budget Reports, 1960, 1962, 1963, 1964, 1965, 1967; Bancroft Library, MSS 73/65c, Carton 4. Numbers in brackets ([ ]) refer to number of instruments sold in a given year. Numbers of A-60 for 1963–65 are estimated on selling price of 1961 A-60 ($23,500). Numbers for 1957 and 1958 based on two-year total reported in memo of May 19, 1959, R. Kanes to H.M. Stearns, Bloch Papers, SC 90-099.

PDP-8 minicomputer—and the development of the fast Fourier Transform in the analysis of NMR peaks. We cannot follow those developments in this paper (Anderson 1992), but it is well known that this technique led to a new round of improvements in instrumentation that laid the basis for the field of medical resonance imaging (or MRI). For his work in this field, Ernst was awarded a Nobel Prize in 1991. In a period of forty years, knowledge transferred from academic setting to research park and back to university had succeeded in transforming chemistry, biochemistry, and medicine.

The success of these strategies can be measured by the chemical community's adoption of NMR and the role of Varian instruments in the generation of this new field of scientific work. Figures 4–9 illustrate

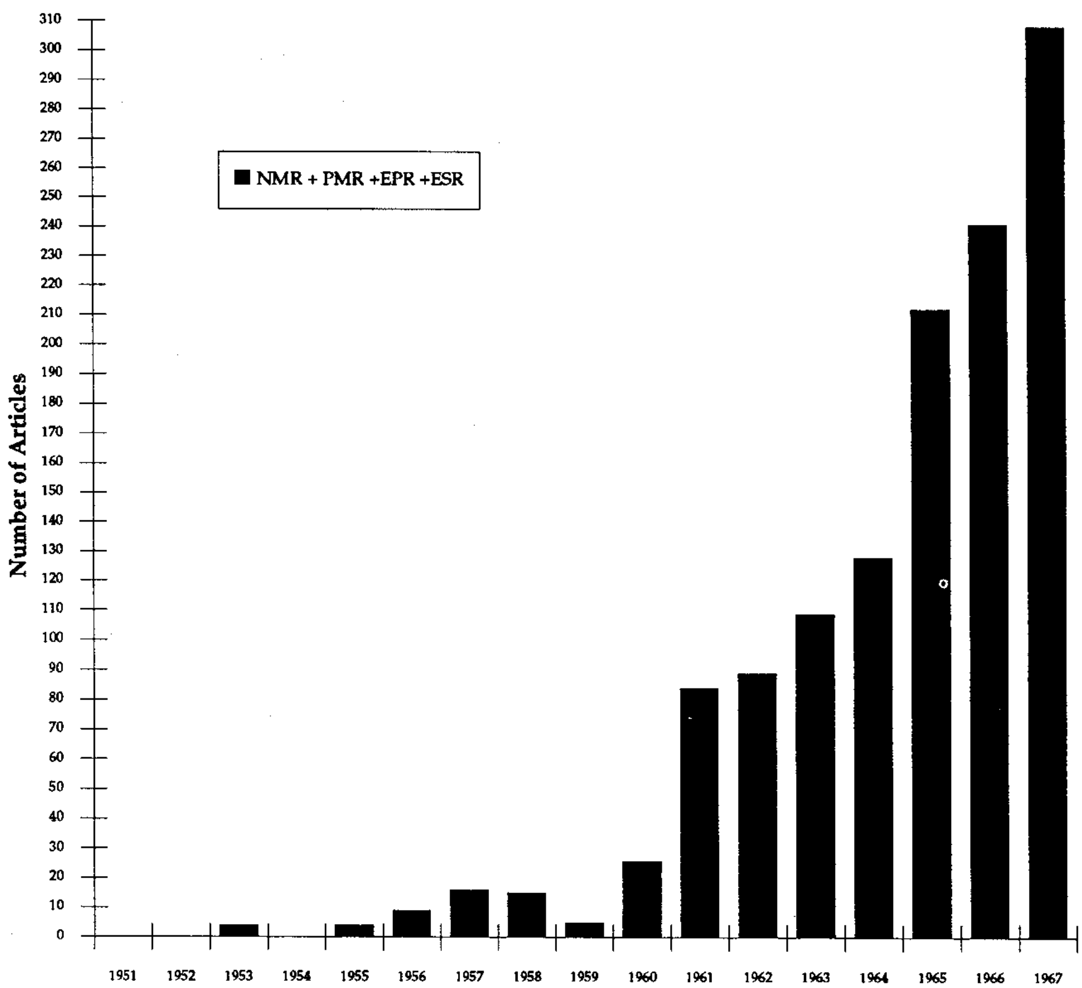


Figure 4. Growth of NMR in leading chemical journals. This graph depicts the number of articles appearing in the *Journal of the American Chemical Society* in which NMR was used.

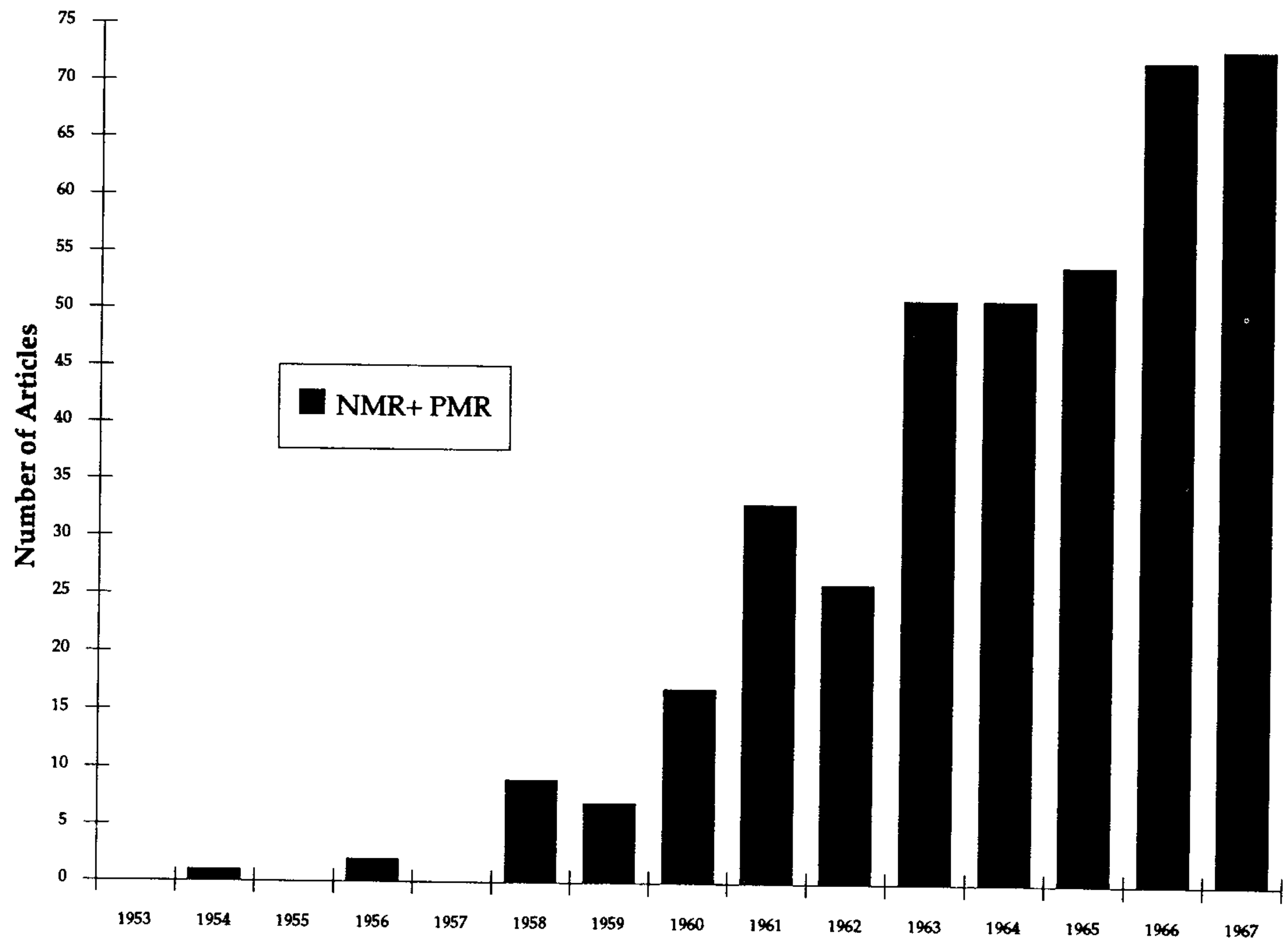


Figure 5. Growth of NMR in leading chemical journals. This graph depicts the number of articles appearing in the *Journal of the Chemical Society* in which NMR was used.

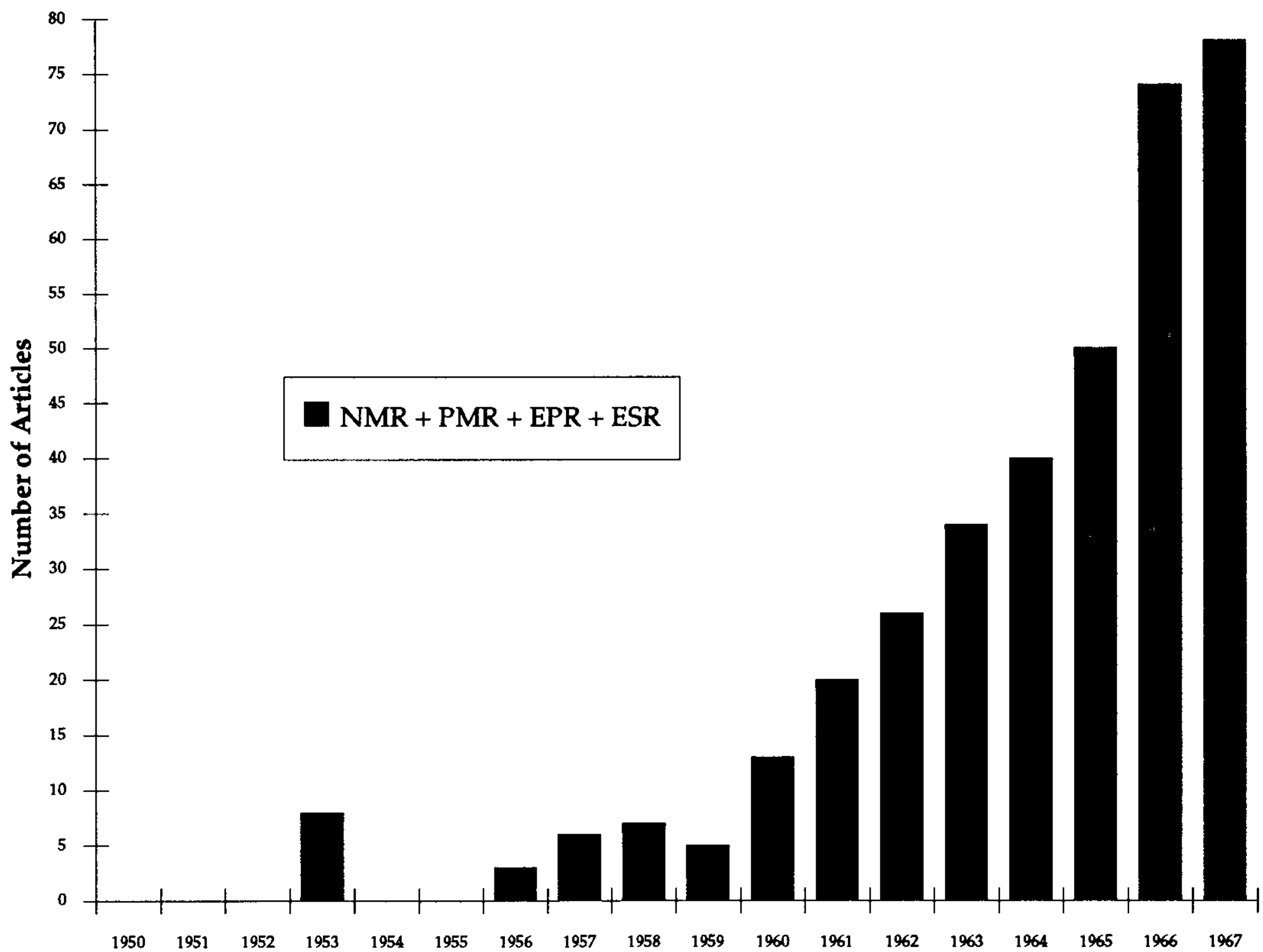


Figure 6. Growth of NMR in leading chemical journals. This graph depicts the number of articles appearing in the *Journal of Physical Chemistry* in which NMR was used.

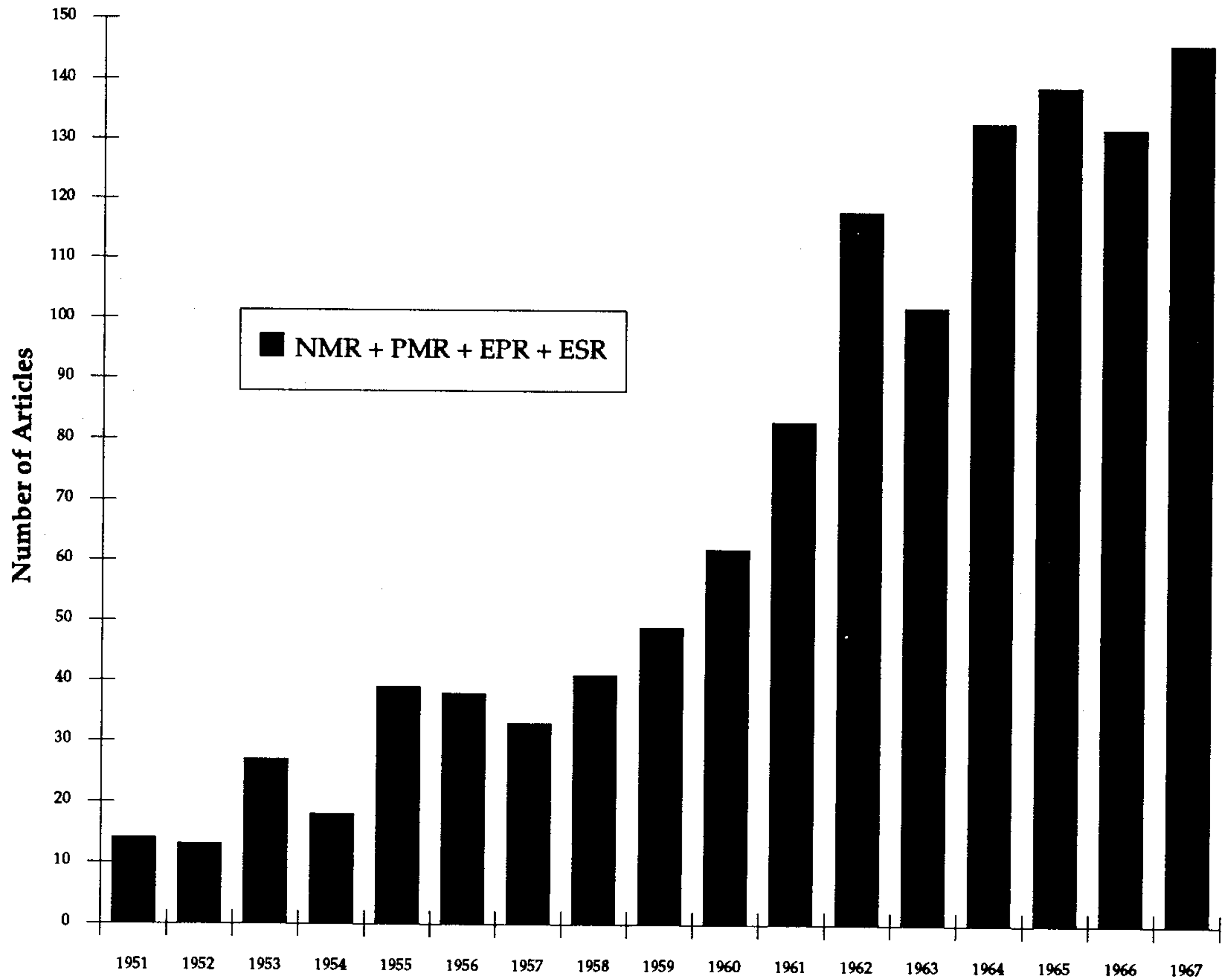


Figure 7. Growth of NMR in leading chemical journals. This graph depicts the number of articles appearing in the *Journal of Chemical Physics* in which NMR was used.

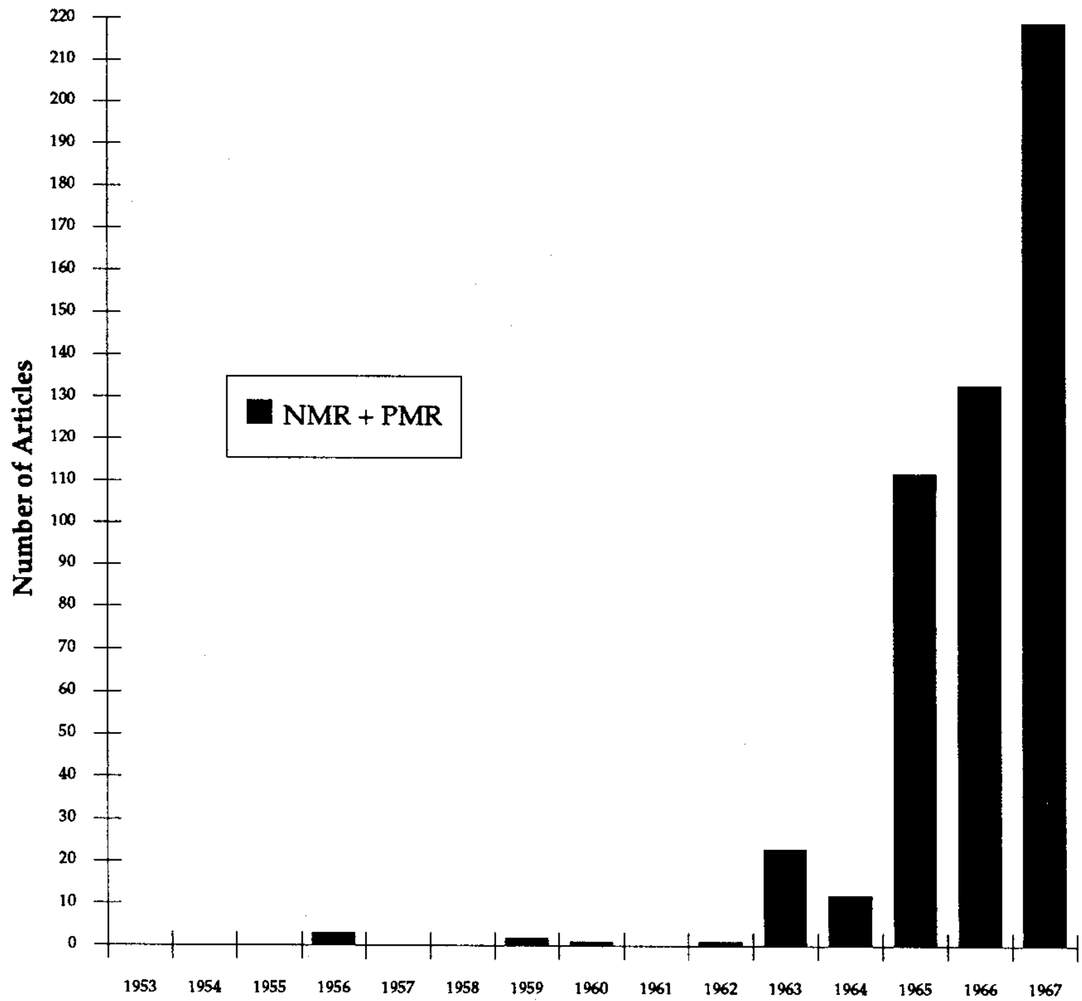


Figure 8. Growth of NMR in leading chemical journals. This graph depicts the number of articles appearing in the *Journal of Organic Chemistry* in which NMR was used.

the number of articles on NMR, proton magnetic resonance, and electron paramagnetic resonance in four major chemical journals, beginning with the publication of the first articles on NMR detection of chemical shift. The publications grew over three distinct periods (fig. 9). The period 1956–60 was characterized by an intense span of research activity on the part of a small community of industrial and academic scientists concerned with establishing NMR as a tool in chemistry. This period was marked by the appearance of the first comprehensive textbooks on NMR spectroscopy, particularly the important text by J. A. Pople, W. G. Schneider, and H. J. Bernstein, *High-Resolution Nuclear Magnetic Resonance* (1959). L. M. Jackman's book, *Applications of Nuclear Magnetic Resonance Spectroscopy in Organic Chemistry* (1959),

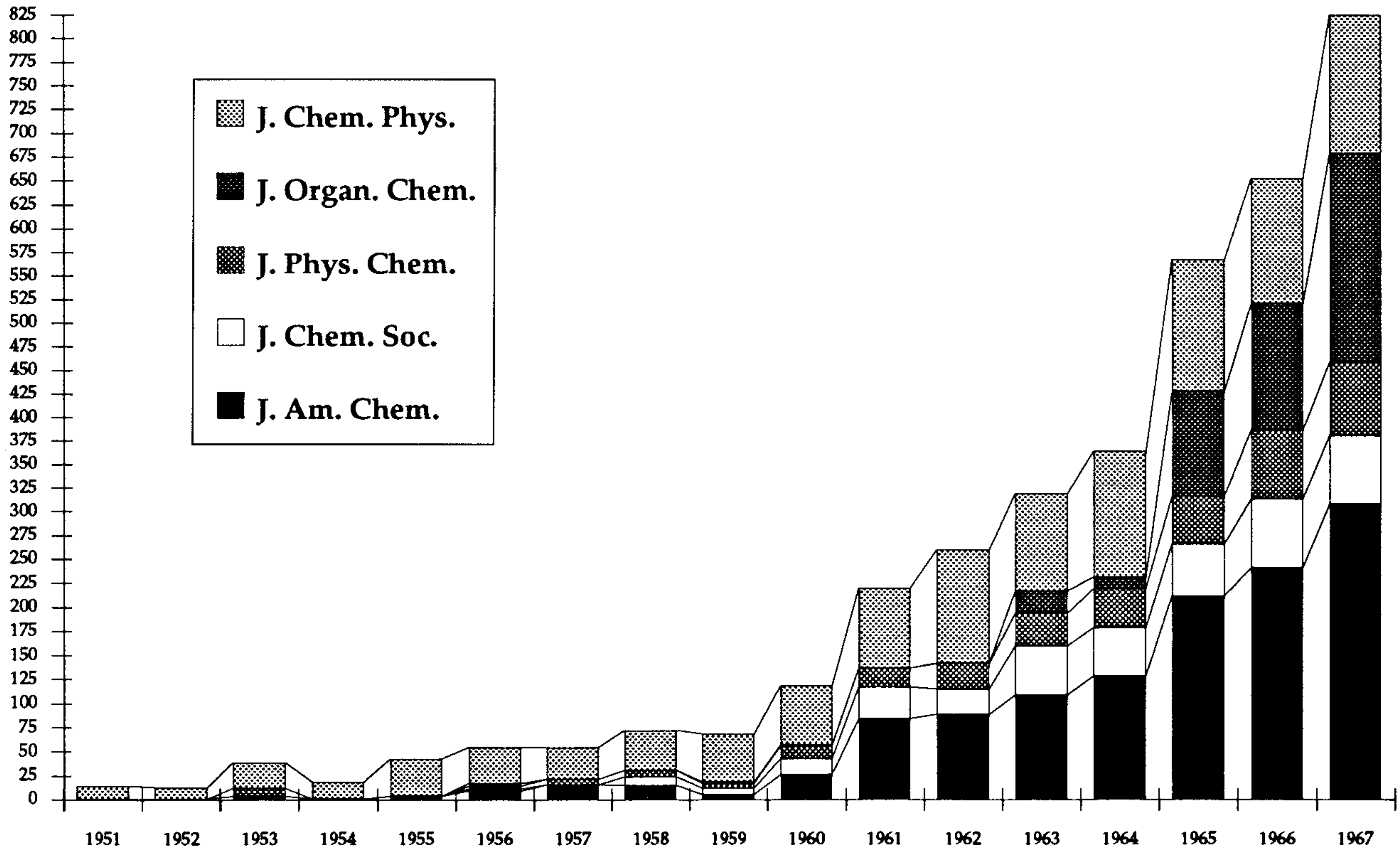


Figure 9. Growth of NMR in leading chemical journals. This graph depicts the cumulative growth of NMR in articles appearing in leading journals.

was the first survey text devoted to structure determination in organic chemistry with NMR. Another significant development of this period was the establishment, in 1957, of the *Journal of Molecular Spectroscopy.*

In his preface to Jackman's text, D. H. Barton, F.R.S., of Imperial College, London, noted that in 1958 applications of NMR to problems in structural organic chemistry were so few that organic chemistry departments could scarcely justify the purchase of expensive high resolution equipment. He continued: "Now, less than two years later, departments which do not possess an nmr spectrometer are at a considerable disadvantage relative to those where such facilities are available" (Jackman 1959, ix). In the preface to the second volume of their important survey text, *Determination of Organic Structures by Physical Methods,* written in October, 1961, F. C. Nachod and W. D. Phillips wrote that, whereas a single chapter had sufficed to cover the fields of NMR and electron-spin resonance (ESR) in the first volume (1955), advances in techniques for structure determination had been made at such a prodigious rate in the intervening six years that it was necessary to include six chapters on these subjects in their second volume, "because in the intervening six years, nuclear magnetic resonance, electron spin resonance, and quadrupole spectroscopies have taken their places as major tools in the elucidation of the geometrical and electronic structures of molecules" (Nachod and Phillips 1962, vii). Nachod and Phillips predicted that in the near future entire chapters would have to be devoted separately to resonances of many nuclei of the periodic table.

The second period of development, beginning in 1961 and ending around 1966, is represented on our graphs by a quantum leap of articles in the field of NMR or utilizing NMR as a technique. This marks the rapid and growing acceptance of NMR among chemists not specializing in NMR who used the instrument as a routine laboratory tool in chemical analysis, particularly organic chemists (note especially the dramatic shift in fig. 5). In this period, the Varian A-60 was introduced. Indeed, of the many articles we have examined from this sample, it is difficult to find any that did not utilize a Varian A-60 or Varian HR-100. In his review of the field of NMR for 1962–63, Saul Meiboom noted that

> the appearance in 1961 of the Varian A-60 spectrometer has greatly contributed to putting routine NMR spectroscopy within the reach of the average chemical laboratory. It is a specialized instrument designed primarily for the organic chemist and is suitable for proton high resolution work only, but its ease of oper-

> ation, superior stability, and repeatability put it in a class by itself. (Meiboom 1963, p. 335)[19]

In August of 1965, in a major review article of NMR, Frank A. Bovey of Bell Labs wrote that as a result of steady instrumental and theoretical improvements NMR spectroscopy had become a nearly indispensable tool to the organic chemist and was of major interest to the physical and theoretical chemist. Bovey suggested that the new physical methods in chemistry had revolutionized the field. If Emil Fischer had returned to an organic laboratory in the early 1940s, Bovey observed, he would have found very little unfamiliar in the analytical procedures. However, he continued:

> If Fischer were to return today, what bewildering changes he would find! IR and ultraviolet spectroscopy are now entirely routine and classical. The NMR spectrometer, unknown even in principle before World War II, holds the center of the stage. It alone can often provide a complete structure as well as conformational and kinetic information for which it would not have occurred to Fischer to ask. (Bovey 1965, p. 121)

In the third period we chart, beginning in 1966, NMR can be considered to have been fully established as a major chemical discipline. Reviewing the NMR literature for the years 1966–67, Richard Jones estimated that more than 4,000 papers containing significant NMR studies were published worldwide during the year (Jones 1968, p. 1). The demand on publication space had become so great that an entire journal devoted to NMR appeared in 1966: *Progress in NMR Spectroscopy. The Annual Review of NMR Spectroscopy* published its first issue in 1968, followed in short order by the initial number of *NMR* (1969).

Varian Associates had made a major impact on chemistry through the marketing of the first commercial NMR instrument, the A-60 (fig. 10). The strategy pursued over a ten-year period of investing in civilian commercial ventures had indeed paid off. As Edward Ginzton explained in the Varian Annual Report for 1966, the strategy had strongly affected the future direction of the company. Microwave tubes represented 73% of the company's total sales in 1962 but less than 35% of

19. Meiboom went on to emphasize the difference between this instrument and the other Varian instruments intended for research work in NMR: "The Varian 100 Mc high resolution spectrometer has about twice the sensitivity of the standard 60 Mc instrument, and of course also has the advantage of increased chemical shifts, facilitating spectrum interpretation. Its successful operation, however, appears to require more than average skill" (Meiboom 1963, pp. 335–36).

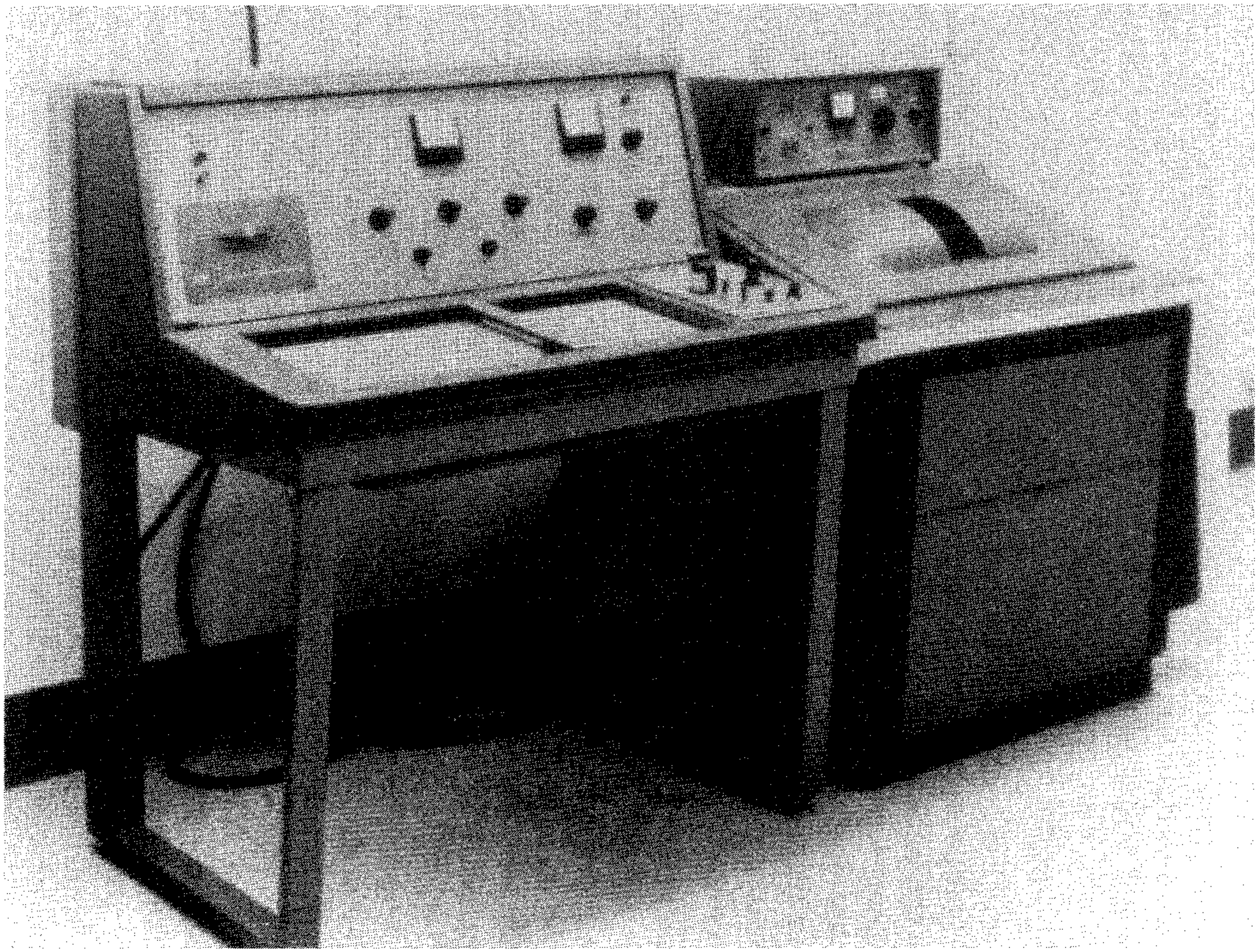


Figure 10. Varian A-60 NMR spectrometer

the total in 1966. The company had expanded its business in instruments and equipment to more than 40% of total sales. If this trend continued, Varian was on its way to being considered primarily an instrument company, a development that promised to make the company less dependent on military contracts in the future (Ginzton, in Varian Associates 1966, pp. 6–7). Their success in the area of commercial scientific instruments for chemistry encouraged Varian Associates to pursue this direction more intensely. In 1965, a merger with Wilkens Instrument and Research led to the formation of subsidiary Varian Aerograph, with approximately 350 persons specializing in instruments for organic chemistry, particularly in the area of gas chromatography:

> In addition to expanding the scope of our instrumentation activities, this action takes our Company one step further to a position of competence in an important area of science: chemistry. Since we are already strong in the fields of physics, electronics, and other technologies, the additional strength in chemistry will be of lasting and basic importance to the future development of your Company. (Varian Associates 1966, p. 10)

Ginzton believed that this new interaction between chemists and physicists in the company was "sure to produce a new generation of instruments and devices which neither organization was prepared to develop on its own" (Varian Associates 1966, p. 10).

While we have emphasized the role of industry scientists in largely creating NMR as a field of routine chemical analysis and thereby actively reshaping the practice of chemistry in academic settings, their success in marketing this new instrumentality depended crucially on both the desire of academic chemists for affordable new instruments of physical chemical analysis and on the availability of funding for those purposes. As much as companies like Varian Associates shaped the field of NMR, government funding from the National Science Foundation and the National Institutes of Health, responding to the Sputnik-era concern to train more scientists and engineers, was equally crucial in bringing about this transition. Edward Ginzton was clear about the fact that funding for science and engineering in the universities had permitted the company to diversify its products in commercial markets:

> Within the United States our markets are strongly influenced by three factors:
>
> 1. The general economic condition of the country and the resultant level of capital outlays by the industry.

> 2. Federal and state government expenditures in support of education, science, health and agriculture.
> 3. Federal expenditure for defense generally, and the unusual commitments in Asia.
> In the second area, the financial commitments by federal and state governments for education, science, health and agriculture are continuing at a high level, in part because of the President's support of the concept of "The Great Society." These fields are important to us as they invariably involve the use of our scientific and related instruments. As examples, the support of university education through research grants provided by the various agencies of the federal government represents to us an important market for our spectrometers and like equipment: the support of health provides us with opportunities to increase the use of our clinical accelerators for cancer therapy. For several years we have observed significant increases in government expenditures for these and related fields, and our participation in these markets continues to be important and successful. (Varian Associates 1966, pp. 10–11)

A National Research Council study completed in 1965—a study in which Varian scientists were consulted—gives a concrete picture of the role of government spending in creating both the market for scientific instrumentation in educational institutions to which Ginzton refers above (Committee for the Survey of Chemistry 1965),[20] and, by virtue of this connection, the condition for creating the institutional infrastructure crucial for the establishment of NMR as a chemical discipline in American universities during the period we have examined. The study underscores that the major source of the explosive developments of chemistry since World War II had been the introduction of new instrumentation, some of which, such as NMR, had been based on physical principles unknown prior to the 1940s. To document their claim the NRC committee conducted a study of the growth in use of physical instrumentation in chemical literature from the period 1952–64 (fig. 11). During this twelve-year period, the chemical literature more than doubled, according to the Chemical Abstracts Service, from 18,540 articles in 1951 to 40,048 papers published in 1963. Surveying a sample of more than 3,000 publications from this literature, the committee concluded from the "use-rate"—the number of instances of use cited per 100 papers reviewed—that the seven classes of major instruments accounted for 80% of the growth of the chemical literature.

20. James N. Shoolery of Varian was cited on p. 94 n.

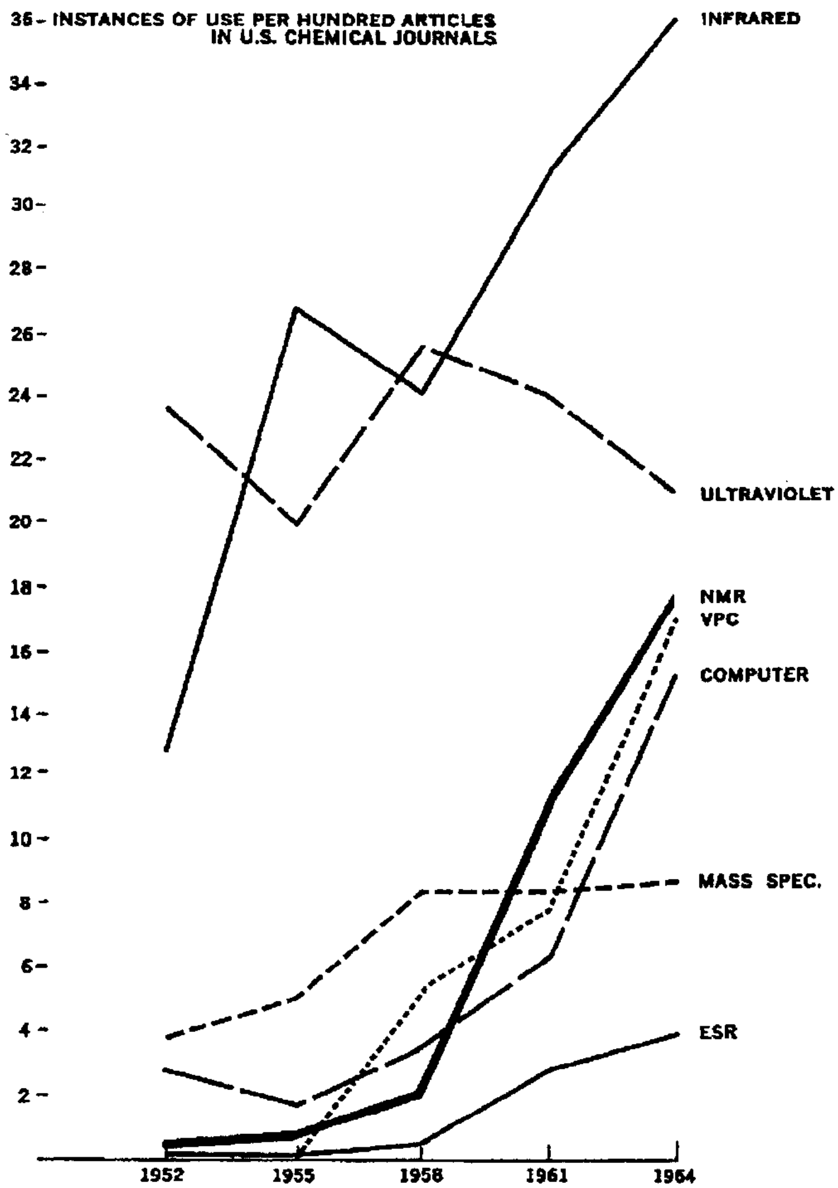


Figure 11. Growth in use of chemical instrumentation. Source: Committee for the Survey of Chemistry, National Academy of Sciences National Research Council, *Chemistry Opportunities and Needs* (Washington, D.C.: 1965), p. 88.

Indeed, even more striking evidence of the importance of the role of the new chemical instruments in the growth of chemistry, the data revealed a total use-rate of 119 for all seven instruments in the literature for 1964. This meant that in each paper *at least one,* and for many papers two or more, major chemical instruments were employed (Committee for the Survey of Chemistry 1965, pp. 86–87). The charted results of the NRC data search reveals that beginning around 1958, NMR and ESR exploded onto the scene as major factors in the "chemical revolution" of the 1950s and 1960s.

Surveying the top 125 U.S. (Ph.D.-granting) chemistry departments, the study determined that investment in major instrumentation (defined by the committee as an instrument costing more than $2,000, including x-ray, mass, IR, UV, and NMR spectrometers and vapor-phase chromatographs) at those institutions between 1954 and 1959 had been $14 million and that the investment in instrumentation between 1960 and 1964 had been $36 million, a 20%–25% annual increase in spending on instruments. This was the market to which Ginzton refers in his annual report above, and the source of these funds for universities had been primarily the National Institutes of Health, the National Science Foundation, and the Atomic Energy Commission. Not only had instrumentation expanded over the decade, but laboratory space had grown by 53%, and their survey of universities indicates that building plans for immediate expansion of laboratory space by an additional 50% had already been made (Committee for the Survey of Chemistry 1965, p. 180, table 26). Over the decade between 1954 and 1964, expenditures for basic chemical research had grown by 15% annually, most of that increase going toward the support of instrumentation, technical staff needed to service such instruments, and postdoctoral students, the main work force employing the new generation of instruments (Committee for the Survey of Chemistry 1965, p. 180, table 26).[21] Surveys conducted by the committee revealed that, in spite of the growth in support for basic chemical research, there had actually been underinvestment in instrumentation (Stine 1992). The backlog in needs for chemical instrumentation in university laboratories was estimated at $20–$35 million. The committee estimated $11 million would be spent on major chemical instruments in 1964. In their report the NRC recommended an expansion by 20% annually in the budgets for chemical instrumentation of federal agencies supporting basic chemical research and a special expenditure of $7 million per

21. See especially Committee for the Survey of Chemistry 1965, fig. 30, p. 176, Numbers of Research and Supporting Personnel in University Chemistry Departments, and tables 29–30 for projected figures. The data collected by the Committee showed that, while the total number of Ph.D.'s awarded had remained constant (1,000) between 1953–54 and 1958–59 and risen to 1,300 between 1958–59 and 1963–64, the number of postdoctoral students during the same period had risen from 510 in 1953–54 to 1,800 in 1963–64. On the basis of trends extrapolated from data collected from 1960–63, the Committee predicted future investment in instrumentation to grow by 19% annually and an 18% increase in support personnel.

year for a period of three years over and above the budgeted amount to meet the deficit in instrumentation. The committee recommended spending $18.5 million on instruments (not including computers or special instrumentation projects) for universities in 1966, $20.8 million in 1967, and $23.6 million in 1968. If these recommendations were to be accepted by the federal agencies, the market Ginzton and Varian Associates could anticipate for the future of NMR was handsome indeed.

Conclusion: Industries, Instruments, and Disciplines

Too frequently discussions of university-industry relations characterize the flow of knowledge as unidirectional, from the university to industry. The university in this model provides basic, general research, and industry applies it. We have focused on one example that suggests that not only academic scientists are engaged in basic research; rather, given the increasingly instrumental character of knowledge production, much basic research is conducted in firms such as Varian. We have emphasized the importance of local, institutional cultures and a set of compatible values as crucial to making possible the exchange of ideas, machines, and techniques, and we have tried to interrogate the stereotype of "academic" values, suggesting that these are not found exclusively (or sometimes even primarily) in the university but rather often in an alternative or more extended community. Moreover, we have tried to modulate the image of profit-seeking industrialist raiding the university for ideas. In the case of NMR, high-powered, and, later, superconducting magnets, we have found profit-seeking motives at times surprisingly absent and at other times equally distributed on both sides of the university boundaries. We have suggested that NMR, as a discipline of technology, science, and knowledge production, was invented in large part by Varian scientists. To be sure, they built a machine; but more importantly, they produced the interpretive techniques and practices that made it an instrument. Equally crucially, through their educational and promotional activities, Varian scientists helped transform the discipline of chemistry in the university. This pattern could be demonstrated with other examples. Recently Nicholas Rasmussen has found a similar theme in the invention and development of the electron microscope. There it was not a start-up company but RCA that helped create the ancillary techniques that made the machine instrumental (Rasmussen, in press).

In this discussion we have concentrated on Varian's relations with the Stanford Physics Department. This focus is certainly too limited. To capture a fuller sense of how universities and industries interact, we must expand the view beyond dyadic relations to look at the networks of knowledge production in a local region. For example, in the development of NMR relationships of exchange, cross-licensing, and contract existed between Varian and companies such as Hewlett-Packard and Fairchild Semiconductor. In recent papers, Rebecca Henderson, Adam Jaffe, and Manuel Trachtenberg have argued for the importance of such connections and knowledge spillovers (Trajtenberg 1990; Blume 1992, pp. 38–73; Jaffe, Henderson, and Trajtenberg 1993) as has Allen Scott in his studies of high-technology regions (Scott 1993). Their findings are underscored in our study by, among other things, the flow of personnel *from industry back to academe:* for example, in his survey of the career trajectories of 28 persons involved in the "take-off" of NMR at Varian, but who later left the company, Martin Packard noted that 28% had left to take up academic positions; 46% joined start-up companies; and 18% joined other major instrument companies (Packard 1980, p. 18). The present case study suggests the importance of these findings for rethinking university-industry relations in light of the distributed character of technical and cultural components of knowledge production. As the example of Varian Associates' development of NMR shows, industry scientists and engineers should be thought of as more than makers of devices; their success depends in large part on their ability to build disciplines.

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