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Scientific Teams and Scientific Laboratories

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ALVIN M. WEINBERG

## Scientific Teams and Scientific Laboratories<sup>1</sup>

SCIENTIFIC TRUTHS discovered in one age are essential for scientific progress in another: the laws of thermodynamics, discovered in the nineteenth century, will remain relevant and necessary for the scientist of the twenty-second century. Similarly scientific truth discovered in one place is required for scientific progress elsewhere: Lord Rutherford's experiments at Manchester on the scattering of alpha particles led eventually to the prolific investigation of nuclear phenomena throughout the world. To paraphrase Alfred Korzybski, man the scientist is both a time-binder and a space-binder.

In this sense science has always been a cumulative, team activity, more than, say, the arts or literature.<sup>2</sup> To be sure, great individual geniuses, like Newton or Maxwell or Darwin, create the revolutions that punctuate scientific progress. (T. S. Kuhn, in his *The Structure of Scientific Revolutions*, calls these turning points in science "paradigm-breaking."<sup>3</sup> I shall refer to them, along with the more modest "important discoveries," simply as "breakthroughs.") Yet the connections of even such individual geniuses with their predecessors and their contemporaries are surely more direct and demonstrable than is the connection between Beethoven and Mozart, or Picasso and Renoir. As Newton wrote to Robert Hooke, "If I have seen further (than you and Descartes) it is by standing upon the shoulders of giants."<sup>4</sup>

Nineteenth-century science was mainly conducted by geographically isolated, though intellectually interacting, individuals; much of today's science is conducted by large interdisciplinary teams. These teams often center around pieces of expensive equipment and are then said to be part of "big science." Team science is characteristically conducted in the large multipurpose scientific laboratory, an institution that is predominantly a phenomenon of World War II and after. My purpose will be first to trace the origins of big team science and to examine its multipurpose institutions, second to estimate the capacity of this new scientific style to launch and carry off the scientific breakthroughs so necessary

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for the progress of science, and finally to speculate on the future of team research and its institutions.

### *I. The Origins of Big Team Science*

The emergence of the large interdisciplinary scientific team as the landmark of science can be traced to at least three separate developments. First is the extraordinary growth of science and the resulting increase in the amount of scientific information produced; second is the emergence and institutionalization of applied science; third, and possibly most important, is the increasing complexity of scientific machinery.

#### *A. The Information Crisis and the Rise of Team Science*

The scientific information explosion has caused scientists to become more specialized. Some scientists respond to the information crisis by confining their range of scientific undertakings to those over which they can still retain command of the relevant information sources. Others form interdisciplinary teams in which are represented different though overlapping ranges of expertise or technique. In principle, the problems that can be tackled successfully by such teams ought to be more complex than those tackled by individuals.

This trend in the sociology of science was foreshadowed in an essay, "The Limits of Science," by Eugene Wigner in 1950.<sup>5</sup> Wigner argued that, for the reason I have mentioned, team research in which individual scientists are orchestrated into a productive whole by a scientific leader would become more common. He then asked how this new social structure would change the course of science. Could the theory of relativity or the Schrödinger equation have been discovered by an interdisciplinary team? Or, for that matter, could the mysteries of the "omega-minus" particle and violation of charge-parity invariance (both discoveries of teams of high-energy physicists) have been unearthed by the typical individual scientist of the nineteenth century? I shall return to these questions later.

The information explosion has been the subject of many essays and studies. Here I will mention only how the spawning of the scientific information specialist has affected the organization of scientific research. In previous generations the scientist gathered his information more or less on his own and rather haphazardly. Today scientists of course continue to browse in this manner, but they are now backed by a host of information services, ranging from libraries to abstract services and specialized information centers.

Already one can see the considerable influence of the information

specialists on those fields of science such as nuclear physics and high-energy physics where the spectroscopy<sup>6</sup> has become so elaborate as to outrun any single individual's capacity to hold all the relevant data in his mind. As a result, much of the output of the nuclear or high-energy spectroscopist goes to a secondary source—such as K. Way's or A. H. Rosenfeld's centers—where the data are compiled and collated. But in the process the role of the individual scientist who first made the measurements is weakened; the citation now often tends to be to the secondary source rather than to the original experimenter. Could this mean that one of the delicious joys and motivations of science—recognition and approbation by one's peers—will be attenuated? Parts of basic science have already acquired some of the facelessness that characterizes applied science, and this trend, it seems to me, will continue as the information crisis deepens.<sup>7</sup>

### *B. The Emergence of Applied Science and the Large Industrial Laboratory*

A second source of the trend toward team science is the rise of applied science and particularly the growth of the large industrial laboratory. Here the interdisciplinary team has predominated from the first, for reasons that are implicit in the strategy of science—that is, the way that scientists choose what they do. To make this point clearer, I shall digress to consider the strategy of scientific research.

Science is the “art of the soluble” according to Peter Medawar.<sup>8</sup> What a scientist does is largely determined by what he thinks he can do successfully. According to this view, science is a meandering stream that pushes salients out wherever the bank is weak and can be conquered; that such meandering may lengthen and make more tortuous the path to the sea is somewhat irrelevant. The river valley (to push the metaphor) is irrigated more heavily, and becomes greener, as a consequence of the meandering.

Insofar as Medawar is referring to basic science, his view of science as the art of the soluble contains much truth. In basic science, the scientist's criteria for deciding what he ought to try are usually internally generated; that is, they derive from the internal logic of the specific field in which he works and from his assessment of how soluble the problem is. Moreover, in basic science success is achieved if one solves the problem he sets out to solve, if he solves a different problem, or even if he can show that a particular approach is unfruitful. For all these reasons, in basic research it is acceptable to tailor problems to one's capacity for solving them. An expert in nuclear magnetic resonance can confine his researches to that segment of the field of nuclear magnetic resonance

over which he can comfortably retain command. Thus basic science (at least before the advent of the big machine) with its internally generated problems, can be pursued adequately within a narrow discipline. If the problem takes the researcher out of his specialty, he is still observing the canons of pure science if he turns to a different problem that is more easily accommodated by his interest and competence. It is for this reason that much of basic science can remain disciplinary and little: the interdisciplinary team is not its social characteristic.

Of course, even as a description of basic little science this is oversimplified; to characterize science as the art of the soluble tells only part of the story. Basic science, at its best, is the art of the soluble *and the important* (as Medawar himself recognizes). Researchers, even poor ones, usually have more ideas than they have resources with which to pursue them; their research strategy always amounts to choosing, from among a variety of soluble problems, the ones they regard as important. What constitutes importance in science? One, though certainly not the only, criterion is the degree to which a given piece of science relates to neighboring sciences. Indeed, the motivation for much basic scientific activity originates outside that activity. Sometimes the motivation lies in a neighboring basic science. For instance, a nuclear physicist may study light-element reactions because these are needed by an astrophysicist who wants to understand the mechanism of stellar evolution. Sometimes the motivation lies in technology: a physicist may investigate the basic properties of plasmas because of their relevance to the controlled release of thermonuclear energy. But the main point is that as soon as a scientist ventures to deal with a question arising in a field outside his discipline he has less control over where to look for a solution. He no longer has the luxury of narrowing the problem to what is soluble with his own expertise. Externally motivated science tends to be interdisciplinary and therefore more of a team activity than internally motivated science.

Applied science is externally motivated par excellence. Its questions are posed from without: from engineering, military, and even social demands. Such questions usually transcend the individual disciplines. The criterion of success in applied science is simply, "Does it work?" not "Does it add to knowledge in a particular discipline?" Thus applied science is characteristically interdisciplinary; it lends itself to—in fact it almost requires—teams of interacting individuals, none of whom by himself commands all the knowledge necessary to make progress, but all of whom, when taken as a whole, hopefully do.

It is therefore no accident that the great institutions of applied science in industry and in government are typically homes for interdisciplinary teams. The jobs of these institutions are set outside the

disciplines, even outside science; in consequence their style is interdisciplinary. Though the first of these laboratories, such as the General Electric Research Laboratory, Bell Telephone Laboratories, and the National Bureau of Standards, appeared around the turn of this century, there was an enormous development of them during and after World War II. The best known of the wartime laboratories were the Radiation Laboratory at the Massachusetts Institute of Technology, which developed radar, and the Metallurgical and Los Alamos laboratories of the Manhattan Project, which developed the atomic bomb. My own experience has been almost entirely confined to the atomic energy laboratories, and so I shall draw largely on them to illustrate some characteristics of the big applied scientific institutions.

From its very beginning in late 1941, the Chicago Metallurgical Laboratory, at which the first fission chain reaction was established, was interdisciplinary. Arthur H. Compton, the director of the Metallurgical Project, realized that the technology of the chain reactor would require physicists, mathematicians, chemists, instrument experts, metallurgists, biologists, and the various engineers who could translate these scientists' findings into practice. The chain reactor was much more than a nuclear physicist's experiment. Uranium to fuel the first reactor had to be purified and reduced to metal. Graphite of unprecedented purity was needed to moderate the neutrons. The chemistry of the new element plutonium was largely unknown. The production of plutonium was very hazardous, and the most sophisticated instruments were needed to keep everything under control. The biological effects of the radiation that would be released had to be assessed if not mitigated.

The difference between most interdisciplinary engineering enterprises and the engineering at the Metallurgical Laboratory lay in the incredible speed with which the latest scientific findings at the Laboratory were converted into engineered chain reactors. Eugene P. Wigner, who headed the theoretical physics group, began to engineer the water-cooled Hanford reactors in early 1942, almost ten months *before* the first chain reaction had been established.

There was nothing very complicated or obscure about the function and purpose of the Metallurgical Laboratory. Its output was a specific gadget and a specific process: the nuclear chain reactor and the production and extraction of plutonium. If the reactor succeeded, the Laboratory succeeded; if it failed, the Laboratory failed. Because of this singleness of purpose, which at least for the first two years was evident to all, there was remarkably little difficulty in forging the teams necessary to get on with the job.

Like most institutions of this sort, the Laboratory was organized into divisions. Enrico Fermi and Eugene Wigner were in charge of the physicists; James Frank and then Sam Allison and Farrington Daniels,



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in charge of the chemists; Charles Cooper, the engineers; and so on. But the over-all project overwhelmed the disciplinary divisions. This was relatively easy because everyone knew the stakes; one could readily submerge his personal aspirations for the sake of achieving the whole objective. This is not to say that there was no tension between the project and the divisions (that is, the disciplines). Even in the dark days of 1943 one could find physicists at the Metallurgical Laboratory working on the spherical harmonic method of solution of the Boltzmann equation (an activity that at the time seemed like an unjustified luxury) instead of estimating more routinely the multiplication constant of the latest reactor design.

This criss-cross organization—with each scientist having a permanent home in a division but being lent out temporarily to an interdisciplinary project—is the usual organization in applied laboratories. The project leaders generally control the funds; the division leaders, the people. The projects maintain pressure on the division managers to keep their outlook and activities relevant as judged by the projects; the disciplinary divisions maintain pressure on the project managers to keep their activities up to the standards of sophistication imposed by the divisions. It is hoped that out of this criss-cross tension between project and division there will come both relevance and sophistication.

The Metallurgical Laboratory was hierarchical. Arthur Compton was boss, but there were many other managers at lower levels, each on top of a pyramid of lesser and usually younger scientists. This pyramidal structure gave very great power to the man on top: he could command information resources; he could order investigations in many directions that would be out of the question had he not had a team at his disposal. In such hierarchical scientific teams, the members lower down must submerge their personalities and to some extent their scientific instincts to those of the boss. One therefore finds genius in such organizations less often than in the universities, where science is conducted more individually. On the other hand, a really good man in a position at the top of a pyramid obviously can get much more done than he can if he works in the usual university setting. Glenn Seaborg at the Metallurgical Laboratory had about thirty chemists working for him, and in only two years his group elucidated much of the chemistry of plutonium in addition to developing a process for extracting plutonium that was used successfully at Hanford!

Though there were many scintillating talents around—Szilard and Fermi and Wigner and Seaborg—the decisions were finally made by Compton. Yet, as in any organization, those with enough energy, confidence, and ability could impose their views in the face of official rejection. At the Metallurgical Laboratory, a showdown of this sort occurred during 1942. The issue was the coolant—and therefore the whole en-

gineering design—of the Hanford plutonium-producing reactors. The prevailing view held that since helium absorbed no neutrons, helium should be used to cool the reactors. With this view Wigner disagreed vigorously; he wanted to cool the reactors with water. To him, the handling of hot and somewhat radioactive helium under pressure seemed much more serious than the loss of nuclear performance caused by the tendency of hydrogen to absorb neutrons. In arguing his case, Wigner commanded all the relevant elements of knowledge—the engineering, the chemistry, the metallurgy, and the physics. And, when the Hanford reactors were actually to be built, the DuPont engineers chose the water-cooled pile rather than the original helium-cooled version.

Writing about these events twenty-seven years after they occurred, I am struck not by their uniqueness but by their generality. The Metallurgical Laboratory, in Anthony Downs's terminology,<sup>9</sup> was a bureaucracy—that is, a large organization that is not governed, or is only indirectly governed, by the feedback from the marketplace. In this sense almost every large laboratory, even if it is part of a big corporation, is a bureaucracy; its connection with the marketplace is usually tenuous. Many of the organizational features and sources of power in the large laboratory are not characteristic specifically of a scientific establishment but rather of any large nonmarket establishment. The hierarchical structure, the possibility of the energetic individual prevailing against the official position, the great logistic power of a big laboratory,<sup>10</sup> above all, the urgent imperative of the various groups to survive and to expand—all these are obvious to students of large organizations, scientific or otherwise.

### *C. The Influence of the Big Scientific Machine*

The third thread in the development of big team science goes back for some of its spirit to the explorations of the fifteenth and sixteenth centuries. To a degree, we would have to regard the great explorers as geographers, and hence scientists of sorts. Their enterprises were on a grand scale by the standards of their time; they required large teams and much money. And at least Columbus among them politicked with John of Portugal and Queen Isabella in much the same way that a promoter of a large accelerator must now politick with the Atomic Energy Commission or the National Science Foundation, or even with the President himself, to sell his project.

Many of today's explorations in basic science involve such elaborate and expensive pieces of hardware that the whole enterprise requires much the same mobilization of resources as was required by the explorers. The most extreme example today of huge mobilization of re-



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sources for a purpose that is at least partly scientific is the exploration of space. And even before we began to use rockets, earth-based astronomy had some of the attributes of modern big science: the 200-inch Hale telescope at Mount Palomar, completed in 1948, was one of the largest and most expensive pieces of scientific machinery until the advent of the large research reactors and large accelerators during and after the war.

The new style of big science based on very large pieces of equipment is generally attributed to Ernest O. Lawrence. His 37-inch cyclotron at Berkeley was a monster for its time; this was followed by the 60-inch, the 184-inch, the synchrocyclotron, the proton synchrotron (Bevatron), in ever-increasing size and complexity. To be sure, there had been earlier scientific teams dominated by great leaders: J. J. Thomson and later Rutherford at the Cavendish Laboratory, Fermi and his neutron group in Rome, and, of course, the German institutes. But Lawrence's laboratory was probably the first in which the central piece of equipment was so elaborate, and possibly so temperamental, as to require a more or less full-time engineering staff. The logistics of keeping the place going—whether this means the scientific machinery or the elaborate organization that tends the machinery—becomes an essential ingredient of the activity. There are engineers and instrument technicians and financial people and personnel experts, many of whom identify rather little with the purpose of the entire laboratory, but each of whom is valued for his specialized expertise.

Thus the modern home of big basic science, especially the big accelerator or reactor laboratory, acquires much of the flavor of the industrial laboratory. The time allotted for use of the machine is rigidly scheduled, and this imposes a regularity on the working habits at least of the technicians who tend the machine. There is a division of labor between those who are expert in electronics and computing and electrical engineering; and this requires coordination. The necessity for explicit planning is taken for granted, in much the same way as planning by a project manager is the accepted way of doing business in the applied laboratory.

The typical home of massive basic science, like CERN in Geneva or the Stanford Linear Accelerator, is however more specialized than is the modern home of applied science; this goes back to the aforementioned distinction between basic science, which tends to be internally motivated and disciplinary, and applied science, which tends to be externally motivated and interdisciplinary. The General Electric Research Laboratory covers a wider range of specialties than does the Stanford Linear Accelerator. The Argonne National Laboratory, with its experts ranging from biomedical researchers and ecologists to high-energy physicists, covers a wider range of specialties than does the nearby Fermi

National Accelerator Laboratory. I imagine that this greater specialization will in the long run pose some difficulties if the question of redeploying the large basic laboratories, like Fermi or SLAC, should ever arise.

## *II. Individual Science and Team Science: Breakthroughs Versus Spectroscopy*

### *A. The Xenon Compounds: A Breakthrough by an Individual*

In 1962 Neil Bartlett, a young chemist at the University of British Columbia, stumbled onto the fact that oxygen could be oxidized by platinum hexafluoride. About the same time Bartlett had noticed, while browsing through a table of ionization potentials, that the energy required to strip an electron off xenon (to form  $\text{Xe}^+$ ) was about the same as that required to form the  $\text{O}_2^+$  ion. He therefore concluded it was worth trying to oxidize xenon with  $\text{PtF}_6$ . Almost on his first try he was successful, and, in 1962, sixty-eight years of chemical dogma came to an end: the first stable compound of a noble gas,  $\text{Xe}(\text{PtF}_6)$ , was produced. The noble gases were no longer noble.<sup>11</sup>

Immediately after Bartlett's discovery, a group of chemists at the Argonne National Laboratory plunged into the new chemistry of the noble gases. They came to this task well prepared: for many years they had been interested in the chemistry of the fluorine compounds of plutonium and other transuranics. Their laboratories were well equipped for handling treacherous, extremely toxic materials like elemental fluorine and  $\text{PuF}_6$ . Almost immediately H. H. Claassen, J. G. Malm, and H. Selig discovered that xenon could be oxidized by fluorine alone, and within a year of frenzied activity many compounds of xenon and other noble gases were prepared and characterized. A blank page in inorganic chemistry had been expanded into a good-sized, well-filled book.<sup>12</sup>

This incident serves to illustrate, in almost too perfect outline, the usually held stereotypes as to the strengths and the weaknesses of the traditional individual and the newer team styles of research. The brilliant initial stroke—Bartlett's crazy idea that xenon could be oxidized if only one chose a sufficiently strong oxidant—was very much the doing of an individual. Not that this idea was absolutely new: in 1933 Linus Pauling had suggested that stable xenon compounds exist, and D. M. Yost even tried, unsuccessfully, to prepare them at California. And, even closer to home, at Oak Ridge S. S. Kirsliis, F. H. Blankenship, and W. R. Grimes, who were developing a reactor that was fueled with molten uranium fluoride, had noticed that the fission product xenon consistently was missing, whereas the fission product krypton was always present as expected in the gas phase. The question of whether the xenon could be disappearing as a chemical compound did arise and was discussed but of course

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was rejected, although, as it turned out,  $\text{XeF}_4$  was being produced. Scientific dogmas of such strength as the nobility of the rare gases are hard to dethrone.

But, once the brilliant, individual breakthrough had been made, the integrated team and the great logistic power of the National Laboratory moved in, massively and professionally, to fill in the spectroscopic details. A field that in earlier times would have remained fertile and exciting for a decade or more was largely elucidated in little more than a year's time.

In attacking the xenon compounds so massively, the Argonne National Laboratory was working very much in the style of the applied laboratory. There was a group leader with a staff of highly professional people, each of whom was an expert. Bartlett with graduate students probably would have been no match for Argonne with its professionals. And indeed, this extraordinary elaboration of a field of chemistry in just a year has led some to suggest that at least in the field of chemistry the future belongs to the professional team supported with superb equipment and unencumbered by teaching commitments, rather than to the professor whose professionalism in research is diluted by teaching.<sup>13</sup> This view has been sharply criticized by representatives of the university scientific community who insist that only individuals can achieve breakthroughs.

In point of fact, the team can and has achieved breakthroughs, and it is by no means clear that the team will snuff out the fire of scientific revolution. In the table below, as an example, I list the Nobel Prizes in physics during the past twenty years, the time during which team physics has grown so markedly. Of course not every discovery that wins a Nobel Prize breaks a paradigm, but I believe most physicists will agree that these discoveries at the very least represent important breakthroughs. Though the individual winners exceed the team winners, the fact remains that team science has produced several Nobel Prizes in physics. Indeed, examples of teams achieving breakthroughs are not hard to find. I shall describe one that occurred in Oak Ridge in the past few years.

#### *B. Anomalous Losses in Channeling: A Breakthrough by a Team*

Charged particles in traversing crystals often become trapped in channels formed by rows of regularly spaced atoms. This phenomenon, called channeling, was predicted theoretically by Mark T. Robinson in 1962, and then was discovered experimentally. In 1964 a team at Oak Ridge, consisting of several nuclear physicists, solid state physicists, and a physical chemist, examined the energy loss of the channeled particles as they emerged from thin crystalline gold foils. They were astonished

## Nobel Prizes in physics, 1948–1968.

Year	Winner	Discovery	Team	Individual
1948	P. M. S. Blackett	Development of the Wilson method and discovery by this method of the $\pi$ - and $\mu$ -mesons		x
1949	H. Yukawa	Prediction of mesons		x
1950	C. F. Powell	Development of the photographic method of the study of nuclear processes and discovery concerning mesons		x
1951	Sir J. D. Cockcroft E. T. S. Walton	Cockcroft–Walton accelerator and first disintegration		x
1952	F. Bloch E. M. Purcell	Nuclear magnetic resonance		x*
1953	F. Zernike	Phase-contrast microscope		x
1954	M. Born W. Bothe	Quantum mechanics; coincidence method		x
1955	P. Kusch W. E. Lamb	Lamb shift; anomalous magnetic moment of electron		x
1956	W. Shockley W. H. Brattain J. Bardeen	Transistor	x	
1957	C. N. Yang T. D. Lee	Nonconservation of parity		x
1958	P. A. Cerenkov I. Y. Tamm I. M. Frank	Cerenkov effect		x
1959	E. G. Segrè O. Chamberlain	Antiproton	x	
1960	D. A. Glaser	Bubble chamber		x
1961	R. Hofstadter R. L. Mössbauer	Electron nucleon and nuclear interaction; Mössbauer effect	x	x
1962	L. D. Landau	Liquid helium, etc.		x
1963	E. P. Wigner Maria Goeppert-Mayer J. H. D. Jensen	Shell theory; symmetry in physics		x
1964	C. H. Townes N. Basov A. Prokhorov	Maser and laser		x*
1965	R. P. Feynman Julian S. Schwinger S. Tomonaga	Quantum electrodynamics		x
1966	A. Kastler	Optical pumping		x**
1967	H. Bethe	Nuclear (astro) physics		x
1968	L. W. Alvarez	Giant bubble chamber and resonances obtained with it	x	

\* Strongly influenced by the wartime teamwork on radar.

\*\* Small team of students.

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to find that the particles lost their energy in discrete jumps: the amount of energy a particle lost depended very sensitively on the angle with the channel axis at which the particle entered the channel. From this quite accidental discovery came a completely new and possibly quite powerful method of probing the details of the interatomic potential in certain crystals.

I tell this story because it illustrates the unique power of an interdisciplinary attack. Here is an instance in which the whole team is much more than the sum of its separate components, where a team as opposed to an individual (as in the case of Bartlett and xenon compounds) achieves a breakthrough. First, the experiments required very thin, perfect gold crystalline foils; these happened to be the specialty of T. S. Noggle, a metallurgist and electron microscopist. Next the 50-MeV (million electron volts) iodine ions had to be accelerated, and their energies after degradation had to be measured with precision; this required experts on Van de Graaff accelerators and particularly on sophisticated time-of-flight techniques. Once the phenomenon was discovered, its full significance required the insight of a young solid state theorist, H. Lutz, as well as a variety of additional experiments that served to corroborate the theoretical predictions. And the team required orchestration: this was supplied by S. Datz, a chemist who had been concerned with the related phenomenon of sputtering.

To be sure, elaborate equipment was needed—a time-of-flight Van de Graaff machine. Nowadays this is not so unusual; there are perhaps two dozen laboratories which possess such instruments. But the number having at the same time an electron microscopist who can make perfect gold crystals a few hundred angstroms thick, an expert on sputtering, and a solid state theorist capable of interpreting the experiments is much smaller. It was very much more the style of research—the willingness of all parties to collaborate fully—that led to the breakthrough. This willingness among professionals to collaborate is actually not to be taken for granted, especially in the academic world. In reading James Watson's *The Double Helix*<sup>14</sup> one is constantly aware of the barriers that were placed between Watson and Francis Crick (who had the major idea about the structure of RNA) and Rosalind Franklin and Maurice Wilkins (who had the means for making the measurements needed). I suppose it is for reasons such as this that I am convinced the success of team science depends on the institution. There must be a tradition of interdisciplinary collaboration between professionals. This is more likely to exist in an institute with hierarchical organization—such as one finds in the applied or project laboratories—than in the typical university.

The example I have given would only marginally qualify as big

science: the machines, though large, are not all that large. If one examines the more typical endeavors of big science, particularly those that require unique accelerators or unique reactors, one finds many examples of breakthroughs by teams. One of the most recent is the finding by Val Fitch and his collaborators (many of whom were students) of the nonconservation of charge parity in the decay of the K-meson or, perhaps even more uniquely tied to the capacity of a single machine, the discovery by Segrè of the antiproton, at the Lawrence Radiation Laboratory in Berkeley. In these cases I would argue that it was the machine properly used and good leadership more than the team. The team was needed mainly because the experiment was so complex. In a certain sense, the team tends to be incidental to the machine in very big science; by contrast, the team is central in those cases—such as the work on DNA and channeling—in which the means are more modest. Here what is important is a delicate balancing and interweaving of individual expertise.

So we see that teams can achieve breakthroughs, operating either in the interdisciplinary mode or in the big science mode. Yet the power of the team seems to me to lie primarily in its ability to do spectroscopy; as team science becomes more and more common, so might the emphasis on the spectroscopic style of science. This trend may be accentuated by the weightiness and inertia of modern big science. Where scientific teams have mobilized around very big pieces of machinery there is an understandable incentive to exploit that machine. The path of development, instead of following the logical demands of the discipline, tends to be constrained to directions that are made accessible by the machinery at hand.

Something like this has always happened in science: one exploits whatever tools one has available. But scientists are naturally much less ready to scrap a 400-MeV proton-synchrocyclotron that costs several million dollars, but which no longer can cut at the main edge of high-energy physics, than they are to scrap, say, an optical microscope. The somewhat bureaucratic imperative to exploit expensive machinery circumscribes the direction of scientific growth. The spectroscopic filling in of details tends to crowd out the breakthroughs, simply because the number of breakthroughs possible with a particular machine is very small compared with the practically infinite spectroscopic detail the machine can generate.

It would be foolish to underestimate the importance of spectroscopy in setting the groundwork for important discoveries and conceptual breakthroughs. Quantum mechanics would have been impossible without its underlying detailed optical spectroscopy. Or, more recently, low-energy physics has a strongly spectroscopic flavor. Most experiments



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seek to measure, in various nuclides, specific properties that already fit into a general theoretical framework. Yet out of this elaborate spectroscopy (conducted, incidentally, by teams) has come a seemingly endless succession of breakthroughs: either in experimental techniques, as in the discovery of the lithium-drifted germanium-detector, or in new insights into nuclear structure, as in the discovery of isobaric analogue states and short-lived isomers.

There is another side to the story which deserves mention. The team, especially around the big machine, is a powerful scientific device. More difficult experiments can be tried with a large team equipped with a unique facility than with a smaller outfit not so equipped. Thus, insofar as breakthroughs flow from difficult experiments, one might expect teams working with powerful and unique apparatus to continue to contribute their share of important discoveries. For example, as soon as the high-flux isotope reactor became available, questions in the phonon distribution in solids that had plagued solid state physicists became answerable.

To make important breakthroughs in science will always require competent, imaginative leadership. But it seems reasonable to expect that the degree of insight required to make such discoveries may be somewhat less than it was in the day of individual science: the team, or the big machine, may offer elements of uniqueness that were formerly supplied by sheer intellectual power. And, since competence is so much more common than genius, the team may be spreading the possibility of significant scientific discovery to many more scientists than in former days. Perhaps this democratization will prove to be one of the main by-products of big team science.

### *III. The Future of Team Research*

#### *A. The Institutional Setting*

It seems clear to me that team science in the modern style is done better in the hierarchical, logistically strong institute than it is in the university. This, coupled with the unrest that wracks the university, suggests that we might see a gradual movement of modern science away from the university and toward the national institute—possibly even a growing separation between education and research. To most writers on this subject, especially since the Seaborg Report,<sup>15</sup> the notion that research and education are inseparable and indissoluble, that the one cannot be done without the other, has acquired the ring of holy dogma. But the facts do not really bear this out: certainly insofar as one is elaborating a certain area, such as the chemistry of xenon, professionals are better than students. For many years applied chemistry

has been conducted to great advantage in the industrial laboratory without benefit of students. I know that at Oak Ridge some (though not all) of our division directors are convinced that they achieve results more quickly and more reliably with professionals than with students.

The universities have responded to the trend toward team research by setting up institutes, interdisciplinary and logistically strong, where team research can be performed effectively, but largely by students. But on the average these institutes suffer from a mismatch between the social ethos of the university and the social ethos of the institute: the one is individual and democratic, the other collective and hierarchical. When the institute acquires a collective and hierarchical character, which I believe is necessary for its success, its tie with the university department becomes more tenuous.

So we may be going full circle. Science in the seventeenth and eighteenth centuries was practiced predominantly in the academies, not the universities. It moved into the German and English universities in the nineteenth century. And perhaps, with the growth of the large team, it may gradually be moving out again, or at least it may not retain as intimate a connection with the university as it has had in the immediate past.

### *B. New Fields for Team Research: The Rise of Big Biology*

The big interdisciplinary team has generally been confined to the physical sciences and to engineering. The biological sciences have remained the bastion of little, individualistic science, probably because the experimental tools needed to conduct biological experiments have typically been small and relatively inexpensive. Yet there are now important trends toward large interdisciplinary teams in the biomedical sciences and, very recently, teams that include engineers as well as physical scientists.

Part of this trend comes from the rise of molecular biology. In one sense the most important parts of molecular biology are really a branch of crystallography; the double helix model for DNA, for example, is based on a crystal structure deduced from X-ray diffraction data. Watson in his book bemoans his lack of expertise in crystallography, a lack which was made up for him by Crick and by Wilkins' group. It is highly significant that of the two men who made the most important discovery of modern biology, one (Crick) was originally a physicist. Again, the extraordinary elucidation of the working of peripheral nerve, for which A. L. Hodgkin and A. F. Huxley received the Nobel Prize, would have been impossible had it not been for the underlying work on electrical properties of nerves by the physicist K. S. Cole. Many biolo-

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gists, especially in the most active fields of biochemistry, feel it necessary to rub shoulders with physicists and with physical chemists.

The second trend discernible in biomedical science is the rise of the very large-scale experiment. With our present concern with low-level insults to the biosphere (radiation, pesticides, smog), it becomes necessary to conduct animal experiments on a scale far greater than had hitherto been customary in biology. The husband and wife team of William B. and Liane Russell at Oak Ridge maintains more than 100,000 mice in order to study the mutagenic effect of moderate levels of radiation. Such biological experimentation immediately becomes a team activity: geneticists to manage the entire experiment; statisticians to scan the data for significance; veterinarians to manage the animals; pathologists to look for somatic effects; and, of course, the whole array of animal attendants, janitors, and cage-washers who are needed to keep 100,000 animals alive and thriving.

I would expect this trend toward very large-scale animal experimentation to become increasingly prevalent as we become more sensitive to the widespread influence of seemingly subtle factors in our environment. A call for such experimentation has been made by René Dubos, for example. Should this call be answered by the funding agencies, we may expect to see an increasing fraction of biology being conducted in the style of big science.

Biologists, particularly biochemists, are beginning to learn how to employ engineers and other supporting scientists, notably analytical chemists. This represents a new trend since biological institutes traditionally have not crossed deeply into the physical sciences, even less into engineering. True, the National Institutes of Health is an enormously large complex, but NIH has not had within it a strong tradition in the physical sciences or in process engineering. By contrast, the atomic energy laboratories have from the beginning spanned the biological sciences, the physical sciences, and the engineering sciences. This unusual juxtaposition has now begun to pay off—for example, in the brilliant development of zonal centrifuges under the leadership of Norman Anderson at the Oak Ridge complex. These centrifuges, which were first developed to separate uranium isotopes, have been modified by Anderson, together with a large team of engineers, to handle biological materials. The centrifuges are now being used very widely to separate, on a large scale, various cell moieties; for instance, they have been used to purify flu vaccine of its antigenic protein impurities. Anderson is now exploiting in his Molecular Anatomy Program (MAN) whatever relevant engineering and analytical expertise he can find in the Oak Ridge complex to systematically separate, and then prepare on large scale, the many cellular particles which now can only be seen in

the electron microscope. Anderson's success I believe is only the forerunner of future successes that biology will enjoy as it enlists the co-operation of the engineering sciences.

### *C. Redeployment of the Big Institutions*

The modern scientific team arose as an integral part of the great laboratories; whether in basic research or in applied research, the team style is the dominant mode in the modern big laboratory. It seems inevitable then that the future of team research will depend on the fate of the big laboratories. I shall therefore close with a few speculations on these institutions.

Though it is evidently impossible to generalize, one can see limits to the prospects of the big laboratories. For example, in those institutions devoted to high-energy physics, the future is limited by the sheer increase in expense of the necessary gadgets. The Alternating Gradient Synchrotron facility which was completed in 1960 cost \$30,650,000; the Stanford Linear Accelerator, completed in 1966, cost \$114,000,000; and the National Accelerator, a 200-GeV proton synchrotron, expected to be operating in 1972, is estimated to cost some \$240,000,000. Presumably each of these devices will become obsolete, not because there is not always more spectroscopy to be done, but rather because people will eventually get bored with spectroscopy. Unless there are occasional stirring breakthroughs—perhaps a breakdown of quantum electrodynamics—I cannot visualize these institutions forever sustaining themselves, or remaining immortal, by simply amassing spectroscopic details about elementary particles.

The atomic energy laboratories must also face questions of redeployment, though for a different reason. True, the two central problems of nuclear energy—breeding and controlled fusion—have yet to be solved. But even these are questions of finite dimensions; the first because it is not all that difficult, the second because it may prove so difficult that interest in it will wane. There is already evidence that the world's atomic energy laboratories are beginning to adjust to these facts, mainly by expanding their areas of concern beyond nuclear energy.

By contrast, the future of the great biological laboratories seems clear enough: the questions which biomedical science seeks to answer are urgent, massive, timeless until they are solved. It seems likely therefore that the need for redeployment will hardly arise for, say, the instrumentalities of the NIH. My guess is that these institutions will acquire a more interdisciplinary flavor, especially by developing engineering skills, simply because the cross between the physical sciences and biomedical research has been so fruitful.

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Thus redeployment of at least some of the big laboratories is in the cards. This realization comes at a time when we hear much about the many social and socio-technological conflicts that plague our modern society—racial unrest, the decay of the city, pollution, overpopulation. John Platt sees modern society on the verge of crises so profound as to warrant launching wartime-like projects to resolve them.<sup>16</sup> The Committee on Government Operations of the United States Senate has been holding hearings during the past couple of years under the chairmanship of Senator Edmund S. Muskie aimed at establishing a Select Committee on Technology and the Human Environment. Everywhere there is a restlessness and concern: the priorities our society has lived with in the postwar world need reassessment; we must abjure our preoccupation with hard science and address ourselves to these subtler, more difficult, and more important human problems.

Whether science can help very much with these social questions is a moot point. Many of us scientists believe that science can help: that almost every one of the conflicts and problems that we face has some technological, as well as social, components, and that therefore science directed specifically at their resolution may be helpful. In this we may be displaying an uninformed naïveté; perhaps racial conflict, urban decay, and overpopulation are beyond help even from science.

Yet this much can be said: if science has something to offer toward resolving these questions, it surely will have to be a broadly interdisciplinary, team type of science. The social components of these problems are more obvious than are the technological ones, but there is always an interaction between the two aspects; it is quite natural to visualize interdisciplinary teams, ranging over social science as well as natural science and engineering, being mobilized to attack some of these desperately troublesome questions. As of now, however, such teams have no natural home: the university is unsuitable because of its prejudice against teams, the national laboratory because of its inexperience in social science. I have therefore suggested the creation of new entities, national socio-technological institutes. Some such institutes might be formed *ab initio*; others by co-opting experts in the social sciences to work in existing hardware-oriented laboratories.

National socio-technological institutes at which one would apply the methods of science to our difficult socio-technological problems might also serve an entirely different purpose: a means of focusing the socially relevant energies of our young people. Many young students seem to be disillusioned with natural science: those who in the previous decades went into physics or chemistry now go into the more “relevant” social sciences, and even the students of the natural sciences are acquiring a taste for socially relevant issues. Yet I can foresee this socially motivated

cohort of students being frustrated all over again if, once they are trained, once they are readied to do battle on behalf of society, they find no instrumentalities to which they can attach themselves to carry on their commendable crusade. I should think that just as the institutions of big science for several decades provided a home for the aspiring scientists of the 1950's and 1960's, so the socio-technological institutions might provide a home for the aspiring social engineers of the 1970's.

There have been several suggestions by now for national socio-technological institutes, most recently, in Sénate Bill 3410 sponsored by Senators Howard Baker and Edmund Muskie to establish national environmental laboratories. It is premature to really assess such proposals. It could be that the difficulties we face go beyond resolution by the methods of science—hard analysis, empirical observation, engineering design. Yet, before taking so pessimistic a view of man's capacity for self-betterment, I would urge trying the interdisciplinary team attack—an approach that was so notably successful in the generation immediately following World War II and that just may help guide us during the coming generation.

#### REFERENCES

1. For alternative approaches to this topic see L. Kowarski, "Team Work and Individual Work in Research," in N. Kaplan, *Science and Society* (Chicago: Rand McNally, 1965), pp. 247-255; and Cecil F. Powell, "Promise and Problems of Modern Science," Concluding Address, *Maria Skłodowska-Curie: Centenary Lectures, Proceedings of a Symposium*, Warsaw, October 17-20, 1967 (Vienna: International Atomic Energy Agency, 1968).
2. Dr. Saul Benison pointed out at the Bellagio conference that I may be overdoing this distinction between art and science: Leonardo trained in the atelier of Verrochio, who influenced much of his early style; Melville was much influenced by the Bible and by Shakespeare. Yet the connection between, say, the physicist Hertz and his predecessor Maxwell is, to my mind, far more explicit and continuous than the connection between two artists. Hertz used Maxwell's equations precisely as Maxwell formulated them; his work flows from Maxwell with an inevitability and logic that can never be matched in the work of an artist who follows an illustrious predecessor. As Professor Edward Shils puts it, "There is a coercive element in the tradition of the sciences that is absent in the arts."
3. University of Chicago Press, 1962.
4. "Letter to Robert Hooke, February 5, 1675/6," in John Bartlett, *Familiar Quotations* (Boston: Little, Brown and Company, 1968).
5. *Proceedings of the American Philosophical Society*, 94 (October 1950), pp. 422-427.
6. As will be apparent later in the discussion, I often extend and generalize the



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word "spectroscopy" to mean both the activity and the results of filling in the scientific details after a major discovery has broken new ground.

7. The facelessness of big team research has been commented on by others—for example, Gerald Holton, "Scientific Research and Scholarship, Notes Toward the Design of Proper Scales," *Dædalus* (Spring 1962), pp. 362-399.
8. P. B. Medawar, *The Art of the Soluble* (London: Methuen & Company Ltd., 1967).
9. *Inside Bureaucracy* (Boston: Little, Brown and Company, 1967).
10. To anyone who has spent some time in a large laboratory, it must be perfectly clear what I mean by its great logistic power; but to those who are unfamiliar with such institutions, perhaps I can illustrate with the experience of a distinguished demographer who spent a summer at Oak Ridge National Laboratory studying urban decentralization. At the end of his stay I asked him what he thought of ORNL as a possible locale for demographic research. He replied: "Demography would be revolutionized if it were conducted there. It would be converted from a small, rather individualistic enterprise into a big-scale, massive business. There would be huge computers with programmers and mathematicians to help one use them, experts of every sort available at the other end of the hall, as well as editorial assistants, draftsmen, travel agents; above all, they would be ready and willing to help you get on with the job." If the large laboratory possesses so much logistic strength in the eyes of a demographer, one can imagine how much greater is its strength in the fields of science it was originally set up to exploit!
11. Neil Bartlett and N. K. Jha, "The Xenon-Platinum Hexafluoride Reaction and Related Reactions," in H. H. Hyman, ed., *Noble-Gas Compounds* (Chicago: University of Chicago Press, 1963), pp. 23-30.
12. Hyman, ed., *Noble-Gas Compounds*; cf. J. H. Holloway, *Noble-Gas Chemistry* (London: Methuen & Company Ltd., 1968).
13. "Basic Chemical Research in Government Laboratories," Report of the Panel on Basic Chemical Research in Government Laboratories of the Committee for the Survey of Chemistry, Division of Chemistry and Chemical Technology, National Academy of Sciences Report 1292-A (Washington, D.C., 1966).
14. James D. Watson, *The Double Helix* (New York: Atheneum, 1968).
15. "Scientific Progress, the Universities, and the Federal Government," Statement by the President's Science Advisory Committee (Washington, D.C.: U.S. Government Printing Office, November 15, 1960).
16. John Platt, "What We Must Do," *Science*, 166 (November 28, 1969), 1115-1121.