IS SCIENCE TOO BIG

No Disciplined Thought Is Driven

EDITOR'S NOTE: Three times in the forty months of its existence, SR/RESEARCH has given over all of its space to diagnosis of the growing pains of the self-conscious giant, Science. The first was in February 1957, and was occupied with the question of how scientific discoveries can democratically be put at the people's disposal. The second was in July 1958, and was concerned with Earth as a water planet and with the need for research in global dimensions to explore the meetings of sea and sky. The third was in October 1958. It looked at man as he stands on the edge of the watery ocean at the bottom of the sea of air that is the broth of his life.

We have not, of course, been alone in our searchings. Taxpayers' reactions to billions of dollars of spending for electronic brains, atom-smashers, strange new kinds of balloons for the deep sea and the deep sky, man-made moons and robot explorers for neighboring planets, have been on the mind of President Eisenhower's Special Assistant for Science and Technology, Dr. James R. Killian, Jr., ever since his appointment late in 1957. Among others who share his preoccupation are the trustees of the Alfred P. Sloan Foundation. They, in the autumn of last year, proposed to the National Academy of Sciences and to the American Association for the Advancement of Science that the questions be stated openly at a public forum.

Last month some 500 scientists gathered in the Caspary Auditorium of Rockefeller Institute in New York for the meeting, on the three days May 14-15-16, in response to a call from the forum's coordinator, Dr. Warren Weaver, to "consider the following questions:

"1. Is not the large support of applied research, and still more particularly the massive present support of development, in unhealthy relation to the meager support for basic research?

"2. Is it not true that industry pays eager lip service to basic research, but in actual fact does not give adequate support to basic research, either within industry or elsewhere?

"3. Has either industry or government learned how to protect basic research from the insistent demands of applied research and development?

"4. Are not universities so deeply invaded by the demands for solving immediate problems and by the temptation of income for so doing, that there are all too few cases of competent scholars pondering about problems simply because it interests them to do so? Is there not a real danger that the scholars in our universities will lose—and indeed have already partly lost—the 'maneuvering room for their continuing reanalysis of the universe?'

"5. Has it been effectively accepted in our country that the spirit of basic research is an essential ingredient of the educational process—and that this fact should affect educational procedures at all levels?"

"It is the purpose of this symposium," the summons concluded, "to set forth and examine with candor the facts concerning the support of basic research in our country; to inquire realistically what are the blocks which prevent our doing what we all say we believe is important; to make concrete suggestions as to ways in which the situation can be improved; and in general to proclaim the fundamental faith which we have in the importance of free and imaginative basic research, and to do this with such competence and vigor as will have a national impact."

After two days and one night of talk, to which a noted contributor was President Eisenhower (Gracing a scientific gathering for the first time since his occupancy of the White House, he announced his approval of Federal spending for a two-mile-long atom-smasher at Stanford University in California. The original $100,000,000 price-tag on it is expected to double at least before the instrument is finished, and upkeep will run between $15-and-25 million a year. Whether its construction should be begun before smaller colossi, now abuilding, can prove whether research value grows with size indefinitely, has been hotly argued.), a selected hundred of the 500 delegates met behind closed doors to draw up conclusions.

THE main challenge before them, everyone agreed, was Dr. Merle A. Tuve's expression of concern that science, in growing big, is growing too far away from austerely disciplined thought. It is the unadorned mind of man, he recalled, and not the fanciful instruments man conceives, that produces all of the beautiful in science. Respect for knowledge itself must stand above pride in intricate organization of the search for knowledge.

Dr. Tuve's remarks had fallen on the second morning of the open meeting like a thunderclap on a sunny day. The applause that followed them had been loud and prolonged. Dr. Tuve clearly had said some things that the great majority of the assemblage felt needed saying. The clapping kept him on his feet so long after he stopped speaking that the moderator of that particular session forgot to announce that science reporters were waiting for a press conference. While the reporters waited, the delegates went to lunch.

Concurrence in Dr. Tuve's views was not unanimous, however. There were some who felt he had overstated the case. Where, for instance, asked Dr. Lloyd Berkner (who has the job of directing huge expenditures for instrumentation in atomic, atmospheric, oceanographic and space research) would the Theory of Evolution be if Charles Darwin had not been able to travel on the *Beagle*? This, in its way, was overstatement, too. The real question was: Had Darwin always been comfortable on the *Beagle*? Nevertheless, there was a point.

The secrecy of the forum's concluding session, at which the hoped-for "national impact" presumably was to be designed, left an unfortunate implication that science is not yet ready to extend full confidence to the public even on those occasions whose prime objective is public understanding. Not until September, when the proceedings are published in book form by the AAAS, will the findings, if any, be known. In its deliberations meanwhile, the Congress of the United States will need to consider, along with pleas for more funds for science, fears of scholars that the search for knowledge is taking on too many characteristics of the big machine. An excerpt from the warning of Dr. Tuve, one of America's most distinguished researchers, begins on the next page. ►

FOR THE SCIENTIST?

from Herds of Giant Research Robots

By MERLE A. TUVE

HUGE new synchrotrons and cosmotrons and electronic computers, and polar expeditions and balloon and rocket flights and great government laboratories costing more each year than the total academic costs of many of our greatest universities—all of these conspicuous aspects of our new national devotion to science are subsidiary and peripheral. They do not serve appreciably to produce or develop creative thinkers and productive investigators. At best they serve in a brief or a rather incidental way, and at worst they devour.

There is a growing conviction among my friends in academic circles that the university today is no place for a scholar in science. A professor's life nowadays is a rat-race of busyness and activity, managing contracts and projects, guiding teams of assistants, bossing crews of technicians, making numerous trips, sitting on committees for government agencies, and engaging in other distractions necessary to keep the whole frenetic business from collapse.

This picture is ignored and even denied by some. But it is much too genuine for a great many others. Too many of our academic leaders have chosen this pattern of activity and personal power in preference to the quieter and more difficult life of dealing with ideas and scholarly initiative.

In this new style world of scientific research, the private research institutes set up some decades ago do not count for much in terms of size, as measured either by numbers of workers or by yearly expenditures. The chief examples are the Rockefeller, Carnegie, Guggenheim, and Bamberger establishments. Probably their greatest contribution to scientific research today is in their continuance as prototypes of well-tested mechanisms for the selection and encouragement of creative individuals and for the maintenance of a productive environment for the scientific investigator.

We all know what we mean by truly basic research. We mean a devoted and almost passionate personal activity in search of new knowledge, not just factual information, but knowledge of the kind which can enlarge our understanding, knowledge which is not facts in isolation but facts related to guiding hypotheses or principles, knowledge which relates to natural law. This kind of truly basic research is a creative activity, an expression of wonder. It is concerned with ideas, hopefully and critically directed toward understanding, and is often the spontaneous effort of one man, or at most of several competent individuals working together. It is not directed or organized. Only in the later stages, often close to technology or to medical use, does it lead to the employment of large groups of specialists operated as a team. It is a quest, not a job to be done. The measure of success is the quality of the effort and the character of the critical selection of goals to be sought, not the quantity of the output of scientific results. These men serve the conviction that greater knowledge and deeper understanding are undeniably good.

I would like to point out that all of us [scientists] have contributed to a more or less purposeful confusion in our use of the words "basic research." We have lumped under "research and development" so many huge technological activities in the national budget, and correspondingly in corporation budgets and elsewhere, that the figures have become practically meaningless. Under "research" and even under "basic research" we have encouraged and budgeted huge enterprises of essentially operational character, most of them promoted with some enthusiastic hope of great national prestige. Essentially these projects are based on the twin arguments that the USA must be first and biggest, and that tax money is not real money but just a voucher for directing the expenditure of national effort toward certain speculative goals because otherwise this effort would not be spent at all or would just be directed toward more personal goals.

After the special usefulness in war technology of men trained in basic physical sciences had been demonstrated during World War II, the idea of *mission-directed basic research* was firmly implanted by our concerted efforts at numerous locations in the dark recesses of the military budgets. We have all been faithfully supervising the growth of this hybrid notion ever since, in the halls of Congress and in the minds of the public. We all know that only an extremely small fraction of the various budgets we help to defend are truly basic, intensive studies devoted to the perception and formulation of new knowledge toward deeper understanding.

BASIC research was enlarged long ago to include the personal accumulation of information known to be of little or no present interest to others. Then the engineers and chemists, followed by the rest of us, began to include under "basic research" the systematic accumulation of measurements by organized groups of technicians with a view to the usefulness of the resulting tables of data for various technological purposes. This kind of activity by large groups of technicians has expanded far beyond systematic data-taking to include whole experimental programs on a speculative basis, only very thinly flavored with the personal interest of a competent individual and containing only minute traces of the love of knowledge.

I might give a mild example. I have served for many years on the executive committee of the United States National Committee for the International Geophysical Year. When the Russians announced that they would join this cooperative world activity, the IGY became important as an example of international collaboration which could be carried on in spite of crucial differences in political policies and beliefs. The IGY's success, then, was important for reasons which probably transcended the intrinsic value of the detailed scientific observations themselves. As a consequence, on the executive committee we had to judge and approve expenditures which seemed outrageously large in relation to their possible scientific merit or importance, and we served as the excuse for logistics for various polar expeditions and airplane and rocket flights which cost

—Carnegie Instit.

Dr. Merle A. Tuve

AUSTERITY IS NOT JUST ANOTHER WORD to Dr. Merle A. Tuve, director of the Department of Terrestrial Magnetism at the Carnegie Institution in Washington, D.C. His own career is filled with examples of what can be done with simple tools in imaginative, determined hands. All that he and Gregory Breit had to work with in the most famous of them was a radio receiving set. Yet with it they confirmed the existence of the ionosphere, the electrical sea that washes the outer depths of Earth's atmosphere. That was in 1925. The radio transmitter of the Naval Research Laboratory was already in operation on the Potomac, and the two men merely recorded the pulses of its broadcasts bouncing back to the ground. Though not much was made of it then, the corollary importance of that historic experiment was the first use of the principle of radar.

Were he a specialist in some narrow category of study, Dr. Tuve's opinions might easily be set aside as applicable only to his particular line of interest. What makes him especially worth listening to is the catholicity of his research taste. His mind ranges all the way from distant space among the stars to the mystery that lies beneath Earth's crust, and from atomic mutation to the proximity fuse and the guided missile to wild growth of cells in cancer. (Which, incidentally, illustrates how disparate a man's work can be nowadays without affecting his good standing as a physicist.)

Born in the town of Canton, South Dakota, 58 years ago this month of June, Dr. Tuve took Bachelor of Science and Master of Science degrees at the University of Minnesota, and a Ph.D. at Johns Hopkins. He taught physics at Princeton and Johns Hopkins before joining the Carnegie Institution staff in 1926. He learned to know the scientific oligarchy of this country from the inside and from the beginning of its period of real power by serving as chairman of Section T of Vannevar Bush's Office of Scientific Research and Development between 1940 and 1945. His honor decorations are too numerous to be arrayed on any shirt-front: Presidential Medal of Merit, Commander of the Order of the British Empire, John Scott Award, Comstock Prize, Research Corporation Award, Howard N. Potts Medal, Outstanding Achievement Medal of the University of Minnesota, Barnard Medal of Columbia University.

No aspect of organized science can be said to be alien to him. He can see the problems of communication from two directions—as a contributor to The Physical Review and other journals, and as editor for ten years of the Journal of Physical Research. He sits on the U.S. National Committee for the International Geophysical Year, chairs the Advisory Panel on Radio Astronomy of the National Science Foundation, and has been a university trustee (Johns Hopkins) since 1955. He is familiar with the problems of women in science through his marriage to a physician (nee Winifred Gray Whitman, she has given him two children: Trygve Whitman and Lucy Winifred), and he keeps in touch with the arts through his membership in the American Academy of Arts and Sciences.

money enough to have subsidized the physical sciences at all of our universities in great luxury for many decades. The money was all spent, mostly of necessity by our military services. We encountered or produced in the IGY many examples of a drastic loss of a sense of proportion between the costs of a project and its substantive content.

When a scientific scholar—as distinguished perhaps from a business executive—speaks of basic research, surely he must have primary reference to the *support of ideas*, not the operations aspect of technological performances or record achievements, however spectacular, such as submarine trips under the polar ice or successful Antarctic logistics for the IGY.

I might suggest that for purposes of discussion the term "academic research" could be considered to refer to the intensely personal activity of individual professional workers in search of scientific knowledge, the kind of activity we all recognize as basic research, even during its drudgery stages. The term "technological research" could be used to refer to the very much larger body of activity (often involving a great many individuals not qualified as independent investigators) which underlies, in its matter of fact way, the work of various practical groups in industry and government.

Now the reason for making this perhaps uncomfortable distinction is not in order to attack technological research as such (except on perhaps one point, namely: that it deflects, distracts, and subtracts some highly creative and effective individuals from the research area we clearly recognize as the support of ideas). The reason for making the distinction is simply to remind you that all of the huge activities of technological research grow out of the highly personal activities of academic research. No array of feedback arguments will convince very many of us that the real germ of new knowledge is the product of team activity or the result of large-scale instruments or implements created in the simple hope of learning something new. The idea and the research objective must come first, then the instrument must be created for approaching this objective and testing the idea. Occasionally, but often in an equivocal sense, the notion that teams and big instruments create new areas of knowledge appears partly true or at least plausible. But I have observed that the new scientific knowledge gained by just operating a huge expedition or big-scale instrumentation often must be inflated by repeated public statements until it appears to have great scientific importance. When asked seriously about it, the non-involved workers in the relevant scientific field are frequently unable to say why the results obtained should be considered so important, except that they cost a lot or involve such a spectacular effort. Most of our present hoop-la about space is in this category.

The enormous expansion of funds and activities called research has left the private research institute as only a minuscule item in the whole picture, but the institute's function seems thereby to have become even more clearcut and conspicuous. In discussing the activities of my own Department in the Carnegie Institution I have said for some years that it is very simply our aim "to be just a good old-fashioned example of the real thing." By this of course we mean that we attempt to support the highly personal activity of the individual research man who does his own research work. This is our interpretation of the "support of ideas."

THIS basic idea of buying a man's time and giving it back to him, as a support for his ideas and his thinking, has some important corollary aspects. One is that he must not have too many people interfering with his time or he becomes a manager and an operator more than a research man. Another corollary is, especially in these days, that his equipment will be smaller and less complicated than the best equipment in his field of work or again he will be converted into a manager or a constructor or a big time operator. In general the corollary is simply one of *reasonable austerity*. Many of us who have soberly discussed the environment required to foster creative personal activity recognize that a moderate degree of austerity is essential to the hard work and disciplined self criticism which are always required for creative intellectual accomplishment.

AUSTERITY as an essential requirement for creative research may sound harsh and out of date to most of the big operators and many of the young technical men in research today. But the undeniable fact is that creative work is nearly always done with limited resources and under difficult conditions.

I think it is important for us to recognize the relatively small size of the annual budget for this academic kind of basic research. Even though our methods are more wasteful, and we buy numerous industrial tools and instruments, and we pay all of our graduate students, the total number of competent and fully trained investigators who are really devoted to seeking new laws and new regularities in nature's processes and not guided toward practical ends such as better radio or radar or better submarine detection or navigation or better rockets or antibiotics is not large. The number of these academic men in basic research is still not too different from the pre-war number of similar fully trained scientific investigators. In some ways we must take a larger discount, in fact, because so many of our principal research men spend such a large fraction of their time now in obtaining and spending large government grants and in supervising large groups of rather poorly qualified workers who have been upgraded into posts as research men but who are not qualified to be independent investigators. It is my impression that the total effort really spent on basic scientific research in the old-fashioned or scholarly sense has not increased by more than a modest fraction and in no way can be compared with the huge figures of five hundred million and eight hundred million dollars per year which are supposed to be spent for basic research in this country.

How much basic research do we "need"? Why do we think we should have more basic research? I have not been able to recognize any objective basis for making a quantitative statement about our need for the academic research I have described above. Even a relative statement regarding the amount or quantity, such as the remark that we need much more basic research than we have in progress today, is hardly to be demonstrated objectively, I think. But each of us has his own convictions on the subject. I suggest that we recognize the depth and quality of our convictions, and note that they actually rest on a higher and broader foundation than, for example, can be objectively demonstrated by economic statistics or by the calculable limitations of our present-day technology. I do not agree that the primary reason for underwriting basic research in science is a utilitarian one, to provide new facts and ideas to be utilized by industry. Instead, one of the good reasons for us to have a productive industrial plant is to give us some excess social energy to invest in science and the arts for their own sakes.

Our individual convictions regarding academic research are thus rooted in our views of what constitutes the good life. We all feel that a prosperous society should not spend its entire energy and resources in enjoyments of the moment, but that it should also add some permanent enrichment to the lives of others (and, for that matter, to the lives of its own individuals). Higher education is recognized in the USA as in itself a "higher good." Scholarly achievement, the recognition and delineation of new knowledge, is nearly everywhere in our country granted a position of respect and honor.

The real foundation, then, for an examination of the question as to how much of this kind of genuine basic research we need, does not lie in our predictions as to the needs of industry or technology for more facts and for new areas of industrial activity or profitable investment. Nor does it lie in the continuingly imminent sterility of our efforts to resolve the differences between the value system of our society and those of other societies by a sheer increase in our ability to destroy or to use force. The quantity of basic research needed in our present society rests on our joint estimate of two things: (a) how high in our value system do we place scholarly achievement, or the creative search for new ideas and the formulation of new knowledge, and (b) how much of our national effort can we now afford to invest in these fruits of our prosperity and our convictions?

I have great confidence that most of our public servants, including industrialists and newspaper columnists along with our elected representatives at all levels, have overestimated our willingness to underwrite our fears and have underestimated our willingness to underwrite our hopes. The position of education, and especially of higher education, in the value scale of the adult American, the working taxpayer, is still well up toward the top of the list.

Even though our present level of effort in academic research in science is reasonably high—in part because graduate students are young and

"Their Own Worst Enemies"

GOVERNMENT funds have been the backbone of the growth of the right kind of basic research in the universities in the postwar period. The chief trouble has been that government funds for basic research have not been available in large enough amounts, compared to the funds available for applied research, for testing and development. It is this fact that has forced many universities to take on the development projects in order to have something going that could be called research. A major problem of the future is to keep the funds for basic research growing at an adequate rate.

Here is where the ugly specter of government control rears its head. Will not the government assume control of the universities if it provides funds for their research? The chief answer to this question is that it has not happened yet. The chief threat of control has come not from the government agencies who administer the funds, but from the panels and advisory committees, composed largely of professors, who pass upon projects and budgets before they are accepted. Many of these groups have steadfastly opposed proper overhead payments on research contracts, have opposed including allowances for the salaries of professors working on the projects, have opposed block or departmental grants, and have required of the prospective research worker such elaborate and detailed proposals and reports that a type of bureaucratic committee control has grown up which suppresses daring ideas and takes administrative control out of the hands of the universities themselves.

Scientists, when they get into government, are their own worst enemies. When they have control over activities of their colleagues—through the recommending of research grants—they become autocrats of the most difficult kind.

—DR. LEE A. DU BRIDGE,
President, California Institute of Technology,
at the Basic Science Symposium.

idealistic and there are many of them because the available project money pays each one a good stipend—it could be higher. And the quality of the effort could represent a deeper commitment to the search for truth and beauty if we seriously undertook to devise and support measures which are honestly directed toward the best support of individual creativeness in science. There is much of it in every fresh generation of graduate students. But I feel that we have directed most of our efforts toward the creation and support of large-scale activities essentially technological in character and conspicuous in possible effects. We have not had much success, as a nation, in encouraging quietly creative scholarship and intensely personal activity in research. Our state universities and our larger private institutions did this moderately well but on a modest scale twenty and thirty years ago. But this kind of local enthusiasm seems less prevalent now that large grants, by every scale of comparison, are made to individual professors. And these professors are selected for support not by faculties and deans or faculty research committees who know them, but by their own self esteem (that is, by their personal requests for support) and by boards or panels in Washington. It is at least highly probable that the present system of selection for research support, strongly biased in favor of the excessively self-confident person and the "big operator," is not well designed to favor the quietly creative scholar. Perhaps all we need is the noisily creative scholar. But he is surely a bit less genuinely decorative to our society, and his contributions, although suitably expensive, are probably less permanently valuable.

I now turn with some hesitation to the suggestion of a mechanism which might serve to increase and stabilize the level of creative basic research in our country. My hesitation relates only to the question of using Federal funds in our educational institutions, but perhaps this has already been accepted by most of you. In any case, I believe we should take firm position on the point that the support of true basic research is the support of ideas and that this always means the support of a creative investigator. I think we should make it clear to Congress and to the public that the whole basic record of scientific progress has been made by individual men who could spend their time freely on the scientific problems which puzzled them. I see no valid reason for not insisting that the sound support of basic research requires us to use the technique long used in the universities and copied by the private research institutes, namely, that of buying a creative man's time and giving it back to him. Congress may have strong views against granting public funds as permanent endowment, but we can surely insist that there are specific and identifiable individuals in the world of scientific research whose lifetime efforts can safely be underwritten in advance as good single investments in basic research.

I MEAN thus to say that we might use public funds to purchase a creative investigator's working lifetime, and then give it back to him to spend in his research efforts. A single lump sum of say $700,000 would pay the remaining lifetime salary of a gifted research man after he has been clearly identified as a creative investigator by the age of 30 or 35, and would pay in addition for one or two technical assistants or two or three students to work with him. And we could stipulate to the (university) Regents or Trustees who accepted such a lump sum to underwrite the scientific investigator's lifetime activities that if he should change from the life of a working research scholar to become a manager of large grants or the supervisor of a large team, the Regents would shift him to their own salary rolls and revert the grant which made him a Distinguished Research Scholar.

One might call such an investigator a Franklin Research Professor or a Jefferson Research Scholar. If we were to allocate forty to sixty million dollars per year to the creation of such Research Professors or Research Scholars, suitably selected by a very small University Grants Committee, in one decade we would have in this country a solid phalanx of 500 or 600 outstanding investigators dedicated to basic research and unquestionably free to devote their personal time and attention to creative ideas for the rest of their lives. The total investment over a decade of $400-to-600-million would amount to perhaps half of the cost of one year of our current activity with space rockets or perhaps the cost of operating the Atomic Energy Commission for two months.

All of us (top-flight scientists) are asked at regular intervals to consider and support programs in individual specialties such as nuclear physics or oceanography or space rocketry or meteorology or the chemotherapy of cancer with sums of public money each totaling from 50 to 800 million dollars per year. If we all believe that the real key to basic research is the continued stable support of the individual research man, to give him full freedom, with moderate austerity, to investigate problems in which he is interested, then what prevents our doing what we all say we believe is important?

"The Tendency of the Group Is to Be Conservative"

IT IS clear that certain broad fields of science, such as astronomy, atmospheric research, oceanography, and space research, lend themselves well to cooperative effort. The research institute has for many years been a highly successful institution in a number of European countries, notably Germany, U.S.S.R., and Sweden. It is practically certain that the expanding horizon of research in this country will dictate the organization of new forms of research activity here. Whether the needs can best be met by establishment of special centers for the purpose, or whether coordinated programs should be set up in more decentralized fashion will be a matter for consideration in each case. We must be alert to the weaknesses as well as to the strengths inherent in massive and concentrated effort. Are we likely, for example, to overemphasize group activity at the expense of the individual researcher? Certainly history indicates that capital discoveries can usually be attributed to a single person or a few individuals, although it is quickly admitted that their particular contributions may be only the climax of a host of smaller research contributions. Those who are familiar with group activities will probably agree, if they are entirely candid, that the tendency of the group is to be conservative although powerful, and, in its dedication to its objective, to react rather conservatively to radical ideas or subject matter lying on the periphery of its main activity. Furthermore, an organized group tends to achieve a singleness of purpose and of method (and occasionally "conceit") which by its very nature is apt to ignore ideas from outside. —DR. ALAN T. WATERMAN,
Director, National Science Foundation, at the Basic Science Symposium.

PERSONALITY PORTRAIT—XXXIX

ATOM AGE MONA LISA: DR. LISE MEITNER

Only An Enigmatic Smile Tells How Her Mind's Eye Saw That Atomic Nucleii Could Fission Like Germs

BRYN MAWR, PA.

AS A woman, I have a certain pride in knowing that the atom age began in a woman's mind. And it has seemed to me that if that beginning could become familiar to schoolgirls, many more women might be inspired to interest themselves in science. So, when I heard that Dr. Lise Meitner had come to Bryn Mawr to lecture in May, I resolved to pay her a call and ask her to tell me the story. For it was in Dr. Meitner's mind that the great moment arrived.

What I have to report from that meeting is stranger than anything I expected. I have discovered a modern Mona Lisa.

Dr. Meitner's smile is lit with hypnotic charm. Her eyes project a glow that can be felt across a room. She talks with wit and grace on any subject the conversation turns to—except her own most notable piece of work.

She is enchanted with the white mouse that two Bryn Mawr students keep as a dormitory pet. She delights in the warmth and fragrance of American spring. She tells, apologetically, of climbing only halfway up the Jungfrau when she was 75. She is sorry she cannot understand Schoenberg's music. She laughs at the way she bamboozled her doctor into approving her latest Atlantic crossing. She avidly explores the theory of the neutrino. She deftly (and briefly) traces her career in terms of beta decay (the slow disintegration of radioactive atoms). But she will not discuss the historic events that began with her receipt of Otto Hahn's Christmas card in December of 1938.

"I am sorry," she told me, with firm politeness. "It is a rule of mine."

The reason for the "rule" has never been explained to anyone. There are only suppositions about it. Foremost among them is that Dr. Meitner has never been reconciled to the destructive use society has made of her brainchild. It was peaceful power—not nuclear bombs—that she visioned when she showed Hahn's card to her nephew, O. R. Frisch, a physicist at Niels Bohr's Institute for Theoretical Physics in Copenhagen.

"It took her a while to make me listen," Frisch wrote subsequently, "but eventually we got to arguing about the meaning of Hahn's result."

Hahn had sent the result with his holiday greeting to Miss Meitner in Stockholm. The message was contrite. Up to the time of her flight from the Nazi pogroms in Germany a few months before, she and he together had spent years in bombarding atoms with slow neutrons. All their work now seemed to have come to naught. Uranium (atomic weight 92) had not broken down into an isotope of radium (atomic weight 88) as Hahn had thought. Mme. Joliot-Curie had proved in France that the newly transmuted staff was really barium (atomic weight 56), hardly better than half the weight of uranium. Who could possibly make that fit into the theory of step-by-step atomic change?

"Very gradually," as Frisch recalls it, his aunt Lise "realized that the nucleus (of the atom) had to be pictured in quite a different way. The picture is not that of a 'particle' (the invading neutron) breaking through a potential barrier, but rather the gradual deformation of the original uranium nucleus, its elongation, formation of a waist and finally separation of the two halves."

In short, Dr. Meitner in Stockholm saw in her mind's eye what Otto Hahn had missed in his laboratory lenses in Berlin: he must have split the atom!

Her mental picture of this division was so like the one biologists see when bacteria multiply that she called it by the same name: fission.

Acting on faith in her intellect alone, with no experimental evidence to support her, Dr. Meitner wrote, in company with her nephew, a letter to the British journal, *Nature*. Published in the issue of February 11, 1939, under the heading, "A New Type of Nuclear Reaction," it said: "It seems possible that the uranium nucleus has only small stability of form, and may, after neutron capture, divide itself into nuclei of roughly equal size (the precise ratio of sizes depending on finer structure features and perhaps partly on chance), these two nuclei will repel each other and should gain a total kinetic energy of around 200 million electron volts."

—Photo by Davis.

Dr. Lise Meitner (fourth from left) with admirers at Bryn Mawr.

Before the letter got into print, Frisch returned to Copenhagen from his Stockholm holiday. There he gave the news to Bohr, who was just about to sail for the United States. Here Bohr passed the word to researchers in American laboratories, who quickly confirmed that when uranium was bombarded with neutrons, the splitting atoms released the energy Dr. Meitner had predicted from Einstein's famous equation: $E = mc^2$.

There is a moral to this story. It lies in O. R. Frisch's words: "It took her a while to make me listen." Even now, at the age of eighty, Lise Meitner has the power to make people listen. The determination that bars discussion of her own epochal contribution to human thought operates automatically in all directions. It keeps her young in spite of the years. It lends a lightness to her walk, a straightness to her back that belies a slight bend of the shoulders. It is a built-in habit, grown strong through exercise.

Contrary to what might be supposed from prevailing custom in this country, it required no willpower on young Lise Meitner's part to persuade her parents that science was a proper career for a girl to contemplate. Her father, a prominent lawyer in Vienna, encouraged all three of his daughters to be equally interested with his five sons in study of the natural sciences. Shy, serious Lise was attracted to the then new discoveries in radioactivity by Henri Becquerel and the Curies. A doctorate in physics at the University of Vienna was almost a matter of course. But when Dr. Meitner presented herself to the famous Max Planck at the University of Berlin in 1907 as a prospective auditor of his lectures in physics, "He said, 'You have a doctor's degree, what more do you want?'"

Another listener to Planck's teachings was Otto Hahn, a young German chemist. He invited Lise to work with him at the Emil Fischer Institute for Chemistry. But because she was a woman, Fischer refused to allow her to go upstairs to the laboratories where Hahn performed his experiments. She was put in a carpenter shop in the basement—a cluttered, damp, and often cold place where she spent many lonely but satisfying hours in measurement of the atomic structure of two naturally radioactive elements: radium and thorium. Later Hahn moved down to the basement and equipped a chemical laboratory there to enable them to work together. Within five years the same Max Planck who had greeted her so skeptically appointed her as his assistant at the Max Planck Institute of Theoretical Physics. And in 1917, after her reputation for brilliance was fully established, Emil Fischer finally allowed her to enter the upstairs laboratories of his Institute—as head of her own physics section. It was there that she and Hahn, in the study of beta decay, began the experiments which ended only after Dr. Meitner had to flee Hitler's persecution of the Jewish people.

In Sweden this indomitable lady found freedom and honor. The Nobel Institute of the University of Stockholm provided her with a laboratory. The Swedish Academy of Sciences elected her as its only living woman member. From her native Vienna came the Max Planck Medal. From the United States came offers of visiting lectureships (at Catholic University in 1946, in 1959 at Bryn Mawr), and honorary degrees) from Smith, Rutgers, Syracuse, and Adelphi).

Full as her life has been, no one has the prerogative to lengthen it, as one recent news reporter inadvertently did. Just a trace of indignation entered her voice after she read the item. "Why I'm not eighty-two, I'm eighty!" she said with a mathematical precision that every woman will understand.

—ROBERTA SILMAN.

SCIENCE IN BOOKS

WHAT THEY ARE TEACHING YOUNG SOVIET DOCTORS

This Translation of a Russian Professor's Notebook Emphasizes Old Fashioned Sympathy for the Patient

THE question—do we treat the disease or the patient?—has been asked on many occasions and for a long time, but we still have to concern ourselves with it today.

We often tend to concentrate our attention on some organ or other and to speak of its diseases, although pathological physiology has shown us that in the disrupted function of an individual organ there is always a response by the body as a whole. This unity of the body also includes the psychic sphere. Later on we shall deal scientifically with typical pictures of diseases, but without dismissing from our minds for one moment the fact that "there is no sickness in a man but there is a sick man."

Disease and the environment constitute an indivisible entity; to treat them separately is to fall into a profound methodological error. When leaving medical school nowadays, the doctor may not begin his medical career without having gained an unmistaken notion of disease as a social phenomenon.

For practical purposes, the patient is an individual with some disorder of his bodily function and of his ability to work. The patient himself always imagines himself to be a complex of subjective sensations and mental experiences. This accounts for the vagueness which we often meet in the patients' complaints: "somewhere inside" they can feel something: "it isn't a pain nor is it a burning sensation": they cannot translate their sensations accurately into words.

Of course, all patients do not react in the same way to their sensations. A lot depends on their type of nervous system. People with an optimistic makeup often ignore their sensations for a long time; they do not lose their spirits even in serious illnesses. The patient with the pessimistic outlook rapidly withdraws into his own innermost world when he is ill; he is pursued by all manner of depressing ideas; how long must he endure this misfortune which has overtaken him—perhaps it will turn into a chronic condition—how will he and his family carry on—will he live? In this way the patient creates new symptoms; becoming pessimistic, he ceases to believe in the diagnosis made by the doctor who is treating him, he does not believe that they are telling him the truth about his condition; he suspects that they are hiding something from him and he is ready to refuse the services of his, up to now, "dear, kind, and attentive doctor."

This type of patient is well described by the neat phrase: "Every patient suffers from his illness plus fear." The emotions of fear are highly individual: a simple cold in the head causes some patients to be much more afraid than others with really serious illnesses.

The doctor is making a great mistake if, in examining the patient, he confines himself to percussion and auscultation and to writing a prescription and does not concern himself with the patient's mental state. Every disease affects the mind to some extent, and the mind, in turn, affects the course of the disease. Although he is logical and sober in relation to all other questions, the patient loses his logic or shows a lack of it in relation to himself. This is not abnormal psychology but the psychology of the human patient.

The mind of the human patient is very susceptible to outside influences. He looks around for something to bolster up his courage, reassure himself and imbue himself with faith in his recovery. He craves reassuring conclusions from the doctor, and is afraid of discouraging ones. He cannot stand vague answers. The conditional "if" does not satisfy him. He wants only a categorical answer from the doctor, one without any conditions or beating about the bush.

The patient's thoughts are in accordance with those sensations and ideas which make him aware of his