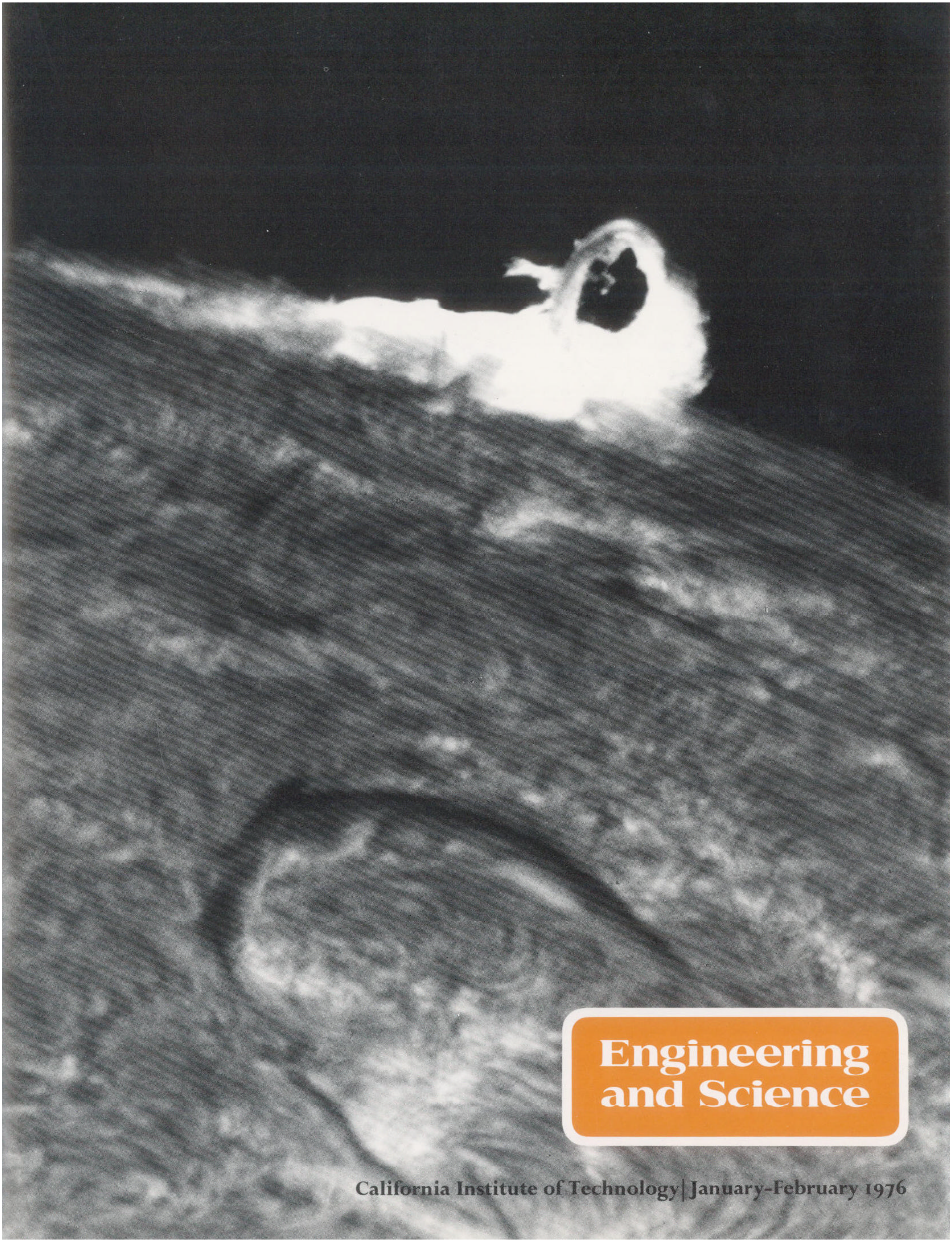
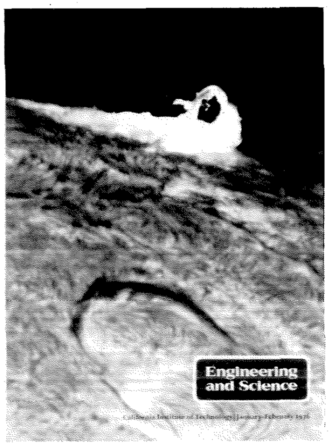


Engineering
and Science
California Institute of Technology | January-February 1976

In This Issue



Solar Flare

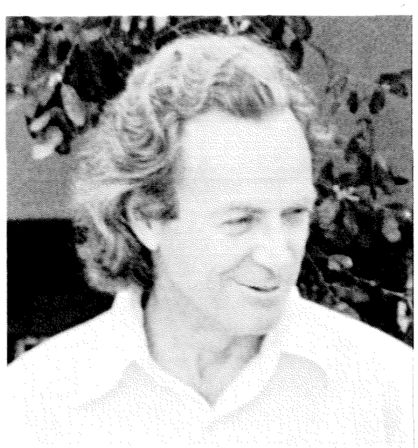
On the cover—a spectacular solar flare, photographed at Caltech's solar observatory on Big Bear Lake on July 9, 1974, at precisely 23:42:12 universal time. As an indication of the size of this flare—the hole inside the loop shown on the cover is about twice the size of the earth. Even so, this is not a particularly large flare. It is, however, one of the most pictorially impressive ones to be recorded at the observatory—as you can see from the photographic evidence on pages 32 and 33. It is worth noting that the shadows you can see in the loop, in these pictures, are rarely, if ever, observed, and are still not understood. Also note that this whole photographic record covers only 14 minutes.

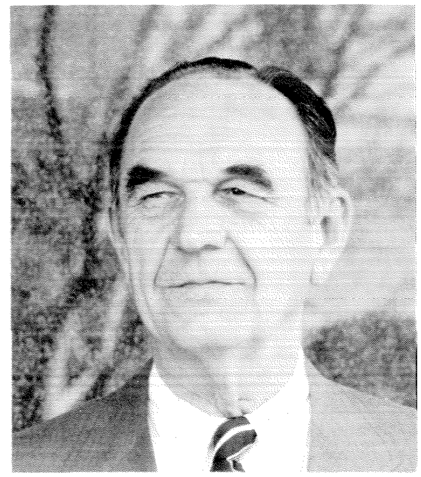
The Way It Was

Richard P. Feynman, who is Richard Chace Tolman Professor of Theoretical Physics at Caltech, and winner of the 1965 Nobel Prize in physics, spent the war years working on the Manhattan District atomic bomb project, first at Princeton University, then at the Los Alamos Scientific Laboratory.

He recalled those years last February, in a public lecture at the University of California at Santa Barbara. His was one of nine talks given in a lecture series there, under the general heading of "Reminiscences of Los Alamos, 1943-1945." The other speakers included George Kistiakowsky, Laura Fermi, Edwin McMillan, and Norris Bradbury, and all the talks are now being edited for book publication by William Badash, professor of history at UCSB. It's a book we look forward to with relish, after sampling only the Feynman talk, "Los Alamos From Below"— which you'll find on page 11.

The article has been adapted from a direct transcript of the talk given by Feynman at Santa Barbara on February 6, 1975.





The Chairman

R. Stanton Avery has been chairman of the Caltech board of trustees since May 15, 1974, when he succeeded Arnold Beckman, who had served as chairman for 10 years. A member of the board since 1971, Avery is founder and chief executive officer of Avery Products Corporation, the world's leading manufacturer of self-adhesive products, with headquarters in San Marino. He is one of a small handful of executives in this country — like Arnold Beckman, Edward Carter, Justin Dart, and William Paley — who founded his own company and is still running it after more than 40 years. Avery Products began as a one-man, part-time, mail order organization. It is now a multinational corporation which last year entered the ranks of the 500 largest manufacturing companies in the U.S.

Some highlights in the life of the remarkable man who accomplished this, and an introduction to your chairman of the board — on page 8, "He Might Have Been Written by Horatio Alger."

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The Social Sciences at Caltech–

A Progress Report

CALTECH'S graduate program in social science represents a major break with tradition, both at the Institute and in the academic community at large. It is the first graduate program to be offered in the Division of the Humanities and Social Sciences; and it is the first of its kind anywhere: a program in social science that is interdisciplinary but scientific, theoretical but directed toward solution of current socioeconomic problems.

The program is primarily designed to prepare students to assume senior staff positions in policy-making organizations where they will be able to conceive and execute complicated research projects and to utilize the products of their research to provide the basis for actual policy decisions. Students who have more traditional academic research interests are being trained to develop and extend the basis for policy-relevant theory. They will turn to academic teaching positions in economics and political science departments and a growing number of university programs in public policy.

"We've based the program on four fundamental building blocks," says Lance Davis, professor of economics and "convener" of the social science faculty. "We want to give our students a first-class grounding in economic and political theory, in the behavioral sciences, and in measurement. During their first year of graduate work, every student should take theory courses in each of these four fields and should be introduced to the problems that arise when one attempts to apply those theories. In the second year they learn how to apply those tools, and in the third year each student executes a research project of his own."

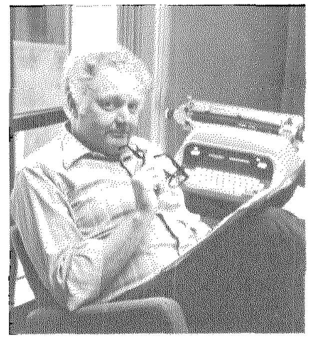
The program differs from a traditional graduate program in the range of disciplines covered. It recognizes the fact that few social problems fall uniquely in the area of economics, politics, or psychology, and that most have elements of all three. In a typical economics PhD program, for example, first-year students would take economic theory and econometrics, but instead of the political science and psychology courses required of the Caltech student, they would take applied courses in economics — labor economics, foreign trade, economic history, or what have you.

In the second year students participate in a year-long policy seminar, to which all of the faculty tries to contribute. For example, Michael Levine, Luce Professor of Law and Social Change in the Technological Society, introduces them to the legal institutions that shape potential solutions of the problems in question. Charles Plott, professor of economics, and Morris Fiorina, associate professor of political science, discuss the possibilities of applying experimental methodology to the solution of social problems. Or Roger Noll, professor of economics, who has served as a senior economist for the President's Council of Economic Advisers and as an adviser to a number of Congressional committees, discusses operational characteristics of various government agencies and the constraints they impose on the solutions to social problems. Or David Montgomery, assistant professor of economics, who also works with the Environmental Quality Laboratory and is spending this year at the Congressional Budget Office in Washington, describes research problems that occur on the interface between science and engineering. Or Robert Forsythe, assistant professor of business economics, introduces them to the usefulness of modern management techniques for certain classes of problems. Most important of all, the students are required to do some policy research work under faculty supervision — perhaps with EQL or the civil systems section of the Jet Propulsion Laboratory.

One such research project was a study by second-year students Steve Matthews and Brian Binger of base housing policies at the Naval Weapons Center at China Lake. The lower-cost housing units at China Lake had always been 100 percent occupied, with a sizable waiting list. In the middle of 1973, people began moving out after it became known that the Navy was going to impose a large rent increase. The Navy requested an analysis of the increase in vacancies and an evaluation of various plans for disposing of excess housing. The possibility of putting several hundred low-cost houses on the market also called for an assessment of the effects on the adjacent community of Ridgecrest.

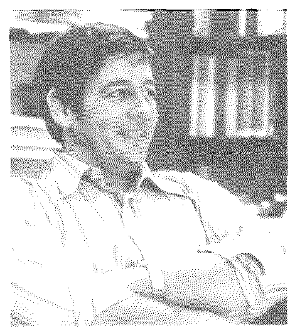
Matthews and Binger's final report not only analyzed the existing situation and what brought it about, but, using established economic forecasting theory, made documented projections and recommendations about the future of the housing situation at the NWC. A crucial finding, since borne out by experience, was a prediction that some types of housing that were still fully occupied when the study began would soon experience a similar increased vacancy rate, a conclusion that the Navy found both surprising and extremely valuable. The students also concluded that the contemplated sale of excess housing would have only a mild, short-term effect on the private housing market.

Evaluating the amount and kind of planning being done by public agencies to try to cope with earthquake-associated problems is another project begun in early 1975. Graduate students Linda Cohen

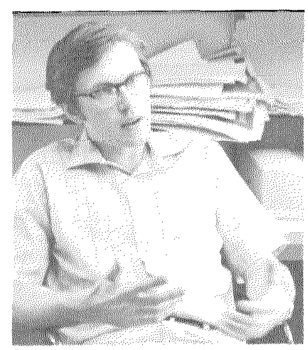


Lance Davis, professor of economics

Michael Levine, Luce professor of law and social change

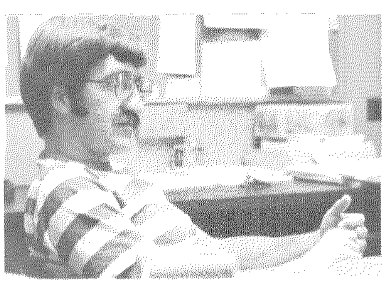


Charles Plott, professor of economics

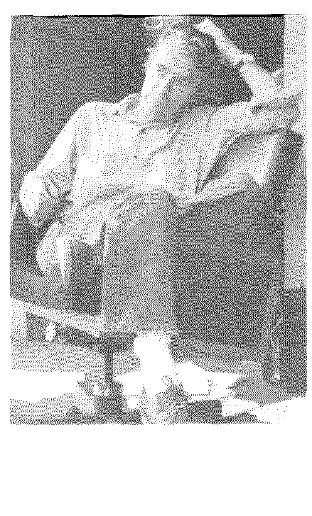


Roger Noll, professor of economics

Robert Forsythe, assistant professor of economics



Morris Fiorina, *associate professor* of political science



James Quirk, professor of economics

and Barry Weingast studied the response of various government agencies to major earthquakes, including the one in San Fernando in 1971, and found several hitherto unnoticed correlations between the performance of an agency and its structure, purpose, and operating procedures before the disaster occurred. Linda Cohen and her adviser, Roger Noll, are also completing a study on building codes in earthquake-prone areas. The study is unique in that it uses engineering models of the capacity of structures to resist earthquakes, geophysical data on earthquake frequency, and economic models of optimal investment strategy to determine — in relation to the earthquake-proneness of an area — what seismic resistance buildings should have for maximum net benefits.

These kinds of applied research projects are good preparation for writing a thesis, and they are also small-scale previews of what the students may expect to be doing after they leave Caltech — helping to establish the basis for policy-making on social problems. "In fact," says Morris Fiorina, associate professor of political science, "in very simple terms this program is based on the fundamental assumption that it is both possible and desirable to study social systems. A great deal is known at a basic level about social systems, but it has been developed along separate academic lines. Small parts (an isolated economic or political event) are frequently fairly well understood, but social scientists have not been particularly successful in putting the parts together. Nevertheless, while social systems are very complicated, they are not beyond understanding, and the need for such understanding is obvious. It is difficult to predict where the important breakthroughs will take place, but it is quite possible that it will be in the area of political economy." In the words of James Quirk, professor of economics, "We may succeed or fail on the basis of how well we integrate economics and political science to come up with something that is the social science equivalent of biochemistry or biophysics."

It is, of course, still early to talk about the "success" or "failure" of social science at Caltech, but it may be time to talk about the end of the beginning, because last September Lee Sparling, the first product of the program, left Pasadena for a job in Washington, D.C., with his course work completed and his thesis well under way.

Sparling first came to Caltech in 1967 as a freshman. As a student, he had the kinds of mathematical interests and abilities that the social sciences at the Institute require. He graduated in 1971 with a BS in both engineering and economics and went on to graduate school at Stanford in economics. But Stanford's rather traditional approach to the subject was not very satisfying, and he was happy to join Caltech's brand-new program in the fall of 1972. The chief inducement, he says, "was that the Caltech program promised to teach not only the basic theory of economics and other social sciences — which is done in any good graduate program — but also how to do empirical work. That combination makes it possible to integrate the theory and apply the empirical work in dealing with some social problems."

In his research, Sparling has been looking into the regulation of freight transportation. Given his background in engineering and his training in social science, it is not surprising he began to wonder how much fuel would be saved if the present regulatory structure was modified so that railroads were freed to compete with other forms of transport. For shipments exceeding about 200 miles, railroads are more efficient than trucks — in terms of both energy consumption and total costs per ton of freight carried — but current regulatory practices prevent them from capturing much of this business. Because regulated shipping rates do not necessarily reflect the cost of providing service, the savings to be gained from better allocation of freight cannot be estimated directly. Instead, it is necessary to "model" the industry's demand, costs, and decision-making procedures to determine what the effect of different regulatory policies would be.

To the layman what a social scientist means when he speaks of "building a model" may not be clear, but to Lance Davis it is a way of characterizing the world. "The world the social scientist attempts to explain is a very complex one," he says, "and it is impossible to understand its operation in all detail. Instead, understanding depends upon simplification; and it is the choice of simplifying models that lies at the heart of the social scientists' art. The model is nothing but a collage of postulates and inferences presented as a mathematical simplification of the processes to be explained. The assumptions, of course, relate to the behavior of the people and institutions whose actions are the subject of the explanation.

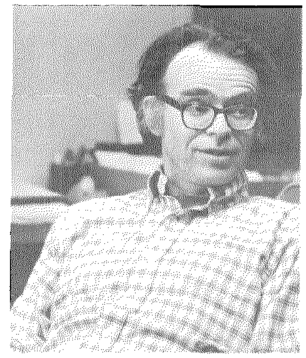
"The mark of successful model building is the simplifying assumption that permits the capture of the essence of the problem while ignoring those parts of reality that are not important. The social scientist must choose his model, estimate its parameters, and test its predictive power. Behind the model lies an understanding of theory, a detailed knowledge of economic and political institutions, and a psychologist's insight into human behavior. Estimation and testing, in turn, involve statistics, econometrics, laboratory experiments, and computer simulation techniques."

If Sparling's model is a useful description of how things *are* in the transportation industry — and the only way to test that is to see if it accurately explains and predicts the industry's performance — then he can build various constraints into his model to tell him how alternative regulatory policies would affect the decision-making process.

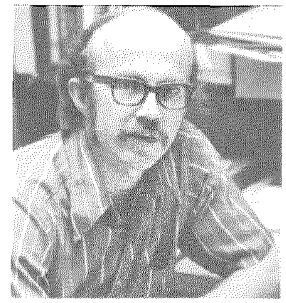
Building Sparling's particular model of a railroad has been a complicated problem in applied mathematics and statistics. For example, he found it necessary to develop a complex non-linear programming model to determine the least costly ways to route boxcars if wholesale changes occurred in the amount and mix of railroad shipments. Based on his work to date, he estimates the cost to the economy of transportation regulation is about $2.5 billion to $4 billion per year, which, though enormous, is half of what others have estimated using cruder techniques. Already his results have played a role in the policy debate, as they were cited and discussed in testimony before a Senate committee investigating the issue of regulatory reform.

A year ago Sparling began looking for a job. Rather than pursue the usual academic openings, he had interviews with both government agencies and private research organizations. Eventually he chose to join the Economic Policy Office of the Antitrust Division of the Justice Department, where he is now involved in an investigation of what economists call industrial structure. Some of his work is in response to requests from attorneys in the middle of antitrust cases, some is research to determine whether antitrust action may be called for, but much is further pursuit of his own research interests in transportation regulation. And it is exactly what he had in mind when he signed up for a Caltech social science PhD three years ago.

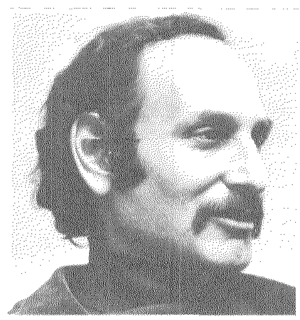
Meanwhile, back in Pasadena, the departmental faculty is working with a group of students whose numbers and needs are growing. The students, like the faculty, are diverse in their formal academic training. Of the fourteen current students, five were mathematics majors as undergraduates, six were economics majors, and one each majored in engineering, biology, and history. Four were Caltech undergraduates, and six began their graduate careers at another university in traditional disciplines. All have outstanding academic records and would qualify for admission to first-rate graduate programs in traditional fields. Of the group, six are in their first year and eight are more advanced students in the process of choosing thesis topics and advisers. In addition, one student, pursuing both a social science and a law degree, is on leave, taking second-year law courses at USC.



Thayer Scudder, professor of anthropology



Robert Bates, associate professor of political science



Louis Breger, associate professor of psychology

Last June, at the end of the first full year of operation, the social science faculty and the students engaged in extensive discussions on how their enterprise was going, and how it might be improved. Both the students and the faculty expressed strong interest in the behavioral fields of social science. This interest underscores a need for expanding the program's capability in psychology — social, experimental, and mathematical. At present, this part of the program is handled by Thayer Scudder, professor of anthropology, Robert Bates, associate professor of political science, and Louis Breger, associate professor of psychology. With only three faculty members covering the entire range of relevant behavioral sciences — anthropology, psychology, sociology, and political behavior — some behavioral areas that are central to the social science program are being completely neglected.

The students also pointed out that economists and political scientists tend to use the same tools, something that the faculty knew but had never acted upon. As a result of these comments, several courses are now being jointly taught by faculty from both these disciplines. An extension of interdisciplinary teaching appears in SS 150 abc, Social Science Aspects of Technology. Funded by a grant from the Alfred P. Sloan Foundation, this year-long course is oriented toward seniors and graduate students in engineering. The first term deals with the theory of the operation of a competitive price system; the second, with problems caused by distortions within a competitive market; and the third, with specific problems of particular concern to engineers — for example, congestion, energy, pollution abatement, land-use planning, earthquake hazards, and the social consequences of disasters. The last term is taught jointly by a member of the social science faculty and by a member of the engineering faculty. Eventually there will also be a series of seminars dealing with the social aspects of current engineering research designed for graduates at the thesis-writing level.

Not only is the social science teaching program interdisciplinary, but the faculty crosses disciplinary lines in their research. "The integrating factors," says Plott, "are their mutual interest in policy analysis and their appreciation for the importance of basic theory and measurement."

For example, says John Ferejohn, "On the basis of certain assumptions, an economist attempts to analyze human behavior in economic choice-making situations. He can predict, for instance, what will happen to the amount of gasoline sold in the U.S. if a tariff is placed on oil or the price of domestic oil is decontrolled. But many times the policy recommendations concern decision-makers who are not acting in purely economic settings. If you make decisions in government or industry, you want to know not only the effect of your oil policy on sales, but also what the Middle Eastern political responses are likely to be. The appropriate model contains both economic and political components. Fortunately, all our economists at Caltech are very sensitive to the need to model both political and economic behavior in analyzing public policy decisions."

The policy orientation of the program necessitates strong emphasis on measurement techniques for testing models of behavior and predicting effects of policy changes. At Caltech two econometricians, David Grether, professor of economics, and Forrest Nelson, assistant professor of economics, share the responsibility of training students in the use of measurement techniques. According to Grether, "We feel it is important that our students receive training in the use of statistical techniques comparable to that of the best economics program in the country. In addition, the interdisciplinary program requires exposure to a different mix of techniques."

For the last several years Ferejohn and Noll have been working together on the strategy of politicians in political campaigns. Currently they are working on the effect of information about voter preferences on campaign strategies. "At least in the early stages of a campaign a candidate makes decisions based on very imperfect information about the preferences of voters with respect to policy issues," says Noll. "We want to determine the consequences of rational behavior for a candidate — that is, behavior that maximizes his chance of winning — in situations in which information is imperfect. Thus far, our investigations have led primarily to qualitative theoretical results, rather than quantitative predictions. For example, we have found that in circumstances that appear to be quite general, it is in the interests of both candidates to make an agreement not to campaign on the issues that are the most important to the voters."

Although both the teaching and research programs have a strongly applied flavor, faculty research, like the graduate curricula, does not ignore basic science. Extending the theory across this boundary between economics and politics requires basic research. "Good models of behavior and choice in a purely market setting have been around a long time, but social processes which involve both political and market behavior need much more study," says Plott. "Slight changes in procedures and organization, for example, make enormous changes in the outcome."

To facilitate this study, Plott has developed an experimental methodology for examining the impact of subtle changes in rules, procedures, and modes of organization. Levine and Plott, for example, were able to demonstrate that within a large class of majority rule committee settings the agenda alone can be used to determine the committee's decision. Fiorina and Plott, with the aid of a grant from the National Science Foundation, have extended the study to wider classes of procedures including even simple election processes.

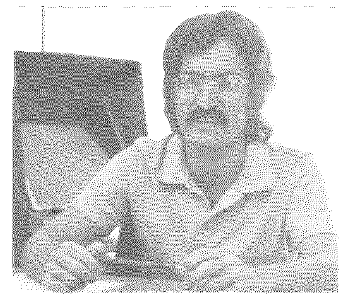
Not only the institutional but also the behavioral assumptions have come under experimental scrutiny by the Caltech faculty. Economists traditionally build models by assuming that everything people do can be explained on the basis of self-interest, but evidence exists that people do not always behave that way. Since successful prediction depends upon an accurate theory of behavior, it is important to determine at what point people cease behaving in what the economists call a rational way. Experimental work carried on by Ferejohn and Grether is designed to discover under what conditions people will behave as the economists predict. Whatever they find, a combination of theory with good experimental design will greatly enhance the social scientists' ability to design effective solutions to social problems.

Despite the innovations of the Caltech faculty, laboratory experimentation in the social sciences is very difficult, and for most of their empirical validation, the social scientists must turn to history. Unlike the more traditional sciences, however, the behavior of people and groups of people frequently depends on their own previous experiences or those of their predecessors, and the rules that govern the relationships between the objects of the theory are not given by nature but depend upon the actions taken by the actors on the social stage. History then becomes an integral part of the social science program. Students have to be aware of the shape and structure of the institutional environment of the historical episode that they choose for their experiments, and they should be cognizant of the evolution of these structures over time. Morgan Kousser, associate professor of history and a student of nineteenth-century legislative behavior, and Lance Davis, an economic historian, offer a second-year course in "cliometrics." In their research, both use social science models to explain history, and the course focuses on the changing nature of social institutions and their importance to social science research.

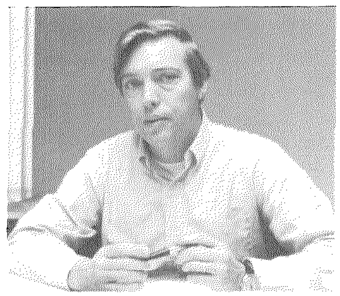
History provides comparisons over time, but variations also occur across cultures. The study of such variation traditionally falls within the purview of anthropology, and Scudder's work offers an opportunity both for testing social science theories across cultures and for developing policy that is relevant to problems of development. The focus of these studies has been on Zambia, and the interdisciplinary nature of the work is underlined by cooperation with Bates, who studied the impact of governmental policies at the village level in Africa. In this area too, the program exhibits its ability to produce truly collaborative work by scholars from several social science disciplines and to focus this work on matters of public policy.

It is going to take a lot of research — experimental, empirical, and theoretical — by all varieties of social scientists to get very far with such problems. But at Caltech the social scientists are in the business of doing just these kinds of research — and of training a small and very talented group of students to do it too.

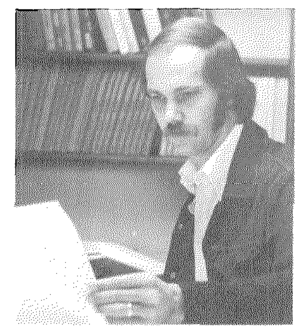
"What we're doing isn't possible in a typical economics, political science, or statistics department," says Roger Noll, who was a 1962 graduate of Caltech in mathematics. "It's multidisciplinary, requiring people — faculty and students — who have the technical talent to become expert in several different traditional fields,



John Ferejohn, associate professor of political science



David Grether, professor of economics



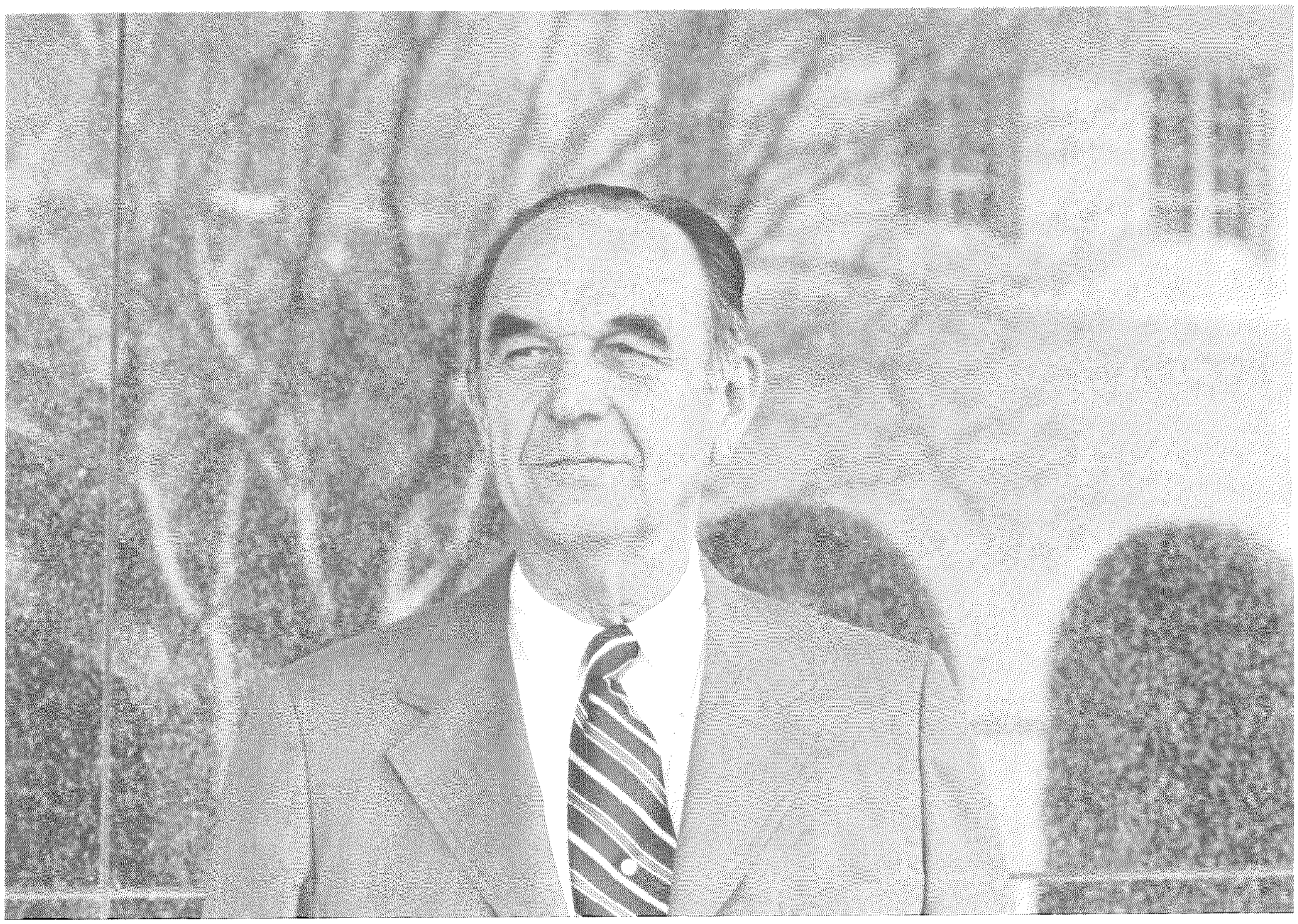
Forrest Nelson, assistant professor of economics

and have an interest in applications. There aren't very many of those people, but we have some and we're training more.

"This kind of social science is exciting and innovative. As such, it is in keeping with the Caltech tradition of getting good people who are at the very forefront of research, and then encouraging them to work on an extremely difficult problem that almost no one else has even attacked.

"It was a big risk to set up the social science program at Caltech, but when Hale came out here to build his telescope, that was a risk, too. Caltech is where it is today because it has been a place that takes big risks to do very difficult things, and that's the only way social science makes sense here."

How successful the innovation will be is still not clear. However, Robert A. Huttenback, chairman of the division, thinks this program may be on the verge of reaching its goal of integrating the social sciences and bringing that unified theory to bear on pressing socioeconomic problems. If he is right, Caltech could once again manage to do something that other institutions have long sought after but never achieved. □



He Might Have Been Written by Horatio Alger

Some notes on R. Stanton Avery, new chairman of Caltech's Board of Trustees

R. STANTON AVERY, chairman of Caltech's Board of Trustees, is one of a small handful of executives in this country — like Arnold Beckman, Edward Carter, Justin Dart, and William Paley — who founded his own company and is still running it after more than 40 years. Starting out in 1935, today Stanton Avery, at age 69, is chairman and chief executive officer of Avery Products Corporation.

Avery Products, which began life as a one-man, part-time, mail-order operation, is now a multi-national corporation, with about 6,000 employees, equally divided between here and abroad. Last year the company entered the ranks of the 500 largest manufacturing companies in the U.S., with sales of nearly $300 million. What Avery sells today is a variety of self-adhesive papers, foils, films, labels, and applicating machines — all arising from the original self-adhesive stickers it first made and sold in 1935.

The manufacture of self-adhesive labels, dispensers, and such, now comprises roughly 45 percent of Avery's total volume. Another 46 percent is done through the sale of self-adhesive papers, foils, and films to the printing trades, to silk screeners, to other label makers and to industry for such things as truck and aircraft markings, double-sided self-adhesive films and plastic foams for sticking things together or as parts of finished products like the plastic bracket you may have stuck on your bathroom mirror with a pad of self-adhesive foam for your electric razor.

The summer of 1932 was not the best time in the world to be looking for your first job, but that's what Stan Avery was doing. He was fresh out of Pomona College, and his training was not very specialized. He was a minister's son, and business was probably the last thing his father had in mind for him. Though he had worked his way through college, it had been at such things as printing cards and programs on a one-armed printing press, washing dishes, parking cars, and working as night clerk in the Midnight Mission in Los Angeles.

Avery had shown considerable enterprise in college, however, by joining with nine other students to spend a year in China. The ten students all lived in the same house at Pomona (dormitories hadn't yet been established) and one of the group was a Chinese boy, Sik Leong Tsui, born in China but raised in Hawaii, whose desire to return to China was so strong that his friends decided to drop out of school for a year and travel through China with him.

They organized The Oriental Study Expedition of Students from Pomona College. Two students were appointed co-managers, and everyone got summer jobs. With their earnings and contributions from the Mudds, the Scrippses, and their own families, the students managed to raise nearly $12,000, and took off for the Orient in the fall of 1929 — just two weeks before Wall Street laid that famous egg.

In China the Pomona group lived mainly on the campuses of Christian colleges, and took courses whenever they could. They spent almost three months in Canton, then worked their way north as the weather warmed up, staying another three months in Shanghai and three more in Peking.

It was an experience the students never forgot — and one that Avery thought he might capitalize on after graduation. For a short time, at least, he considered becoming an importer of Oriental goods. As a start he ordered a sample case of smoked Tahitian bananas from a firm in Papeete, with the thought of becoming their representative for the U.S. But, by the time the samples had been sampled (the bananas were delicious) and a re-order placed, the company had gone out of business.

After working for a while as joint head of the "Hollerith Department" — the early form of punch-card data processing — in the Los Angeles Department of Charities, Avery took a job with the Adhere Paper Company, a pioneer in the field of self-adhesive paper products, at the invitation of Don Dreher, one of the ten Pomona students on the Oriental Study Expedition. Dreher had a 1/5th interest in the company — and considerable confidence in Avery's abilities.

Avery had always had a strong streak of Yankee ingenuity. (After all, one of his ancestors invented the first nail-making machine early in the nineteenth century, and Avery Tractors were once important farm machines.) Even in college he kept inventing things, without having the time or money to develop them. At Adhere, then, his job was to design, build, and operate the machines for Adhere's line of strip-gummed self-adhesive papers, self-adhesive funeral signs for automobile windshields, and double-sided stickers called Gum Tacks.

Unfortunately, the Adhere Paper Company did not flourish, and Avery soon left them to work as a part-time bookkeeper and flower packer in the Los Angeles Flower Market.

Avery had designed and built a machine for making an improved form of Gum Tack for Adhere. When sales on the Gum Tacks failed to develop, however, he acquired Adhere's interest in the machine in return for unpaid back wages to finally sever their relationship.

In his spare time Avery soon converted his machine to the manufacture of self-adhesive stickers mounted in multiple on common backing sheets.

These were the first commercially successful self-adhesive labels. Up to then, labels had to be moistened, like stamps, before they would stick to anything. From the start, Avery labels had removable backing — which covered the adhesive and made it possible to print the label, die-cut it, or package it without sticking together.

Avery called his first products Kum-Kleen labels. He sold them by mail (1,000 for $1) to gift shops and other small retail stores. Since he had no capital he only made up labels when he got an order. Later, when customers asked to have their names put on the labels, he added his first printing press.

The Kum-Kleen label business grew rapidly and steadily. At first Avery ran it alone, and still worked half the day in the flower market. Soon it became a full-time job. Then Mrs. Avery began to help out. Then they got a friend in. Then another. Everybody got $20 a week, including Avery.

At this point an interesting thing happened; the plant burned to the ground. Only an incurable optimist like Avery could have seen this as a great opportunity, but he built new machinery with the $3,000 fire insurance, modernized his production line, and within 30 days was back in business, using a new process which is today basic to the entire self-adhesive label industry.

Another catastrophe that proved to be a boon to the business was World War II. The water-moistened glue used in ordinary labels required labor, and labor was scarce and costly. But Avery labels not only used a synthetic adhesive; they could also be quickly applied, so all kinds of special uses were found for them. For example, when the armed services needed labels for their new Mae-West-shaped radio transmitters that wouldn't peel off in seawater, Avery supplied them — by the millions.

Avery Products was really booming by 1950 when the biggest disaster of all threatened its existence. Avery was practically alone in its field. Then it found that the largest label company in the country was using the Avery die-cutting process. Avery brought suit for patent infringement and lost. The field was wide open.

"It's really dramatic to see how private enterprise

It's really dramatic to see how private enterprise works when competition sets in

works when competition sets in," Avery says. Until then Avery had a monopoly and as much business as it could comfortably handle. The company's growth had always been steady. Now it was spectacular. When it lost its patent suit, Avery Products was doing about $1 million a year. The next year it went to $2 million. It's been soaring ever since.

No wonder Avery has become an eloquent spokesman for the private sector of business.

Not that he intended to, of course. It actually came about because his nephew, Sanny White, was taking a course in the Colonial Foundations of History at the University of Michigan in Dearborn. Annoyed at what he thought was the instructor's anti-business bias, Sanny finally spoke out. The professor was not at all surprised to hear this complaint. "You see," he said, "I try to present *both* sides of *all* subjects in this class — which means I am offending *somebody* all the time."

To make it up to Sanny, then, the professor suggested that he take over the next session of the class so he could present the case for business. Sanny demurred; *he* couldn't do that, he said — but he had an uncle who could. Fine. Bring on the uncle.

So Sanny put in a despairing call to Avery and begged him to come to Dearborn. After all, Avery was just what the class needed — a capitalist member of the Establishment who could explain the virtues of capitalism to the younger generation. Avery had never really thought of himself in this way, but it was a perfectly true statement. So he agreed to give the talk and, for the first time, began to think about business as a social institution.

By the time he stood before the history class of the University of Michigan in December 1970, Avery had formulated the thoughts he has been asked to present from numerous public platforms ever since. After introducing himself to the class, Avery listed a few disclaimers:

"I am not representing anyone but myself.

"I am not a student of anything. I'm an observer.

"I am not a member of the Chamber of Commerce or the National Association of Manufacturers.

"I am not a Republican — and not really a Democrat, though I have been registered as one for 40 years.

"Avery Products manufactures self-adhesive products — price stickers, instruction labels, signs, bumper stickers.

"As a capitalist, I am particularly proud of the multi-national nature of our operations today. We completed a new plant in Grenoble, France, last January. We are about to finish a tape factory in Brazil. My wife and I recently visited our new tape plant in Cramlington, England. We have more than 15 factories outside the U.S.

"One of the benefits from overseas operations is that you never return without being happy all over again with the fact that our country has an extraordinarily stable form of government. And that we have an economic system which permitted me to express myself by starting a business which I do not think would have been started by a government bureau or under anything but our private enterprise system. (Self-adhesive materials are still not being manufactured in Russia and only recently in the Iron Curtain countries to my knowledge, though it is an industry with sales of somewhere between ½ billion and a billion dollars in the free world today.)

"What does this mean to our country and the world?

"Avery makes a very small contribution in comparison with the giant industries in Detroit, (some of which started the same way!) but it counts up."

To shift, in mid-speech, to Stanton Avery addressing the Caltech Management Club in May 1975:

"In the absence of the necessary humility to make me keep quiet," he told his 1975 audience, "I thought you might be interested in the thoughts that have evolved since that first discussion of the private sector with a class in Colonial history in Michigan. . .

"In 1974, Avery Products was paid almost $300 million for our self-adhesive papers, foils, films, labels, and applicating machines by our customers on a worldwide basis. . .

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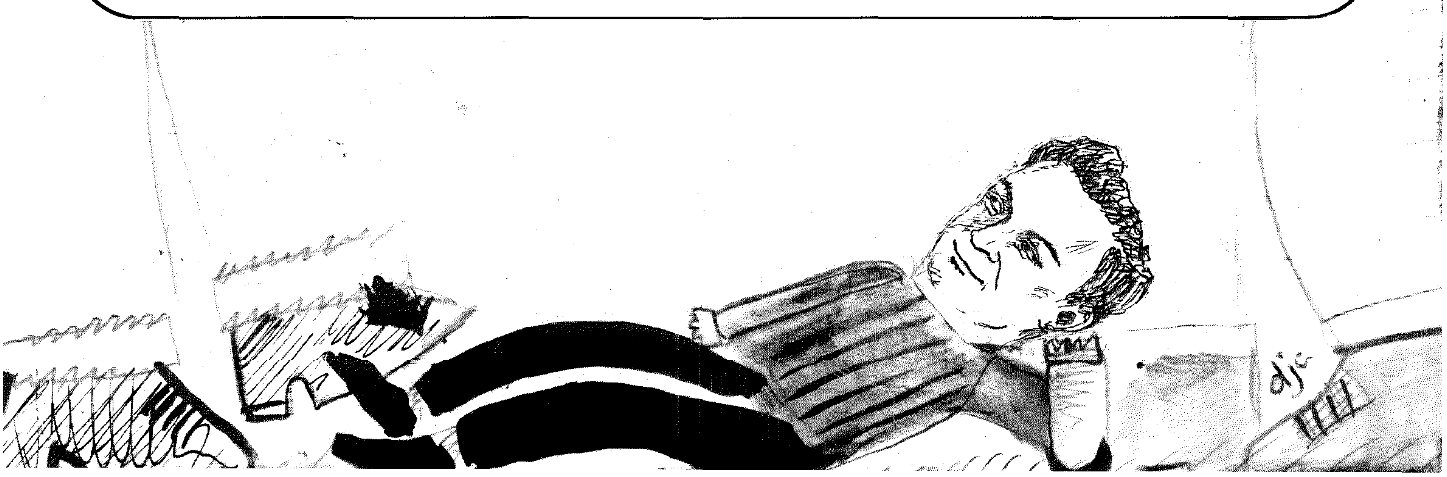
Los Alamos From Below

Reminiscences of 1943-1945

by RICHARD P. FEYNMAN

Illustrations by David Clemesha



WHEN I say "Los Alamos From Below," I mean that, although in my field at the present time I'm a slightly famous man, at that time I was not anybody famous at all. I didn't even have a degree when I started to work with the Manhattan Project. Many of the other people who tell you about Los Alamos — people in higher echelons — worried about some big decisions. I worrked about no big decisions. I was always flittering about underneath.

So I want you to just imagine this young graduate student that hasn't got his degree yet but is working on his thesis, and I'll start by saying how I got into the project, and then what happened to me.

I was working in my room at Princeton one day when Bob Wilson came in and said that he had been funded to do a job that was a secret, and he wasn't supposed to tell anybody, but he was going to tell me because he knew that as soon as I knew what he was going to do, I'd see that I had to go along with it. So he told me about the problem of separating different isotopes of uranium to ultimately make a bomb. He had a process for separating the isotopes of uranium (different from the one which was ultimately used) that he wanted to try to develop. He told me about it, and he said, "There's a meeting —."

I said I didn't want to do it.

He said, "All right, there's a meeting at three o'clock. I'll see you there."

I said, "It's all right that you told me the secret because I'm not going to tell anybody, but I'm not going to do it."

So I went back to work on my thesis — for about three minutes. Then I began to pace the floor and think about this thing. The Germans had Hitler and the possibility of developing an atomic bomb was obvious, and the possibility that they would develop it before we did was very much of a fright. So I decided to go to the meeting at three o'clock.

By four o'clock I already had a desk in a room and was trying to calculate whether this particular method was limited by the total amount of current that you get in an ion beam, and so on. I won't go into the details. But I had a desk, and I had paper, and I was working as hard as I could and as fast as I could, so the fellows who were building the apparatus could do the experiment right there.

It was like those moving pictures where you see a piece of equipment go *bruuuuup, bruuuuup, bruuuuup*. Every time I'd look up, the thing was getting bigger. What was happening, of course, was that all the boys had decided to work on this and to stop their research in science. All science stopped during the war except the little bit that was done at Los Alamos. And that was not much science; it was mostly engineering.

All the equipment from different research projects was being put together to make the new apparatus to do the experiment — to try to separate the isotopes of uranium. I stopped my own work for the same reason, though I did take a six-week vacation after a while and finished writing my thesis. And I did get my degree just before I got to Los Alamos — so I wasn't quite as far down the scale as I led you to believe.

One of the first interesting experiences I had in this project at Princeton was meeting great men. I had never met very many great men before. But there was an evaluation committee that had to try to help us along, and help us ultimately decide which way we were going to separate the uranium. This committee had men like Compton and Tolman and Smyth and Urey and Rabi and Oppenheimer on it. I would sit in because I understood the theory of the process of what we were doing, and so they'd ask me questions and talk about it. In these discussions one man would make a point. Then Compton, for example, would explain a different point of view. He would say it should be *this* way, and he would be perfectly right. Another guy would say, well, maybe, but there's this other possibility we have to consider against it.

I'm jumping! Compton should say it *again!* So everybody is disagreeing, all around the table. Finally, at the end, Tolman, who's the chairman, would say, "Well, having heard all these arguments, I guess it's true that Compton's argument is the best of all, and now we have to go ahead."

It was such a shock to me to see that a committee of men could present a whole lot of ideas, each one thinking of a new facet, while remembering what the other fellow said, so that, at the end, the decision is made as to which idea was the best — summing it all up — without having to say it three times. So that was a shock. These were very great men indeed.

It was ultimately decided that this project was *not* to

"Los Alamos From Below" has been adapted from a talk given by Richard P. Feynman, Richard Chace Tolman Professor of Theoretical Physics at Caltech, in the First Annual Santa Barbara Lectures on Science and Society, given at the University of California at Santa Barbara in 1975. This was one of nine lectures presented in a series of "Reminiscences of Los Alamos, 1943-1945." The lectures are now being edited for publication by Dr. Lawrence Badash of the Department of History, UCSB.

be the one they were going to use to separate uranium. We were told then that we were going to stop, because in Los Alamos, New Mexico, they would be starting the project that would actually make the bomb. We would all go out there to make it. There would be experiments that we would have to do, and theoretical work to do. I was in the theoretical work. All the rest of the fellows were in experimental work.

The question was — What to do now? Los Alamos wasn't ready yet. Bob Wilson tried to make use of this time by, among other things, sending me to Chicago to find out all that we could find out about the bomb and the problems. Then, in our laboratories, we could start to build equipment, counters of various kinds, and so on, that would be useful when we got to Los Alamos. So no time was wasted.

I was sent to Chicago with the instructions to go to each group, tell them I was going to work with them, and have them tell me about a problem in enough detail that I could actually sit down and start to work on it. As soon as I got that far, I was to go to another guy and ask for another problem. That way I would understand the details of everything.

It was a very good idea, but my conscience bothered me a little bit because they would all work so hard to explain things to me, and I'd go away without helping them. But I was very lucky. When one of the guys was explaining a problem, I said, "Why don't you do it that way?" In half an hour he had it solved, and they'd been working on it for three months. So, I did something! Then I came back from Chicago, and I described the situation — how much energy was released, what the bomb was going to be like, and so forth.

I remember a friend of mine who worked with me, Paul Olum, a mathematician, came up to me afterwards and said, "When they make a moving picture about this, they'll have the guy coming back from Chicago to make his report to the Princeton men about the bomb. He'll be wearing a suit and carrying a briefcase and so on — and here you're in dirty shirtsleeves and just telling us all about it, in spite of its being such a serious and dramatic thing."

There still seemed to be a delay, and Wilson went to Los Alamos to find out what was holding things up. When he got there, he found that the construction company was working very hard and had finished the theater, and a few other buildings that they understood, but they hadn't gotten instructions clear on how to build a laboratory — how many pipes for gas, how much for water. So Wilson simply stood around and decided, then and there, how much water, how much gas, and so on, and told them to start building the laboratories.

When he came back to us, we were all ready to go and we were getting impatient. So they all got together and decided we'd go out there anyway, even though it wasn't ready.

We were recruited, by the way, by Oppenheimer and other people, and he was very patient. He paid attention to everybody's problems. He worried about my wife who had TB, and whether there would be a hospital out there, and everything. It was the first time I met him in such a personal way; he was a wonderful man.

We were told to be very careful — not to buy our train ticket in Princeton, for example, because Princeton was a very small station, and if everybody bought train tickets to Albuquerque, New Mexico, in Prince-

When I said, "I want a ticket to Albuquerque, New Mexico," the man says, "Oh, so all this stuff is for you!"

ton, there would be some suspicions that something was up. And so everybody bought their tickets somewhere else, except me, because I figured if everybody bought their tickets somewhere else. . .

So when I went to the train station and said, "I want to go to Albuquerque, New Mexico," the man says, "Oh, so all this stuff is for *you!*" We had been shipping out crates full of counters for weeks and expecting that they didn't notice the address was Albuquerque. So at least I explained why it was that we were shipping all those crates; *I* was going out to Albuquerque.

Well, when we arrived, the houses and dormitories and things like that were not ready. In fact, even the laboratories weren't quite ready. We were pushing them by coming down ahead of time. So they just went crazy and rented ranch houses all around the neighborhood. We stayed at first in a ranch house and would drive in in the morning. The first morning I drove in was tremendously impressive. The beauty of the scenery, for a person from the East who didn't travel much, was sensational. There are the great cliffs that you've probably seen in pictures. You'd come up from below and be very surprised to see this high mesa. The most impressive thing to me was that, as I was going up, I said that maybe there had been Indians living here, and the guy who was driving stopped the car and walked around the corner and pointed out some Indian caves that you could inspect. It was very exciting.

When I got to the site the first time, I saw there was a technical area that was supposed to have a fence around it ultimately, but it was still open. Then there was supposed to be a town, and then a *big* fence further out, around the town. But they were still building, and my friend Paul Olum, who was my assistant, was standing at the gate with a clipboard, checking the trucks coming in and out and telling them which way to go to deliver the materials in different places.

When I went into the laboratory, I would meet men I had heard of by seeing their papers in the *Physical Review* and so on. I had never met them before. "This is John Williams," they'd say. Then a guy stands up from a desk that is covered with blueprints, his sleeves all rolled up, and he's calling out the windows, ordering trucks and things going in different directions with building material. In other words, the experimental physicists had nothing to do until their buildings and apparatus were ready, so they just built the buildings — or assisted in building the buildings.

The theoretical physicists, on the other hand, could start working right away, so it was decided that they wouldn't live in the ranch houses, but would live up at the site. We started working immediately. There were no blackboards except for one on wheels, and we'd roll it around and Robert Serber would explain to us all the things that they'd thought of in Berkeley about the atomic bomb, and nuclear physics, and all these things. I didn't know very much about it; I had been doing other kinds of things. So I had to do an awful lot of work.

Every day I would study and read, study and read. It was a very hectic time. But I had some luck. All the big shots except for Hans Bethe happened to be away at the time, and what Bethe needed was someone to talk to, to push his ideas against. Well, he comes in to this little squirt in an office and starts to argue, explaining his idea. I say, "No, no, you're crazy. It'll go like this." And he says, "Just a moment," and explains how *he's* not crazy, *I'm* crazy. And we keep on going like this. You see, when I hear about physics, I just think about physics, and I don't know who I'm talking to, so I say dopey things like, "No, no, you're wrong," or "You're crazy." But it turned out that's exactly what he needed. I got a notch up on account of that, and I ended up as a group leader under Bethe with four guys under me.

I had a lot of interesting experiences with Bethe. The first day when he came in, we had a calculator, or glorified adding machine, a Marchant that you work by hand. And so he said, "Let's see." The formula he'd been working out, he says, "involves the pressure squared; the pressure is 48; so the square of 48 is —."

I reach for the machine.

He says, "It's about 2300." So I plug it out just to find out.

He says, "You want to know exactly? It's 2304." And it came out 2304.

So I said, "How do you do that?"

He says, "Don't you know how to take squares of numbers near 50? If it's near 50, say 3 below (47), then the answer is 3 below 25 — like 47 squared is 2200, and how much is left over is the square of what's residual. For instance, it's 3 less and the square of that is 9, so you get 2209 from 47 squared."

So he knew all his arithmetic, and he was very good at it, and that was a challenge to me. I kept practicing. We used to have a little contest. Every time we'd have to calculate anything we'd race to the answer, he and I, and I would lose. After several years I began to get in there once in a while, maybe one out of four. You have to *notice* the numbers, you see — and each of us would notice a different way. We had lots of fun.

Well, when I was first there, as I said, the dormitories weren't ready. But the theoretical physicists had to stay up there anyway. The first place they put us was in an old school building — a boys' school that had been there previously. I lived in a thing called the Mechanics' Lodge. We were all jammed in there in bunk beds, and it wasn't organized very well because Bob Christie and his wife had to go to the bathroom through our bedroom. So that was very uncomfortable.

The next place we moved to was called the Big House, which had a balcony all the way around the outside on the second floor, where all the beds were lined up next to each other, along the wall. Downstairs there was a big chart that told you what your bed number was and which bathroom to change your clothes in. Under my name it said "Bathroom C," but no bed number! By this time I was getting annoyed.

At last the dormitory was built. I went down to the place where rooms were assigned, and they said, you can pick your room now. You know what I did? I looked to see where the girls' dormitory was, and then I picked a room that looked right across — though later I discovered a big tree was growing right in front of the window of that room.

They told me there would be two people in a room, but that would only be temporary. Every two rooms would share a bathroom, and there would be double-decker bunks in each room. But I didn't *want* two people in the room.

The night I got there, nobody else was there, and I decided to try to keep my room to myself. Now my wife was sick with TB in Santa Fe, but I had some boxes of stuff of hers. So I took out a little nightgown, opened the top bed, and threw the nightgown carelessly on it. I took out some slippers, and I threw some powder on the floor in the bathroom. I just made it look like somebody else was there. OK? So, what happened? Well, it's supposed to be a men's dormitory, see? So I came home

It wasn't organized very well because Bob Christie and his wife had to go to the bathroom through our bedroom.

that night, and my pajamas are folded nicely, and put under the pillow at the bottom, and my slippers put nicely at the bottom of the bed. The lady's nightgown is nicely folded under the pillow, the bed is all fixed up and made, and the slippers are put down nicely. The powder is cleaned from the bathroom and *nobody* is sleeping in the upper bed.

Next night, the same thing. When I wake up, I rumple up the top bed, I throw the nightgown on it sloppily and scatter the powder in the bathroom and so on. I went on like this for four nights until everybody was settled and there was no more danger that they would put a second person in the room. Each night, everything was set out very neatly, even though it was a men's dormitory.

I didn't know it then, but this little ruse got me involved in politics. There were all kinds of factions there, of course — the housewives faction, the mechanics faction, the technical peoples faction, and so on. Well, the bachelors and bachelor girls who lived in the dormitory felt they had to have a faction too, because a new rule had been promulgated: No Women in the Men's Dorm. Well, this is absolutely ridiculous! After all, we are grown people! What kind of nonsense is this? We had to have political action. So we debated this stuff, and I was elected to represent the dormitory people in the Town Council.

After I'd been in it for about a year and a half, I was talking to Hans Bethe about something. He was on the big Governing Council all this time, and I told him about this trick with my wife's nightgown and bedroom slippers. He started to laugh. "So *that's* how you got on the Town Council," he says.

It turned out that what happened was this. The woman who cleans the rooms in the dormitory opens this door, and all of a sudden there is trouble: Somebody is sleeping with one of the guys! Shaking, she doesn't know what to do. She reports to the chief charwoman, the chief charwoman reports to the lieutenant, the lieutenant reports to the major. It goes all the way up, through the generals to the Governing Board.

What are they going to do? What are they going to do? They're going to think about it, that's what! But, in the meantime, what instructions go down through the captains, down through the majors, through the lieutenants, through the chars' chief, through the charwoman? "Just put things back the way they are, clean 'em up, and see what happens." OK? Next day, same report. For four days, they worried up there about what they're going to do. Finally they promulgated a rule: No Women in the Men's Dormitory! And that caused such a *stink* down below that they had to elect somebody to represent the......

I would like to tell you something about the censorship that we had there. They decided to do something utterly illegal and censor the mail of people inside the United States — which they have no right to do. So it had to be set up very delicately as a voluntary thing. We would all volunteer not to seal the envelopes of the letters we sent out, and it would be all right for them to open letters coming in to us; that was voluntarily accepted by us. We would leave our letters open; and they would seal them if they were OK. If they weren't OK in their opinion, they would send the letter back to us with a note that there was a violation of such and such a paragraph of our "understanding."

So, very delicately amongst all these liberal-minded scientific guys, we finally got the censorship set up, with many rules. We were allowed to comment on the character of the administration if we wanted to, so we could write our senator and tell him we don't like the way things are run, and things like that. They said they would notify us if there were any difficulties.

So it was all set up, and here comes the first day for censorship: Telephone! *Briiing!*

Me: "What?"

"Please come down."

I come down.

"What's this?"

"It's a letter from my father."

"Well, what is it?"

There's lined paper, and there's these lines going out with dots — four dots under, one dot above, two dots under, one dot above, dot under dot...

"What's that?"

I said, "It's a code."

They said, "Yah, it's a code, but what does it say?"

I said, "I don't know what it says."

They said, "Well, what's the key to the code? How do you decipher it?"

I said, "Well, I don't know."

Then they said, "What's this?"

I said, "It's a letter from my wife — it says TJXYWZ TW1X3."

"What's that?"

I said, "Another code."

"What's the key to it?"

"I don't know."

They said, "You're receiving codes, and you don't know the key?"

I said, "Precisely. I have a game. I challenge them to send me a code that I can't decipher, see? So they're making up codes at the other end, and they're sending them in, and they're not going to tell me what the key is."

Now one of the rules of the censorship was that they aren't going to disturb anything that you would ordinarily do, in the mail. So they said, "Well, you're going to have to tell them please to send the key in with the code."

My wife's letter says, "It's very difficult writing because I feel that the ___ is looking over my shoulder."

I said, "I don't *want* to see the key!"

They said, "Well, all right, we'll take the key out."

So we had that arrangement. OK? All right. Next day I get a letter from my wife that says, "It's very difficult writing because I feel that the ______ is looking over my shoulder." And where the word was, there is a splotch made with ink eradicator.

So I went down to the bureau, and I said, "You're not supposed to touch the incoming mail if you don't like it. You can look at it, but you're not supposed to take anything out."

They said, "Don't be ridiculous. Do you think that's the way censors work — with ink eradicator? They cut things out with scissors."

I said OK. So I wrote a letter back to my wife and said, "Did you use ink eradicator in your letter?" She writes back, "No, I didn't use ink eradicator in my letter, it must have been the _____ "— and there's a hole cut out of the paper.

So I went back to the major who was supposed to be in charge of all this and complained. You know, this took a little time, but I felt I was sort of the representative to get the thing straightened out. The major tried to explain to me that these people who were the censors had been taught how to do it, but they didn't understand this new way that we had to be so delicate about.

So, anyway, he said, "What's the matter, don't you think I have good will?"

I said, "Yes, you have perfectly good will but I don't think you have *power*." Because, you see, he had already been on the job three or four days.

He said, "We'll see about *that*!" He grabs the telephone, and everything is straightened out. No more is the letter cut.

However, there were a number of other difficulties. For example, one day I got a letter from my wife and a note from the censor that said, "There was a code enclosed without the key, and so we removed it."

So when I went to see my wife in Albuquerque that day, she said, "Well, where's all the stuff?"

I said, "What stuff?"

She said, "Litharge, glycerine, hot dogs, laundry."

I said, "Wait a minute — that was a list?"

She said, "Yes."

"That was a *code*," I said. "They thought it was a code — litharge, glycerine, etc." (She wanted litharge and glycerine to make a cement to fix an onyx box.)

All this went on in the first few weeks before we got each other straightened out. Anyway, one day I'm piddling around with the computing machine, and I notice something very peculiar. If you take 1 divided by 273 you get .004115226337... It's quite cute, and then it goes a little cockeyed when you're carrying; confusion occurs for only about three numbers, and then you can see how the 10 10 13 is really equivalent to 114 again, or 115 again, and it keeps on going, and repeats itself nicely after a couple of cycles. I thought it was kind of amusing.

Well, I put that in the mail, and it comes back to me. It doesn't go through, and there's a little note: "Look at Paragraph 17B." I look at Paragraph 17B. It says, "Letters are to be written only in English, Russian, Spanish, Portuguese, Latin, German, and so forth. Permission to use any other language must be obtained in writing." And then it said, "No codes."

So I wrote back to the censor a little note included in my letter which said that I feel that of course this cannot be a code, because if you actually *do* divide 1 by 273 you do, in fact, *get* all that, and therefore there's no more information in the number .004115226337... than there is in the number 273 — which is hardly any information at all. And so forth. I therefore asked for permission to use Arabic numerals in my letters. So, I got that through all right.

There was always some kind of difficulty with the letters going back and forth. For example, my wife kept mentioning the fact that she felt uncomfortable writing with the feeling that the censor is looking over her shoulder. Now, as a rule, we aren't supposed to mention censorship. *We* aren't, but how can they tell *her*? So they keep sending me a note: "Your wife mentioned censorship." *Certainly* my wife mentioned censorship. So finally they sent me a note that said, "Please inform your wife not to mention censorship in her letters." So I start my letter: "I have been instructed to inform you not to mention censorship in your letters." *Phoom, Phoooom,* it comes right back! So I write, "I have been instructed to inform my wife not to mention censorship. How in the heck am I going to do it? Furthermore, *why* do I have to instruct her not to mention censorship? You keeping something from me?"

It is very interesting that the censor himself has to tell me to tell my wife not to tell me that she's....But they had an answer. They said, yes, that they are worried about mail being intercepted on the way from Albuquerque, and that someone might find out that there was censorship if they looked in the mail, and would she please act much more normal.

So I went down the next time to Albuquerque, and I talked to her and I said, "Now, look, let's not mention censorship." But we had had so much trouble that we at last worked out a code, something illegal. If I would put a dot at the end of my signature, it meant I had had trouble again, and she would move on to the next of the moves that she had concocted. She would sit there all day long, because she was ill, and she would think of things to do. The last thing she did was to send me an advertisement which she found perfectly legitimately. It said, "Send your boyfriend a letter on a jigsaw puzzle. We sell you the blank, you write the letter on it, take it all apart, put it in a little sack, and mail it." I received that one with a note saying, "We do not have time to play games. Please instruct your wife to confine herself to ordinary letters."

Well, we were ready with the one more dot, but they straightened out just in time and we didn't have to use it. The thing we had ready for the next one was that the letter would start, "I hope you remembered to open this letter carefully because I have included the Pepto Bismol powder for your stomach as we arranged." It would be a letter full of powder. In the office we expected they would open it quickly, the powder would go all over the floor, and they would get all upset because you are not supposed to upset anything. They'd have to gather up all this Pepto Bismol...But we didn't have to use that one. OK?

As a result of all these experiences with the censor, I knew exactly what could get through and what could not get through. Nobody else knew as well as I. And so I made a little money out of all of this by making bets.

One day I discovered that the workmen who lived further out and wanted to come in were too lazy to go around through the gate, and so they had cut themselves a hole in the fence. So I went out the gate, went over to the hole and came in, went out again, and so on, until the sergeant at the gate begins to wonder what's happening. How come this guy is always going out and never coming in? And, of course, his natural reaction was to call the lieutenant and try to put me in jail for doing this. I explained that there was a hole.

You see, I was always trying to straighten people out. And so I made a bet with somebody that I could tell about the hole in the fence in a letter, and mail it out. And sure enough, I did. And the way I did it was I said, "You should see the way they administer this place (that's what we were *allowed* to say). There's a hole in the fence 71 feet away from such and such a place, that's this size and that size, that you can walk through."

Now, what can they do? They can't say to me that there is no such hole? I mean, what are they going to do? It's their own hard luck that there's such a hole. They should *fix* the hole. So I got that one through.

I also got through a letter that told about how one of the boys who worked in one of my groups, John Kemeny, had been wakened up in the middle of the night and grilled with lights in front of him by some idiots in the Army there because they found out something about his father, who was supposed to be a communist or something. Kemeny is a famous man now.

Well, there were other things. Like the hole in the fence, I was always trying to point these things out in a non-direct manner. And one of the things I wanted to point out was this — that at the very beginning we had terribly important secrets; we'd worked out lots of stuff about bombs and uranium and how it worked, and so on; and all this stuff was in documents that were in wooden filing cabinets that had little, ordinary, common padlocks on them. Of course, there were various things made by the shop — like a rod that would go down and then a padlock to hold it, but it was always just a padlock. Furthermore, you could get the stuff out without even opening the padlock. You just tilt the cabinet over backwards. The bottom drawer has a little rod that's supposed to hold the papers together, and there's a long wide hole in the wood underneath. You can pull the papers out from below.

So I used to pick the locks all the time and point out that it was very easy to do. And every time we had a meeting of everybody together, I would get up and say that we have important secrets and we shouldn't keep them in such things; we need better locks. One day Teller got up at the meeting, and he said to me, "Well, I don't keep my most important secrets in my filing cabinet; I keep them in my desk drawer. Isn't that better?"

I said, "I don't know. I haven't seen your desk drawer."

Well, he was sitting near the front of the meeting, and I'm sitting further back. So the meeting continues, and I sneak out and go down to see his desk drawer. OK?

I don't even have to pick the lock on the desk drawer. It turns out that if you put your hand in the back, underneath, you can pull out the paper like those toilet paper dispensers. You pull out one, it pulls another, it pulls another...I emptied the whole damn drawer, put everything away to one side, and went back upstairs.

The meeting was just ending, and everybody was coming out, and I joined the crew and ran to catch up with Teller, and I said, "Oh, by the way, let me see your desk drawer."

The sergeant at the gate begins to wonder how come this guy is always going out and never coming in.

"Certainly," he said, and he showed me the desk.

I looked at it and said, "That looks pretty good to me. Let's see what you have in there."

"I'll be very glad to show it to you," he said, putting in the key and opening the drawer. "If," he said, "you hadn't already seen it yourself."

The trouble with playing a trick on a highly intelligent man like Mr. Teller is that the *time* it takes him to figure out from the moment that he sees there is something wrong till he understands exactly what happened is too damn small to give you any pleasure!

After I was able to open the filing cabinets by picking the locks, they got filing cabinets that had safe combinations. Now, one of my diseases, one of my things in life, is that anything that is secret I try to undo. And so the locks to those filing cabinets represented a challenge to me. How the hell to open them? So I worked and worked on them. There are all kinds of stories about how you can feel the numbers and listen to things and so on. That's true; I understand it very well — for oldfashioned safes. But these had a new design so that nothing would be pushing against the wheels while you were trying them, and none of the old methods would work.

I read books by locksmiths, which always say in the beginning how they opened the locks when the safe is under water and the woman in it is drowning or something, and the great locksmith opened the safe. And then in the back they tell you how they do it, and they don't tell you anything sensible. It doesn't sound like they could really open safes that way — like *guess* the combination on the basis of the psychology of the person who owns it! So I always figured they were keeping the method a secret, and like a kind of disease, I kept working on these things until I found out a few things.

First, I found out how big a range you need to open the combination, how close you have to be. And then I invented a system by which you could try all the necessary combinations — 8,000, as it turned out, because

Books are always telling how the great locksmith opens the safe under water when the woman in it is drowning.

you could be within two of every number. And then I worked out a scheme by which I could try numbers without altering a number that I had already set, by correctly moving the wheels, so that I could try all the combinations in eight hours. And then finally I discovered (this took me about two years of researching) that it's easy to take the last two numbers of the combination off the safe if the safe is open. If the drawer was pulled out, you could turn the number and see the bolt go up and play around and find out what number it comes back at, and stuff like that. With a little trickery, you can get the combination off.

So I used to practice it like a cardsharp practices cards, you know — all the time. Quicker and quicker and more and more unobtrusively I would come in and talk to some guy. I'd sort of lean against his filing cabinet, and you wouldn't even notice I'm doing anything. I'm not doing anything — just playing with the dial, that's all, just playing with the dial. But all the time I was taking the two numbers off! And then I would go back to my office and write the two numbers down, the last two numbers of the three. Now, if you have the last two numbers, it takes just a minute to try for the first number; there's only 20 possibilities, and it's open. OK? It takes about three minutes to open a safe if you know the last two numbers.

So I got an excellent reputation for safe-cracking. They would say to me, "Mr. Schmultz is out of town, and we need a document from his safe. Can you open it?"

I'd say, "Yes, I can open it, but I have to go get my tools."

I didn't need any tools, but I'd go to my office and look up the number of his safe. I had the last two numbers for everybody's safe in my office. I'd put a screwdriver in my back pocket to account for the tool I claimed I needed. I'd go back to the room and close the door. The attitude is that this business about how you open safes is not something that everybody should know because it makes everything very unsafe. So I'd close the door and then sit down and read a magazine or do something. I'd average about 20 minutes of doing nothing, and then I'd open it. Well, I really opened it right away to see that everything was all right, and then I'd sit there for 20 minutes to give myself a good reputation that it wasn't too easy, that there was no trick to it. And then I'd come out, sweating a bit, and say, "It's open. There you are."

Once, however, I did open a safe purely by accident, and that helped to reinforce my reputation. It was a sensation, but it was pure luck.

I went back to Los Alamos after the war was over to finish some papers, and there I did some safe opening that — well, I could write a safecracker book *better* than any previous safecracker book. It would start by explaining how I opened the safe — absolutely cold, without knowing the combination — which contained *more* secret things than any safe that's ever been opened. I opened the safe that contained the secret of the atomic bomb — *all* the secrets, the formulas, the rates at which neutrons are liberated from uranium, how much uranium you need to make a bomb, how much was being made and available, all the theories, all the calculations, the WHOLE DAMN THING!

This is the way it was done.

I was trying to write a report. I needed some material but it was a Saturday. I thought everybody worked. I thought it was like Los Alamos *used* to be. So I went down to get some documents from the library. The library at Los Alamos had all these documents in a great vault with a lock and dial of a kind I didn't know anything about. Filing cabinets I understood, but I was an expert only on filing cabinets. Not only that, but there were guards walking back and forth in front with guns. I couldn't get that vault open. OK?

But then I thought, wait! Old Freddy DeHoffman is in charge of deciding which documents now can be de-classified. He had to run down to the library and back so often, he got tired of it. And he got a brilliant idea. He would get a copy made of every document in the Los Alamos library. And he'd stick them in *his* files. He had *nine* filing cabinets, one right next to the other in two rooms, full of all the documents of Los Alamos.

I went up to his office. The office door was open. It looked like he was coming back any minute; the light was lit. So I waited. And, as always when I'm waiting, I diddled the knobs. I tried 10-20-30 — didn't work. I tried 20-40-60 — didn't work. I tried everything, because I'm waiting, with nothing to do.

Then I began to think. You know, I have never been able to figure out how to open safes cleverly, so maybe

I'd sort of lean against his filing cabinet, and you wouldn't even notice I'm doing anything.

those locksmith people don't either. Maybe all the stuff they tell me about psychology is right. I'm going to open this one by psychology.

The first thing the book says is: "The secretary is very often nervous that she will forget the combination." She's been told the combination, but she might forget, and the boss might forget. She has to know. So she nervously writes it somewhere. Where? List of places where a secretary might write combinations, OK? It starts right out with the most clever thing: You open the drawer, and on the wood along the outside of the drawer is written carelessly a number, as if it is an invoice number. That's the combination number. So. It's on the side of the desk, OK? I remembered that; it's in the book.

The desk drawer was locked, but I picked the lock easily. I pulled out the drawer, looked along the wood. Nothing. All right, all right. There were a lot of papers in the drawer. I fished around among the papers, and finally I found it, a nice little piece of paper which has the Greek alphabet — alpha, beta, gamma, delta, and so forth — carefully printed.

The secretaries have to know how to make those letters and what to call them when they're talking about them, right? So they each had a copy of the thing. But — carelessly scrawled across the top is, *pi is equal to 3.14159*. Well, why does she need the numerical value of pi? She's not computing anything. So I walked up to the safe. 31-41-59 — doesn't open. 13-14-95 — doesn't open. 95-14-13 — doesn't open. For 20 minutes I turned pi upside down. Nothing happened.

So I started walking out of the office, and I remembered in the book about the psychology, and I said, "You know, it's true. Psychologically, DeHoffman is *just* the kind of a guy to use a mathematical constant for his safe combination. And the other important mathematical constant is *e*." So I walk back to the safe. 27-18-28 — click, clock, it opens.

I checked, by the way, that all the rest of the filing cabinets had the same combination.

Well, I want to tell about some of the special problems I had at Los Alamos that were rather interesting. One thing had to do with the safety of the plant at Oak Ridge. Los Alamos was going to make the bomb, but at Oak Ridge they were trying to separate the isotopes of uranium — uranium 238 and uranium 235, the explosive one. They were *just* beginning to get infinitesimal amounts from an experimental thing of 235, and at the same time they were practicing the chemistry. There was going to be a big plant, they were going to have vats of the stuff, and then they were going to take the purified stuff and repurify and get it ready for the next stage. (You have to purify it in several stages.) So they were practising on the one hand, and they were just getting a little bit of U235 from one of the pieces of apparatus experimentally on the other hand. And they were trying to learn how to assay it, to determine how much uranium 235 there is in it — and though we would send them instructions, they never got it right.

So finally Segrè said that the only possible way to get it right was for him to go down there and see what they were doing. The Army people said, "No, it is our policy to keep all the information of Los Alamos at one place."

The people in Oak Ridge didn't know anything about what it was to be used for; they just knew what they were trying to do. I mean the higher people knew they were separating uranium, but they didn't know how powerful the bomb was, or exactly how it worked or anything. The people underneath didn't know at *all* what they were doing. And the Army wanted to keep it that way. There was no information going back and forth. But Segrè insisted they'd never get the assays right, and the whole thing would go up in smoke. So he finally went down to see what they were doing, and as he was walking through he saw them wheeling a tank carboy of water, green water — which is uranium nitrate solution.

He says, "Uh, you're going to handle it like that when it's purified too? Is that what you're going to do?"

They said, "Sure — why not?"

"Won't it explode?" he says.

Huh! *Explode?*

And so the Army said, "You see! We shouldn't have let any information get to them! Now they are all upset."

Well, it turned out that the Army had realized how much stuff we needed to make a bomb — 20 kilograms or whatever it was — and they realized that this much material, purified, would never be in the plant, so there was no danger. But they did *not* know that the neutrons were enormously more effective when they are slowed down in water. And so in water it takes less than a tenth — no, a hundredth — as much material to make a reaction that makes radioactivity. It kills people around and so on. So, it was *very* dangerous, and they had not paid any attention to the safety at all.

So a telegram goes from Oppenheimer to Segrè: "Go through the entire plant. Notice where all the concentrations are supposed to be, with the process as *they* designed it. We will calculate in the meantime how much material can come together before there's an explosion."

Two groups started working on it. Christie's group worked on water solutions and my group worked on dry powder in boxes. We calculated about how much material they could accumulate safely. And Christie was going to go down and tell them all at Oak Ridge what the situation was, because this whole thing is broken down and we *have* to go down and tell them now. So I happily gave all my numbers to Christie, and said, you have all the stuff, so go. Christie got pneumonia; I had to go.

I never traveled on an airplane before. I traveled on an airplane. They strapped the secrets in a little thing on my back! The airplane in those days was like a bus, except the stations were further apart. You stopped off every once in a while to wait.

There was a guy standing there next to me swinging a chain, saying something like, "It must be *terribly* difficult to fly without a priority on airplanes these days."

I couldn't resist. I said, "Well, I don't know. I *have* a priority."

A little bit later he tried again. "It looks like this. There are some generals coming. They are going to put off some of us number 3's."

"It's all right," I said, "I'm a number 2."

He probably wrote to his congressman — if he wasn't a congressman himself — saying, "What are they doing sending these little kids around with number 2 priorities in the middle of the war?"

At any rate, I arrived at Oak Ridge. The first thing I did was have them take me to the plant, and I said nothing, I just looked at everything. I found out that the situation was even worse than Segrè reported because he noticed certain boxes in big lots in a room, but he didn't notice a lot of boxes in another room on the other side of the same wall — and things like that. Now, if you have too much stuff together, it goes up, you see.

So I went through the entire plant. I have a very bad memory, but when I work intensively I have a good short-term memory, and so I could remember all kinds of crazy things like building 90-207, vat number so and so, and so forth.

I went home that night, and I went through the whole thing, explained where all the dangers were, and what you would have to do to fix this. It's rather easy. You put cadmium in solutions to absorb the neutrons in the water, and you separate the boxes so they are not too dense, according to certain rules.

The next day there was going to be a big meeting. I forgot to say that before I left Los Alamos Oppenheimer said to me, "Now, the following people are technically able down there at Oak Ridge: Mr. Julian Webb, Mr. so and so, and so on. I want you to make sure that these people are at the meeting, that you tell them how the thing can be made safe, so that they really *understand*."

I said, "What if they're not at the meeting? What am I supposed to do?"

He said, "Then you should say: *Los Alamos cannot accept the responsibility for the safety of the Oak Ridge plant* unless ________!"

I said, "You mean me, little Richard, is going to go in there and say —?"

He said, "Yes, little Richard, you go and do that."

I really grew up fast!

So when I arrived, sure enough, the big shots in the company and the technical people that I wanted were there, and the generals and everyone who was interested in this very serious problem. And that was good because the plant would have blown up if nobody had paid attention to this problem.

Well, there was a Lieutenant Zumwalt who took care of me, and he told me that the colonel said I shouldn't tell them how the neutrons work and all the details because we want to keep things separate, so just tell them what to do to keep it safe.

I said, "In my opinion it is impossible for them to obey a bunch of rules unless they understand how it works. So it's my opinion that it's only going to work if I tell them, and *Los Alamos cannot accept the responsibility for the safety of the Oak Ridge plant unless they are fully informed as to how it works!*"

It was great. The lieutenant takes me to the colonel and repeats my remark. The colonel says, "Just five minutes," and then he goes to the window and he stops and thinks. That's what they're very good at — making decisions. I thought it was very remarkable how a problem of whether or not information as to how the bomb works should be in the Oak Ridge plant or not had to be decided and *could* be decided in five minutes. So I have a great deal of respect for these military guys, because I never can decide anything very important in any length of time at all.

So in five minutes he said, "All right, Mr. Feynman, go ahead."

So I sat down and I told them all about neutrons, how they worked, da da, ta ta ta, there are too many neutrons together, you've got to keep the material apart, cadmium absorbs, and slow neutrons are more effective than fast neutrons, and yak yak — all of which was elementary stuff at Los Alamos, but they had never heard of any of it, so I turned out to be a tremendous genius to them.

I was a god coming down from the sky! Here were all these phenomena that were not understood and never heard of before — but I knew all about it; I could give them facts and numbers and everything else. So, from being rather primitive back there at Los Alamos, I became a super-genius at the other end.

The result was that they decided to set up little groups to make their own calculations to learn how to do it. They started to re-design plants, and the designers of the plants were there, the construction designers, and engineers, and chemical engineers for the new plant that was going to handle the separated material.

They told me to come back in a few months, so I came back when the engineers had finished the design of the plant. Now it was for me to look at the plant. OK?

How do you look at a plant that ain't built yet? I don't know. Well, Lieutenant Zumwalt, who was always coming around with me because I had to have an escort everywhere, takes me into this room where there are these two engineers and a *looooooong* table covered with a stack of large, long blueprints representing the various floors of the proposed plant.

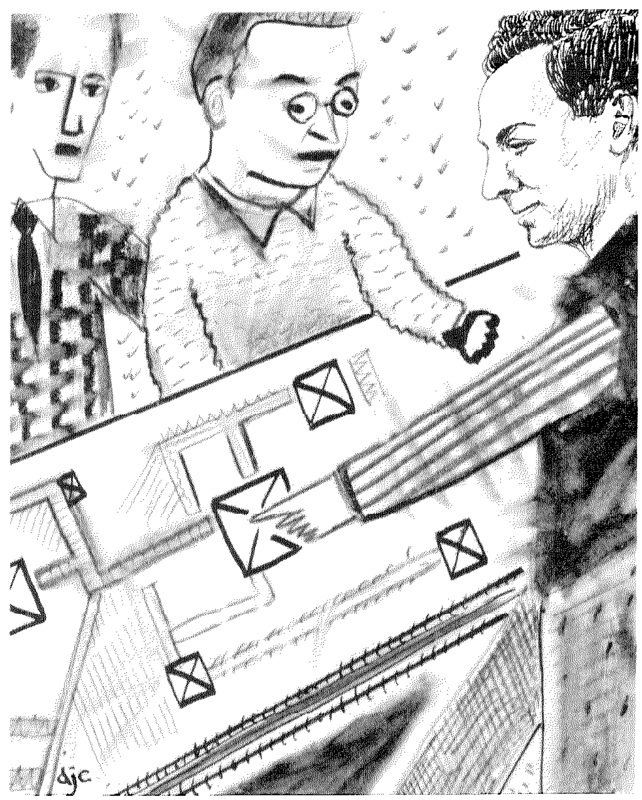
I took mechanical drawing when I was in school, but I am not good at reading blueprints. So they start to explain it to me, because they think I am a genius. Now, one of the things they had to avoid in the plant was accumulation. So they had problems like when there's an evaporator working, which is trying to accumulate the stuff, if the valve gets stuck or something like that and too much stuff accumulates, it'll explode. So they explained to me that this plant is designed so that if any one valve gets stuck nothing will happen. It needs at least two valves everywhere.

Then they explain how it works. The carbon tetrachloride comes in here, the uranium nitrate from here comes in here, it goes up and down, it goes up through the floor, comes up through the pipes, coming up from the second floor, *bluuuuurp* — going through the stack of blueprints, down-up-down-up, talking very fast, explaining the very, very complicated chemical plant.

I'm completely dazed. Worse, I don't know what the symbols on the blueprint mean! There is some kind of a thing that at first I think is a window. It's a square with a little cross in the middle, all over the damn place. I think it's a window, but no, it can't be a window, because it isn't always at the edge. I want to ask them what it is.

You must have been in a situation like this when you didn't ask them right away. Right away it would have been OK. But now they've been talking a little bit too long. You hesitated too long. If you ask them now they'll say, "What are you wasting my time all this time for?"

I don't know what to do. (You are not going to believe this story, but I swear it's absolutely true — it's such sensational luck.) I thought, what am I going to *do?* I got an idea. Maybe it's a valve? So, in order to find out whether it's a valve or not, I take my finger and I put it down on one of the mysterious little crosses in the middle of one of the blueprints on page number 3, and I say, "What happens if this valve gets stuck?" — figuring they're going to say, "That's not a valve, sir, that's a window."



I put my finger on one of the mysterious little crosses and say, "What happens if this valve gets stuck?"

So one looks at the other and says, "Well, if *that* valve gets stuck — " and he goes up and down on the blueprint, up and down, the other guy up and down, back and forth, back and forth, and they both look at each other and they *tchk, tchk, tchk,* and they turn around to me and they open their mouths like astonished fish and say, "You're absolutely right, sir."

So they rolled up the blueprints and away they went and we walked out. And Mr. Zumwalt, who had been following me all the way through, said, "You're a genius. I got the idea you were a genius when you went through the plant once and you could tell them about evaporator C-21 in building 90-207 the next morning," he says, "but what you have just done is so *fantastic* I want to know how, *how* do you do that?"

I told him you try to find out whether it's a valve or not.

Well, another kind of problem I worked on was this. We had to do lots of calculations, and we did them on Marchant calculating machines. By the way, just to give you an idea of what Los Alamos was like: We had these Marchant computers — hand calculators with numbers. You push them, and they multiply, divide, add and so on, but not easy like they do now. They were mechanical gadgets, failing often, and they had to be sent back to the factory to be repaired. Pretty soon you were running out of machines. So a few of us started to take the covers off. (We weren't supposed to. The rules read: "You take the covers off, we cannot be responsible...") So we took the covers off and we got a nice series of lessons on how to fix them, and we got better and better at it as we got more and more elaborate repairs. When we got something too complicated, we sent it back to the factory, but we'd do the easy ones and kept the things going. I ended up doing all the computers and there was a guy in the machine shop who took care of typewriters.

Anyway, we decided that the big problem — which was to figure out exactly what happened during the bomb's explosion, so you can figure out exactly how much energy was released and so on — required much more calculating than we were capable of. A rather clever fellow by the name of Stanley Frankel realized that it could possibly be done on IBM machines. The IBM company had machines for business purposes, adding machines called tabulators for listing sums, and a multiplier that you put cards in and it would take two numbers from a card and multiply them. There were also collators and sorters and so on.

So Frankel figured out a nice program. If we got enough of these machines in a room, we could take the cards and put them through a cycle. Everybody who does numerical calculations now knows exactly what I'm talking about, but this was kind of a new thing then — mass production with machines. We had done things like this on adding machines. Usually you go one step across, doing everything yourself. But this was different — where you go first to the adder, then to the multiplier, then to the adder, and so on. So Frankel designed this system and ordered the machines from the IBM company, because we realized it was a good way of solving our problems.

We needed a man to repair the machines, to keep them going and everything. And the Army was always going to send this fellow they had, but he was always delayed. Now, we *always* were in a hurry. *Everything* we did, we tried to do as quickly as possible. In this particular case, we worked out all the numerical steps that the machines were supposed to do — multiply this, and then do this, and subtract that. Then we worked out the program, but we didn't have any machine to test it on. So we set up this room with girls in it. Each one had a Marchant. But *she* was the multiplier, and *she* was the adder, and this one cubed, and we had index cards, and all she did was cube this number and send it to the next one.

We went through our cycle this way until we got all the bugs out. Well, it turned out that the speed at which we were able to do it was a hell of a lot faster than the other way, where every single person did all the steps. We got speed with this system that was the predicted speed for the IBM machine. The only difference is that the IBM machines didn't get tired and could work three shifts. But the girls got tired after a while.

Anyway, we got the bugs out during this process, and finally the machines arrived, but not the repairman. These were some of the most complicated machines of the technology of those days, big things that came partially disassembled, with lots of wires and blueprints of what to do. We went down and we put them together, Stan Frankel and I and another fellow, and we had our troubles. Most of the trouble was the big shots coming in all the time and saying, "You're going to break something!"

We put them together, and sometimes they would work, and sometimes they were put together wrong and they didn't work. Finally I was working on some multiplier and I saw a bent part inside, but I was afraid to straighten it because it might snap off — and they were always telling us we were going to bust something irreversibly. When the repairman finally got there, he fixed the machines we hadn't got ready, and everything was going. But he had trouble with the one that I had had trouble with. So after three days he was still working on that *one* last machine.

I went down, I said, "Oh, I noticed that was bent."

He said, "Oh, of course. That's all there is to it!" *Bend!* It was all right. So that was it.

Well, Mr. Frankel, who started this program, began to suffer from the computer disease that anybody who works with computers now knows about. It's a very serious disease and it interferes completely with the work. The trouble with computers is you *play* with them. They are so wonderful. You have these switches — if it's an even number you do this, if it's an odd number you do that — and pretty soon you can do more and more elaborate things if you are clever enough, on one machine.

And so after a while the whole system broke down. Frankel wasn't paying any attention; he wasn't supervising anybody. The system was going very, very slowly — while he was sitting in a room figuring out how to make one tabulator automatically print arc-tangent X, and then it would start and it would print columns and then *bitsi, bitsi, bitsi,* and calculate the arc-tangent automatically by integrating as it went along and make a whole table in one operation.

Absolutely useless. We *had* tables of arc-tangents. But if you've ever worked with computers, you understand the disease — the *delight* in being able to see how much you can do. But he got the disease for the first time, the poor fellow who invented the thing.

And so I was asked to stop working on the stuff I was doing in my group and go down and take over the IBM group, and I tried to avoid the disease. And, although they had done only three problems in nine months, I had a very good group.

The real trouble was that no one had ever told these fellows anything. The Army had selected them from all over the country for a thing called Special Engineer Detachment — clever boys from high school who had engineering ability. They sent them up to Los Alamos. They put them in barracks. And they would tell them *nothing*.

Then they came to work, and what they had to do was work on IBM machines — punching holes, numbers that they didn't understand. Nobody told them what it was. The thing was going very slowly. I said that the first thing there has to be is that these technical guys know what we're doing. Oppenheimer went and talked to the security and got special permission so I could give a nice lecture about what we were doing, and they were all excited: "We're fighting a war! We see what it is!" They knew what the numbers meant. If the pressure came out higher, that meant there was more energy released, and so on and so on. They knew what they were doing.

Complete transformation! *They* began to invent ways of doing it better. They improved the scheme. They worked at night. They didn't need supervising in the night; they didn't need anything. They understood everything; they invented several of the programs that we used — and so forth.

So my boys really came through, and all that had to be done was to tell them what it was, that's all. As a result, although it took them nine months to do three problems before, we did nine problems in *three* months, which is nearly ten times as fast.

But one of the secret ways we did our problems was this: The problems consisted of a bunch of cards that had to go through a cycle. First add, then multiply — and so it went through the cycle of machines in this room, slowly, as it went around and around. So we figured a way to put a different colored set of cards through a cycle too, but out of phase. We'd do two or three problems at a time.

But this got us into *another* problem. Near the end of the war for instance, just before we had to make a test in Albuquerque, the question was: How much would be released? We had been calculating the release from various designs, but we hadn't computed for the specific design that was ultimately used. So Bob Christie came down and said, "We would like the results for how this thing is going to work in one month" — or some very short time, like three weeks.

I said, "It's impossible."

He said, "Look, you're putting out nearly two problems a month. It takes only two weeks per problem, or three weeks per problem."

I said, "I know. It really takes much longer to do the problem, but we're doing them in *parallel*. As they go through, it takes a long time and there's no way to make it go around faster."

So he went out, and I began to think. Is there a way to make it go around faster? What if we did nothing else on the machine, so there was nothing else interfering? I put a challenge to the boys on the blackboard — CAN WE DO IT? They all start yelling, "Yes, we'll work double shifts, we'll work overtime," — all this kind of thing. "We'll *try* it. We'll *try* it!"

And so the rule was: All other problems *out*. Only one problem and just concentrate on this one. So they started to work.

My wife died in Albuquerque, and I had to go down. I borrowed Fuchs' car. He was a friend of mine in the dormitory. He had an automobile. He was using the automobile to take the secrets away, you know, down to Santa Fe. He was the spy. I didn't know that. I borrowed his car to go to Albuquerque. The damn thing got three flat tires on the way. I came back from there, and I went into the room, because I was supposed to be supervising everything, but I couldn't do it for three days.

It was in this *mess*. There's white cards, there's blue cards, there's yellow cards, and I start to say, "You're not supposed to do more than one problem — only one problem!" They said, "Get out, get out, get out. Wait — and we'll explain everything."

So I waited, and what happened was this. As the cards went through, sometimes the machine made a mistake, or they put a wrong number in. What we used to have to do when that happened was to go back and do it over again. But they noticed that a mistake made at some point in one cycle only affects the nearby numbers, the next cycle affects the nearby numbers, and so on. It works its way through the pack of cards. If you have 50 cards and you make a mistake at card number 39, it affects 37, 38, and 39. The next, card 36, 37, 38, 39, and 40. The next time it spreads like a disease.

So they found an error back a way, and they got an idea. They would only compute a small deck of 10 cards around the error. And because 10 cards could be put through the machine faster than the deck of 50 cards, they would go rapidly through with this other deck while they continued with the 50 cards with the disease spreading. But the other thing was computing faster, and they would seal it all up and correct it. OK? Very clever.

That was the way those guys worked, really hard, very clever, to get speed. There was no other way. If they had to stop to try to fix it, we'd have lost time. We couldn't have got it. That was what they were doing.

Of course, you know what happened while they were doing that. They found an error in the blue deck. And so they had a yellow deck with a little fewer cards; it was going around faster than the blue deck. Just when they are going crazy — because after they get this straightened out, they have to fix the white deck — the *boss* comes walking in.

"Leave us alone," they say. So I left them alone and everything came out. We solved the problem in time and that's the way it was.

I would like to tell a little about some of the people I met. I was an underling at the beginning. I became a group leader. But I met some very great men. It is one of the great experiences of my life to have met all these wonderful physicists.

There was, of course, Fermi. He came down once from Chicago, to consult a little bit, to help us if we had some problems. We had a meeting with him, and I had been doing some calculations and gotten some results. The calculations were so elaborate it was very difficult. Now, usually I was the expert at this; I could always tell you what the answer was going to look like, or when I got it I could explain why. But this thing was so complicated I couldn't explain *why* it was like that.

So I told Fermi I was doing this problem, and I started to describe the results. He said, "Wait, before you tell me the result, let me think. It's going to come out like this (he was right), and it's going to come out like this because of so and so. And there's a perfectly obvious explanation for this —"

He was doing what I was supposed to be good at, ten times better. So that was quite a lesson to me.

I got behind a truck windshield, because ultraviolet light can't go through glass, so I could see the thing.

Then there was Von Neumann, the great mathematician. We used to go for walks on Sunday. We'd walk in the canyons, and we'd often walk with Bethe, and Von Neumann, and Bacher. It was a great pleasure. And Von Neumann gave me an interesting idea; that you don't have to be responsible for the world that you're in. So I have developed a very powerful sense of social irresponsibility as a result of Von Neumann's advice. It's made me a very happy man ever since. But it was Von Neumann who put the seed in that grew into my *active* irresponsibility!

I also met Niels Bohr. His name was Nicholas Baker in those days, and he came to Los Alamos with Jim Baker, his son, whose name is really Aage Bohr. They came from Denmark, and they were *very* famous physicists, as you know. Even to the big shot guys, Bohr was a great god.

We were at a meeting once, the first time he came, and everybody wanted to *see* the great Bohr. So there were a lot of people there, and we were discussing the problems of the bomb. I was back in a corner somewhere. He came and went, and all I could see of him was from between people's heads, from the corner.

In the morning of the day he's due to come next time, I get a telephone call.

"Hello — Feynman?"

"Yes."

"This is Jim Baker." It's his son. "My father and I would like to speak to you."

"Me? I'm Feynman, I'm just a —."

"That's right. OK."

So, at 8 o'clock in the morning, before anybody's awake, I go down to the place. We go into an office in the technical area and he says, "We have been thinking how we could make the bomb more efficient and we think of the following idea."

I say, "No, it's not going to work. It's not efficient. Blah, blah, blah."

So he says, "How about so and so?"

I said, "That sounds a little bit better, but it's got this damn fool idea in it."

So forth, back and forth. I was always *dumb* about one thing. I never knew who I was talking to. I was always worried about the physics. If the idea looked lousy, I said it looked lousy. If it looked good, I said it looked good. Simple proposition.

I've always lived that way. It's nice, it's pleasant — if you can do it. I'm lucky. Just as I was lucky with that blueprint, I'm lucky in my life that I can do this.

So, this went on for about two hours, going back and forth over lots of ideas, back and forth, arguing. The great Niels kept lighting his pipe; it always went out. And he talked in a way that was un-understandable — mumble, mumble, hard to understand. His son I could understand better.

"Well," he says finally, lighting his pipe, "I guess we can call in the big shots *now*." So then they called all the other guys and had a discussion with them.

Then the son told me what happened. The last time he was there, he said to his son, "Remember the name of that little fellow in the back over there? He's the only guy who's not afraid of me, and will say when I've got a crazy idea. So *next* time when we want to discuss ideas, we're not going to be able to do it with these guys who say everything is yes, yes, Dr. Bohr. Get that guy and we'll talk with him first."

The next thing that happened, of course, was the test, after we'd made the calculations. I was actually at home on a short vacation at that time, after my wife died, and so I got a message that said, "The baby is expected on such and such a day."

I flew back, and I *just* arrived when the buses were leaving, so I went straight out to the site and we waited out there, 20 miles away. We had a radio, and they were supposed to tell us when the thing was going to go off and so forth, but the radio wouldn't work, so we never knew what was happening. But just a few minutes before it was supposed to go off the radio started to work, and they told us there was 20 seconds or some thing to go, for people who were far away like we were. Others were closer, 6 miles away.

They gave out dark glasses that you could watch it with. Dark glasses! Twenty miles away, you couldn't see a damn thing through dark glasses. So I figured the only thing that could really hurt your eyes — bright light can never hurt your eyes — is ultraviolet light. I got behind a truck windshield, because the ultraviolet can't go through glass, so that would be safe, and so I could *see* the damn thing. OK.

Time comes, and this *tremendous* flash out there is so bright that I duck, and I see this purple splotch on the floor of the truck. I said, "That ain't it. That's an after-image." So I look back up, and I see this white light changing into yellow and then into orange. The clouds form and then they disappear again; the compression and the expansion forms and makes clouds disappear. Then finally a big ball of orange, the center that was so bright, becomes a ball of orange that starts to rise and billow a little bit and get a little black around the edges, and then you see it's a big ball of smoke with flashes on the inside of the fire going out, the heat.

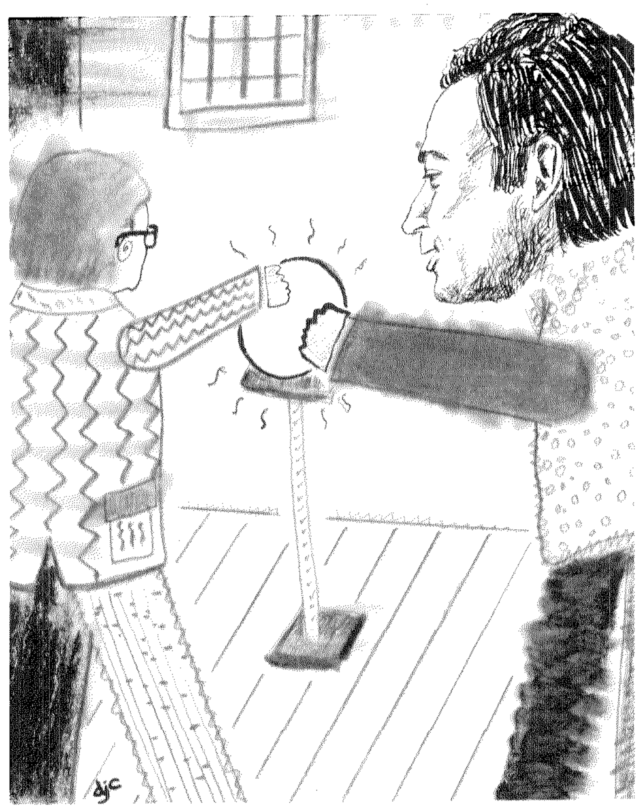
All this took about one minute. It was a series from bright to dark, and I had *seen* it. I am about the only guy who actually looked at the damn thing — the first Trinity test. Everybody else had dark glasses, and the people at six miles couldn't see it because they were all told to lie on the floor. I'm probably the only guy who saw it with the human eye.

Finally, after about a minute and a half, there's suddenly a tremendous noise — *BANG,* and then a rumble, like thunder — and that's what convinced me. Nobody had said a word during this whole thing. We were all just watching quietly. But this sound released everybody — released me particularly because the solidity of the sound at that distance meant that it had really worked.

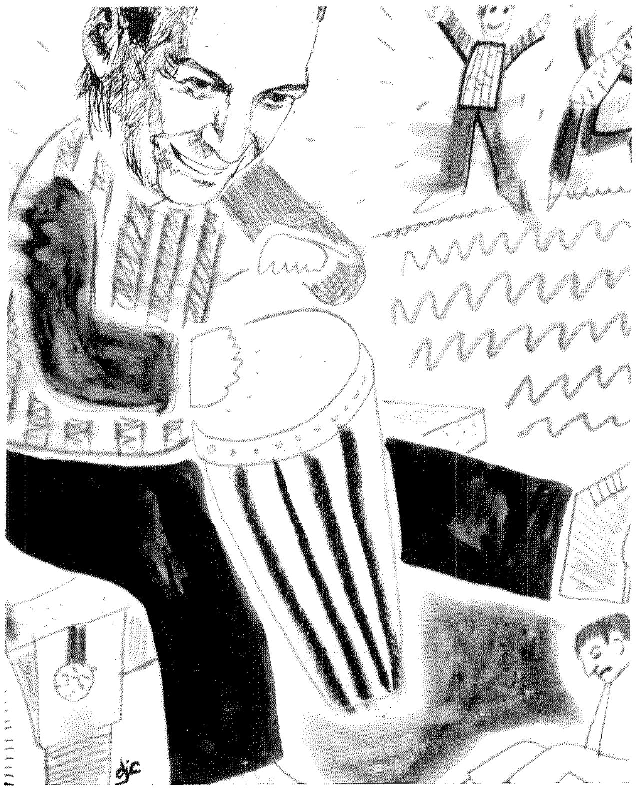
The man standing next to me said, "What's that?"

I said, "That was the bomb."

The man was William Laurence. He was there to write an article describing the whole situation. I had been the one who was supposed to have taken him around. Then it was found that it was too technical for him, and so later Mr. Smyth came and I showed him around. One thing we did, we went into a room and there on the end of a narrow pedestal was a small silver-plated ball. You could put your hand on it. It was warm. It was radioactive. It was plutonium. And we stood at the door of this room, talking about it. This was a new element that was made by man, that had never existed on the earth before, except for a very short period possibly at the very beginning. And here it was all isolated and radioactive and had these properties.



On a pedestal was a small silver-plated ball. It was warm. It was radioactive. It was plutonium.



There was tremendous excitement at Los Alamos. We all ran around. I sat on the end of a jeep and beat drums.

And we had made it. And so it was *tremendously* valuable.

Meanwhile, you know how people do when they talk — you kind of jiggle around and so forth. He's kicking the doorstop, you see, and I said, "Yes, the doorstop certainly is appropriate for this door." The doorstop was a hemisphere of yellowish metal — gold, as a matter of fact.

What had happened was that we needed to do an experiment to see how many neutrons were reflected by different materials in order to save the neutrons so we didn't use so much material. We had tested many different materials. We had tested platinum, we had tested zinc, we had tested brass, we had tested gold. So, in making the tests with the gold, we had these pieces of gold and somebody had the clever idea of using that great ball of gold for a doorstop for the door of the room that contained the plutonium.

After the thing went off, there was tremendous excitement at Los Alamos. Everybody had parties, we all ran around. I sat on the end of a jeep and beat drums and so on. But one man I remember, Bob Wilson, was just sitting there moping.

I said, "What are you moping about?"

He said, "It's a terrible thing that we made."

I said, "But you started it. You got us into it."

You see, what happened to me — what happened to the rest of us — is we *started* for a good reason, then you're working very hard to accomplish something and it's a pleasure, it's excitement. And you stop thinking, you know; you just *stop*. So Bob Wilson was the only one who was still thinking about it, at that moment.

I returned to civilization shortly after that and went to Cornell to teach, and my first impression was a very strange one. I can't understand it anymore, but I felt very strongly then. I sat in a restaurant in New York, for example, and I looked out at the buildings and I began to think, you know, about how much the radius of the Hiroshima bomb damage was and so forth...How far from here was 34th St?...All those buildings, all smashed — and so on. And I would go along and I would see people building a bridge, or they'd be making a new road, and I thought, they're *crazy,* they just don't understand, they don't *understand*. Why are they making new things? It's so useless.

But, fortunately, it's been useless for about 30 years now, isn't it? So I've been wrong for 30 years about it being useless making bridges and I'm glad that those other people had the sense to go ahead. □

"*During that same year we paid out:*

$148 million for materials to other companies
$83 million in wages, salaries, and benefits
$18 million for others' services
$8 million for replacement of equipment through depreciation
$4 million for interest on borrowed money
$14 million in reinvested profits
$3 million in dividends to our shareholders

"From the standpoint of society, the most important thing Avery Products did during the year was create $296,000,000 of value.

"The second most important thing it did was put the whole thing — one way or another — right back into the economy. Contrary to public opinion, nothing was secretly tapped off. . .

"Another interesting and gratifying thing about doing business in a free economy is that you know with certainty that your customers in every instance bought from you the best quality and service at the lowest prices available.

"If they could have bought better quality, or paid less, they would have done so. Unlike a controlled economy, where customers stand in line to buy whatever is offered, in a free economy the supplier stands in line to sell the best quality he can make, at the lowest price necessary to make the sale against competing needs.

"The system has evolved, in short, into an amazingly effective device for converting land, air, base metals, oil, falling water, uranium, and a myriad of other resources into useful things, along with the value, the money, by which those things are purchased."

With material like this at the heart of his public presentations, it's no surprise that Stanton Avery is often called to the lecture platform to state the case for capitalism. As a capitalist, he's a natural. Except for his humor (which is in fact a very big except) he might have been written by Horatio Alger. Amiable, forthright, witty, modest, unassuming, and soft-spoken, he is the antithesis of the cartoonists' capitalist — and all the more effective for it.

Avery Products was a one-man operation for many years. Then H. Russell Smith joined the company as vice president and general manager in 1946, became president in 1956, and now shares the chairmanship with Avery. As Avery has been able to release more of his business responsiblities, he has taken on more community service activities. Most of these are educational and cultural, and they are concentrated in the Los Angeles area. Caltech, of course, is now his major interest, but besides being chairman of the Board of Trustees at Caltech he is also a member of the Board of Trustees of the Huntington Library and Art Gallery and a member of the Board of Fellows of the Claremont University Center — where he served as chairman from 1965 to 1973. He is also a director of The Music Center Foundation, vice chairman of the Board of Governors of the Performing Arts Council of the Music Center, trustee of the Los Angeles County Museum of Art, a director of the Los Angeles World Affairs Council, and a member of the Los Angeles Committee of Foreign Relations.

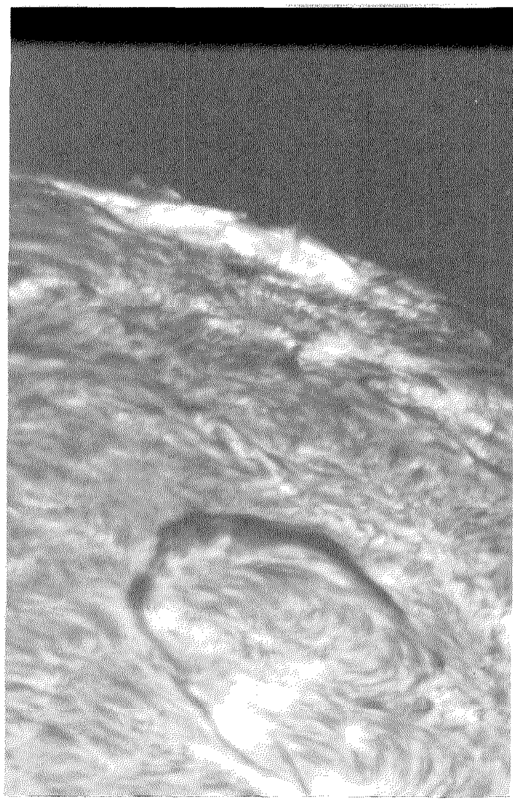
Ask him which of these activities he likes best, and he'll quote a psychiatrist friend whose children wanted to know which of them she loved best. Holding up her hand, she said, "Which of my fingers do I love the most?"

Capitalism is a system in which the baser instincts and drives of the individual are captured and used for the benefit of mankind

Avery lives in Pasadena with his second wife, Ernestine. The first Mrs. Avery died in 1965. There are three Avery children (Dennis, deputy city attorney in San Diego; Russell, an architect in San Diego; and Judith, married and the mother of four children), and two Avery stepsons, Larry and Stephen Onderdonk. And, to date, there are nine Avery grandchildren.

For a number of years Avery kept a boat at Newport Beach, but most of his relaxing now is done on an 8,000-acre working ranch near Paso Robles, where he raises grain and cattle, and does some hunting — for doves, quail, and wild pigs.

Stanton Avery has come a long way since he was just a young man who "found it easier to start a business with no capital at all than to get a job with no training at all." But great success hasn't changed him greatly — if at all. As a bona fide capitalist, he still holds by his minister's son's definition of capitalism: "Capitalism is a system in which the baser instincts and drives of the individual are captured and used for the benefit of mankind." □



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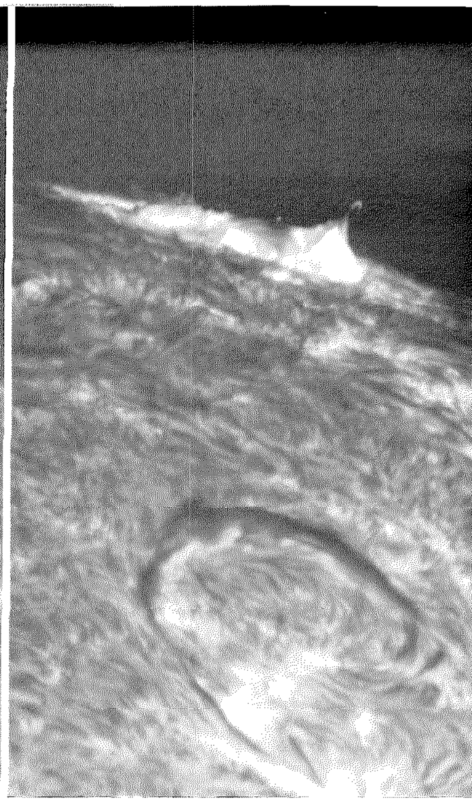

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SOLAR FLARE

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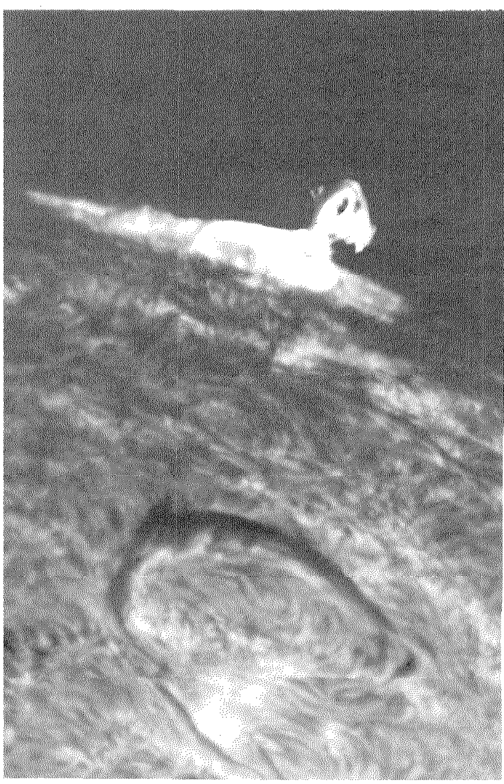

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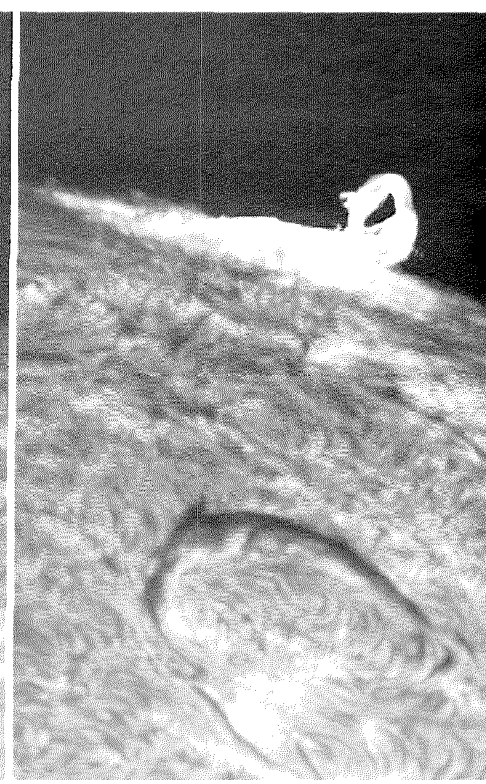

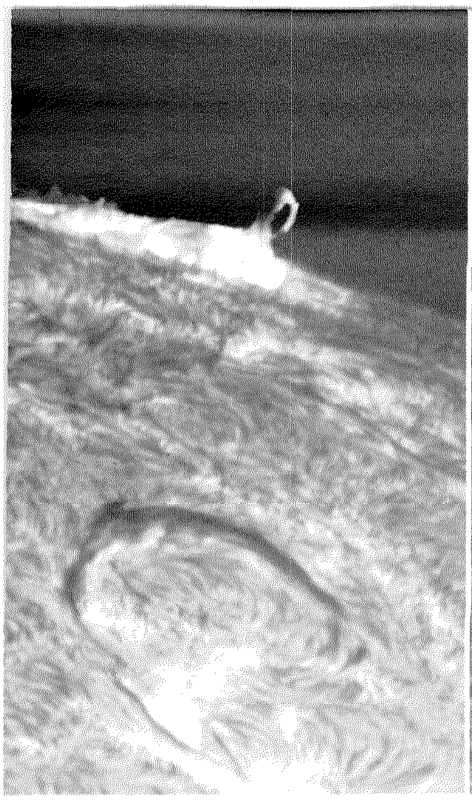

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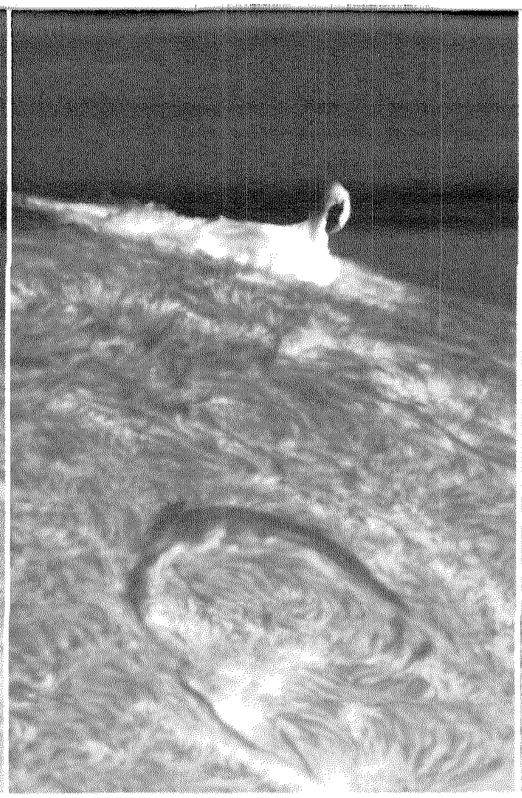

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These spectacular photographs, taken at Caltech's solar observatory on Big Bear Lake, record a particularly impressive solar flare on the surface of the sun on July 9, 1974. Although this is not a large flare, the fact that we see it at the edge of the sun gives us a better understanding of the role of magnetic fields. The first frame, at 22:44:40 universal time, shows the preflare state. In this photograph in hydrogen alpha light we know the bright area is a region of high magnetic field and enhanced activity. The flare begins as a bright blob at 23:34:24. As magnetic fields reconnect and the magnetic stresses are relieved, a twisted magnetic loop filled with hot plasma rises from the surface. As it rises further we see that there are many fine loops making up the large one. Finally the loops break up. The temperature in this flare, measured by soft x-rays, is about 20 million degrees. The hole inside the loop is about twice the size of the earth.

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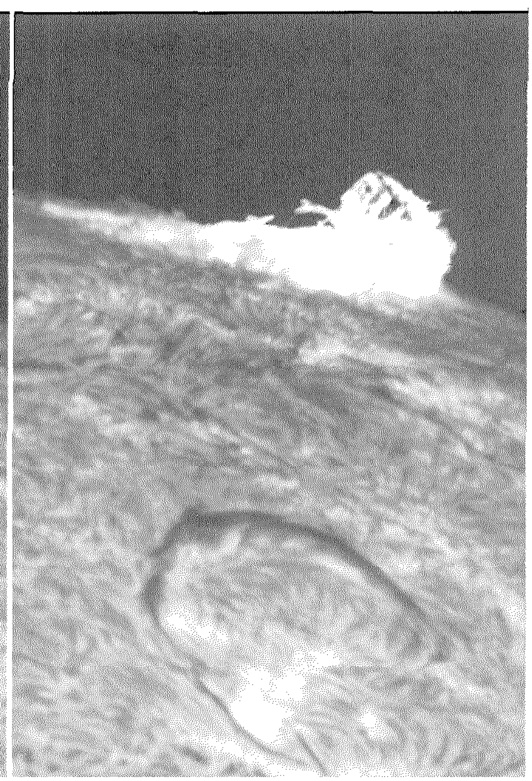


What physicist Glennys Farrar covets is more hours in the day

No Time For Dabbling

IF THERE'S one thing Glennys Reynolds Farrar hasn't time for, it's just dabbling around. Whatever she does, she does thoroughly — and she knows most of what there is to know about it before she's through. Coupled with a high degree of intelligence and no small amount of charm, this industrious tenacity has resulted in considerable accomplishment in her 30 years.

Vocationally, the object of Glennys's attention is theoretical physics, and she's one of Caltech's young and promising assistant professors in that field. ("She's a first-rate scientist," says a senior colleague, "full of original ideas; young, but learning fast.") But she also brings persistence and determination — and competence — to such diverse pursuits as cooking, hiking, cross-country skiing, and playing the piano. In fact, she recently demonstrated these qualities in buying a piano. She enlisted the aid of her piano teacher — Caltech's "pianist-in-residence," James Boyk — in helping her find the right instrument for her. He describes her approach to the project as "wanting to learn *all* about it, right now, in detail — and giving 100 percent of her attention to finding out. In no time she became a kind of lay expert in what to look for in a piano."

For Glennys, one bit of fallout from this kind of attitude has been the efficient abbreviation of some of the standard academic timetables. For example, her College Entrance Examination Board tests — taken for "practice" at the end of her junior year in high school — resulted in scores in the 800 neighborhood and a hurried decision to skip her senior year. UC Berkeley was happy to admit her after only three years of high school (a 40-student high school for children of American Army personnel in France).

Though she had gone only as far as trigonometry in math and had had no physics, Glennys, characteristically, signed up for advanced freshman physics. She knew she wanted to be a physicist, so anything elementary seemed a waste of time. This is a good example of what she calls her "compulsion to do the 'best' thing — which usually means the hardest — and do it well." (Today, when Caltech students ask her advice about whether to take track A or B physics, she finds it difficult not to recommend the advanced — and more difficult — track B for anyone who plans to go into the field.)

For a few weeks, advanced freshman physics at Berkeley almost defeated her. ("I'd think I understood the lectures, and then I wouldn't be able to do the problems.") A very obliging TA gave her a lot of help in the way of explanations and extra problems, and by the end of the first term she ranked second in the class. She still has the grade card on which the TA inscribed her "A" and his congratulations.

If finishing college took the regulation four years, the record also shows that she was one of the first physics undergraduates at Berkeley to take graduate courses. In her senior year she was allowed to enroll in two first-year graduate courses and a second-year one. She was also a TA, and she wound up graduating at the top of Berkeley's class of 1967.

Glennys had married Stanley Farrar, a first-year law student, at the end of her sophomore year, and the two finished their Berkeley stints at the same time. While Stan studied for the California bar that summer, she acted as a TA and studied Hindi, and in August the Farrars left for a year in India — he to do a research project, she to try to do a first-year graduate independent studies program. Though she took along books on electricity and magnetism, elementary particle theory, and field theory (all of which she studied faithfully), carried on an extensive question-and-answer correspondence with her adviser in Berkeley, and occasionally took the "long, uncertain bus ride out to Delhi University" to consult, Glennys doesn't feel she learned much physics. She did enjoy the total experience and appreciates what she learned about India.

Not learning much about physics, Farrar style, didn't result in serious delays in her academic progress. She entered Princeton as a graduate student in the fall of 1968, took the General Exam the following June, and turned in her dissertation in December of 1970.

After spending the spring term as a postdoctoral fellow, Glennys became a member of the Institute for Advanced Study. In the next two years she feels she did a "lot of good work, but it was all cooped up inside." While she felt the need to make a change, it was a somewhat complicated problem because she had to reconcile her own continuing desire to be at the "best" place, the available openings, and the fact that her husband's job with a prestigious Wall Street law firm was one he had no desire to leave.

When Caltech offered her a senior research fellowship, there was no doubt in Glennys's mind that she wanted to take it, but she did consider several other offers closer to New York. Finally, the Farrars agreed that Glennys would accept the Caltech position, even if it meant living apart for a year. Fortunately, it didn't come to that. Stan was offered an exciting job in a Los Angeles law firm, and the Farrars were able to move to Pasadena together.

Glennys's recollection that good ideas were bottled up inside her while she was at the Institute for Advanced Study may have some validity because a productive period began for her in the spring of 1973, which she spent at SLAC (Stanford Linear Accelerator Center). She and Stanley Brodsky of the SLAC staff recognized that if protons, pions, and other "hadrons" are indeed made of quarks, then when they are scattered off each other through some fixed angle, the scattering probability should follow some simple scaling laws; that is, it should have a definite dependence on the total energy of the collision. Since the particular energy dependence to be expected depends on the number of quarks in the particles being scattered, the prediction provides a test of whether the particles are actually made of quarks.

"These scaling laws are based on some very elegant and fundamental notions about the quark model," say Thomas Appelquist and Adam Schwimmer, who were visiting associates in theoretical physics at Caltech last year. Appelquist is from Harvard and Schwimmer is from the Weizmann Institute. "It was a very nice observation, which made it possible to account for some experiments that had been done, and to predict the outcome of some that hadn't yet been tried."

Most of the theoretical work Glennys does has immediate consequences for experimental results. ("She stays close to the real world," says Appelquist.) Since she has been at Caltech, much of her work has been devoted to formulating a consistent theory of the very small distance interactions between quarks, and determining their consequences for the behavior of ordinary particles. Sometimes she wonders whether this is the best approach to attack what she considers to be the outstanding problem of theoretical physics: Why are quarks confined inside protons, mesons, and the other known particles? There is a lot of evidence that they are there. Particles made out of them can be hit together so hard that many new particles (but never quarks) are created in the collision. Why don't the quarks ever break loose? Some fundamental force — not yet understood — must be keeping them confined.

"That problem may well be solved by someone who is working on it directly, rather than with my back-handed approach," Glennys says. "I tend to go about it by asking myself, 'If we assume that this or that is true, what consequences would it have? Would we still have a consistent picture? I try to get as much guidance as possible from physical rather than mathematical arguments."

For Glennys, the most important quality a theorist can have is good judgment about what problems to work on, and how to attack them. "What would be really fabulous," she says, "would be to have a Feynman or Gell-Mann kind of intuition about the right questions to ask — not just what is important, but what it may be possible to answer — as well as an ability to solve problems."

Needless to say, any theorist occasionally finds himself following a blind alley, and Glennys ruefully recalls one example: "Adam Schwimmer and I worked out a beautiful explanation for all the strange things that had been observed when an electron and a positron are annihilated with enough energy to produce the new particles discovered at SLAC in 1974. It agreed with everything that had been seen and, best of all, had a very definite consequence that could be easily tested. We called one of the SLAC experimentalists who was studying the process and asked him to look at the data and see if our 'prediction' was true. About a week later he called back: It wasn't."

But she's philosophical about it. "Of course the news was disappointing, but we still learned a lot from the thinking we'd done, and we enjoyed it. Besides, the fact that nature is not so easily explained is why it's such a challenge to try to understand it. That particular problem remains unsolved — but that gives you an even healthier respect for nature's ingenuity."

Glennys doesn't spend even all of her Caltech time in research, of course. She has graduate students, and she thoroughly enjoys working with them. Last year she began working with undergraduates as one of the team of physicists in charge of track B of freshman physics. She worked very hard at this assignment, but it wasn't until third term that she felt she'd hit her stride. By then she was less anxious about whether the students would feel she knew what she was doing, and she'd found her own informal and egalitarian style. Now she really relishes the give and take in her classes.

This year she is a member of the faculty committees on Institute programs and student housing — probably at least partly as a result of having volunteered several suggestions to members of these committees in the past. But she believes in lobbying for what she wants, and in giving her time to making it work. She also believes in — and practices, in spite of inner trepidation — asking questions when she doesn't understand. ("I may sound dumb, but that won't kill me.")

She attends national and international conferences when she can to hear reports of the work of her colleagues and to announce her own; and she visits various national research laboratories to work and observe. The fall term of this year, for example, was spent at CERN (Centre Européen pour la Recherche Nucleaire) in Geneva, Switzerland. This kind of experience is very important professionally as well as scientifically, so she is fortunate to have some of it financed by an Alfred Sloan Fellowship for Basic Research in the amount of nearly $20,000 to be spent over the next four years.

For recreation Glennys plays tennis (about once a week), gardens (most weekends), and (every chance she gets) goes hiking, backpacking, and (in season) cross-country skiing. Playing the piano is a fairly recent, and very important, activity. Though she had brief periods of music lessons on the piano and violin as a child, serious study of the piano is something she started less than three years ago. She is rapidly improving the level of her skill but faces the fact that the amount of time she has for music will tend to vary inversely with how intensely her research is going.

She loves to cook, and guests of the Farrars testify that the food — provided by both Glennys and Stan — is ambrosial. Glennys thinks she probably started acquiring her interests and skills as a result of her mother's turning partial responsibility for the cooking of family meals over to her when Glennys was about eight years old. This was Mrs. Reynolds's way of handling Glennys's complaint that she only got to help with the "grungy things like washing lettuce." The passage of time, incidentally, has not cured her of loathing that job.

Unfortunately, the combination of her schedule and her self-imposed demands for performance doesn't give Glennys as much time as she'd really like for anything. What she covets is more hours in the day. In fact, the only people she envies are those who don't need the eight hours of sleep a night that she requires. She once tried to train herself to do with less, but in that project determination, for once, failed her. A dismayed and exhausted Glennys Farrar found that those hard-earned extra hours were a total waste of time. □

Speaking Of . . .

A Science Adviser

I want to say a few words about the plan to "put science back in the White House" — as some refer to it — by reestablishing an Office of Science and Technology Policy in the Executive Offices. The Director of the Office will also be the Science and Technology Adviser to the President. Such a move has been in the mill since President Ford expressed an interest in it and recommended that Congress create legislation to carry it through. The legislative process on this matter is now nearing completion. The House bill (H. R. 10230) to establish such an office has been passed, the President has given it his endorsement, and the House bill is before the Senate.

To assist in formulating some of the issues that will be addressed by the proposed OSTP, the President has appointed two interim advisory groups on science and technology. One of these ad hoc task forces, under Dr. William O. Baker, President of Bell Laboratories, will focus on anticipated scientific advances that may affect national policies in the years ahead. And I'm pleased that Dr. John Baldeschwieler of Caltech will be serving on this group. The other task force — under the chairmanship of the man for whom this auditorium was named, Dr. Simon Ramo — will study the contributions of technology to our economic strength and explore ways that technology can improve our productivity, our environment, and the role of our Government in carrying out its international goals and responsibilities.

The major questions that are always raised when the position of Science Adviser is discussed are: What effect will a Science Adviser have in the White House? What influence might he have on the President's thinking? Will his advice be accepted and have any impact on national policy?

These are questions that remain to be answered. Much depends not only on the knowledge and convictions of the Science Adviser — and the way he is able to present his case — but on the receptiveness of the President and his top aides. In this regard I firmly believe that the time is ripe for the acceptance of effective counseling on national matters involving science and technology. The role they play and can play in almost every facet of our lives now makes them too important *not* to be considered most thoughtfully at the highest level of Government. I think we will see this process taking place in a growing way and with increasingly beneficial results for both science and the Nation.

If I am right, this will provide us with the resources and spirit needed to establish a new renaissance of research and development, one that will elicit exciting and challenging opportunities for all of us privileged to be a part of the science and engineering community — and one that could profoundly affect the future of our society and the course of human progress.

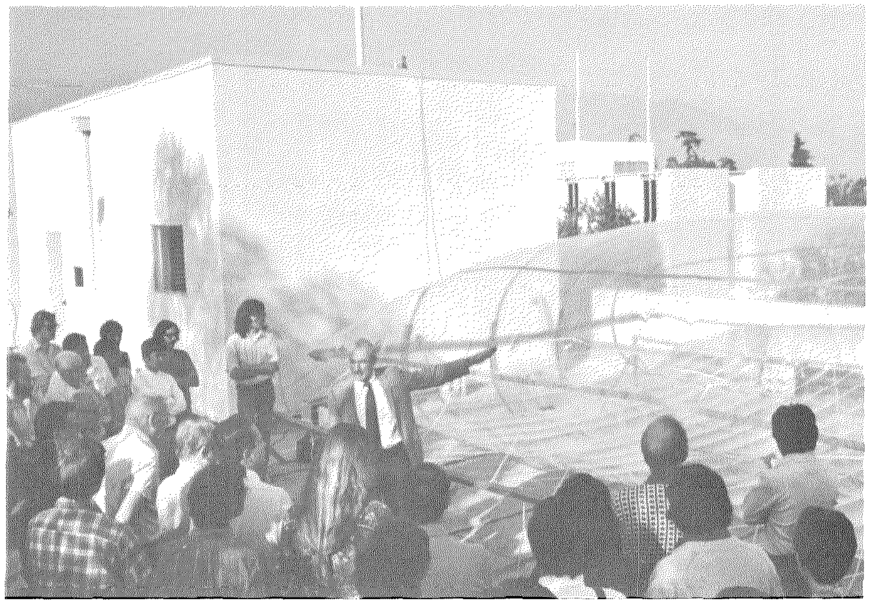
–H. Guyford Stever, director of the National Science Foundation and science adviser to the President, in a talk to the Caltech-JPL Management Club on November 25, 1975.

Air Pollution

Caltech has officially dedicated its new Air Quality Laboratory, made possible in part by a gift from the Pasadena Lung Association. The lab, on the roof of the Keck Engineering Laboratories, is now being used by Sheldon Friedlander, professor of chemical and environmental health engineering, and other Caltech engineers and scientists to study atmospheric pollutants in the Los Angeles Basin. At a conference held on campus last month on Strategies for Air

James Olds, Bing Professor of Behavioral Biology, describes the work in progress in his laboratory to Guy Stever, PhD '41, back on campus as one of the Caltech Y's Leaders in America.

Pollution Control in the South Coast Air Basin, Friedlander reported that the improvement in smog control achieved since the 1960s has leveled off in the last two or three years.



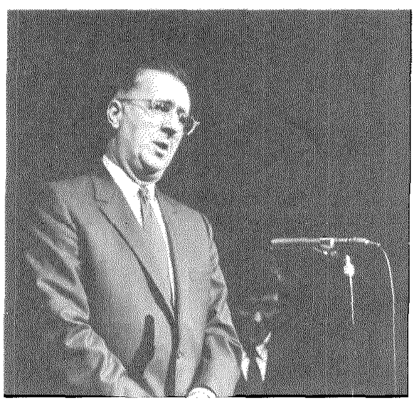
At the dedication of Caltech's new Air Quality Laboratory, Sheldon Friedlander shows off the lab's 2,000-cubic-foot Teflon balloon, used to capture samples of smog for analysis.

Extroverts

For more than twenty years, starting in 1954, "a sturdy band of extroverted egg-heads" functioning as the Caltech Stock Company performed in a series of musical extravaganzas honoring various campus characters. All the shows were written by Kent Clark, professor of English (words), and Elliott Davis, lawyer and business executive (music), and they celebrated such events as Linus Pauling's Nobel Prize ("The Road to Stockholm" — 1954), Lee DuBridge's retirement from Caltech ("Lee and Sympathy" — 1969), and Arnold Beckman's retirement as chairman of the board of trustees ("Beautiful Beckman" — 1975).

All told, Clark and Davis turned out a total of about 11 shows before the Stock Company went out of business — partly from exhaustion — in 1975. But some of their greatest moments have now been preserved on a phonograph record, "Let's Advance on Science," for sale at the Caltech Bookstore (Pasadena, 91125 — *Advt.*)

The record consists of 12 songs retrieved from tapes of the old Clark-Davis shows, delivered by such surprising performers as Ray Owen, professor of biology and dean of students (who is shown at the left below singing the love song "Loob Dub" to Fran Middlebrook), and William H. Corcoran, professor of chemical engineering and vice president for Institute relations (left, straying into the field of geology to sing "That's Not Gneiss"). Kent Clark, who is no mean performer himself, is the enthusiastic gentlemen leading the chorus line below.



Goulet?

Eddy and MacDonald?

A Chorus Line?

Books

THE OPIUM WAR, 1840-1842
by Peter Ward Fay
The University of North Carolina Press. . . $14.95

Reviewed by Charlton M. Lewis

Peter Fay's richly textured, evocative account of the Opium War revives a familiar question, asked by Commissioner Lin Tse-hsu in a letter to Queen Victoria in 1839 and still asked by Chinese today. Why did the British send naval artillery and troops to China in order to protect the import of a pernicious drug? For the Chinese there has been only one answer: incorrigible greed.

With precise and copious detail, Fay illuminates the historical complexities of that verdict. From the initial description of the brown gum drawn from the seed capsule of the opium poppy (*papaver somniferum*), with its smell of new-mown hay and look of tar, to the concluding scene where mandarins "in their cumbrous boots, long petticoats, and conical caps," crowd the quarter-deck of Pottinger's flagship to sign the Treaty of Nanking (1842), the story moves with lilt and style.

Fay confirms what has not always been clear in Western accounts: that as the cloistered world of the old Canton trade burst apart in the late 1830's, the root cause was the English import of opium. Thirsting for tea, but short of exchange commodities with appeal in China — sandalwood, bird's nests, ivory, rattans — English merchants resorted first to bullion and then, as the trade burgeoned, to opium.

Made contraband by imperial edicts, the drug was delivered by "agency houses" such as Jardine's, Dent's and Russell's to receiving ships well out in the Gulf of Canton. Chinese took delivery in their "scrambling dragons" and "fast crabs," and vanished into the Pearl River estuary. As the Canton delta became saturated, smuggling spread up the coast. After 1830 the bullion flow was reversed, arousing the concern of the Court: Silver was leaving China along with the tea.

Fay brings out the irresponsibility on both sides. For the English, the firms which bought the opium from the East India Company in India did not import it into China; the agency houses that imported it did not actually smuggle it. No one was to blame. On the Chinese side, officials who were schooled to foster harmony in their jurisdictions averted their gaze, or perhaps squeezed from the traffic themselves. By the time Commissioner Lin forced the confrontation in 1839, opium so dominated the market that the British would not consider commercial relations without it.

In interpreting the war, Fay is sensitive to cultural anachronisms within China. While the ironclad British gunboat, *Nemesis*, wreaked havoc on the inland waterways, Chinese officials marshalled troops trained by shooting whistling arrows at paper targets. The Confucian world did not prize military efficiency. Weak and disorganized, the Chinese successfully reverted to an impermeable moral righteousness. In the treaty negotiations, they refused to accept responsibility for the opium traffic, and the settlement (ultimate irony) said nothing about the drug.

Fay modestly disclaims qualifications as a China specialist. Yet this splendid popular history fills an urgent need that the specialists have too long neglected.

Charlton M. Lewis, a Sinologist, is associate professor in the Department of History at Brooklyn College, City University of New York.

ROMANTIC REVOLUTIONARY
A Biography of John Reed
by Robert A. Rosenstone
Alfred A. Knopf............... $15.00

Reviewed by Peter Ward Fay

Ten Days That Shook the World, John Reed's firsthand account of the Bolshevik Revolution of 1917, was written in two months, sold 5,000 copies in three, and has since been published in translation in Russia (of course), Spain, Mexico, Brazil, Italy, Poland, Cuba, France, and God knows where else. (Only He does, by the way, since there exists no international register of these things.) Rosenstone happening to be in Japan — he and his wife, the artist Cheri Pann, spent the past academic year there — he was told that the Japanese translation of 1957 has already run through 21 printings. It is enough to make even Lafcadio Hearn turn whatever color the Japanese equate with envy.

Many people, myself included, have supposed that *Ten Days* is the only thing John Reed wrote, and that his short career — he died in the fall of 1920 at the age of 32 — was one continuous preparation for the writing of it. Not the least of the merits of Rosenstone's biography is the attention he gives Reed the poet, Reed the producer and playwright, Reed the journalist, lover (extended affairs with Mabel Dodge and Louise Bryant punctuated with briefer liaisons), adventurer, and bohemian. Rosenstone is two-thirds done before we reach revolutionary Petrograd. If Reed's life pointed from the beginning in the direction of serious political involvement, Rosenstone does not show it and Reed did not know it.

What Reed *did* know even as a sickly child in Oregon was that life is something to be seized, experienced, lived furiously and heroically. At Harvard (Class of 1910) this made him pushy. In New York — Rosenstone is marvelous at recreating the social and intellectual life of Greenwich Village just before the war — a growing talent for writing led him to Max Eastman and the *Masses*. Covering the violent Paterson silk strike of early 1913 added four days in jail (the kind of raw encounter Reed loved) and generated the first installment of that radical conviction that eventually made him one of the founders of the Communist Labor Party — and thus drew him back to Russia, to the 2nd Congress of the Communist International and the typhus that killed him. But that came later. It was years before political commitment occupied the center of Reed's personal stage. The work, for example, that made him the most sought-after journalist of his day was his account of two months with Pancho Villa in Mexico. And what fascinated him about Villa was less the Mexican's politics than the larger-than-life quality of the man, the heroic in him.

Perhaps Reed never stopped trying to be a Villa. Here he is in Petrograd: "Seizing one of Lenin's dramatic phrases — 'History will not forgive us if we do not assume power now' — he enjoyed rolling it off his tongue as he roamed about the city, notebook in hand." Did he ever really pass from adolescent fantasy to adult reality? It is the theme of the biography, the reason for its title "romantic revolutionary," the question Rosenstone never fully resolves. One reviewer has already remarked that Rosenstone is very close to his subject, so close it is often hard to tell which of the two is speaking and in whose prose. A fair comment; if Rosenstone does not swallow Reed whole — he allows, for example, that *Ten Days* is "streaked with bias" — he nevertheless identifies with him, embraces him, speaks through him. But perhaps that is the key to the biography's success. Rosenstone literally relives Reed's life. And though I am even now not fully convinced of the depth of Reed's political radicalism, though he is so little my kind of person that I am not certain I even *like* him very much, there is no question that I am a great deal closer to an understanding of him and his age than I was before I read this thorough, continuously interesting, and sometimes quite strangely moving book.

Robert Rosenstone and Peter Fay are both professors of history at Caltech.

Letters

Guaranteed a Fegger

Potomac, Maryland

This afternoon I received my copy of *Engineering and Science* for October-November, and it could not have arrived at a better time. I had just finished teaching my Advanced Physical Science class and was contemplating the complexities of Markov Chains, a topic which my students are presenting for me, and thinking about how we would ease into an elementary example of a stochastic process, when a particularly tenacious student, once again started to question me about "Caltech." These scenarios generally start out with questions like, ". . . just what kind of a place is Caltech?" Today, I had some concrete evidence about your school.

I first pointed to the "FEG" formula. The student then asked, "Is that *the* Gray of that old battered chem text you have us read for the Advanced Placement Exam in Chemistry?" "The same," I said. The student then added ". . . and don't we have some books by Feynman in the school?" Again I concurred. We then talked about the "FEG" formula. Before the conversation ended, I turned to the back page of *Engineering and Science* and asked, "Are you so dedicated to science that you could ignore a 250-pound lion, in favor of a science lecture?" The student pondered this a moment and then said, "Caltech sounds like a fun place; I'm going to consider that school!"

Now, I don't know if this student will apply to Caltech or not, but I can guarantee that he will be a "fegger" — you can spot 'em while they're young — they're different. Whether or not you get this student, I want you to know that we enjoyed your publication. I might also add that science is alive and well in the secondary hinterland of this great nation.

CHARLES C. PHILIPP,
Chairman,
Science Department
Winston Churchill High School

Poetic Justice

Ricketts House, Caltech

Dr. Fowler's passage, "Resonance," (*E&S* October-November) is competently written, as prose. It is not poetry. A comparison of:

The realization
That the red giant stage
Of stellar evolution
Involved helium burning
Which transforms helium
Into carbon and oxygen
Was just as far-reaching
As the discovery
That the main sequence stage
Involves the conversion
Of hydrogen into helium.

with this passage by e. e. cummings:

What if a much of a which of a wind
gives the truth to summer's lie;
bloodies with dizzying leaves the sun
and yanks immortal stars awry?

will make that fact clear.
Turning good prose into bad poetry does Dr. Fowler a disservice.

NICK OKASINSKI, '78

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Before the smoke cleared, our chemists went to work...

It was the worst communications fire in U.S. history. The blaze, in one of the world's largest telephone switching centers, silenced more than 170,000 phones covering a 300-block area of New York City.

But while the fire was still burning out of control, the Bell System mobilized to restore service, and people from the New York and other Bell System companies, AT&T, Western Electric and Bell Labs jumped in to help. People like Barbara Reagor and Doug Sinclair.

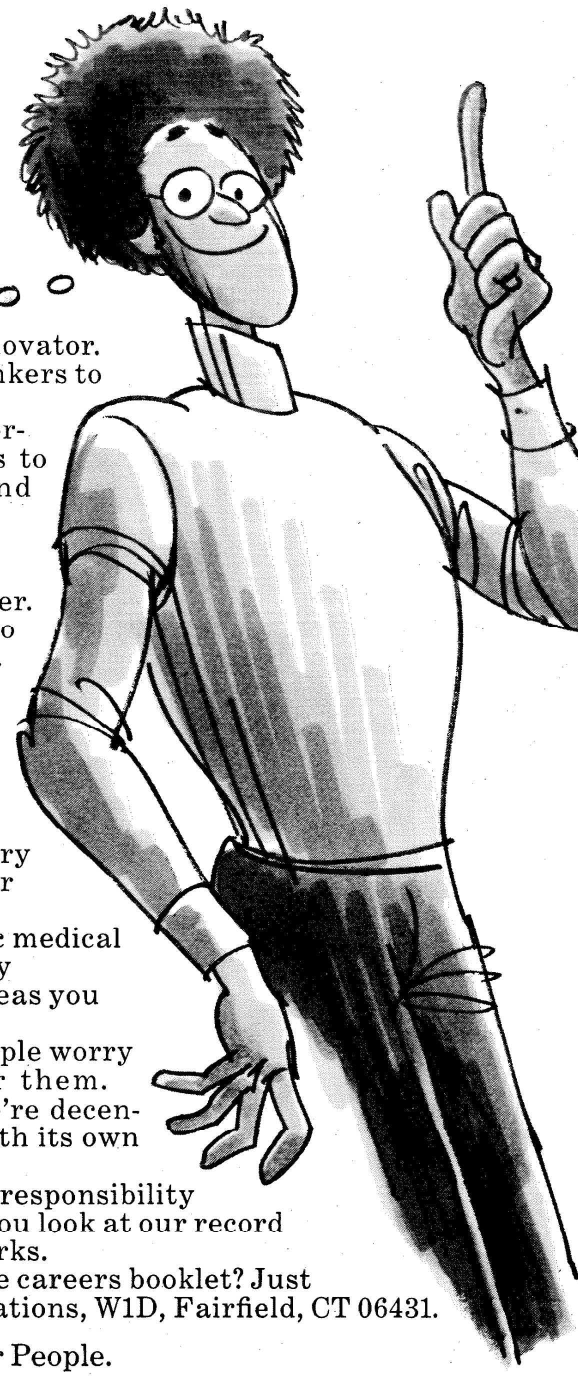
Barbara, who is working toward her master's in chemistry, and Doug, who received his Ph.D. in chemistry in 1972, are part of a team of Bell Labs materials researchers. They specialize in telephone equipment problems caused by contaminants in the environment, flooding and fires.

In this case the problem was caused by smoke–deposited on switching equipment contacts–that would interfere with the electrical continuity needed to make telephone connections.

In the laboratory, Barbara used a scanning electron microscope with an X-ray fluorescence detector to analyze samples of the smoke deposits. And at the fire site, Doug collected samples and tested methods of removing the smoke from the contacts. The answer: dissolving the deposits with trichloroethane. This procedure was used by craftspeople to clean the more than six million switching contacts in the building.

The fire is already history. Telephone service was restored in just over three weeks–a task that ordinarily would have taken over a year. It was an achievement made possible by the combined resources and teamwork of the Bell System–including people like Barbara Reagor and Doug Sinclair.

We're looking for engineers who think like Thomas Edison.



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