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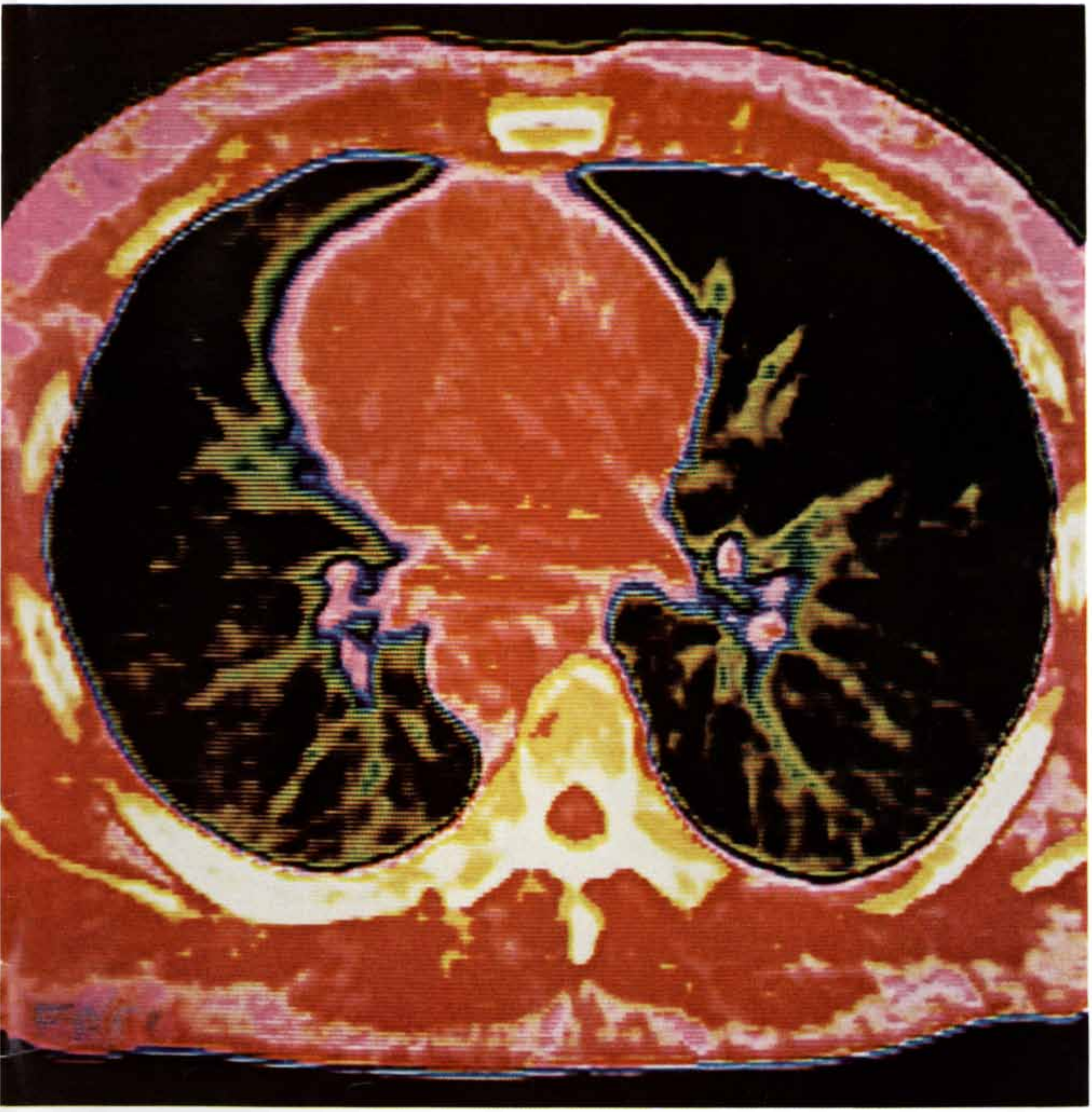

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Western Electric Reports:

Ion implantation with a new twist.

Western Electric produces millions of semiconductor components a year for use in Bell System telecommunications equipment. An essential step in the process is "doping"—introducing a precisely controlled impurity into the semiconductor material to alter its electrical characteristics. Until now, that's generally been done through diffusion techniques which entail masking a semiconductor wafer and "baking in" impurities—basically a broad brush process.

Bell Labs engineers felt that using ion beams to implant the dopants would be better than diffusion techniques. Ion implantation would improve the performance of some existing devices and would permit the design of new devices that require very precise control during manufacture.

But until recently, conventional ion implantation systems had serious drawbacks for heavy doping of wafers at fast production rates.

Electrical systems, in which a moving beam scans a stationary wafer, tolerate only relatively low currents before the beam starts to spread apart. And low currents mean low production rates.

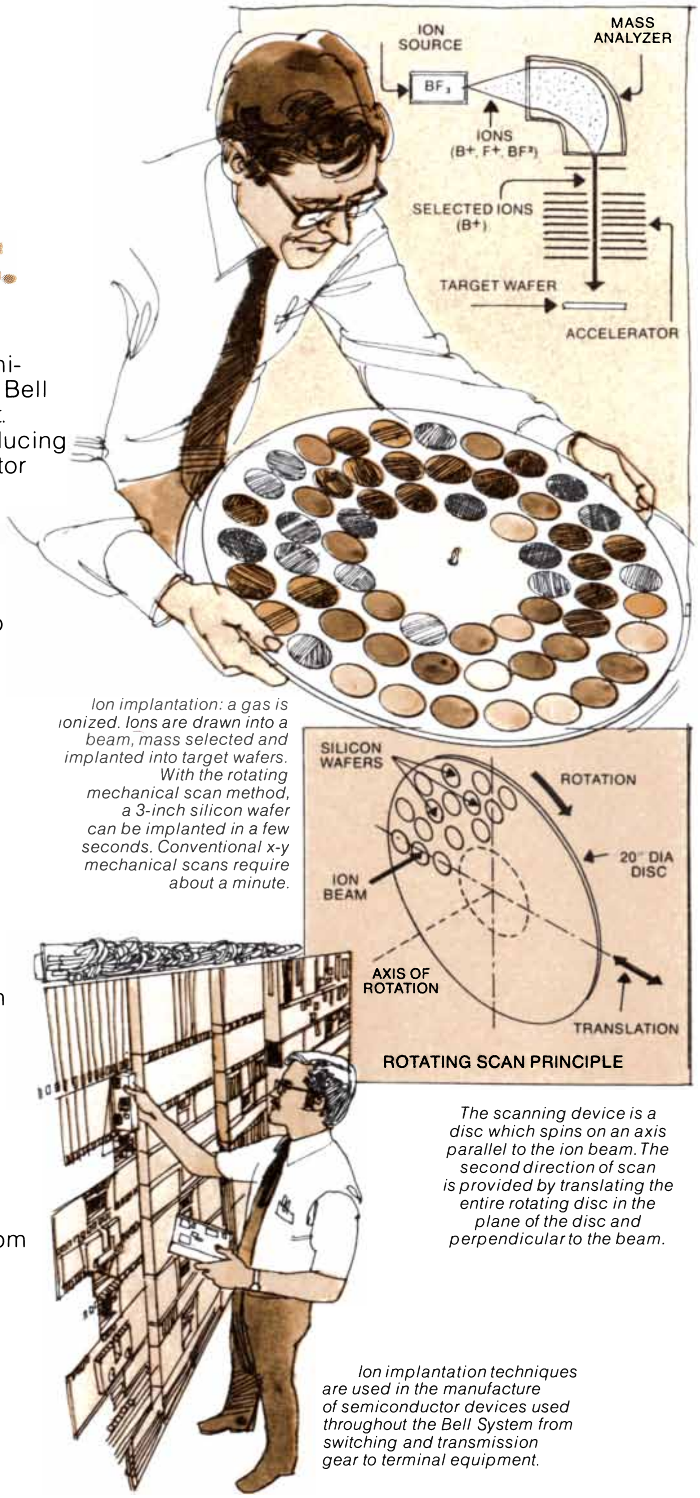
Mechanical (x-y) systems in which wafers are moved back and forth across a stationary beam, can use high currents but are unacceptably slow.

Now, engineers at Western Electric's Engineering Research Center in Princeton, N. J. have developed a new rotating mechanical scanner that overcomes the low speed limitation while still accommodating a high beam current.

The heart of the rotating scanner is a 20-inch aluminum disc which holds 60 silicon wafers in concentric rings. The disc rotates at about 1000 rpm as it moves from side to side through the path of a stationary ion beam. The beam traces a long overlap spiral on the disc.

To ensure uniform dosage, a digital control system adjusts the speed of the disc's side-to-side motion as the spiral moves toward the center of the disc.

Benefit: Rotating ion implantation techniques are one more manufacturing innovation that allows the Bell System to meet your communications needs reliably and economically.



Ion implantation: a gas is ionized. Ions are drawn into a beam, mass selected and implanted into target wafers. With the rotating mechanical scan method, a 3-inch silicon wafer can be implanted in a few seconds. Conventional x-y mechanical scans require about a minute.

The scanning device is a disc which spins on an axis parallel to the ion beam. The second direction of scan is provided by translating the entire rotating disc in the plane of the disc and perpendicular to the beam.

Ion implantation techniques are used in the manufacture of semiconductor devices used throughout the Bell System from switching and transmission gear to terminal equipment.

Western Electric

Established 1845 SCIENTIFIC AMERICAN October 1975 Volume 233 Number 4

ARTICLES

DEPARTMENTS

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THE COVER

The picture on the cover is a section through the chest of a living human subject made by the technique of reconstruction from projections (see "Image Reconstruction from Projections," page 56). In that technique a series of X-ray exposures made from different angles around the body are combined by computer to present a cross-sectional picture on the screen of a cathode-ray tube. In the picture on the cover the chest is seen as though it were viewed from above the subject's head. The dark spaces to the left and right are the lungs. The large red area in the middle is the heart. The white areas are bone; below the center is the spinal column, and around the lungs are sections through the ribs. In general the tomato red areas are muscular tissue and the lavender areas are fatty tissue. The branched areas in the lungs are blood vessels and bronchi. The picture was made (by the Delta Scanner built by Ohio-Nuclear, Inc.) in the course of a study that was conducted by Ralph J. Alfidi, M.D., of the Cleveland Clinic Foundation.

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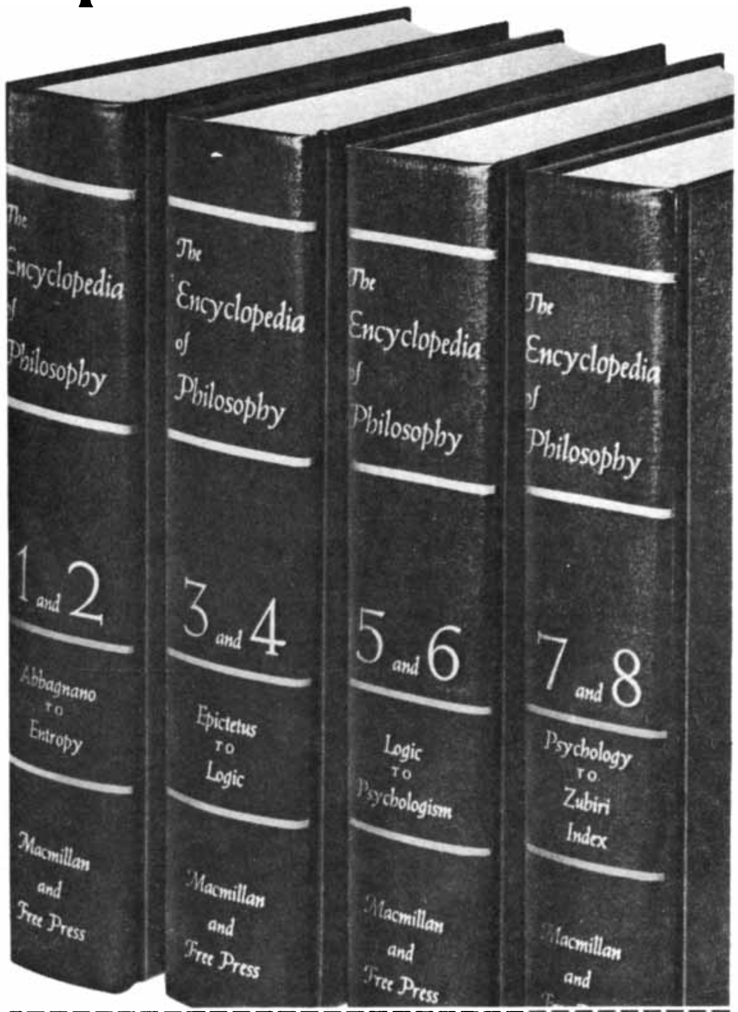

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LETTERS

Sirs:

Although I found your July article "The Accuracy of Strategic Missiles," by Kosta Tsipis, interesting, I must take exception to his conclusions. He asserts: "Nuclear weapons require a small error radius only if they are intended to be very accurate against hardened missile silos. Nuclear weapons intended for use against soft targets, such as industrial and urban complexes, airports, naval bases and transportation centers, do not exceed a small error radius to be effective." These statements are not necessarily true. There are a number of hard military targets other than missile silos, such as buried command posts, nuclear-weapons storage facilities and submarine pens that nuclear weapons may not be effective against unless they are accurate. Secondly, some urban industrial targets are also quite hard (cracking towers at oil refineries, blast furnaces at steel mills, hydroelectric plants, dams and transportation choke points such as tunnels) and require accurate delivery of nuclear weapons for a high-confidence kill. . . .

It should be noted that many urban industrial targets are surrounded by large civilian populations. If effectiveness simply means "destroy the target," then large-yield, moderately accurate weapons might do the job. However, if a more refined definition of effectiveness means "destroy the target with minimum collateral damage and loss of civilian lives," then low-yield nuclear (or non-nuclear) munitions with high delivery accuracy are required for effectiveness.

The conclusion that implementation of high accuracy will lead to "two possible reactions on the part of the Russians—launch on warning or resort to mobile land-based strategic missiles" is not necessarily true. There may be a third reaction, namely if the U.S. is increasing the flexibility of its forces so that small attacks against carefully selected targets with accurate low-yield weapons will be effective with minimum collateral damage, then the Russian response may be to estimate the numbers of attacking reentry vehicles and their potential targets (based on trajectories) before launching their ICBM's. This response would allow time for communication, negotiations and deescalation. It is not likely that the U.S. would launch all its ICBM's if only a few Soviet missiles were launched at areas other than missile sites. Perhaps the Soviets would respond in a similar manner.

The response of building mobile land-based strategic missiles is also not necessarily true. In fact, the Soviets might attempt to improve their force survivability through several different approaches, including building harder silos, mobility (sea-based, land-based or airborne) or proliferation of aim points (empty silos or deception). Defense is ruled out by treaty. However, improved force survivability has always been considered a stabilizing influence from a deterrence standpoint even if it is achieved by a land-based mobile force. Admittedly some of these options are harder to monitor to ensure that one side or the other is not cheating on number counts.

Finally, the suggestion that we should "freeze" the quality of our ICBM's is a very poor means for achieving stability. To paraphrase Murphy's law: "If it has been shown that a technology is feasible, eventually someone will demonstrate or use it." If the U.S. does not continue to develop its ICBM quality, the Soviets (or Chinese) may, and they could ultimately put the U.S. at a technological disadvantage. Even if both sides agreed to limit missile testing, it would not guarantee that improved accuracy will not be developed. Some improvement in accuracy will come as nonguidance errors are reduced as a result of better definition of the shape of the earth and reduction of environmental uncertainties, without missile hardware or software improvements. Even advanced guidance concepts can be fully developed on the ground in laboratories with simulations of missile environments, trajectories and target signatures. Aircraft and other subscale flight tests can give sufficient confidence in a system for only a few full-scale flight tests to be needed for verification. After all, we went to the moon with only a limited number of flight tests.

In summary, I feel the U.S. should continue to develop missile accuracy through aggressive research programs. We should not treat accuracy (the ability to hit an intended target) any differently from how we treat reliability (the ability to function as intended). Congress can control the implementation of improved accuracy in our missile forces on the basis of our needs to improve force flexibility or to respond to Soviet developments. The fact that we know how to achieve high ICBM accuracy is not in itself destabilizing. On the contrary, its proper implementation and deployment can enhance our deterrent posture by increasing policy options through greater force flexibility.

WILLIAM C. YENGST

Science Applications, Inc.
La Jolla, Calif.

Sirs:

Mr. Yengst's letter closely echoes the arguments of Secretary Schlesinger in support of the strategy of counterforce. This strategy, first advocated in public by President Nixon and now supported mainly by members of the military-industrial complex and the defense "think tanks," is contrary to the policy of deterrence since it envisions preemptive attacks against the missile silos of an adversary. It is therefore outside the mainstream of established U.S. foreign-policy thinking. It has been interpreted by arms-limitation advocates as a strategy devised in order to find uses for the thousands of nuclear warheads accumulated in the U.S. arsenal and lacking credible targets in the context of deterrence, or as a promotional activity needed to persuade Congress to find new industry-promoted programs for improving the accuracy of U.S. ICBM's. Since

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STATE ____________________ ZIP __________ SA-3 such improvements in accuracy are unjustifiable in the context of the existing strategy of nuclear deterrence, counterforce was invented to provide the rationale for them. I do not have space here to examine the arguments of these two interpretations. I would only emphasize that counterforce is by no means an accepted strategy, and therefore it is injudicious to argue, as Mr. Yengst does, by taking its validity for granted. Rather I should like to focus on the reasons that have persuaded me that restraint in deployment of new strategic weapons is at this time a preferable course for this country than the precipitate deployment of a new generation of very accurate strategic weapons.

The declared objective of our nuclear policy is to deter any adversary from coercing or attacking the U.S. To prevent the adversary from acquiring new strategic weapons that could enhance his ability to coerce or attack us successfully would greatly contribute to this objective. Our past practice of preemptively deploying the latest strategic weapons our technology affords us has neither forced nor persuaded the U.S.S.R. to stop deploying strategic weapons increasingly threatening to our security. Quite on the contrary, our deployments create in the U.S.S.R. political and military pressures that have forced it, in spite of its economic difficulties, to follow our example in the strategic-arms race.

On the other hand, if we exercise restraint and use our indisputable, and much feared by the Russians, technological superiority to negotiate a mutual ban on deployment of new strategic weapons, we shall achieve, without expense, what hundreds of billions of dollars' worth of deployed strategic systems have failed to do: prevent the Russians from deploying any more nuclear weapons against us. Verification of the faithful implementation of such a ban is quite possible: no military officer will ever use an untested weapon, particularly a strategic nuclear weapon. Many tests of the actual weapon are needed to develop it, to train personnel to use it and to gain confidence in its reliability. Hence it is not only impossible to deploy a new system undetected but also impossible even to develop one clandestinely.

The disadvantages of deploying a new technology without first attempting to negotiate its mutual ban is vividly illustrated by the case of MIRV. Our failure to propose such a negotiated ban on MIRV in 1969, before we started the deployment of multiple warheads and at a time the Russians were receptive to the idea of a ban, has had counterproductive results: we acquired the means to deliver several thousand supernumerary warheads that do not add to the credibility of our deterrence and we are faced with the certain prospect of 1,320 Russian MIRVed missiles aimed against the U.S. A deterioration of our national security could have been avoided by using our technological superiority correctly. Mr. Yengst is of course right in his concern about the proliferation of nuclear weapons and strategic delivery systems to nations other than the U.S.S.R. Indeed, the problem of proliferation is at least as ominous as an untrammeled deployment of strategic weapons by the U.S.S.R. To deploy a new generation of very accurate ICBM's and endow it with the rationale embodied in the strategy of counterforce, however, is to encourage, by deed and word, other countries to acquire nuclear weapons, that is, to exacerbate the very problem Mr. Yengst worries about.

Restraint in deployment accompanied by continuing research can convince others of the futility of attempting to derive political utility from nuclear weapons. On the other hand, pursuit of the strategy of counterforce, in spite of its lack of realism, may frighten other nations into believing that it is essential for their security to deploy a nuclear deterrent against the U.S. Before we deploy a new strategic weapon we must ask not only what it can do for our national security but also what it may induce an opponent to do in response, and whether or not what he may do is to our ultimate advantage.

KOSTA TSIPIS

Massachusetts Institute of Technology
Cambridge

ERRATUM

In the department "The Illustrations" for September the photograph on the cover and the photographs in the article "Jupiter" were credited to the Jet Propulsion Laboratory of the California Institute of Technology. All these photographs should have been credited to the University of Arizona and the Ames Research Center of the National Aeronautics and Space Administration.

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Now, there's a great new space-age product called the CheckMaster designed specifically to keep you in perfect balance, for every check you write, every day of the year. And it's actually easier to use than a calculator.

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Open your checkbook holder and turn on the built-in computer. Press the "Balance" key, and your bank balance is recalled on the display. The CheckMaster memory is so powerful that it never forgets your balance—even months after you last recall it.

Enter the amount of your check, and press the "Check" key. The check amount is automatically deducted from your balance, and your new balance is displayed—and all with just one key stroke.

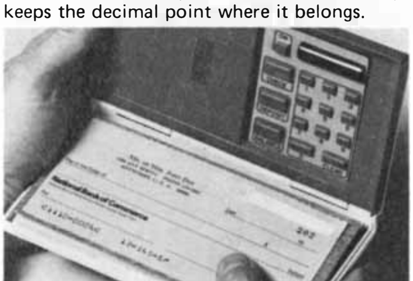
Or enter the amount of a deposit, and press the "Deposit" key. Your deposit is automatically added to your balance, and again, your new balance is displayed.

MANY EXTRA FEATURES

The CheckMaster does so much, so easily, and it's great fun to use. Here are seven reasons why:

1. **Easily corrects mistakes** If you enter the wrong digits, press the "Clear" key. Only your mistake will be cleared—never the balance.

2. **Worry-free decimal** Just enter the digits. The unit's dollar-position decimal always keeps the decimal point where it belongs.



3. **Low battery signal** The unit's penlight batteries will last one year with average use. A low battery signal on the display will indicate when it's time to replace them.

4. **Overdraft alert** CheckMaster will signal an overdrawn account plus show the overdraft amount and help you avoid the embarrassment of having a check accidentally bounce.

5. **Safety switch** If you forget to turn off your computer, don't worry. Whenever you close your case, your unit shuts off automatically.

6. **Private viewing angle** Don't worry about anyone seeing your balance. The red display can only be viewed by the user and registers up to $9,999.99 or any six digits.

7. **Perfect size** The CheckMaster's handsome tan and cream-colored case measures 7/8" x 3 5/8" x 6 3/4" and weighs only 8 ounces.

To find out your exact balance, even months after you've last recalled it, simply open your case and press the balance key. CheckMaster's memory never forgets. Designed for both men and women, CheckMaster holds any standard-sized personal checks, check register, credit cards and important papers.

RUGGED AND SHOCKPROOF

Drop it, sit on it, drop it again—CheckMaster's shock-proof case will withstand plenty of abuse. The integrated circuit is hermetically sealed for a lifetime of trouble-free service. A spot of gold is even used in the circuit's final sealing process to insure CheckMaster's very high level of reliability.

CONTROL YOUR PURCHASES

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The CheckMaster is perfect insurance against bounced checks, overdrafts, arithmetic errors and bank errors. Its powerful memory, simple operation, and many extra features make it another example of how space-age technology has made fun out of one of the most time-consuming household tasks.

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The CheckMaster will make such an improvement in helping you balance your checkbook that we make the following unusual money-back guarantee: use the CheckMaster until your next bank statement arrives or for one full month. If the CheckMaster does not balance your checkbook perfectly and actually pay for itself in either convenience or actual savings, return it for a prompt and courteous refund. You can't lose.

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50 AND 100 YEARS AGO

SCIENTIFIC AMERICAN

OCTOBER, 1925: "As long ago as the 17th century Newton defended the view that light consists of streams of little particles. At the dawn of the 19th century, however, experiments were performed which were thought to give positive evidence that light consists of waves. Maxwell interpreted them as electromagnetic waves, and in such terms we have ever since been explaining light rays, X rays and radio rays. Very recently, however, a group of electrical effects of light have been discovered for which the idea of light waves suggests no explanation but whose interpretation is obvious according to a modified form of Newton's corpuscular theory. When light or X rays fall on certain metals, such as sodium, the metals give off electrons. It was to account for this 'photoelectric effect' that Einstein, about 20 years ago, suggested that light and X rays do not consist of waves but of concentrated bundles of energy that he calls quanta. Strong support for the quantum theory of radiation has recently come from work with the scattering of X rays by electrons. We should expect, if X rays are waves, that the scattered waves should have the same wavelength as the original ones. Spectra, however, prove that a part of the scattered X rays is of distinctly greater wavelength than the primary rays. The quantum theory gives a direct explanation if we think of the X ray as a little particle that is deflected when it collides with an electron. If the electron is loose, it will recoil, so that part of the quantum's energy is spent in setting the electron in motion. Moreover, the recoiling electrons have now been observed. The manner in which we are to reconcile these new ideas with the long established facts of the interference and refraction of light is very obscure. Most physicists look forward to a solution in some combination of the wave and quantum theories."

"Britain's new battleships, *Nelson* and *Rodney*, now approaching the launching stage, will incorporate all the modifications suggested by war experience, and especially by the Battle of Jutland. Their main armament consists of nine 16-inch guns, mounted in triple turrets. Contrary to all precedent the guns are grouped together in the forward part of the ship, leaving the after section denuded of heavy armament. Stern fire has been sacrificed in deference to an important lesson learned at Jutland: that the really vulnerable points of a capital ship are its turrets, barbette trunks and contiguous magazines. At least two of the three British capital ships lost in that engagement were sunk by shell that pierced the turret walls or roof and transmitted a flash to the powder room. The grouping of the turrets on the forward deck makes possible the concentration of very thick armor over this area of the ship."

"When the relation between the real brightness of a star, or the amount of heat radiated by it, and its surface temperature is plotted on vertical and horizontal coordinates, we find that the points are very far from being distributed at random. The majority congregate in the neighborhood of an S-shaped line, spreading somewhat on each side but showing a very strong gregarious tendency. The points represent a sequence of stars, beginning with bright, hot stars and passing through fainter and cooler ones and finally to the faint red dwarf stars. This set of stars is called by Professor Eddington the 'main sequence.' There is nothing in the theories of Eddington and Jeans to account for this concentration. To explain it we must recall that the source of stellar energy appears to involve the gradual transformation of matter into energy, according to the principle of relativity. Does this transformation proceed always at the same rate (like radioactivity), or does it go on faster and faster as the temperature rises? Jeans maintains the first opinion, Eddington the second. Arguments based on the existence of a main sequence favor the second view."

SCIENTIFIC AMERICAN

OCTOBER, 1875: "Mr. Lick has fixed on Mount Hamilton, in Santa Clara County, Cal., as the most eligible site for the establishment of the observatory in which the great telescope is to be located. Mount Hamilton is 4,448 feet high."

"One of the most disastrous storms that has ever visited our coast recently swept over a portion of Texas and southwestern Louisiana, destroying hundreds of lives and an immense amount of property. Little has been positively determined regarding the cause of these cyclones. From observation it appears they may originate wherever a lower stratum of warm, moist air is rapidly elevated above the sea level. In this moist air an immense mechanical power is stored up, and when condensation caused by its elevation occurs, its moist vapor turns into rain, hail or snow, and an influx of air from all sides rushes in to fill the partial vacuum thus formed. It has been proved that this influx toward a central region is immediately followed by the formation of a whirl, the subsequent development of which is due to further supplies of moist air."

"At a recent session of the French Academy of Sciences, M. Wurtz presented a communication from M. Lecoq, announcing the discovery of a new simple body, a metal analogous and allied to zinc and cadmium, and found in blende or sulphide of zinc in Spain. The existence of the substance was revealed by spectral analysis, two lines appearing which could not be traced to any other element. The new metal has not been reduced from its combinations, so that its physical characteristics remain undetermined. The discoverer patriotically names the new element gallium."

"Mr. H. M. Stanley, the reliever of Dr. Livingstone, is now chief of an African exploring expedition. Starting from Zanzibar on the coast, he began a journey of 720 miles to the great Victoria Nyanza lake and his most important discovery is the verification of Speke's description of Victoria Nyanza as one great inland sea."

"The great fair of Nijni-Novgorod, in Russia, has recently closed its annual display, an exhibition that has been repeated every year for the past four centuries. The merchants and producers of Siberia, of Persia, of China and of Tartary have met the manufacturers of western Russia, exchanged their raw produce for the manufactured goods of St. Petersburgh, Moscow and the west, and separated for another twelvemonth. The fair is a vast market, a temporary city that began with the interchange of commodities between barbarians 400 years ago at a location some 80 miles distant from Nijni-Novgorod. It is estimated that fully a million people come and go while the fair is in progress, and the value of goods exchanged during the fair just closed is computed at about $120 million."

Performance news from Westinghouse

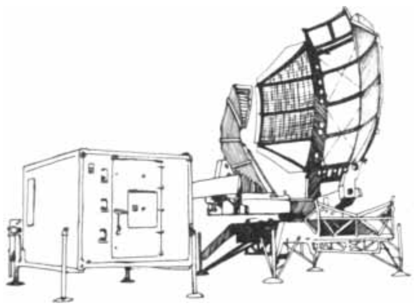
World's most advanced radar family

Westinghouse builds the most cost-effective ground radar systems available today for air defense and traffic control. We have a complete line of tactical and fixed air surveillance radars, ranging from our military AN/TPS-43 to our civil air traffic control ARSR-3.

State-of-the-art radar—The FAA has ordered 14 new long-range, fixed-site, air route surveillance radar (ARSR-3) systems, with an option to order an additional eight. Fully compatible with the latest automated control systems, ARSR-3 will have a range in excess of 230 nmi and a MTBF of 750 hours in the full diplex mode. This state-of-the-art radar will reliably detect small private and large commercial aircraft, since false alarms from ground and weather clutter will be virtually eliminated. ARSR-3 systems modularity will also make site installation easier. Digital circuitry and solid-state electronics will make ARSR-3 more reliable than any ground radar station now in use.

New high-performance radar — We are nearing completion of the prototype phase of the AN/TPS-63, a new-generation tactical radar for the U.S. Marine Corps. This highly mobile radar has the performance necessary to remove, in most cases, the effects of radar ground clutter. It can see through the weather that normally might be expected throughout any site in the world. With this clarity of presentation, and the system reliability associated with modern solid-state components, the AN/TPS-63 will be a very effective air-defense radar.

Tactical 3D radar — The USAF has awarded Westinghouse a contract for the production of 23 new-generation



AN/TPS-43E radars. This radar can be erected in one hour, disassembled in 30 minutes, and moved readily by truck, helicopter, or aircraft. The latest in our family of long-range stacked-beam radars, AN/TPS-43E gives coverage up to 240 nmi with simultaneous height, range, and azimuth data on all aircraft within this coverage. Developed originally for the USAF, AN/TPS-43 also serves the air forces of six other countries. It is recognized as one of the most cost-effective systems available today and can be supplied for fixed-station operation.

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DH-1-75

Westinghouse Defense

THE AUTHORS

HUGH C. McINTYRE ("Natural-Uranium Heavy-Water Reactors") is editorial director of Corpus Communication, Research and Publishing Services in Toronto. His interest is in technical journalism, to which end he obtained his bachelor's degree in chemistry at Mount Allison University in New Brunswick in 1949 and his master's degree in English literature at the University of Toronto in 1953. He worked as a chemist and a high school teacher until he joined the magazine *Canadian Chemical Processing* in 1959. In 1961 he joined Maclean Hunter Publishing Co., where he edited and managed a series of technical publications. After a leave of absence of two years, which he spent with the Ontario government's Commission on Post Secondary Education, he became an assistant editor of *The Financial Post.* He took up his present work in 1973.

BIRGIT SATIR ("The Final Steps in Secretion") is associate research physiologist in the department of physiology-anatomy at the University of California at Berkeley. "I am Danish," she writes, "and received my Ph.D. in biochemistry in 1961 from the University of Copenhagen." She left Denmark in 1962 to marry Peter Satir, who is a cell biologist at Berkeley, and to begin her career in the U.S. She notes that she has "spent 14 years in a research position, which appears to be the fate of many female scientists in this country, particularly if their spouses are in a related field." Her interests include "two lively boys" and travel for the experience of different cultures and different languages. On a trip to Tokyo, she writes, "I picked up a new hobby: Ikebana; I have two certificates in that art."

SHELDON LEE GLASHOW ("Quarks with Color and Flavor") is professor of physics at Harvard University. He notes that he is an alumnus of the Bronx High School of Science, class of 1950, and describes the school as "a most important element in my education." Three of his colleagues in the Harvard physics department are also alumni of the school. Glashow received his bachelor's degree at Cornell University in 1954 and his M.S. and Ph.D. from Harvard in 1955 and 1958 respectively. After work in research at the Niels Bohr Institute in Copenhagen, the European Organization for Nuclear Research (CERN) in Geneva and the California Institute of Technology he taught at Stanford University and the University of California at Berkeley until he joined the Harvard faculty in 1966.

RICHARD GORDON, GABOR T. HERMAN and STEVEN A. JOHNSON ("Image Reconstruction from Projections") are respectively a member of the image-processing unit at the National Cancer Institute, professor of computer science at the State University of New York at Buffalo and associate consultant in the Biophysical Sciences Unit of the department of physiology and biophysics at the Mayo Clinic. Gordon obtained his bachelor's degree (in mathematics) at the University of Chicago and his Ph.D. (in chemical physics) from the University of Oregon. Herman, who was born in Budapest, received his bachelor's, master's and doctor's degrees, all in mathematics, at the University of London; he also has a master's degree in electrical engineering from the University of California at Berkeley. Johnson was graduated from Utah State University and took his Ph.D. (in solid-state physics) at Stanford University.

JEREMY A. SABLOFF and WILLIAM L. RATHJE ("The Rise of a Maya Merchant Class") are respectively associate professor of anthropology at Harvard University and associate professor of anthropology at the University of Arizona. Sabloff, who also serves as associate curator of middle-American archaeology at Harvard's Peabody Museum of Archaeology and Ethnology, was graduated from the University of Pennsylvania and received his master's degree and his Ph.D. from Harvard. "I divide my time," he says, "between teaching, research, writing and advising the more than 300 Harvard and Radcliffe students of Dunster House, where my wife, our one-year-old son and I live." (Sabloff is Allston Burr Senior Tutor of Dunster House.) Rathje received his bachelor's degree in 1967 at the University of Arizona and his Ph.D. in 1971 from Harvard. Among other activities he is director of the University of Arizona's garbage project, which involves studying modern household refuse to see if the information thus obtained about a familiar civilization can be obtained for an ancient civilization by applying similar techniques. Apart from his professional work he draws cartoons and writes songs.

JOHN D. ISAACS and RICHARD A. SCHWARTZLOSE ("Active Animals of the Deep-Sea Floor") work at the University of California; Isaacs is professor of oceanography at the San Diego campus and director of the universitywide Institute of Marine Resources, and Schwartzlose is academic administrator of the marine-life group at the Scripps Institution of Oceanography. Isaacs has been associated with the university since he obtained his bachelor's degree at the Berkeley campus in 1944. From 1958 to 1974 he was director of the marine-life research program at the Scripps Institution. His recent teaching activities have included two courses for undergraduates: marine archaeology and frontiers of marine science. Schwartzlose received his bachelor's and master's degrees (in geography) at Berkeley and started work at the Scripps Institution in 1952.

DIANA DEUTSCH ("Musical Illusions") is a member of the research faculty at the Center for Human Information Processing of the University of California at San Diego. "I was born and grew up in London," she writes, "and obtained a First Class Honours B.A. in psychology, philosophy and physiology at the University of Oxford." Her Ph.D. (in psychology) is from the University of California at San Diego. She is coauthor of a textbook on physiological psychology and is writing a book on the psychology of music. "I had originally intended to become a musician," she notes, "and still spend much of my spare time playing the piano and listening to music. Given this interest, I find it particularly satisfying to do scientific research in an area that relates to musical experience."

HERBERT F. YORK ("The Debate over the Hydrogen Bomb") is professor of physics and director of the Program on Science, Technology and World Affairs at the University of California at San Diego. His career has been both governmental and academic: he was director of the Lawrence Livermore Laboratory, chief scientist of the Advanced Research Projects Agency (ARPA) of the Department of Defense, director of defense research and engineering in the office of the Secretary of Defense and chancellor of the University of California at San Diego. He also served twice as a member of the President's Science Advisory Committee. His article is based on part of his book, *The Advisors,* which has just been published by W. H. Freeman and Company.

JOHN TYLER BONNER, who in this issue reviews *Sociobiology: The New Synthesis,* by Edward O. Wilson, is professor of biology at Princeton University.

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You can't experience today's high fidelity with yesterday's record changer.

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Established 1845 SCIENTIFIC AMERICAN October 1975 Volume 233 Number 4

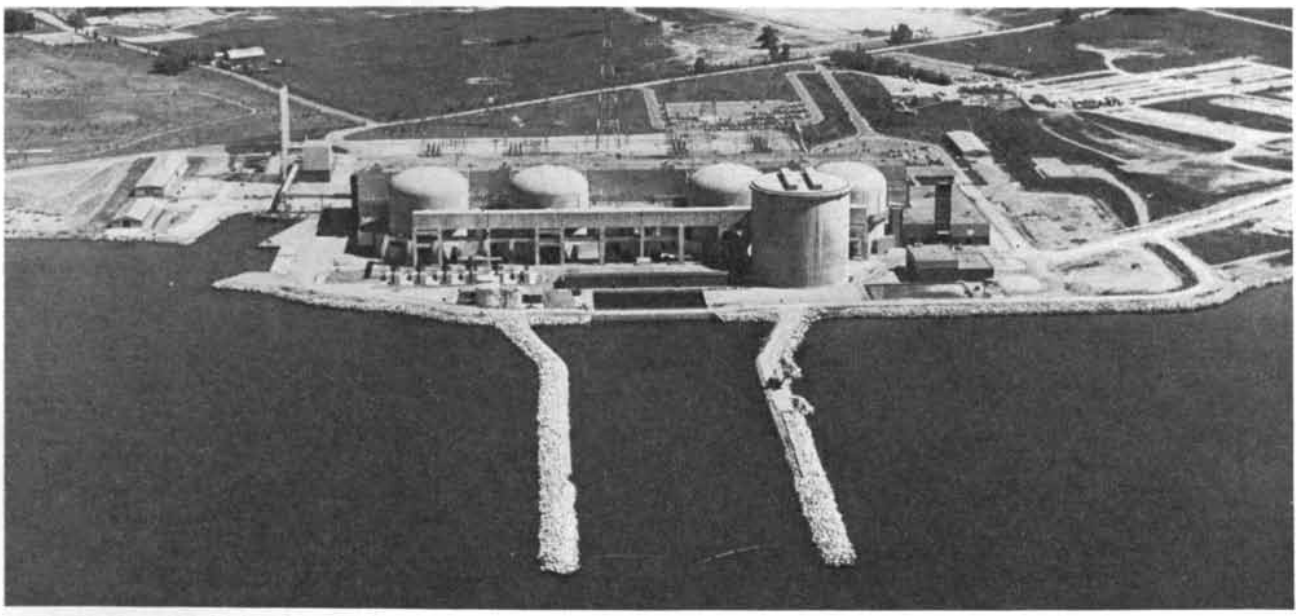
Natural-Uranium Heavy-Water Reactors

In the U.S. power reactors are fueled with enriched uranium and are cooled by ordinary water. The Canadian "Candu" system, working with unenriched uranium and heavy water, offers interesting alternatives

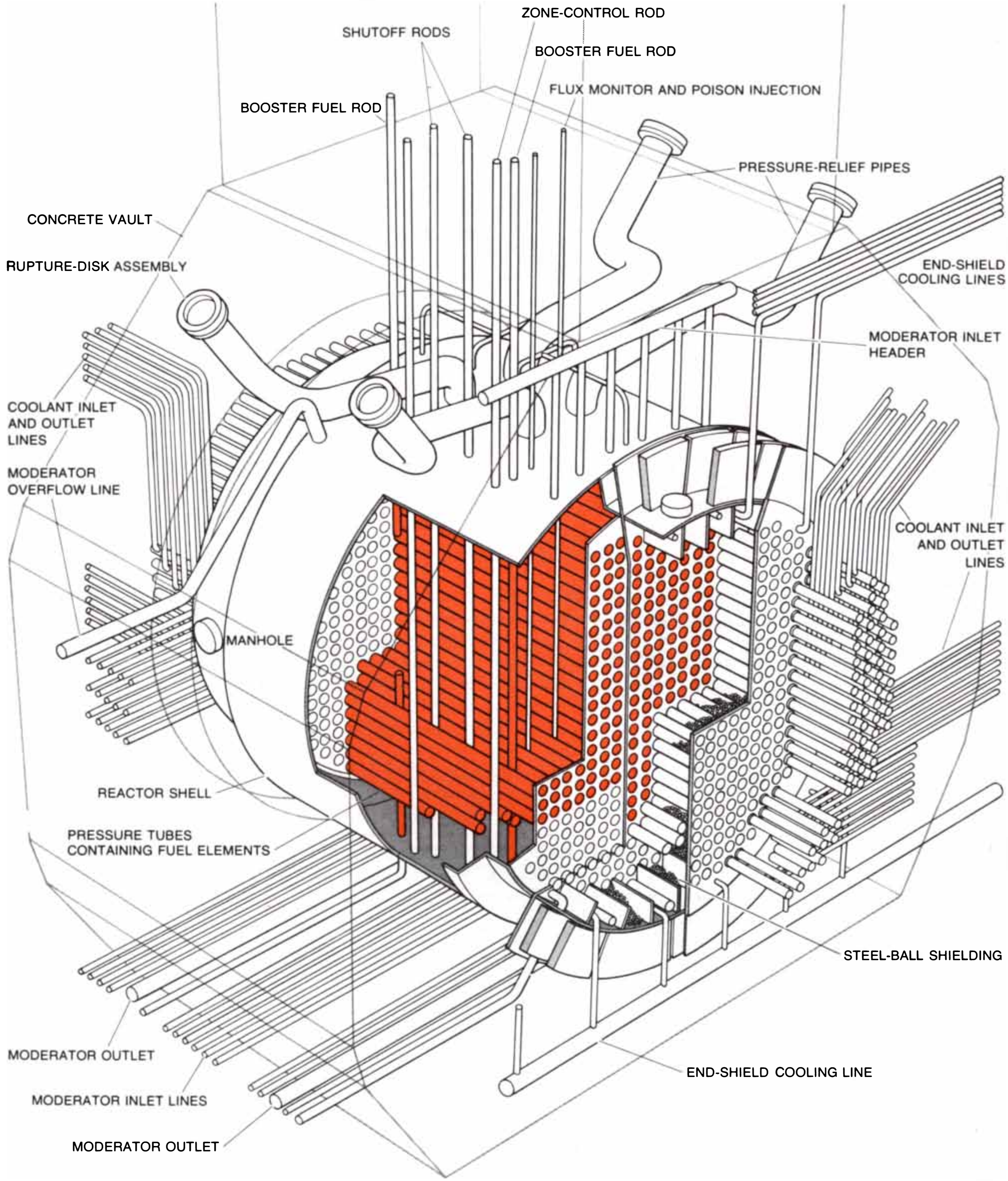
by Hugh C. McIntyre

The U.S. nuclear-power industry, which has dominated the world market with its ordinary-water reactors fueled with enriched uranium, is in a period of deepening uncertainty. In spite of a clear need for nonpetroleum energy sources, a shortage of capital funds and the objections of environmentalists to the siting of nuclear power plants have created a situation in which half of the nuclear power reactors planned for the U.S. over the next decade have been postponed or canceled. Indeed, there is much pessimism about whether nuclear power will be allowed to make its promised contribution to forestalling future energy shortages in the U.S.

Meanwhile, as opponents of nuclear power often point out, domestic uranium supplies are being fed into uranium-235-enrichment plants at a rate that threatens to strip the U.S. of economic deposits of uranium by the year 2000. Acting with what seemed to be commendable foresight, the now superseded U.S. Atomic Energy Commission proposed to solve the long-range uranium-supply problem by developing the fast breeder reactor, which would produce more nuclear fuel



NUCLEAR POWER STATION at Pickering, Ont., has four Candu reactors. Each has a gross generating capacity of 540 megawatts of electricity (MWe); the net capacity after supplying station needs is 514 MWe. The "du" in Candu refers to deuterium oxide (heavy water) and uranium. In the Candu reactor concept deuterium oxide acts both as the moderator, for slowing down neutrons, and as the coolant, or heat-transport medium. The fuel is natural (unenriched) uranium in oxide form. The Pickering station, the first entirely commercial nuclear power facility in Canada, is owned by Ontario Hydro. First of Pickering's units began operating in 1971.



CANDU NUCLEAR REACTOR differs from U.S. nuclear power reactors not only in using natural uranium and heavy water but also in that the coolant for the Candu reactor flows through several hundred individual pressure tubes containing the fuel. In American systems the fuel elements are contained in a single massive pressure vessel through which the coolant flows (*see illustrations on pages 20 and 21*). As this diagram shows, the pressure tubes in the Candu reactor vessel are arranged horizontally. The outer shell for a reactor capable of generating 750 MWe is 28 feet in diameter, 19.5 feet long and 1.25 inches thick. The tubes are made of a zirconium alloy four millimeters (one-sixth inch) thick. The heavy-water coolant, at a pressure of 1,450 pounds per square inch, leaves the reactor at a temperature of about 310 degrees Celsius (590 degrees Fahrenheit) and passes to a steam generator (not shown) where ordinary light water is converted into steam at a temperature of about 250 degrees C. (482 degrees F.) and a pressure of 570 pounds per square inch. The steam drives the turbogenerator that generates electricity. The many pipes that convey the coolant into and away from the reactor ultimately are gathered into two large headers that are connected to the steam generator. The Candu reactor is designed so that the fuel bundles in the individual pressure tubes can be replaced while reactor is running at full power.

than it consumed. The breeder program, since inherited by the Energy Research and Development Administration (ERDA), not only is running behind schedule but also has acquired its own critics. Even its supporters are beginning to ask not when the breeder will be ready but whether it will be ready before the uranium is exhausted. Moreover, not one spent-fuel recovery plant, designed to recycle U-235 and to extract plutonium for fast breeder reactors, has yet gone into operation.

SYSTEM	CUMULATIVE LOAD FACTOR THROUGH JUNE, 1974	ELECTRIC GENERATION (MEGAWATT HOURS)
CANDU REACTORS	56.62 PERCENT	37,065,523
BOILING-WATER REACTORS	49.89	189,229,239
PRESSURIZED-WATER REACTORS	49.35	248,553,124

CUMULATIVE LOAD FACTORS, expressed as a percent of design capacity over an extended period, have been consistently higher for nuclear power reactors of the Candu type than for either of two reactor types that currently supply all U.S. nuclear power. Figures in second column are cumulative load factors for all U.S. and Canadian (Candu) nuclear stations through June, 1974. Figures were assembled by *Nuclear Engineering International.*

Confronted with these problems, U.S. nuclear authorities are looking abroad for possible technological solutions. One promising concept that is gaining attention is the Canadian-designed line of power reactors named Candu. These reactors use deuterium oxide (heavy water) as a moderator and natural (unenriched) uranium as a fuel.

The 20-year development history of the Candu reactor is not well known outside Canada. The first prototype reactor in the Candu series went critical in June, 1962. The second reactor in the series and the first commercial installation began feeding 200 megawatts of electric power (MWe) into the Ontario power grid in 1967. The second commercial installation, a four-reactor complex at Pickering, Ont., went into operation in 1971. With a designed gross generating capacity of 2,160 MWe, it was for nearly two years the largest nuclear station in North America (until the completion of the Commonwealth Edison Zion 2 unit in Illinois surpassed it by 40 MWe). The Candu reactor concept is now available in standard "off the shelf" designs of 600- and 750-MWe capacity. Eighteen new reactors with a total capacity of some 12,500 MWe are scheduled for completion at five sites in three Canadian provinces by 1983. In addition Candu power stations are now operating in India and Pakistan; construction has been started on a station of 600 MWe in Argentina, and a similar station is planned for South Korea. It is noteworthy that Candu reactors have a record of cumulative availability that is better than that of either the boiling-water reactor or the pressurized-water reactor, which are the established power reactors in the U.S. and Europe.

Compared with the present troubled state of the U.S. nuclear industry, the history of the Candu reactors seems almost placid. There has yet to be a major public debate in Canada about the siting of a nuclear power plant. The Pickering station, for example, is within the city limits of Toronto, which has a population of 2.5 million. Although there have been several accidents and malfunctions in Candu nuclear plants, none has so far caused any injury, any radioactive contamination outside the reactor building or any stoppage in the delivery of electric power.

The use of natural uranium as fuel and heavy water as a moderator developed logically out of Canada's needs and background in reactor technology. A restrained effort to sell the Candu reactor abroad has been focused on the requirements of developing countries or medium-size developed countries outside the "nuclear club." So far Canada has made no serious effort to sell the Candu concept to U.S. utilities.

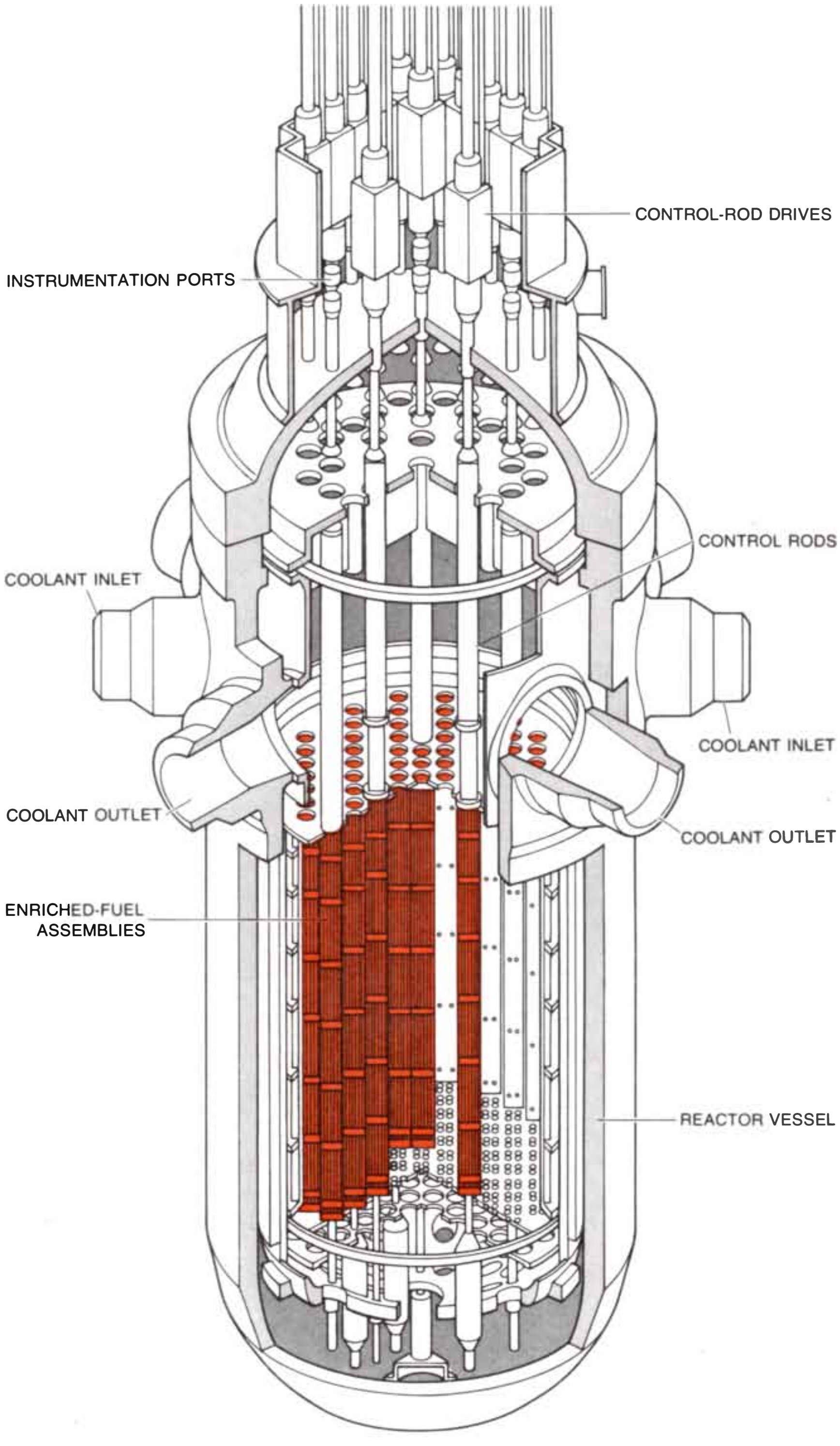
If the situation of the U.S. electric-utility industry should get worse, however, a proposal made last winter by Aaron L. Segal of Cornell University might be taken more seriously. He suggested (at hearings held in New York on Project Independence) that one way the U.S. could benefit from Canada's energy resources would be for it to finance a line of Candu stations along the border to pump electricity south, "like a giant power cow." The U.S. could also logically explore the feasibility of using light water or an organic liquid as the heat-transfer medium for a natural-uranium reactor moderated with heavy water. These are concepts that the Canadians, with limited resources, are just beginning to explore.

All the nuclear power plants built in the U.S. operate with uranium fuel that has been enriched in one of three giant gaseous-diffusion plants so that it contains between 1 and 4 percent U-235. In natural uranium the content of U-235 is only .72 percent. To sustain a nuclear reaction in a reactor fueled with either natural or enriched uranium it is necessary to use a moderator, a substance that is effective in slowing down the high-velocity neutrons released in the fission of U-235 so that they are maximally effective in splitting other nuclei of U-235. When a nucleus of U-235 splits, it releases on the average 2.5 neutrons. The objective in reactor design is to limit the loss of neutrons to competing processes so that exactly one neutron survives to split another nucleus of U-235 and thus maintain a chain reaction. The number of surviving neutrons is expressed by the multiplication factor K. When K is less than 1, there is no chain reaction; when K is greater than 1, the chain reaction proceeds exponentially. Most reactors are equipped with control rods made of a strong neutron absorber such as boron, which are automatically pushed in or out of the reactor core to maintain K at exactly 1.

All nuclei absorb neutrons to a greater or lesser degree. Hence the reactor de-

UNIT COST	PICKERING	LAMBTON (1)	LAMBTON (2)
CAPITAL	4.60	1.70	1.70
OPERATION AND MAINTENANCE	1.10	.96	.96
FUELING	.98	10.60	13.32
HEAVY-WATER UPKEEP	.35		
TOTAL UNIT ENERGY COST (MILLS PER KILOWATT HOUR)	7.03	13.26	16.18

COMPARISON OF ELECTRIC-POWER COSTS in the largest Candu nuclear power station at Pickering with costs in two equally new coal-fired units of comparable size at Lambton, Ont., shows that nuclear power is about half as expensive as fossil-fuel power. Although the capital investment at Pickering is more than two and a half times higher per kilowatt-hour than it is at Lambton, nuclear fuel and supplemental supplies of heavy water, needed for upgrading and replacement, cost only about a tenth as much as coal. It should be noted that the cost comparison is somewhat unfair to the coal units because their output changes with the demand. The nuclear station operates at maximum capacity to supply a base load.



PRESSURIZED LIGHT-WATER REACTOR, developed to power U.S. nuclear submarines, is one of the two systems used in all U.S. nuclear power plants now operating (about 55). Because light water captures neutrons about 600 times more readily than heavy water does, a light-water reactor will not operate unless the fuel is enriched to contain 1 to 4 percent uranium 235 instead of the .72 percent naturally present. A pressurized light-water reactor capable of generating 1,100 MWe requires a pressure vessel about 15 feet in diameter, 45 feet tall and six to 11 inches thick. When the reactor is charged with 196,000 pounds of uranium oxide containing an average of 3.2 percent U-235, it will operate for 10 to 12 months before fresh fuel is needed. Initial charge for a 750-MWe Candu reactor is 292,000 pounds of unenriched uranium oxide. Light-water coolant is heated to 320 degrees C., and it circulates at 2,250 pounds per square inch. A separate steam generator produces steam for turbine at a temperature of 285 degrees C. and a pressure of 1,000 pounds per square inch.

signer must carefully select all components—fuel containers, moderators, coolants and structural materials—to conserve neutrons. In addition certain fission products, such as xenon 135, have such an enormously large "capture cross section" for neutrons that they act as reactor poisons. The great virtue of heavy water as a moderator is that it has a neutron-capture cross section only about a 600th as large as the capture cross section of light water. (In Candu reactors the .2 percent of light-water impurity remaining in the heavy water captures about as many neutrons as the heavy water itself.)

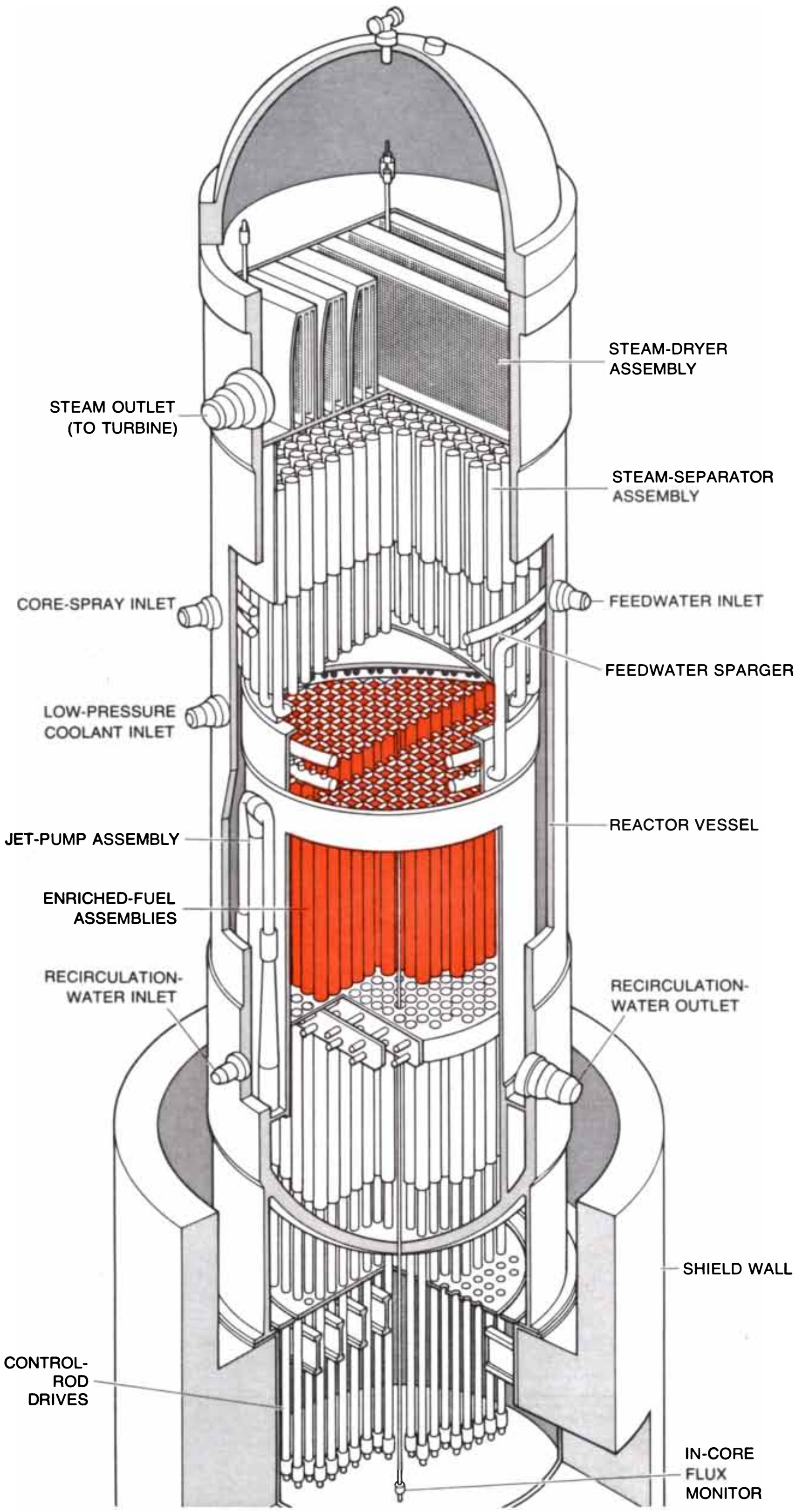
If light water is used as a moderator in a power reactor, it is necessary to have a uranium fuel enriched in U-235 in order to raise the probability that at least one neutron per fission will encounter another nucleus of U-235 before being absorbed or lost from the reactor. When heavy water is used as a moderator, the concentration of U-235 in natural uranium is sufficient to sustain the chain reaction. The main drawback of heavy water, of course, is that it is fairly expensive (about $50 per pound). The compensating advantage is that the use of heavy water obviates the need for building expensive uranium-enrichment facilities or, what amounts to the same thing, buying enriched uranium from those who have the facilities.

The entire question of reactor economics, as one might suspect, is rich in controversy. In making comparisons between rival systems, utilities try to reduce everything to a single figure: the total unit energy cost (TUEC). Unfortunately it is hard to make comparisons between one country and another because of widely differing interest rates, fuel prices and operating policies. To speak only of the Canadian experience, the latest total energy cost figures from Ontario Hydro show that the Pickering nuclear station is generating power for less than half the cost of an equally new coal-fired station of similar size at Lambton, Ont. [*see bottom illustration on preceding page*].

The capital cost of the Pickering station came to about $365 per kilowatt of installed electric-generating capacity (KWe). It appears that the capital cost for a U.S. plant of the same size, at current prices, would be only about 80 percent as much, or roughly $300 per KWe. Thus if U.S. enriched-uranium reactors had been installed at Pickering instead of Candu reactors, Ontario Hydro might have saved .9 mill per kilowatt-hour (KWh) in capital cost plus another .35 mill per KWh in heavy-water upkeep charges. On the other hand, the indications are that enriched fuel in the U.S. is now about five times as expensive as Candu natural-uranium fuel, amounting to a credit of 3.9 mills per KWh in Pickering's favor. On that basis a station similar to Zion 2 would produce power at a cost of about 9.6 mills per KWh if it were sited in Ontario, whereas Pickering produces power for 7.03 mills. This conclusion might be disputed, but it is nevertheless clear that the Candu system is at least competitive with current U.S. nuclear generating systems.

The Candu system has other points in its favor. Unlike U.S. light-water reactors, in which the entire reactor core is enclosed in a single large pressure vessel with a wall as much as a foot thick, the Candu system has fuel rods encased in individual pressure tubes [*see illustration on page 18*]. Although the U.S. pressure vessels are acknowledged to be well designed and extremely safe, it is evident that the failure of one pressure tube or even several in a Candu reactor would have less serious consequences than a break in a large pressure vessel. In the event that one of the pipes supplying cooling water to a U.S. reactor should rupture, the reactor core would have to be cooled promptly by water from an emergency cooling system. How successfully such a system would operate in a real crisis is a matter of earnest dispute. The Candu reactors have a similar emergency cooling system, but in addition a large volume of heavy-water moderator, isolated from the heavy-water cooling cycle, is constantly available to absorb heat in a coolant-loss accident. A final safety advantage of the Candu scheme is the use of on-power fueling, which means that a smaller quantity of heat from decaying fission products is stored in the reactor at any one time than is stored in U.S. reactors, in which fission products accumulate for nine or 10 months between refuelings. On-power refueling also increases the overall availability of Candu reactors to the utility operator. No one of these features was chosen in isolation to enhance the characteristics of the system. Rather, each one stemmed logically from Canada's political, economic and technological position during the decade (1954–1964) in which Candu was developed.

Although assigning the paternity of any complex technical enterprise, involving the ideas of scores of contributors, is hazardous, it is generally acknowledged that the principal credit for the Candu concept goes to W. Bennett Lewis, who was appointed director of



BOILING-WATER REACTOR requires a pressure vessel about three times larger in volume than the vessel needed for a pressurized-water reactor of the same power output. Therefore the vessel for a 1,065-MWe boiling-water reactor would be about 21 feet in diameter and 76 feet high. The vessel walls need to average only about six inches in thickness because the operating pressure of the vessel is only 965 pounds per square inch. The fuel charge is 362,000 pounds of uranium oxide containing an average of 1.1 percent U-235. Steam is generated in reactor and enters turbine at a temperature of about 280 degrees C. Like the pressurized-water reactor, boiling-water reactor must be shut down for refueling.

the Chalk River Nuclear Laboratories in 1946 after a distinguished wartime career in Britain in the development of radar. When Lewis came to Chalk River, a small community some 125 miles west of Ottawa, he found an isolated branch of the Manhattan project left over from World War II. The chief technical effort at Chalk River had been the design and construction of ZEEP-1 (Zero-Energy Experimental Pile No. 1), a reactor moderated by heavy water in which uranium 238 would be transmuted into plutonium. ZEEP did not, in fact, go critical until after the first atomic bomb had been dropped on Hiroshima. By 1947, however, a much larger heavy-water research reactor, NRX, was completed and a still larger one was planned, although it was not finished until 1957.

The design and operation of such reactors trained a corps of specialists in reactor physics, chemistry and engineering with special experience in heavy-water technology. Lewis recognized perhaps more clearly than anyone else that this resource of trained manpower could be used to develop a power reactor that employed heavy water as a moderator and natural uranium as a fuel. The concept would enable Canada to exploit her large resources of uranium without becoming involved in U-235 enrichment, which was then and for many years thereafter a secret technology. In this way Canada, and other nations if they wished, could enjoy the benefits of nuclear power without undertaking work that was closely associated with weapons technology.

In August, 1951, Lewis presented "An Atomic Power Proposal" to C. J. Mackenzie, president of Canada's National Research Council. Mackenzie passed the proposal on to C. D. Howe, Minister of Trade and Commerce, who responded favorably. The document, which was not declassified until 1955, listed three Chalk River findings that indicated the feasibility of a natural-uranium power reactor. First, the operation of NRX showed that energy could be extracted from uranium at a cost from a third to a fourth that of the equivalent thermal output from coal or oil, at prevailing prices. Second, one could expect that a charge of 15 tons of natural uranium, in the form of uranium oxide, would provide an output of 400,000 thermal kilowatts, equivalent after conversion to 120,000 KWe. Third, the system seemed capable of producing steam at 550 degrees Fahrenheit and a pressure of 1,500 pounds per square inch. Although these values were well below the temperatures and pressures achieved in modern fossil-fuel power plants, they would compare favorably with temperatures and pressures achieved in enriched-fuel light-water reactors. The key to the concept was, in Lewis' words, "neutron economy." In order to conserve the much weaker neutron flux from unenriched uranium, every effort had to be made to prevent the thermal (slowed-down) neutrons from being absorbed before they could trigger new fissions in the fuel.

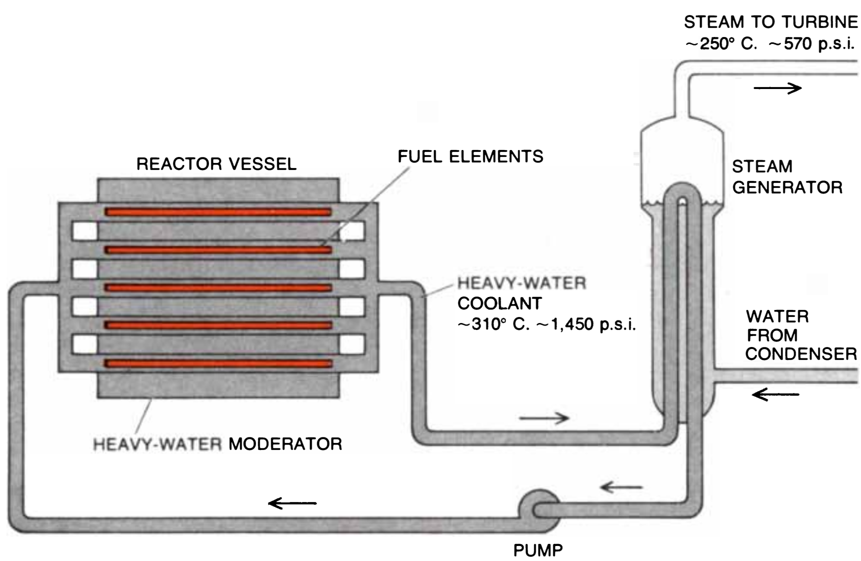
Following Lewis' proposal machinery was set in motion to initiate a Canadian nuclear-power effort. A Crown corporation, Atomic Energy of Canada Limited (AECL), was commissioned in 1952 to exploit the commercial possibilities of nuclear energy. The first board of directors included Richard L. Hearn, the general manager of Canada's largest utility, Ontario Hydro, which was already anticipating a shortage of new hydroelectric sites and was increasingly committed to thermal power generation.

In January, 1954, a group of engineers detached from Canadian utilities was assembled at Chalk River under the leadership of Harold A. Smith (now Ontario Hydro's vice-president for engineering and operations). They were to get an accelerated course in reactor physics from AECL personnel before designing a nuclear power plant. By 1955 a tripartite agreement was concluded: AECL would provide the scientific backing, Ontario Hydro would operate the demonstration power reactor and the Canadian General Electric Company would have the prime responsibility for the design and manufacture of the components. The three participants were to split the costs on a 70-25-5-percent basis. Eight of the senior Chalk River engineers moved to Peterborough, Ont., under J. S. Foster (now president of AECL) to undertake the actual engineering design of the Nuclear Power Demonstration plant (NPD).

The engineering group under Smith, which remained at Chalk River, was armed with a report, NPG-5, that summarized the reflections of Chalk River on the project. NPG-5 envisioned that the reactor would be enclosed in a pressure vessel, similar to those in U.S. designs, and that the fuel rods would consist of metallic uranium tightly jacketed in a light metal to resist attack by water.

Smith and his engineering group immediately began to modify NPG-5. In place of a pressure vessel they proposed that the fuel rods be enclosed in individual tubes through which the heavy-water coolant would flow at a pressure of 1,450 pounds per square inch. The heavy-water moderator would surround the tubes and would be roughly at atmospheric pressure. The pressure-tube configuration, which closely resembles the design of steam boilers in fossil-fuel power plants, was perhaps the most important single decision in the development of the Candu system.

The Chalk River scientific group had no objection to the pressure-tube scheme, but it pointed out that the tubes would interpose a substantial mass of potentially neutron-absorbing material among the fuel rods, which would not be the



ORIGINAL CANDU CONCEPT (*left*) is compared with two variations in an advanced stage of development. In the first of the two variations (*middle*) light water replaces heavy water as the coolant and is allowed to boil inside the reactor, thereby producing steam directly to drive a turbine. In the second variation (*right*) the coolant is terphenyl, an

case in the pressure-vessel arrangement. In order to conserve the precious neutrons the pressure tubes would have to be made out of a material, such as zirconium, with a small neutron-capture cross section. At first this requirement baffled the design engineers; zirconium was so new as a commercial metal that neither reliable mechanical specifications nor accurate cost figures were available. Quoted prices ranged all the way from $60 to $500 a pound. Finally a new zirconium alloy called Zircalloy-2, developed for the U.S. submarine-reactor program, was found to be satisfactory.

The next change the engineering group made in the original NPG-5 design was to turn the reactor on its side so that the pressure tubes would be horizontal. In this way the structural members needed to support the reactor would not interfere with the hundreds of inlets in the reactor face for coolant, fuel and instruments. Loading a long, heavy fuel bundle into a narrow, hot horizontal tube presented problems of handling. The engineers asked: Why not cut the fuel elements into short lengths?

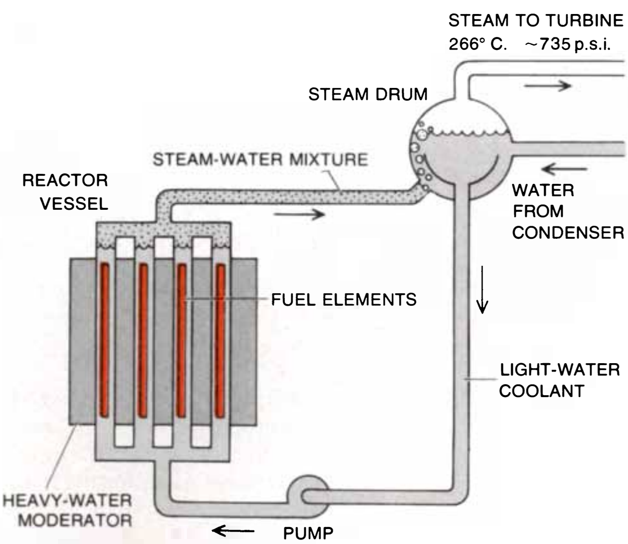
The physicists responded enthusiastically. Cutting the elements into chunks opened the way to shuffling the fuel, thereby solving the problem of getting the maximum burnup out of all the fuel in the reactor core. Burnup is expressed as the number of megawatt-days of thermal power generated per ton of fuel. In the typical case of a single long fuel element the region near the midpoint, which is exposed to the highest neutron flux, may achieve a burnup of 10,000 megawatt-days per ton, whereas the ends of the element, which are exposed to a much lower flux, may reach only 40 percent of that value. A stubby element, on the other hand, could be inserted into the fuel channel at one end and gradually pushed into the central core by the insertion of more short pieces. By the time the element was finally ejected at the other end of the channel it would have achieved a burnup of nearly 10,000 megawatt-days, 30 times what had been thought possible only five years earlier.

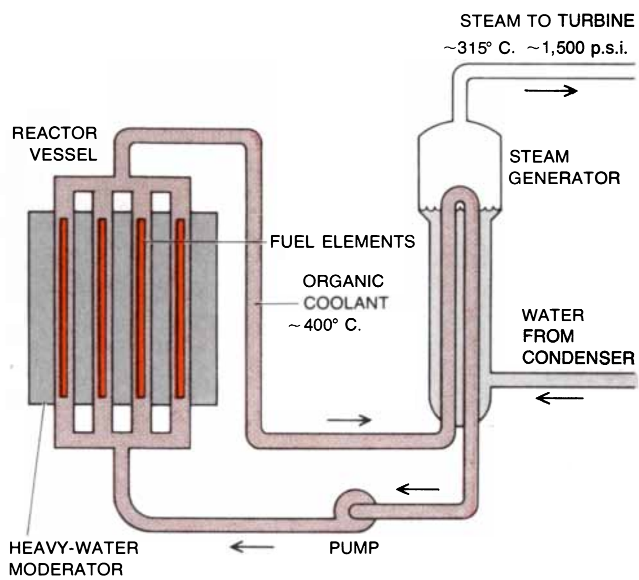
The final significant change worked out in this interaction between engineers and scientists was the substitution of uranium oxide fuel for metallic fuel. The oxide fuel is better able to retain fission products, including gaseous ones, at high temperature without swelling. It also resists attack by water. Only its much lower thermal conductivity militated against it. That deficiency was solved by bundling together a number of thin oxide rods coated with Zircalloy, through which the heavy water would circulate. By December, 1956, a new report, NPG-10, was drafted by the Chalk River engineering-development group and was given to the Peterborough design-engineering group for assessment. The demonstration reactor envisioned in NPG-10 was essentially the Candu reactor of today.

The first prototype embodiment of the NPG-10 power reactor, the Nuclear Power Demonstration plant, was designed to generate 22 MWe, a tenth of what was considered a commercial scale at the time. The NPD was sited 30 miles up the Ottawa River from the Chalk River Nuclear Laboratories. Even the modest scale of nuclear engineering required by the reactor was a challenge to existing Canadian manufacturing facilities.

For example, the vessel enclosing the reactor core was made of aluminum sheets with a total thickness of 1¼ inches, shaped into a cylinder 15 feet long and 17 feet in diameter. The fabrication of the vessel required 200 separate joints, which consumed two tons of weld metal. The welds were checked by taking 12,000 radiographs, which revealed 50 initial defects necessitating the removal of 20 feet of weld. Repeated inspections showed that there was only about a 50 percent chance of correcting the defect on the first try. The second round of inspection disclosed 26 remaining defects, the third round 13, the fourth round seven, the fifth round five and the sixth round none.

Further lessons were learned during the operation of the NPD. In the first attempt at refueling while the reactor was running, two leaks in the head of the fueling machine, operating at 1,200 pounds per square inch, allowed 75 tons of heavy water to escape over a period of two days until the leaks could be repaired. As fast as the heavy water leaked out it was collected in a sump and pumped directly back into the reactor, impure and oily as it was, to keep the fuel cool. The total volume of leakage





organic liquid with a boiling point much higher than that of water. With terphenyl as a heat-transport medium it should be possible to generate turbine steam with a temperature of at least 315 degrees C. (599 degrees F.), thereby raising the overall thermal efficiency of a nuclear power plant from present value of 30 percent to 35 percent. (Modern fossil-fuel generating stations achieve 40 percent.) Atomic Energy of Canada Limited has been operating an experimental organic-cooled reactor at Whiteshell, Man., since 1966.

CANDU POWER REACTORS	START-UP DATE	GENERATING CAPACITY (MEGAWATTS)
ROLPHTON, QUE.:		
NUCLEAR POWER DEMONSTRATION PLANT (NPD)	1962	22
DOUGLAS POINT, ONT.:		
OPERATING	1967	200
GENTILLY, QUE.:		
BOILING LIGHT WATER (BLW) (EXPERIMENTAL)	1971	250
UNDER CONSTRUCTION (CONVENTIONAL CANDU)	1979	600
PICKERING, ONT.:		
OPERATING	1971	4 × 540
UNDER CONSTRUCTION	1980	4 × 540
BRUCE, ONT.:		
UNDER CONSTRUCTION	1976	4 × 745
PLANNED	1982	4 × 750
POINT LEPREAU, N.B.:		
UNDER CONSTRUCTION	1980	600
DARLINGTON, ONT.:		
PLANNED	1983	4 × 800
PAKISTAN:		
OPERATING (KARACHI)	1971	125
INDIA:		
OPERATING (RANA PRATAP)	1972	2 × 200
PLANNED (KALPAKKAM)	1980 ?	2 × 200
PLANNED (NARORA)	1980 ?	2 × 200
ARGENTINA:		
UNDER CONSTRUCTION (RIO TERCERO)	1979	600
SOUTH KOREA:		
PLANNED (WOLSUNG)	1980	600

EIGHT CANDU POWER REACTORS, ranging in output from 200 to 540 MWe, are now operating in three countries. The count does not include the 22-MWe Nuclear Power Demonstration plant (NPD), which proved out the Candu concept, and the 250-MWe experimental unit at Gentilly, Que., which uses light water instead of heavy water as a coolant in a reactor that otherwise follows the Candu scheme. The two Candu units at Rana Pratap were a Canadian-Indian project in which Indian companies made many of the components. Candu-type units planned for Kalpakkam and Narora will be completed without further Canadian aid. Plutonium for India's nuclear explosion of 1974 was "cooked" in a small heavy-water natural-uranium reactor, a copy of Canada's NRX reactor at Chalk River, Ont.

exceeded the total inventory of heavy water normally required in the NPD both as a moderator and as a coolant.

With the experience gained with the NPD, plans were drawn up for the first commercial station, to be located at Douglas Point, directly west of Toronto on the eastern shore of Lake Huron. This 200-megawatt station would be financed by the Canadian government and owned initially by the AECL but would be purchased by Ontario Hydro "as soon as it was operating efficiently." Two principal changes were made in designing the Douglas Point reactor: the vessel enclosing the reactor core was fabricated of stainless steel instead of aluminum, and a small number of "booster rods" containing slightly enriched uranium were introduced so that the reactor could be restarted faster after a shutdown. Otherwise the changes were few, since the original target date for the completion of the Douglas Point station was 1964, only two years after the first operation of the 22-MWe prototype.

In spite of an intensive program of development, a variety of problems led to delays that prevented the start-up of the Douglas Point station until early in 1967. From the outset the seals on the transport pumps that circulated the heavy water leaked badly. Within a few months the station had to be shut down so that the pumps could be rebuilt and fitted with controlled-leakage throttle bushings. Ever since then the reactor operators have found it difficult to control the flow of heavy-water coolant because of the large gland flow required by the new bushings. To date Douglas Point's cumulative availability has been disappointing: only 45 percent compared with 60 percent for the NPD prototype.

To be fair it should be noted that much of Douglas Point's downtime can be attributed to a persistent shortage of the most critical component of the Canadian nuclear-power program: heavy water. Many familiar with the problem blame the heavy-water drought on an American, Jerome Spevack, head of the Deuterium Corporation. That company held patents to variants of the GS hydrogen sulfide ion-exchange process, which was used by the U.S. to produce heavy water at Savannah River, Ga., and was subsequently licensed to AECL.

Spevack persuaded political authorities in the coastal province of Nova Scotia to back his plan for producing heavy water at Glace Bay, near the depressed coal town of Sydney on Cape Breton Island. AECL refused to provide Spevack's Canadian firm, Deuterium of Canada, with financial assistance but agreed to buy its product. Unfortunately the Sydney plant produced only a trickle of heavy water before it was forced to shut down, defeated by insoluble design problems and by corrosion caused by the use of salt water as a coolant. Spevack managed to sell his interest, leaving Nova Scotia with a useless $90-million plant. The plant has now been completely rebuilt by AECL at a cost of more than $100 million and is about to be restarted.

Following the collapse of Spevack's venture AECL was left without a domestic supply of heavy water. Canadian General Electric was thereupon persuaded to build a plant at Port Hawkesbury in Nova Scotia. Beset by various problems of its own, the plant nevertheless proved capable by 1967 of achieving more than half of its design capacity of 400 tons of heavy water per year, using AECL's version of the GS process. Unfortunately for General Electric it had contracted to sell heavy water on an unrealistic declining price schedule, which turned out to be far below the prevailing world price of about $50 per pound. When the contract price fell to $17 per pound early this year, General Electric asked for relief. AECL has now agreed to buy back the plant for $90 million, a figure that at least allows General Electric to recover its investment.

Meanwhile the domestic success of the Candu concept had led to an acute shortage of heavy water in 1970. In order to provide heavy water for the four-reactor Pickering station, AECL had to buy the heavy-water inventory of the experimental Swedish Marviken heavy-water station for $11.9 million and pay an additional $55.8 million to the U.S. for almost two million pounds of heavy water extracted at Savannah River. A few million dollars' worth of heavy water was even imported from the U.S.S.R. in 1970 and 1971.

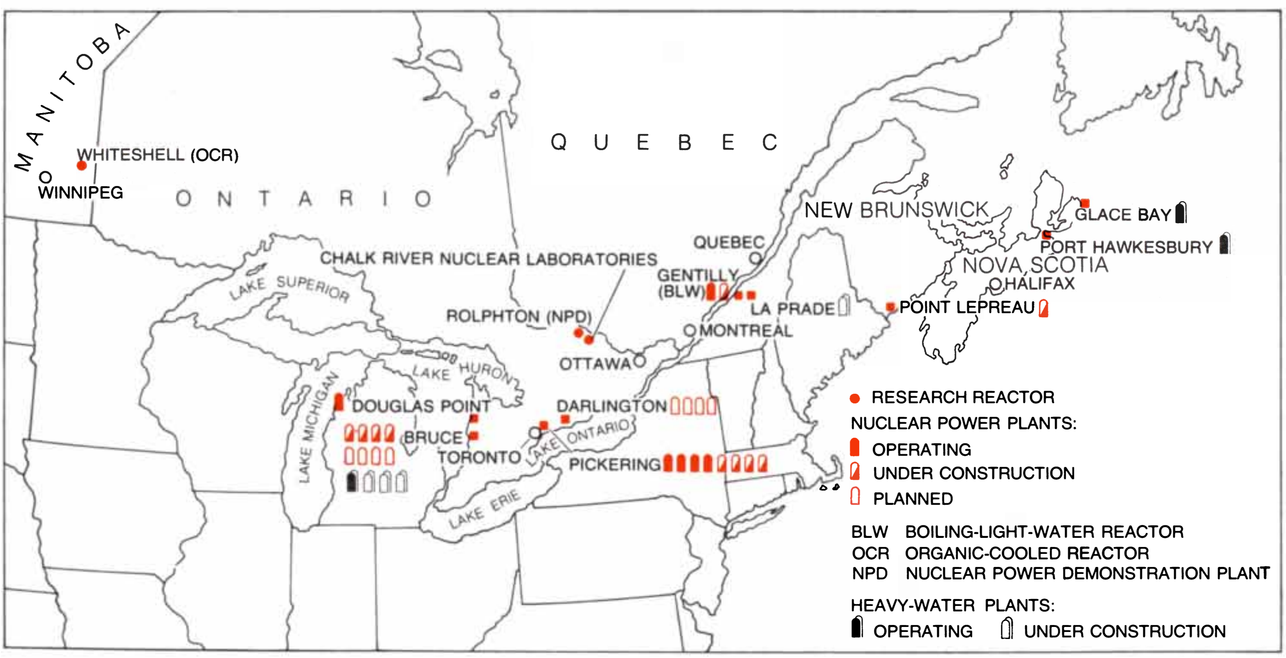
In spite of these purchases AECL had to juggle its limited heavy-water supplies. For example, Douglas Point had to be shut down in order to get enough heavy water to start up the larger and more efficient Pickering station. Douglas Point was subsequently restarted with heavy water borrowed from Quebec's first nuclear station at Gentilly. The plant at Gentilly is an experimental prototype to test the performance of a modified Candu system using heavy water as a moderator but light water as a coolant. The shortage of heavy water finally came to an end with the completion late in 1973 of a heavy-water plant with an annual design capacity of 800 tons, built by AECL at Bruce, near Douglas Point. The plant has since been bought by Ontario Hydro.

Roughly one ton of heavy water is needed for each additional megawatt of installed Candu capacity. At present the Bruce plant and a plant half as large at Port Hawkesbury are running at just over 70 percent of their design capacity. The start-up of the rebuilt Glace Bay plant will provide a total annual production of about 1,000 tons, or enough for a nuclear-power growth rate of 1,000 MWe per year. Beginning in 1980, however, Ontario Hydro alone expects to expand its nuclear capacity at the rate of 2,500 MWe per year. In addition AECL has further commitments to Quebec and New Brunswick, and obligations to Argentina, South Korea and Britain as well. To provide the needed heavy water Ontario Hydro is building three more heavy-water plants, all at the Bruce site, bringing total capacity to 3,200 tons per year. (Some of the process heat for heavy-water production is being supplied by nuclear steam from the Douglas Point nuclear station.) AECL will supplement the heavy-water production of its two Nova Scotia plants with 800 tons per year from a plant at La Prade, near the Gentilly nuclear site. The total projected output of all the Canadian plants (assuming that they will operate at 70 to 75 percent of capacity) is 3,500 tons of heavy water per year, worth some $350 million at current prices.

As Canada's only utility with experience in nuclear engineering, Ontario Hydro has taken the lead in modifying the Candu concept. Its cautious approach is typical of a large utility, and it has stayed with the basic Candu concept for stations projected well into the 1980's. On the other hand, it has worked steadily to improve the details of the system: instrumentation, pumps and other subsystems needed to minimize leakage of the costly heat-transfer medium. Wherever possible, sophistication has been replaced by simplicity.

The performance of the 2,160-MWe Pickering station, in spite of a construction strike that delayed the completion of the first unit by nine months and an operators' strike that shut the plant down for a few months in 1973, has been exemplary. In February, 1973, the fourth unit was operating at full power only 12 days after first going critical—a record for any type of power reactor. Since the first two units went into service they have operated at a capacity factor of 80 percent (equivalent to producing 80 percent of theoretical capacity at continuous full power). Unit No. 4 has achieved a record capacity factor of 93 percent. Only unit No. 3, shut down for half of 1974 because of cracks in its pressure tubes, has scored no better than 65 percent.

The cracking incident, the only major technical malfunction at Pickering so far, led to vigorous corrective action. Within three months, at the end of 1974, a total of 17 leaking tubes were identified and were removed from the radioactively hot core. The tubes were sent to Chalk River, where it was determined that the tiny cracks (less than an inch in length) were in regions of high residual stress near rolled joints, and had resulted from faulty rolling technique. The cracks propagated only during cold shutdown; at operating temperature the zirconium hydride that had propagated the crack was reabsorbed into solution. Replacement tubes were installed within three months and the unit was restarted in April of this year. Similar tubes in unit No. 4 and in two of the Bruce units were



CANDU REACTORS AND HEAVY-WATER PLANTS are in four of Canada's 10 provinces. Candu reactors now supply 14 percent of the power in one province, Ontario. (For the U.S. as a whole nuclear energy supplies 8.5 percent.) The three heavy-water plants now operating have a capacity of 1,600 tons per year. When units now under construction are completed, capacity will be 4,800 tons.

removed and were relieved by repetitive stressing as a precaution against further cracking. A correct rolling technique was prescribed for all future tube installations.

It is perhaps obvious that the strength of the Candu system in contributing to the country's energy self-sufficiency is more than simply technical. It is also a matter of social organization. In contrast to the U.S. system of encouraging private industry to develop nuclear power under the supervision of public authorities at various levels, the Canadian approach has been one of government-guided development. Work on the Candu concept has from the first been a partnership involving a governmental regulatory authority (the Atomic Energy Control Board), a public development organization (AECL) and private manufacturers (mainly Canadian General Electric and Westinghouse of Canada), together with publicly owned utilities.

Unlike General Electric and Westinghouse in the U.S., which respectively promoted the boiling-water reactor and the pressurized-water reactor, no Canadian company was large enough to act as both a supplier and a designer of complete nuclear power stations. For a brief period in the 1960's the Civilian Atomic Power Department of Canadian General Electric attempted to fill this role. It sold one complete station to Pakistan, the Kanupp reactor, which went critical in 1971 and is still operating successfully (although not yet at its design capacity of 125 MWe because of limitations in the associated power grid).

Encouraged by this success, Canadian General Electric tried to market its own version of the Candu system, named HWR. This system was similar to the Candu one but had vertical pressure tubes. After a year or so without contracts the company liquidated the operation. Most of the designers and engineers were absorbed back into AECL's "power projects" group, and Canada was back to a single reactor-design group.

As things now stand (and they have changed very little since 1962) the Atomic Energy Control Board is the final authority in Canada's nuclear affairs. It regulates the siting, the design and the safety and security systems of all nuclear installations; the manufacture, transport and utilization of all nuclear equipment, nuclear fuel and other radioactive material, and all import and export of nuclear commodities and equipment. In its major actions the board works closely with its sister government organization AECL (from which it draws most of its personnel). This is not to say that there is always perfect harmony among the regulatory authorities. For example, AECL would like to sell more reactors abroad than the Atomic Energy Control Board has so far allowed it to.

There is no question that the Canadian government was acutely embarrassed by India's underground nuclear explosion in 1974. AECL had regarded India as an apt pupil in nuclear matters. The Canada-India Research reactor (CIR) in Trombay in which India "cooked" its plutonium (from its own natural uranium) was a copy of Chalk River's NRX, and Canada contributed technical assistance and a major share of the cost. AECL also took a prominent part in aiding and financing a two-unit power station of the Candu type at Rana Pratap. The Indian effort was a joint one in which AECL deliberately tried to encourage Indian self-sufficiency, including the development of a local component-manufacturing industry. The success of the policy is suggested by the fact that even when Canada had cut off all nuclear trade and aid after the 1974 explosion, India's Atomic Energy Commission announced that it was proceeding with two more two-unit stations of the Candu type at Kalpakkam and Narora. It conceded, however, that their completion might be delayed by as much as five years as a result of the withdrawal of Canadian help.

At present Canada's nuclear firms have full order books. For example, in Ontario the first unit of the 3,000-MWe Bruce station will start up early next year, with all four units scheduled to be operating by 1978. By that time the project of doubling the size of the Pickering station will be in full swing, with completion scheduled for 1985. By then another 3,000-MWe station will be well under way at the Bruce site and still another 3,000-MWe station at Darlington will have been started, with completion scheduled for 1984. By 1980 two more 600-MWe nuclear units will be in operation, one at Gentilly in Quebec and the other at Point Lepreau in New Brunswick; they will be the first commercial units in each province. Manitoba Hydro, still busy completing a huge hydroelectric system on the Nelson River, has intimated that it will make a nuclear commitment by 1980.

Beyond this, Candu stations will be running by 1979 at Rio Tercero in Argentina (which has built an expensive heavy-water prototype plant of its own at Atucha). A unit of similar size should be operating a year later in South Korea. Both countries have expressed interest in a second unit.

Meanwhile in Britain the nuclear authorities have recognized a technological gap between its gas-cooled, graphite-moderated reactor program and the fast breeder reactor, which is expected to prevail in the future. The gap, expected to last for at least a decade, is being filled by the "steamer," a heavy-water moderated, vertical-pressure-tube reactor using slightly enriched uranium, which owes much to the Candu experience and for which Canada has agreed to supply 1,000 tons of heavy water by 1980.

AECL scientists and engineers, however, are far from complacent about today's Candu system. They point out several major weaknesses: the pressurized-heavy-water coolant cannot be raised to a temperature high enough to generate steam of a pressure and temperature comparable to that in conventional fossil-fuel stations. The Candu units therefore require large turbines with a special blade design to prevent erosion from wet steam. If a way could be found to generate hotter and higher-pressure steam from a Candu reactor, the overall thermal efficiency of the station could be raised from the present 30 percent to about 40 percent, which is now achieved in the best fossil-fuel power plants.

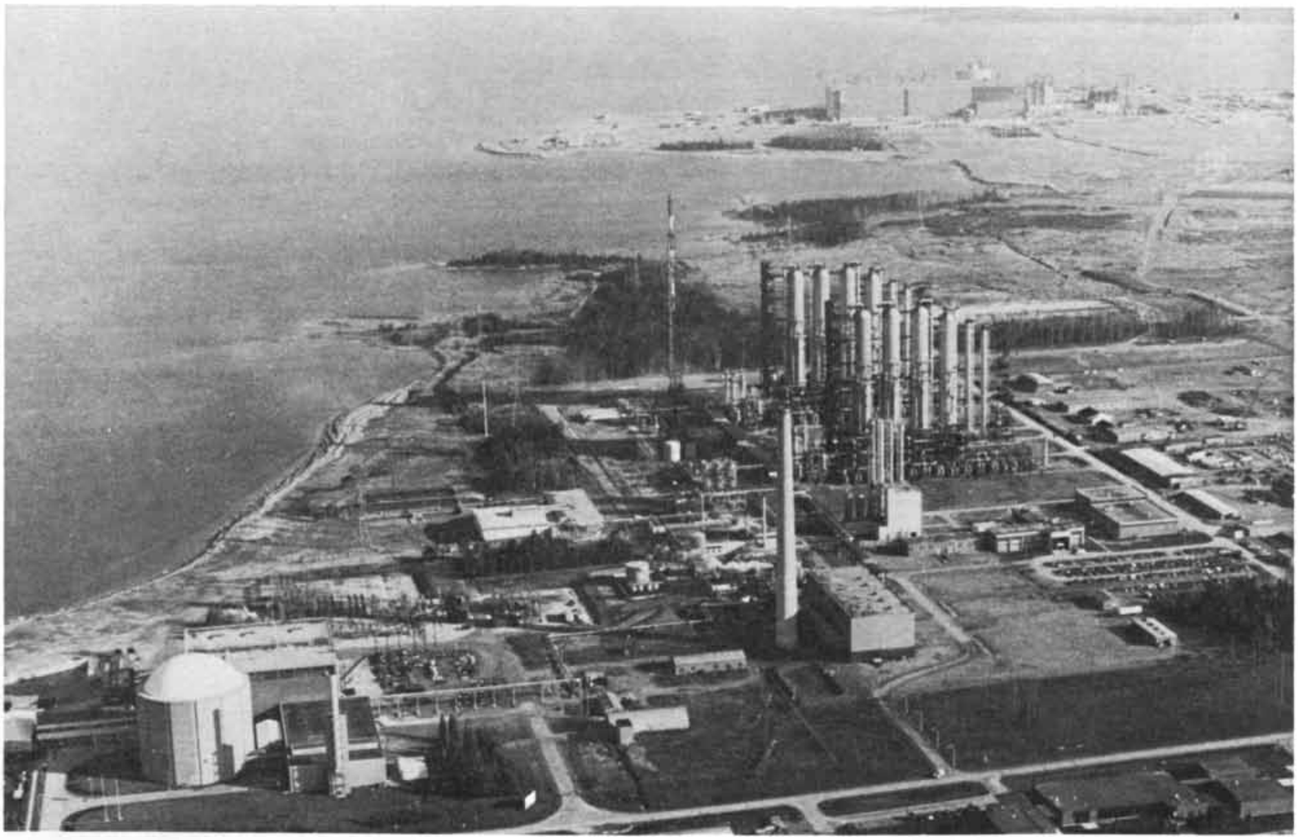
Another deficiency is that the Candu reactor cannot be controlled as finely as the utilities would like. Among the fission products of uranium 235 is iodine 137, which decays in a few minutes to gaseous xenon 135. The last has such an enormously large capture cross section for thermal neutrons that even in low concentrations it can devour enough thermal neutrons to lower K below the critical value of 1. When a Candu reactor is running at close to full power, the steady-state equilibrium between the neutron flux and the concentration of xenon 135 is such that criticality is maintained. If one wanted to cut back the reactor abruptly to half power because of lower power demand, however, the delicate equilibrium would be upset and the reactor would "poison out," that is, shut itself down. The reactor cannot be restarted until the xenon 135 decays into daughter products with lower capture cross sections, which takes about 40 hours. This deficiency is overcome in the latest Candu designs by the insertion of rods of slightly enriched uranium.

Finally, Candu reactors, like the present generation of U.S. reactors, are admittedly wasteful of uranium because of their once-through fueling. Even Canada, richly endowed with uranium though it is by world standards, sees an increasing shortage, even for its own purposes, by the end of the century. AECL believes some relief can be gained by turning to a thorium cycle in a reactor that is something less than a true breeder. AECL experts state that with little, if any, change in the Candu reactor it should be possible to burn thorium in combination with either U-235 or plutonium. Calculations show that a self-sufficient thorium cycle with a capacity of 1,000 MWe could be launched with an initial inventory of no more than 1,200 metric tons of natural uranium to provide the initial supply of U-235. In the thorium cycle the common isotope of thorium, thorium 232, is converted to fissionable uranium 233 by the absorption of a neutron.

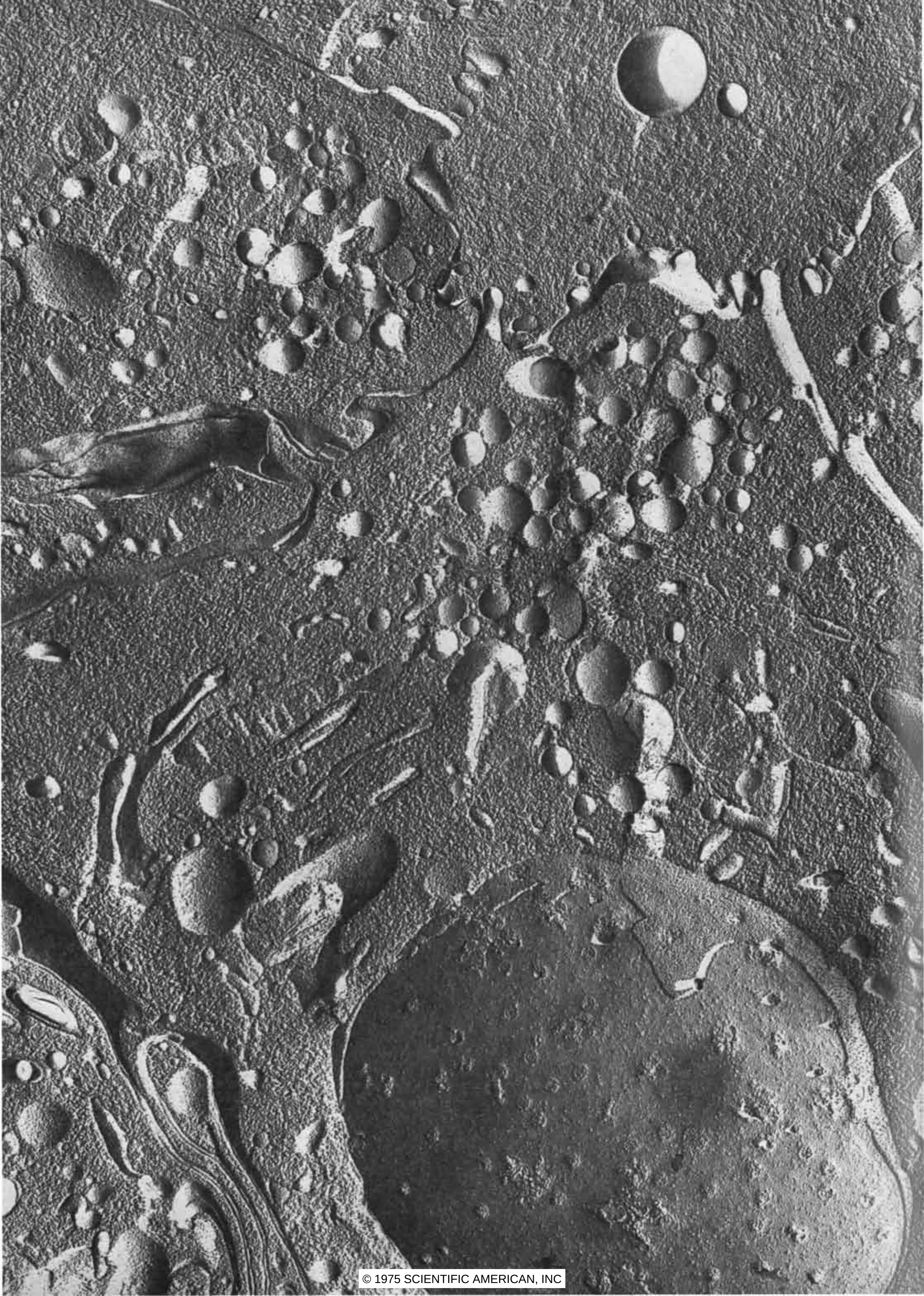
Meanwhile, until the development work on the thorium cycle has progressed further, AECL is pursuing a more direct route toward upgrading the low efficiency of the all-natural-uranium cycle. It has nearly completed a modest fuel-fabrication pilot plant at the Chalk River Nuclear Laboratories to produce plutonium-enriched uranium fuel elements for testing in research reactors. The idea is to add about half a gram of plutonium 239 to each kilogram of uranium. Since a kilogram of natural uranium contains 7.2 grams of U-235, the "spiked" fuel would contain a total of 7.7 grams of fissionable material. Even with this minor enrichment it should be possible to approximately double the total power output of each fuel pellet before it must be replaced. The anticipated improvement is from 8,000 megawatt-days per ton to 15,000 megawatt-days.

In addition to considering new fuel cycles for Candu, AECL is restudying heat-transport mediums. A prototype reactor using light water as a coolant instead of heavy water has been operating at Gentilly since 1971. A research reactor at Whiteshell in Manitoba has been operating since 1966, cooled by terphenyl, an organic liquid that boils at something above 700 degrees F.

Ontario Hydro is directing its efforts toward operating the present Candu reactors more efficiently. One proposal is to run the reactors at close to full power and to store excess steam at high pressure in large underground caverns to handle peak loads. Another use for off-peak steam would be to supply a large-scale district heating system. Since the Pickering station is less than 20 miles from downtown Toronto, engineers of Ontario Hydro have been studying district-heating systems in Sweden to see how they might be adapted to Canadian circumstances. Another attractive possibility, based on the use of waste heat rather than on primary steam production, is for year-round fish farming. A feasibility study is now under way to see how the waste heat from a nuclear power station can be used most effectively for aquaculture. In Canada's frigid climate heat must be considered a valuable resource rather than a pollutant.



HEAVY-WATER PLANT on the shore of Lake Huron at Bruce, Ont., is the towers beyond the smokestack at right center in this photograph. On the point beyond the towers is the Bruce Nuclear Power Station, which is still under construction. The domed building in the left foreground is the Douglas Point Nuclear Power Station, the prototype of the Candu system. When the Bruce heavy-water plant is completed in 1980, it will consist of four units with a total capacity of 3,200 tons per year. Only one unit is now in operation. A typical sample of water from Lake Huron, which supplies the Bruce plant, contains from 145 to 148 molecules of deuterium oxide in every million molecules of water. The separation of deuterium oxide from light water is carried out by a modified distillation process that exploits the fact that deuterium oxide has a slightly higher boiling point than light water: 101.52 degrees C. as against 100 degrees at normal atmospheric pressure. The distillation process yields deuterium oxide with a minimum purity of 99.75 percent.



THE FINAL STEPS IN SECRETION

Precisely how does the living cell secrete substances? It seems that such substances are packaged in a membrane that releases its contents when it fuses with the outer membrane of the cell

by Birgit Satir

The main business of many cells in a multicellular organism is to manufacture and export products that are put to use elsewhere in the organism or outside it. The products of these secretory cells range from ions and simple molecules to complex molecules of protein and polysaccharide. In our laboratory at the University of California at Berkeley we have been concerned with the export side of the business, that is, with ascertaining how the manufactured product is transported from the cell's cytoplasm, where it is made, through internal or external membranes and into the cell's environment. Even though the locus of secretion is minuscule and a single secretory event is completed within a millisecond or so, we have been able to determine the major steps in the process.

In considering secretion it is useful to bear in mind the distinction between the prokaryotic cell, which lacks a distinct nucleus, and the eukaryotic cell, which has one. In bacteria and other prokaryotic cells, where the internal organization of the cell is relatively simple, the secretory process is also simple in that a product made in the cytoplasm passes through the plasma membrane, or outer membrane, of the cell and is released directly into the surrounding medium. Eukaryotic cells differ from the simpler ones in having elaborately folded intracellular membranes, collectively known as the endoplasmic reticulum. The endoplasmic reticulum divides the cytoplasm of the eukaryotic cell into two compartments, the cytoplasmic matrix and the cisternal space. In these more highly organized cells many secretory products are made on the membranes of the endoplasmic reticulum and then pass into the cisternal space, where they are modified and packaged for release at an appropriate time.

Many spectacular examples of specialized secretory events are known both in protozoans and in more complex organisms. In the more complex organisms the product often accumulates at the apex of the secretory cell in a membrane-bounded container: the secretory granule or vesicle. For example, the apex of an acinar cell of the human pancreas is usually filled with spherical zymogen granules, which are the secretory vesicles of this cell. They contain the digestive enzymes of the pancreatic juice, such as trypsin and chymotrypsin, in a precursor form.

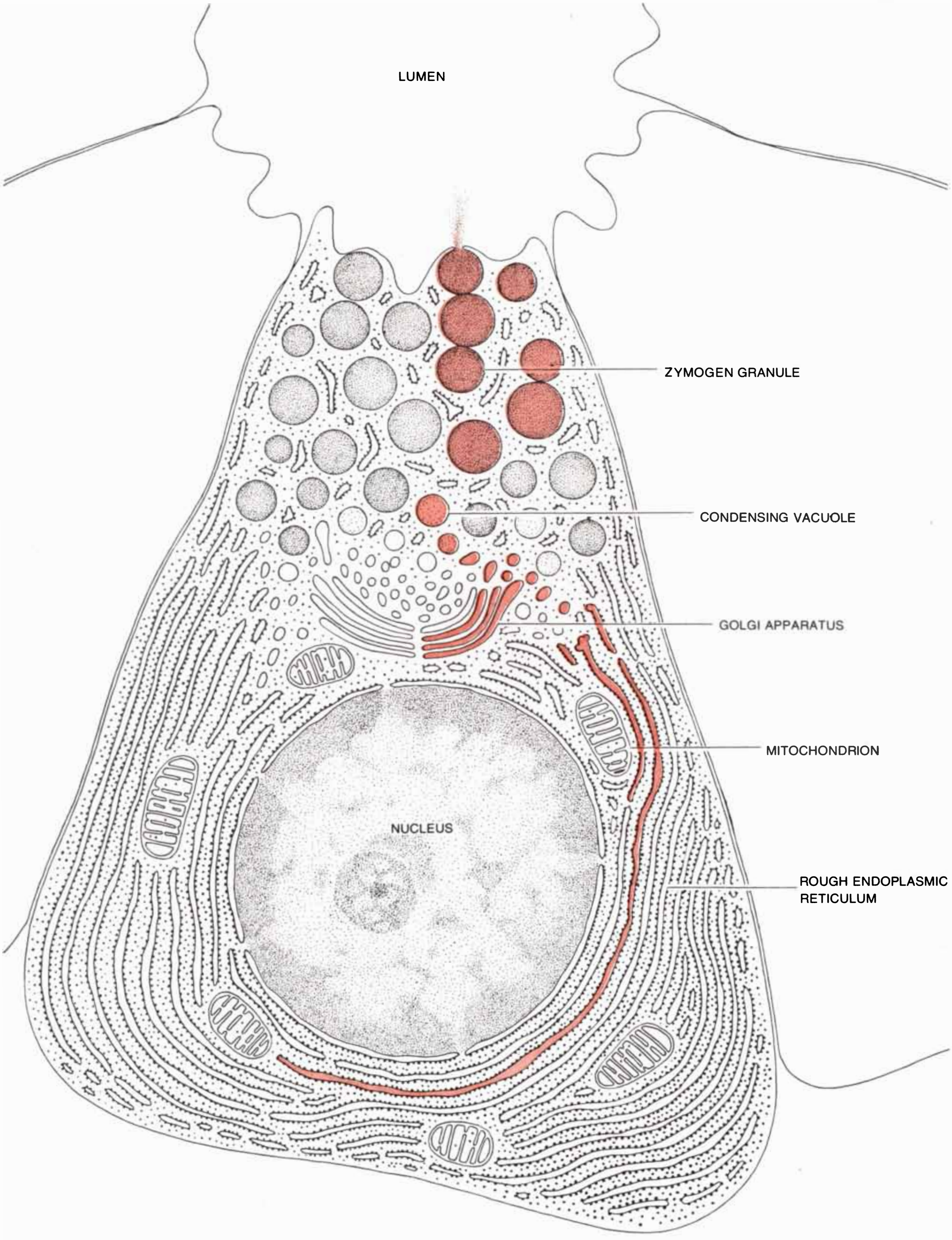
A second example is found in the highly differentiated sperm cell of many animals. As the sperm cell develops, a special secretory granule in it matures. It is a membrane-bounded vesicle called the acrosomal granule. The granule comes to lie above the nucleus at the front end of the sperm cell, where it awaits contact with the protective layers around the egg cell. At a crucial moment the acrosomal granule fuses with the plasma membrane of the sperm cell and releases its content of digestive enzymes, which disrupt the protective coats of the egg cell and enable the sperm to reach the egg-cell plasma membrane.

Another important secretory event takes place at the synapse, or region of contact, between a nerve cell and a muscle cell in a vertebrate animal. The nerve cell's long axon branches and becomes filled with small vesicles containing a neurotransmitter such as acetylcholine. When a nerve impulse traveling down the axon of a motor nerve cell reaches the synapse, a swarm of vesicles quickly fuse with the cell membrane, releasing the neurotransmitter. The released substance diffuses across the narrow space between the axon and the muscle cell. The muscle cell is thus stimulated to contract. This structural picture fits rather well the physiological work done by Sir Bernhard Katz and his colleagues at University College London, who showed that acetylcholine is released in quanta, or packets. Each quantum corresponds to the emptying of one synaptic vesicle.

A final example of eukaryotic secretion is one that we are working on in our laboratory. It involves the secretion of mucocysts and trichocysts by ciliated protozoa such as *Tetrahymena* and *Paramecium.* When one of these single-cell organisms encounters adverse conditions, it discharges mucocysts and trichocysts en masse. In *Tetrahymena* and *Paramecium* the secretory vesicles are not gathered at the apex of the cell but instead lie along precisely defined rows running most of the length of the cell.

Much of the clarification of the pathway by which a secretory product moves from its site of synthesis to the final ma-

FREEZE-FRACTURE ELECTRON MICROGRAPH of parts of several cells in the salivary gland of a rat shows numerous round protuberances and depressions that represent the vesicles, or tiny packages, that carry saliva to the outer membrane of the cells. In the freeze-fracture technique tissue is first frozen and then fractured. When the tissue fractures, it tends to do so at two-layered membranes, so that bodies that project through the membranes appear as protuberances on one fractured surface and as depressions on the other. In this micrograph, which enlarges the structures 32,000 diameters, several structures other than secretory vesicles appear. The large round object at lower right is the nucleus of one of the cells. The convoluted forms at lower left are part of the endoplasmic reticulum, where the substance to be secreted is made. The lozenge-shaped objects are mitochondria.

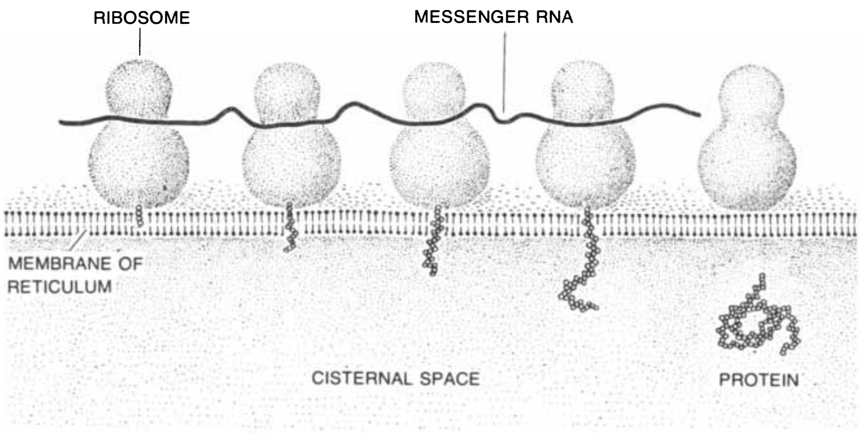


ROUTE OF SECRETION for an acinar cell of the human pancreas is outlined. Such a cell makes precursors of the digestive enzymes of the pancreatic juice. The secretory products are made on the "rough" endoplasmic reticulum. They then pass to the part of the "smooth" endoplasmic reticulum known as the Golgi apparatus, where they are concentrated. Next the secretory products are packaged in membrane-bounded vesicles, or containers, called zymogen granules. The contents of a single granule are discharged into the lumen, or extracellular space, by means of a fusion between the membrane of the granule and the cell membrane.

ture secretory vesicle has resulted from the work of George E. Palade and his collaborators, beginning some 20 years ago when Palade was at the Rockefeller Institute for Medical Research. In those early days of applying electron microscopy to biology Palade and Keith R. Porter realized that the endoplasmic reticulum could be divided into several compartments, including a "rough" endoplasmic reticulum where the particles known as ribosomes are bound to the membranes and a "smooth" endoplasmic reticulum where the membranes are free of ribosomes. It is the ribosomes that synthesize protein. In protein-secreting cells such as the ones found in the pancreas the rough endoplasmic reticulum is often quite extensive.

The secretory proteins of the pancreatic acinar cell are manufactured in this extensive reticulum, which lies in concentric sheets around the basal end of the nucleus. When cells are fractionated to prepare isolated homogeneous cell components, the sheets often disintegrate into small, ribosome-studded, membrane-bounded fragments called microsomes. Colvin M. Redman and David D. Sabatini, working at Rockefeller University, prepared microsomes under conditions where the synthesis of protein could still proceed. They were able to show that radioactively labeled amino acids, which are eventually incorporated into the proteins made by the microsomes, never appeared free in the medium. Instead all labeled protein was injected into the center of the microsome, that is, into the compartment corresponding to the cisternal space. Because of the directional nature of the secretion, Redman and Sabatini call this process vectorial synthesis and transport. In the eukaryotic cell the process may correspond to the passage of secretory material through the plasma membrane of the bacterial cell, since for the bacterial cell the cisternal space is effectively connected to the cell's external environment.

Philip Siekevitz and Lucien Caro and their colleagues, working with Palade, employed two techniques to follow the passage of the newly synthesized secretory proteins through the pancreatic acinar cell. They worked first with cell fractionation and later with electron-microscope autoradiography, labeling with radioactive tritium an amino acid that becomes incorporated into proteins in a process that can be followed at successive times. (In this kind of autoradiography a specimen prepared for electron microscopy is coated with a thin layer of photographic emulsion. In time the tritium decays, leaving a track in the emulsion. When the emulsion is developed and the micrograph is then made, the site of the labeled protein is revealed.) The label was found to move from the rough endoplasmic reticulum to the zymogen granules to the cell's external environment [*see illustration on opposite page*]. In the pancreatic acinar cell, as in many cells, much of the smooth endoplasmic reticulum is organized into a specialized grouping of membranes and larger and smaller vesicles called the Golgi apparatus (after its discoverer, the Italian microscopist Camillo Golgi).



MECHANISM OF SECRETION begins with ribosomes that are attached to the outside of the membrane of which the rough endoplasmic reticulum is made. The ribosomes read the genetic message borne by a molecule of messenger RNA and translate it into precursors of the secretory protein. The products of this action pass from the ribosomes into the cisternal space, or inner cavity, of the reticulum, moving from there to the Golgi apparatus.

Many diagrams of secretory cells show a continuity from the internal space of the rough endoplasmic reticulum through the smooth endoplasmic reticulum to the outside. This interpretation is controversial, since in actuality such a continuity is rarely observed. Instead it seems as if one compartment of the intracellular membrane system is often connected to the next by means of a shuttle wherein small bits of membrane bud off the end of one compartment and fuse with the next compartment. Which compartments are always interconnected and which are intermittently interconnected by means of shuttle vesicles depends on the cell being examined.

The various subdivisions of the endoplasmic reticulum lie in specific zones of the cell: the rough endoplasmic reticulum is at the base, the Golgi zone is just above the nucleus and the zymogen granules are at the apex of the cell. The path the secretory product takes is therefore well defined in space. Part of it is thought to be delineated by microtubules: fibers that may guide or propel the secretory vesicles toward the cell membrane. Drugs that disassemble microtubules (for example colchicine, an inhibitor of cell division) block secretion in many systems, possibly by acting on the transport of secretory vesicles through the cytoplasm.

As the product passes through the Golgi apparatus it is concentrated, condensed and often enzymatically altered. In the electron microscope such packaged products may have a crystalline or dense amorphous appearance. A number of different products may be packaged together in the final secretory granule.

Notwithstanding this rather detailed knowledge of the overall secretory pathway within the eukaryotic cell, the final steps in secretion, which are termed exocytosis (from the Greek for "out of the cell"), have remained enigmatic. One difficulty has been that electron microscopy, which examines only one small part of a cell at a time, rarely catches a part where exocytosis is taking place, so that micrographs clearly showing the final release of the secretory product by the fusion of the granule membrane and the plasma membrane are relatively scarce. Some workers have gone so far as to say that the release occurs instantly through membrane gates that allow the product to leak out without an actual fusion of the inner and outer membranes. This view, however, is not likely to be generally correct, since morphological evidence that has been accumulated from many different secretory systems illustrates the continuity between vesicular membranes and plasma membranes while a product is being expelled. Nonetheless, the sequence of causative events responsible for the fusion of the two membranes during exocytosis has been far from clear.

A second and more serious difficulty has been the question of how two membranes recognize each other as partners in the fusion process. Before the contents of a secretory vesicle are released the vesicle must somehow find its right location—the correct site on the partner membrane of the cell—or fusion and the release of a product will not take place. This necessity was recognized in 1957 by José del Castillo and Katz, who hypothesized that in order to obtain the release of a neurotransmitter at a synapse it was necessary that there be contact between specific sites on the synaptic vesicles and specific sites on the membrane of the axon. In our laboratory we have been working on certain new approaches, which show that the major aspects of the hypothesis put forward by del Castillo and Katz are correct and that at the least they play a major role in the final secretory processes of protozoans. Our laboratory and others are extending these findings to the secretory systems of vertebrates.

These new studies were made possible by radical changes over the past few years in views of the molecular construction of membranes. The basic fabric of most cellular membranes is a double layer of lipid molecules, with hydrophilic (water-soluble) heads and hydrophobic (water-insoluble) tails; the heads face outward and the tails face inward end to end. Excluded from the region of the hydrophobic tails are water and electrically charged molecules [see "A Dynamic Model of Cell Membranes," by Roderick A. Capaldi; SCIENTIFIC AMERICAN, March, 1974].

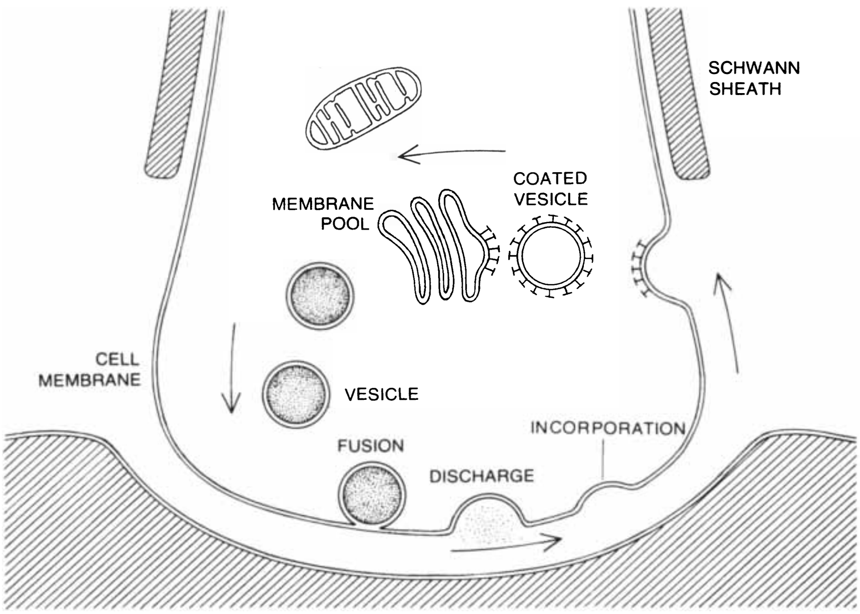
Certain proteins can penetrate this bilayer; other proteins interact with its two outer surfaces. (The penetrating proteins are called integral proteins and the interacting ones peripheral proteins.) What has recently been perceived is the dynamic nature of the membrane and the differences in its local composition. The proteins actually float through the lipid matrix, as has been elegantly shown by the work of David Frye and Michael Edidin at Johns Hopkins University. Moreover, certain proteins can be made to aggregate at specific places in the membrane. This new emphasis has been described by S. J. Singer of the University of California at San Diego and Garth Nicolson of the Salk Institute for Biological Studies as "the fluid-mosaic model of the cell membrane."

The fluid-mosaic model has received confirmation through the development of a totally new way of examining the local internal structure of a membrane. It is the freeze-fracture, freeze-etch procedure, where a cellular membrane is rapidly frozen to the temperature of liquid nitrogen. (Rapid freezing prevents the formation of crystalline ice, which can distort the true structure.) In this state the membrane, given a more or less gentle tap, falls apart along the plane of least resistance, which is the central plane of the lipid bilayer, as was first demonstrated by Daniel Branton, who was then at the University of California at Berkeley. In this way two half-membranes, each containing half of the lipid bilayer, can be exposed [*see illustration on opposite page*]. One side of each half-membrane is the true membrane surface; the other is the completely new view of the interior of the membrane and is termed the fracture face. The half-membrane nearest the cytoplasm of the cell is labeled the *P* (for protoplasmic) half and the other half-membrane is the *E* (for exterior) half.

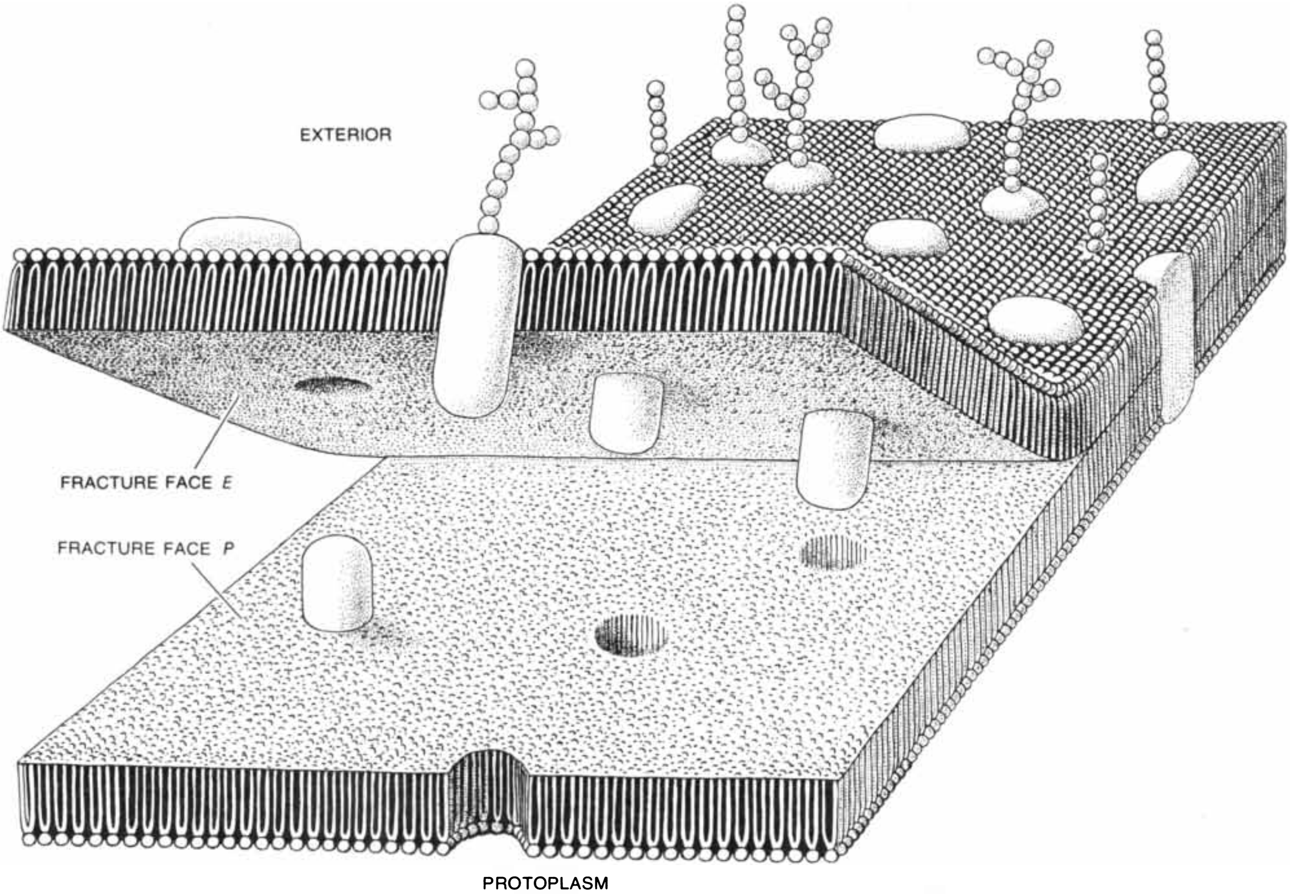
When the fracture encounters an integral protein or a complex of lipid and protein, it does not go through it but passes around it on one side or the other. The background lipid is smooth, so that the protein regions stand out as particles and depressions. The half-membranes produced by fracturing fit together like pieces of a jigsaw puzzle: for each particle on one half-membrane a corresponding depression is found on the other half-membrane. Since the actual fracture face is extremely delicate, the position of particles and depressions is usually ascertained by examining a platinum-and-carbon-shadowed replica of the fracture face rather than by direct observation of the tissue itself.

The secretory product in *Tetrahymena* is apparently produced in the usual manner by vectorial synthesis into the cisternal space of the endoplasmic reticulum. Large vesicles containing the product move from the interior of the cell out toward the plasma membrane. There, at defined sites, the now mature, elongated, saclike vesicles come to rest. At this stage the content has become crystalline and the vesicle is known as a mature mucocyst.

When we examined the cell membrane of *Tetrahymena* by means of the freeze-fracture technique, we found a remarkable array of particles just above the places where mucocysts were resting. Each array consists of a ring of about nine small particles (15 nanometers in diameter) surrounding a central particle. We call this structure the fusion rosette. It corresponds quite clearly to the hypothetical site in the cell membrane postulated by del Castillo and Katz.



FATE OF MEMBRANE of a synaptic vesicle is traced. A vesicle, which originates in the cell's membrane pool, carries a quantity of material that transmits the impulse of a nerve cell to an adjacent nerve or muscle cell. Each vesicle fuses with the cell membrane at a specific site and discharges its contents. The vesicle membrane then becomes part of the cell membrane. Elsewhere, near the Schwann sheath, equal amounts of membrane are retrieved by coated vesicles that arise from the cell membrane and return to the membrane pool.



FREEZE-FRACTURE TECHNIQUE is depicted as it is employed to elucidate the structure of a membrane. The intact membrane appears at upper right. It consists of a double layer of lipid molecules, which have water-soluble heads and water-insoluble tails. The heads face outward and the tails inward, end to end. Certain proteins, shown here as large elliptical objects, can either penetrate the membrane or interact with its two outer surfaces. Proteins and lipids facing the exterior surface are often combined with sugars, shown as straight and branched linked chains. When the membrane is frozen and fractured, it comes apart at the plane between the lipid tails. The penetrating proteins are not cleaved and hence appear on a fracture face as protuberances and depressions.

Similar sites have been found in *Paramecium* in connection with the discharge of trichocysts. One advantage of the work with *Paramecium* is that several laboratories have found mutant versions that either lack trichocysts or only discharge them when the organisms have been grown at certain temperatures. Janine Beisson, Marcelle Lefort-Tran and I, working at the Center of Molecular Genetics in France, examined such mutants with the freeze-fracture technique. We found that in them the fusion-rosette particles were absent or much reduced. This is the first direct demonstration of a genetic basis for a functioning particle array in the secretory process. We concluded that the fusion rosette is an obligatory component of the fusion event.

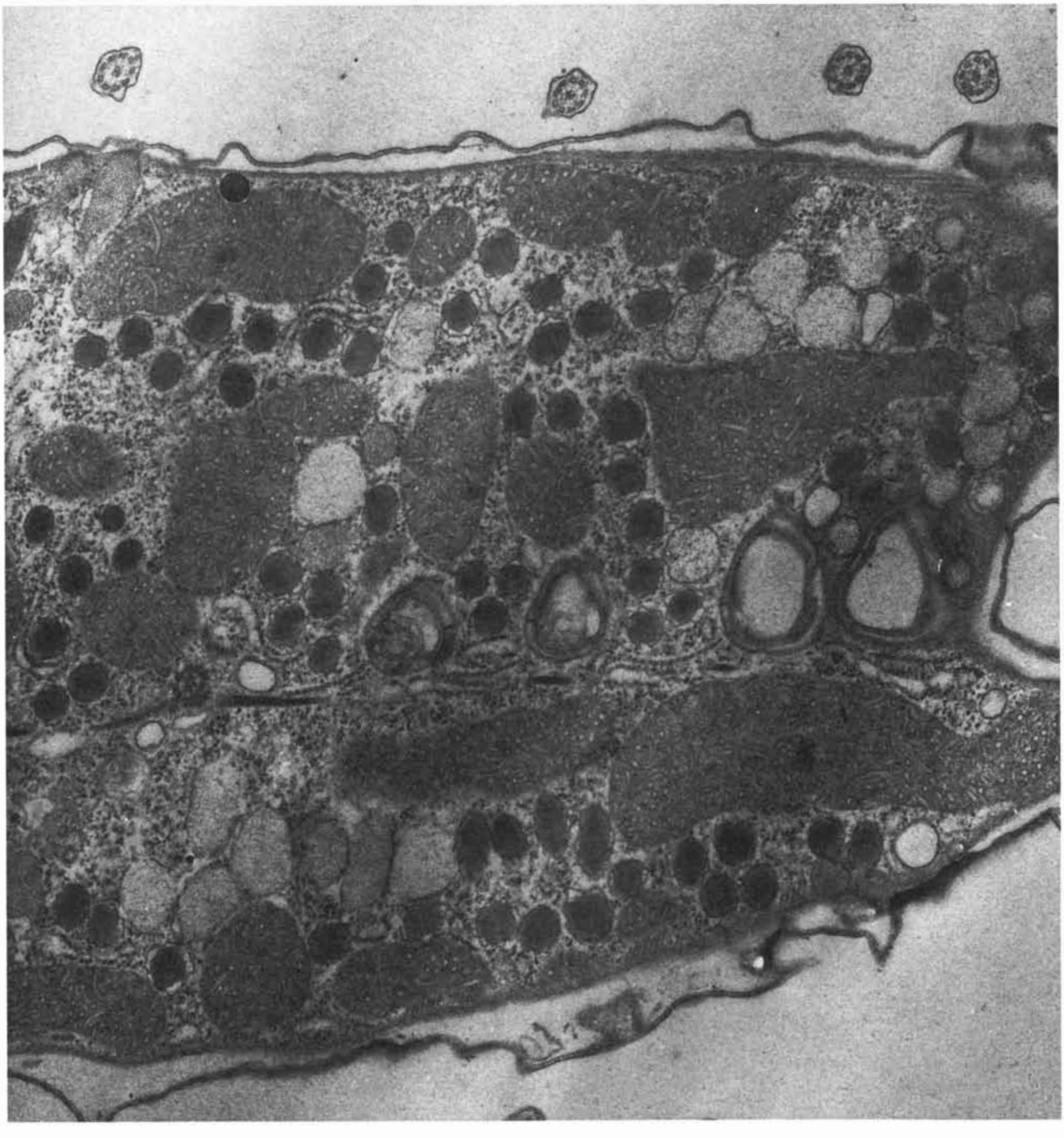
That conclusion is supported by recent work in our laboratory, where we have treated *Tetrahymena* with the local anesthetic dibucaine in a procedure first described by Guy A. Thompson, Jr., and his colleagues at the University of Texas at Austin. We found that dibucaine causes the discharge of all mature mucocysts and the simultaneous disappearance of all rosettes. Mature mucocysts reappear only after a recovery period of several hours. By then the fusion rosettes have also reappeared.

Although fusion rosettes are found in many protozoans, the exact form of the array may change considerably in different secretory cells, partly because secretory granules come in many different sizes. In the cell membranes of vertebrate animals the corresponding array is often more difficult to find. A few apparently similar arrays have been found in membranes of vertebrates and invertebrates, but it is not clear whether they correspond to the fusion rosettes in function.

The presence of fusion rosettes in the cell membrane of *Tetrahymena* led us to search for possible matching sites in the membrane of the mucocyst. Freeze-fracture images of mucocysts located in their "resting" position close to the plasma membrane reveal the presence of an annulus, or ring, at the anterior end of the organelle. It consists of five or six rows of particles with a diameter of 10 nanometers. The extreme tip of the mucocyst is bare of particles over a circular area about 60 nanometers in diameter. This clear area approximately circumscribes the fusion rosette. The annulus is absent in mucocysts that are not close to the plasma membrane. It follows that the formation of the annulus is one of the events taking place while the mucocyst matures.

Matching arrays of particles on fusing membranes during exocytosis are reminiscent of matching clusters of particles that are found in special locations when two cells are coupled electrically or metabolically. These clusters form the gap junction, a region of close contact between cells. The junction penetrates the hydrophobic interiors of the adjoining membranes, so that ions and small water-soluble molecules can pass readily from one cell to the next. Permeability changes, which allow ions or small molecules to enter a cell at a particular site, are associated with exocytosis in many systems.

In *Tetrahymena* one of the first indications of fusion is a change in the content of a mucocyst from a crystalline state to an amorphous one. This change is followed by an abrupt change in the appearance of the mucocyst from an elongated sac to a sphere, which has a diameter of 700 nanometers. Presumably changes in permeability, together with a solubilization of the mucocyst's content, lead to an inflow of water, so that the organelle swells and changes shape. Solubilization, continued expansion and the release of contents are expedited by the puncture of the membrane. What is seen in conventional micrographs is the end result, which is that the membrane of the secretory vesicle is joined to the membrane of the cell. The site looks something like an omega (Ω), which is upside down (℧) if the micrograph is oriented so that the point of junction is at the top. The usual electron-microscope procedures do not show



SINGLE-CELL ORGANISM TETRAHYMENA appears in an electron micrograph at an enlargement of 24,000 diameters. When the organism is stimulated, it discharges to the exterior the secretory contents of the membrane-enclosed organelles called mucocysts. In this cross section of a cell the four circular structures in the clear space at the top are cilia. Inside the cell, directly below the second cilium from the right, is a discharging mucocyst; it is spherical and light gray. To its right and slightly below it is a group of three dark circular structures; they are undischarged mucocysts. The black circle at upper left is an artifact.

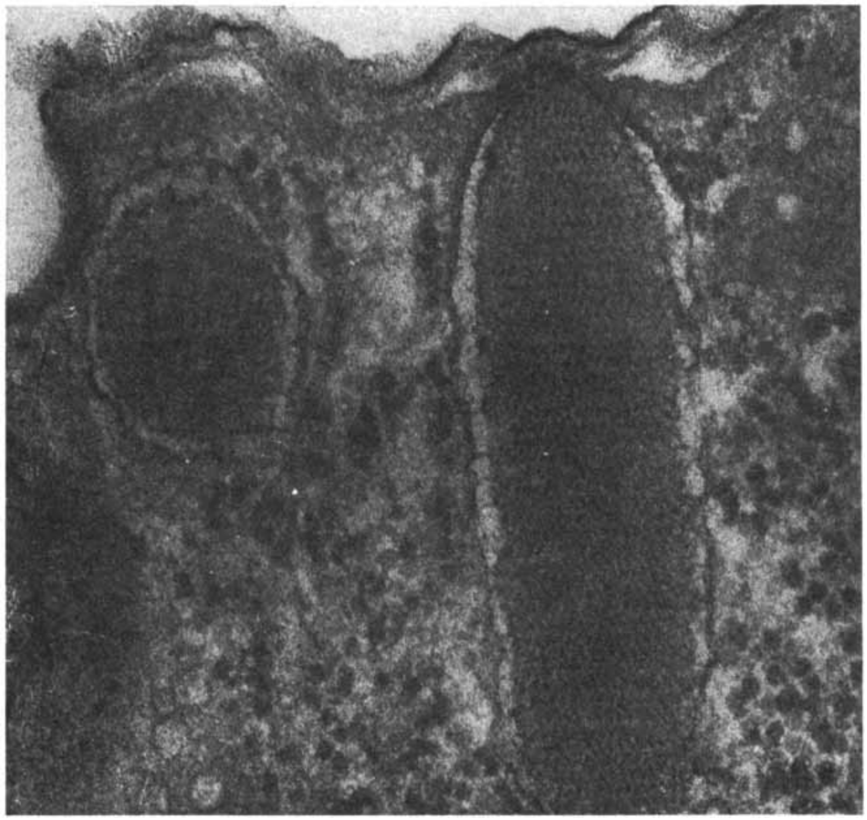
what rearrangements occur within the membranes.

From freeze-fracture images of the *P* and *E* faces of the plasma membrane of *Tetrahymena* one can reconstruct many of the key details of the membrane-fusion events that take place between the time when the secretory vesicle leaves the resting state and the time when the omega shape appears. The reader will remember the geometrical setting: the elongated mature mucocyst, with its annulus of particles, underlies a fusion rosette of the plasma membrane. The first sign of fusion between the two partner membranes is the appearance of a depression, the fusion pocket, in the center of the rosette [*see illustration on next page*]. As fusion progresses, the bigger particles of the rosette separate at the edge of the fusion pocket, which in turn widens and deepens.

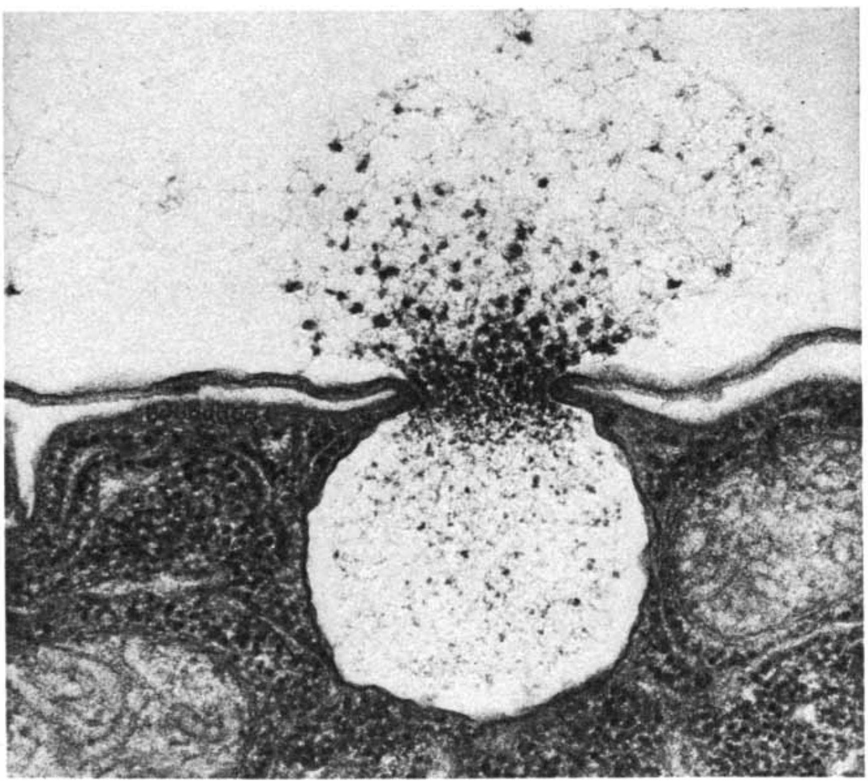
At a certain stage the particles from the annulus of fracture face *P* of the mucocyst membrane become visible at the inner edge of the collar of the fusion pocket. This event shows that fusion between the two *P* halves of the partner membranes is completed, since they can now be seen in the same fracture plane. The result can also be observed in micrographs of fracture face *E* of the plasma membrane, although it is somewhat less spectacular, since here all that is involved is a hole to the exterior.

When the partner membranes become continuous, the fracture runs from the plasma membrane to the mucocyst membrane, clearly outlining the now spherical vesicle and identifying the transition zone at the annulus. Since the final diameter of the opened fusion pocket never exceeds 250 nanometers, it is likely that the annulus of particles acts both as a reinforcing ring, hindering any tendency of the membranes to rupture as the content of the mucocyst is explosively discharged, and as a stabilizing point around which the membrane of the mucocyst flows in and becomes part of the plasma membrane.

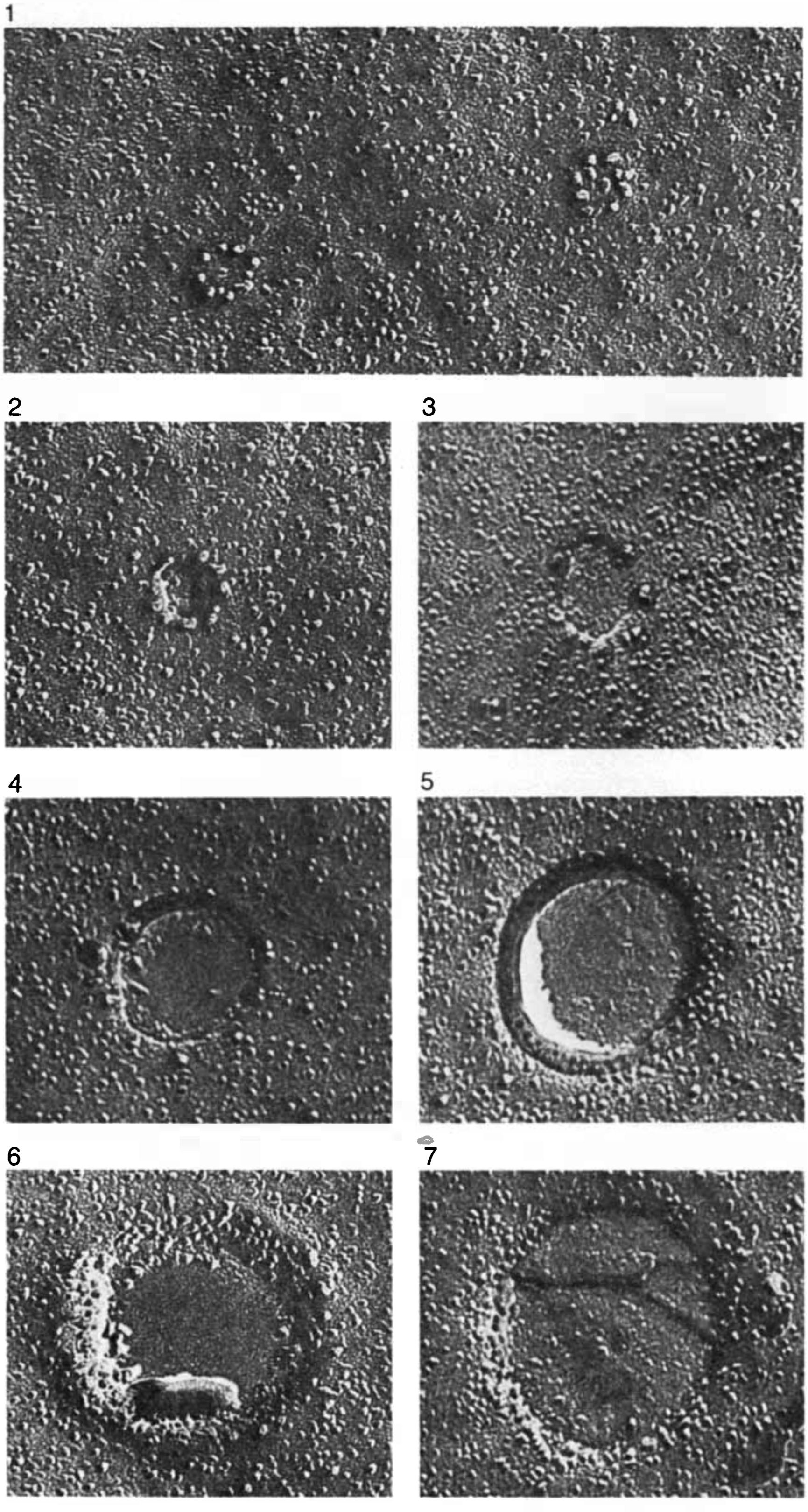
Since it is known in a general way what the images seen in freeze-fracture micrographs correspond to biochemically, it is possible to construct molecular models of the fusion-release sequence [*see illustration on page 37*]. They provide an indication of the enzymatic events that may be progressing during fusion and further reasons why particle arrays may be necessary. For example, the actual zone of fusion is limited initially to the smooth 60-nanometer area of the fusion rosette on the one hand and the similarly smooth area within the



MATURE MUCOCYST of *Tetrahymena* has become elongated and has taken up a position near the external membrane of the cell, which is the wavy line at top. The mucocyst is ready to discharge its crystalline secretory content by fusing with the cell membrane at a specific site. Enlargement of this electron micrograph is approximately 103,000 diameters.



DISCHARGE OF MUCOCYST is evident in this electron micrograph, as is the fusion of the mucocyst membrane and the cell membrane. The content expands and empties to the outside of the cell. The mucocyst has become a sphere with a diameter of 700 nanometers.



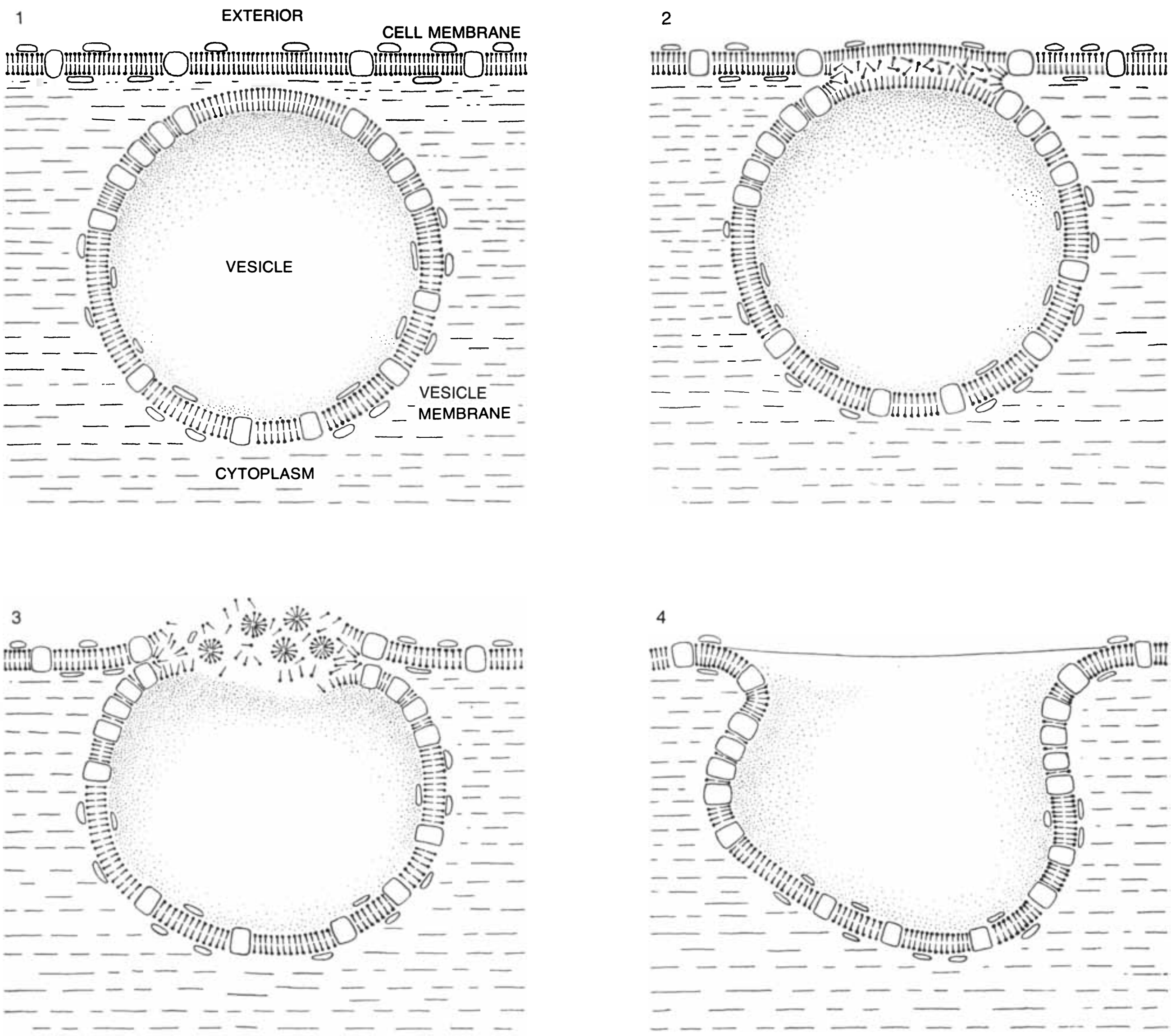
EVENTS OF FUSION are seen in a series of electron micrographs of *Tetrahymena* cell membranes at successive stages in fusion. The enlargement is 114,000 diameters. The membranes were prepared by the freeze-fracture technique. Each micrograph shows the half of the membrane that faces the cytoplasm of the cell. At the outset fusion rosettes appear (*1*) directly above the places where mature mucocysts are resting. The first sign of fusion between the mucocyst membrane and the cell membrane is the appearance of a pocket in the center of a rosette (*2*). As fusion progresses the bigger particles of the rosette separate at the edge of the fusion pocket (*3*), which grows wider and deeper (*4–7*). When particles from the annulus, or ring, of the mucocyst membrane appear, the membrane fusion is complete.

annulus of the mucocyst membrane on the other. Therefore mainly lipids are involved. Jack A. Lucy of the University of London suggests that fusion usually occurs in areas of perturbed lipid bilayers, where the fluidity of the lipids is increased. One means of increasing their fluidity is removal of the hydrophobic tails of the lipids by the enzyme phospholipase. In *Tetrahymena* the rings of particles may define the area of fusion by limiting the spread of the change in fluidity.

Relatively little is understood about the trigger mechanism that induces the fusion of membranes and the release of a vesicle's content. (The mechanism has been termed the stimulus-secretion coupling.) The basic mechanism probably involves a temporary increase in the number of calcium ions in the cytoplasm of the cell. When Ca^{++} is increased experimentally in the mast cell of the rat, the cell releases its secretory product, histamine. In *Tetrahymena* dibucaine might induce a mass discharge of mucocysts by a similar increase in the concentration of calcium ions in the cytoplasm.

An unexpected dividend from our studies of exocytosis is a widening understanding of what happens to the vesicular membrane after fusion. The evidence that we and many other workers have obtained suggests that after fusion the vesicular membrane flows into the cell membrane. In *Tetrahymena* there is no sign that vesicles pinch off and move back into the cell after fusion; instead one finds at the sites of fusion the small, shallow profiles that would be expected if most of the mucocyst membrane had become incorporated into the cell membrane. John E. Heuser and Thomas S. Reese of the National Institutes of Health have found that depletion of the synaptic vesicles of a nerve terminal by prolonged stimulation leads to a significant expansion of the nerve-cell membrane, which would be expected after massive incorporation.

What is the significance of this incorporation? It doubtless varies according to the type of cell. In the synapse the membrane is usually retracted again but at a site different from the point of fusion, so that no net increase of the cell membrane is registered under normal conditions. The membrane molecules taken up in retraction, however, are not necessarily the ones incorporated at fusion, so that fusion may be a mechanism for renewing the cell membrane. Whenever retraction does not keep pace with incorporation, membrane fusion



MOLECULAR MODEL of secretory event begins (*1*) with a mature secretory vesicle situated below the membrane of the cell. At a precise site, which in freeze-fracture micrographs would show a fusion rosette on the cell membrane and a corresponding annulus on the vesicle membrane, the two membranes touch (*2*). Lipids in each membrane are moved aside, apparently because an enzyme has made them more fluid. Secretory vesicle discharges its contents (*3*). Remaining vesicle membrane fuses with cell membrane (*4*).

can serve as a mechanism promoting the growth or differentiation of the cell membrane.

If salt is added to the drinking water of a duckling, certain of the bird's secretory cells show a hundredfold increase in the volume of their cell membranes within a short time. Russell J. Barnett and his co-workers at Yale University School of Medicine have ascertained that a cell-membrane enzyme (Na^+-K^+ ATPase) made during this process is detectable first on the Golgi apparatus, then in small vesicles and then as part of the newly expanding cell membrane. They conclude that the enzyme-bearing vesicles pinch off from the Golgi apparatus and deliver their membrane material to the cell membrane as it grows.

The acrosomal discharge of a sperm cell is another instance where membrane fusion is followed by rapid membrane expansion. No one yet knows how much of the membrane surrounding the newly extending acrosome actually derives from the membrane of the acrosomal vesicle and how much must be assembled anew from molecular pools. In many other cases where cell projections with special properties grow (an example being the formation of a pollen tube in a plant) vesicular fusion may be an important contributor to the expansion of the cell membrane or to the composition of enzymes.

Tetrahymena is a single-cell organism that divides by binary fission. In every cell cycle (occupying about four hours) it has to generate enough new cell membrane for a daughter cell. Moreover, the new membrane must come to possess a duplicate of the original membrane's pattern, with all arrays such as fusion rosettes re-formed in their exact meridional positions. Our present working hypothesis is that much of the new membrane could be contributed by fusing mucocyst membranes, provided that each rosette found after a cell has divided represents one fusion event during one cell cycle. A distinct advantage of this hypothesis that new membrane is added by vesicular fusion is the localized growth it entails, since localized growth could be the means by which the meridional template is normally maintained as a new cell is formed.

QUARKS WITH COLOR AND FLAVOR

The particles called quarks may be truly elementary. Their "colors" explain why they cannot be isolated; their "flavors" distinguish four basic kinds, including one that has the property called charm

by Sheldon Lee Glashow

Atomos, the Greek root of "atom," means indivisible, and it was once thought that atoms were the ultimate, indivisible constituents of matter, that is, they were regarded as elementary particles. One of the principal achievements of physics in the 20th century has been the revelation that the atom is not indivisible or elementary at all but has a complex structure. In 1911 Ernest Rutherford showed that the atom consists of a small, dense nucleus surrounded by a cloud of electrons. It was subsequently revealed that the nucleus itself can be broken down into discrete particles, the protons and neutrons, and since then a great many related particles have been identified. During the past decade it has become apparent that those particles too are complex rather than elementary. They are now thought to be made up of the simpler things called quarks. A solitary quark has never been observed, in spite of many attempts to isolate one. Nonetheless, there are excellent grounds for believing they do exist. More important, quarks may be the last in the long series of progressively finer structures. They seem to be truly elementary.

When the quark hypothesis was first proposed more than 10 years ago, there were supposed to be three kinds of quark. The revised version of the theory I shall describe here requires 12 kinds. In the whimsical terminology that has evolved for the discussion of quarks they are said to come in four flavors, and each flavor is said to come in three colors. ("Flavor" and "color" are, of course, arbitrary labels; they have no relation to the usual meanings of those words.) One of the quark flavors is distinguished by the property called charm (another arbitrary term). The concept of charm was suggested in 1964, but until last year it had remained an untested conjecture. Several recent experimental findings, including the discovery last fall of the particles called *J* or psi, can be interpreted as supporting the charm hypothesis.

The basic notion that some subatomic particles are made of quarks has gained widespread acceptance, even in the absence of direct observational evidence. The more elaborate theory incorporating color and charm remains much more speculative. The views presented here are my own, and they are far from being accepted dogma. On the other hand, a growing body of evidence argues that these novel concepts must play some part in the description of nature. They help to bring together many seemingly unrelated theoretical developments of the past 15 years to form an elegant picture of the structure of matter. Indeed, quarks are at once the most rewarding and the most mystifying creation of modern particle physics. They are remarkably successful in explaining the structure of subatomic particles, but we cannot yet understand why they should be so successful.

The particles thought to be made up of quarks form the class called the hadrons. They are the only particles that interact through the "strong" force. Included are the protons and neutrons, and indeed it is the strong force that binds protons and neutrons together to form atomic nuclei. The strong force is also responsible for the rapid decay of many hadrons.

Another class of particles, defined in distinction to the hadrons, are the leptons. There are just four of them: the electron and the electron neutrino and the muon and the muon neutrino (and their four antiparticles). The leptons are not subject to the strong force. Because the electron and the muon bear an electric charge, they "feel" the electromagnetic force, which is roughly 100 times weaker than the strong force. The two kinds of neutrino, which have no electric charge, feel neither the strong force nor the electromagnetic force, but interact solely through a third kind of force, weaker by several orders of magnitude and called the weak force. The strong force, the electromagnetic force and the weak force, together with gravitation, are believed to account for all interactions of matter.

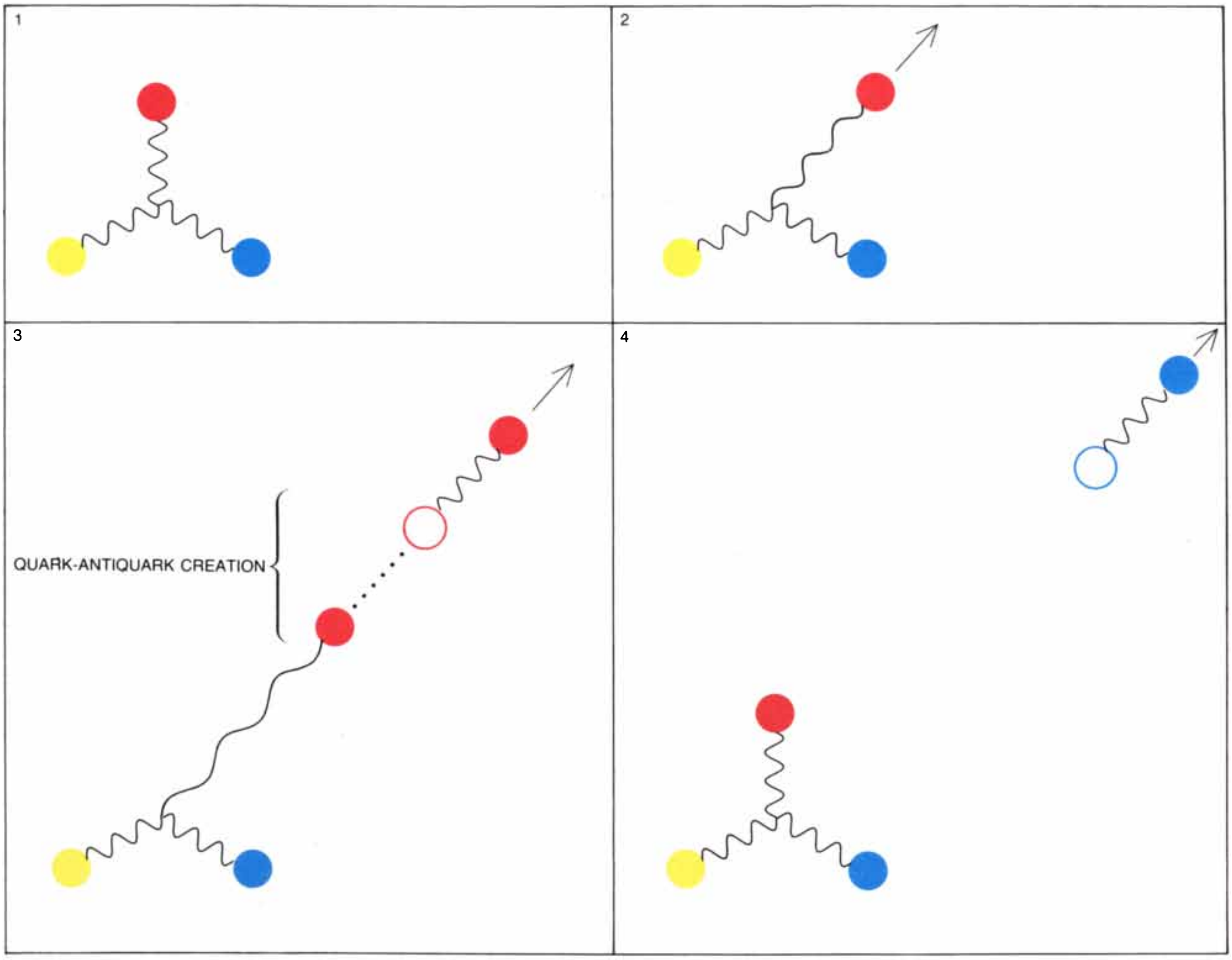
The leptons give every indication of being elementary particles. The electron, for example, behaves as a point charge, and even when it is probed at the energies of the largest particle accelerators, no internal structure can be detected. The hadrons, on the other hand, seem complex. They have a measurable size: about 10^{-13} centimeter. Moreover, there are hundreds of them, all but a handful discovered in the past 25 years. Finally, all the hadrons, with the significant exception of the proton and the antiproton, are unstable in isolation. They decay into stable particles such as protons, electrons, neutrinos or photons. (The photon, which is the carrier of the electromagnetic force, is in a category apart; it is neither a lepton nor a hadron.)

The hadrons are subdivided into three families: baryons, antibaryons and mesons. The baryons include the proton and the neutron; the mesons include such particles as the pion. Baryons can be neither created nor destroyed except as pairs of baryons and antibaryons. This principle defines a conservation law, and it can be treated most conveniently in the system of bookkeeping that assigns simple numerical values, called quantum numbers, to conserved properties. In this case the quantum number is called baryon number. For baryons it is +1, for antibaryons −1 and for mesons 0. The conservation of baryon number then reduces to the rule that in any interaction the sum of the baryon numbers cannot change.

Baryon number provides a means of distinguishing baryons from mesons, but it is an artificial means, and it tells us nothing about the properties of the two kinds of particle. A more meaningful distinction can be established by examining another quantum number, spin angular momentum.

Under the rules of quantum mechanics a particle or a system of particles can assume only certain specified states of rotation, and hence can have only discrete values of angular momentum. The angular momentum is measured in units of $h/2\pi$, where h is Planck's constant, equal to about 6.6×10^{-27} erg-second. Baryons are particles with a spin angular momentum measured in half-integral units, that is, with values of half an odd integer, such as 1/2 or 3/2. Mesons have integral values of spin angular momentum, such as 0 or 1.

The difference in spin angular momentum has important consequences for the behavior of the two kinds of hadron. Particles with integral spin are said to obey Bose-Einstein statistics (and are therefore called bosons). Those with half-integral spin obey Fermi-Dirac statistics (and are called fermions). In this context "statistics" refers to the behavior of a population of identical particles. Those that obey Bose-Einstein statistics can be brought together without restriction; an unlimited number of pions, for example, can occupy the same state. The Fermi-Dirac statistics, on the other hand, require that no two particles within a given system have the same energy and be identical in all their quantum numbers. This statement is equivalent to the exclusion principle formulated in 1925 by Wolfgang Pauli. He applied it in particular to electrons, which have a spin of 1/2 and are therefore fermions. It requires that each energy level in an



CONFINEMENT OF QUARKS in known subnuclear particles can be explained if the quarks are assumed to come in three varieties, arbitrarily designated by colors. A baryon (such as a proton or a neutron) is a bound system of three quarks, including one of each of the three colors (*1*), so that the baryon as a whole is colorless. The quarks are bound together by the strong force, which also holds together atomic nuclei. The force between colored quarks, however, differs in character from that between colorless composite particles: it does not diminish with distance but remains constant. As a result, when a quark is separated from a baryon (*2*), the potential energy of the system rapidly increases and would reach enormous values if another process did not intervene: from the potential energy a quark and an antiquark are created (*3*). The new quark restores the baryon to its original form, and the antiquark (*open circle*) adheres to the dislodged quark, forming another kind of particle, a meson (*4*). At any one moment the quark and the antiquark in a meson are the same color, but the three colors are equally represented. Because of the nature of the strong force, solitary quarks cannot be observed; any attempt to isolate a quark merely results in the creation of a new hadron or hadrons.

atom contain only two electrons, with their spins aligned in opposite directions.

One of the clues to the complex nature of the hadrons is that there are so many of them. Much of the endeavor to understand them has consisted of a search for some ordering principle that would make sense of the multitude.

The hadrons were first organized into small families of particles called charge multiplets or isotopic-spin multiplets; each multiplet consists of particles that have approximately the same mass and are identical in all their other properties except electric charge. The multiplets have one, two, three or four members. The proton and the neutron compose a multiplet of two (a doublet); both are considered to be manifestations of a single state of matter, the nucleon, with an average mass equivalent to an energy of .939 GeV (billion electron volts). The pion is a triplet with an average mass of .137 GeV and three charge states: +1, 0 and −1. In the strong interactions the members of a multiplet are all equivalent, since electric charge plays no role in the strong interactions.

In 1962 a grander order was revealed when the charge multiplets were organized into "supermultiplets" that revealed relations between particles that differ in other properties in addition to charge. The creation of the supermultiplets was proposed independently by Murray Gell-Mann of the California Institute of Technology and by Yuval Ne'eman of Tel-Aviv University [see "Strongly Interacting Particles," by Geoffrey F. Chew, Murray Gell-Mann and Arthur H. Rosenfeld; SCIENTIFIC AMERICAN, February, 1964]. The introduction of the new system led directly to the quark hypothesis.

The grouping of the hadrons into supermultiplets involves eight quantum numbers and has been referred to as the "eightfold way." Its mathematical basis is a branch of group theory invented in

QUARKS					LEPTONS			
SYMBOL	MASS (GeV)	ELECTRIC CHARGE	STRANGENESS	CHARM	NAME	SYMBOL	MASS (GeV)	ELECTRIC CHARGE
d	.338	−1/3	0	0	ELECTRON	e^-	.0005	−1
u	.336	+2/3	0	0	ELECTRON NEUTRINO	ν_e	0	0
s	.540	−1/3	−1	0	MUON NEUTRINO	ν_μ	0	0
c	1.5	+2/3	0	+1	MUON	μ^-	.105	−1

QUARKS AND LEPTONS, the two kinds of particle that seem to be elementary, exhibit an apparent symmetry. The quarks are much more massive than the leptons, and they have fractional charges instead of integral ones, but both groups consist of two pairs of particles (indicated by colored rectangles). Either member of a pair is readily transformed into the other by the weak interaction. All ordinary matter can be constructed of just the *d* and *u* quarks and the electron and electron neutrino; the muon, the muon neutrino and the *c* and *s* quarks, which display the properties of strangeness and charm respectively, are important only in high-energy physics. Each kind, or flavor, of quark comes in three colors.

	NAME	SYMBOL	MASS (GeV)	CHARGE STATES	BARYON NUMBER	SPIN	ISOTOPIC SPIN	STRANGENESS
BARYONS	NUCLEON	N	.939	0, +1	+1	1/2	1/2	0
BARYONS	LAMBDA	Λ	1.115	0	+1	1/2	0	−1
BARYONS	OMEGA	Ω	1.672	−1	+1	3/2	0	−2
MESONS	PION	π	.139	−1, 0, +1	0	0	1	0
MESONS	K	K	.496	0, +1	0	0	1/2	+1
MESONS	PHI	φ	1.019	0	0	1	0	0
MESONS	J	J	3.095	0	0	1	0	0

HADRONS form the class of particles thought to be constructed of quarks. They are divided into baryons, made of three quarks, and mesons, made of a quark and an antiquark. (Antibaryons consist of three antiquarks.) The groups are distinguished by baryon number and by spin angular momentum, which has half-integral values for baryons and integral values for mesons. Each line in the table represents a multiplet of particles identical in all properties except electric charge, provided that small differences in mass are ignored. Isotopic spin is a function of the number of particles in a multiplet, and strangeness measures distribution of electric charge among them. Only a few representative hadrons are shown.

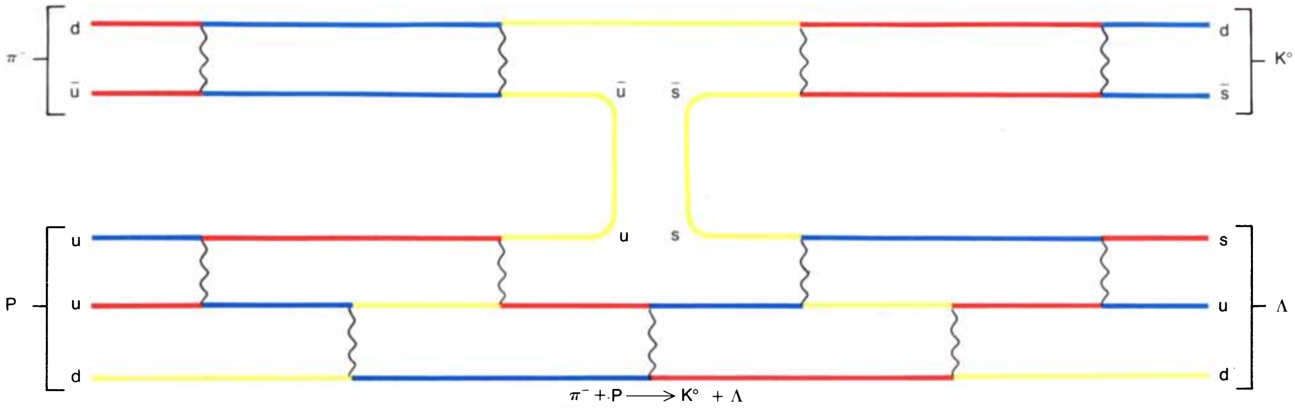
the 19th century by the Norwegian mathematician Sophus Lie. The Lie group that generates the eightfold way is called SU(3), which stands for the special unitary group of matrices of size 3 × 3. The theory requires that all hadrons belong to families corresponding to representations of the group SU(3). The families can have one, three, six, eight, 10 or more members. If the eightfold way were an exact theory, all the members of a given family would have the same mass. The eightfold way is only an approximation, however, and within the families there are significant differences in mass.

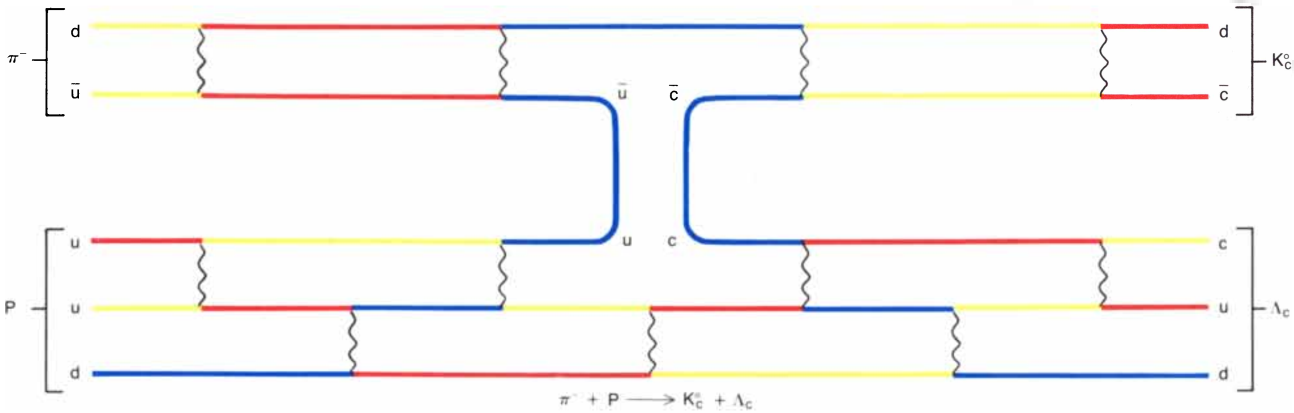
The construction of the eightfold way begins with the classification of the hadrons into broad families sharing a common value of spin angular momentum. Each family of particles with identical spin is then depicted by plotting the distribution of two more quantum numbers: isotopic spin and strangeness.

Isotopic spin has nothing to do with the spin of a particle; it was given its name because it shares certain algebraic properties with the spin quantum number. It is a measure of the number of particles in a multiplet, and it is calculated according to the formula that the number of particles in the multiplet is one more than twice the isotopic spin. Thus the nucleon (a doublet) has an isotopic spin of 1/2; for the pion triplet the isotopic spin is 1.

Strangeness is a quantum number introduced to describe certain hadrons first observed in the 1950's and called strange particles because of their anomalously long lifetimes. They generally decay in from 10^{-10} to 10^{-7} second. Although that is a brief interval by everyday standards, it is much longer than the lifetime of 10^{-23} second characteristic of many other hadrons.

Like isotopic spin, strangeness depends on the properties of the multiplet, but it measures the distribution of charge among the particles rather than their number. The strangeness quantum number is equal to twice the average charge (the sum of the charges divided by the number of particles in a multiplet) minus the baryon number. By this contrivance it is made to vanish for all hadrons except the strange ones. The triplet of pions, for example, has an average charge of 0 and a baryon number of 0; its strangeness is therefore also 0. The nucleon doublet has an average charge of + 1/2 and a baryon number of +1, so that those particles too have a strangeness of 0. On the other hand, the lambda particle is a neutral baryon that forms a family of one (a singlet). Its aver-





STRANGE AND CHARMED PARTICLES should be created in interactions of ordinary matter. The interactions are displayed here as intersections of lines representing quarks and other particles. Within a hadron the quarks continually exchange massless particles called gluons (*wavy lines*), the carriers of the strong force. By emitting a gluon a quark changes its color but not its flavor. Strange particles can be created (*top*) when a *u* quark in a proton and a $\bar{u}$ antiquark in a pion annihilate each other and give rise to an *s* quark and an $\bar{s}$ antiquark. Products are a *K* meson and a lambda baryon. At higher energy the same annihilation could yield a $\bar{c}$ quark and a $\bar{c}$ antiquark (*bottom*). This process, which has not been observed, would yield a charmed meson and a charmed baryon.

age charge of 0 and its baryon number of +1 give it a strangeness of −1.

On a graph that plots electric charge against strangeness the hadrons form orderly arrays. The mesons with a spin angular momentum of 0 compose an octet and a singlet; the octet is represented graphically as a hexagon with a particle at each vertex and two particles in the center, and the singlet is represented as a point at the origin. The mesons with a spin of 1 form an identical representation, and so do the baryons with a spin of 1/2. Finally, the baryons with a spin of 3/2 form a decimet (a group of 10) that can be graphed as a large triangle made up of a singlet, a doublet, a triplet and a quartet. The eightfold way was initially greeted with some skepticism, but the discovery in 1964 of the negatively charged omega particle, the predicted singlet in the baryon decimet, made converts of us all.

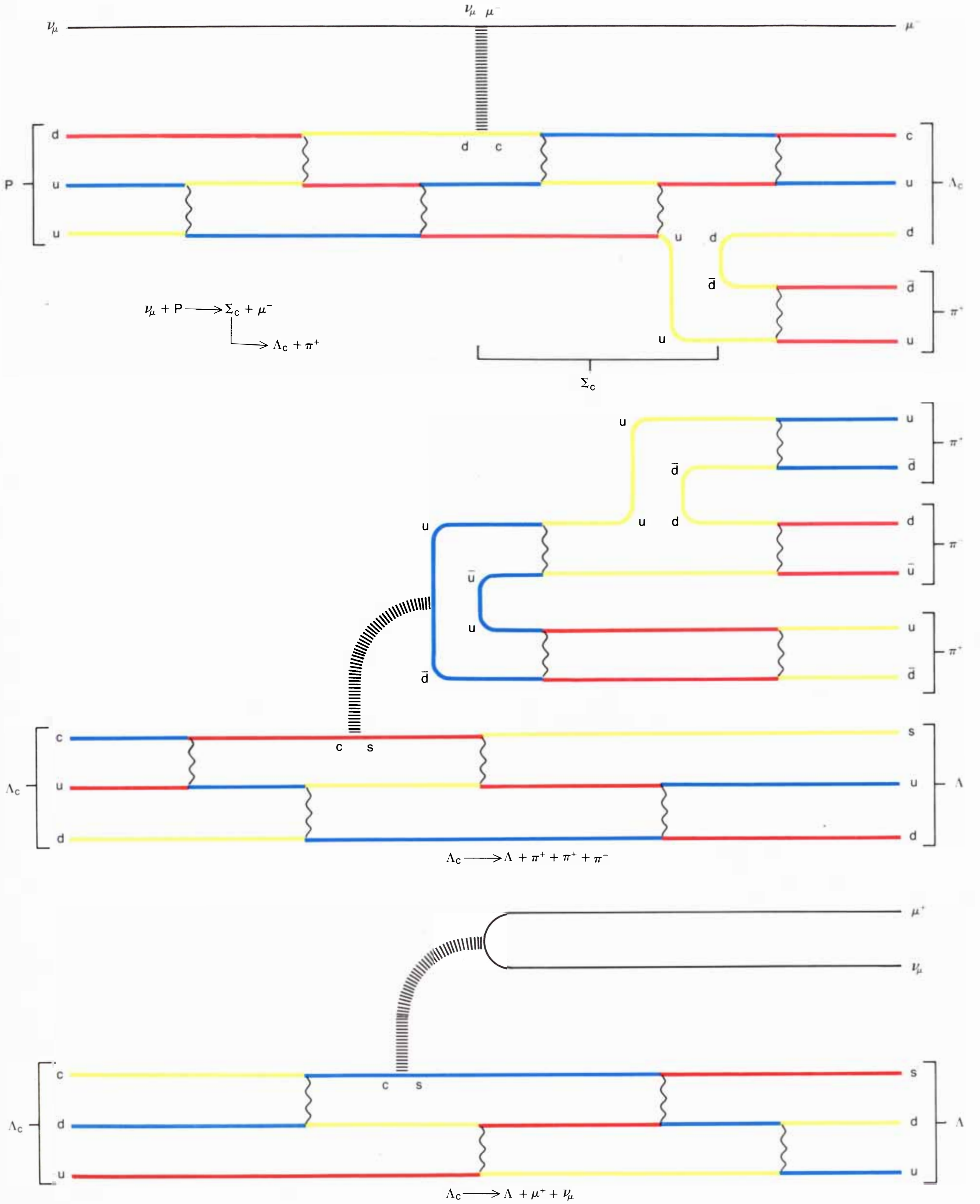
The regularity and economy of the supermultiplets are aesthetically satisfying, but they are also somewhat mystifying. The known hadrons do fit into such families, without exception. Mesons come only in families of one and eight, and baryons come only in families of one, eight and 10. The singlet, octet and decimet, however, are only a few of many possible representations of SU(3). Families of three particles or six particles are entirely plausible, but they are not observed. Indeed, the variety of possible families is in principle infinite. Why, then, do only three representations appear in nature? It early became apparent that the eightfold way is in some approximate sense true, but it was also plain from the start that there is more to the story.

In 1963 an explanation was proposed independently by Gell-Mann and by George Zweig, also of Cal Tech. They perceived that the unexpected regularities could be understood if all hadrons were constructed from more fundamental constituents, which Gell-Mann named quarks. The quarks were to belong to the simplest nontrivial family of the eightfold way: a family of three. (There is also, of course, another family of three antiquarks.)

The quarks are required to have rather peculiar properties. Principal among these is their electric charge. All observed particles, without exception, bear integer multiples of the electron's charge; quarks, however, must have charges that are fractions of the electron's charge. Gell-Mann designated the three quarks *u*, *d* and *s*, for the arbitrary labels "up," "down" and "sideways."

The mechanics of the original quark model are completely specified by three simple rules. Mesons are invariably made of one quark and one antiquark. Baryons are invariably made of three quarks and antibaryons of three antiquarks. No other assemblage of quarks can exist as a hadron. The combinations of the three quarks under these rules are sufficient to account for all the hadrons that had been observed or predicted at the time. Furthermore, every allowed combination of quarks yields a known particle.

Many of the necessary properties of



WEAK INTERACTIONS, mediated by the *W* particle (*hatched lines*), can change the flavor of a quark, but they have no effect on its color. Single charmed particles can therefore be created by the weak force, whereas they appear only as pairs in other interactions. A neutrino interacts with a proton (*top*), converting a *d* quark into a *c* quark while at the same time the neutrino is transformed into a muon. The immediate product is a charmed sigma baryon, but that quickly decays by the emission of a pion to a charmed lambda baryon. The charmed lambda particle can itself decay by emitting a *W* particle, converting the *c* quark into an *s* quark and thereby forming a strange lambda baryon. The strange lambda baryon can be accompanied by three pions, produced by repeated quark-pair creation (*middle*), a process that may have been observed recently at Brookhaven National Laboratory, or by a muon and a neutrino (*bottom*), a decay mode that may explain several events detected at the Fermi National Accelerator Laboratory.

the quarks can be deduced from these rules. It is mandatory, for example, that each of the quarks be assigned a baryon number of $+1/3$ and each of the antiquarks a baryon number of $-1/3$. In that way any aggregate of three quarks has a total baryon number of $+1$ and hence defines a baryon; three antiquarks yield a particle with a baryon number of -1, an antibaryon. For mesons the baryon numbers of the quarks ($+1/3$ and $-1/3$) cancel, so that the meson, as required, has a baryon number of 0.

In a similar way the angular momentum of the hadrons is described by giving the quarks half-integral units of spin. A particle made of an odd number of quarks, such as a baryon, must therefore also have half-integral spin, conforming to the known characteristics of baryons. A particle made of an even number of quarks, such as a meson, must have integral spin.

The *u* quark and the *s* quark compose an isotopic-spin doublet: they have nearly the same mass and they are identical in all other properties except electric charge. The *u* quark is assigned a charge of $+2/3$ and the *d* quark is assigned a charge of $-1/3$. The average charge of the doublet is therefore $+1/6$ and twice the average charge is $+1/3$; since the baryon number of all quarks is $+1/3$, the definition of strangeness gives both the *u* and the *d* quarks a strangeness of 0. The *s* quark has a larger mass than either the *u* or the *d* and makes up an isotopic-spin singlet. It is given an electric charge of $-1/3$ and consequently has a strangeness of -1. The antiquarks, denoted by writing the quark symbol with a bar over it, have opposite properties. The $\bar{u}$ has a charge of $-2/3$ and the $\bar{d}$ $+1/3$; both have zero strangeness. The $\bar{s}$ antiquark has a charge of $+1/3$ and a strangeness of $+1$.

Just two of the quarks, the *u* and the *d*, suffice to explain the structure of all the hadrons encountered in ordinary matter. The proton, for example, can be described by assembling two *u* quarks and a *d* quark; its composition is written *uud*. A quick accounting will show that all the properties of the proton determined by its quark constitution are in accord with the measured values. Its charge is equal to $2/3 + 2/3 - 1/3$, or $+1$. Similarly, its baryon number can be shown to be $+1$ and its spin 1/2. A positive pion is composed of a *u* quark and a $\bar{d}$ antiquark (written $u\bar{d}$). Its charge is $2/3 + 1/3$, or $+1$; its spin and baryon number are both 0.

The third quark, *s*, is needed only to construct strange particles, and indeed it provides an explicit definition of strangeness: A strange particle is one that contains at least one *s* quark or $\bar{s}$ antiquark. The lambda baryon, for example, can be shown from the charge distribution of its multiplet to have a strangeness of -1; that result is confirmed by its quark constitution of *uds*. Similarly, the neutral *K* meson, a strange particle, has a strangeness of $+1$, as confirmed by its composition of $d\bar{s}$.

Until quite recently these three kinds of quark were sufficient to describe all the known hadrons. As we shall see, experiments conducted during the past year seem to have created hadrons whose properties cannot be explained in terms of the original three quarks. The experiments can be interpreted as implying the existence of a fourth kind of quark, called the charmed quark and designated *c*.

The statement that the *u*, *d* and *s* quarks are sufficient to construct all the observed hadrons can be made more precisely in the mathematical formalism of the eightfold way. Since a meson is made up of one quark and one antiquark, and since there are three kinds, or flavors, of quark, there are nine possible combinations of quarks and antiquarks that can form a meson. It can be shown that one of these combinations represents a singlet and the remaining eight form an octet. Similarly, since a baryon is made up of three quarks, there are 27 possible combinations of quarks that can make up a baryon. They can be broken up into a singlet, two octets and a decimet. Those groupings correspond exactly to the observed families of hadrons. The quark theory thus explains why only a few of the possible representations of SU(3) are realized in nature as hadron supermultiplets.

The quark rules provide a remarkably economical explanation for the formation of the observed hadron families. What principles, however, can explain the quark rules, which seem quite arbitrary? Why is it possible to bind together three quarks but not two or four? Why can we not create a single quark in isolation? A line of thought that leads to possible answers to these questions appeared at first as a defect in the quark theory.

As we have seen, it is necessary that the quarks have half-integral values of spin angular momentum; otherwise the known spins of the baryons and mesons would be predicted wrongly. Particles with half-integral spin are expected to obey Fermi-Dirac statistics and are therefore subject to the Pauli exclusion principle: No two particles within a particular system can have exactly the same quantum numbers. Quarks, however, seem to violate the principle. In making up a baryon it is often necessary that two identical quarks occupy the same state. The omega particle, for example, is made up of three *s* quarks, and all three must be in precisely the same state. That is possible only for particles that obey Bose-Einstein statistics. We are at an impasse: quarks must have half-integral spin but they must satisfy the statistics appropriate to particles having integral spin.

The connection between spin and statistics is an unshakable tenet of relativistic quantum mechanics. It can be deduced directly from the theory, and a violation has never been discovered. Since it holds for all other known particles, quarks could not reasonably be excluded from its dominion.

The concept that has proved essential to the solution of the quark statistics problem was proposed in 1964 by Oscar W. Greenberg of the University of Maryland. He suggested that each flavor of quark comes in three varieties, identical in mass, spin, electric charge and all other measurable quantities but different in an additional property, which has come to be known as color. The exclusion principle could then be satisfied, and quarks could remain fermions, because the quarks in a baryon would not all occupy the same state. The quarks could differ in color even if they were the same in all other respects.

The color hypothesis requires two additional quark rules. The first simply restates the condition that color was introduced to satisfy: Baryons must be made up of three quarks, all of which have different colors. The second describes the application of color to mesons: Mesons are made of a quark and an antiquark of the same color, but with equal representation of each of the three colors. The effect of these rules is that no hadron can exhibit net color. A baryon invariably contains quarks of each of the three colors, say red, yellow and blue. In the meson one can imagine the quark and antiquark as being a single color at any given moment, but continually and simultaneously changing color, so that over any measurable interval they will both spend equal amounts of time as red, blue and yellow quarks.

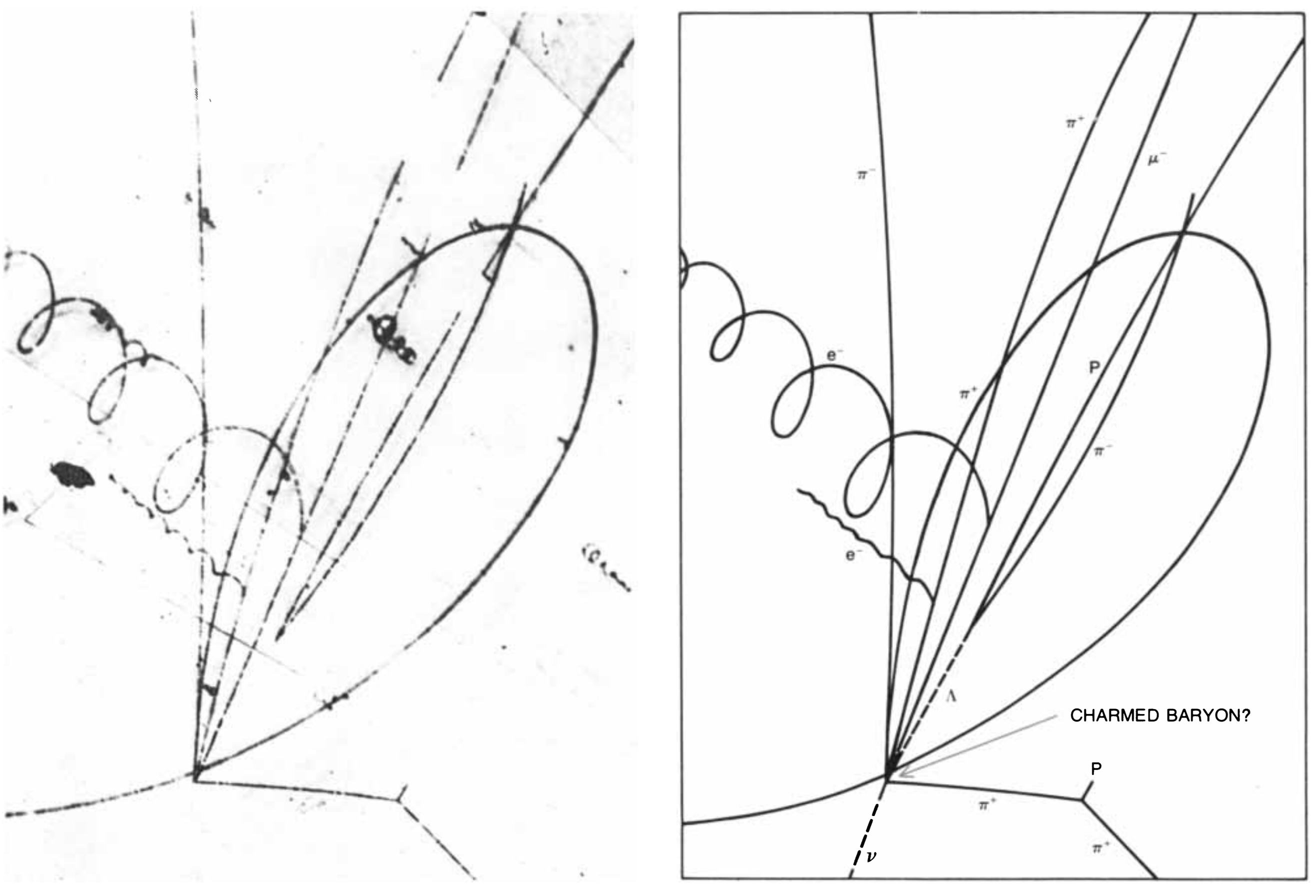
The price of the color hypothesis is a tripling of the number of quarks; there must be nine instead of three (with charm yet to be considered). At first it may also appear that we have greatly increased the number of hadrons, but that is an illusion. With color there seem to be nine times as many mesons and 27 times as many baryons, but the rules for assembling hadrons from colored quarks ensure that none of the additional particles are observable.

Although the quark rules imply that we will never see a colored particle, the color hypothesis is not merely a formal construct without predictive value. The increase it requires in the number of quarks can be detected in at least two ways. One is through the effect of color on the lifetime of the neutral pion, which almost always decays into two photons. Stephen L. Adler of the Institute for Advanced Study has shown that its rate of decay depends on the square of the number of quark colors. Just the observed lifetime is obtained by assuming that there are three colors.

Another effect of color can be detected in experiments in which electrons and their antiparticles, the positrons, annihilate each other at high energy. The outcome of such an event is sometimes a group of hadrons and sometimes a muon and an antimuon. At sufficiently high energy the ratio of the number of hadrons to the number of muon-antimuon pairs is expected to approach a constant value, equal to the sum of the squares of the charges of the quarks. Tripling the number of quarks also triples the expected value of the ratio. The experimental result at energies of from 2 GeV to 3 GeV is in reasonable agreement with the color hypothesis (which predicts a value of 2) and is quite incompatible with the original theory of quarks without color.

The introduction of the color quantum number solves the problem of quark statistics, but it once again requires a set of rules that seem arbitrary. The rules can be accounted for, however, by establishing another hypothetical symmetry group analogous to the SU(3) symmetry proposed by Gell-Mann and by Ne'eman. The earlier SU(3) is concerned entirely with combinations of the three quark flavors; the new one deals exclusively with the three quark colors. Moreover, unlike the earlier theory, which is only approximate, color SU(3) is supposed to be an exact symmetry, so that quarks of the same flavor but different color will have identical masses.

In the color SU(3) theory all the quark rules can be explained if we accept one postulate: All hadrons must be represented by color singlets; no larger multiplets can be allowed. A color singlet can be constructed in two ways: by combining an identically colored quark and antiquark with all three colors equally represented, or by combining three



CHARMED BARYON may have been detected by Nicholas P. Samios and his colleagues at Brookhaven in the aftermath of a collision between a neutrino and a proton. The photograph at left was made in a bubble chamber filled with liquid hydrogen; the particle tracks in the photograph are identified in the diagram at right. The neutrino enters from the bottom left; its track is not visible because only particles with an electric charge ionize hydrogen molecules and leave a trail of bubbles in the chamber. The possible charmed baryon does have an electric charge, but its track cannot be seen either because it is too short; the particle must decay in about 10^{-13} second, so that even at very high speed it does not move far enough to ionize more than a few molecules of hydrogen. The charmed particle decays into a neutral lambda particle, a strange baryon. The lambda particle leaves no track, but its decay products form a vertex that points toward the initial interaction. Four pions and a muon are also created, and two electrons struck by fast-moving particles spiral to the left in the bubble chamber's magnetic field. The presence of the charmed particle is not certain; several other interpretations of the event are possible, and although they are all unlikely, they cannot be excluded.

quarks or three antiquarks in such a way that the three colors are all included. These conditions, of course, are equivalent to the rules for building mesons, baryons and antibaryons, and they ensure that all hadrons will be colorless. There are no other ways to make a singlet in color SU(3); a particle made any other way would be a member of a larger multiplet, and it would display a particular color.

Although the color SU(3) theory of the hadrons can explain the quark rules, it cannot entirely eliminate the arbitrary element in their nature. We can ask a still more fundamental question: What explains the postulate that all hadrons must be color singlets? One approach to an answer, admittedly a speculative one, has been suggested recently by many investigators; it incorporates the color SU(3) model of the hadrons into one of the class of theories called gauge theories.

The color gauge theory postulates the existence of eight massless particles, sometimes called gluons, that are the carriers of the strong force, just as the photon is the carrier of the electromagnetic force. Like the photon, they are electrically neutral, and they have a spin of 1; they are therefore called vector bosons (bosons because they have integer spin and obey Bose-Einstein statistics, vector because a particle with a spin of 1 is described by a wave function that takes the form of a four-dimensional vector). Gluons, like quarks, have not been detected.

When a quark emits or absorbs a gluon, the quark changes its color but not its flavor. For example, the emission of a gluon might transform a red *u* quark into a blue or a yellow *u* quark, but it could not change it into a *d* or an *s* quark of any color. Since the color gluons are the quanta of the strong force, it follows that color is the aspect of quarks that is most important in the strong interactions. In fact, when describing interactions that involve only the strong force, one can virtually ignore the flavors of quarks.

The color gauge theory proposes that the force that binds together colored quarks represents the true character of the strong interaction. The more familiar strong interactions of hadrons (such as the binding of protons and neutrons in a nucleus) are manifestations of the same fundamental force, but the interactions of colorless hadrons are no more than a pale remnant of the underlying interaction between colored quarks. Just as the van der Waals force between molecules is only a feeble vestige of the electromagnetic force that binds electrons to nuclei, the strong force observed between hadrons is only a vestige of that operating within the individual hadron.

From these theoretical arguments one can derive an intriguing, if speculative, explanation of the confinement of quarks. It has been formulated by John Kogut and Kenneth Wilson of Cornell University and by Leonard Susskind of Yeshiva University. If it should be proved correct, it would show that the failure to observe colored particles (such as isolated quarks and gluons) is not the result of any experimental deficiency but is a direct consequence of the nature of the strong force.

The electromagnetic force between two charged particles is described by Coulomb's law: The force decreases as the square of the distance between the charges. Gravitation obeys a fundamentally similar law. At large distances both forces dwindle to insignificance. Kogut, Wilson and Susskind argue that the strong force between two colored quarks behaves quite differently: it does not diminish with distance but remains constant, independent of the separation of the quarks. If their argument is sound, an enormous amount of energy would be required to isolate a quark.

Separating an electron from the valence shell of an atom requires a few electron volts. Splitting an atomic nucleus requires a few million electron volts. In contrast to these values, the separation of a single quark by just an inch from the proton of which it is a constituent would require the investment of 10^{13} GeV, enough energy to separate the author from the earth by some 30 feet. Long before such an energy level could be attained another process would intervene. From the energy supplied in the effort to extract a single quark, a new quark and antiquark would materialize. The new quark would replace the one removed from the proton, and would reconstitute that particle. The new antiquark would adhere to the dislodged quark, making a meson. Instead of isolating a colored quark, all that is accomplished is the creation of a colorless meson [*see illustration on page* 39]. By this mechanism we are prohibited from ever seeing a solitary quark or a gluon or any combination of quarks or gluons that exhibits color.

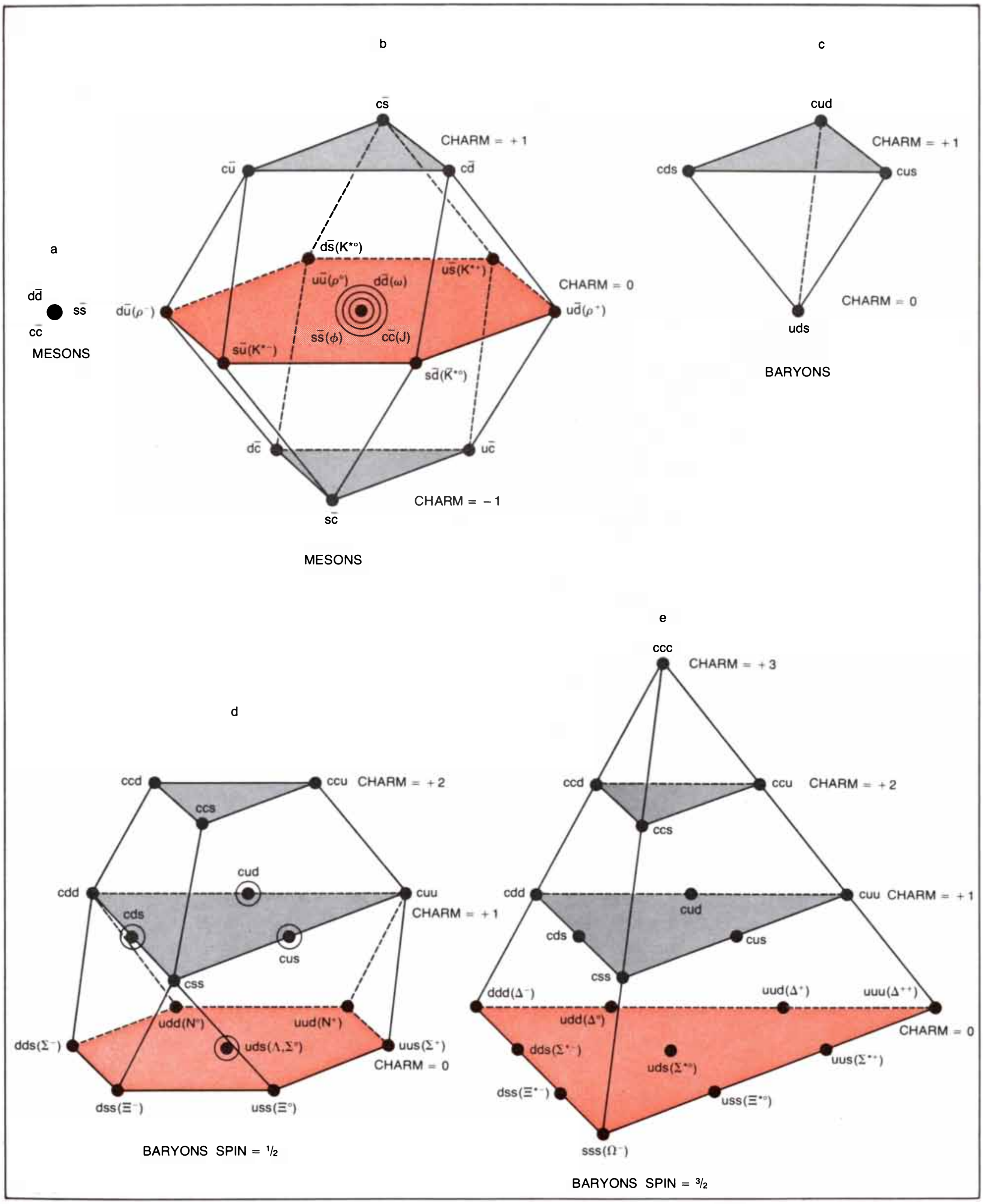
If this interpretation of quark confinement is correct, it suggests an ingenious way to terminate the apparently infinite regression of finer structures in matter. Atoms can be analyzed into electrons and nuclei, nuclei into protons and neutrons, and protons and neutrons into quarks, but the theory of quark confinement suggests that the series stops there. It is difficult to imagine how a particle could have an internal structure if the particle cannot even be created.

Quarks of the same flavor but different color are expected to be identical in all properties except color; indeed, that is why the concept of color was introduced. Quarks that differ in flavor, however, have quite different properties. It is because the *u* quark and the *d* quark differ in electric charge that the proton is charged and the neutron is not. Similarly, it is because the *s* quark is considerably more massive than either the *u* or the *d* quark that strange particles are generally the heaviest members of their families. The charmed quark, *c*, must be heavier still, and charmed particles as a rule should therefore be heavier than all others. It is the flavor of quarks that brings variety to the world of hadrons, not their color.

As we have seen, the flavors of quarks are unaffected by the strong interactions. In a weak interaction, on the other hand, a quark can change its flavor (but not its color). The weak interactions also couple quarks to the leptons. The classical example of this coupling is nuclear beta decay, in which a neutron is converted into a proton with the emission of an electron and an antineutrino. In terms of quarks the reaction represents the conversion of a *d* quark to a *u* quark, accompanied by the emission of the two leptons.

The weak interactions are thought to be mediated by vector bosons, just as the strong and the electromagnetic interactions are. The principal one, labeled *W* and long called the intermediate vector boson, was predicted in 1938 by Hideki Yukawa. It has an electric charge of −1, and it differs from the photon and the color gluons in that it has mass, indeed a quite large mass. Quarks can change their flavor by emitting or absorbing a *W* particle. Beta decay, for example, is interpreted as the emission of a *W* by a *d* quark, which converts the quark into a *u*; the *W* then decays to yield the electron and antineutrino. From this process it follows that the *W* can also interact with leptons, and it thus provides a link between the two groups of apparently elementary particles.

The realization that the strong, weak and electromagnetic forces are all carried by the same kind of particle—bosons with a spin of 1—invites speculation that all three might have a common basis in some simple unified theory. A step



SUPERMULTIPLETS OF HADRONS that include the predicted charmed particles can be arranged as polyhedrons. Each supermultiplet consists of particles with the same value of spin angular momentum. Within each supermultiplet the particles are assigned positions according to three quantum numbers: positions on the shaded planes are determined by isotopic spin and strangeness; the planes themselves indicate values of charm. The mesons are represented by a point (*a*) and by an Archimedean solid called a cuboctahedron (*b*), which comprises 15 particles, including six charmed ones. The mesons shown are those with a spin of 1, but all mesons fit the same point and cuboctahedron representations. The baryons form a small regular tetrahedron (*c*) of four particles, a truncated tetrahedron (*d*) of 20 particles and a larger regular tetrahedron (*e*), also made up of 20 particles. Both mesons and baryons are identified by their quark constitution, and for those particles that have been observed the established symbol is also given. Each figure contains one plane (*color*) of uncharmed particles that are identical with earlier representations of the "eightfold way."

toward such a unification would be the reconciliation of the weak interactions and electromagnetism. Julian Schwinger of Harvard University attempted such a unification in the mid-1950's (when I was one of his doctoral students, working on these very questions). His theory had serious flaws. One was eliminated in 1961, when I introduced a second, neutral vector boson, now called Z, to complement the electrically charged W. Other difficulties persisted for 10 years, until in 1967 Steven Weinberg of Harvard and Abdus Salam of the International Center for Theoretical Physics in Trieste independently suggested a resolution. By 1971 it was generally agreed, largely because of the work of Gerhard 't Hooft of the University of Utrecht, that the Weinberg-Salam conjecture is successful [see "Unified Theories of Elementary-Particle Interaction," by Steven Weinberg; SCIENTIFIC AMERICAN, July, 1974].

Through the unified weak and electromagnetic interactions, quarks and leptons are intimately related. These interactions "see" the four leptons and distinguish between the three quark flavors. The W particle can induce one kind of neutrino to become an electron and the other kind of neutrino to become a muon. Similarly, the W can convert a u quark into a d quark; it can also influence the u quark to become an s quark, although much less readily.

There is an obvious lack of symmetry in these relations. The leptons consist of two couples, married to each other by the weak interaction: the electron with the electron neutrino and the muon with the muon neutrino. The quarks, on the other hand, come in only three flavors, and so one must remain unwed. The scheme could be made much tidier if there were a fourth quark flavor, in order to provide a partner for the unwed quark. Both the quarks and the leptons would then consist of two pairs of particles, and each member of a pair could change into the other member of the same pair simply by emitting a W. The desirability of such lepton-quark symmetry led James Bjorken and me, among others, to postulate the existence of a fourth quark in 1964. Bjorken and I called it the charmed quark. When provisions are made for quark colors, charm becomes a fourth quark flavor, and a new triplet of colored quarks is required. There are thus a total of 12 quarks.

Since 1964 several additional arguments for charm have developed. To me the most compelling of them is the need to explain the suppression of certain interactions called strangeness-changing neutral currents. An explanation that relies on the properties of the charmed quark was presented in 1967 by John Iliopoulos, Luciano Maiani and me.

Strangeness-changing neutral currents are weak interactions in which the net electric charge of the hadrons does not change but the strangeness does; typically an s quark is transformed into a d quark, and two leptons are emitted. An example is the decay of the neutral K meson (a strange particle) into two oppositely charged muons. Such processes are found by experiment to be extremely rare. The three-quark theory cannot account for their suppression, and in fact the unified theory of weak and electromagnetic interactions predicts rates more than a million times greater than those observed.

The addition of a fourth quark flavor with the same electric charge as the u quark neatly accounts for the suppression, although the mechanism by which it does so may seem bizarre. With two pairs of quarks there are two possible paths for the strangeness-changing interactions, instead of just one when there are only three quarks. In the macroscopic world the addition of a second path, or channel, would be expected always to bring an increase in the reaction rate. In a world governed by quantum mechanics, however, it is possible to subtract as well as to add. As it happens, a sign in the equation that defines one of the reactions is negative, and the two interactions cancel each other.

The addition of a fourth quark flavor must obviously increase the number of hadrons. In order to accommodate the newly predicted particles in supermultiplets the eightfold way must be expanded. In particular another dimension must be added to the graphs employed to represent the families, so that the plane figures of the earlier symmetry become Platonic and Archimedean solids.

To the meson octet are added six charmed particles and one uncharmed particle to make up a new family of 15. It is represented as a cuboctahedron, in which one plane contains the hexagon of the original uncharmed meson octet. The baryon octets and decimet are expected to form two families having 20 members each. They are represented as a tetrahedron truncated at each vertex and as a regular tetrahedron. In addition there is a smaller regular tetrahedron consisting of just four baryons. Again, each figure contains one plane of uncharmed particles [*see illustration on opposite page*].

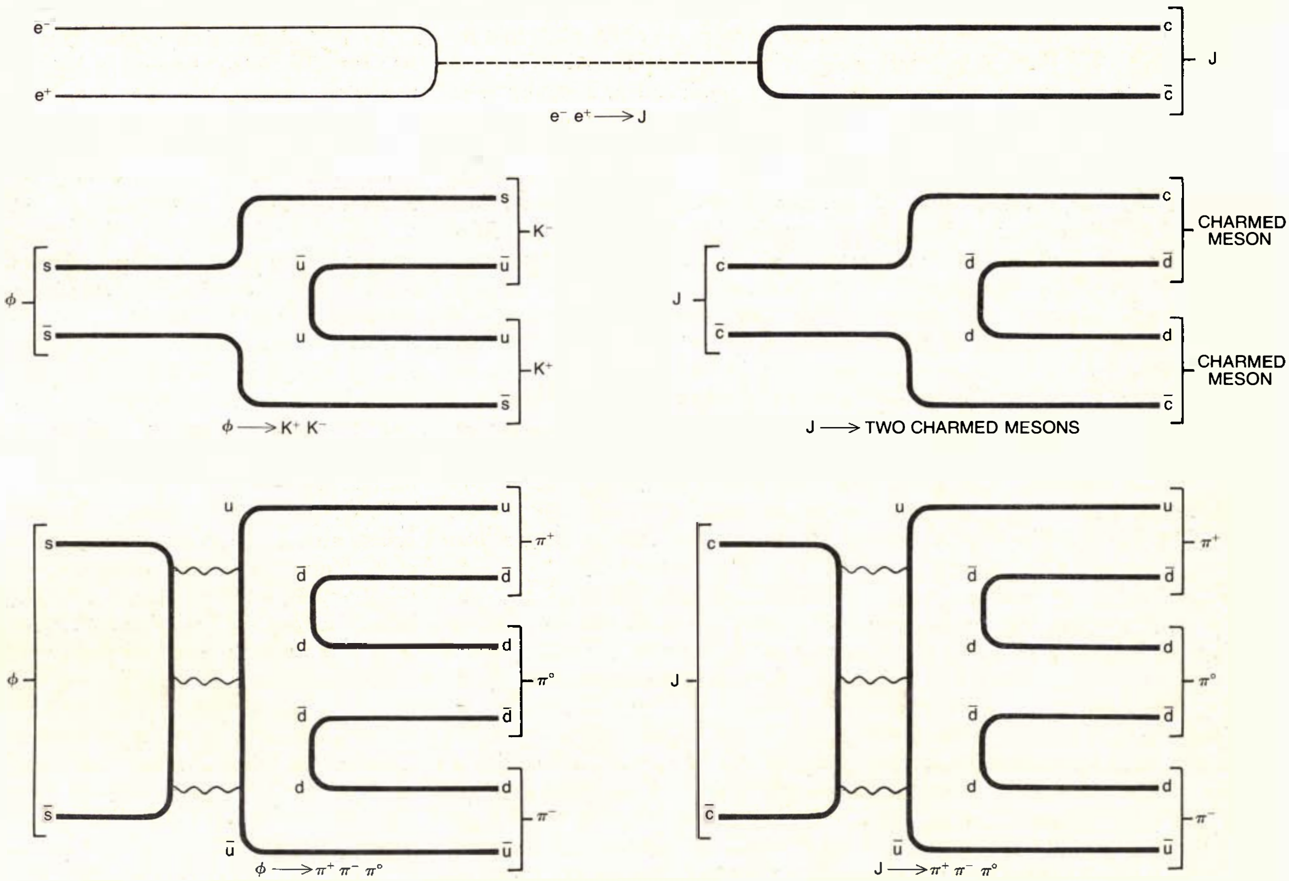
It now appears that the first of the new particles to be discovered is a meson that is not itself charmed. That conclusion is based on the assumption that the predicted meson is the same particle as the J or psi particle discovered last November. The announcement of the discovery was made simultaneously by Samuel C. C. Ting and his colleagues at the Brookhaven National Laboratory and by Burton Richter, Jr., and a group of other physicists at the Stanford Linear Accelerator Center (SLAC). At Brookhaven it was named J, at Stanford psi. Here I shall adopt the name J. For two excited states of the same particle, however, the names psi′ and psi″ will be employed, since they were seen only in the SLAC experiments.

The J particle was found as a resonance, an enhancement at a particular energy in the probability of an interaction between other particles. At Brookhaven the resonance was detected in the number of electron-positron pairs produced in collisions between protons and atomic nuclei. At SLAC it was observed in the products of annihilations of electrons and positrons. The energy at which the resonances were observed—and thus the energy or mass of the J particle—is about 3.1 GeV [see "Electron-Positron Annihilation and the New Particles," by Sidney D. Drell; SCIENTIFIC AMERICAN, June].

The J particle decays in about 10^{-20} second, certainly a brief interval, but nevertheless 1,000 times longer than the expected lifetime of a particle having the J's mass. The considerable excitement generated by the discovery of the J was largely a result of its long lifetime.

A great many explanations of the particle were proposed; for example, it was suggested that it might be the Z. I believe there is good reason to interpret the J as being a meson made up of a charmed quark and a charmed antiquark, that is, a meson with the quark constitution $c\bar{c}$. Thomas Appelquist and H. David Politzer of Harvard have named such a meson "charmonium," by analogy to positronium, a bound state of an electron and a positron. Charmonium is without charm because the charm quantum numbers of its quarks (+1 and −1) add up to zero.

The charmonium hypothesis can account for the anomalous lifetime of the J if one considers the ultimate fate of the decaying particle's quarks. There are three possibilities: they can be split up to become constituents of two daughter hadrons, they can both become part of a single daughter particle or they can be annihilated. An empirical rule, first



J PARTICLE is interpreted as a bound state of a *c* quark and a *c̄* antiquark, called charmonium. It was discovered last November by physicists at Brookhaven and at the Stanford Linear Accelerator Center (SLAC). At SLAC it was made (*top*) when electrons and positrons annihilated each other to create a virtual photon (*broken line*), which then materialized to yield the new meson. The *J* particle has a mass of 3.1 GeV (billion electron volts). It does not exhibit charm because the charm quantum numbers of its quarks (+1 and −1) add to zero; in the same way a more familiar particle, the phi meson, has zero strangeness because it is made up of a strange quark and a strange antiquark. In spite of this analogy the two particles decay by different modes. For both the preferred mode consists in contributing the quark and the antiquark to two different daughter mesons. For the phi meson that is just possible (*middle left*) because the mass of the phi (1.019 GeV) is slightly greater than the combined mass of two strange *K* mesons. For the *J* it is not possible (*middle right*) because the lightest charmed particle is more than half as massive as the *J*. The *J* must therefore decay by the annihilation of its quarks (*bottom right*); the annihilation yields three gluons (*wavy lines*), which are transformed into three pions. This mode of decay is suppressed, and the equivalent process for the phi meson (*bottom left*) is rarely observed.

noted by Zweig, states that decays of the first kind are allowed but the other two are suppressed. For the *J* particle to decay in the allowed manner it must create two charmed particles, that is, two hadrons, one containing a charmed quark and the other a charmed antiquark. That decay is possible only if the mass of the *J* is greater than the combined masses of the charmed daughter particles. There is reason to believe the lightest charmed particle has a mass greater than half of the mass of the *J*, and it therefore appears that the *J* cannot decay in the allowed mode. The *J* cannot decay in the second way, either, keeping both its quarks in a single particle, because the *J* is the least massive state containing a charmed quark and a charmed antiquark. It must therefore decay by the annihilation of its quarks, a decay suppressed by Zweig's rule. The suppression offers a partial explanation for the particle's extended lifetime.

Zweig's rule was formulated to explain the decay of the phi meson, which is made up of a strange quark and a strange antiquark and has a mass of about 1 GeV. The two particles are closely analogous, but the decay of the *J* is appreciably slower than that of the phi. Why should Zweig's rule be more effective for *J* than it is for phi? Furthermore, what explains Zweig's arbitrary rule?

A possible answer is provided by the theoretical concept called asymptotic freedom, which holds that the strong interactions become less strong at high energy. At sufficiently high energy the proton behaves as if it were made up of three freely moving quarks instead of three tightly bound ones. The concept takes its name from the fact that the quarks approach the state of free motion asymptotically as the energy is increased. Asymptotic freedom offers an explanation for the discrepancy between the phi and the *J* particles in the application of Zweig's rule. Because the *J* is so massive, or alternatively so energetic, the strong interaction is of diminished strength, and it is particularly difficult for the quark and the antiquark to annihilate each other.

Like positronium, charmonium should appear in many energy states. Two were discovered at SLAC soon after the first state was found; they are psi′, with a mass of about 3.7 GeV, and psi″, with a mass of about 4.1 GeV. They appear to be simple excited states of the lowest-lying state of charmonium, the *J* particle. Psi′ decays only a little more quickly than *J*, and half the time its decay products are the *J* particle itself and two pions. Thus it sometimes decays by the second suppressed process described by Zweig's rule, that is, by contributing both of its quarks to a single daughter particle. The extended lifetime implies that psi′ also lies below the energy threshold for the creation of a pair of charmed particles.

Psi″ decays much more quickly and therefore must be decaying in some mode permitted by Zweig's rule. Its decay products have not yet been determined, but it is possible they include charmed hadrons.

Numerous other excited states of charmonium follow inevitably from the theory of quark interactions [*see illustration at right*]. One, called *p*-wave charmonium, is formed when the particle takes on an additional unit of angular momentum. Some fraction of the time psi′ should decay into *p*-wave charmonium, which should subsequently decay predominantly to the ground state, *J*. At each transition a photon of characteristic energy must be emitted. Recent experiments at the DORIS particle-storage rings of the German Electron Synchrotron in Hamburg have apparently detected the decays associated with the *p*-wave particle. In a few percent of its decays psi′ yields the *J* particle and two photons, with energies of .2 GeV and .4 GeV. At SLAC psi′ has been found to decay into an intermediate state and a single photon with an energy of .2 GeV. The intermediate state, which is presumably the same particle as the one observed at DORIS, then decays directly into hadrons.

The correspondence of theory and experiment revealed by the discovery of the *p*-wave transitions inspires considerable confidence that the charmonium interpretation of the *J* particle is correct. There is at least one more predicted state, called paracharmonium, that must be found if this explanation of the particle is to be confirmed. It differs from the observed states in the orientation of the quark spins: in *J*, psi′ and psi″ (collectively called orthocharmonium) they are parallel; in paracharmonium they are antiparallel. Paracharmonium has so far evaded detection, but if the theoretical description is to make sense, paracharmonium must exist.

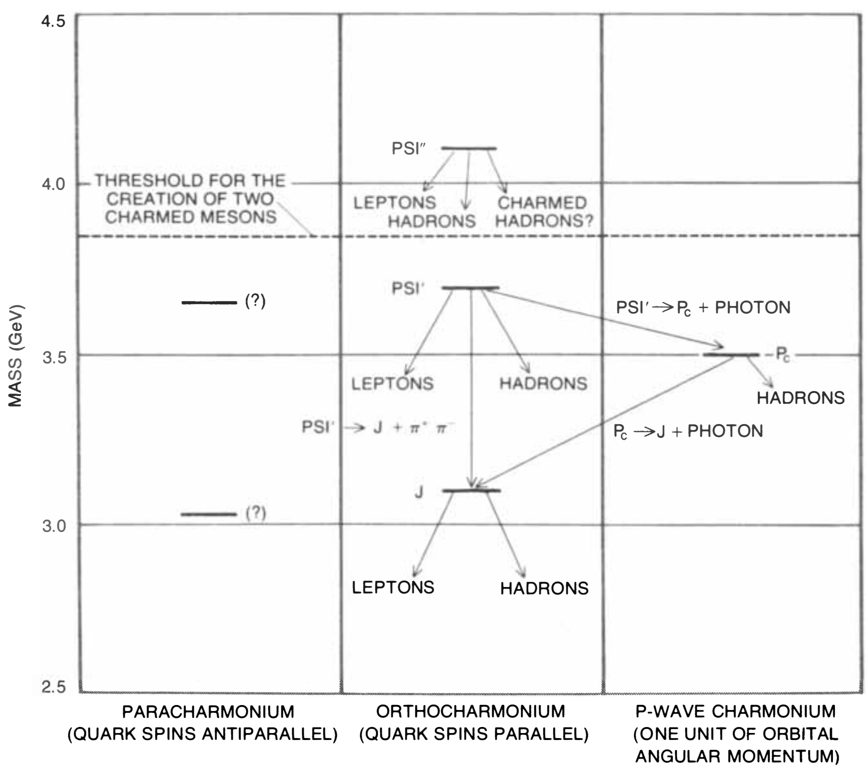
In addition to the various states of (uncharmed) charmonium, all the predicted charmed particles must also exist. If the *J* is in fact a state of charmonium, we can deduce from its mass the masses of all the hadrons containing charmed quarks.

An important initial constraint on the range of possible masses was provided by the interpretation of the suppression of strangeness-changing neutral currents. If the suppression mechanism is to work, the charmed quark cannot be too much heavier than its siblings. On the other hand, it cannot be very light or charmed hadrons would already have been observed. An estimate from these conditions suggested that charmed particles would be found to have masses of about 2 or 3 GeV.

After the discovery of the *J*, I performed a more formal analysis with my colleagues at Harvard, Alvaro De Rújula and Howard Georgi. So did many others. Our estimates indicate that the least massive charmed states are mesons made up of a *c* quark and a $\bar{u}$ or $\bar{d}$ antiquark; their mass should fall between 1.8 GeV and 2.0 GeV. A value within that range could be in agreement with the supposition that psi′ lies below the threshold for the creation of a pair of charmed mesons, but psi″ lies above it.

The least massive charmed baryon has a quark composition of *udc;* we predict that its mass is near 2.2 GeV. As might be expected, since the *c* quark is the heaviest of the four, the most massive predicted charmed hadron is the *ccc* baryon. We estimate its mass at about 5 GeV.

An important principle guiding experimental searches for charmed hadrons is the requirement that in most kinds of interaction charmed particles can be created only in pairs. Two hadrons must be produced, one containing a charmed quark, the other a charmed antiquark; the obvious consequence is a doubling of the energy required to create a charmed particle. An important exception to this rule is the interaction of neutrinos with other kinds of particles, such as protons. Neutrino events are exempt because neutrinos have only weak interactions and quark flavor can be changed in weak processes. Many experimental techniques have been tried in the search for charm during the past 10 years, yet no charmed particle has been unambiguously identified. Nevertheless, two recent experiments, both involving neutrino interactions, are encouraging. In both charm may at last have appeared,



CHARMONIUM must exist at several energy levels, distinguished by the state of motion of the constituent quarks. The *J* particle is the ground state of orthocharmonium, in which the quark spins are parallel. Two excited states of orthocharmonium, designated psi′ and psi″, were discovered at SLAC shortly after the *J* particle. Psi′, like the *J* particle, seems to be too light to decay into two charmed hadrons, but the rapid decay of psi″ suggests that it has the necessary mass. Two other forms of the particle, called paracharmonium, in which the quark spins are antiparallel, have not been discovered. *P*-wave charmonium, in which the quarks have a unit of orbital angular momentum in addition to spin angular momentum, may have been detected at the German Electron Synchrotron in Hamburg and at SLAC.

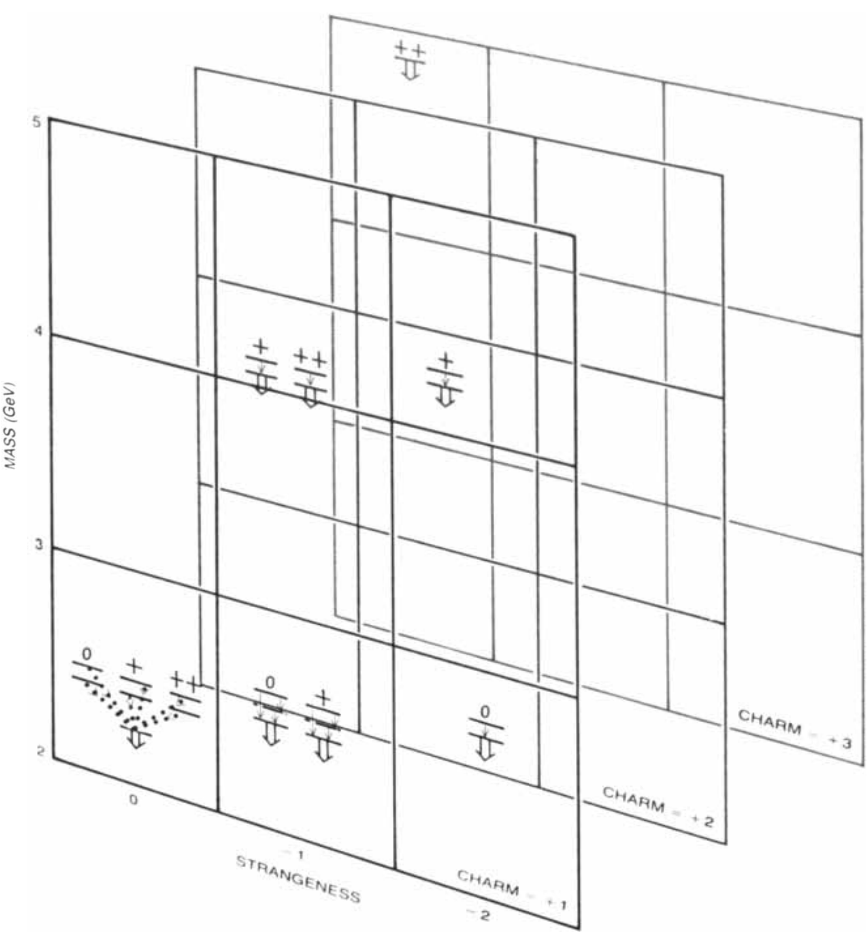
but even if that proves to be an illusion, the experiments suggest promising lines of research.

One of the experiments was conducted at the Fermi National Accelerator Laboratory in Batavia, Ill., by a group of physicists headed by David B. Cline of the University of Wisconsin, Alfred K. Mann of the University of Pennsylvania and Carlo Rubbia of Harvard. In examining the interactions of high-energy neutrinos they found that in several percent of the events the products included two oppositely charged muons. One of the muons could be created directly from the incident neutrino, but the other is difficult to account for with only the ensemble of known, uncharmed particles. The most likely interpretation is that a heavy particle created in the reaction decays by the weak force to emit the muon. The particle would have a mass of between 2 and 4 GeV, and if it is a hadron, some explanation must be found for its weak decay. Most particles with masses that large decay by the strong force. The presence of a charmed quark in the particle might provide the required explanation.

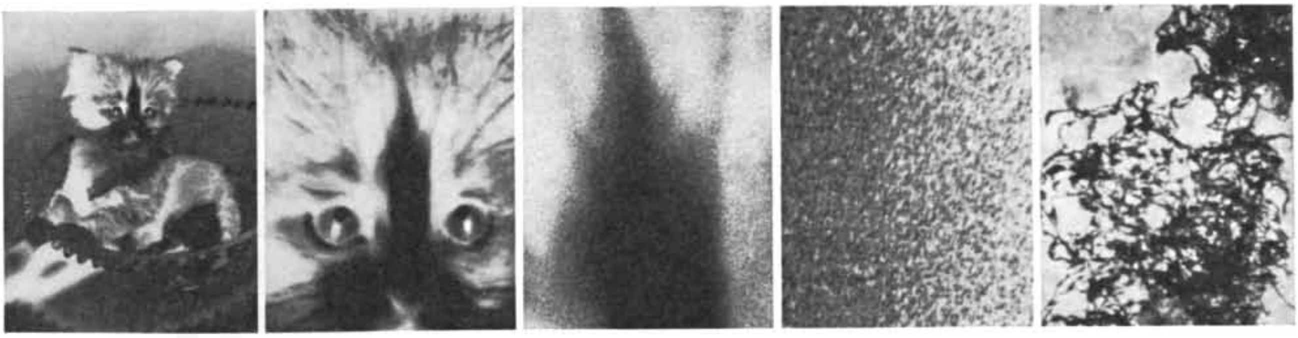
The second experiment was performed at Brookhaven by a group of investigators under Nicholas P. Samios. They photographed the tracks resulting from the interaction of neutrinos with protons in a bubble chamber. In a sample of several hundred observed collisions one photograph seemed to have no conventional interpretation [*see illustration on page 44*]. The final state can be construed as the decay products of a charmed baryon. The process would provide convincing evidence for the existence of charm if it were not attested to by only one event. A few more observations of the same reaction would settle the matter.

It would be misleading to give the impression that the description of hadrons in terms of quarks of three colors and four flavors has solved all the outstanding problems in the physics of elementary particles. For example, continuing measurements of the ratio of hadrons to muon pairs produced in electron-positron annihilations have confounded prediction. The ratio discriminates between various quark models, and an argument in support of the color hypothesis was that at energies of from 2 to 3 GeV the ratio is about 2. At higher energy, high enough for charmed hadrons to be created in pairs, the ratio was expected to rise from 2 to about 3.3. The ratio does increase, but it overshoots the mark and appears to stabilize at a value of about 5. Perhaps charmed particles are being formed, but it seems that something else is happening as well: some particle is being made that does not appear in the theory I have described. One of my colleagues at Harvard, Michael Barnett, believes we have not been ambitious enough. He invokes six quark flavors rather than four, so that there are three flavors of charmed quark. It is also possible there are heavier leptons we know nothing about.

Finally, even if a completely consistent and verifiable quark model could be devised, many fundamental questions would remain. One such perplexity is implicit in the quark-lepton symmetry that led to the charm hypothesis. Both the quarks and the leptons, all of them apparently elementary, can be divided into two subgroups. In one group are the u and d quarks and the electron and electron neutrino. These four particles are the only ones needed to construct the world; they are sufficient to build all atoms and molecules, and even to keep the sun and other stars shining. The other subgroup consists of the strange and charmed quarks and the muon and muon neutrino. Some of them are seen occasionally in cosmic rays, but mainly they are made in high-energy particle accelerators. It would appear that nature could have made do with half as many fundamental things. Surely the second group was not created simply for the entertainment or edification of physicists, but what is the purpose of this grand doubling? At this point we have no answer.



CHARMED BARYONS are expected to be considerably more massive than other hadrons. None of the charmed particles have yet been unambiguously identified, but their masses have been predicted from the mass of the *J* particle. Some of the charmed particles must exist in more than one charge state (indicated by zeros and plus signs) and at several energy levels (indicated by their position with respect to the mass scale at left). Some of the particles can decay by the strong interaction (*dotted arrows*) or the electromagnetic interaction (*solid black arrows*) into states that have the same quantum numbers but smaller mass; others can decay only by the weak interaction (*open arrows*) into uncharmed particles. The form of the table is determined largely by the requirement that a baryon be made up of exactly three quarks; there can be no particle with a strangeness of −2 and a charm of +3, for example, because that would require five quarks: two strange ones and three charmed ones.



Here kitty, kitty, kitty, kitty!

Between the eyes, the upward arrow. All done with the statistics of distribution of skeins of ultramicro silver filaments. For color, little clouds of dye.

When magnification is carried too far, the subjective effect called graininess manifests itself. It correlates with granularity, a physically measurable quantity. That's not just playing with words. Graininess, quite appropriately, is the sensation one gets when looking at a handful of grain. If instead of lying helter-skelter in your palm the kernels were uniformly spaced, graininess would no longer be the word for the sensation, even though you were still seeing individual grains. It's randomness that elicits the graininess sensation.

Except at extreme right above, one is seeing only random variations in spacing and overlapping of the individual silver skeins or dye clouds. We quantify these variations as deviations from density 1.0 when scanned in a densitometer with a 48-μm aperture. For all kinds of uniformly exposed Kodak films, the root-mean-square of the deviations multiplied by 1,000 and appropriately rounded off becomes "diffuse rms granularity value."

Here is how these values relate to the words we use to state graininess:

45, 50, 55	"Very Coarse"
33, 36, 39, 42	"Coarse"
26, 28, 30	"Moderately Coarse"
21, 22, 24	"Medium"
16, 17, 18, 19, 20	"Fine"
11, 12, 13, 14, 15	"Very Fine"
6, 7, 8, 9, 10	"Extremely Fine"
5 or less	"Micro Fine"

Emulsion progress over the years has consisted largely of lifting the up-sloping line that plots light sensitivity against granularity. Result: the number of pictures taken per year goes up much faster than acres of film manufactured—less material required for more and better results with less in the way of equipment and folderol. Good for the customer. Good for us.

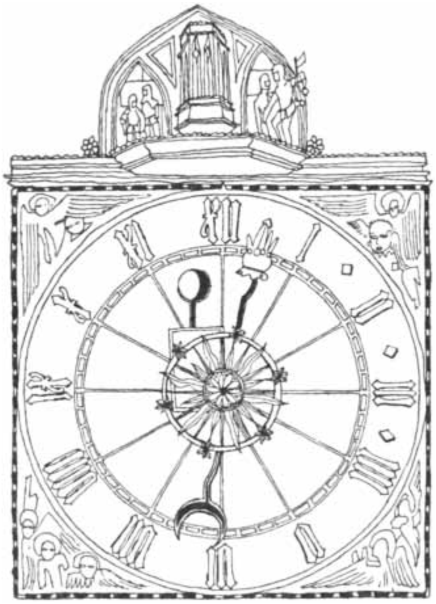
KITTY IS MISSING! Circulars are to be run off bearing the kitty's picture. But, since the printing press either does or does not deposit ink at any given point on the paper, the information about that all-important streak on the head is to be conveyed through arrayed dots of varying size.

Dealers in materials for the graphic arts (check the Yellow Pages) can supply KODALITH AUTOSCREEN Ortho Film 2563 (ESTAR Base). It makes possible a simpler procedure to convert a photo to dots than used for the illustrations in the magazine you are now reading. The sensitivity of its emulsion varies cyclically with a frequency of 133 per inch. This results in a grid of dots whose size varies with brightness distribution over the image. The film is quite slow; for direct photography, instead of making halftones from existing photographs, only still-life subjects are practical. But that might be quite useful if instead of finding your lost cat you just want to reproduce photographs decently with a duplicating machine. The film can be exposed in a conventional view camera.

Details in Kodak Publication No. Q-20 ("Making Halftones with KODALITH AUTOSCREEN Ortho Film 2563"), available from above dealers or Kodak, Dept. 412-L, Rochester, N.Y. 14650.

KITTY MAY BE HIDDEN IN HOLOGRAPHIC FRINGES. Why people should want to render an image holographically they don't always say. At any rate, it is reported in *Applied Physics Letters* for August 1, 1975 that KODAK Holographic Plate Type 120-01, employed in a stunt described in 1840 by Sir John F. W. Herschel, yields what is believed to be the highest diffraction efficiency (for red-light reconstruction) ever reported in an infrared-recording material, with spatial frequency better than 1300 cycles/mm. It was Sir John who revealed the existence of the infrared in the first place, and then he found that it erased latent image in a light-exposed photographic material. Now it turns out that infrared holographic images can be recorded by Herschel-effect erasure in prefogged 120-01 plates. Doesn't seem to work on our other high-resolution emulsions. See *Applied Physics Letters* for details. Type 120 plates are stocked by dealers in holographic specialties.

Kodak



SCIENCE AND THE CITIZEN

The Magnetic Monopole

The recent report that a magnetic monopole may have been detected took many physicists by surprise, but not because they had doubted that the particle exists. The monopole has been all but essential in physical theory for almost 45 years, and the principal mystery has been its failure to appear in observations and experiments [see "Magnetic Monopoles," by Kenneth W. Ford; SCIENTIFIC AMERICAN, December, 1963]. If a monopole has now in fact been observed, the negative results of earlier attempts become even more curious, and they may imply that the properties of the particle are somewhat different from what had been supposed.

In the theory of electromagnetism the monopole is complementary to the electron. Just as the electron is characterized by an electric charge, so does the monopole have a magnetic charge, or intrinsic pole strength. An electron at rest is surrounded by an electric field, and when it is in motion, it acquires a magnetic field. A monopole, conversely, has only a magnetic field when it is at rest but acquires an electric field when it is in motion. Because of the symmetry the monopole brings to electromagnetism, most physicists have been confident that sooner or later one would be found.

The recent possible observation of a monopole was reported by P. Buford Price and Edward K. Shirk of the University of California at Berkeley and W. Zack Osborne and Lawrence S. Pinsky of the University of Houston. They made the discovery in the course of an experiment whose primary object was not the detection of monopoles but the investigation of massive atomic nuclei in cosmic radiation. The experiment employed a novel particle detector that was suspended from a balloon for two and a half days at an altitude of about 25 miles.

The detector had three components. One consisted of a fine-grained photographic emulsion of the kind ordinarily used to record the tracks of electrically charged particles. The second was a Cerenkov detector: a sheet of plastic with a high refractive index coated on its underside with another layer of photographic film. When a particle passes through a medium at a speed greater than the speed of light in that medium, it emits the light known as Cerenkov radiation. Particles passing through the detector would radiate if they were moving at more than .68 *c*, or .68 times the speed of light in a vacuum. The Cerenkov radiation would be registered by the film.

The third component of the detector employed a technique that Price and some of his colleagues have been instrumental in developing. It consisted of 33 thin sheets of a carbonate plastic, Lexan. Particles passing through such a plastic are known to disrupt the molecular structure of the polymer. When the sheets are etched with a caustic solution, the damaged areas are removed faster than the rest of the plastic, leaving small conical pits that mark the path of the particle. From the shape of the pits and the rate at which they form, some of the properties of the particle can be deduced.

The track of the suspected monopole was discovered during the summer; Price and his colleagues describe it in *Physical Review Letters*. From the photographic emulsion it appeared initially to have been made by an atomic nucleus with a charge, and hence an atomic number, of about 80 and a velocity of about .5 *c*; the Cerenkov detector confirmed that the velocity was below .68 *c*. Such particles are by no means common in cosmic rays, but occasionally they are observed. The Lexan sheets, however, seemed to record the passage of a much different particle. If it was a nucleus, its charge and atomic number had to be at least 125 and its velocity could not be less than .92 *c*. Even aside from the contradictory evidence of the other detectors, it seemed unlikely that such a particle would be found in cosmic rays. The most massive nucleus previously observed was that of curium, with an atomic number of 96; heavier nuclei are so unstable that they could not survive long enough to reach the earth.

The tracks in all three detectors can be accounted for if the particle is not a nucleus but a monopole with a magnetic charge 137 times as strong as the electric charge of the electron. Its velocity cannot be determined from the Lexan sheets, except that it must be high enough for the particle to have penetrated all 33 layers. The track in the photographic emulsion suggests a velocity of about .5 *c*. One unanticipated consequence of this interpretation is that monopoles must be very massive, perhaps 500 times as massive as the proton.

The existence of monopoles was predicted in 1931 by P. A. M. Dirac. He demonstrated that such particles are not forbidden by any of the provisions of quantum mechanics, and on that basis alone their existence seemed mandatory. He also showed that if monopoles exist, an aspect of nature that had seemed quite arbitrary could be explained in more fundamental terms. Electric charge appears only in integral multiples of the electron's charge, and the unit of that charge is related to two universal constants, *c* and the smallest unit of angular momentum, $\hbar$ (Planck's constant divided by 2π), by a constant of proportionality equal to 137. By setting the magnetic charge of the monopole at 137, the relation between electric charge and the universal constants could be made unitary.

Since Dirac made his prediction several techniques have been applied to the search for monopoles. At most particle accelerators attempts have been made to manufacture them; if they are as massive as the track found by Price and his colleagues implies, the failure of those attempts is readily understood. It has been supposed that monopoles reaching the earth might come to rest near the surface, particularly in magnetic materials such as iron ores and manganese nodules on the sea floor. Attempts to extract them from those materials have failed, as have efforts with lunar rocks and meteorites. Finally, monopoles passing through minerals such as mica ought to leave damage tracks similar to those Price and his colleagues observed in Lexan. No such tracks have been discovered.

If monopoles are shown to exist, other

theories must take account of them. Perhaps most important, monopoles must have some effect on quantum electrodynamics, the modern theory of electromagnetism. Quantum electrodynamics without monopoles is quantitatively the most successful theory in the history of physics, having been verified with great precision both in the realm of subnuclear events and on the scale of planetary astronomy. It is not obvious how monopoles could be incorporated in it without impairing its predictive power.

The existence of monopoles also sets a constraint on the theory that explains some subatomic particles as composites of more elementary things called quarks. The quarks are thought to have fractional charges, and if they do, the existence of monopoles requires that a quark never be seen in isolation; quarks must remain permanently bound in particles with integral charge. As it happens, the confinement of quarks has recently been proposed for quite different reasons [see "Quarks with Color and Flavor," by Sheldon Lee Glashow, page 38].

Given the theoretical standing of monopoles, their discovery would be welcome. The particle track observed by Price and his colleagues cannot, however, be considered unequivocal evidence that they exist, mainly because it pertains to only one event. In particle physics there are innumerable phenomena seen only once that remain without explanation. Confirmation of the discovery is therefore essential. The best confirmation, of course, would be to capture a monopole—as Price has said, to bring one back alive.

Working under Pressure

Simulated dives carried out in a water-filled high-pressure chamber at the University of Pennsylvania have demonstrated that men can work effectively at depths as great as 1,600 feet below the surface of the ocean, or more than twice the depths previously achieved in actual working dives in the oil fields of the North Sea. The four volunteers in the experiments were subjected to pressures as high as 712 pounds per square inch, or nearly 50 times normal atmospheric pressure. The record for a simulated dive in a dry chamber is 2,001 feet, achieved by two French divers three years ago. The record for an open-sea dive, 1,148 feet, was set in the Gulf of Mexico this past June by a U.S.-British naval team.

The recent tests at the University of Pennsylvania's Institute for Environmental Medicine differed from the dry-chamber record and the open-sea record in that the volunteers carried out typical maintenance tasks with a six-foot, 2,400-pound "Christmas tree," or wellhead, in a chamber filled with water. The volunteer subjects were "taken down" in 40 minutes to a pressure equivalent to that at 800 feet, where they remained for two hours in order to allow their tissues to become saturated with the helium contained in the oxygen-helium breathing mixture. In another 40 minutes they were subjected to a pressure equivalent to that at 1,200 feet, where they remained overnight. The next morning they were subjected to a pressure equivalent to that at 1,600 feet in only 20 minutes. After "working" for 55 minutes under close observation at 1,600 feet they were returned quickly to the 1,200-foot level. This technique, termed saturation-excursion diving, enabled the volunteers to reach their maximum depth in less than half the time required in any previous laboratory or open-sea experiment. So far, however, no way has been found to speed up the decompression process, which from 1,200 feet takes eight days. The lengthy period is required to flush helium out of regions such as the lens of the eye and the fluids of the inner ear, which have the poorest blood supply. (It has been known for many years that helium or some other "safe" gas must be used in place of nitrogen in the breathing mixture supplied to deep-sea divers because nitrogen under pressure is a narcotic.)

In simulated dives to 1,200 feet the volunteers, who "descended" in pairs, initially exhibited muscular weakness, tremor and lack of coordination, accompanied by mental depression, nausea and drowsiness. It was found, however, that these effects wore off rapidly and that by the next morning the volunteers felt essentially normal. On being further compressed to a simulated depth of 1,600 feet they experienced no adverse effects. All four volunteers carried out their assigned tasks successfully; two of the four performed at least as well as they had in tests at sea level.

The simulated dives, part of a program begun six years ago to investigate the basic limits of human tolerance and performance in undersea activity, was supervised by Christian J. Lambertsen, director of the Institute for Environmental Medicine.

The River Blindness

There are villages in tropical Africa and Central America where as many as 15 percent of the people are blind. They are victims of "river blindness," a frequent complication of the parasitic disease onchocerciasis, which is estimated to afflict more than 20 million people and which has recently been recognized as a major public-health problem throughout the tropical world. Like most parasitic infections, it afflicts people far removed from medical care, and so its true extent, the details of its epidemiology and even the precise mechanism by which it causes its effects are not well understood. At a meeting in Washington sponsored by the Pan American Health Organization and the World Health Organization specialists in the disease and its control summarized what is known about the infection, what remains to be learned about it and what might be done to control it.

The agent of onchocerciasis is a threadlike worm, *Onchocerca volvulus,* that inhabits the skin and the connective tissue. The males are several inches long; the females may be more than a foot. Many of the worms become encysted in fibrous nodules that are unsightly and sometimes painful but not usually dangerous in themselves. The worms produce embryos, however: microfilariae about a third of a millimeter long. The microfilariae get out of the nodules and move through the skin, causing a wide variety of unsightly and disabling skin changes. Entering the lymph, they infest the lymph glands and cause swelling and elephantiasis. From the lymph they enter the bloodstream and invade the internal organs, where they can apparently interfere with normal functioning. In particular the microfilariae, usually moving directly from nodules on the head, invade the tissues of the eye, where they impair or destroy sight by causing inflammation and opacity of the cornea, by somehow inducing glaucoma or cataract and by other processes.

The worm's intermediate host and the vector of the disease, depending on the locality, is one or another species of the black-fly genus *Simulium.* A female fly bites an infected person, ingesting scores of the microfilariae; the embryos develop in the fly for a few days; the fly bites again and the cycle is renewed. The flies breed in rivers and fast-flowing streams, and so it is that the disease is endemic in river basins and on well-watered savanna. As a result, as Alfred A. Buck of the WHO pointed out at the Washington meeting, in many areas "where water is one of the most precious elements of life the fertile valleys are abandoned because of river blindness."

Onchocerciasis can be treated by excision of the nodules or by chemotherapy. Nodulectomy does not get all the worms, and the two available drugs (one for the worms and one for the microfilariae) have serious side effects. Developing new drugs for a disease that afflicts poor societies is an uneconomic business and will require support from international agencies, a study group at the meeting reported. The most promising approach to the control of onchocerciasis, however, is the application of effective larvicides to the breeding waters of the flies that transmit it.

Lost Wheels

A major technological difference between the Old World and the New in pre-Columbian times was the New World's ignorance of continuous rotary motion and its mechanical applications: the millstone, the wheeled vehicle and the potter's wheel. In the New World grain was milled by crushing it between a stone roller and a stone slab or by pounding it; the only vehicles were sleds and improvised skids; pottery was painstakingly fashioned out of coils of clay. Such, at least, had been the prehistorian's view until a few seasons ago, when Terence Grieder of the University of Texas at Austin excavated a burial in the Peruvian highlands 250 kilometers north of Lima. The grave, near the pre-Columbian settlement of Pashash, was that of an aristocratic woman who had been buried in the latter half of the fifth century along with a wealth of grave goods. Among the buried offerings were scores of wheel-turned clay pots.

The Pashash pots, Grieder reports in *Archaeology,* were roughly hemispherical cups with a low, flared pedestal foot. The cup and the foot were turned separately on a wheel; a circular channel was then cut into the base of the cup to receive the pedestal. After the cup and the foot were assembled the potter spun his wheel again, the interior of the foot was smoothed (perhaps with a cloth) and a background color was brushed on the surface of the rotating cup. The cups from the grave are the same shape as ordinary Pashash household ware, but the household pottery shows no evidence of wheel turning.

Among the grave offerings Grieder also found 15 stone cups that had evidently been turned on a lathe. As he reconstructs the process, a stone blank was mounted rigidly on a shaft that also bore a flywheel. A length of cord was then wrapped around the shaft and quickly pulled off, so that the weighted shaft and the blank rotated at considerable speed. The unwanted stone was then removed with a fixed bit. On neither the wheel-turned clay pieces nor the stone cups are there any toolmarks indicating use of a bow drill as a source of power; such a method, of course, produces rotation first in one direction and then in the other.

Because the grave goods and other examples of Pashash lathework are ceremonial, Grieder proposes that both techniques of rotary motion were confined to the production of a few objects for an elite class. Some 200 years later Pashash craftsmen had abandoned both the wheel and the lathe, and a revolutionary New World technological advance vanished without a trace.

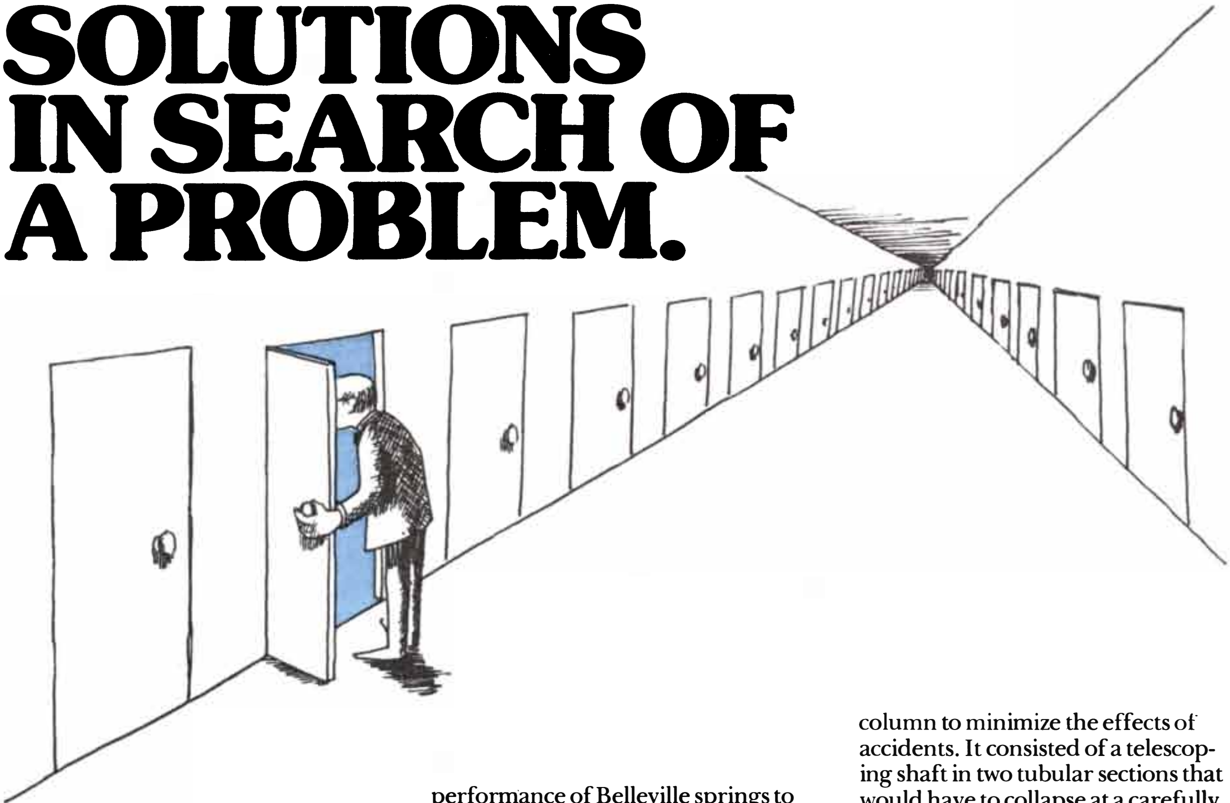
Clean Slate

Graffiti—amateur inscriptions and decorations in public places—can be regarded as an intolerable nuisance or untrammeled self-expression. From either point of view the need is for clean surfaces; otherwise the public official loses all hope of restoring walls and conveyances to their original condition and the graffito artist runs out of canvas. The National Bureau of Standards has looked into the matter and puts forward a reasonable solution: Coat the surface in the first place with a special substance from which markings can be removed easily. The study is described in *Dimensions,* a bureau publication.

The bureau undertook the study for the U.S. Department of Housing and Urban Development, which was concerned about graffiti in Federally financed public-housing developments. First the bureau investigated commercially available products that can remove graffiti. After a preliminary screening that eliminated more than two-thirds of the potential graffito removers, the bureau tested 24 products and finally chose five (all of them liquid paint removers) that erased the largest variety of spray-paint markings from brick and other common surfaces.

Then the bureau looked into preventive coatings. The investigation turned up three that resist permanent bonding of most of the common types of marking, two that are highly resistant to spray paint and four that resist crayon, felt-tip pens and lipstick. The three products that proved to be most broadly successful are generically classified as a urethane, a dimethyl silicone and a styrene acrylonitrile terpolymer. McClure Godette, a chemist who worked on the project, said of them: "These coatings cost just slightly more than a coat of paint for the same surface area, and they can be useful in making any future graffiti defacement easier to clean up."

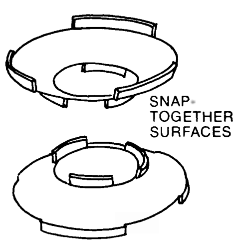
SOLUTIONS IN SEARCH OF A PROBLEM.



In a rational world, first comes the problem then the solution. Right? Right. . . but not always. Sometimes the solution comes first and the problem has to be searched out. A 100-year-old idea, for example, can be adapted to break through technological limitations imposed by older engineering materials. Which brings us to our story.

Merci, M. Belleville

Back in 1866 one Julien Belleville patented a coned metal disc spring that we now call, to no one's surprise, a Belleville spring. It has a convex saucer shape and a center hole. Under load it flattens and returns to its original shape when the pressure is off, even as you and I.

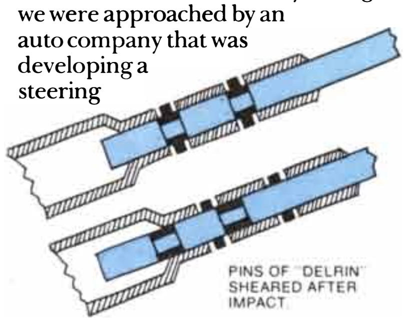


So, we've been working on Belleville springs made of an engineering plastic – Delrin® acetal resin. We've designed springs of "Delrin", molded them as individual discs and as large sheets with *snap-together surfaces*. And we've even extended the performance of Belleville springs to other geometries, thanks to another pretty good invention, the computer.

We've got a lot of answers – for possible use in door closer springs, springs in switches, relays and push buttons, furniture innersprings and, from one of our engineers who thinks young, pogo sticks. Maybe you have a new or even old idea that can expand in function and usefulness when it's matched with our engineering plastics technology.

Born to fail

That isn't the name of a soap opera. Rather it describes an unusual technique: planned failure. We've always known that "Delrin", because of its reproducible properties, could be designed to fail at a predetermined stress. Then several years ago we were approached by an auto company that was developing a steering column to minimize the effects of accidents. It consisted of a telescoping shaft in two tubular sections that would have to collapse at a carefully predetermined stress.



Together with the company we worked out the technology of injection molding "Delrin" into the tubular sections as part of the mold. The design depended on "Delrin" shear pins and bearing collars.

And so we have another apt illustration of a solution looking for a problem and in this case finding it.

Invitation to a Dialog

Surprising breaks with tradition can happen when you look at your problem and our engineering plastics technology with a new, unrestricted curiosity. If our expertise in the properties and uses of engineering plastics may be of help, you can continue this dialog by either writing Dick Johannes, Du Pont Co., Plastics Department, Rm. D-13064, Wilmington, DE 19898 on your company letterhead, or calling Dick at (302) 774-5826.

DU PONT
REG U S PAT OFF

IMAGE RECONSTRUCTION FROM PROJECTIONS

A mathematical technique makes it possible to use a series of X-ray exposures made from different angles to reveal the internal organs of the body in cross section instead of superimposed on one another

by Richard Gordon, Gabor T. Herman and Steven A. Johnson

Until quite recently the physician has had at his disposal only a few means of gaining information about the three-dimensional location and arrangement of normal and diseased organs and tissues within his patient's body. His sense of touch, conventional X-ray pictures, radioactive tracers and exploratory surgery are all useful but have distinct limitations. Today the practice of medicine is entering an era in which the internal structures of the body are being made far more accessible by noninvasive procedures. Such procedures may effect a revolution in medicine comparable to that brought about late in the 19th century by the introduction of anesthetics and sterile techniques. One particular noninvasive procedure, called reconstruction from projections, is coming into service in hospitals throughout the world. It is a technique of mathematically combining X-ray images made from numerous angles into images in three dimensions of the organs within the body.

In conventional radiography X rays diverge from a source and pass through the body, projecting an image of bones, organs, air spaces and any existing tumors onto a sheet of film. Invaluable though the procedure is, it suffers from a major shortcoming: on the film the structures overlap and are sometimes difficult or impossible to distinguish from one another. This is particularly true when the X-ray density of one structure differs only slightly from the density of a neighboring one, as is often the case with a tumor and the tissue in which it is embedded.

In attempting to surmount this shortcoming the physician often makes a number of X-ray pictures from different angles, in which the internal organs appear in different relationships to one another. This qualitative procedure is now being replaced by the new quantitative, mathematical technique that combines X-ray pictures to yield a representation of the internal structure in three dimensions. With such vital information available diagnosis becomes more accurate, and more precise guidance can be given to the hand of the surgeon and to therapeutic radiation aimed at a tumor.

The mathematical methods for reconstructing images from projections are being applied in a broad range of endeavors outside medicine. For example, in microscopy and in industrial nondestructive testing three-dimensional internal structure has been reconstructed from projected images made not only with X rays but also with light, electrons and protons. In astronomy similar methods are used to reconstruct images in two dimensions of celestial objects from their radio and X-ray signals [see "Giant Radio Galaxies," by Richard G. Strom, George K. Miley and Jan Oort; SCIENTIFIC AMERICAN, August]. Analogous processes may well underlie the way we perceive the world visually.

A Rediscovered Method

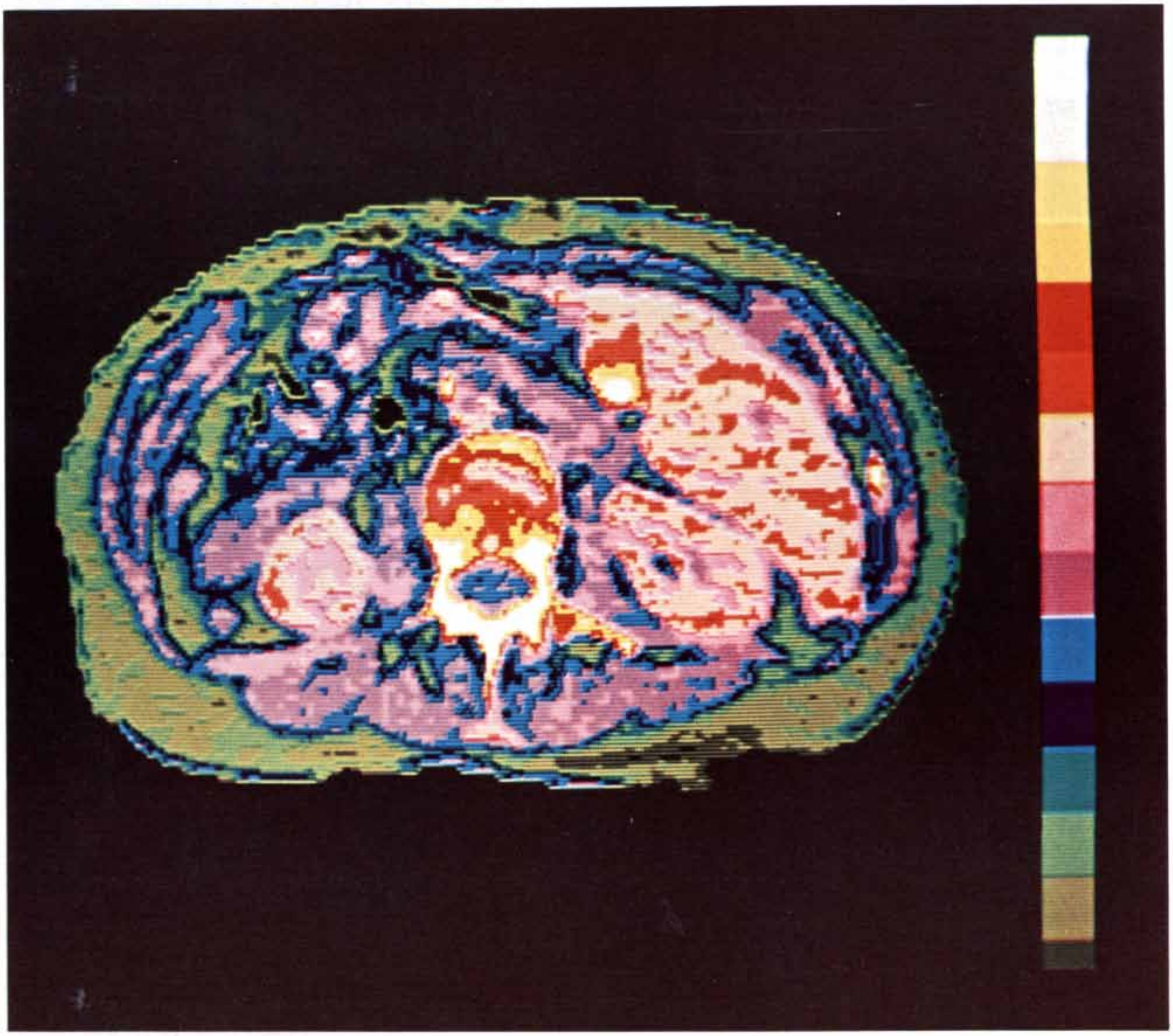
The history of these reconstruction techniques began in 1917 with the publication of a paper by the Austrian mathematician J. Radon, in which he proved that a two-dimensional or three-dimensional object can be reconstructed uniquely from the infinite set of all its projections. This result has been repeatedly rediscovered by mathematicians, radio astronomers, electron microscopists and medical radiologists. As early as 1922 several radiologists independently devised another X-ray technique for determining three-dimensional structure, a technique that is quite distinct from reconstruction from projections. This technique, known as tomography, is now practiced in every major hospital and has been the traditional method of obtaining three-dimensional information. In most tomographic instruments the X-ray source moves in one direction and the photographic film simultaneously moves in the opposite direction. The patient lies in between. If the patient's body is regarded as a series of planes parallel to the film, there is only one plane whose projected image remains stationary with respect to the film as the film moves. Hence that plane remains sharply focused on the film and the others are blurred.

If Radon and the early tomographers had been aware of their common problem, many of the developments of the past few years might have been launched half a century ago. On the other hand, the full exploitation of reconstruction techniques would have had to await the arrival of modern computers, because the number of computations required for each reconstruction is formidable.

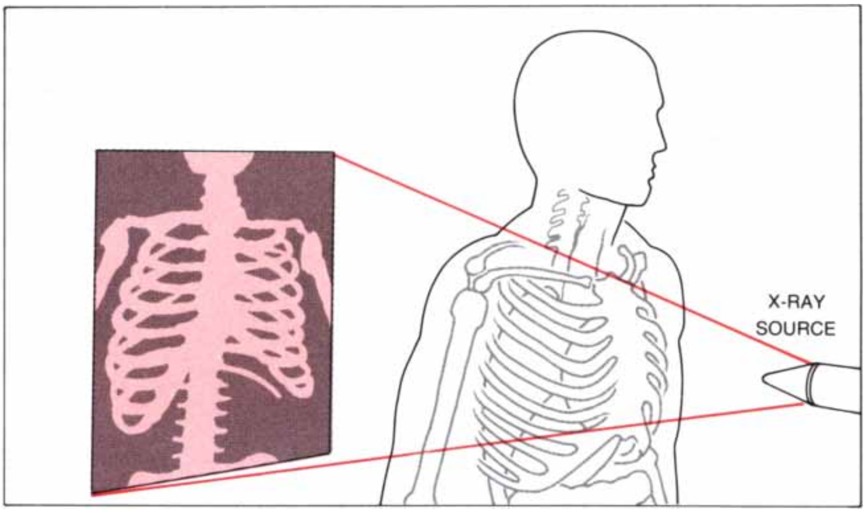
To appreciate how many computations are needed, let us consider the problem of reconstructing the anatomy of the brain within a cube 20 centimeters (eight inches) on a side. To represent the small details, we may want to have a resolution of two millimeters, which means that we would reconstruct the brain tissue as an array of small cubes each two millimeters on a side. We would then have the brain represented in a large cube consisting of $100 \times 100 \times 100$, or a million, small cubes. Even if a high-speed digital computer took only a twentieth of a second to perform the computation for each small cube, more than half a day would be required to complete the computations.

Fortunately there are ways to greatly simplify the procedure. For example, the X rays can be directed so that they pass through the patient's body in parallel rays as the patient (or the apparatus) is rotated in steps around a single axis. If a photographic image were made at each step, that is, for each projection, structures in the patient's body lying in a plane perpendicular to the axis of rotation would be recorded as a single one-dimensional line. By measuring the X-ray density along that line on each image we isolate the information from the desired plane. Thus we can reconstruct the single two-dimensional plane and merely stack a sequence of such planes to get a full three-dimensional picture. The reduction of the computation time results from our ability to isolate a few planes of interest. The technique of reconstructing a three-dimensional picture therefore comes down to the technique of reconstructing a two-dimensional picture, or cross section, from its one-dimensional projections.

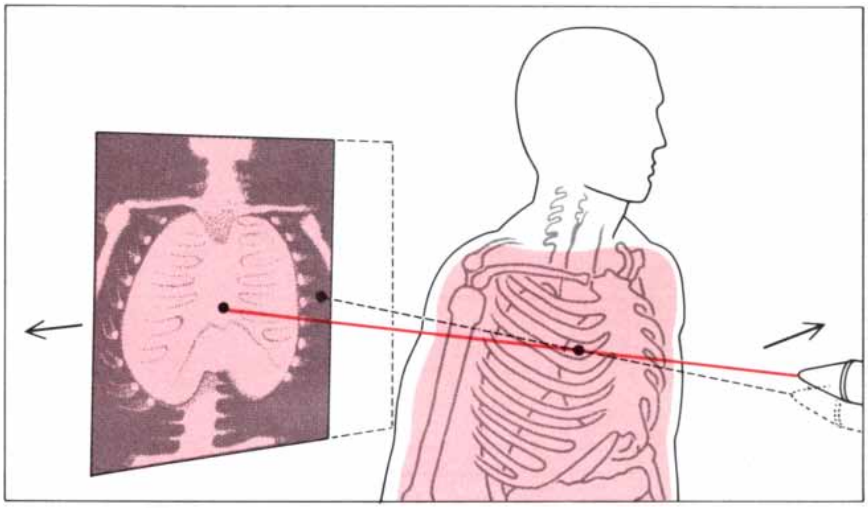
When any method of reconstruction



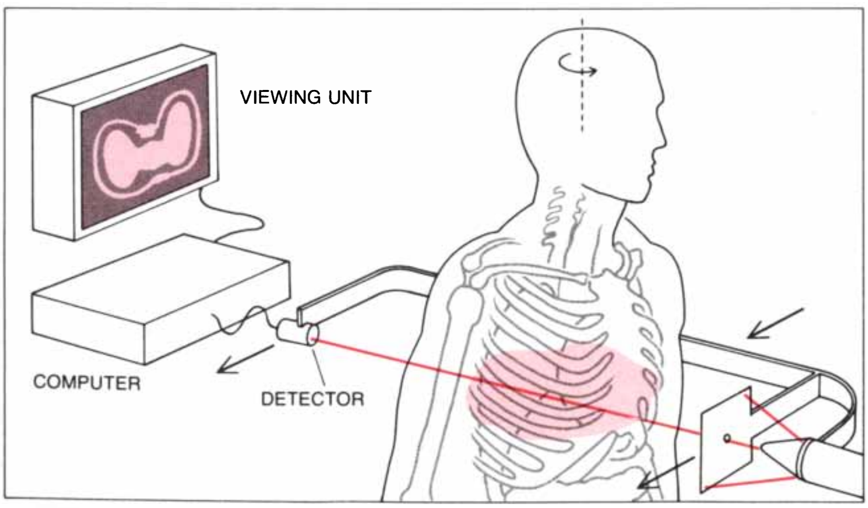
CROSS SECTION OF ABDOMEN of a living human subject appears in this X-ray picture made by reconstruction from projections. This section is seen from the same perspective as the one on the cover of this issue: from above the subject's head. The vertical bar at the right is the key to the X-ray density of the tissues in the section; white represents the greatest density, black the least density and the colors the densities in between. The white areas below the center of the section are the spinal column. The large light-colored area running from upper right to lower right is the liver. The light area near the upper end of the liver is the gall bladder, which here contains an X-ray opaque dye for diagnostic purposes. The two roundish areas to the left and right of the spinal column are the kidneys. The small dark areas extending from the top around to the lower left are sections through loops of intestine. This picture, like the one on the cover, was made by the Delta Scanner built by Ohio-Nuclear in the course of a study by Ralph J. Alfidi, M.D., of the Cleveland Clinic Foundation. The picture required 180 separate X-ray projections, made at one-degree intervals around a half-circle. Total dose of X rays was approximately the same as that needed to make a single conventional X-ray plate.



CONVENTIONAL X-RAY PICTURE is made by allowing the X rays to diverge from a source, pass through the body of the subject and then fall on a sheet of photographic film.



TOMOGRAM is made by having the X-ray source move in one direction during the exposure and film in the other direction. In projected image only one plane in body remains stationary with respect to moving film. In the picture all other planes in body are blurred.



RECONSTRUCTION FROM PROJECTIONS is made by mounting the X-ray source and an X-ray detector on a yoke and moving them past the body. The yoke is also rotated through a series of angles around the body. Data recorded by detector are processed by a special computer algorithm, or program. Computer generates picture on a cathode-ray screen.

is applied to a finite amount of real data (in contrast to the infinite number of mathematically precise projections required by Radon's theorem), it yields reconstructions that are at best only estimates of the object's actual structure. Moreover, the relative accuracy of various mathematical methods has been found to depend on the nature of the data collected. To test the accuracy of a particular method we can reconstruct an object whose structure is already known. That is, we can take any two-dimensional picture, regard it as a "test picture," calculate its one-dimensional projections at various angles, perform the reconstruction and compare the reconstructed picture with the original.

The Summation Method

A rough but nonetheless elegant method of obtaining an approximate reconstruction is the summation method. Because of its simplicity it can be utilized photographically or even with a pencil and a straightedge. In the summation method the density of each point in the reconstructed picture is obtained by adding up the densities of all the rays going through that point. For example, if the test picture consists of a single point and three projections of it are made, the reconstruction is a six-pointed star [*see illustration on opposite page*]. The star demonstrates the roughness of the summation method: it is the "point-spread function" of that particular process of reconstruction. An exact method of reconstruction would reconstruct a point as a point and not as a star. It should be mentioned, however, that a method that succeeds in reconstructing a single point as a point is not necessarily an exact method for reconstructing more complex pictures.

One-dimensional projections of more complex test pictures can be made photographically by moving the test picture across a piece of film at a certain angle while exposing the film to light [*see top illustration on page 60*]. The projection is recorded as a blurred set of parallel streaks across the film, with the density of each streak corresponding to the integrated density of the picture at that angle. One can then photographically add up the streak pictures on another sheet of film. The accuracy of the reconstruction is improved as additional streak pictures are included.

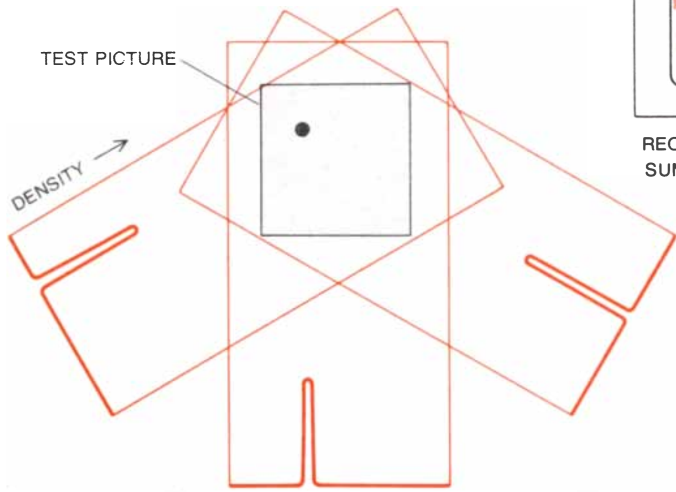
The streak pictures can themselves be approximated by a set of parallel lines whose spacings are inversely proportional to the projected density of the test picture [*see bottom illustration on page 60*].

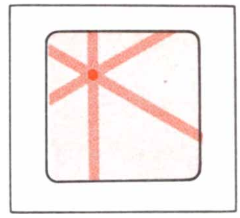
Pictures generated by the superposition of lines are moiré patterns. We now see that any picture, through its approximate reconstruction by the summation method, can be generated as a moiré pattern.

In addition we can see that each point in a classical tomogram is actually the sum of the rays passing through the corresponding point of the plane in the sharpest focus. Thus the tomographic image is formed by a process that is mathematically identical with the summation method. Since hospitals have already made a considerable investment in tomographic equipment, it may prove worthwhile to apply reconstruction methods in order to improve the quality of classical tomograms.

For the purpose of obtaining three-dimensional information that is quantitatively accurate for medical applications there are two major objections to employing the summation method for the reconstructions. First, the reconstruction is inaccurate because every point in the original is blurred in the reconstruction by the star shape of the point-spread function. Second, if we take the reconstruction and calculate its projections, we find that they are not the same as the projections of the original picture. In order to overcome both disadvantages an algebraic method has been devised. The method, known as ART (Algebraic Reconstruction Technique), is utilized with a digital computer.

How can pictures and their projections be represented in a computer? A picture can be stored as a two-dimensional array of numbers, each number representing the X-ray density of one small square in the picture. Such a picture element is called a pixel. A one-dimensional projection of the picture can likewise be stored as a one-dimensional list of numbers. Just as the darkness of each streak in the streak pictures represents the density of the original test picture along a certain line, so each number on the one-dimensional list in the computer represents the total X-ray density of the pixels across a narrow strip of the picture. That density is called the ray sum.





RECONSTRUCTION BY SUMMATION METHOD

SUMMATION METHOD is a rough technique for reconstructing images from a series of projections. Here three projections are made of a simple two-dimensional test picture containing a single point. Each projection is a one-dimensional distribution of the density, or darkness, across the test picture as it is seen from a specific angle. In the case of this test picture the projection looks the same from all directions. The picture can be reconstructed from the projections: the density of each point on the reconstructed picture is estimated by adding up the densities of all the rays going through that point. The reconstruction of the single point is a "star," or spokelike image. The star is the "point-spread function" of the reconstruction technique. It approximately demonstrates the nature of summation method.

The ART Algorithm

The ART algorithm, or computer program, is iterative: it begins with an initial estimate for the two-dimensional array of numbers that represents a reconstructed picture, and then it repeatedly modifies the estimate until the density values stop changing. One possible initial estimate is an array of zeros—a picture that is a complete blank.

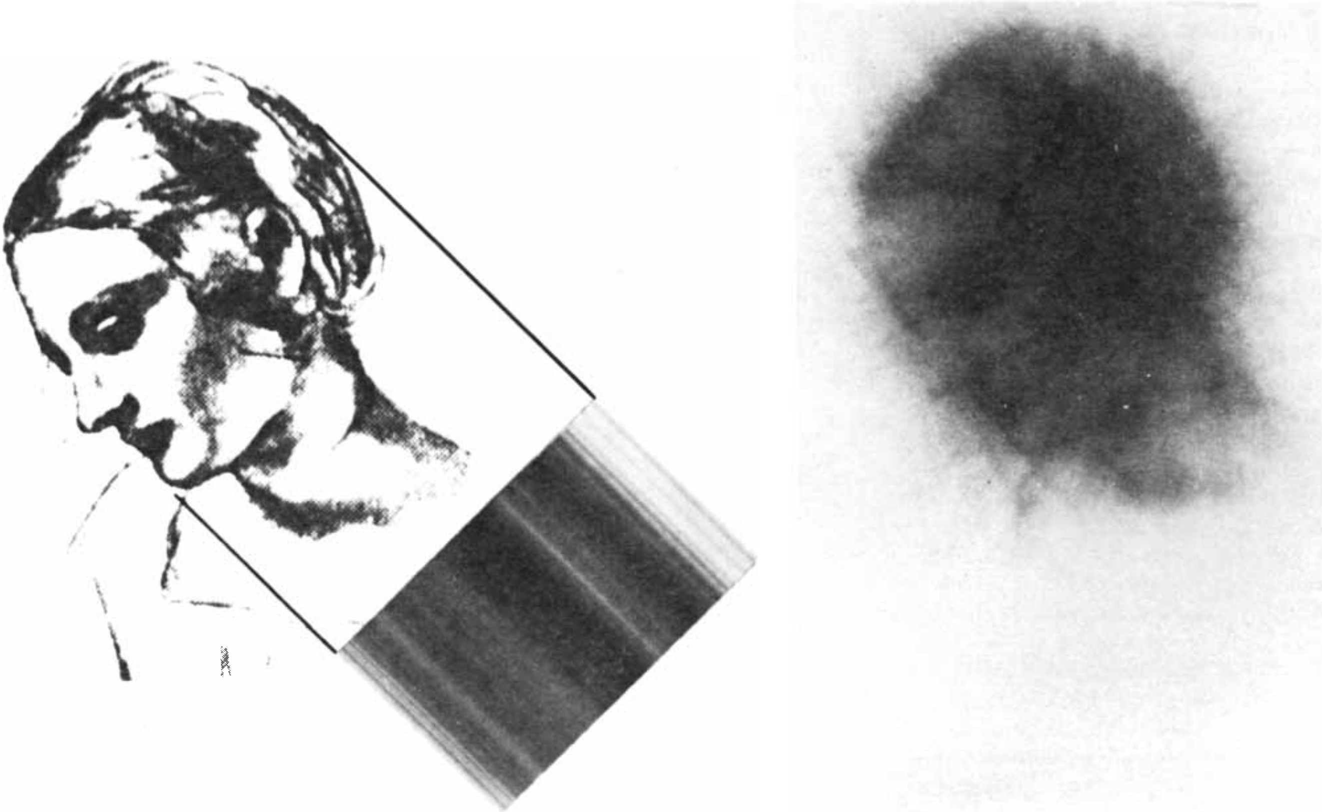
Let us imagine that we are beginning an iteration. We pick one ray, or strip, across the estimated picture and calculate the corresponding ray sum. Then we compare that ray sum with the ray sum obtained from the original projected X-ray image. The two ray sums will usually differ. We calculate the difference and divide it among all the pixels intersected by the ray. The modified ray sum of the estimate now matches the ray sum of the original. Since the rays of one projection cross the rays of another projection, each computation partially undoes the matching of the ray sums by the preceding computations. It can nevertheless be shown mathematically that if under ideal conditions the corrections are made over and over again, the picture will approach a reconstruction whose ray sums are identical with the ray sums of the projections from the original object.

During a given iteration one may encounter negative values for the pixels. Since it is physically impossible for an X-ray density to be less than zero, we can constrain the values of the densities available to ART by making all negative values equal to zero. Problems arise when such constraints are introduced; in particular, a picture reconstructed by constrained ART is not equivalent to the superposition of the point-spread functions (the stars) of the individual points in the original object. We thus lose mathematical elegance, although in return we gain precision: constrained ART produces more accurate reconstructions than its unconstrained version.

There are many other physical constraints that can be incorporated into the computation. For example, we may know in advance the range of X-ray densities of a specific kind of tissue such as bone even though we do not know its precise location. In that case we can alter the ART program to meet specific needs: once the bones are located we can constrain their pixel densities so that they lie within the known density range and proceed with a more accurate computation for the distribution of the X-ray density in the soft tissues.

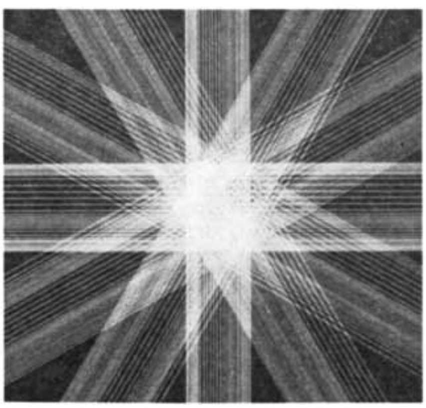
With any instrument operating in the real world one cannot measure the ray sums exactly. There are always experimental errors and fluctuations in the number of X-ray photons. These errors and fluctuations show up in the data as noise. One way of overcoming the difficulty is not to insist that each ray sum be satisfied precisely but only to require that it be satisfied to within a certain tolerance. One version of the algebraic reconstruction technique, ART3, has been designed for this purpose.

There are many methods in addition



COMPLEX PICTURE CAN BE RECONSTRUCTED with a photographic analogue of the summation method devised by B. K. Vainshtein of the Institute of Crystallography in Moscow. The projection of the picture is made by moving a sheet of film across it as the film is exposed to light. The result is a "streak picture," a set of parallel lines whose darkness depends on the total density of the original picture along each line. A series of such projections can be made at various angles. The reconstruction is obtained by superposing the streak pictures photographically. Reconstruction at right was made with 18 projections spaced at intervals of 10 degrees.

to the algebraic reconstruction technique for reconstructing images from their projections. One, known as the convolution method, can be derived from Radon's original formula. It is essentially a modified form of the summation method, and it lends itself to rapid computation. The reconstructions can also be computed optically or on a specially designed electronic image tube, thereby eliminating the need for a digital computer. Another reconstruction method is the Fourier method, which involves a transformation of the data from the projections into what is known as Fourier space. Points in Fourier space for which there are no data are estimated by interpolation. The reconstruction is then obtained from the Fourier space by taking the inverse Fourier transform. Neither the convolution method nor the Fourier method, however, readily allows the introduction of constraints.



SETS OF PARALLEL LINES can approximate a streak picture. The spacing of the lines is made closer for higher densities and wider for lower densities. This computer-generated set of lines reconstructs the faint outline of the letter *G*. Such a reconstruction is a type of moiré, or interference, pattern.

From Mathematics to Medicine

How are these mathematical methods made into tools for medical diagnosis? First, an instrument is needed to collect the data from the X-ray projections. Such data could in principle be obtained from a set of ordinary X-ray images. However, making certain that the images are properly aligned and then transferring the data from the films into a computer is a major undertaking. For this reason a number of special-purpose instruments with built-in computers have been devised. Most of these instruments reconstruct cross-sectional planes of the patient by directing parallel beams of X rays through his body.

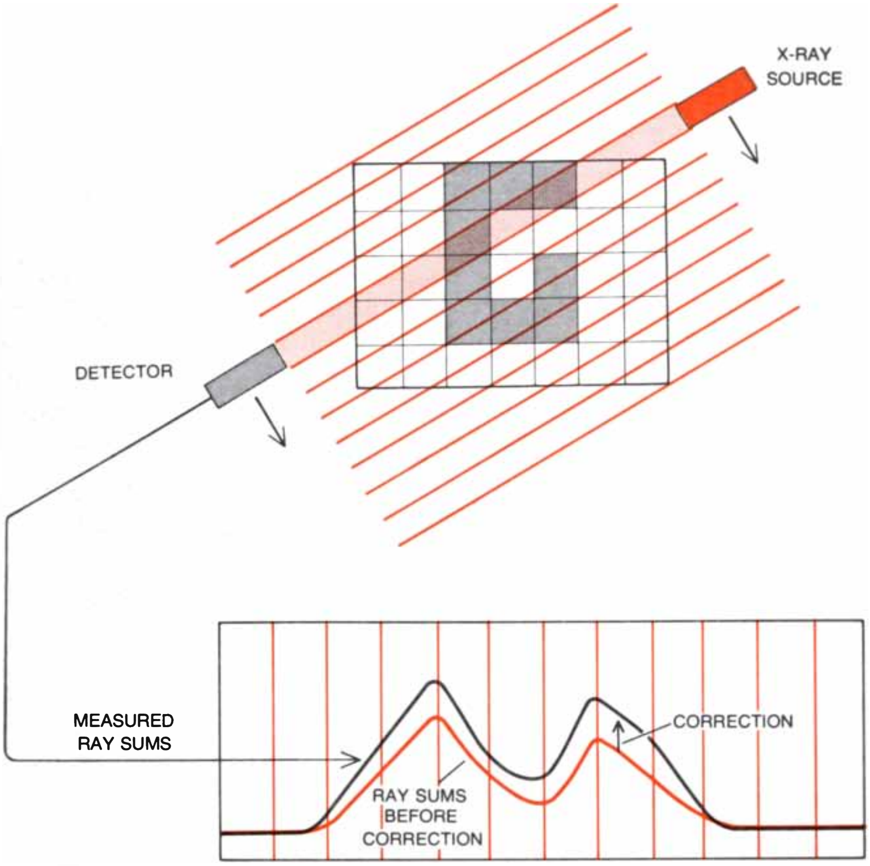
The simplest way to get parallel X rays is to place a collimator made of lead in front of the X-ray source. The X-ray detector is on the opposite side of the patient from the source and the collimator. The source, the collimator and the collector then scan across the patient in a direction perpendicular to the beam of X rays.

The first commercially available instrument for the reconstruction of cross-sectional planes through the human body was the EMI scanner, developed at the Central Research Laboratories of EMI Ltd. in England. The machine, largely the work of G. N. Hounsfield, has generally been well received and is being installed in hospitals around the world. The first EMI scanner sold in the U.S. went into service at the Mayo Clinic in June, 1973.

The instrument is designed primarily for scanning the brain. The patient's head is held in an elastic hemispherical cup. The rim of the cup is attached to one side of a watertight plastic box. The box is filled with water, which serves to limit the difference between the maximum and minimum X-ray signals received by the detector. The detector is a sodium iodide crystal that scintillates, or gives off photons of visible light, when it is struck by X-ray photons. The amount of light emitted by the crystal is measured with a photomultiplier tube, which is connected to a small high-speed computer.

An X-ray projection is recorded by sampling the output of the detector at 160 equally spaced positions along a single scan direction. The 160 ray sums for the projection are stored in a magnetic-disk memory by the computer. The entire unit is then rotated one degree around the patient's head to obtain the next projection. After five minutes the data have been collected for 180 projections, one for each degree in a semicircle around the head.

When all the projections have been completed, the resulting 28,800 ray sums (160 times 180) are processed by the computer to form a reconstruction of a cross section of the patient's head. The original EMI scanner used the unconstrained ART algorithm. The computer takes five and a half minutes to calculate the reconstruction, which is then displayed on a cathode-ray tube as a picture consisting of an array of 80 pixels on a side. The latest model displays a picture 160 pixels on a side and apparently uses the convolution algorithm because of its greater computational speed.



ALGEBRAIC RECONSTRUCTION TECHNIQUE ("ART") has been devised to overcome the inaccuracy of the summation method. ART is executed on a digital computer, in which a picture is stored as a two-dimensional array of numbers, each number representing the X-ray density of one pixel, or small picture element (*squares*). A one-dimensional projection of the picture is stored as a list of numbers, each representing the ray sum, or total X-ray density along a ray: a narrow strip of the picture at a certain angle (*colored band*). ART is an iterative method that assigns an initial set of X-ray densities to the two-dimensional picture it is to reconstruct, calculates the ray sum of each point along a one-dimensional projection of the estimated picture, compares that ray sum with the ray sum of the real object stored in the computer, calculates the difference and divides it among all the pixels intersected by the ray. Modified ray sum then matches the original. Operation is repeated for all rays from all projections until a representational picture is reconstructed.

Scanning Economics

Many other companies and university research groups are building scanners, both for experimental purposes and for sale. Performance and price competition among these instruments will be important for the consumer of medical services. The prices of scanners currently range from $200,000 to $700,000 per unit, and a single examination costs the patient about $200.

The newest scanners include an instrument specialized for breast diagnosis and others capable of reconstructing any cross section of the body. The current engineering emphasis is on speed of scanning and reduction of the number of moving parts (by the use of many detectors in combination with a fan-shaped beam of X rays). Commercial companies that have announced instruments include American Science and Engineering, Artronix, EMI, General Electric, Neuroscan, Ohio-Nuclear, Pfizer and Siemens. In many cases development and testing is partly financed by contracts with and grants from the National Institutes of Health.

What the Scanners Reveal

The success of the scanners and of the mathematics on which they are based is evident in a comparison of a cross section of a normal human brain with a corresponding cross section reconstructed from X-ray projections [*see illustration on page 65*]. The ability of the reconstruction technique to distinguish between small differences in X-ray density can reveal abnormalities too subtle to be perceived in a single conventional X-ray picture. Formerly it was necessary to inject an X-ray-opaque liquid into the bloodstream or a bubble of air into the ventricular cavities of the brain in order to obtain sufficiently high contrast between certain areas, procedures that were difficult, time-consuming and sometimes painful to the patient. The reconstructions, on the other hand, can easily distinguish between normal blood, clotted blood, brain tissue, fatty tissue and cerebrospinal fluid. The joint use of contrast liquids and reconstruction methods has proved to be a powerful diagnostic tool for special cases.

The noninvasiveness of the reconstruction technique, what its images can reveal and the absence of any undesirable side effects associated with its use make it feasible to apply it to the examination or screening of patients in order to detect tumors and other abnormalities at a much earlier stage of development than would be possible with more traditional diagnostic methods. The reconstruction technique also provides a much more accurate means of evaluating the

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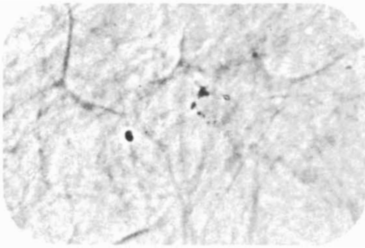
Preoperative mammogram (in this case, a xeroradiograph) shows a partially calcified nonpalpable breast lesion.



The suspicious region is biopsied and the excised tissue radiographed with the Faxitron. Because this small lesion is not palpable to the surgeon, immediate x-ray examination of the tissue is essential to show whether or not the lesion has been removed.

The specimen x-ray reveals the calcifications shown in the mammogram, confirming that the surgeon has indeed excised the proper area. In addition, the specimen x-ray identifies calcified sites that may not have been visible in the mammogram.

Specimen x-rays: mandatory adjunct to mammography for early detection of cancer.

At the Virginia Mason Clinic in Seattle, an HP cabinet x-ray permits fast radiographic analysis of excised breast tissue for detection of cancer-indicative microcalcifications.

Specimen radiography has proved to be a "mandatory adjunct to mammography" according to the Virginia Mason Clinic in Seattle, Washington, where pathologists have used the procedure for seven years in detecting and localizing breast calcifications.

Using a Hewlett-Packard 43805 Faxitron cabinet x-ray, pathologists have radiographed more than 800 breast biopsies since 1968, when the unit was donated to the clinic by the Washington State Division of the American Cancer Society. Specimen radiographs from the Faxitron have made possible the detection of calcifications within breast lesions which are not grossly apparent: calcifications as small as 10 to 20 microns can be identified, permitting diagnosis at an early, more curable stage.

Because specimen x-rays can be made quickly during surgery, while the patient is anesthetized, the Faxitron has become important in cases where a mammogram indicates a calcification that is not clinically palpable. Specimen radiography is mandatory in such situations because it is the only way the surgeon, operating on a lesion that cannot be felt, can verify whether or not the correct area has been removed. With the easy-to-operate Faxitron available in the pathology lab, pathology personnel can very rapidly take their own high-resolution films without prolonging the surgery. Diagnosis can be quick because the specimen radiograph enables the pathologist to pinpoint the critical areas for histological examination.

Although primarily used at Virginia Mason Clinic for breast biopsies, the Faxitron also lends itself to examination and detection of bone lesions and vascular lesions of various body organs.

The 43805N Faxitron is certified by Hewlett-Packard to meet the stringent 1975 safety regulations of the Bureau of Radiological Health. Price of the unit is $4100*.

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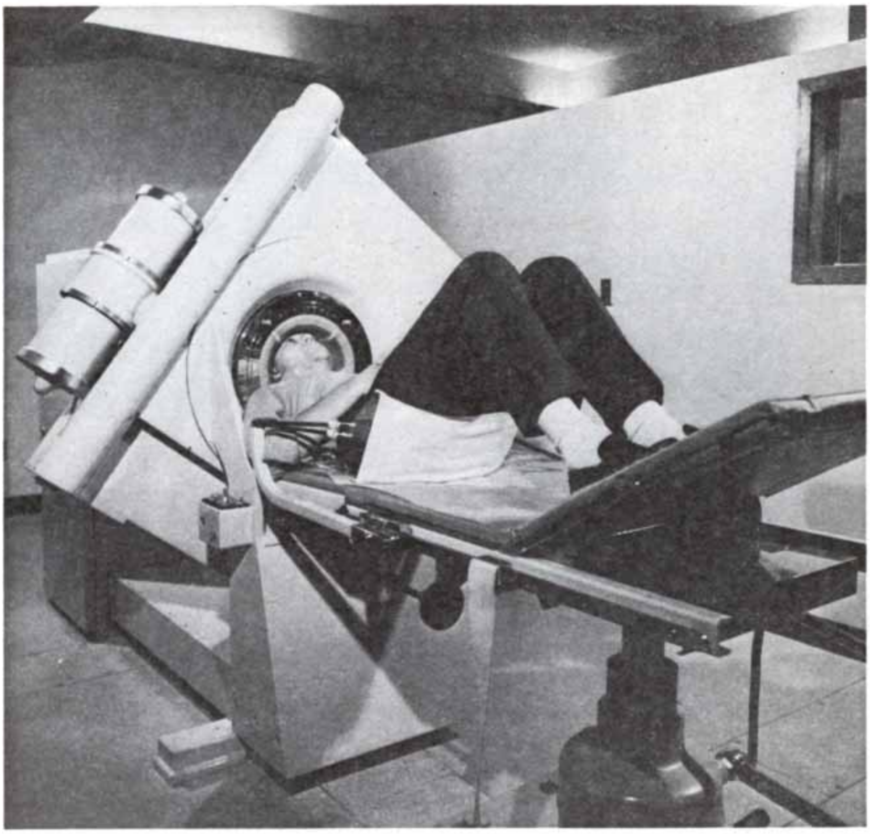
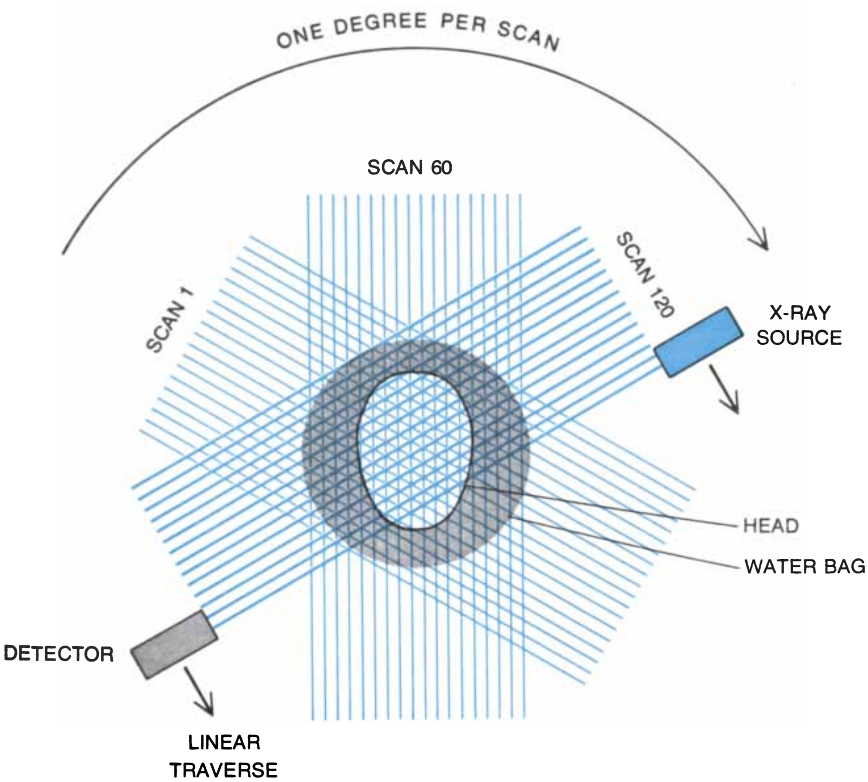


EMI SCANNER, developed at the Central Research Laboratories of EMI, Ltd., in England, was the first commercially available instrument for the reconstruction of planes through the head. The rectangular apparatus surrounding the subject's head is rotated to make the series of X-ray projections. The scanning X-ray source is in the large cylinder at the left.



SCANNER SAMPLES RAY SUMS AT 160 POINTS along each projection; in five and a half minutes 180 projections are taken at one-degree intervals around the patient's head.

patient's condition during and after treatment. The total dose of X rays is equivalent to or less than the dose received in conventional radiography, and by using improved algorithms to reduce the number of projections it may prove possible to decrease it.

With the scanners currently on the market it is necessary that the parts of the body being examined be held immobile. One cannot, however, stop the beating of the heart for the purpose of examination or have the patient hold his breath for five minutes. Under special circumstances there is an alternative approach to making pictures of structures whose motion is periodic. This approach has been taken in experiments with animals at the Mayo Clinic, with only modest modifications in the available clinical equipment.

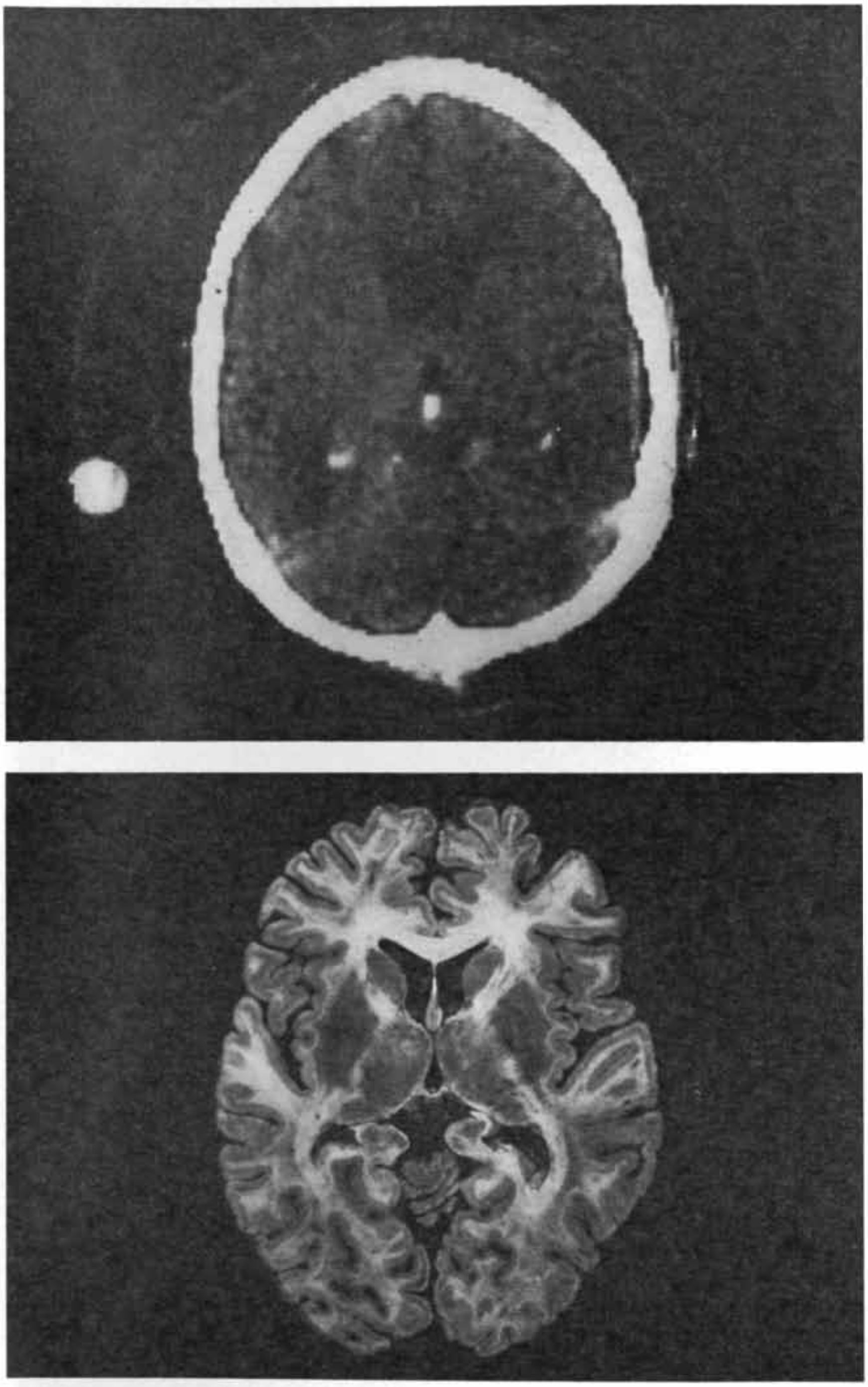
There are two major differences between the geometric arrangement of the apparatus in these experiments and that in the usual scanner. First, the X rays are allowed to diverge in cone-shaped beams from a number of stationary sources and to pass through the entire volume of the tissue being examined, rather than being confined to one plane by a movable source and a collimator. Second, the projection of the tissue is not recorded as a one-dimensional array of X-ray densities but as a full two-dimensional image, with the aid of a medical-X-ray image intensifier coupled to a television camera.

Cross Sections of a Beating Heart

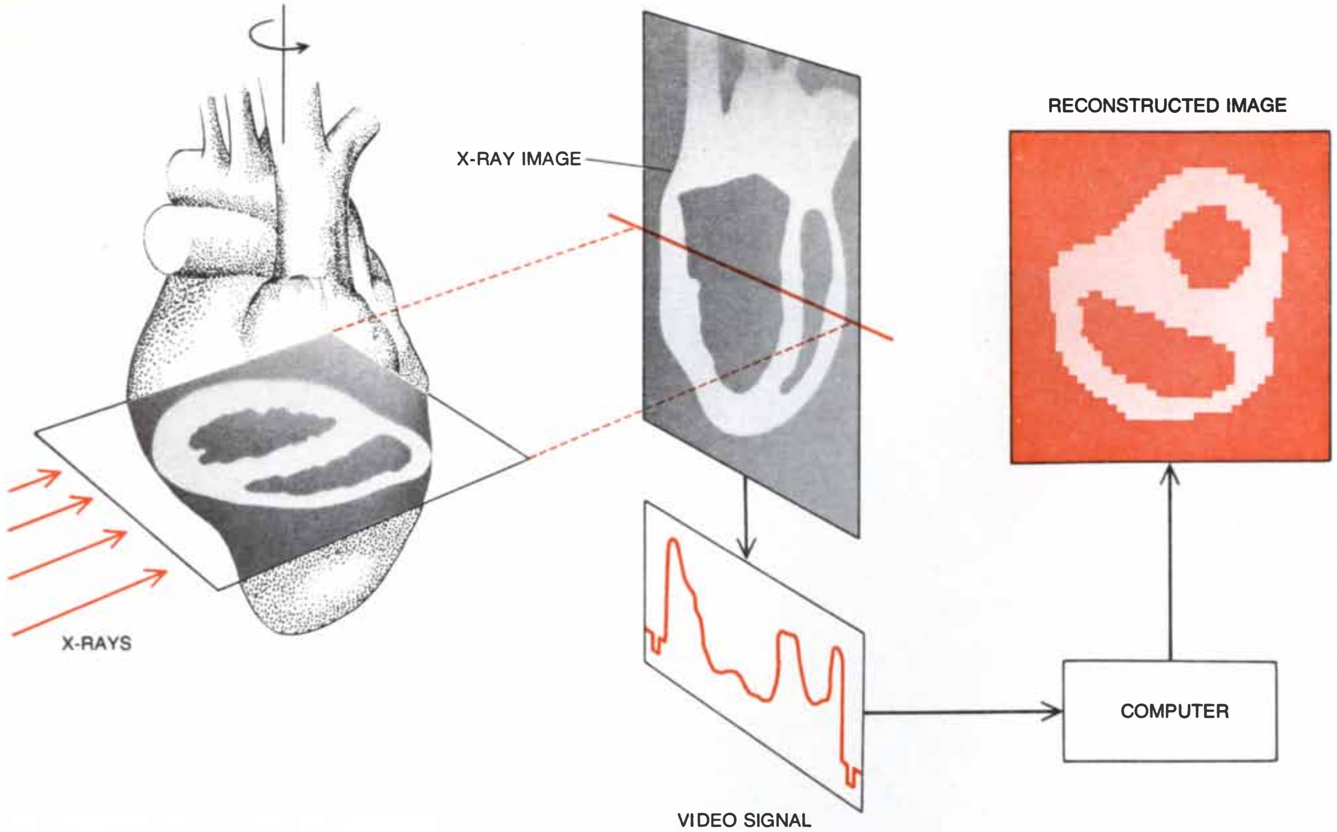
The first of the experimental reconstructions at the Mayo Clinic were done with a preserved isolated heart to compare the reconstructed cross section with the actual cross section. The heart was suspended below a motor and rotated in a cone-shaped beam of X rays [*see top illustration on page 66*]. After getting encouraging results the Mayo group designed an experiment to make full three-dimensional reconstructions of a live, isolated beating heart. The walls of the right ventricle of a dog's heart were removed, leaving only the much more muscular left ventricle. A plastic bag contained the heart and tubes for supplying the coronary arteries with oxygenated blood to keep the heart alive. The entire assembly was rotated as the preserved heart had been. The blood being pumped by the ventricle was mixed with a contrast liquid to increase its X-ray density and circulated independently of the blood supplying the coronary arteries. The heart rate and rhythm were controlled by means of attached electrodes.

The experiment was run completely by a computer that controlled the beating of the heart, triggered pulses of X rays from the source at the appropriate time during each beat, collected and stored the data from the X-ray detector and rotated the heart for each new projection. A set of between 30 and 50 projections over 180 degrees could be obtained in about two minutes.

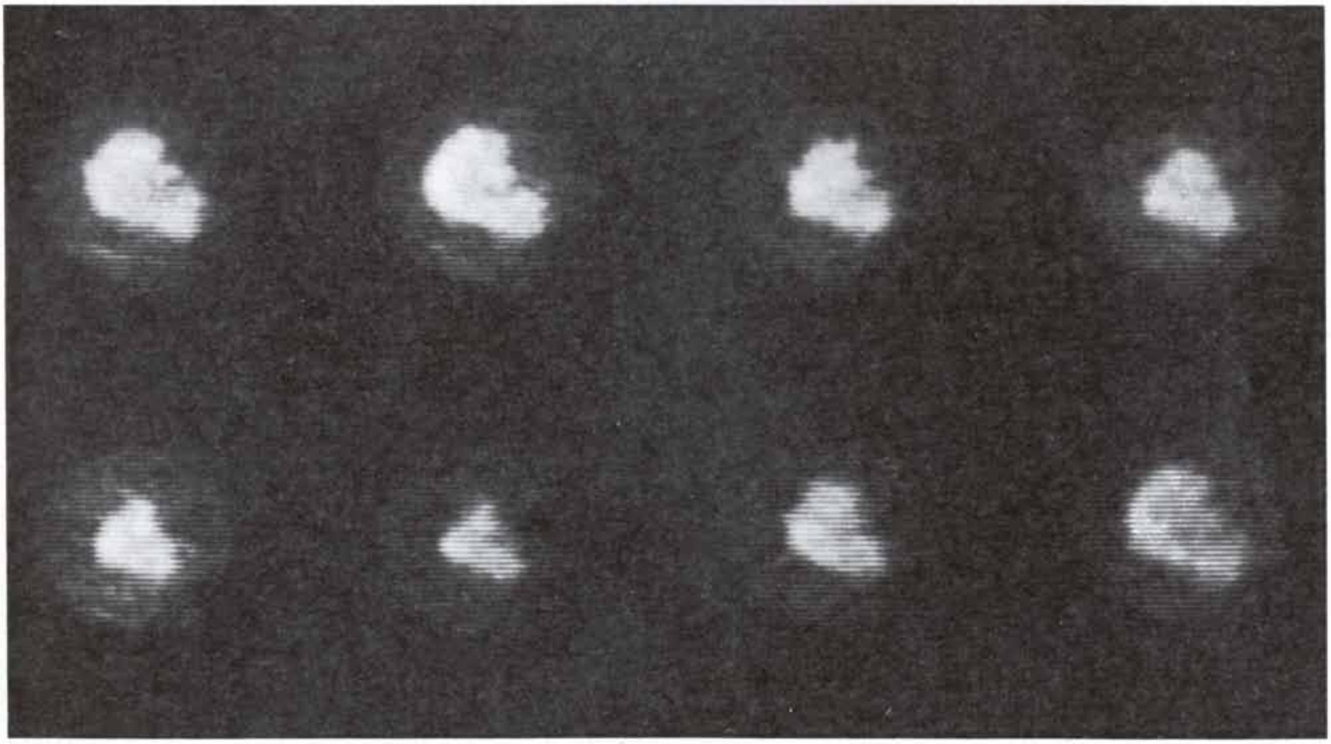
The cross sections of the heart were subsequently reconstructed by the same computer. Each cross section took two



COMPARISON OF TWO CROSS SECTIONS, one an image of a normal human brain obtained by the EMI scanner (*top*) and the other a photograph of a section of a dissected normal human brain (*bottom*), illustrates the operation of the scanner. The anatomical details of the two brains can be readily compared. The reconstructed spot that appears to the left of the scanner image corresponds to a plastic rod that is used for calibrating the X-ray densities in the image. The scanner image of the head consists of 11,200 (160 × 160) picture elements. The image was made in the course of a study by D. F. Reese of the Mayo Clinic.



DOG'S HEART was reconstructed plane by plane by ART3 as it was rotated around a single axis and illuminated by a diverging conical beam of X rays. The rendering of the reconstructed image at the right corresponds to the section indicated by the colored line on the vertical X-ray image in the center. Essentially the same technique was used to make reconstructions of the isolated beating left ventricle of a dog's heart (*see illustration below*). This work was done in the laboratory of E. H. Wood of the Mayo Clinic.



ISOLATED BEATING LEFT VENTRICLE is shown in cross section at intervals of 1/15th second during a single cardiac cycle. (The heart rate was 120 beats per minute.) All the cross sections represent the same anatomic section midway between the base and the apex of the ventricle. Images show contraction of the left ventricular chamber (*bright central area*) followed by subsequent dilation of the chamber as the heart relaxed. The reconstructed images were made by R. A. Robb and E. L. Ritman of the Mayo Clinic.

minutes of computing time and was displayed on a cathode-ray tube as a square picture with 64 pixels on a side. As a result of this work we can now watch any cross section of the heart as it changes shape during the heartbeat, or view all the cross sections together as they appear at a given moment.

With that preparation the three-dimensional form and dimensions of a heart can be measured as a function of time, and we can begin to study the heart's dynamic properties in both the normal and the diseased state. Yen-Ching Pao of the University of Nebraska has applied an engineering method of computation known as finite-element analysis to these data to investigate the distribution of stresses and strains within the walls of the heart. Experiments by the Mayo group have recently advanced to the reconstruction of the working heart and lungs within an intact living animal. It is not possible, of course, to place electrodes in the heart of a human patient without some risk. Furthermore, the spontaneous beating of the heart is not always regular, particularly in those suffering from heart disease. Therefore similar cardiac studies and diagnostic procedures with human patients will have to await the development of a reconstruction device that can record the projection data for a stop-action reconstruction in a hundredth of a second at a rate of 60 reconstructions per second. Such a device might be realized by using a large number of X-ray sources and detectors arranged in an arc around the patient, all operating in rapid succession. The instrument would undoubtedly prove to be quite expensive, since each X-ray source and its associated detector system cost tens of thousands of dollars. Only in this way, however, could the coronary circulation be reconstructed.

Prospects

Although reconstruction from projections is able to reveal the internal structures of the body in three dimensions, it does not yet yield the fine detail that physicians are accustomed to seeing in a conventional X-ray picture. There are two major reasons for this shortcoming. First, for a given dose of X rays there is only so much information that can be extracted in the form of a reconstruction. Second, when the volume within the patient's body is divided into smaller and smaller pixels, the computing time increases rapidly until it becomes prohibitively expensive.

Two factors may diminish these problems in the future. First, the development of improved algorithms is an active area of research and may make it possible to extract more structural information from a given dose of X rays. Second, the excessive computing time may be reduced by advances in computer technology: special-purpose computers, computers that employ light instead of electrons to manipulate information, and "parallel" computers (which are organized to perform many operations simultaneously). New display techniques will facilitate the presentation of large quantities of three-dimensional information. It may also be possible to obtain images of the three-dimensional distribution of individual chemical elements by recording the energy of X-ray photons before and after they are transmitted through the body.

X rays are not the only means of making projections of structures within the body. Transmitted ultrasound, gamma rays from radioactive isotopes either inside or outside the body, fast subatomic particles from accelerators, even magnetic fields—all can be made to yield their own kind of projections of the body's internal structure. Some of these techniques are capable of discriminating between structures with greater sensitivity than X rays. For example, with magnetic fields it may be possible to reconstruct images eliminating everything but the blood flowing through the body. The mathematical methods are essentially identical with those we have described for X rays.

There are other physical approaches, not based on reconstruction from projections, to getting three-dimensional information about structures within the body. Among them are acoustic holography and techniques based on the transmission or reflection (echoes) of ultrasonic waves. Each of these methods, including reconstruction from projections, yields a unique map of the variation in some combination of physical properties of the tissues within the body. Since those properties are themselves functions of the body's anatomy and physiology, the different kinds of images derived from their measurement provide information for different kinds of diagnostic task. As the number of physical properties measured increases, diagnostic characterization becomes more specific, but such specificity is accompanied by increasing complexity and cost. Therefore it will be necessary to carefully evaluate each imaging technique (or combination of imaging techniques) for each diagnostic purpose.

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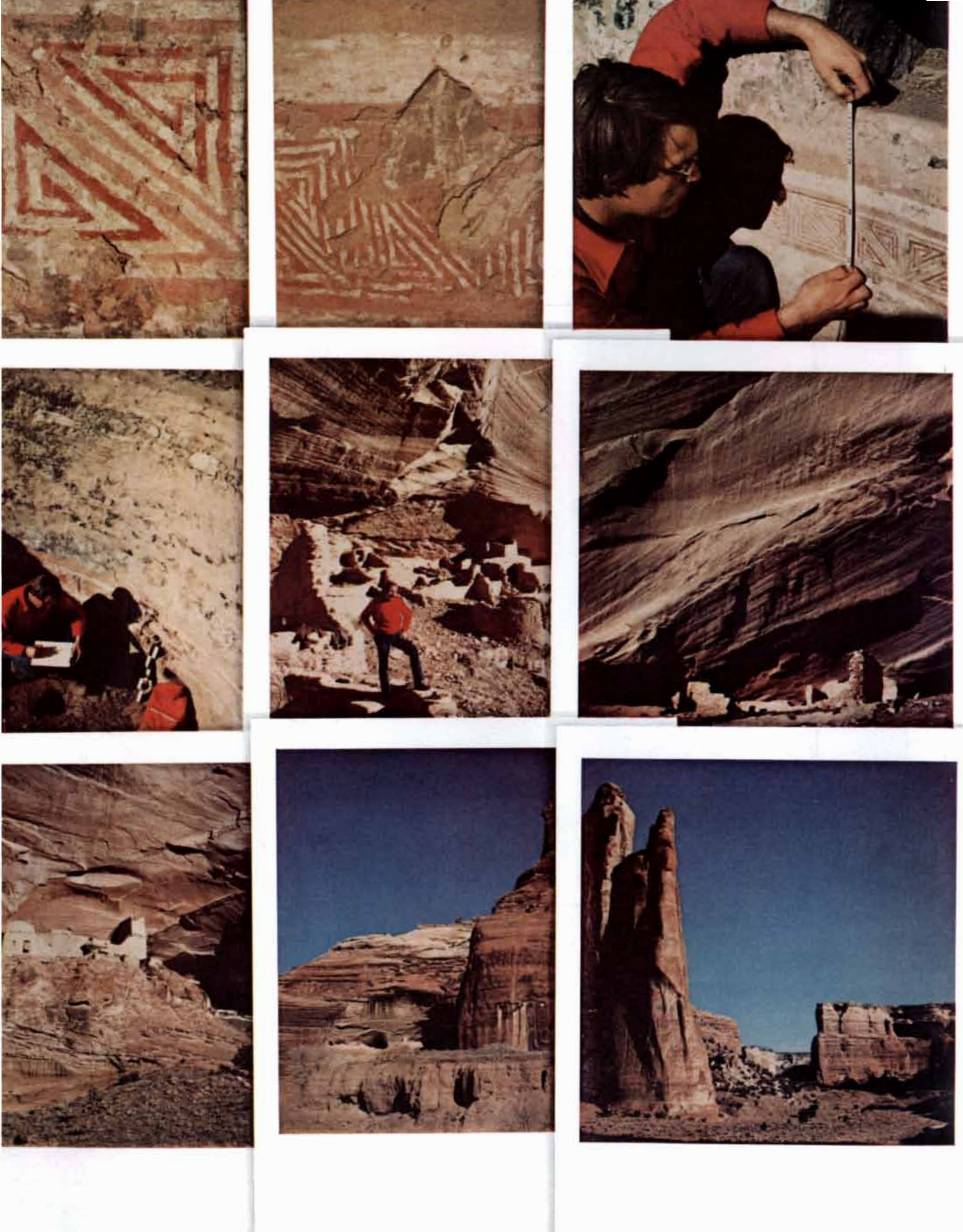

Sequence of photographs showing a 12th or 13th Century ceremonial room mural in Anasazi (Pueblo) Indian cliff ruin in Canyon del Muerto, Arizona. Photographs ranging from 10.4 inches (upper left) to 5 miles (lower right) display the find in relation to its surroundings. All photographs are reproduced at their actual size.

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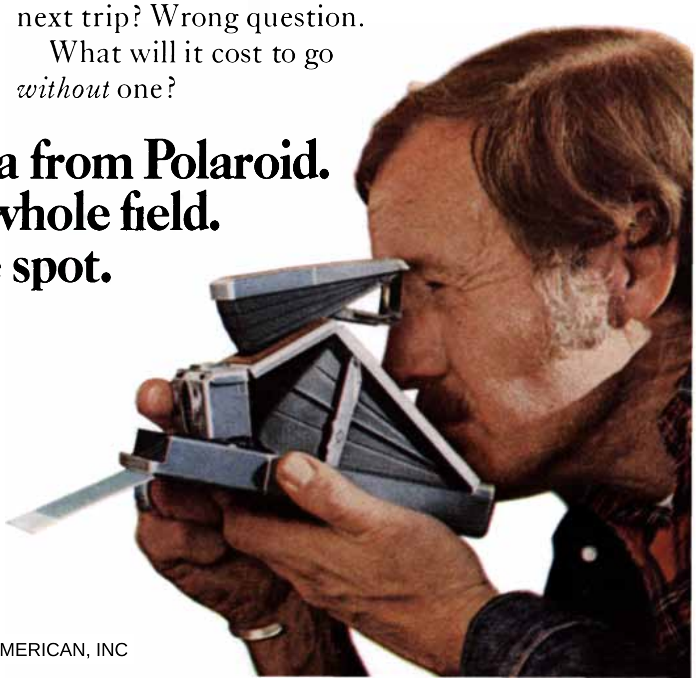
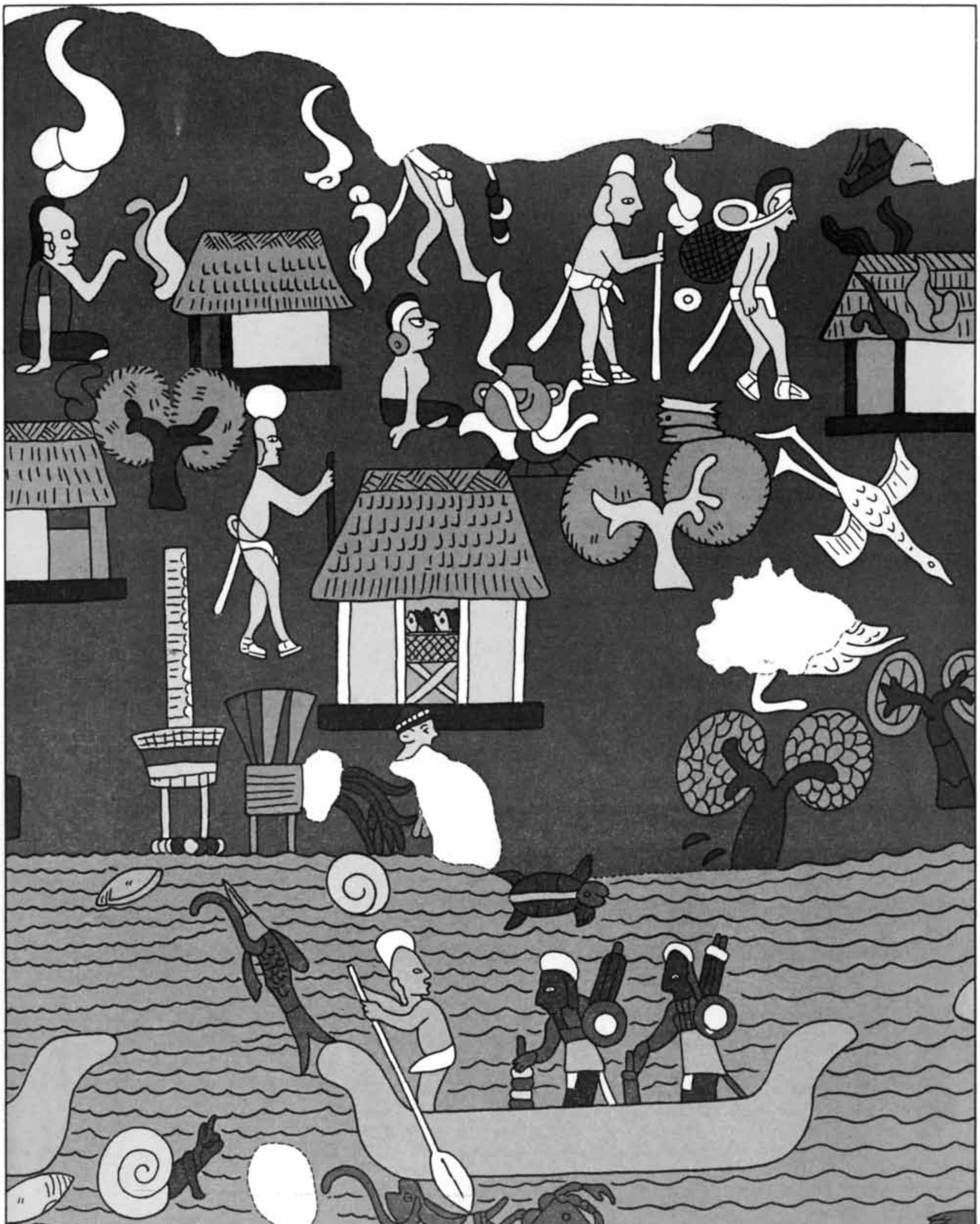




SCOUTING CANOE, with a single paddler and two armed warriors aboard, is lying just off the coast in this copy of a wall painting from the Temple of the Warriors at Chichén Itzá. The warriors are carrying Mexican weapons: dart throwers in hand and quivers of darts next to their round shields. They may represent the vanguard of the Toltecs who rebuilt Chichén Itzá during the 10th century.

The Rise of a Maya Merchant Class

The centuries just before the Spanish conquest are usually viewed as a time of Maya decline and decadence. A review of the evidence suggests instead that new leaders were pursuing new objectives

by Jeremy A. Sabloff and William L. Rathje

The eighth century was a turbulent period in world history. In the Old World the victory of Charles Martel at Tours had halted the Moslem advance in Europe, but the Moslems, having besieged Constantinople for a second time, were still strong enough to keep the Mediterranean as their private sea and at the same time to crush the Maitraka dynasty in India and drive the T'ang dynasty's frontier guards out of Chinese Turkestan. In the New World, although no one in the Old World was aware of it, a period of economic prosperity and cultural flowering in what are now Mexico and Guatemala was ending in disaster. In the highlands of Mexico the great urban center at Teotihuacán was sacked and burned around A.D. 700; in the southern part of the Yucatán lowlands the Classic civilization of the Maya collapsed, its magnificent ceremonial centers ceased to function and the region was depopulated. The aftermath of this New World debacle is our concern here.

Many students of Maya prehistory characterize the period between the collapse in the southern lowlands and the arrival of the Spanish conquistadors as an era of slow decline for all the Maya, in both the southern and the northern lowlands. Few New World writings predating the 16th-century arrival of the Spanish have survived, and what has survived is intelligible only in part. Those who undertake to reconstruct pre-Columbian events therefore must depend largely on archaeological evidence.

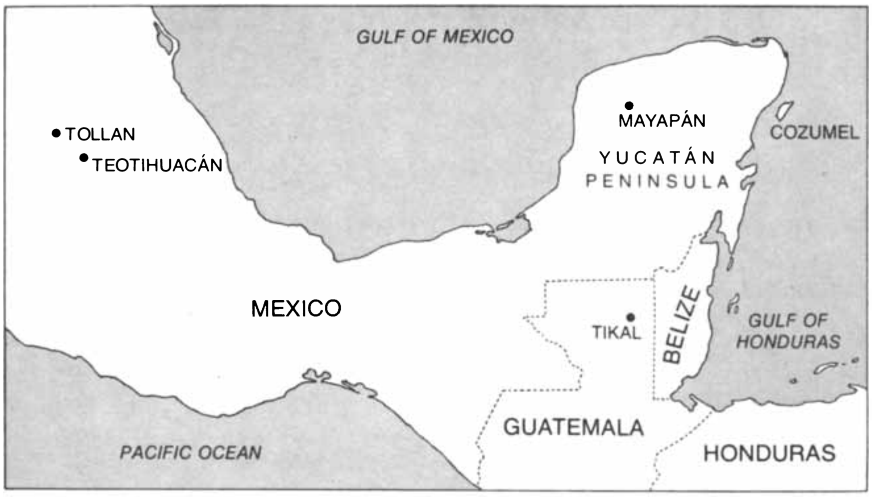
Such evidence can be interpreted in more than one way. For example, two decades ago Tatiana Proskouriakoff presented a brief account of several seasons' work conducted at Mayapán in northern Yucatán by the Carnegie Institution of Washington [see "The Death of a Civilization," by Tatiana Proskouriakoff; SCIENTIFIC AMERICAN, May, 1955]. A large walled city, Mayapán flourished from the middle of the 13th century to the middle of the 15th. The most neutral of scholars' terms for this period in Middle America is Late Postclassic; many simply call it "the decadent period." The Carnegie workers found that most of the high standards of civilization maintained by the Maya during the Early and Late Classic periods (A.D. 300–800) were virtually nonexistent at Mayapán. The imposing structures carefully built of masonry, the monumental stone sculpture adorned with inscriptions, even such a comparatively modest skill as the production of fine polychrome-painted pottery—all were conspicuous by their absence. That is why Proskouriakoff titled her account "The Death of a Civilization."

Recent archaeological investigations on the island of Cozumel, off the east coast of Yucatán, lead us to a contrary interpretation of that period in Maya history. Instead of decline and cultural stagnation we see a cultural reorientation: a transfer of authority to new hands and a consequent pursuit of new objectives. Before presenting the evidence from Cozumel and offering a reinterpretation of the older evidence it will be useful to describe the general scene.

The first major city-state in North America arose in the highland Valley of Mexico around the time of the beginning of the Christian Era. The site of its ruins is today called Teotihuacán. Between A.D. 250 and 600 the city's numerous neighborhoods—some religious, some bureaucratic and many industrial-mercantile—reached their point of highest development. A house count conducted by René Millon of the University of Rochester in the 1960's suggests that at the city's apogee no fewer than 75,000 people lived in it; the number may have been as high as 125,000. Among the city's workers were craftsmen who produced such valued export goods as pottery, obsidian artifacts and various ornaments made of semiprecious stones. The craftsmen formed a key element in an economy that carried wares overland from Teotihuacán to parts of Mexico and Guatemala hundreds of kilometers away. If the demands of trade were the same then as they were later, the trade apparatus brought back to the city an abundance of commodities, some exotic (such as feathers, aromatic resin and cacao beans) and some ordinary (such as salt and cotton cloth).

During the period when Teotihuacán enjoyed the benefits of long-range overland trade a parallel development was taking place in the lowlands of Guatemala and southeastern Mexico [*see illustration on page* 79]. The people involved in this southern florescence were the Maya. Their history goes back to at least the middle of the first millennium B.C., but it was not until the Early Classic period (A.D. 300–600) that the record of their remarkable achievements literally began to be hewn in stone. At Tikal, the greatest of the Classic Maya cities in the southern lowlands, is a stone monument bearing a date equivalent to A.D. 292.

The Maya custom of erecting dated monuments has been a great convenience to scholars. It has made possible the reconstruction of a fairly exact chronological record of Maya activities. For example, dates on monuments in the southern lowlands indicate that the Maya were active there during much the same time that the Maya in the northern lowlands were building such great centers as Dzibilchaltún. The monument



ECONOMIC ARENA was dominated from the beginning of the Christian Era until the Spanish conquest in the 16th century, first by traders from Teotihuacán and then by traders from nearby Tollan. Both cities were in the Mexican highlands, and the trade routes were overland. When Teotihuacán fell, a group of Maya, the Putun, began trading by sea instead of overland. The principal Putun sea route, around Yucatán, is shown on the opposite page.

dates also provide evidence that the seventh century, the traditional time of transition from the Early Classic period to the Late Classic, was a turning point that reflected more than local Maya events.

Unrest was rife in Mexico proper during much of the seventh century. It was at the end of the century that Teotihuacán was burned. The effect of the event was widely felt. As the influence of Teotihuacán over Middle America faded, Maya civilization during the Late Classic period (A.D. 600–800) was able to reach new heights. Older ceremonial centers expanded significantly in size and new ones were founded; such hallmarks of Classic civilization as ball courts, palaces and great temples were erected. A new mastery of technique and style was achieved both in monumental sculpture and in polychrome pottery design. The population at the great Maya centers expanded rapidly. For example, it is estimated that at the beginning of the period the population of Tikal was between 30,000 and 50,000.

These trends brought Maya civilization to its highest point but they also laid the foundations for its sudden collapse. In little more than a century, between A.D. 770 and 890, the ceremonial centers were abandoned; not one monument in the southern lowlands bears a date later than A.D. 900. The factors responsible for the collapse of Classic civilization in the southern lowlands and the depopulation of the area are complex and do not concern us here. They appear to include an overtaxing of the lowland environment and external pressures of an economic and military kind.

At about the time of the Classic Maya collapse, however, another city-state in the highlands of Mexico began to fill the vacuum left by the destruction of Teotihuacán. Mexican scholars identify the city as Tollan (modern Tula), the capital of the Toltecs, and tradition dates its founding at about A.D. 900. Smaller and economically less important than Teotihuacán, Tollan nevertheless made itself felt as a military power.

The Tollan import-export trade extended to the west, north and south of the Valley of Mexico. Concerning the trade to the south, archaeological analyses of imported pottery unearthed at Tollan indicate that overland trade routes extended to Costa Rica and Nicaragua. As far as their nearer neighbors, the Maya, were concerned the most spectacular evidence of Toltec influence is found at Chichén Itzá.

Originally built by the Maya of the Late Classic period, Chichén Itzá was enlarged and rebuilt in Toltec style during the 10th century. The great center probably represents the height of Toltec influence on Maya architecture. The influence of the militant emigrants from Tollan was not limited to architecture, however. By A.D. 1000 the Toltec had assumed political sway over much of the northern lowlands. The traditional interpretation of the Maya archaeological record accepts that date as the start of the period of decline known as the Early Postclassic.

Some 200 years later Tollan was burned just as Teotihuacán had been half a millennium earlier. By A.D. 1224 Chichén Itzá stood abandoned, and Toltec power was extinguished in Yucatán. In the traditional interpretation the Maya decline of the Early Postclassic period deepened at about this time into the Maya decay of the Late Postclassic.

The scene is now set. The actors who enter are Maya from the gulf coast lowlands that are today the states of Tabasco and Campeche. Analyses of postconquest documents by such scholars as Ralph L. Roys, France V. Scholes and J. Eric S. Thompson identify these people as a group known as the Putun. They spoke a dialect unlike the Yucatec Maya dialect heard generally throughout Yucatán; it was Chontal Maya, one of the dialects of the Cholan Maya group. The niche in society that the Putun occupied was a new one: the seafaring merchant.

The importance of trade among the peoples of Middle America is difficult to overemphasize. The economic influence of Teotihuacán had led to a growing secularization of life and the development of a market economy not only in highland Mexico but also elsewhere. At the same time an initial emphasis on trade in luxury goods had gradually been transformed into a demand for the ordinary commodities that were obtainable from distant sources. The trade itself, however, had been almost exclusively overland. In an economy that lacked both wheeled vehicles and pack animals this meant that transport capacity was limited to what a trader and his porters, hired or slave, could carry on their back.

With the collapse of Teotihuacán's political and economic empire at the start of the eighth century the seafaring Putun found themselves in a unique position. No central organization remained to handle the procurement and export of the many kinds of raw materials and other goods produced in the greater Maya region. For example, salt was an important Maya export. It was collected from natural salt pans along the gulf coast of the Yucatán Peninsula and in the northern coastal lagoons. Cacao beans are another example. The beverage made from them was the Mexicans' gift to the Old World; the English word "chocolate" is a corruption of the Nahuatl word for the drink. The beans served another purpose: they were used as currency throughout Middle America. Early Spanish explorers found that the cost of a strong slave was 100

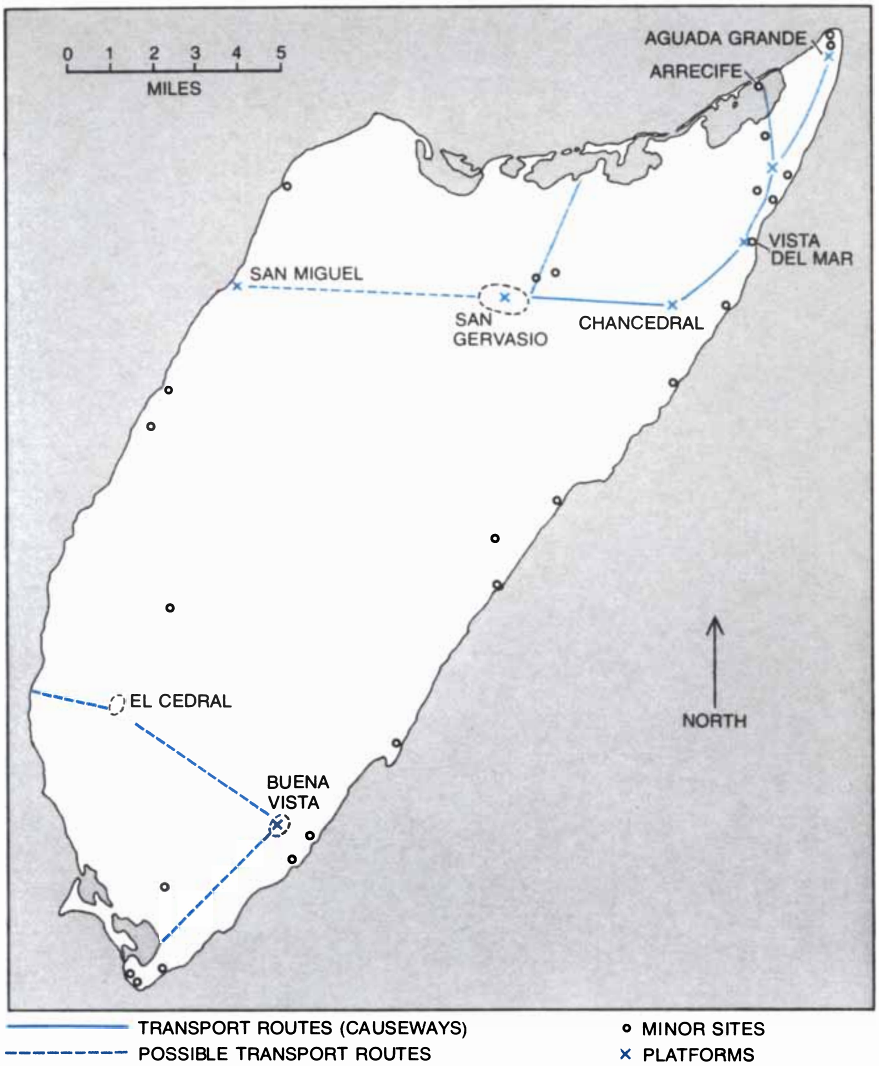
------ PUTUN SEA ROUTE

A SALT — *D* OBSIDIAN

B CACAO — *E* COPPER

C JADE — *F* QUETZAL PLUMES

PUTUN SEA ROUTE connected the gulf coast of Yucatán, close to overland trade routes from highland Mexico, with ports in the Gulf of Honduras on the opposite side of the peninsula. Cozumel, an island just off the east coast, was an important midway point for the maritime merchants who traded in salt, cloth, cacao, jadeite, feathers, obsidian and copper.



ISLAND OF COZUMEL, more than 175 square miles in extent, has 34 pre-Columbian sites. Sites on the coast probably gave residents of the island early warning of approaching vessels. Storage platforms high enough to stay dry during seasonal floods were connected to coastal shipping points by a system of all-weather causeways. The shrine of the goddess Ix Chel, which drew many Maya pilgrims to Cozumel, may have been located at San Gervasio.

cacao beans. Tabasco, on the border of the southern lowlands, was a major cacao-producing area. A third article of trade was cotton cloth; Maya weavers excelled in the production of cotton textiles. A fourth was honey, a major Maya export.

Not only were the Putun in the right place at the right time to take over the interrupted trade in these products and others but also, being seafarers, they were able to transport the merchandise more effectively than any overland trader. It was not simply a matter of a better means of transportation; the Putun planned for convenience and efficiency. For example, the Putun potters were pioneers in the mass production of simple shapes. They specialized in a hard, thin-walled ware that archaeologists call Fine Orange. And the pottery was not merely mass-produced; it was often shaped so that the finished products could be nested for ease of shipment [*see top illustration on page 82*].

The long-distance coastwise trade route of the Putun eventually extended from Tabasco to the Gulf of Honduras, with the island of Cozumel acting as an entrepôt. Little is known of the Putun boats, which are described by early European visitors as being log dugouts of various sizes. Spanish accounts refer to canoes carrying as many as 50 men. Pictures of Maya canoes executed by Maya artists always show smaller craft. That may be because the artists drew canoes only big enough to accommodate the occupants; the largest number of passengers shown is seven. In any event, by the time of the first Spanish contacts the Putun dialect had become the lingua franca of trade throughout the Maya area. The merchants' reputation as voyagers was such that Thompson calls them the "Phoenicians of the New World."

The literature of European exploration tells of an encounter between a Spanish ship and the oceangoing canoe of what may well have been a Putun trader. The contact occurred during the fourth voyage of Columbus, who was looking over one of the Bay Islands off the coast of Honduras. Late in July, 1502, his ship came alongside a large dugout, eight feet wide and "as long as a galley." It was being propelled by some 20 paddlers. The captain, probably the trader-owner, is described as an old man. Apparently he had taken his family along on the voyage: a palm-leaf shelter amidships was occupied by women and children. Unlike the naked Indians of the Caribbean with whom Columbus was familiar, the women wore cotton garments and hid their faces from Spanish stares with brightly colored shawls. The canoe's cargo included a number of these shawls and other cotton clothing, such as dyed sleeveless shirts. There were also long wood "swords" edged with obsidian or flint, cast copper bells, copper hatchets and crucibles for melting copper. What the Maya voyagers valued most, judging by the way they searched the canoe bottom for any that were spilled, were cacao beans. The Spanish took samples of all the Maya merchandise and gave the canoeists some trinkets in exchange. The two vessels then parted, but not before Columbus had seized the old captain to serve as an interpreter.

The Putun expansion had begun toward the end of the eighth century. It was not exclusively economic; the traders also made their military power felt. Their early contacts with highland Mexico must have given them an initial strategic advantage over their neighbors. Among their arms were various Mexican weapons unfamiliar to other Maya at the time, notably darts and dart throwers. It appears certain that the Putun expansion in one direction, along the borders of the southern lowlands, was a factor in the collapse of Classic Maya civilization there. At the same time the expansion in the other direction, into and across the northern lowlands, contributed to a late flowering in that Maya area.

By the end of the 12th century, as Proskouriakoff has shown, many artistic aspects of Classic Maya civilization had long been in a state of decay. In the traditional interpretation such decay reflects a generalized social decline. In reality, however, the economic life of Maya civilization during those centuries was becoming increasingly complex and vigorous. The assembly and distribution of both raw materials and manufactured goods were expanding rapidly. For example, in the manufacture of pottery mass-production methods had replaced the traditional piecework. At the same time the standard of living among the non-elite was on the rise; the intricate system of long-distance trade gave the farm population access to the wide variety of new materials the traders brought back to Yucatán from other areas. Perhaps most important of all, as religious authority became decentralized the developing market economy provided something to take its place: a mercantile authority.

In the course of the Putun's trade-oriented political expansion in the northern lowlands of the Yucatán Peninsula they seem to have been governed by a cultural ethic that was new among the Maya. What might be called mercantile pragmatism, it reflected the ascendancy of a merchant class at the expense of the old theocracy. In characterizing the so-called decadent period of Maya civilization most archaeologists have neglected to take into account either the rise of the new class or the significance of their new cultural ethic. Archaeological findings on the island of Cozumel provide a corrective to this neglect.

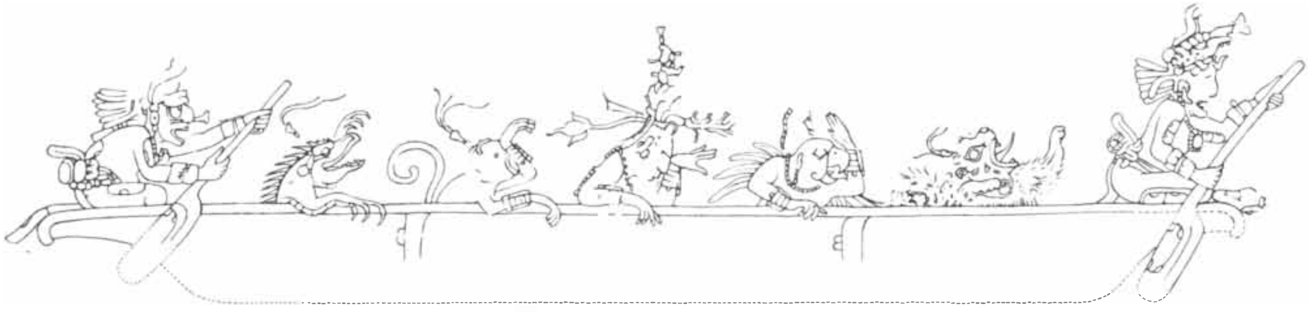
Cozumel has an area of more than 175 square miles. Over the years the Mexican National Institute of Anthropology and History has registered more than a score of archaeological sites on Cozumel, most of them along the coast [*see illustration on opposite page*]. Investigations by such scholars as A. Escalona Ramos and William T. Sanders indicate that the first Maya inhabitants arrived on the island at a time around the beginning of the Christian Era. Cozumel remained a quiet backwater until the beginning of the Terminal Classic period, around A.D. 800. Thereafter the importance of the island grew; among other reasons was the fact that it was the site of a shrine that was visited by pilgrims from all parts of Yucatán. Cozumel reached its peak between A.D. 1300 and 1500, the period of supposed Maya decadence. This record of insular rise and preeminence is so closely synchronous with the rise of the Putun that when early in the 1970's an opportunity arose for us to reconnoiter Cozumel, we were eager to take advantage of it. Our respective institutions (the Peabody Museum of Archaeology and Ethnology at Harvard University and the Department of Anthropology at the University of Arizona) agreed to undertake an expedition, and the National Geographic Society contributed financial support. We worked on the island with the authorization and cooperation of the Mexican National Institute in 1972 and 1973 as directors of a joint Cozumel project. Our objective was to learn how the island had been useful to the Putun traders and whether or not the archaeological record at Cozumel reflected the traders' mercantile culture.

The largest pre-Columbian structures on Cozumel are of a kind that would be of little interest to students of aesthetics. They nonetheless represent a major investment in labor and materials by the Putun. The primary importance of Cozumel to the traders was its strategic location as a stopover and as a storage depot. The island is low, however, and is subject to flooding during seasonal rains or hurricanes. The Putun therefore built huge stone platforms, high enough to be safely out of the reach of floodwaters, at more than half a dozen sites. The platforms were constructed of limestone rubble. The largest is at Buena Vista; it consists of a group of connected platforms, averaging five meters in height, which cover an area of more than seven hectares (17 acres).

All the platforms on Cozumel appear to have been built during the three-century span of the "decadent" Late Postclassic period. Although the main motive for building them must have been to provide a safe storage place during floods, it is not the only motive that can be imagined. For example, certain Putun goods, such as salt, came to market seasonally. If such goods are stored by the ultimate consumer, there may be a seasonal oversupply and a price drop. If they are stored by the merchant, using large spaces such as the Cozumel platforms, the price will remain stable. The investment of time and effort in building the platforms would thus have been sensible from more than one mercantile point of view.

Another major construction program on Cozumel was the building of raised stone causeways; these seem to have provided all-weather links between freight-transfer points at the water's edge and the storage platforms inland. Like the platforms, the causeways were built of limestone rubble. They are not easy to trace today, except in a few places where they still provide dry footing across permanently swampy areas.

One large causeway passes through a free-standing arch that is now in ruins. Both causeway and arch seem to have served a purpose other than a mercantile one. The arch evidently marked the main entrance to a group of ruins now known as the San Gervasio zone. This may be where the famous pilgrims' shrine stood: the temple sacred to the goddess Ix Chel. The causeway may therefore have been a pilgrims' road to Ix Chel's shrine or to some other important religious precinct. In the Maya pantheon Ix Chel was the consort of Itzamna; she played a rather superior Juno to his Jupiter, being not only the goddess of the moon but also the patroness of weaving, medicine and



SEVEN-PASSENGER CANOE, the largest represented in Maya art, was among several subjects delicately incised on animal bone and buried with other grave goods in a tomb located under Temple I at Tikal. The paddlers and their grotesque passengers are all members of the Maya pantheon; the fine drawing, incompletely preserved in some parts, is an example of Early Classic craftsmanship. The only descriptions of larger Maya canoes are by European observers; the largest log dugouts are said to have carried 50 men.

childbirth. Visits to her shrine were frequent, perhaps because the only speaking oracle in the Maya world was located there.

Even if the San Gervasio causeway did not serve commerce directly, the Putun might have considered it a good investment. Those who came to worship on Cozumel could well have remained to trade. For that matter, the pilgrim traffic alone would have been an important source of income for the islanders.

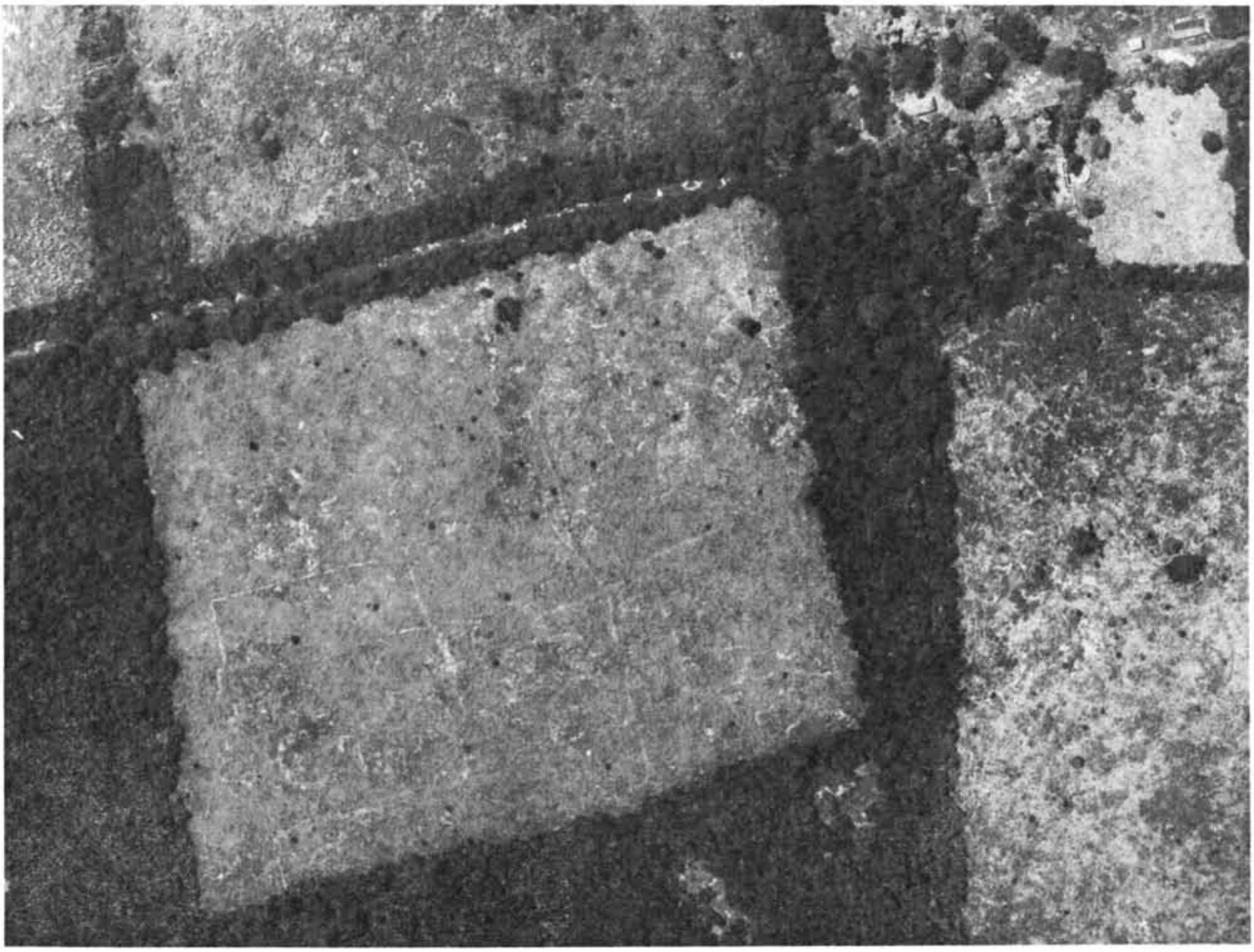
An unexpected discovery of the Cozumel project was an islandwide system of walled fields [*see illustration below*]. The walls were evidently built during the same three centuries of the so-called decadent period. Like the platforms and the causeways, they consist of limestone rubble. They vary in height from half a meter to a meter and a half. The walls almost certainly marked the boundaries of fields that the island's farmers fertilized by burning and then planted to maize, beans, squash or other crops. Although the fields are somewhat irregular in shape, the average area enclosed is 4,000 square meters, or about one acre.

It is possible that the prosperity of Cozumel at the height of Putun power was accompanied by an increase in population sufficient to place island land at a premium. If this was the case, the walls may have marked property lines as well as field boundaries. The effort invested in constructing such an enclosure system invites the speculation that the walls represent a shift by the Putun from the traditional Maya pattern of farmland held in common to one of farmland held as private property.

The construction of a system of coastal defense points was another Putun activity on Cozumel that called for a considerable investment of time and labor. Fourteen of those structures still stand along the leeward and windward shores of the island. Spanish accounts of early contacts with the Maya describe fires being lighted on top of the structures as beacons to warn the island's inhabitants when raiders were approaching.

The structures of this early-warning network vary in size and in architectural style. In addition to their defensive function some may have served as shrines or fulfilled other purposes. All, however, seem to have been built during the same 300-year period, and there is evidence that at one time the network included at least 20 coastal structures.

Settlements on the Yucatán mainland opposite Cozumel during this period were fortified. Tulum is a good example: the cliff on which it stands provided protection on the seaward side and wall systems guarded the landward approaches. Cozumel, however, had no natural defenses. Moreover, the island's many field walls were not high enough to be used defensively. The Putun seem to have relied mostly on building storage areas, residences and administrative and



BOUNDARY WALLS of the agricultural field system at Cozumel appear as faint white traces in this aerial photograph of a newly cleared part of the San Gervasio zone in the northern part of the island. The islandwide system of field walls suggests the possibility that the Putun rulers of Cozumel may have abandoned the tradition of communal farmland in favor of private land ownership.

religious structures some distance inland from the defenseless coast. The interior areas were nonetheless linked to certain coastal points by the causeway system. If an enemy approached, the coast watchers could alert the population so that inland forces could use the causeways to concentrate at the threatened point on the coast.

Two other archaeological findings at Cozumel help to fill out the picture of a pragmatic mercantile people. First, the houses built by the traders had imposing façades: under the thatch that shaded the portico in front of the house rose a stone wall, smoothly plastered and often decorated with bright paintings. The wall was broken by doors that led into the dim interior [*see illustration on next page*]. This wall, however, was the only masonry construction in the house. The inviting doorways led directly into a thatch-roofed interior of about the same area as the thatched portico, enclosed by three walls made only of poles. These masonry false fronts, somewhat resembling the set of a Hollywood "Western," were built not only on Cozumel but also elsewhere in Putun-dominated Yucatán.

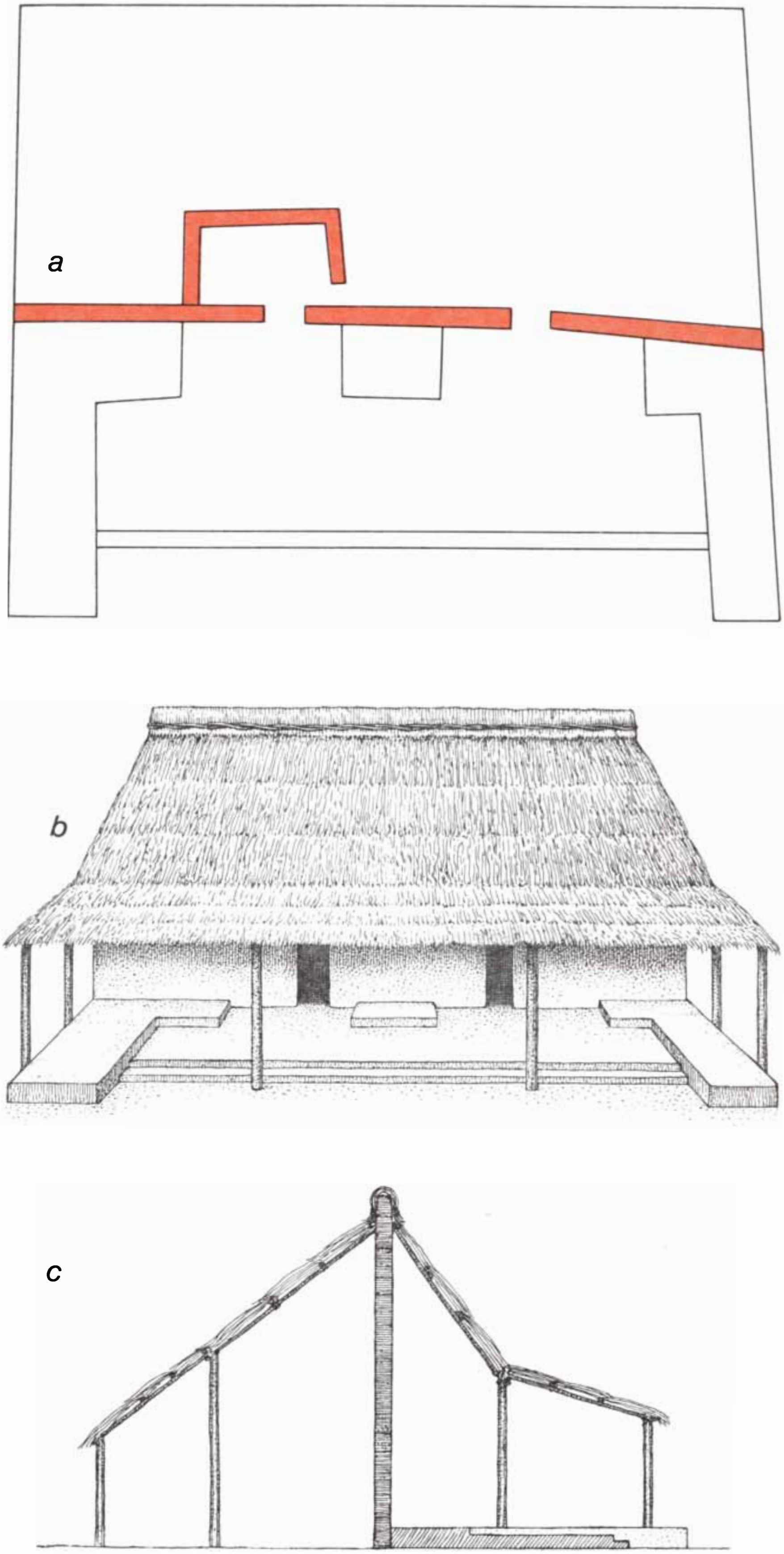
The other finding was the discovery of several caches of ax blades made from the green jadeite the Maya preferred for ceremonial objects. When one uncovers such a cache at a site of the Classic period, the workmanship of the jade is to be marveled at. In the Late Postclassic caches at Cozumel, however, quality is minimal. Almost all the ax blades are broken or otherwise damaged. One has the impression of bargain merchandise: seconds and irregulars.

What conclusions can be drawn from this picture of a pragmatic mercantile class? We have constructed a model based on our interpretation of the Cozumel fieldwork and on a reinterpretation of findings made elsewhere in Yucatán by other investigators. In broad outline the model would probably hold true for a rising merchant class in many different societies, but in its details it is peculiarly Maya. Its principal suggestion is that the developments of the three so-called decadent centuries, traditionally taken as indications of social decline, appear quite logical and progressive when they are regarded from a non-elite viewpoint. In other words, one has a very different view of Maya civilization looking at it from the bottom rather than from the top. Consider some of the Cozumel findings from that viewpoint.

The Putun dominance of Yucatán is

TIME INTERVAL	MEXICO PROPER		MAYA AREA	PERIOD AT COZUMEL
900 B.C. TO 300 B.C.	(1250 B.C.) OLMEC (400 B.C.)	(600 B.C.)	MIDDLE PRECLASSIC (FIRST OCCUPATION OF MAYA LOWLANDS)	(ISLAND UNOCCUPIED)
300 B.C. TO A.D. 300	(A.D. 50)		LATE PRECLASSIC (GROWTH OF CEREMONIAL CENTERS)	LATE FORMATIVE (ISLAND FIRST OCCUPIED)
A.D. 300 TO A.D. 600	TEOTIHUACAN	MONTE ALBAN	EARLY CLASSIC (RISE OF MAYA CLASSIC CIVILIZATION)	EARLY PERIOD I (OCCUPATION CONTINUES)
A.D. 600 TO A.D. 800	(A.D. 700)		LATE CLASSIC (PREEMINENCE OF CITIES SUCH AS TIKAL)	EARLY PERIOD II (OCCUPATION CONTINUES)
A.D. 800 TO A.D. 1000	(A.D. 900)		TERMINAL CLASSIC (COLLAPSE IN SOUTHERN LOWLANDS)	PURE FLORESCENT (PERIOD OF PROMINENCE BEGINS)
A.D. 1000 TO A.D. 1250	TOLTEC (A.D. 1190)	(A.D 1000)	EARLY POSTCLASSIC (PREEMINENCE OF CHICHÉN ITZÁ)	MODIFIED FLORESCENT (INCREASING PROMINENCE)
A.D. 1250 TO A.D. 1520	(A.D 1345) AZTEC (A.D. 1521)		LATE POSTCLASSIC (PREEMINENCE OF MAYAPÁN UNTIL *CA.* A.D. 1450)	DECADENT (COZUMEL APOGEE *CA.* A.D. 1400)
A.D. 1520 TO A.D. 1600	SPANISH CONQUEST AND COLONIAL PERIOD			(DRASTIC POPULATION DECLINE)

MAYA CHRONOLOGY, from the first occupation of the lowlands until arrival of the Spanish, is compared with successive periods at Cozumel (*right*) and in Mexico proper (*left*).



FALSE-FRONT HOUSE, its investment in masonry construction confined to one wall (*a*, *color*), is an example of applied Putun pragmatism. All a visitor to the house could see was a shady thatched portico (*b*), backed by a smoothly plastered and brightly painted wall. Only prying eyes would discover that the rest of the house was simple pole-and-thatch (*c*). House plan seen here was unearthed at San Gervasio; Mayapán contains similar structures.

evidence that the earlier theocratic elite had yielded power to a mercantile class. Just as other merchants have tried to do throughout history, the Putun wanted to keep their capital liquid. To spend the profits of commerce on high-quality grave goods was of no possible use to them; less than perfect jade serves the dead just as well. By the same token the raising of great monuments did little to benefit trade; a dry warehouse is more useful than a lofty temple. Nevertheless, appearances are always important. An inexpensive false front, heavily plastered to conceal shoddy masonry, made a logical investment. Such a house wall might not endure generation after generation, but the Putun were not building for posterity.

Some further examples emphasize the pragmatism of Cozumel's merchant rulers, men whose survival depended on quick reactions to changing economic and political circumstances. One such change came with the collapse of Toltec power in the 13th century. When Chichén Itzá, the paramount Toltec city in Yucatán, was suddenly abandoned, the entire Yucatecan trading network lost its prime central place. To reestablish the lost link a new center was founded within a few years: Mayapán. A fact strongly supporting the view that the new city was built to take the place of the old is that the earliest buildings at Mayapán almost literally reproduce the central core of Chichén Itzá on a smaller scale.

Some indications in the archaeological literature imply that the founders of Mayapán came from Cozumel, with the full support of the island's rulers. Such an action would have been logical; it was certainly in the interests of the Putun on Cozumel to stabilize the disrupted Yucatán trade network as soon as possible.

An even more drastic change accompanied the first probing Spanish voyages from Cuba to Yucatán: Córdoba's three ships in 1517, Grijalva's four in 1518 and Cortes' 11-ship fleet in 1519. The mainland Maya ambushed Córdoba's men when they landed on the north coast of Yucatán and killed many of them when they landed on the west coast. Grijalva landed twice on Cozumel without incident; the islanders simply retreated into the interior when the Spanish approached. Even when Grijalva sent a bilingual Jamaican who lived on Cozumel to summon the Maya chiefs to a meeting, they refused to respond. This passive behavior contrasts sharply with the hostile reaction of the mainland Maya to Grijalva's landings.

Cortes and his fleet met with quite a

different response on Cozumel, if one is to believe the eyewitness account of Bernal Díaz. By chance one ship (Díaz was aboard) arrived at the island ahead of the rest of the flotilla. As when Grijalva landed at Cozumel, the inhabitants fled into the interior. In one deserted settlement the Spanish landing party appropriated 40 "fowl" (presumably turkeys) and the contents of a shrine, including some gold pendants heavily alloyed with copper. They also seized three Putun stragglers.

Cortes arrived soon afterward and ordered the release of the prisoners. Returning to them all the articles taken from the shrine (the fowl had been eaten) and adding some Spanish trade goods as gifts, Cortes asked them to seek out their leaders and arrange a meeting. The next day the chief official of the settlement appeared, followed by the men, women and children who had fled. "They went about among us," Díaz reported, "as if they had been friendly with us all their lives."

Cortes knew that when Córdoba had landed on the west coast of Yucatán in 1517, the Maya, on seeing the Spaniards, had called out "*Castilan.*" To a good subject of Castile and León this seemed too much of a coincidence to be put aside, and Cortes asked the Cozumel chiefs if they knew of any other Spaniards in the area. The chiefs replied that indeed two Spaniards were living on the mainland nearby, and that "some traders had spoken to them two days before."

Cortes now asked the traders to take letters to his unknown countrymen, together with trade goods in the event that they had to be ransomed from the Maya who held them. He then sent the traders to the mainland in one of his smaller vessels. The result was the rescue from slavery of Geronimo de Aguilar, who had been cast away on the Yucatán coast in 1511 during a voyage from Panama to Santo Domingo. Aguilar, whose Maya was by now fluent, promptly joined Cortes as an interpreter. (The other Spaniard, a shipmate of Aguilar's, elected to remain in Yucatán with his Maya wife and their children.)

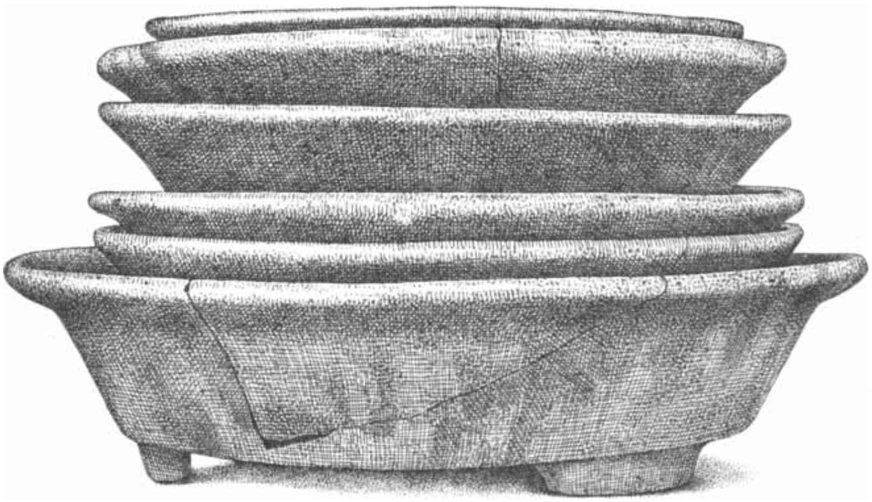
Soon thereafter Cortes desecrated what appears to have been Ix Chel's shrine. The Spaniards had no knowledge of the deity's identity, but Díaz states that the temple was one that was visited by many pilgrims from the mainland. Moreover, he actually heard what may have been the speaking oracle ("an old Indian in a long cloak") deliver what Díaz called a "black sermon." Cortes also heard the oracle, and after loftily telling the island chiefs that no good would come of idolatry, he had his men destroy the temple images and ordered Maya masons to build a new altar. When it was finished, Cortes placed an image of the Virgin Mary on it and added a timber cross. His actions did not make the people of Cozumel take up arms or flee. They seemed unperturbed and even burned incense before the new holy image.

How is one to interpret the difference between the hostile Maya of the mainland and the neutral Maya on Cozumel? For one thing, as long-range traders the Putun were accustomed to encountering foreigners. If they believed the strangers' intentions were hostile, which may have been how they viewed Grijalva and the men of Cortes' first ship, they did what they could to avoid contact. Word would have long since reached them through trade channels about the slaughter the Spaniards could accomplish with their strange and fearful weapons.

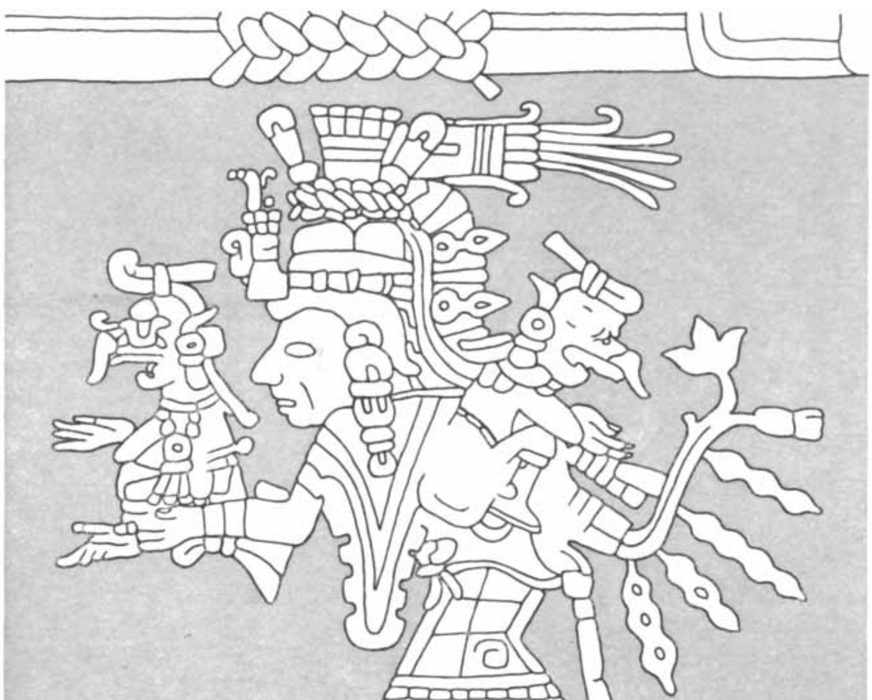
Perhaps too the Putun already sensed the Spaniards' political and economic potential. It is clear that they chose to overlook the actions of Cortes at Ix Chel's temple. Again, their acknowledgment that Spaniards were to be found on the mainland and their delivery of Cortes' messages were acts of gratuitous goodwill. There is evidence, in their farewell to Cortes, that the pragmatic Putun may by then have seen in the Spaniards the wave of the future. Before Cortes embarked the Putun asked for and received a "letter of recommendation" that they could show to other Spanish voyagers calling at Cozumel in order to ensure their future good treatment at Spanish hands.

As a final example of Putun pragmatism, consider the shrine of Ix Chel and its oracle. Can it be mere coincidence that the only oracular shrine in the Maya world was located on Cozumel? Oracles are in a far better position to be flexible in the endorsement of social and cultural change than the conservative priests who follow ritual in other kinds of shrine. There is even an example of this type of flexibility from modern Maya history. During the long and bloody "War of the Castes" that disrupted Yucatán periodically from 1847 to 1901 the rebellious Maya regularly turned for guidance to Chan Santa Cruz, a shrine in the fastness of Quintana Roo. There the priests spoke for the gods through the medium of three "talking crosses," dictating the war policy of the rebels.

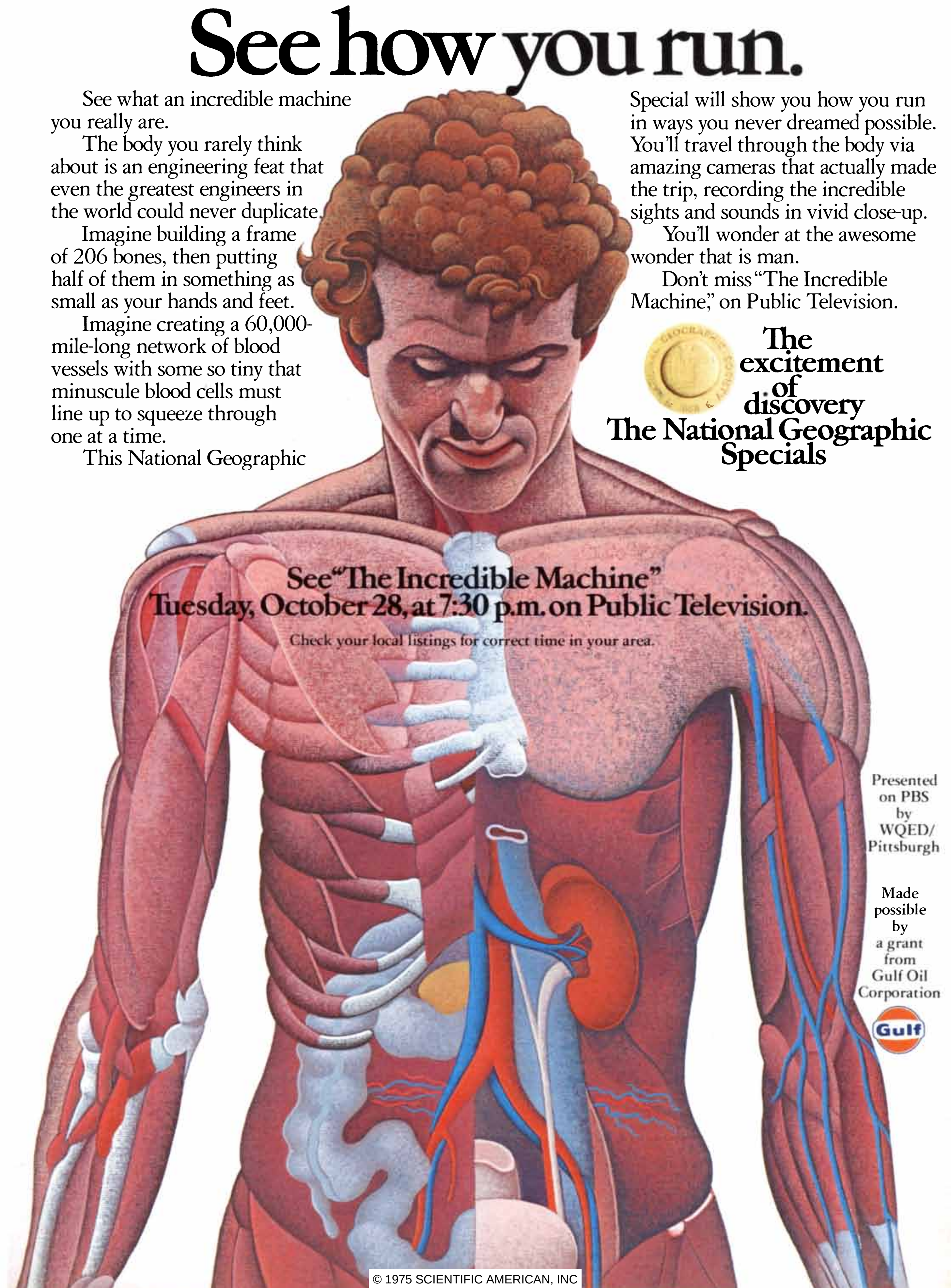
It is time for a final question. Can a period of history that witnesses the rise of a merchant class, the development of a new ethic and a substantial increase in economic complexity (including such events as the introduction of mass manufacture and an improvement in the general standard of living) be fairly considered the decadent last gasp of a dying civilization? Looking from the top down, from the viewpoint of an elite, the answer is perhaps yes. Looking from the bottom up, we think, the answer is clearly no. In examining the past one should not be limited to any one social point of view.



EASE OF TRANSPORTATION dictated the uniformity in diameter and wall slope of these shallow Maya basins; their shape let them be stacked for shipment. The basins were found on Cozumel. They are probably Putun traders' merchandise brought from northern Yucatán.



MAYA GODDESS, Ix Chel, is portrayed carrying two images of the rain god, Chac, in this wall painting from a temple at Tulum, a Late Postclassic site on the mainland opposite Cozumel. Ix Chel's shrine on Cozumel had the only oracle known in the ancient Maya world.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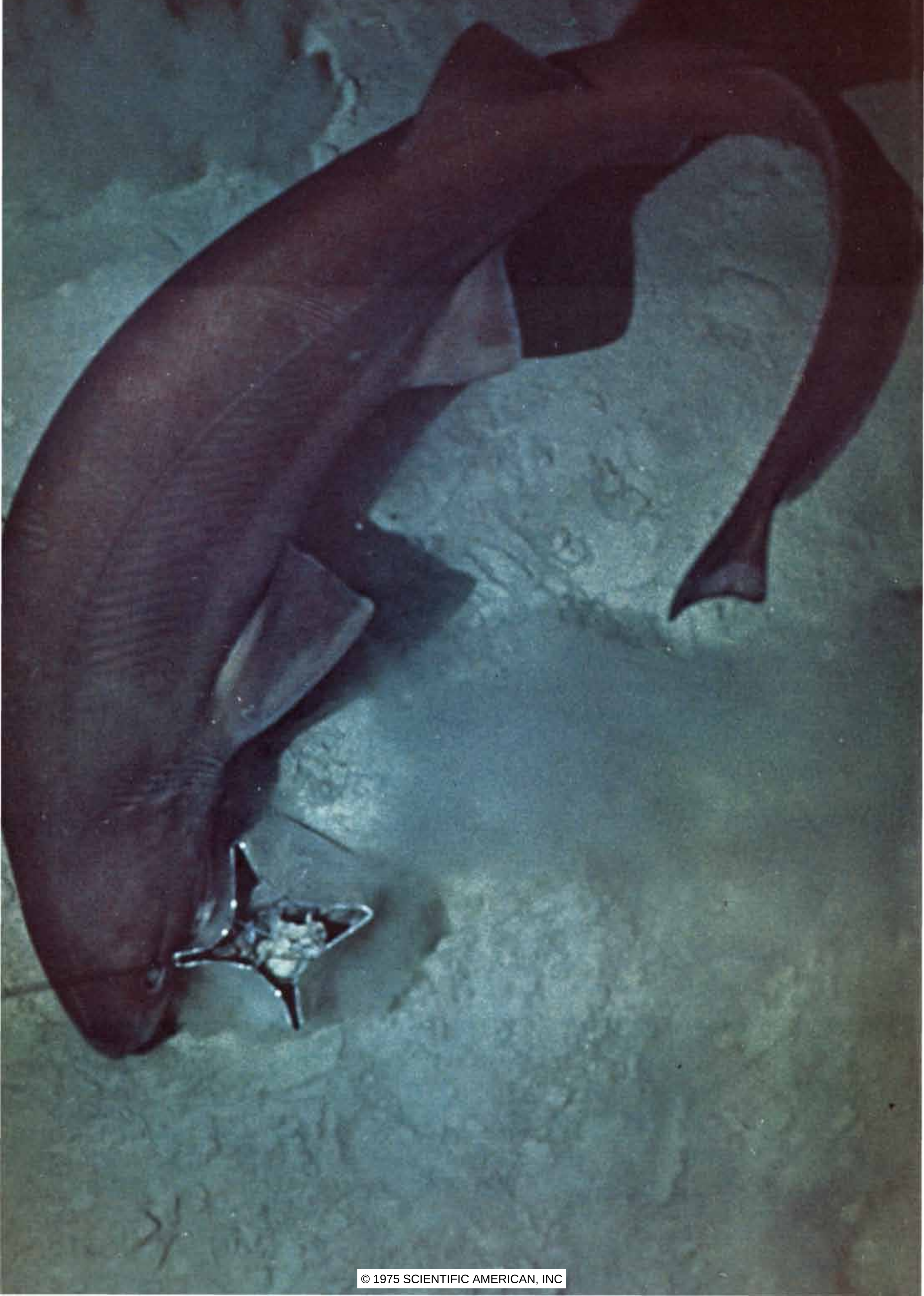


Active Animals of the Deep-Sea Floor

Baited automatic cameras dropped to the bottom of the ocean reveal a surprising population of large fishes and other scavengers that find and consume dead animals that fall from the waters far above

by John D. Isaacs and Richard A. Schwartzlose

Photographs made by automatic cameras that have been dropped to the deep-ocean floor have confirmed a finding that was first suggested by evidence gathered from baited traps some years ago. A deep-sea population of large, active animals thrives in what was generally assumed to be a province inhabited mainly by small, feeble creatures such as worms, snails and sponges. The thousands of pictures make it clear that much of the deep-sea floor teems with numerous species of scavengers: vigorous invertebrates and fishes, including some gigantic sharks, that are supported by a marine food web whose extent and complexity is only beginning to be perceived.

The celebrated expedition of H.M.S. *Challenger* in the 1870's laid to rest the old idea that the deep waters of the open ocean were a lifeless desert. The *Challenger's* trawls and dredges brought to the surface and to the attention of biologists a vast collection of deep-midwater and bottom-dwelling creatures from even the deepest ocean trenches [see "The Voyage of the 'Challenger,'" by Herbert S. Bailey, Jr.; SCIENTIFIC AMERICAN, May, 1953]. The inventory included some of the most grotesque forms into which higher animals have ever been molded by adaptation to extreme conditions, and several animals that had been thought to be long extinct.

FIFTEEN-FOOT SHARK attacks crushed bait can in the color photograph on the opposite page, obtained by the authors from a depth of some 750 meters in the eastern Mediterranean Sea. Such large fish typically arrive at the bait from three to eight hours after the camera system reaches bottom, frightening off most other feeding creatures. The line (*left*) leads to the camera.

In this century much has been added to that inventory and to our understanding of deep-living animals. The investigations were conducted, however, by means of trawls and dredges, by direct observation from deep-diving submersible vehicles and by inspection of photographs made by cameras lowered on cables from ships, and it is characteristic of these methods that they ordinarily sample only the stationary or slow-moving animals of the deep-sea communities; the deeper the investigation is, the more difficult and the less effective it is. That gave rise to the prevalent assumption of a few years ago that the ocean bottom was sparsely inhabited by weak creatures specially adapted to live on the only food material then thought to be available at great depths: a terminal food web supported by the thin but constant rain of detritus that sifts down from the surface layers and that is metabolized by primitive filter feeders or bacteria and deposit feeders in the bottom ooze. A proper reconnaissance of the active deep-sea creatures whose presence was revealed by the baited traps required new tools.

Over the past seven years Meredith Sessions, Richard Shutts and the authors, working at the Scripps Institution of Oceanography, have designed and constructed robot motion-picture and still cameras and other instruments with which to explore the bottom and to study the nature, distribution and behavior of its inhabitants. We find, first of all, that the population of sea-floor invertebrates is by no means sparse and that many of its species are far from weak. And they are not alone. Their domain is shared by a population of scavenging fishes and crustaceans adapted for the prompt discovery and consumption of larger falls of food: the bodies of dead animals descending from above, mid-water creatures that happen to approach the bottom and juveniles of their own species that return to the deeps from the shallower water where they undergo their early development.

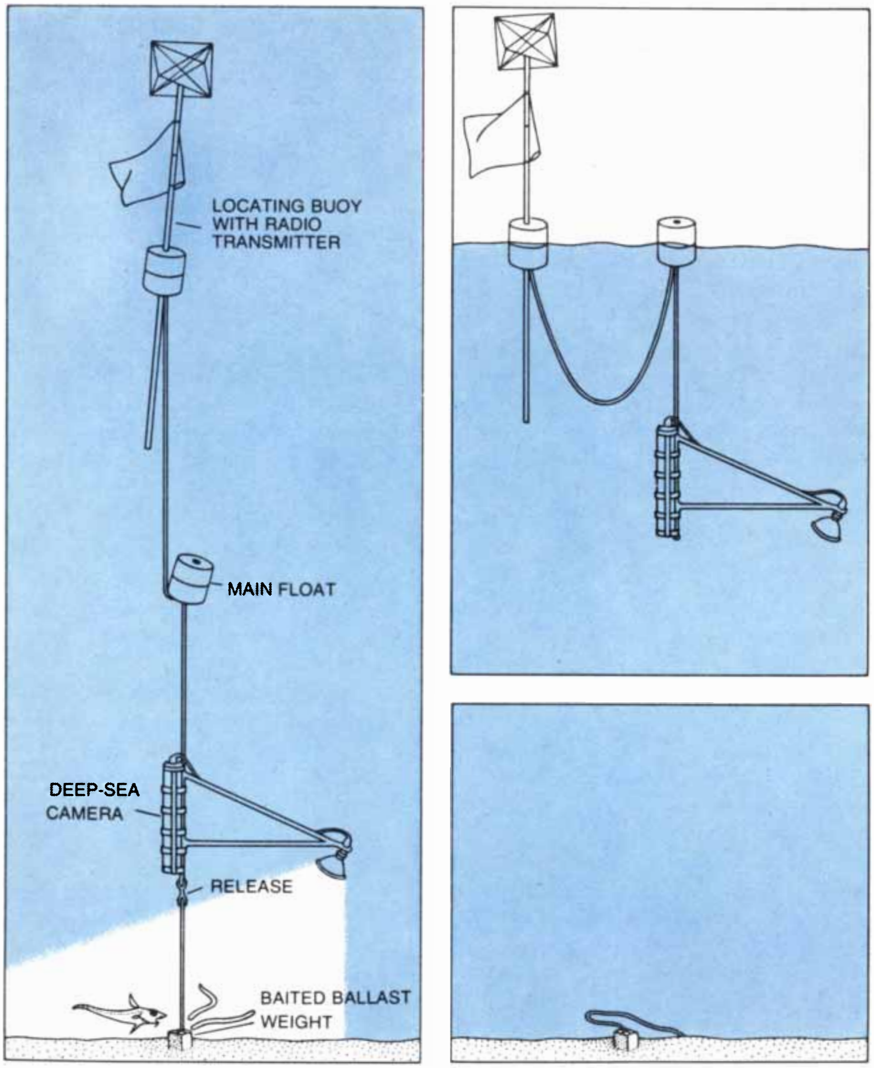
The cameras we have devised for the ocean-floor study are free-fall devices connected to a recovery buoy, a floodlight and a bait holder. Released at the surface, the camera falls to the bottom and remains there, anchored by the bait holder for between 12 and 48 hours, making still photographs or short bursts of motion pictures at five- to 15-minute intervals. The bait is in the foreground of each frame and the camera's view is either vertical or oblique. At the end of the mission the bait ballast is released and the camera and its buoy rise to the surface, where a radio transmitter broadcasts a signal that aids in recovery. The free-fall technique allows a research ship to conduct various other missions while a number of cameras it has distributed keep functioning on the sea floor. And the work can be done from rather simple, inexpensive vessels, provided only that they have reasonable navigation and sonic-sounding equipment.

In a typical drop the camera is released in water that is between 400 and 7,000 meters deep. Greater depths call for a special camera housing and flotation gear because of the immense pressures. The camera operates during the descent, which may take as long as several hours, but ordinarily not much is seen in photographs made on the way down except for the "snow" of small particles, which are ubiquitous in the midwaters of the sea, and a few fleeting small crustaceans.

Sometimes the very first photographs

on the bottom, made a few minutes after the camera reaches it, show great activity, with fishes and invertebrates already tearing at the bait. More commonly, however, the first scenes show only brittle stars, large shrimps, amphipods (small crustaceans) and perhaps a fish or two. On more than half of the missions at least one fish has been photographed within 30 minutes of the camera's arrival on the bottom, even at the greater depths. (Curiously the first photographs sometimes record a fish of a species that is not seen again in the sequence, as though the camera had invaded the territory of a creature that was not a part of the population of active scavengers and wanted nothing to do with the subsequent activity.)

Usually the number of fish gathered around the bait increases slowly, reaching a maximum after a few hours. Often the scene develops into one of furious activity, with several species of fish competing for the bait, thrashing and tearing at it and sometimes attacking one another. Shrimps, brittle stars, amphipods and other invertebrates encroach on the melee. In almost half of the sequences from drops down to 2,000 meters the party ends abruptly after three to eight hours, when some creature, usually a large shark, moves in, frightens off the other fish and consumes the bulk of the bait. In any case the time comes when most of the bait has been eaten. The fish depart, and slowly crabs, sea urchins, snails and other such creatures arrive to complete the task of sanitizing the sea floor.

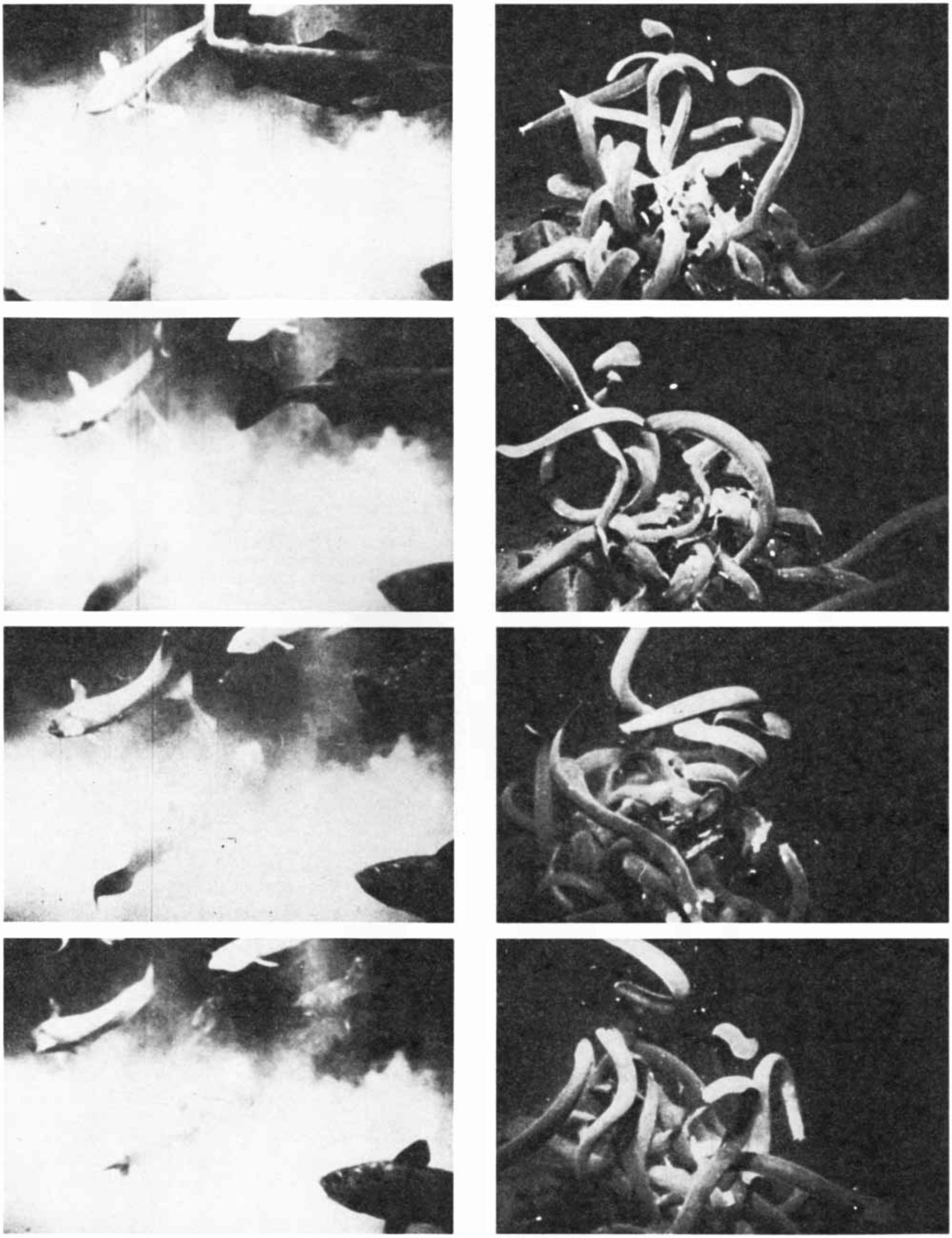


FREE-FALL CAMERA is dropped to the ocean bottom, where it is held in position by a baited ballast weight and a float made of foamed plastic (*left*). The camera can be suspended above the bait as shown here or can be positioned off to the side. At a preset time a clock mechanism cuts the camera system loose from the ballast and the system rises to the surface (*right*). An attached buoy with a flag, a radar target and a radio transmitter marks the site.

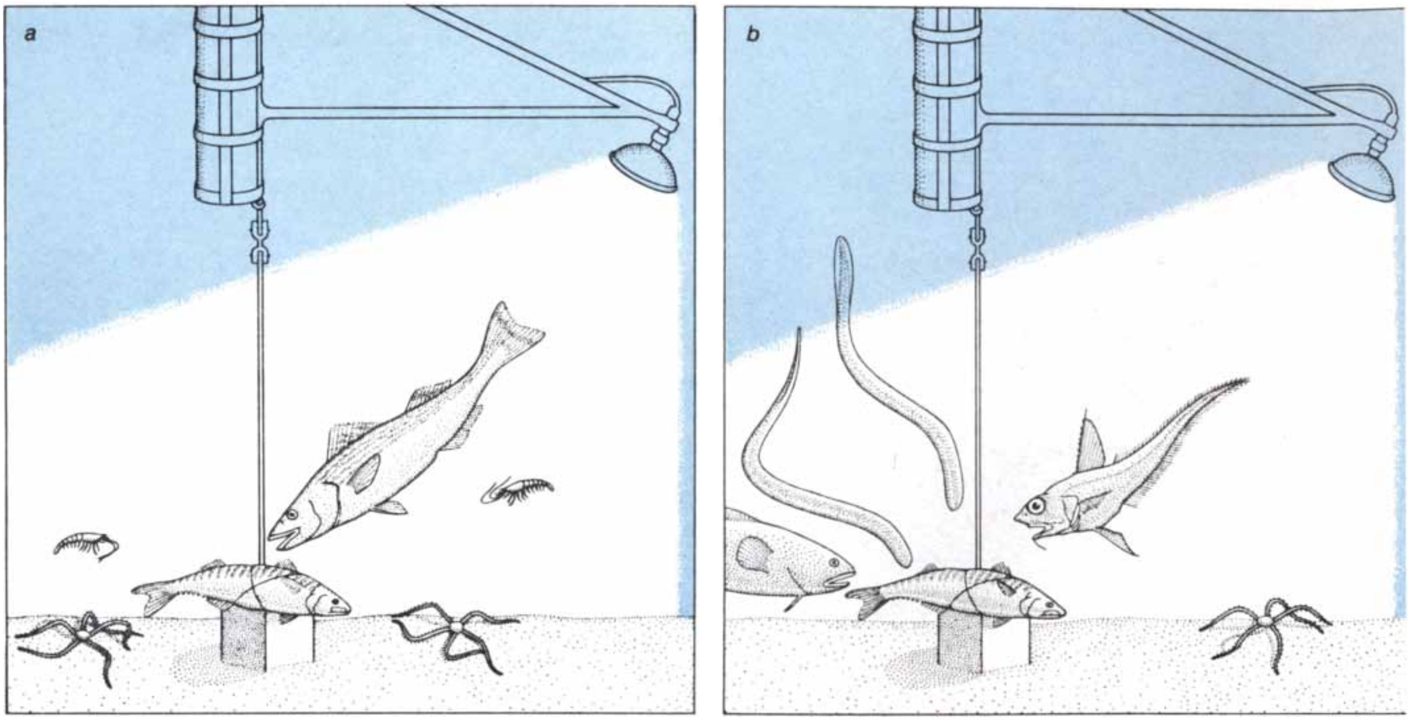
Sometimes the direction of the near-bottom currents has been determined and it can be seen that these latecomers to the banquet plod upstream toward the bait—an indication that they are probably following a scent. The fishes also probably depend on scent for close-in detection of the bait; on some occasions we could relate the number of fish gathered around the bait to the strength of the current, which suggests that an increased current had carried the scent more widely. Like wide-ranging scavengers on land, however, fish that are farther away are probably led to the area of the bait by other cues. They may sense the successive collapse of loosely established territories as the scavengers that held them move closer to the bait, each invading the area once held by an absent neighbor. The western prairie wolf, vultures and other terrestrial scavengers respond to just such a territorial collapse to converge on large kills.

Surprisingly, we find particularly abundant bottom populations in regions of the Pacific where the surface populations are the least abundant, and vice versa. For example, under the least productive surface waters of the North Pacific Gyre, on an austere bottom paved with manganese nodules nearly 6,000 meters down, more than 40 large fish and shrimps were attracted by the bait within a few hours; at least four species were represented. Moderately productive areas of the Indian Ocean, the California Current system and biologically poor areas around the Hawaiian Islands have also shown remarkably large numbers of deep-sea creatures. On the other hand, only 4,000 meters down under the most productive oceanic waters of the world, in the Antarctic over a bottom of soft organic ooze, the bait was visited by only a few eelpouts, brotulids and rattail fish (grenadiers), along with some small crustaceans, the louselike isopods. The brotulids (fishes distantly related to cods) remained almost motionless for hours, nibbling gently at the bait. Camera drops along the underwater ridges of the Line Islands, a highly productive equatorial area of the Pacific, revealed only a few eels. Photographs from the bottom below the rich Peru Current showed very few fish but large numbers of invertebrates: furious masses of amphipods that stripped the bait in a few hours.

The explanation for such a puzzling distribution must lie in the nature of this part of the marine food web. These abyssal roving scavengers must depend for their sustenance in substantial part on



TWO MOTION-PICTURE SEQUENCES show the frenzied activity that develops around the bait. These pictures were made with a 16-millimeter camera dropped to the Pacific floor at a depth of 1,300 meters some 50 miles off the coast of southern California. In sequence at left a variety of fishes attack bait, stirring up the sediment on the bottom. In the sequence at the right the bait is entirely hidden by a mass of hagfish: primitive eyeless and jawless chordates that drill into the bait and consume it from the inside out.

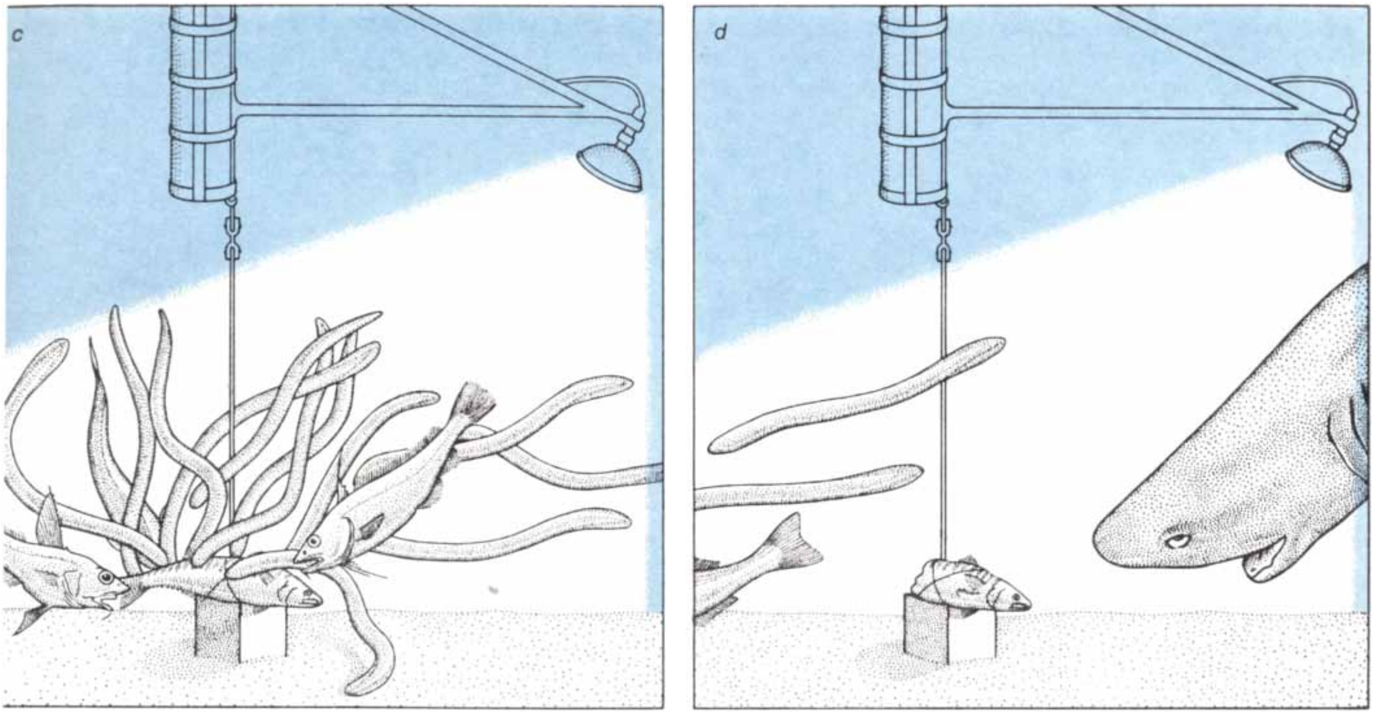


ONE LIKELY SEQUENCE OF EVENTS following the arrival of a baited camera system on the ocean floor is shown in the idealized drawings on these two pages. The first few photographs made after touchdown usually show only brittle stars, shrimps and other small crustaceans (*a*). Within 30 minutes or so the larger fishes begin to arrive on the scene (*b*). The number of fish gathered around the

falls of large food particles. Such particles include fish and marine mammals that have died, fragments of forage from surface-feeding schools of fish, vertically migrating creatures, garbage from ships and (nowadays) animals killed by underwater explosions, ships' propellers and whaling. (The North Pacific Gyre photographs were made below an area patrolled by a weather ship, and her garbage may be partly responsible for the large numbers of animals observed there.) Falling food material can support a population of deep scavengers only if the food descends to the bottom, and it may be that the mid-water population below highly productive regions is sufficiently dense and continuous to consume such food on the way down. Regions of low productivity, on the other hand, may support such sparse and discontinuous mid-water populations that a substantial proportion of dead surface creatures do fall all the way to the sea floor. And there on the floor even a meager fall can support a sizable population, whose scavenging can be much more intensive than that of fish living on the relatively brief passage of falls through the immense volume of the mid-waters.

Somewhat different explanations are possible, perhaps in combination with this mid-water-population effect. The western north-central Pacific, where so many fish were photographed over a manganese-nodule bottom, is an area through which tuna and other large surface fishes and whales and other cetaceans periodically migrate. Just as terrestrial deserts are traps for aged, infirm or injured creatures, so may this marine desert be a trap. The number of large migrants that succumb in traversing its unproductive surface waters may be sufficient to support the bottom population. We know nothing of the "natural" end of the largest fishes and cetaceans; it could well be that these vast areas of low productivity inflict the final stress on aging and infirm members of the populations of great marine creatures, exhausting their ultimate reserves. Deep below areas of high productivity, on the other hand, the principal descending food may be rather small particles, sufficient to support a sizable and continuous population of mid-water creatures as well as throngs of small bottom scavengers that do not need to rove far for sustenance. Moreover, in such waters the large near-surface animals have plentiful food and are not in such a precarious situation as the schools of large migrants crossing the great marine deserts. Areas of high productivity might therefore fail to support the larger scavengers of the deep-sea floor; few active, roving fish would be searching for large food falls such as our bait represents.

Some of the fishes now being observed for the first time at great depths at low latitudes turn out to be species that are well known in near-surface waters at high latitudes. The flatnose codling, the sablefish and the arctic sleeper shark are common inhabitants of the bottom off the coast of southern California and Lower California; the sablefish, which is found in commercially valuable numbers off the coast of Washington, British Columbia and Alaska, is particularly abundant on the deep-sea floor of the Southern California Bight thousands of miles to the south. On the basis of the incidence of this species in a random series of pictures made with unbaited cameras, we have estimated that there are 800,000 tons of sablefish at depths of between 800 and 1,500 meters in the southern California waters; they seem to be of the same race as the commercially harvested variety far to the north. At least along the Pacific coast of North America, then, some species that are generally considered arctic fishes apparently represent mere near-surface outcroppings of populations that inhabit



bait increases slowly, reaching a maximum after a few hours (*c*). After three to eight hours some much larger creature, usually a shark, moves in and consumes the bulk of the bait (*d*). Afterward sea urchins and snails accumulate to finish off the scraps. The latecomers to the feast appear to sense some kind of territorial collapse as successive waves of scavengers converge on large kills.

the cold, deep waters of continental borders to the south. If that is true of other coasts as well, the size and extent of a number of fish populations may be dramatically larger than has been thought. The total numbers of large shrimps in particular must be immense, since they are almost always attracted to the bait at all depths and in all regions we have sampled.

In one of our sequences a small brown shark appears in the bottom waters of the Santa Barbara Basin, which contain virtually no dissolved oxygen. We have also photographed dead mid-water fish on the bottom there. The combination suggests another source of food in some deep waters. Perhaps anoxic basins serve as traps for unsuspecting vertically migrating animals that have never encountered pockets of suffocating water. A scavenger adapted for short forays into such a basin could find a rich harvest. Perhaps the brown shark is so adapted, able to survive a brief period of oxygen deficit.

Sometimes no fish appear in our pictures; the bait is completely consumed by vast numbers of invertebrates such as shrimps and swarming amphipods. Motion pictures from the Santa Cruz Basin off southern California show the amphipods accumulating until the bait is totally covered and the surrounding water is nearly saturated by a roiling mass of these crustaceans. In that sequence one sablefish came near the bait but left immediately, gaping as if to rid itself of amphipods. Is it possible that where there are swarms of aggressive invertebrates such as amphipods, fish are not able to compete for the small amount of food that reaches the bottom? Amphipods can sustain themselves on small food particles, but they are also capable of quickly and completely devouring the much larger pieces that fall to the ocean floor. Deep-living octopuses are other invertebrates that tend to monopolize the bait. In one motion-picture sequence made at about 4,000 meters a small octopus squats on the bait, keeping the grenadiers at tentacle's length, and in an unusual sequence made with a still camera off Cedros Island in Lower California two octopuses fend off a single grenadier from the bait.

The bait is often taken over aggressively and completely by innumerable hagfish, primitive eyeless and jawless chordates that drill into dead creatures and consume them from the inside out [see "The Hagfish," by David Jensen; SCIENTIFIC AMERICAN, February, 1966]. Hagfish thrive at a depth of from 200 meters to nearly 2,000 meters. At first we were puzzled by the reluctance of other fishes to penetrate the Gorgon's-head tangle of hagfish and feed on the bait. Closeup motion pictures gave the answer: the hagfish enclose the bait in a thick cocoon of slime that other fishes apparently find distressing. On a number of occasions fish emerge from the feeding mass making frantic efforts to clear their gills of slime. The spectacular ability of the hagfish to exude slime has long been known; the exudation of a single hagfish can convert a large container of water into a slimy gel. Their employment of this defense mechanism to sequester food, however, had not been suggested.

Some of the scavenging bottom fishes display a feeding capability that could scarcely be predicted from an examination of their jaw and tooth structure. The grenadiers are quite capable of tearing out the abdomen of a bait fish; sablefish shake large baits as terriers do and spin furiously to twist off mouthfuls of food.

The large sharks that frequent the deep-ocean floor have been photographed to depths of about 2,000 meters. Their behavior in approaching the bait appears to be mediated in part by a sense of smell: the fish execute slow, deliberate geometrical maneuvers that apparently combine to establish a complex search pattern. On some occasions, when the bait holder has been hung somewhat above a rough bottom, the sharks are quite unable to discover it. Clearly they are accustomed only to food resting on the sea floor; when they cannot find it, they nudge and bite at rocks or other sea-floor objects under the bait. Even their search for bait that is on the ocean floor usually requires more than one sortie before the bait is found. Several picture sequences end just as the shark is in a position to seize the bait, with the bait dead ahead within the width of the shark's jaws. Yet the next sequence, made five minutes later, may show the bait untouched and the shark still engaged in a slow, deliberate search. Like sharks, hagfish are unable to discover the bait when it is a meter or so above the bottom, whereas eels and grenadiers have no difficulty locating bait that is well above the bottom. Knowledge of such feeding limitations will help in the development of better techniques for some deep-water fisheries, where the depredations of sharks and hagfish greatly limit the catch.

One of the shyest fishes we have photographed, and perhaps one of the deepest-living, is the brotulid. In a number of photographs from the deepest locations brotulids lurk at the outer edge of a group of grenadiers. In one motion-picture series a brotulid hovers like a motionless blimp through sequence after sequence, facing the feeding grenadiers. Unlike most of the deep-water fishes, the brotulids have very reduced eyes, and it may be that their behavior is related to diminished visual acuity. Creatures living more than several hundred meters below the surface are maneuvering in a profound darkness that is lighted only by the faint glow or brighter brief flashes of bioluminescent organisms. Only fishes with a highly developed visual system can be expected to be guided by visual cues. Brotulids may be responding not to the presence of the bait but to the sound of the feeding fish.

Many sequences yield fresh insights into feeding behavior. A crab gingerly lifts some sea urchins off the bait, holds them away from its body like a spider ejecting a distasteful insect from its web and drops them. Sheltered by the empty bait holder, a small spiny lobster flails its antennae in a strong current, apparently grasps a small swimming crustacean between the antennae and conveys it to its mouth by some movement too quick for us to make out. A grenadier goes after small food particles in the sediment with a sudden explosive thrust into the bottom, throwing a cloud of sediment through its gills.

SWIRLING MASS of shrimps and amphipods (deep-sea relatives of sand fleas) completely covers the bait and fills the surrounding water in this oblique photograph, made at a depth of 7,000 meters in the Peru-Chile Trench. Exhibiting surprisingly aggressive behavior, the small crustaceans stripped the bait in a few hours. Curiously no fishes appeared in this sequence of pictures, filmed under one of the most highly productive fisheries in the oceans.

The uniformly large size of most of the fish photographed at great depths presents something of a puzzle. Very few small fish are ever seen. The grenadiers photographed at from 750 to 6,000 meters in the Pacific, Antarctic and Indian oceans are all large, mature fish, some measuring more than a meter in length. We have so far recovered no free grenadier eggs or juvenile fish from collections made off California anywhere between the surface and the bottom, although females with ripe ovaries have been collected near the bottom. All the arctic sleeper sharks photographed off southern California and Lower California have been very large, but most of the photographs have shown only a small portion of their total length, which we can only estimate as being between five and eight meters. They also must be quite common, since they have been photographed in nearly half of our missions off California down to 2,000 meters; on a number of occasions when more than one large shark was photographed during a mission, we could tell from distinguishing scars and other markings that the sharks were different individuals.

It is probable that the juveniles of these deep-living fishes inhabit much shallower depths than the adults. The young of the sablefish, for example, are numerous in many places at depths of 100 meters or so. It may be that the rarity of juveniles is merely the result of great adult longevity and low fecundity. On the other hand, juveniles of many species must return to the deep bottom environment; indeed, their return may constitute a meaningful importation of food from the more productive upper layers for the nourishment of the total adult population. We usually think of the relation between juveniles and adult stocks only in terms of replacement. Juveniles may also be important as prey; indeed, in some freshwater environments the young of a species are a principal prey of adults of the same species. To the degree that this process, which has been called Faginism, is important to the food economy of the deep-sea populations, we would of course expect to observe a paucity of juveniles in the populations: most of them are quickly consumed by their elders!

The baited camera suffers from some limitations common to many simple exploratory tools. Its sample is highly selective. Quantification and even identification can be doubtful. There are several ways to deal with these problems. One can, for example, set out fishlines and traps in order to retrieve specimens for sure identification. A particularly promising means of quantification is an unbaited drifting camera that lightens itself until it rises just a few meters above the bottom. It drifts with the current, making overlapping photographs and meanwhile recording the direction and distance of drift. Such a camera has been successfully operated in waters about 1,000 meters deep. We have plans to develop a drifting camera that will remain submerged for several months, making photographs along a drift track as long as 200 kilometers. The drifting camera should yield a meaningful census of the creatures that have been observed by stationary baited cameras and thus help us to assess the potential food harvest from the deep-ocean floor and to understand the ramifications of this remarkable branch of the marine food web.

Meanwhile vast areas await investigation with the baited camera. A series of photographic "sections" across the continental shelves along all the major land masses and down the slopes to the abyss would fill many gaps in our knowledge of bottom-dwelling animals and should reveal entirely new fishery resources. Among the environments that are of special interest to oceanographers and marine biologists are the floor of the Arctic Ocean, the deep delta of the Congo River, the Antarctic slope, the deeps of the Mediterranean, the top of seamounts and mid-ocean ridges and the slope and bottom of deep oceanic trenches. Since the free-fall cameras can be operated from inexpensive, unspecialized craft, they are particularly suitable for studies by investigators in underdeveloped countries who want to know more about the creatures and the potential deep fisheries off their coast.

MUSICAL ILLUSIONS

Presenting certain sequences of tones simultaneously to both ears produces paradoxical auditory illusions. Surprisingly, right-handed subjects and left-handed subjects perceive the illusions differently

by Diana Deutsch

When we listen to music, we do not merely hear a set of independent tones; we perceive the tones as being linked together in combinations such as melodies and chords. One of the central problems in the study of the perception of music is how the human brain sorts and organizes a complex set of tonal stimuli into such combinations. When I set out to investigate the principles by which we link successive tones into musical sequences, I obtained some surprising results, including a series of paradoxical auditory and musical illusions. Even more surprising, right-handed and left-handed subjects perceive the auditory illusions in different ways.

In these experiments a computer was programmed to control two sine-wave generators so that the tones could be precisely regulated in terms of amplitude, duration and frequency. The technique used is known as dichotic presentation. The tonal sequences were presented to the listener through earphones so that when one ear received one tone, the other ear received another tone.

In the first experiment the listener heard a sequence consisting simply of a high tone alternating with a low tone. The tones were in octave relation and their frequencies were 400 and 800 hertz; on the musical scale these are closest to G_4 (392 hertz) and G_5 (784 hertz). The sequence was presented simultaneously to both ears at equal amplitude. The sequence at one ear, however, was out of phase with the sequence at the other: when one ear received the high tone, the other ear received the low tone, and vice versa.

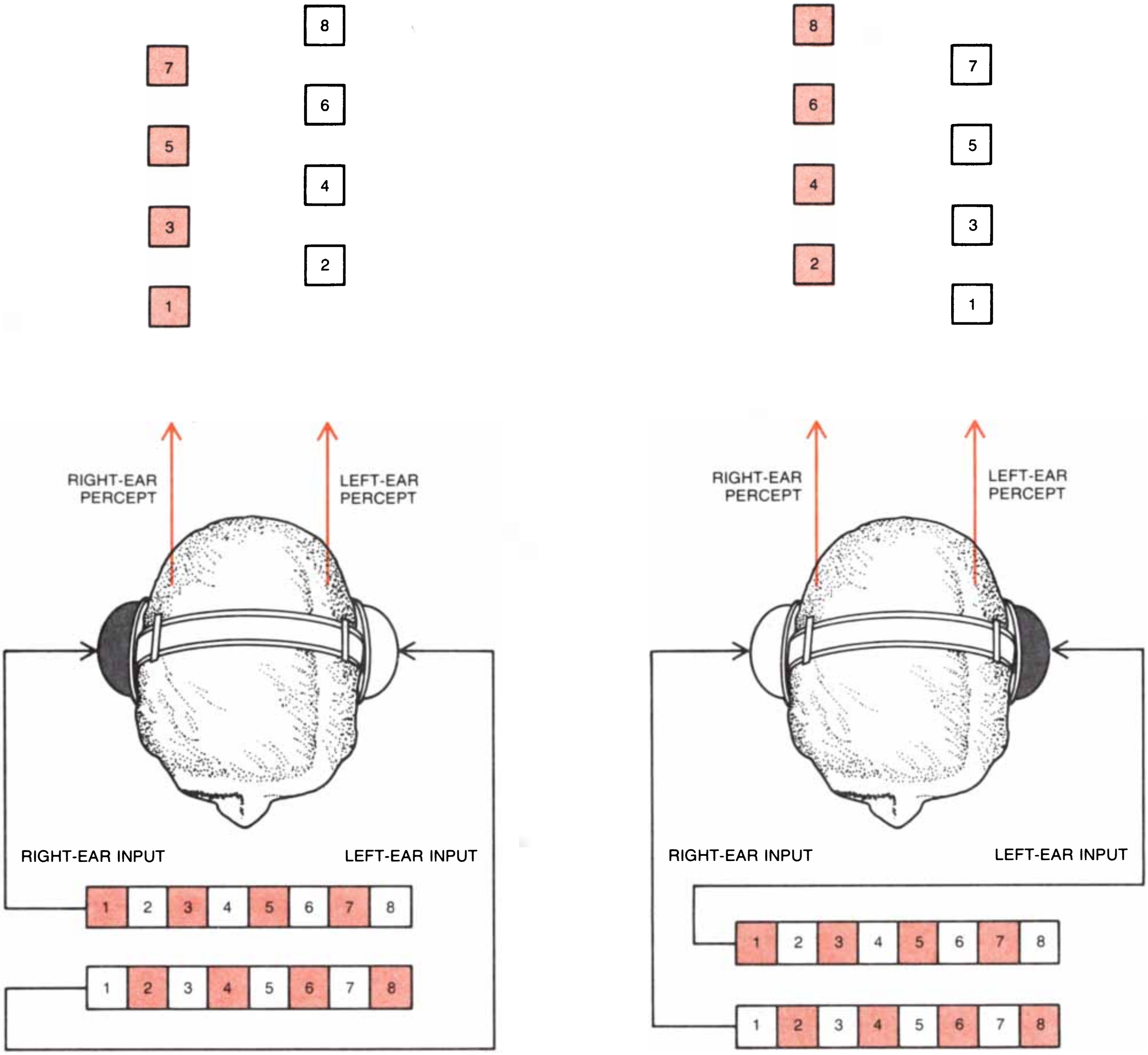
Although the listener was presented with a single, uninterrupted two-tone chord, I have found only one individual in the 100 or so I have tested who was able to describe the two-tone chord correctly. Most listeners heard only a single tone that shifted from one ear to the other, and as it shifted, its pitch simultaneously shifted from the high tone to the low one. In other words, the listener alternately heard the high tone in one ear and the low tone in the other. When the earphones were reversed, most people experienced exactly the same thing: the ear that had previously heard the high tone still heard it, and the ear that had heard the low tone continued to hear the low tone. It seemed, however, that the earphone that had originally emitted the high tones was now emitting the low tones and that the earphone that had emitted the low tones was now emitting the high ones [*see top illustration on opposite page*].

Right-handed subjects tended strongly to hear the high tone in their right ear and the low tone in their left ear and to maintain this percept when the earphones were reversed. Left-handed subjects were just as likely to localize the high tone in their left ear as in their right. In right-handed people the left hemisphere of the brain is dominant, and its primary auditory input comes from the right ear. In left-handed people either hemisphere may be dominant. The difference in the localization of the tones in right-handed and left-handed subjects suggests that high tones are perceived as coming from the ear that provides the strongest input to the dominant hemisphere, and that low tones are perceived as coming from the ear that provides the strongest input to the nondominant hemisphere.

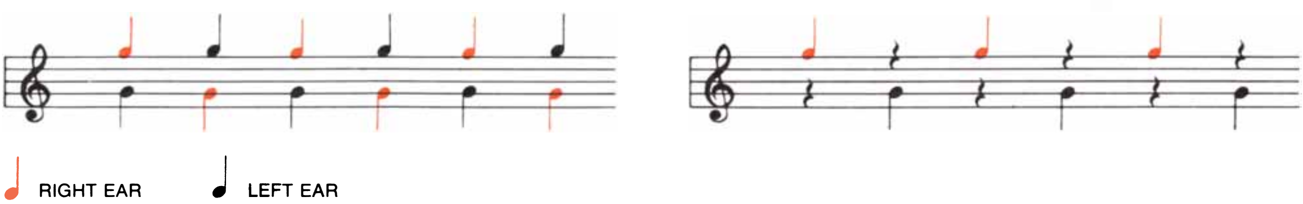
Although most listeners showed a preference for localizing the high tone in one ear and the low tone in the other, it often happened that after continued listening the high and the low tones suddenly reversed position. Such reversals occurred without warning in the middle of a sequence, but they were most likely to occur when the sequence was abruptly discontinued and then started afresh. Some subjects experienced frequent reversals. We have here, I believe, an auditory analogue of reversing visual figures such as the Necker cube [*see illustration on page 95*]. In both the auditory illusion and the visual one the percepts alternate spontaneously and never occur simultaneously.

The two-tone illusion presents a paradox for theories of pitch perception and auditory localization. If we assume that the listener attends to one ear and ignores the other, then the alternating pitches should both seem to be localized in the same ear. Alternatively, if we assume that the listener attends to each ear in turn, the perceived tone should not change in pitch as it shifts from ear to ear. The fact remains that for most people the high tone is heard as though in one ear and the low tone as though in the other. The paradox is that the low tone is localized in an ear that is actually receiving a high tone at that moment.

Some people did perceive the two-tone chord as a single tone that alternated from one ear to the other, with the pitch either remaining the same or changing only slightly. Others reported complex percepts, such as two low tones alternating from ear to ear together with an intermittent high tone in one ear, or a sequence in which the pitch relations seemed to change gradually over a period of time. Some listeners remarked on striking differences between the timbres of the tones, for example that the low tones sounded like a gong and the high tones like a flute. These complex percepts tended to be unstable and often



TWO-TONE AUDITORY ILLUSION is created when a sequence consisting of a high tone (*colored squares*) alternating with a low tone (*white squares*) is presented so that when one ear receives the high tone, the other ear simultaneously receives the low tone, and vice versa. Each tone was a quarter of a second in duration. The frequencies were 800 and 400 hertz. The sequence is presented for 20 seconds. Most right-handed subjects hear a high tone in the right ear alternating with a low tone in the left ear. The surprising thing is that the low tone is localized to the left ear while that ear is actually receiving a high tone. When the earphones are reversed (*right*), most people continue to hear the high tone in the right ear and the low tone in the left ear. To the listener it seems that the earphone that previously emitted the high tones was now emitting the low tones and that the earphone that had emitted the low tones was now emitting the high tones. Most left-handed subjects also hear a high tone in one ear alternating with a low tone in the other ear, but the high tone is just as likely to be localized in the left ear as in the right. Some listeners hear a single tone that alternates from ear to ear. Others, particularly left-handers, report complex percepts consisting of several tones.



REPRESENTATION IN MUSICAL NOTATION of the two-tone sequence is given at the left. The most common illusory percept is depicted at the right. The closest notes in the musical scale to the actual 400- and 800-hertz tones are G_4 (392 hertz) and G_5 (784 hertz).

SUDDEN REVERSAL of the high tone and the low tone occurs for some subjects with prolonged listening to the two-tone sequence. The high tone seems to shift from the right ear to the left, and simultaneously the low tone shifts from the left ear to the right.

changed within a few seconds. Complex percepts were reported by a higher proportion of left-handed subjects than right-handed ones.

Is the alternating-tone illusion based on the absolute pitch levels in the stimulus sequence or on the relative pitch levels? To find out I selected a group of listeners who had consistently localized the high tone in the right ear and the low tone in the left ear and who showed no tendency to reverse the pattern. I presented three different sequences of tones to these subjects: the first dichotic sequence alternated tones of 200 and 400 hertz; the second, tones of 400 and 800 hertz, and the third, tones of 800 and 1,600 hertz. Virtually all the subjects reported hearing the higher tone of each sequence in the right ear and the lower tone in the left ear. The results clearly show that the illusion is based on the pitch relation between the competing tones.

How can we account for this auditory illusion? There is clearly no simple explanation, but we may suppose separate brain mechanisms exist for determining what pitch we hear and for determining where the tone appears to be coming from. Indeed, these two mechanisms may even be differentiated anatomically. Half a century ago the neuroanatomist S. Poljak proposed that there is an anatomical separation in the lower levels of the auditory system between the mechanisms serving discriminatory functions and those serving localization functions. Recently E. F. Evans and P. G. Nelson of the University of Keele in England have provided neurophysiological support for this scheme. There seems to be a similar separation in the visual system. Gerald E. Schneider of the Massachusetts Institute of Technology has found that if the part of the brain known as the superior colliculus is removed from a hamster, the animal can discriminate between patterns but cannot tell where an object is. On the other hand, if the visual cortex of the brain is removed, the hamster shows poor pattern discrimination but can easily locate objects in space.

If we suppose there are two separate auditory mechanisms, one for determining the pitch we hear and another for determining where the sound is coming from, we are in a position to explain the illusion. An additional hypothesis that is needed is that although under the conditions of the illusion only one ear follows the sound for pitch, the perceived tone is localized by the brain toward the ear receiving the higher tone. Let us take the case of a listener who follows the se-

THREE SETS OF PAIRED TONES were tested to determine whether the two-tone illusion is based on absolute pitch levels or on the pitch relation between the tones. The subjects were listeners who had previously localized the 800-hertz tone in the right ear and the 400-hertz tone in the left ear. The first set (*Stimulus A*) had tones of 200 and 400 hertz, the second set (*B*) had tones of 400 and 800 hertz and the third set (*C*) had tones of 800 and 1,600 hertz. In each set one ear received a high tone while the other ear simultaneously received a low tone. The results were clear: the higher of the two tones was almost always localized in the right ear and the lower of the two tones in the left ear. This indicates the illusion is based on the pitch relation between competing tones.

quence of pitches that is delivered to his right ear. When the high tone is presented to the right ear and the low tone to the left, the listener should hear a high tone in his right ear, since the right ear is both determining pitch and receiving the higher tone.

When the low tone is presented to the right ear and the high tone to the left, the listener should hear a low tone, since it is the tone presented to his pitch-determining ear, but the tone should seem to be coming from the left ear because the brain localizes the perceived tone at the ear that is receiving the higher tone. The entire sequence should therefore be a high tone in the right ear alternating with a low tone in the left. It is obvious that reversing the position of the earphones would not alter the perceived sequence.

In the case of a listener who follows the sequence of pitches that is delivered to his left ear, the dichotic sequence would be perceived as a high tone in the left ear alternating with a low tone in the right. And the reversals experienced by some listeners would be due to a change in which ear is following pitch.

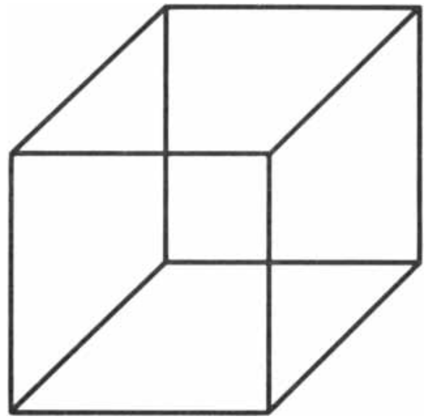
In collaboration with P. L. Roll, a graduate student in the department of psychology at the University of California at San Diego, I devised an experiment to test that hypothesis. Three consecutive high tones were presented to the listener's right ear and three low tones were simultaneously presented to the left ear. Then two low tones were presented to the right ear and two high tones to the left ear, again simultaneously. Listeners heard the pattern 10 times without pause and then reversed their earphones and listened to it again [*see lower illustration on next page*].

The results confirmed the hypothesis. The perceived tone, regardless of whether it was high or low, appeared to come from the ear that was receiving the higher frequency. As for the pattern of the tones, subjects who were right-handed tended significantly to hear the pattern delivered to the right ear rather than the pattern delivered to the left ear. That is, when the right ear received three high tones followed by two low tones, the listener reported hearing three high tones in his right ear followed by two low tones that appeared to be coming from his left ear.

When the earphones were reversed, a person who always followed pitch with the same ear experienced a new illusion. The ear that heard three high tones before the reversal of the earphones now heard only two high tones, and the ear that heard two low tones now heard three low tones. Reversal of the earphones thus seems to cause one of the high tones to disappear and to create an additional low tone, even though there is absolutely no change in the dichotic sequence being presented.

In another experiment I presented the two-tone sequence through loudspeakers rather than earphones. The subject stood in an anechoic (echo-free) room, equidistant between two loudspeakers, one on his left and one on his right. When a low tone came from the loudspeaker on the left, a high tone came simultaneously from the loudspeaker on the right, and vice versa. The subject heard a set of high tones apparently emanating from the loudspeaker on the right that alternated with a set of low tones from the speaker on the left. As the listener turned slowly he continued to hear the high tones on his right and the low tones on his left until he was facing one speaker and the other speaker was directly behind him. He then heard a single tone of constant pitch that seemed to be coming from both speakers. If the listener continued to turn in the same direction until he had rotated 180 degrees from his original position, the speaker that had originally seemed to be emitting the high tones now seemed to be emitting the low tones, and the speaker that had been emitting the low tones now seemed to be emitting the high tones.

The illusion also occurs when the loudspeakers are placed side by side, both facing the listener, and even when they are placed at some distance. This indicates that the illusion is based not on simple competition between the ears but rather on competition between different regions of perceived auditory space.



REVERSAL OF NECKER CUBE, in which the back face periodically becomes the front one, is analogous to the sudden reversals experienced in listening to the two-tone sequence (*see upper illustration on opposite page*). In both this visual illusion and the auditory one reversals occur spontaneously.

The illusions I have been describing are based on two alternating tones. What happens if the listener is presented with more elaborate musical sequences instead? In one experiment I devised a dichotic sequence consisting of the C-major scale in its ascending and its descending forms. When a note from the ascending scale was presented to one ear, a note from the descending scale was simultaneously presented to the other ear, with the successive notes in each scale alternating between ears [*see illustration on page 97*].

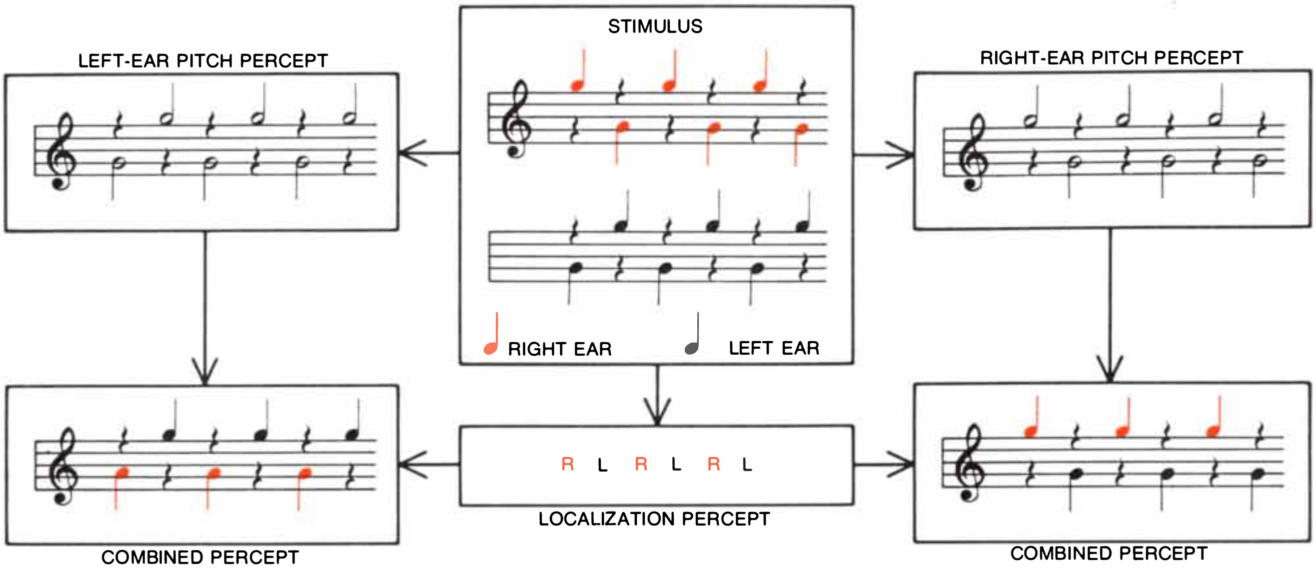
This musical sequence generates another set of illusions. About half of the right-handed subjects heard the correct sequence of pitches but heard them as two separate melodies, a higher one and a lower one. The two melodies appeared to be moving in opposite directions with respect to pitch. Moreover, the higher tones all seemed to be emanating from the right earphone and the lower tones from the left earphone. When the earphones were reversed, there was no change in what was perceived. I should add that my own percept is the same. The higher tones appear quite unambiguously to come from the right earphone and the lower tones from the left, however many times I reverse the earphones.

Other subjects perceived the sequence differently. A few reported hearing all the higher tones in the left ear and all the lower tones in the right ear, regardless of how the earphones were positioned. For still other subjects, when the earphones were reversed, the apparent location of the tones was reversed also. Right-handers and left-handers were found to differ in terms of these localization patterns. Right-handers showed a strong tendency to hear the higher tones on the right and the lower tones on the left; left-handers, however, displayed no such tendency. Thus it appears that we tend to refer the higher tones to the dominant side of auditory space and the lower tones to the nondominant side.

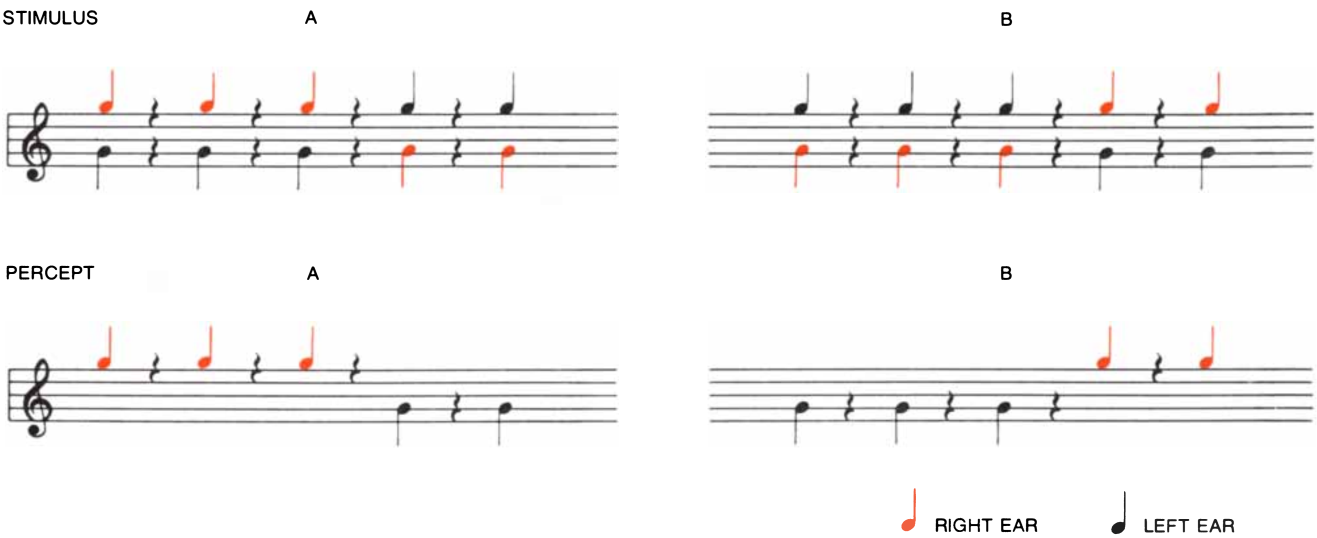
A few listeners perceived the sequence as being composed of higher and lower melodic lines moving in opposite directions, but they localized the individual tones in a variety of unpredictable ways.

Some subjects reported hearing only one sequence of four tones that repetitively descended and ascended. When they were asked to sing along with the sequence, they sang the higher tones and not the lower ones. Interestingly enough, a third of these listeners correctly identified the switching of individual notes between ears. The other two-thirds reported a variety of effects, such as hearing the entire sequence in one ear, or having the sequence travel from the left ear to the right as the tones went from high to low and travel back again to the left ear as the tones went from low to high.

When I analyzed the reports of the subjects who had listened to this musical sequence, I found that all the listeners, regardless of how they perceived it, formed perceptual groupings of tones based on frequency range. That is, they either heard all the tones as two simultaneous nonoverlapping pitch sequences



TWO AUDITORY DECISION MECHANISMS, one for determining apparent pitch and the other for determining where the sound appears to be coming from, could interact to create the two-tone illusion. It is hypothesized that the perceived tone is always localized in the ear that is receiving the higher tone but that only one ear follows the sound for pitch. If the listener follows the sequence of pitches delivered to his right ear, the combined operation of the two auditory decision mechanisms results in the percept of a high tone in the right ear followed by a low tone in the left ear (*percept at lower right*). If the listener follows the sequence of pitches delivered to his left ear, however, high tone is heard in the left ear and low tone is heard in the right ear (*percept lower left*).



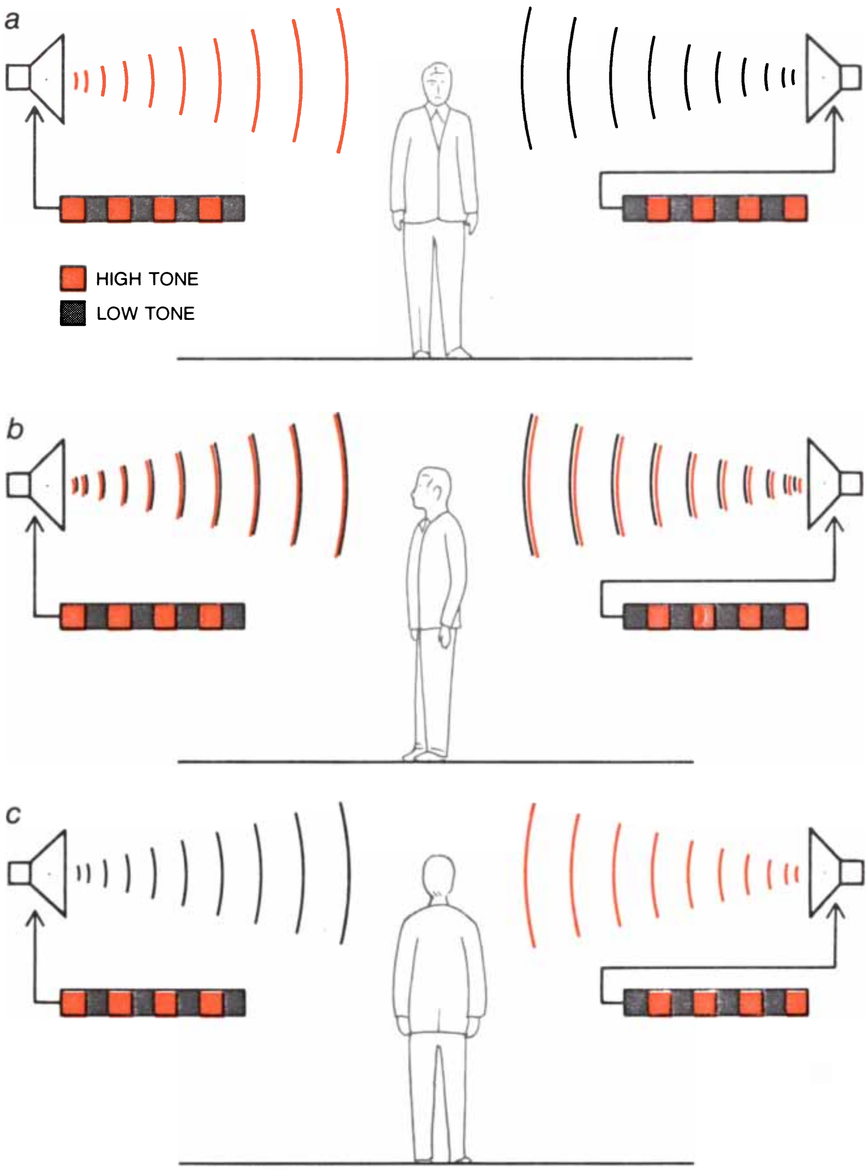
TONAL SEQUENCE that presents a different pattern of pitches to each ear was devised to investigate the mechanisms determining apparent pitch and the localization of sound. The pattern to the right ear consisted of three high tones followed by two low tones; the pattern to the left ear consisted of three low tones followed by two high tones (*Stimulus A*). The sequence was presented simultaneously to both ears and was repeated 10 times. Most right-handed subjects perceived three high tones in the right ear followed by two low tones in the left ear (*Percept A*). In other words, they heard only the pattern presented to the right ear, but each tone was localized to the ear receiving the higher frequency. When the earphones were reversed (*Stimulus B*), subjects now heard three low tones in the left ear and two high tones in the right ear. Again the pattern presented to the right ear was the one that was followed.

that ascended or descended in opposite directions, or they heard the higher tones and little or nothing of the lower ones. Since most people in our culture are much more familiar with the major scale than they are with the melodic patterns reported by the subjects, it is particularly surprising that not one of the subjects reported hearing a full ascending or descending scale.

In a further test of the effect I presented the dichotic musical sequence to a group of subjects and then had them listen to only the ascending-scale component of the sequence. When the subjects were asked if the ascending scale had been a part of the total sequence, all of them replied that it had not. It appears that the mechanism responsible for grouping tonal stimuli by their frequency range is so powerful that it can mask the perception of a familiar musical scale present in the total sequence.

An important part of listening to music involves the linking of tonal stimuli into sequences. When more than one tone is presented at a time, the listener is forced to decide which successive tone to link with which. Knowledge of the rules underlying how such linkages are made is of critical importance if we are to understand the perception of music. Half a century ago Max Wertheimer, one of the founders of the Gestalt school of psychology, proposed several principles of perceptual organization. One of them is the principle of proximity, which states that nearer elements are grouped together in preference to elements that are spaced farther apart. Another is the principle of good continuation, which states that elements that follow each other in a given direction are perceived as belonging together. Wertheimer's principles are easily demonstrated by visual examples, and indeed two principles can be set in opposition in a single demonstration. When that is done, one principle of organization often proves to be stronger than the other [see top illustration on page 103].

The paradoxical musical sequence is another example of conflict between two principles of perceptual organization. If the principle of good continuation is applied, we should perceive either the full ascending scale or the full descending one. On the other hand, if the principle of proximity is applied, we should group the higher tones together and the lower tones together. And as we have seen, a subject who listens to the paradoxical sequence always applies the principle of proximity.



EXPERIMENTAL ARRANGEMENT for creating an auditory illusion with loudspeakers instead of earphones is depicted. The listener stands between two loudspeakers in an echo-free room (*a*). When one speaker is playing a high tone, the other speaker is simultaneously playing a low tone, and vice versa. To the listener, however, it appears that the speaker on his right is emitting only high tones (*colored arcs*) that alternate with low tones (*black arcs*) from the speaker at his left. When the listener turns to face one of the speakers (*b*), he now hears a single tone of constant pitch apparently coming from both speakers. If the listener turns again so that he has rotated 180 degrees from his original position, the speaker that originally appeared to be emitting the high tones now appears to be emitting the low tones, and the speaker that had emitted low tones now appears to be emitting high tones (*c*).

The grouping of tonal stimuli by frequency range is often found in traditional music. When a solo instrument plays a melody and its accompaniment, the two elements are generally in different frequency ranges; more often than not the melody is in the higher range.

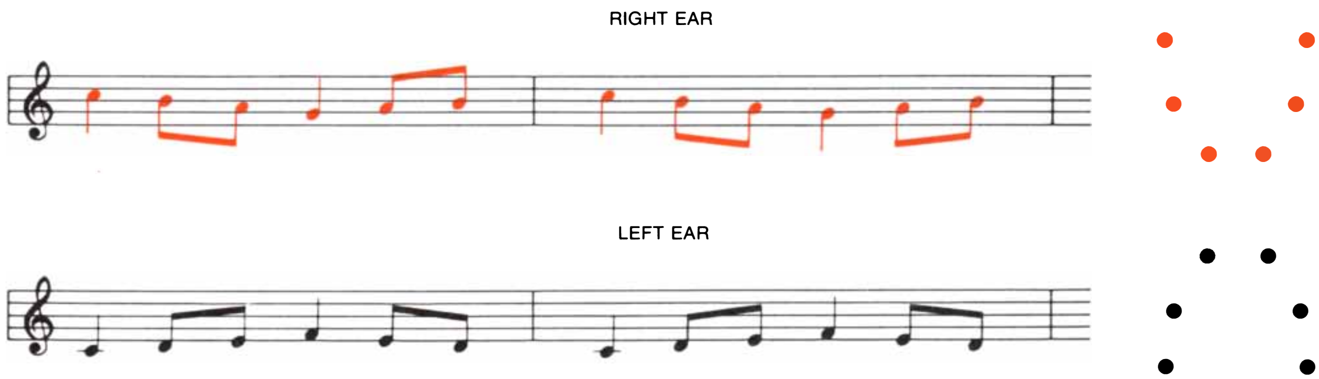
An interesting musical technique used by classical composers is the presentation of a sequence of tones in rapid succession, alternating between two frequency ranges, with the result that they are heard as two melodic lines [see bottom illustration on page 103]. W. J. Dowling, who was then at the University of California at Los Angeles, has investigated the effect under experimental conditions. He presented pairs of well-known melodies so that successive notes came from different melodies. When the pitch ranges overlapped, recognition of the melodies was very difficult. When the two melodies were in different pitch ranges, recognition was much easier. Dowling interpreted his findings in terms of the tendency to group tonal stimuli into separate pitch ranges so that tones in different ranges do not interfere with one another.

Albert S. Bregman and John Campbell of McGill University have investigated another interesting perceptual property of very rapid sequences of tones that are drawn from two separate frequency ranges. Listeners found it difficult to preceive the order of tones in such sequences, although the problem did not arise when the tones were close together in pitch. It appears that if the rate of presentation is very rapid, we cannot form order relations between the elements of different tonal streams.

Why do many listeners, on hearing the paradoxical musical sequence I have described, localize all the higher tones in one ear and all the lower tones in the other? Since all the tones are perceived, the illusion must have a basis different from that of the two-tone-chord illusion. The musical illusion is created by tones from overlapping pitch ranges. In everyday life similar sounds are likely to emanate from the same source and different sounds from different sources. Hence the best interpretation of the dichotic musical sequence, in terms of the real world, is the assumption that sounds in one frequency range are emitted from one earphone and sounds in the other frequency range are emitted from the other earphone. The power of unconscious inference is so strong that it overrides the actual localization cues.

Unconscious inference as the basis of

PARADOXICAL MUSICAL SEQUENCE consists of the C-major scale in its ascending and descending forms (*a*). When a component of the ascending scale is presented to one ear, the matching component of the descending scale is presented to the other ear. Successive notes in ascending scale (*b*) and in descending scale (*c*) actually alternate from ear to ear. The most common illusion reported is two melodies, a higher one and a lower one (*d*). The higher melody is heard in the right ear and the lower one in the left ear.



PERCEPTION OF PARADOXICAL SEQUENCE by a right-handed subject who was musically sophisticated and had absolute pitch is depicted in musical notation (*left*). When he reversed the earphones, he wrote: "Same result: high in right ear." A subject who had no musical training depicted the illusion in diagram form (*right*). Asked to sing the two melodies, she sang them correctly.

You want the ozone question
So does

And most scientists agree there is time to find the answer.

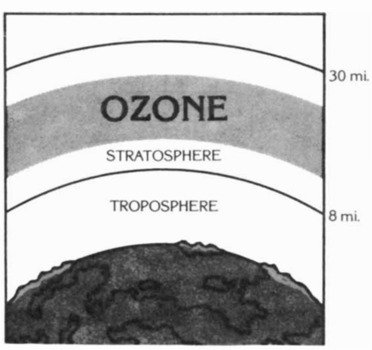
Fluorocarbons are liquids and gases used in refrigeration, for air conditioning, and as propellants in about half the aerosol spray cans sold in this country. Some say that these useful, normally safe compounds will cause a health hazard by attacking the earth's ozone layer. We believe this is an oversimplification.

The point is, to date there is no conclusive evidence to prove this statement. To understand, then, why there is a controversy, it is necessary to unsimplify the issue. We must treat the real world on its own terms, and they are complex.

The model that raised the question.

Ozone is continually created and destroyed by natural forces scientists are seeking to understand. The ozone depletion theory, based on a computer model of the stratosphere, was reported in 1974 by two chemists at the University of California.

This mathematical model calculates how fluorocarbons

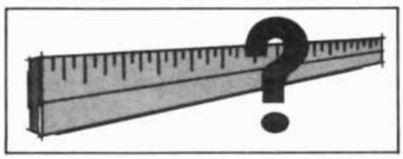


in the stratosphere behave under the influence of a series of variables (such as temperature, altitude, sunlight, chemical concentration) to affect the ozone layer.

In order to estimate hypothetical reactions, and because little is actually known about the real ones, the modelers made a number of assumptions about the way the upper atmosphere behaves.

The unmeasured yardstick.

Before any judgments can be made using this model as a stratospheric yardstick, its accuracy must be determined.



Does it describe the real, three-dimensional world? To find this out, the validity of the modeler's basic assumptions must be determined.

Turning assumptions into facts.

Before a valuable industry is hypothesized out of existence, more facts are needed. To get these facts Du Pont and the other fluorocarbon manufacturers are funding independent technological investigations in universities and research laboratories. Under the direction of acknowledged scientific experts, this research is designed to either prove or disprove the assumptions most important to the computer case against fluorocarbons.

Some research has been carried out since the model was first presented. Scientists now have a better idea of the accuracy of the assumptions in the model.

ASSUMPTION: The ozone-depleting reaction with chlorine from fluorocarbons takes place at a rate that demands an immediate decision on fluorocarbon use.

FACT: Recent determinations of reaction rates disclose that the ozone/chlorine reaction actually takes place at a slower rate than that assumed by the model. In addition, the same research has shown that the reaction of chlorine with stratospheric methane proceeds at a faster rate. Since this reaction tends to remove chlorine from the ozone layer, the net effect of both reactions is to lessen the originally-calculated impact of fluorocarbons. In fact, the impact was overstated by 300%.

RESEARCH: To guide future measurements of stratospheric reactions, a laboratory program has been funded to measure the reactions of chlorine compounds and ozone under simulated stratospheric conditions.

Most scientists agree there is time to conduct the research needed to settle the controversy one way or the other... before a final decision is made on fluorocarbon production and use.

ASSUMPTION: There is no other way to get fluorocarbons out of the atmosphere except by the ozone-depleting reaction.

FACT: One well-known class of chemical reactions not considered in the model is that of chlorine compounds in the

answered one way or the other. Du Pont.

atmosphere in heterogeneous reactions.

In an article in SCIENCE (Feb. 14, 1975), Professors S. C. Wofsy, M. B. McElroy, and N. D. Sze of Harvard University caution that "If additional removal processes could be identified... or if additional sinks could be identified for stratospheric odd chlorine, the atmospheric and biological impacts of [fluorocarbons] would be reduced accordingly."

RESEARCH: Atmospheric chemistry involving ion molecule reactions has been described in recent months by several investigators. Reaction rates with ion molecules are known to be extremely fast and are believed to occur primarily in the lower stratosphere.

Thus, ion molecules could react with fluorocarbons, allowing them to be removed from the atmosphere.

ASSUMPTION: Fluorocarbons are the only significant source of chlorine available for interaction with ozone in the stratosphere.

FACT: Many chlorine-containing materials are present in the atmosphere in varying concentrations. Of particular significance, large amounts of methyl chloride and carbon tetrachloride have been discovered in the troposphere and stratosphere.

In addition, new calculations on the injection of gaseous chlorine compounds into the stratosphere from volcanic eruptions have shown this as a significant contributor of chlorine not taken into account by the model.

RESEARCH: Scientists are completing an inventory of chlorine-containing compounds in the atmosphere. It must be determined how nature deals with chlorine from these natural sources, before it can be shown that chlorine from fluorocarbons might pose a threat to the ozone layer.

Additional research.

A fluorocarbon industry research program is funding the development of a computer model that will better reflect the complex chemistry of the stratosphere.

In addition, other studies are under way to broaden our understanding of the total ozone production/destruction balance. These will concern themselves with other stratospheric reactions affecting ozone.

A panel of highly qualified academic scientists will advise on the technical programs covering various facets of the problem. This panel of independent experts will review the projects, providing a critical opinion on the pertinence of each, the probability of their success, and the completeness of the overall investigation.

Conclusion.

Much more experimental evidence is needed to evaluate the ozone depletion theory. Fortunately, as most scientists agree, there is time to gather this evidence. Du Pont has joined with other fluorocarbon manufacturers to provide funds for work by independent university scientists. Governmental agencies are also conducting research to help in the assessment of the theory.

Should the theory be proven correct after all the evidence is in, Du Pont, as we have stated, will stop the manufacture and sale of the offending compounds.

In the meantime, we believe that to act without the facts—whether it be to alarm consumers, or to enact restrictive legislation—is irresponsible. Final decisions cannot be made with only the information at hand.

The independent research described above is presently being carried out by scientists at the following institutions:

Cambridge University—England
Environmental Research and Technology, Inc.
Massachusetts Institute of Technology
State University of New York
The Battelle Memorial Institute
The University of Reading—England
University of California
University of Denver
University of Illinois
University of Maryland
University of Michigan
Washington State University
York University—Canada
Xonics, Inc.

This is the second in a series of discussions by Du Pont to offer a perspective on an important subject. If you would like copies of either this or the first discussion, please write to: FREON*, Room 24517, Du Pont Company, Wilmington, DE 19898.

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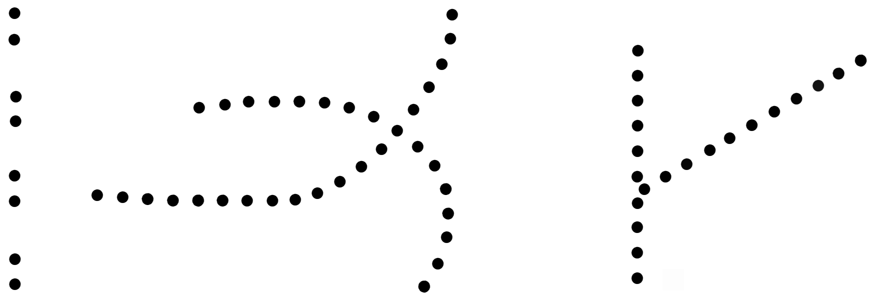
illusions has been well documented in visual perception. A familiar example is the Ames room, which appears to be rectangular when it is viewed monocularly from a certain position but actually is not rectangular at all. Our percept that all rooms are rectangular is so strong that objects or people placed in the Ames room appear to be larger or smaller than they actually are. Another striking example of unconscious inference in visual perception is presented when we view a picture of a mold of a human face [*see illustration on next page*]. Even though the nose and other features of the mold are projecting inward, they always look as though they are projecting outward in the usual orientation. The illusion holds even when we view the actual mold from a distance.

In most right-handed people the left hemisphere of the brain is dominant for speech. Studies have shown that the right hemisphere, which often is called the nondominant hemisphere, also has specialized functions. Studies by neurologists of deficits in music perception resulting from brain injury indicate that such deficits are more likely to exist when there is damage to the dominant hemisphere. The evidence for musical deficits, however, is much less clear-cut than that for speech deficits. It is likely that some musical functions are mediated by both hemispheres. Further, it seems that some musical attributes are processed mainly in the dominant hemisphere and others in the nondominant hemisphere.

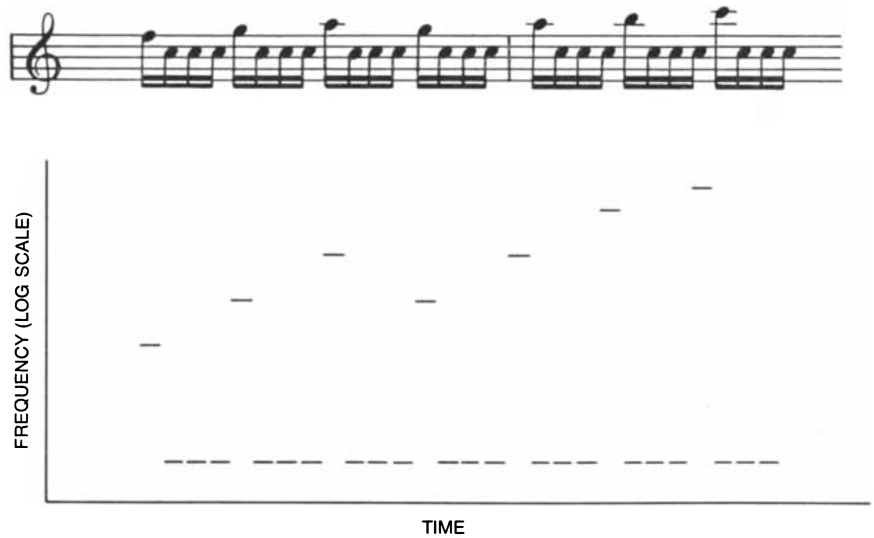
The nondominant hemisphere appears to play a more important role in processing the quality of nonverbal sounds. People with damage to the nondominant hemisphere show deficits in discriminating complex sounds and timbres. Dichotic-listening studies with normal individuals have found that right-handers process complex sounds better through the left ear than through the right.

For example, H. W. Gordon, who was then at the California Institute of Technology, presented pairs of two-tone chords generated by an electric organ simultaneously, one to each ear. Right-handers showed better recognition of the chords they heard in their left ear. Since the left ear has stronger connections to the right hemisphere, it appears that the right, or nondominant, hemisphere has a stronger involvement in the recognition of complex tones or chords.

On the other hand, the processing of sound sequences appears to take place chiefly in the dominant hemisphere. Pa-



PRINCIPLES OF PERCEPTUAL ORGANIZATION, proposed by Max Wertheimer, are demonstrated in these visual examples. The dots that are closer together are perceived as pairs (*left*), illustrating the principle of proximity. Dots that follow each other in a given direction are perceived as lines (*middle*), which demonstrates the principle of good continuation. When these two principles are set in opposition (*right*), one may be dominant. Even though the dot at the junction is closer to the vertical row, it is perceived as belonging to the oblique row. In this instance the principle of good continuation is the stronger.

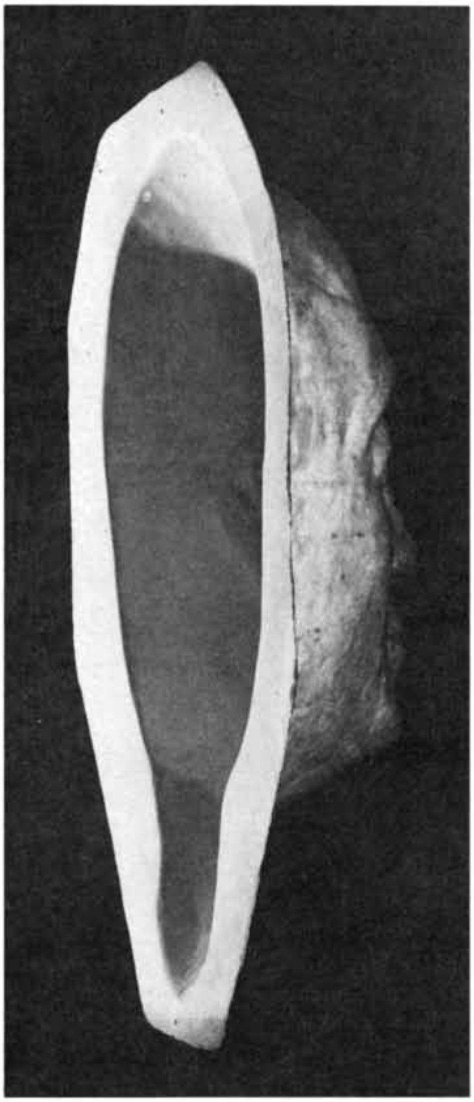


PRINCIPLE OF PROXIMITY plays an important role in the perception of melody. Two excerpts from classical music show how tones from two frequency ranges are grouped into separate melodic lines. In *Capriccio for Recorder and Basso Continuo* (*top*) by Georg Philipp Telemann the sequence creates two separate melodies, each in a different frequency range. In Telemann's *Sonata in C Major for Recorder and Basso Continuo* a repetitive single pitch in the lower range forms a ground against which the melody is heard (*bottom*).

tients with damage to the dominant hemisphere show deficits in identifying the temporal order of auditory stimuli and in processing rhythms. And dichotic listening studies with normal individuals have found that right-handers process sound sequences better through the right ear than through the left. George M. Robinson and D. J. Solomon of Duke University presented pairs of rhythmically patterned tonal sequences simultaneously, one to each ear. Right-handers identified the sequences presented to the right ear better than those presented to the left. Y. Halperin, I. Nachshon and A. Carmon of the Hadassah Musical School in Jerusalem employed dichotic sequences made up of tones of different pitches and also found better identification of the sequences presented to the right ear. These findings are in agreement with the results I obtained with the two-tone-chord illusion in which right-handed listeners tended to follow the sequence of pitches delivered to the right ear.

Since listening to music involves many functions, including the appreciation of timbre and the organization of tonal sequences, it would appear that both cerebral hemispheres play important but to some extent complementary roles in musical perception. The degree of involvement of each hemisphere would of course depend on both the type of music and the perceptual strategy of the listener.

It is clear from the studies with auditory and musical illusions that there are substantial differences among human beings in how even the simplest tonal sequences are perceived when different spatial locations are involved. The musical experience of the listener may well play an important role. The finding that differences in the perception of tonal sequences are correlated with handedness, however, indicates that variations in auditory perception are also very likely to result from differences between individuals at a basic neurological level. Such differences may be responsible for many variations in musical taste and appreciation. Indeed, certain controversies in musical aesthetics may have as their source fundamental differences in the nervous system of the listeners rather than differing evaluations of a common auditory percept.



HOLLOW MOLD OF A HUMAN FACE is shown in the photograph at the left. When the hollow mold is viewed from the back, the face appears to project outward even though the features of the face are actually projecting inward. Because our percept that all faces project outward is so strong, we unconsciously infer that the hollow mold of a face here must be projecting outward. Unconscious inference may also be the basis of the illusion created by the paradoxical musical sequence (*see upper illustration on page 98*).

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The Debate over the Hydrogen Bomb

A recently declassified report sheds light on the original U.S. decision to develop the "Super." The unanimous opposition of the Oppenheimer committee, overruled then, appears now to have been basically correct

by Herbert F. York

In 1948 Czechoslovak Communists carried out a coup in the shadow of the Red Army and replaced the government of that country with one subservient to Moscow. Also in 1948 the Russians unsuccessfully attempted to force the Western allies out of Berlin by blockading all land transport routes to the city. In early 1949 the Communist People's Liberation Army captured Peking and soon afterward established the People's Republic of China. Taken together, these and similar but less dramatic events were generally perceived in the West as resulting in the creation of a monolithic and aggressive alliance stretching the full length of the Eurasian continent, encompassing almost half of the world's people and threatening much of the rest. Then in the fall of 1949 the Russians exploded their first atomic bomb and ended the brief American nuclear monopoly.

At the end of World War II most atomic scientists in the U.S. had estimated that the U.S.S.R. would need four or five years to make a bomb based on the nuclear-fission principle; the time interval from the first American test to the first Russian one turned out to be four years and six weeks. Even so, nearly everyone, including most U.S. Government officials and most members of Congress, reacted to the event as if it were a great surprise. Many of them had either forgotten or had never known the experts' original estimates, and in any case the accomplishment simply did not fit the almost universal view of the U.S.S.R. as a technologically backward nation.

Besides being a great surprise the Russian test explosion was a singularly unpleasant one. The U.S. nuclear monopoly had been seen by many as compensating for the difference between the hordes of conscripts supposedly available to the Communist bloc and the smaller armies available to the Western countries. Coming as it did at a time when virtually all Americans saw the cold war as rapidly going from bad to worse, the Russian test was seen as a challenge that demanded a reply. The immediate challenge being nuclear, a particularly intensive search for an appropriate response was conducted by those responsible for U.S. nuclear policy.

Most of the proposed responses involved substantial but evolutionary changes in the current U.S. nuclear programs: expand the search for additional supplies of fissionable material, step up the production of atomic weapons, adapt such weapons to a broader range of delivery vehicles and end uses, and the like. One proposal was radically different. It called for the fastest possible development of the hydrogen bomb, which was widely referred to at the time as the superbomb (or simply the Super). This weapon, based on the entirely new and as yet untested principle of thermonuclear fusion, was estimated to have the potential of being 1,000 or more times as powerful as the fission bombs that had marked the end of World War II. Work on the theory of the superbomb had already been going on for seven years, but it had never had a very high priority, and so far it had yielded no practical result. A number of scientists and politicians endorsed the proposal, but for years Edward Teller had been its leading advocate. The superbomb proposal led to a brief, intense and highly secret debate.

The opponents of the proposal argued that neither the possession of the new bomb nor the initiation of its development was necessary for maintaining the national security of the U.S., and that under such circumstances it would be morally wrong to initiate the development of such an enormously powerful and destructive weapon. In essence they contended that the world ought to avoid the development and stockpiling of the superbomb if it was at all possible, and that a U.S. decision to forgo it was a necessary precondition for persuading others to do likewise. Furthermore, they concluded that the dynamism and relative status of U.S. nuclear technology were such that the U.S. could safely run the risk that the U.S.S.R. might not practice similar restraint and would instead initiate a secret program of its own.

The advocates of the superbomb maintained that the successful achievement of such a bomb by the Russians was only a matter of time, and so at best our forgoing it would amount to a deliberate decision to become a second-class power, and at worst it would be equivalent to surrender. They added that undertaking the development of the superbomb was morally no different from developing any other weapon.

The secret debate about what the American response ought to be took place within the Government itself. Many organizations were involved, including the National Security Council, the Department of Defense, the Department of State and the Congressional Joint Committee on Atomic Energy, but the initial focus of the debate lay within the Atomic Energy Commission.

The early official reaction of the AEC's Los Alamos Scientific Laboratory to the Russian test was a proposal to step up the pace of the nuclear-weapons program in all areas. Among other measures, Norris E. Bradbury, the director, recommended that the laboratory go on a six-day work week and that they expand the staff, particularly in theoretical physics.

This acceleration was to include not only programs for improving fission weapons by conventional means but also tests of the booster principle. (In this context "booster" refers to a synergistic process in which the explosion of a comparatively large mass of fissionable fuel, say plutonium or uranium 235, causes a comparatively small mass of thermonuclear fuel, say deuterium and tritium, to burn violently. The high-energy neutrons produced in the thermonuclear process then react back on the fission explosion, boosting, or accelerating, it to a higher efficiency than would otherwise be the case.) The booster concept had been known for several years, and even before the Russian test it had been agreed to include a full-scale experimental test of the process in a 1951 nuclear-test series. The AEC's Director of Military Application, General James McCormack, Jr., received these proposals from the Los Alamos laboratory and sought the advice of the AEC's scientific experts on them. Other AEC division heads were similarly studying proposals for expanding the relevant programs within their jurisdiction.

At the same time Teller, then at Los Alamos, Ernest O. Lawrence, Luis W. Alvarez and Wendell M. Latimer at the University of California at Berkeley, Robert LeBaron at the Department of Defense, Senator Brien McMahon, Chairman of the Joint Committee on Atomic Energy, his staff chief William L. Borden and Commissioner Lewis L. Strauss of the AEC had all come to focus on the superbomb as the main element of the answer to the Russian atomic bomb, and they initiated a concerted effort to bring the entire Government around to their point of view as quickly as possible.

As a result of all this concern and activity the AEC called for a special meeting of its General Advisory Committee to be held as soon as possible. This committee was one of the special mechanisms established by the Atomic Energy Act of 1946 for the purpose of managing the postwar development of nuclear energy in the U.S. Its function was to provide the AEC with scientific and technical advice concerning its programs. The members of the committee were all men who had been scientific or technological leaders in major wartime projects. J. Robert Oppenheimer, who was elected chairman of the committee,

FIRST SUPERBOMB TEST in which a large thermonuclear, or fusion, explosion was successfully ignited by a comparatively small fission explosion took place at the Eniwetok Proving Ground in the Marshall Islands on November 1, 1952 (local time). The device, with the code name Mike, released an amount of energy equivalent to that released by the explosion of 10 megatons, or 10 million tons, of TNT. As had been predicted five years earlier by the scientist members of the General Advisory Committee of the Atomic Energy Commission, yield of first superbomb was approximately 1,000 times larger than the yield of the first atomic, or all-fission, bombs.

had been director of the Los Alamos laboratory during the period when the first atomic bomb had been designed and built there. The other members, all scientists, were Oliver E. Buckley, James B. Conant, Lee A. DuBridge, Enrico Fermi, I. I. Rabi, Hartley Rowe, Glenn T. Seaborg and Cyril S. Smith. Many of the members of this committee and later General Advisory committees also served on other high-level standing committees and some key *ad hoc* committees, and so a rather complex web of interlocking advisory-committee memberships developed. As a result several of these men, including Oppenheimer, had much more influence than the simple sum of their various committee memberships would indicate.

Oppenheimer was not only the formal leader of the General Advisory Committee but also, by virtue of his personality and background, its natural leader. His views were therefore of special importance in setting the tone and determining the content of the committee's reports in this matter, as in most other matters.

Throughout Oppenheimer's service on the committee he generally supported the various programs designed to produce and improve nuclear weapons. At the same time he was deeply troubled by what he had wrought at Los Alamos, and he found the notion of bombs of unlimited power particularly repugnant. Ever since the end of the war he had devoted much of his attention to promoting the international control of atomic energy with the ultimate objective of achieving nuclear disarmament. He and Rabi had in effect been the originators of the plan for nuclear-arms control that later became known as the Baruch Plan. Oppenheimer's inner feelings about nuclear weapons were clearly revealed in an often quoted remark: "In some sort of crude sense which no vulgarity, no humor, no overstatement can quite extinguish, the physicists have known sin, and this is a knowledge which they cannot lose."

The call for the special meeting, in addition to raising the question of a high-priority program to develop the Super, also asked the committee to consider priorities in the broadest sense, including "whether the Commission is now doing things we ought to do to serve the paramount objectives of the common defense and security." As for the Super, the Commission wanted to know "whether the nation would use such a weapon if it could be built and what its military worth would be in relation to fission weapons." The meeting of the Oppenheimer committee was held on October 29 and 30, 1949; all members were present except Seaborg, who was in Europe. The committee in the course of its deliberations heard from many outside experts in various relevant fields, including George F. Kennan, the noted student of Russian affairs, General Omar Bradley, Chairman of the Joint Chiefs of Staff, and the physicists H. A. Bethe and Robert Serber. Toward the end of the two-day meeting the advisers had a long session with the Atomic Energy commissioners and with their intelligence staff. The next day the committee prepared its report.

The General Advisory Committee report consisted of three separate sections that were unanimously agreed on and two addenda giving certain specific minority views. In 1974 the report was almost entirely declassified, with only a very few purely technical details remaining secret.

Part I of the report dealt with all pertinent questions other than those directly involving the Super. The advisory committee in effect reacted favorably to the proposals of the various AEC division directors with regard to the expansion of the facilities for separating uranium isotopes, for producing plutonium and for increasing the supplies of uranium ore. These proposals and the committee's endorsement of them were followed eventually by a substantial increase in the rate of production of fissionable materials.

In Part I the committee also recommended the acceleration of research and development work on fission bombs, particularly for tactical purposes. Under the heading "Tactical Delivery" the report stated: "The General Advisory Committee recommends to the Commission an intensification of efforts to make atomic weapons available for tactical purposes, and to give attention to the problem of integration of bomb and carrier design in this field."

This quoted paragraph deserves special emphasis, since it has often been suggested that Oppenheimer, Conant and some of the others opposed nuclear weapons in general. They did apparently find them all repugnant, and they did try hard to create an international control organization that would ultimately lead to their universal abolition. In the absence of any international arms-limitation agreements with reliable control mechanisms, however, they explicitly recognized the need to possess nuclear weapons, particularly for tactical and defensive purposes, and they regularly promoted programs designed to increase their variety, flexibility, efficiency and numbers. For the next few years, right up to the time Oppenheimer's security clearance was removed, he continued strongly to promote the idea of an expanded arsenal of tactical nuclear weapons. The only type of nuclear weapon the General Advisory Committee opposed—and it did so openly—was the Super.

Part I of the report further recommended that a project be initiated for the purpose of producing "freely absorbable neutrons" to be used for the production of uranium 233, tritium and other potentially useful nuclear materials. Perhaps most important of all in the present context, Part I also stated: "We strongly favor, subject to favorable outcome of the 1951 Eniwetok tests, the booster program." This short phrase makes it abundantly clear that the Oppenheimer committee favored conducting research fundamental to understanding the thermonuclear process, and that its grave reservations were specifically and solely focused on one particular application of the fusion process.

Part II discussed the Super. It outlined what was known about the hydrogen bomb, and it expanded on the unusual difficulties its development presented, but it concluded that the bomb could probably be built. In part it said: "It is notable that there appears to be no experimental approach short of actual test which will substantially add to our conviction that a given model will or will not work. Thus, we are faced with a development which cannot be carried to the point of conviction without the actual construction and demonstration of the essential elements of the weapon in question. A final point that needs to be stressed is that many tests may be required before a workable model has been evolved or before it has been established beyond reasonable doubt that no such model can be evolved. Although we are not able to give a specific probability rating for any given model, *we believe that an imaginative and concerted attack on the problem has a better than even chance of producing the weapon within five years.*"

That last sentence (the italics are added) deserves special emphasis. It has been suggested in the past that the General Advisory Committee in general and Oppenheimer in particular were deceptive in their analysis of the technological prospects of the Super; in other words, that they deliberately painted a falsely gloomy picture of its possibilities in order to reinforce their basically ethical opposition to its development. Given the technological circumstances then prevailing, this statement of the program's prospects could hardly have been more positive.

The report then discussed what might be called the "strategic economics" of the Super as they were then conceived: "A second characteristic of the super bomb is that once the problem of initiation has been solved, there is no limit to the explosive power of the bomb itself except that imposed by requirements of delivery. [In addition there will be] very grave contamination problems which can easily be made more acute, and may possibly be rendered less acute, by surrounding the deuterium with uranium or other material.... It is clearly impossible with the vagueness of design and the uncertainty as to performance as we have them at present to give anything like a cost estimate of the super. If one uses the strict criteria of damage area per dollar, it appears uncertain to us whether the super will be cheaper or more expensive than the fission bombs."

In Part III the committee members got to what to them was the heart of the matter, the question of whether or not the Super should be developed: "Although the members of the Advisory Committee are not unanimous in their proposals as to what should be done with regard to the super bomb, there are certain elements of unanimity among us. We all hope that by one means or another the development of these weapons can be avoided. We are all reluctant to see the United States take the initiative in precipitating this development. We are all agreed that it would be wrong at the present moment to commit ourselves to an all-out effort toward its development.

"We are somewhat divided as to the nature of the commitment not to develop the weapon. The majority feel that this should be an unqualified commitment. Others feel that it should be made conditional on the response of the Soviet government to a proposal to renounce such development. The Committee recommends that enough be declassified about the super bomb so that a public statement of policy can be made at this time."

In the two addenda those members of the committee who were present (that is, all except Seaborg) explained their reasons for their proposed "commitment not to develop the weapon." The first addendum was written by Conant and signed by Rowe, Smith, DuBridge, Buckley and Oppenheimer. In part it said: "We base our recommendation on our belief that the extreme dangers to mankind inherent in the proposal wholly outweigh any military advantage that could come from this development. Let it be clearly realized that this is a super weapon; it is in a totally different category from an atomic bomb. The reason for developing such super bombs would be to have the capacity to devastate a vast area with a single bomb. Its use would involve a decision to slaughter a vast number of civilians. We are alarmed as to the possible global effects of the radioactivity generated by the explosion of a few super bombs of conceivable magnitude. If super bombs will work at all, there is no inherent limit in the destructive power that may be attained with them. Therefore, a super bomb might become a weapon of genocide.

"We believe a super bomb should never be produced. Mankind would be far better off not to have a demonstration of the feasibility of such a weapon until the present climate of world opinion changes.

"In determining not to proceed to develop the super bomb, we see a unique opportunity of providing by example some limitations on the totality of war and thus of limiting the fear and arousing the hopes of mankind."

Contrary to a frequently suggested notion, the members of the Oppenheimer committee were not at all unmindful of the possibility that the U.S.S.R. might develop the Super no matter what the U.S. did. Indeed, they regarded it as entirely possible and explained why it would not be crucial: "To the argument that the Russians may succeed in developing this weapon, we would reply that our undertaking it will not prove a deterrent to them. Should they use the weapon against us, reprisals by our large stock of atomic bombs would be comparably effective to the use of a 'Super.'"

The minority addendum, signed by Fermi and Rabi, expressed even stronger opposition to the Super but loosely coupled an American renunciation with a proposal for a worldwide pledge not to proceed: "It is clear that the use of such a weapon cannot be justified on any ethical ground which gives a human being a certain individuality and dignity even if he happens to be a resident of an enemy country.

"The fact that no limits exist to the destructiveness of this weapon makes its very existence and the knowledge of its construction a danger to humanity as a whole. It is necessarily an evil thing considered in any light.

"For these reasons we believe it important for the President of the United States to tell the American public, and the world, that we think it wrong on fundamental ethical principles to initiate a program of development of such a weapon. At the same time it would be appropriate to invite the nations of the world to join us in a solemn pledge not to proceed in the development of construction of weapons of this category."

As with the majority, Fermi and Rabi also explicitly took up the possibility that the Russians might proceed on their own, or even go back on a pledge not to: "If such a pledge were accepted even without control machinery, it appears highly probable that an advanced state of development leading to a test by another power could be detected by available physical means. Furthermore, we have in our possession, in our stockpile of atomic bombs, the means for adequate 'military' retaliation for the production or use of a 'Super.'"

On December 2 and 3, five weeks after the special meeting, the General Advisory Committee convened for one of its regularly scheduled meetings and carefully reviewed the question of the Super once again. According to Richard G. Hewlett, the AEC's official historian, Oppenheimer reported to the commissioners that no member wished to change the views expressed in the October 30 report.

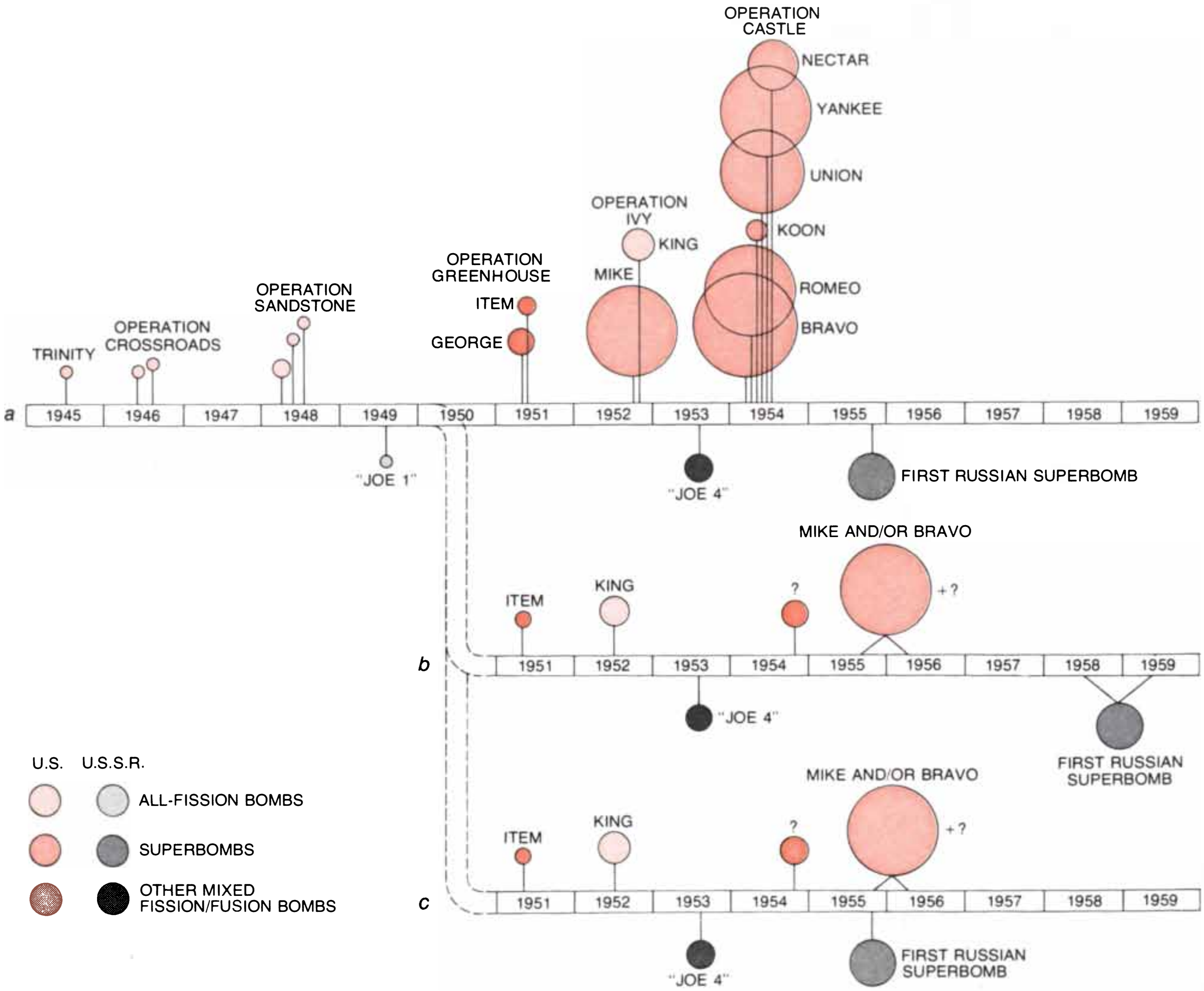
For a time it appeared that the views of the Oppenheimer committee had a chance of being accepted. David E. Lilienthal, chairman of the AEC, was receptive to the committee's point of view. He similarly favored two parallel responses to the Russian test: (1) increasing the production of fission weapons and developing a greater variety of them, particularly for tactical situations, and (2) officially announcing our intention to refrain from proceeding with the Super while simultaneously reopening and intensifying the search for international control of all kinds of weapons of mass destruction. Lilienthal considered the complete reliance on weapons of mass destruction to be a fundamental weakness in U.S. policy, and he viewed a "crash" program on the hydrogen bomb as foreclosing what might be the last good opportunity to base U.S. foreign policy on "something better than a headlong rush into war with weapons of mass destruction." "We are," he said, "today relying on an asset that is readily depreciating for us, i.e., weapons of mass destruction. [A decision to go ahead with the Super] would tend to confuse and, unwittingly, hide that fact and make it more difficult to find some other course."

As we know now, the advice of the Oppenheimer committee was rejected. Early in 1950 President Truman, acting on the basis of his own political judgment and on the totality of the advice he had received on the matter, issued directives designed to set in motion a major U.S. program to develop the hydrogen bomb.

It is not possible here to give a full description of what happened next, but the following chronological outline of the Russian and American superbomb programs is designed to show how the "race" for the superbomb did in fact come out, and to facilitate making judgments about the General Advisory Committee's advice and about "what might have been."

First of all, it is now known that both countries initiated high-priority programs for the development of a hydrogen bomb at about the same time (late 1949–early 1950), and both had been seriously studying the subject for some years before that.

The first U.S. test series that included experiments designed to investigate thermonuclear explosions took place at Eniwetok in the spring of 1951. Known as Operation Greenhouse, the series included two thermonuclear experiments. One, with the code name Item, was a test of the booster principle. This experiment, it must be emphasized, was planned and programmed before the first Russian atomic-bomb test. The other (which actually took place first) was called George. It was a response to Joe 1, as the first Russian atomic-bomb test was called by the U.S. intelligence establishment. Reduced to its essentials, the purpose of the experiment was to show, as a minimum, that a thermonuclear reaction could under ideal conditions be made to proceed in an experimental device. This experiment came to play a key role in the Super program. As Teller later put it: "We needed a significant test. Without such a test no one of us could have had the confidence to proceed further along speculations, inventions and the difficult choice of the most promising possibility. This test was to play the role of a pilot plant in our development."



TWO HYPOTHETICAL OUTCOMES are postulated in an effort to evaluate how much risk would have been involved in a U.S. decision not to proceed with the superbomb. They are depicted in this historical chart as branches of the time line representing the actual world (*a*). The first branch is referred to by the author as the "most probable alternative world" (*b*), the second as the "worst plausible alternative world" (*c*). Both branches originate at January, 1950, the date President Truman announced his decision to go ahead with the superbomb. The circles denote nuclear-test explosions; the labels are U.S. code names. Area of each circle is proportional to the region that could be destroyed by that bomb. Bombs of "nominal" size (less than 50 kilotons) have been omitted after 1950.

The George shot served its purpose well. During the final stages of calculations concerned with the expected performance of this device, Teller and Stanislaw Ulam came up with the climactic idea that made it possible to achieve the goal of the superbomb program: they invented a configuration that would make it possible for a small fission explosion to ignite an arbitrarily large fusion explosion.

The first test of a device designed to ignite a large thermonuclear explosion by means of a comparatively small quantity of fissionable material took place at Eniwetok on November 1, 1952 (local time). The device, known as Mike, produced a tremendous explosion, equivalent in its energy release to 10 megatons (10 million tons) of TNT. As had been repeatedly predicted since the early 1940's, the yield was roughly 1,000 times larger than the yield of the first atomic bombs. For certain practical reasons relating to the pioneering nature of the test, this first version of the Teller-Ulam configuration had liquid deuterium as its thermonuclear fuel. (The last point needs special emphasis. The Teller-Ulam invention, contrary to folklore, was not the notion of substituting easy-to-handle lithium deuteride for the hard-to-handle liquid deuterium. That possibility had been recognized several years earlier.)

Also in November, 1952, the U.S. tested a very powerful fission bomb, with the code name King, that had an explosive yield of 500 kilotons, or half a megaton. Its purpose was to provide the U.S. with an extraordinarily powerful bomb by means of a straightforward extension of fission-weapons technology, in case such large bombs should become necessary for any strategic or political reason. Originally proposed by Bethe as a substitute for the Super program, it became instead a backup for it.

The first Russian explosion involving fusion reactions took place on August 12, 1953. Russian descriptions of this test and later ones confirm that it was not a superbomb. It was only some tens of times as big as the standard atomic bombs of the day, about the same size as but probably smaller than King, the largest U.S. fission bomb. It evidently involved one of several possible straightforward configurations for igniting a fairly small amount of thermonuclear material with a comparatively large amount of fissionable material. It was the first device anywhere to use lithium deuteride as a fuel, and presumably it could have been readily converted into a practical weapon if there had been any point in doing so. It seems to have been a development step the U.S. bypassed in its successful search for a configuration that would make it possible to produce an arbitrarily large explosion with a relatively small quantity of fissionable material.

In the spring of 1954 the U.S. successfully exploded six more variants of the superbomb in Operation Castle. Their yields varied widely. The first and most famous of these tests, with the code name Bravo, was exploded on March 1, 1954, at Bikini. Its design, which was initiated before the Mike explosion, also incorporated the Teller-Ulam configuration, but it had the more practical lithium deuteride as its thermonuclear fuel. Bravo's yield was 15 megatons, even more than Mike's, and it was readily adaptable to delivery by aircraft.

On November 23, 1955, the U.S.S.R. exploded a bomb that had a yield of a few megatons. According to a statement made by Secretary Khrushchev, this device involved an "important new achievement" that made it possible by "using a relatively small quantity of fissionable material... to produce an explosion of several megatons." Khrushchev's remark is generally taken as confirmation that the test was the first one in which the Russians incorporated the Teller-Ulam configuration or something like it. It also used lithium deuteride as a fuel and was therefore a true superbomb, comparable to the U.S. Bravo device exploded 20 months earlier, except for its yield, which was still probably only about a fifth the yield of Bravo.

With this chronology in mind, what can one say about what might have happened if the U.S. had followed the advice of Oppenheimer and the rest of the General Advisory Committee, backed by Lilienthal and the majority of the AEC commissioners, and had not initiated a program for the specific purpose of developing the Super in the spring of 1950?

At best the invention of very large, comparatively inexpensive bombs of the Super type would have been forestalled or substantially delayed. Very probably the work on the booster principle, which presumably would still have gone forward, would have led eventually to the ideas underlying the design of very big bombs, but those ideas might well have been delayed until both President Eisenhower and Secretary Khrushchev were in power. Those two leaders were both more seriously interested in arms-limitation agreements than their predecessors had been, and it is at least possible that they might have been able to deal successfully with the superbomb. To be sure, such a favorable result was not very probable (certainly it had much less than an even chance of coming about), but its achievement would have been so beneficial to mankind that at least some small risk was clearly worth running.

To evaluate just how much risk would have been involved let us next examine three other outcomes, which I have labeled the "actual world," the "most probable alternative world" and the "worst plausible alternative world" [*see illustration on opposite page*].

In both of the hypothetical alternative worlds I assume that the U.S. would have forgone the development of the Super but that the Russians would have ignored this American restraint and would have proceeded at first just as they did in the actual world. I also assume that the U.S. would have vigorously followed the positive elements of the Oppenheimer committee's advice; thus the booster project and other ideas for improving fission bombs would have been accelerated. The difference between the most probable alternative world and the worst plausible alternative world lies in the timing of the test of the first Russian superbomb. In the worst plausible world I assume that this test would have come on the same date that it did in the actual world. In the most probable alternative world, however, I assume that the test would have been substantially delayed.

In both of the two hypothetical alternative worlds, then, the Russians in August, 1953, would have exploded Joe 4, a large bomb deriving part of its explosive energy from a thermonuclear fuel and yielding a few hundred kilotons. Such a device, however, would have had no real effect on the "balance of terror." In both alternative worlds the U.S. would surely have already tested the 500-kiloton all-fission bomb in November, 1952 (or probably earlier, since the timing of Operation Ivy was determined by the availability of the much more complicated Mike device). Therefore the explosion of Joe 4 would have meant that the U.S.S.R. had caught up with but not surpassed the U.S. insofar as the capability of producing enormous damage in a single explosion was concerned.

Then what would have happened?

From that point the Russians might conceivably still have gone on to produce their multimegaton explosion in

November, 1955, but I think it is very probable that they would not have done so until much later. In the actual world they had the powerful stimulus of knowing from our November 1952 test that there was some much better, probably novel way of designing hydrogen bombs so as to produce much larger explosions than the one they demonstrated in their August 1953 experiment. A careful analysis of the radioactive fallout from the Mike explosion may well have provided them with useful information concerning how to go about it. In the hypothetical world where the U.S. would have followed the Oppenheimer-Lilienthal advice that stimulus and information would have been absent. Moreover, a comparison of the way nuclear-weapons technology advanced in the U.S. and the U.S.S.R. during that period makes it seem likely there would have been a much longer delay—probably some years—before they took that big and novel a step without such stimuli and information. Therefore in the most probable alternative world the first Russian superbomb test would have been delayed until well after the first American superbomb test (in other words, delayed until 1957 or 1958), whereas in the worst plausible alternative world it would have occurred just when it did in the actual world: in August, 1955.

What would the U.S. have done in the meantime?

It would have been known immediately that the Russian explosion of August, 1953, was partly thermonuclear and that this test was many times as big as the Russians' previous explosions. If one assumes that following this Russian test the American program in the worst plausible world would have gone along just as it did in the actual world following President Truman's 1950 decision, then the U.S. would have set off the Mike explosion in April, 1956. A simple duplication of those earlier events at this later time, however, would have been unlikely. Any analysis of U.S. reactions to technological advances by the U.S.S.R. shows that the detection of the August 1953 event would have resulted in the initiation of a very large, high-priority American program to produce a bigger and better thermonuclear device. Such a program would undoubtedly have had broader support than the one actually mounted in the spring of 1950. Moreover, the general scientific and technological situation in which a hydrogen-bomb program would have been embedded in 1953 would have been significantly different from the actual one in 1950. For one thing, the kind of theoretical work in progress on the Super before President Truman's decision would have continued and would have provided a solider base from which to launch a crash program. In addition the booster program would presumably have continued along the path already set for it in 1948 (which included a test of the principle in 1951), and therefore in 1953 there would have been available some real experimental information concerning thermonuclear reactions on a smaller scale.

Last but not least, there had been great progress in computer technology between 1950 and 1953. When the real Mike test was being planned, fast electronic computers such as MANIAC and the first UNIVAC either were not quite operating or were in the early stages of their operating career. By a year or so later they were in full running order and much experience had been gained in their utilization, so that they would have been much more effective in connection with any hypothetical post–Joe 4 American crash program. For all these reasons it is plausible to assume that the U.S. would have arrived at something like the Teller-Ulam design for a multimegaton superbomb either in the same length of time or, even more likely, in a somewhat shorter period, say sometime between September, 1955, and April, 1956.

These dates bracket the actual date when the Russians arrived at roughly the same point in the actual world. A few months' difference either way at that stage of the program, however, would not have been meaningful. It takes quite a long time, typically several years, to go from the proof of a prototype to the deployment of a significantly large number of weapons based on it. Differences in production capacity would have played a much more important role than any small advantage in the date of the first experiment, and such differences as then existed surely favored the U.S. Hence even in the worst plausible alternative world the nuclear balance would not have been upset. Moreover, in the most probable alternative world the date the Russians would have arrived at that stage would have been delayed until well after the first large U.S. Mike-like explosion had showed them there was a better way; thus in this most probable case the U.S. would still have enjoyed a substantial lead.

In short, the common notion that has persisted since late 1949 that some sort of disaster would have resulted from following the Oppenheimer-Lilienthal advice is in retrospect almost surely wrong. Moreover, even if by some unlikely quirk of fate the Russians had achieved the Superbomb first, the large stock of fission bombs in the U.S. arsenal, together with the 500-kiloton all-fission bomb for those few cases where it would have been appropriate, would have adequately ensured the national security of the U.S.

This history and the conjectures about possible alternative pasts show that Op-

J. ROBERT OPPENHEIMER AND EDWARD TELLER met at a Washington reception in 1963. Behind the two men is Glenn T. Seaborg, who was then chairman of the AEC. At the left is Oppenheimer's wife. Oppenheimer had just received the Fermi Award of the AEC. Ten years earlier, in the aftermath of the secret debate over whether or not the U.S. should proceed with the development of the hydrogen bomb, he had been banned from all Government work by virtue of the fact that his security clearance had been removed. Teller had been a leading advocate of the development of the hydrogen bomb from the early 1940's. The General Advisory Committee of the AEC, of which Oppenheimer was chairman, had recommended in 1949 that the U.S. not initiate an "all-out" effort to develop the Super.

penheimer, Conant, Fermi, Rabi and the others were right in their advice about the Super, and that they were right for the right reasons. They had correctly assessed the relative technological state of affairs, correctly judged the margin of safety inherent in the situation and correctly projected the ability of the U.S. to catch up rapidly if that should become necessary. The national security of the U.S. did not require the initiation of a high-priority program to develop the Super. It was therefore entirely appropriate to attempt to use the first Russian atomic explosion as a lever for reopening the entire question of nuclear-arms control.

The authors of the report could not, of course, predict the details of the alternative chronologies outlined above, and they did not try to do so, but they could and did correctly assess the general situation and the limits of the probable futures inherent in it. The large rate of production of fissionable material already in effect, the planned expansion in that rate, the resulting immense stock of fission weapons forecast for the early and middle 1950's and the existence of an entirely adequate means for delivering those weapons guaranteed that even the sudden surprise introduction of a few superbombs by the U.S.S.R. could not really upset the balance of power. The situation was reinforced by the projection, which proved to be correct in the King shot, that if need be the power of the World War II fission bombs could be multiplied up to the megaton range simply by more astutely employing the techniques and materials already known and available.

In the course of presenting its general admonition not to proceed with the crash development of the Super, the Oppenheimer committee made certain specific predictions about it. An examination of these predictions shows that they stood the test of time fairly well.

In their discussion of the superbomb the committee members said that "an imaginative and concerted attack on the problem has a better than even chance of producing the weapon within five years." Four years and four months later Bravo, the first practical American thermonuclear weapon, was tested at Bikini. Given the unknowns and uncertainties existing at the time, that is a remarkably accurate prediction. They went on to say that "once the problem of initiation has been solved, there is no upper limit... except that imposed by requirements of delivery." That also seems to be the case. The largest bomb exploded so far (by the Russians in 1961) is said to have been some 58 megatons, four times the size of Bravo, and there is every reason to believe bombs could indeed be made even larger than that.

The report also said that there "appears to be no experimental approach short of an actual test which will add to our conviction that a given model will or will not work" and that "many tests may be required before a workable model has been evolved." History has borne out the first part of the prediction. A quarter of a century had to pass and other inventions had to be made before thermonuclear explosions were produced on a laboratory scale by means of lasers, and even those are probably not closely relevant to the superbomb problem. The second part of the prediction turned out to be less precise. The number of U.S. tests needed to develop and check out a bomb was three: George, Mike and Bravo. The Russians needed only two tests, but they had an invaluable piece of information that was not available to the American workers: the sure knowledge that both small and large thermonuclear explosions were really possible. These numbers were very probably smaller than the "many" the Oppenheimer committee had in mind, but even so they were in each case sufficient to provide the other side with an adequate early warning that thermonuclear work was in progress.

Another interesting and perceptive technological prediction is contained in the report's statement about "very grave contamination problems which can easily be made more acute... by surrounding the deuterium with uranium." The very high levels of radioactive fallout associated with large hydrogen bombs do in fact result from such use of uranium. The very first test of a practical superbomb, Bravo, produced a blanket of fallout that evidently contributed to the death of one innocent bystander (the radioman of the *Fortunate Dragon*, a Japanese fishing ship) and came within a hair's breadth of killing hundreds of Marshall Islanders living on two nearby atolls. The fallout accident in turn provided the initial spark behind the movement to ban nuclear-weapons tests that ultimately led to the Partial Test-Ban Treaty of 1963.

The foregoing account is, I think, enough to show that the Oppenheimer committee's advice was sound, but it may not be enough to show unequivocally that President Truman should have taken this sound advice. The President, unlike the AEC commissioners and their advisers, had to take into account a broader array of information and political ideas than those discussed in detail here. The overall intensity of the cold war was increasing, Mao Tse-tung and Joseph Stalin had proclaimed the Sino-Soviet bloc and many important Republicans were withdrawing or modifying their support of the bipartisan foreign and military policies that had been in effect since the beginning of World War II. As the fall of 1949 wore on and the arguments about the Super began to leak out from behind the curtain of secrecy, those opinions favoring the Super were, in the overall context of the time, both simpler and more widely persuasive than those opposing it. There can be little doubt that Congressional and public opinion was beginning to come down heavily on the side of a strong response to the first Russian atomic-bomb test, and building the Super seemed to many to be just the kind of thing to keep the Russians in their place. President Truman, a professional politician, could therefore have concluded that rejecting the Super and running even a small risk of being second best was politically too difficult an alternative. Moreover, his decision to proceed with the Super, made on January 31, 1950, was based on the advice of the special committee of the National Security Council charged with studying the matter. Those committee members responsible for international relations (Secretary of State Dean Acheson) and national defense (Secretary of Defense Louis A. Johnson) strongly supported going ahead; the only reservations were expressed by the one committee member who was not responsible for those elements of national-security policy, namely Lilienthal, chairman of the AEC.

Nonetheless, it now seems clear to me in retrospect that President Truman should have taken the advice of the Oppenheimer committee; he should have held back on initiating the development of the Super while making another serious try to achieve international control over all nuclear arms, particularly the Super. The benefits that could have flowed from forestalling the Super altogether were incalculable; the chances of succeeding in doing so were small, but so were the risks in trying. It was certainly one of the few opportunities, and as Lilienthal said then, it may have been the last good opportunity to base American foreign policy on something better than reliance on weapons of mass destruction or, as it is now phrased, on the prospect of "mutual assured destruction."

MATHEMATICAL GAMES

Concerning an effort to demonstrate extrasensory perception by machine

by Martin Gardner

In modern extrasensory-perception (ESP) experiments probability and statistics play indispensable roles in determining if ESP events have indeed occurred. Targets are set up, subjects make a large number of guesses and then the results are analyzed to see if there are significant deviations from chance. The results are usually recorded by hand, which has given rise to a persistent criticism. Because those who record ESP data are almost always firm believers in ESP, often with a large personal stake in a favorable outcome, the possibility of belief's biasing the results looms very large.

The biasing can, of course, be entirely unconscious. Over and over again it has been demonstrated that people with a strong belief make unwitting recording errors that tend to favor their belief. In tests of psychokinesis (PK), for example, when subjects try to influence the fall of dice, secret cameras have shown that handwritten records kept by "sheep" (believers) display significant errors favoring PK, whereas similar records kept by "goats" (skeptics) display an equal bias in the other direction.

With the rise of electronic and computer technology it naturally occurred to many workers in the field of ESP research that one simple way to guard against unconscious recording errors is to make the process as automatic as possible. Let the machine, incorporating an efficient randomizer, select the targets, and design the machine so that it makes a permanent, unalterable record of both targets and trial guesses. It is true that such machines are not fraudproof; witness the scandal last year when Walter J. Levy, Jr., director of J. B. Rhine's Institute for Parapsychology, resigned after it was found that he had been tinkering with the apparatus to improve scores. Apart from cases of outright chicanery, however, an electronic apparatus is an excellent way to eliminate unconscious bias.

Several crude devices for testing ESP were used on rare occasions from the late 1930's on, but the first major tests with an electronic machine were made in 1962. They were done with a system called VERITAC, which had been designed and built by a worker at the Air Force Cambridge Research Laboratories. The system randomly selects digits from 0 through 9. It prints a record of the selected digit, the subject's guess as to what the digit is, the time of each trial and the time interval between selection of the target and the guess. Counters on the control console provide instant feedback of results, but the counters can be disconnected if it is wished. After a trial run VERITAC goes into a locked condition and remains locked until a teletypewriter prints out the data.

The machine can be set for one of three modes. In the clairvoyant mode the subject guesses the digit after it has been selected. In the precognitive mode the guess precedes the selection. And in the general extrasensory perception (GESP) mode the target is observed by someone who acts as a telepathic sender to a subject in another room. Hence a hit can be the result of telepathy, clairvoyance or both.

In the 1962 experiment each of 37 subjects completed five runs of 100 trials each for each of the three modes, making a total of 55,500 trials. When results were analyzed, using the familiar chi-square test for statistical significance, there was no deviation from chance either for the entire group or for any individual. Nor were there significant differences in the scores of sheep and goats.

C. E. M. Hansel, discussing this historic experiment in his book *ESP: A Scientific Evaluation* (Scribner's, 1966), pointed out that VERITAC's instant feedback of results to the subject made it an ideal teaching machine. "With the VERITAC machine, subjects could be given long practice sessions so that any ESP ability that might be present could be strengthened. Thus parapsychologists would have both a testing and a training machine. It could also be modified to provide a reward after each hit and punishment, such as a mild electric shock, after each miss. It would then constitute a conditioning machine...."

"If 12 months' research on VERITAC can establish the existence of ESP," Hansel wrote on the final page of his book, "the past research will not have been in vain. If ESP is not established, much further effort could be spared and the energies of many young scientists could be directed to more worthwhile research."

Most parapsychologists did not look favorably on that kind of ESP testing. One exception was Russell Targ, then a physicist with Sylvania Electric Products specializing in laser and plasma research. In 1966, the year Hansel's book appeared, a short note in *Electronics* (December 26, page 36) reported that Targ was working on an ESP teaching box designed by David B. Hurt, an engineer at Fairchild Camera and Instrument. The subject tries to guess which of four buttons will light up, the notice said, and the box "reinforces by punishment as well as by reward."

Five years later, with a grant from the Parapsychology Foundation (founded by the well-known spiritualistic medium Eileen J. Garrett), Targ and Hurt designed and built a more advanced ESP teaching device. In 1972 Targ was hired by the Electronics and Bioengineering Laboratory of the Stanford Research Institute (SRI). Since then he and his associate Harold E. Puthoff, a physicist and Scientologist who had joined the SRI staff a year earlier, have been engaged in parapsychological research. The two men have become best known for their testing of Uri Geller, the Israeli magician who professes to have paranormal powers. Here, however, we shall be concerned only with their ESP-teaching-machine experiment. It marks the second milestone in the attempt with an electronic apparatus to establish the existence of ESP abilities in man.

The research was made possible by a grant of $80,000 from the National Aeronautics and Space Administration, with the Jet Propulsion Laboratory of the California Institute of Technology serving as the administrator. The final 61-page report was published by SRI in August of last year with the title "Development of Techniques to Enhance Man/Machine Communication." The authors are Targ, Phyllis Cole and Puthoff. Since Targ was the senior investigator, I shall henceforth use his name only.

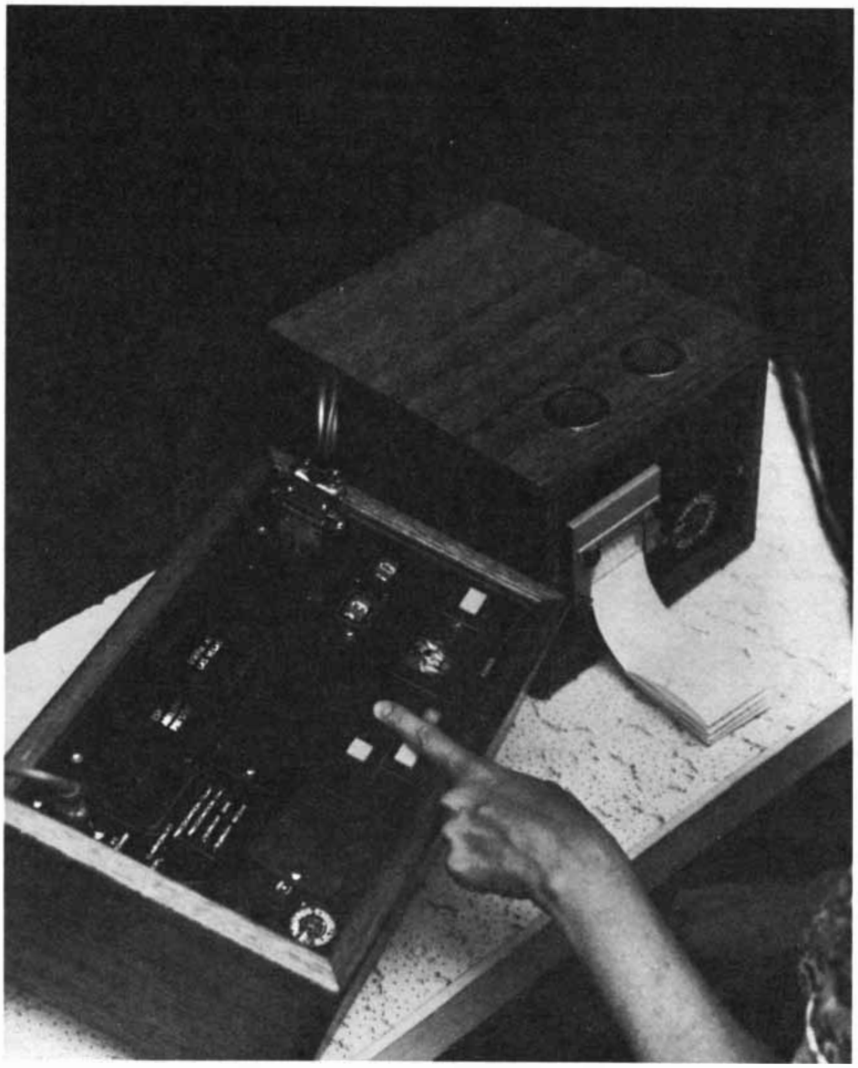
The report is not classified. Its cover states that it was prepared for distribution "in the interest of information exchange." Since the work was financed by public funds, anyone interested in it is entitled to ask SRI for a copy. The address is Menlo Park, Calif. 94025. I have been told that only 50 copies of the report were printed and that all have long since been distributed but that SRI has permission to reprint the report any time it wants to do so. It is to be hoped that this will be done, because it is an important report that every serious student of parapsychology should have access to.

Let us have a look at Targ's machine [*see illustration on this page*]. Models of it are manufactured by Aquarius Electronics, Box 96, Albion, Calif. 95410. (Similar and more compact models are now being made by other companies.) There are four square panels, each of which can display a colored transparency. Before any picture is displayed, however, a randomizer in the machine selects one of the four pictures as the target. The subject tries to guess the target, indicating his choice by pressing the square button nearest that panel. As soon as the subject indicates his choice, a light goes on behind the correct target picture to provide feedback and reinforcement. When there is a hit, a bell sounds. A counter to the right of the panels displays the number of the trial (from 1 to 25). A second counter displays the number of hits.

If a subject feels that he does not "know" the right button, he can press a "pass" button below the panels, and no guess will be recorded. Another button to the right of the pass button resets the counters to zero. Above the panels are five "encouragement lights" to provide additional reinforcement. The first legend, "Good beginning," lights up as soon as there are six hits, and it goes out if the hits rise to eight. The second legend, "ESP ability present," lights up at eight hits. "Useful at Las Vegas" appears at 10 hits. Twelve hits light up "Outstanding ESP ability," and 14 hits "Psychic, medium, oracle."

To the left of the panels is a rotary switch. Throughout the NASA project the machine was set for clairvoyance. The switch can also be set for precognition and telepathy. For telepathic testing it is necessary to plug a "telepathy adapter" into the model's output jack. This accessory box, connected to the teaching machine by a 25-foot cable, displays the targets to a telepathic sender in another room, who sees them before the subject makes his choice. A report on the use of an earlier model of the machine in its precognitive mode is given in a paper by Puthoff and Targ, published as Chapter 22 of astronaut Edgar D. Mitchell and John White's anthology *Psychic Exploration* (Putnam's, 1974). The two authors present their theory that events send out waves that propagate backward in time but decay rapidly. The closer the event to the precognition, the stronger the precognition; therefore the machine is designed to select its target with a quarter-second to one-second delay after a choice has been made. The authors believe that the "familiar *déjà vu* phenomenon is the most common form of precognition," not (as some parapsychologists have argued) a hazy recollection of an experience in a previous incarnation. They are also convinced that awaking just before an alarm clock rings is another familiar instance of precognition. Since that is a "large, timely and unpleasant event," its backward wave in time makes a strong impression on the sleeping mind.

Back to clairvoyance. The first phase of Targ's NASA project was to test two individuals under informal conditions. Subject A1, identified only as the son of an SRI scientist, worked at home with the machine, with his father recording the data on score sheets. Subject A2, identified only as a scientist not at SRI, worked in the laboratory but kept his own handwritten records. Subject A1 made 9,600 trials, obtaining a mean score of 26.06 hits per 100. The rising slope of his learning curve was .077. Subject A2 had a mean score of 30.50 over 1,400 trials and a learning slope of .714. These were encouraging results, but because of the lack of controls it was called



A subject works with the Aquarius ESP Teaching Machine

TRIAL	SCORE	MACHINE S CHOICE	SUBJECT'S CHOICE	
0 0	0 0	0	4	RESET
2 5	0 7	2	0	
2 4	0 7	1	2	
2 3	0 7	0	3	
2 2	0 7	3	0	
2 1	0 7	0	0	HIT
2 0	0 6	0	3	
1 9	0 6	0	1	
1 8	0 6	1	0	
1 7	0 6	2	2	HIT
1 6	0 5	0	3	
1 5	0 5	2	0	
1 4	0 5	2	0	
1 3	0 5	2	7	PASS
1 3	0 5	2	3	
1 2	0 5	3	3	HIT
1 1	0 4	3	0	
1 0	0 4	2	1	
0 9	0 4	2	2	HIT
0 8	0 3	1	3	
0 7	0 3	0	0	HIT
0 6	0 2	3	7	PASS
0 6	0 2	2	7	PASS
0 6	0 2	0	3	
0 5	0 2	2	2	HIT
0 4	0 1	0	3	
0 3	0 1	2	0	
0 2	0 1	1	1	HIT
0 1	0 0	2	1	

Paper-tape record of an ESP machine trial

Phase 0 and represented only a pilot study.

Phase 1 tightened the controls a bit by hooking a printer up to the machine. A typical printout is shown in the illustration at the left. The printer counts the number of trials from 1 through 25 (holding the count for a "pass"), records the machine's choice of target (0 through 3), records the guess and keeps a running total of hits.

Of the 145 volunteer subjects for Phase 1, 100 were "employees, relatives and friends" of SRI (79 adults and 21 children under 15). All did their work alone in an SRI laboratory, keeping their own records. Each worked on two or more machines in different locations. Twenty-two subjects of junior-high-school age or younger were at a private school where an experimenter was in attendance. The remaining 23 subjects were junior-high-school students at a public school where the tests were supervised by teachers.

The overall scores of the 145 subjects were at the chance level for both ESP and learning. A questionnaire given to the private-school students revealed that 15 of the 22 students actually tried to get low scores. "This tendency to experiment with different modes of interacting with the machine," Targ writes, "was not taken into account in recording or analyzing data." Nine of the 145 subjects had slight upward learning slopes, and 11 showed significant ESP. None showed a significant learning decline.

Targ was well aware that the Phase 1 controls were much too loose to justify the NASA investment in the study. Although he does not spell it out, it is easy to see how bias could have crept in. In the first place, the printer keeps no record of the total number of trials by any subject. There was no supervision of the 100 SRI employees, relatives and friends. One may assume that a high proportion of them were sheep. On the further assumption that no one consciously cheated, how could their unconscious bias operate?

The most obvious way is by decisions as to when a run of 25, recorded on paper tape, is to be preserved or discarded. Suppose there is a sudden disturbance: someone comes into the room, a fire engine goes by outside or a telephone rings. If the run is low in hits, there could be a strong feeling that the noise disturbed ESP and that the run should therefore not be counted. Or there might be an internal disturbance to justify tearing off and throwing away a run. The subject's foot falls asleep, his head starts to ache, disturbing thoughts cross his mind and so on. His finger could fumble and give him the impression that he had pressed the wrong button. Imagine yourself acting as an unsupervised subject. Now suppose any of the above disturbances, which could be a plausible basis for discarding a run, occurs. You note, however, that the run is high in hits. Would you then discard it?

Suppose you decide, but only in a vague way, to make a practice run. As you watch hits accumulate on the counter, would it not be easy to fool yourself into believing a practice run had not been intended after all? You keep the run. If the hits had been low, you would have discarded it.

All of this obviously also applies to the students. At the private school how carefully did the experimenter supervise the subjects? Did he keep watch at all times, or did he occasionally read a book or leave the room? And would the experimenter have strenuously objected if a student explained why he did not want to save a run?

At the public school how well did the teachers supervise the subjects? Targ tells us that many subjects "complained of the noise and confusion inherent in the location." And again: "Several dozen Phase 1 participants had complained that the clatter of the printer was a distraction." I am not guessing when I say that the paper-tape records of Phase 1 were turned in to Targ in disconnected bits and pieces.

Targ clearly perceives the weakness of his experimental design for this phase. It is the design of a physicist trained to investigate physical laws—laws that do not exhibit psychological quirks. An experimental psychologist would have constructed a printer that kept an unalterable record of all trials. Subjects would have been required to start at Trial 1, continue to a predetermined limit agreed to by both the experimenter and the subject and then turn in an unbroken tape. VERITAC was carefully designed to forestall bias by the simple expedient of keeping a time record of all trials. In Targ's defense it should be said that he regarded Phase 1 as being no more than a loose screening process designed to pick out high scorers in preparation for the crucial Phase 2, during which all psychological bias would be eliminated.

To eliminate it, a Model 33 Teletype was plugged into the system so that in addition to the paper printout a record of all trials was kept on punched tape. The punched tape was necessary not only for keeping an unalterable total record but also for ease in computer reading and analysis. The punched tape was fed to a computer on a trial-by-trial basis. The computer analyzed the data while the choices were being made.

Only the best subjects from Phase 0 and Phase 1 were used. There were 12 in all. This included subject A2 from Phase 0. (Subject A1, a student, had left the area to return to college.) Eleven subjects were chosen from Phase 1. Because of complaints about the noise of the printer during Phase 1, the printer was kept in another room with the teletype. Indeed, both the printer and the teletype were in the experimenter's own office, where they were inaccessible to the subjects.

The final outcome of Phase 2 must have disappointed Targ. No subject did better than chance on hits. No subject showed a significant learning curve. In short, the experiment was a failure.

One feature of Phase 2 is of unusual interest. Subject A13, who had "demonstrated some paranormal ability in other tests conducted at SRI," was offered the following rewards: $1 for 10 hits in 25, $2 for 11 hits, $5 for 12, $10 for 13 and $20 for 14.

Rhine has continually expressed his conviction that monetary motivation (among others) greatly increases ESP. "Subject motivation to score high," he wrote in 1964, "has long stood out as the mental variable that seems most closely related to the *amount* of psi effect shown in test results." To support his hypothesis Rhine invariably recalls that famous occasion in 1932 when Hubert Pearce, his star performer, made a run of 25 correct guesses of ESP cards—a miracle by anybody's statistical criteria. Rhine motivated Pearce by offering him $100 for each correct hit. The final sum of $2,500 was so large that Rhine had to tell Pearce he did not really mean it. That has always struck me as a dirty trick to play on Pearce, who was poor in those Depression days.

Since then Rhine has reported many other instances of perfect runs of 25, usually after some kind of motivation. The most notable was in 1936, when one of Rhine's assistants was testing Lillian, a nine-year-old who had been the highest scorer among a group of children. As Rhine told it in 1944:

"One day, after an unusually high score, she looked out the window, a happy little smile on her face.

" 'Don't say anything,' she said. 'I'm going to try something.'

"The alert experimenter cut the cards again, just to be safe."

Continuing the description from Rhine's 1964 account: "Before the run the child paused, laid the cards down, and with eyes closed, moved her lips as if talking to herself. She played off the cards without appearing to be focusing on the backs."

Apparently Rhine had learned the hazards of offers of cash. In this case Lillian had been earlier promised 50 cents if she made a perfect score. One presumes that the payoff was made. The following week in Rhine's laboratory Lillian's powers failed when she tried to guess ESP cards in sealed envelopes. On one of these tests, however, she made 24 misses. Rhine considers that a "nearly significant" instance of "psi missing," or ESP avoidance of the target.

There are other instances of subjects obtaining perfect scores in which strong motivation, Rhine believes, commonly played a major role. Not once does he entertain the possibility that strong motivation also motivates clever subjects to cheat. Magicians can give you 20 easy ways to obtain perfect runs with ESP cards. They range from seeing the large ESP symbols reflected in the experimenter's glasses (under certain conditions they can even be seen reflected by the corneas of people not wearing glasses) to almost imperceptible fingernail nicks along the edges of cards and even subtler methods that I would prefer not to disclose (but that can easily be reinvented by an ingenious child). It is noteworthy that not a single run of 25 has been obtained under controlled conditions, in spite of Rhine's persistent belief that in the uncontrolled cases the chances of success were genuinely $1/5^{25}$, or one in 298,023,223,876,953,125.

There is little doubt that Targ's money offer gave subject A13 a strong incentive. As Targ puts it, he "was highly motivated to generate trials." Out of more than 20,000 trials about 13,500 were made under the payment agreement. Alas, the offers had no effect. Subject A13's scores, faithfully recorded on the unalterable punched tape, remained at the chance level.

The results confirmed numerous tests by goats that have shown no correlation between motivation and ESP. Such failures are generally not reported to the public because they are not newsworthy. Richard C. Sprinthall and Barry S. Lubetkin gave the results of such a test in *The Journal of Psychology* (Vol 60, 1965, pages 313–318). Twenty-five volunteer students were asked to guess ESP cards for a run of 25, and 25 other volunteer students were given an identical test except that a firm offer was made of $100 to anyone who got 20 hits. The highest score among the 50 was 10. Overall scores were at the chance level. The mean score for the unmotivated group, 5.56, was slightly higher than the 5.40 for the motivated group.

Whenever a major experiment, such as the SRI test of Targ's ESP machine, is a conspicuous failure, parapsychologists themselves become strongly motivated to give reasons for the failure. If the test is supervised by a skeptical psychologist, or even if a goat is a mere observer, the favorite excuse is to invoke a kind of Catch 22: Skepticism destroys the subtle operation of psi. It is a catch unique to parapsychology. In other sciences failure by a doubting scientist to replicate an experiment is counted as disconfirming evidence. Because psi powers are said to be adversely influenced by doubt, however, parapsychologists are not impressed by replication failures unless they are obtained by sheep. In this case no goat was present, so that Targ turned to Catch 23.

Catch 23 asserts that psi powers are negatively influenced by complexity. As Rhine once phrased it, "...elaborate precautions take their toll. Experimenters who have worked long in this field have observed that the scoring rate is hampered as the experiment is made complicated, heavy, and slow-moving. Precautionary measures are usually distracting in themselves." Catch 23 achieves a truly remarkable result. It makes it impossible to establish psi powers by tests that are convincing to the goats who are the vast majority of professional psychologists. As long as testing is informal and under sloppy controls, you get results. If you tighten controls, the experiment inevitably gets complicated and scores fall.

Let us look at the way Targ puts it. "First, the subjects were definitely aware that they were in a test situation, despite attempts to provide a quiet, pleasant, nonthreatening atmosphere. All knew they were selected to participate because they performed well during the screening process, and this knowledge created varying degrees of tension....

"The subjects in this experiment have uniformly complained about the new experimental conditions in that 'It all feels different, being connected to a computer' despite the fact that the new working conditions were much quieter and more congenial than those in the pilot studies. We have spent considerable time interviewing the more articulate of our previous high-scoring subjects. From these conversations we have determined that they have lower levels of confidence when working with the printer connected to the teaching machine than they do when working with an experimenter watching them. And they have the least comfort when working with the teletypewriter punch operating. We have not done blind studies to confirm these perceptions, but they are certainly supported by the decline in scoring rate observed as we have progressed through the three recording techniques used in the program."

At no time does Targ consider the hypothesis that Phase 2 disconfirmed the existence of clairvoyant powers. "Based on arguments by E. P. Wigner...," Targ goes on, "we may hypothesize that increasing the complexity of the observation system for an event makes the event increasingly sensitive to 'observer' effects. Therefore, we may have a situation wherein the more complex configuration for observing a subject's performance causes greater perturbation of his perceptual channel."

That, of course, is the voice of a physicist familiar with observer effects in quantum mechanics. It is not the voice of a psychologist. In this case the "observer" is not even a person. It is a computer in another room!

What is Targ to do? With an important experiment showing no results, it would be understandable if he sought a way to "rehabilitate" (his word) some of the good scorers in the two pilot phases. What better way to do this than to drop the "complex" controls of Phase 2 and go back to the absence of controls in Phase 0.

Eight subjects were used for Phase 3. Seven made no printouts of any kind; they were merely observed by an experimenter. Their results were at the chance level. The eighth subject, A3 in Phase 1, asked to use the printer and work without an observer. He was allowed to make as many practice runs as he liked. Indeed, he made 4,500 practice guesses as against 2,500 "real" guesses. His score was at the chance level on the practice runs. His "real" runs showed slight ESP and a moderate upward learning slope.

This partly rehabilitated subject is the only subject identified in the report. He is Duane Elgin, a policy-research analyst at SRI. The report closes with an appendix in which Elgin states his firm belief in ESP and discusses his reaction to the failure of Phase 2. The thing that disturbed him most, he writes, was his constant confusion over whether he was in a clairvoyant state of mind or a precognitive one. When he made a guess, was he guessing the picture just selected or was his mind aiming at the picture that would be chosen next? Targ makes much of this confusion, which he hopes can be minimized in future experiments. One wonders why Elgin did not also worry about the possibility that his PK powers might be causing the randomizer to select the picture he intended to choose next. (Such a possibility had not been overlooked by Targ. In 1972 he had announced that this PK hypothesis "will be the subject of a future investigation.") Elgin ends by stating his belief that the tests were valuable exercises for his "psychic muscles." He feels he is much better now in "other situations where I might use ESP abilities, in particular, telepathy, precognition and clairvoyance."

The story of the failure of this expensive experiment is almost a paradigm of what has happened numerous other times in ESP research. High-scoring subjects are first identified by loosely controlled screening, then as their testing proceeds, under better (that is, more complex) controls, their psi powers mysteriously fade. In addition to Catch 22 and Catch 23, parapsychologists have a string of other good ones. Catch 24 says that, for reasons nobody understands, high-scoring subjects tend to lose their powers. Subject A3's ability did return. In spite of that, and regardless of Targ's desire to repeat the tests in some manner that would avoid the awful complexity of a computer's "observing" the trials from a distant location, NASA decided to provide no additional funds.

Now for the answers to last month's Bible problems. When I asked for four positive integers, all different, that could be arranged to make a complex fraction, $(a / b) / (c / d)$, that would equal $(d / c) / (b / a)$, it was intended as a joke. The two expressions are easily shown to be equivalent when any real numbers whatever are substituted for the four terms.

The answer to the first enigma is DAVID; to the second, Lot's wife. The rebus is "No A; H!" or Noah. Dmitri Borgmann informs me that there are many similar single-letter biblical rebuses: B = "Aha—B!" (Ahab), M = "Ha—M!" (Ham), T = "Lo—T!" (Lot), and so on.

The only solution to Alan Wayne's remarkable cryptarithm, SIX + SIX + SIX = NINE + NINE, is 942 + 942 + 942 = 1413 + 1413. Note that 1413 are the first four digits of π backward, and 942/3 is 314, the first three digits of π.

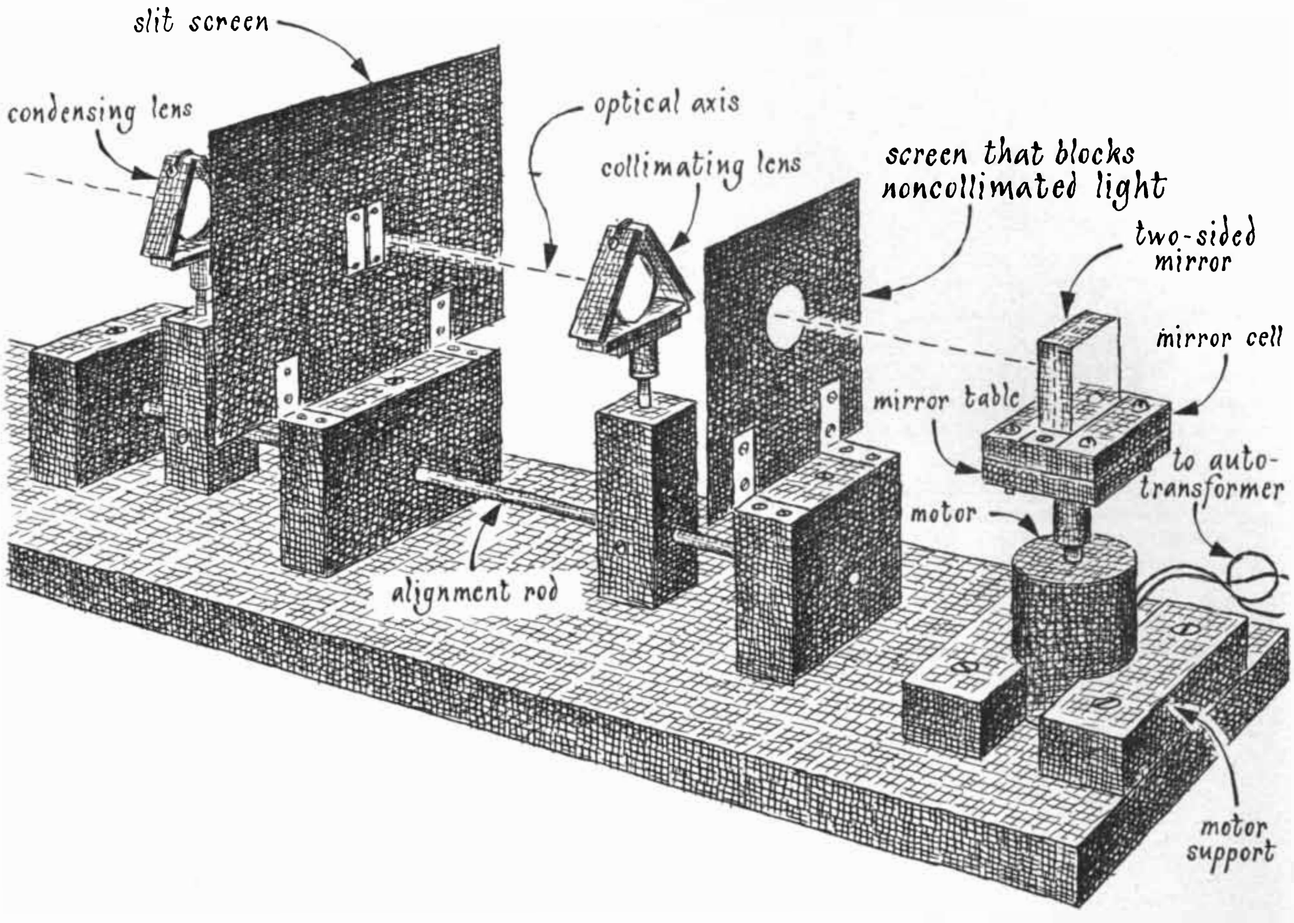
THE AMATEUR SCIENTIST

An amateur's version of A. A. Michelson's apparatus for measuring the speed of light

Conducted by C. L. Stong

A century and a quarter ago the French physicist Jean Bernard Léon Foucault made an approximate measurement of the speed of light. His determination, which amateurs can easily duplicate, was made with an apparatus that consisted of four basic parts. A mirror that rotated at a known speed reflected a beam of light from a bright slit to a fixed mirror placed a known distance from the rotating mirror. The fixed mirror reflected the beam back to the rotating mirror. During the round trip of the beam the rotating mirror turned through a small angle. Foucault calculated the angle by observing with an eyepiece the displacement of the reflected image of the slit. In order to determine the speed he divided the distance by the interval of time required for the mirror to rotate through the observed angle. The calculated speed turned out to be 298,000 kilometers per second, which is equivalent to 185,168 miles per second.

About 50 years ago a finely wrought version of the same instrument was designed by the American physicist A. A. Michelson. In 1926 it indicated a speed of 299,769 kilometers, which is close to the currently accepted value of 299,792.5 kilometers (186,282 miles) per second. Experiments of this kind have fascinated Sam Epstein (3929 South Orange Drive, Los Angeles, Calif. 90008). Last year Epstein, who is a chemist, built a homemade version of Michelson's



Optical bench of Sam Epstein's apparatus instrument, primarily to learn how closely he could approach Michelson's result. Epstein describes his experiment.

"I did not aspire to duplicate Michelson's experiment, which was performed over a distance of 22 miles, since I had available a working distance of only about 2,000 feet. The experiment can be done over still shorter distances, however, by folding the light beam with additional fixed mirrors to make a total path of some 2,000 feet. The result is achieved at the expense of slit-image intensity, which decreases as the number of reflections increases. Alternatively, even shorter working distances can be accommodated by rotating the mirror at speeds proportionately higher than the 7,200 revolutions per minute of my apparatus. The total length of the triangular path between the two reflecting surfaces of my rotating mirror (via the fixed mirrors) was 1.131 kilometers (3,711 feet). The distance from the slit image, which appears on a translucent screen, to the rotating mirror was 18.57 meters (60.93 feet). I refer to the path as the radius arm.

"This combination of working distance, mirror speed and radius arm resulted in an observed displacement of the slit image of more than 10 centimeters with respect to the position of the image when the rotating mirror was at rest. I measured the displacement of the image to within .01 centimeter. I was therefore able to determine the speed of light to within .1 percent of the accepted value.

"Light reflected by the rotating mirror falls on the distant fixed mirrors only a small percentage of the time. It arrives at the screen as a series of short flashes that constitute a small fraction of the light transmitted by the slit. Although the well-known effect called persistence of vision causes the observer to see a continuous image of the slit, the image is relatively dim. For this reason a high-intensity light must be used. My light source is a carbon arc between graphite electrodes; it is the kind found in professional motion-picture projectors.

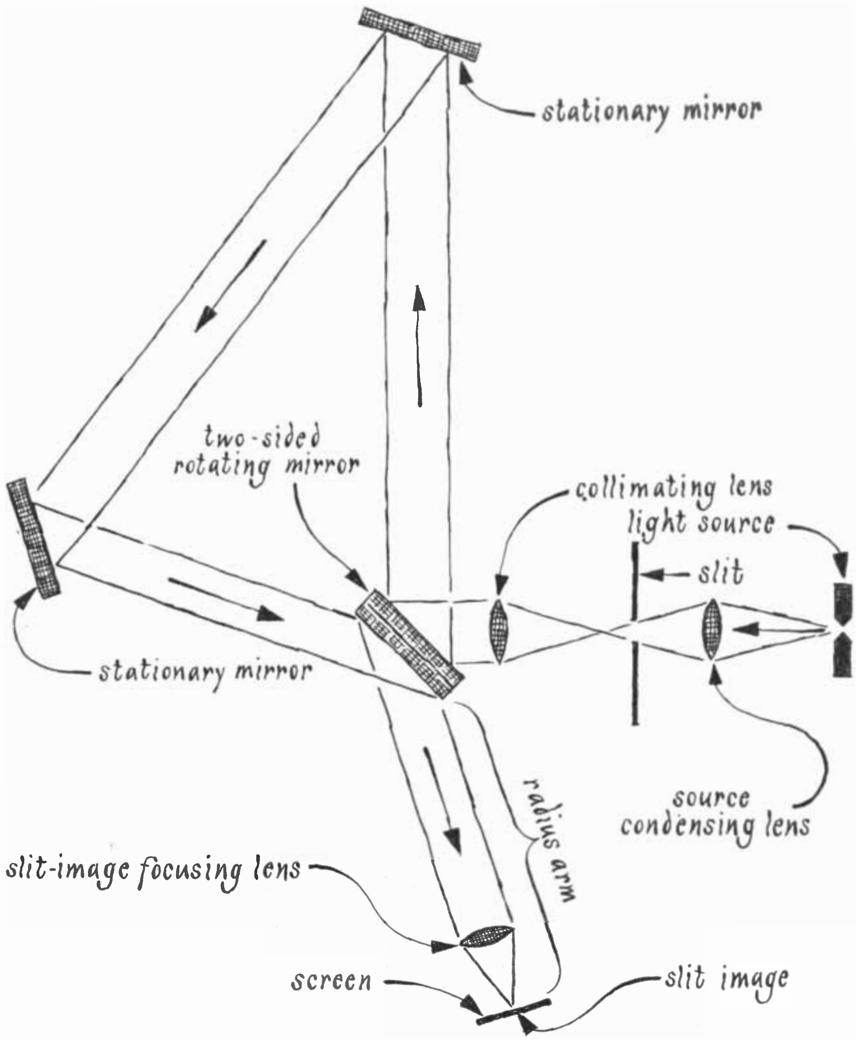
"A household appliance such as a toaster, an iron or a room heater of about 1,000 watts must be connected in series with the arc as a ballast resistor to limit the current to about 10 amperes. The light can be made even brighter by inserting a full-wave rectifier of silicon diodes in the circuit. Silicon diodes rated for 10 amperes at a reverse potential of 200 volts or more are currently available from dealers in surplus materials for less than 25 cents each. All connections in the circuit must be tight to avoid secondary arcing.

"The arc can be struck by pushing the carbons together momentarily and pulling them apart. An alternative is to place the electrodes almost in contact and then brush both tips simultaneously with a third electrode held in one hand. The hazard of receiving an electric shock can be avoided by standing on a dry, nonconducting surface such as a rubber mat, wearing dry leather gloves and adjusting one electrode at a time.

"The arc lamp can be constructed largely from Transite, an asbestos board that is available from dealers in lumber. To confine the light the working parts of the lamp should be enclosed in a Transite housing fitted with a ventilation chimney, although the outer ends of the graphite electrodes must be accessible so that they can be pushed inward from time to time as the arc consumes the graphite. The length of the arc can be monitored continuously by focusing a reflected image of the glowing tips of the electrodes on a small observing screen outside the housing [*see illustration on next page*]. A carbon arc should never be observed directly without eye protection such as a welder's eye shield. I found by experiment that the optimum space between the tips of the electrodes is about five millimeters.

"The slit and the condenser lens should be placed so that the images of the glowing tips of the electrodes fall beyond the ends of the slit. Light from the arc should pass through the slit but light from the glowing electrodes should not. The condenser lens should be far enough from the arc so that the glass does not heat up excessively.

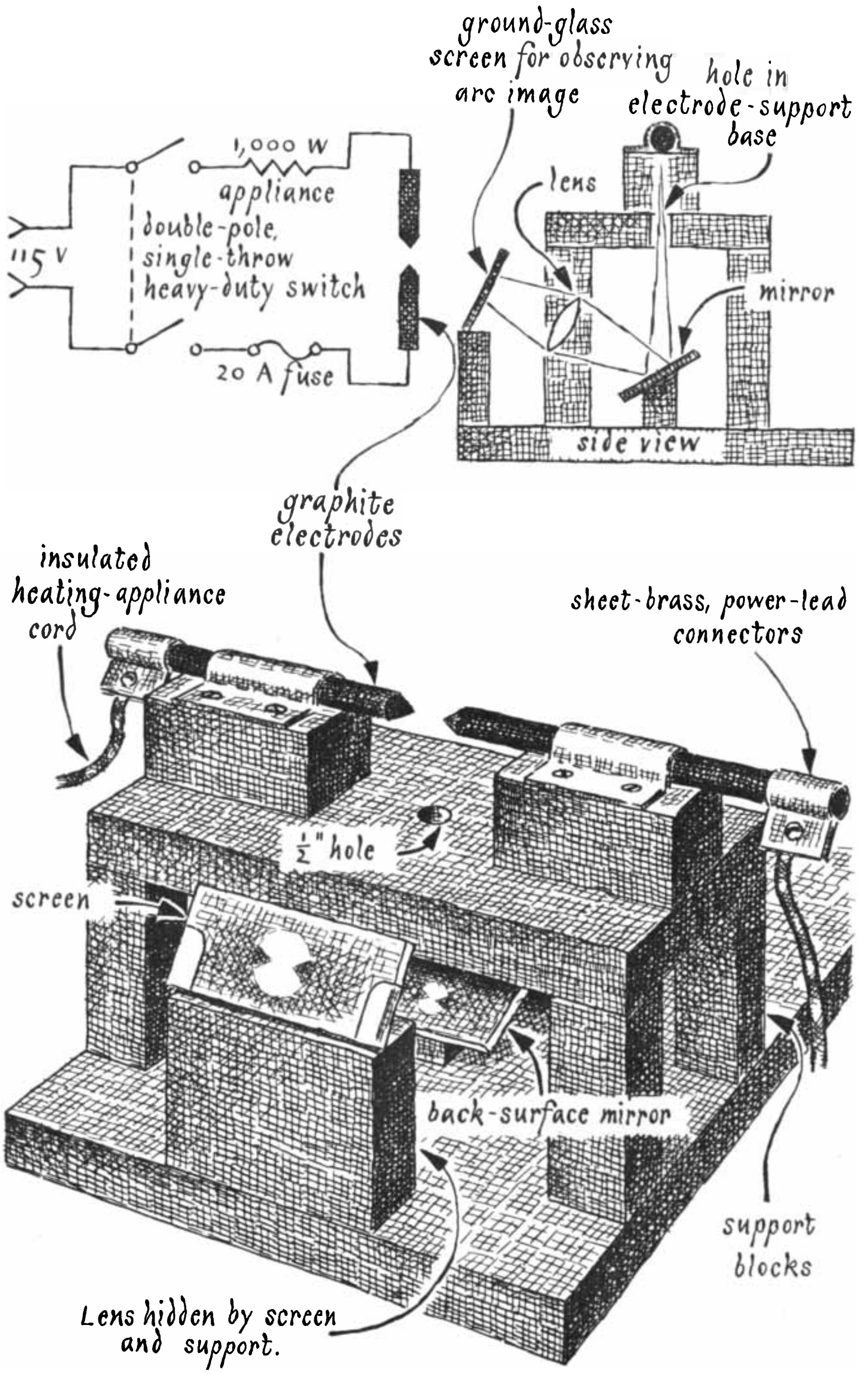
"The distance between the collimating lens and the slit must be adjusted to equal exactly the focal length of the lens. The diameter of this lens should be sufficient to intercept rays that diverge



Path followed by the light beam

from the full width of the slit. Excess light from the ends of the slit that would diverge beyond the collimating lens is blocked by a screen with an aperture equal in diameter to the width of the collimating lens [*see illustration on page 120*]. Ordinary two-inch magnifying glasses of about six-inch focal length serve as both the condensing lens and the collimating lens in my apparatus. All components on the optical bench must be solidly mounted and centered on the optical axis of the system.

"The rotating mirror consists of a pair of front-surface aluminized mirrors, each a quarter of an inch thick, cemented back to back with epoxy. They need be only slightly larger than is necessary to intercept the collimated beam. The mirror is clamped by a cell that is rigidly attached to a flange on the shaft of the motor. I improvised the cell with wood and strap metal. All edges of the mirror and the reflecting surfaces inside the cell were cushioned with foam rubber during assembly.



Details of the arc lamp

"The fixed mirrors are supported by cells improvised from wood. They can be rotated around both their horizontal and their vertical axes. Unless the first fixed mirror is mounted at exactly the same level as the rotating mirror, which is unlikely considering the distance between them, an adjustable auxiliary mirror must also be inserted in the optical train to deflect the light beam from the rotating mirror to its target.

"The base of the first of my two auxiliary mirrors also supports a vacuum phototube. The cathode of the tube receives two flashes of light during each revolution of the two-sided mirror. The resulting pulses of unidirectional electric current from the tube are used to measure the speed at which the mirror rotates.

"The rotating mirror is turned by the variable-speed motor of a sewing machine. Mine is a Dayton motor rated at one-fifth horsepower. It operates on 115 volts of alternating current at a maximum of 2.4 amperes at 10,000 r.p.m. The motor is designed to operate with a special speed controller. Both the motor and its companion controller are available from W. W. Grainger, Inc., an electrical-supply firm with retail outlets in all major U.S. cities.

"The controller alone is not capable of adjusting the speed of the motor to the accuracy required by the experiment. I set the controller to operate the motor at about 8,000 r.p.m., and then I energize the controller with a variable autotransformer, such as a Variac. The autotransformer is adjusted to reduce the speed to exactly 7,200 r.p.m.

"Various schemes can be contrived to measure the rate at which the rotating mirror turns. The phototube develops two pulses of direct current for each revolution of the mirror. The pulses can be amplified to flash a small neon lamp at the same rate. An inexpensive crystal-controlled oscillator could be installed to cause a neighboring neon lamp to flash at the rate of 7,200 flashes per second. The resulting stroboscopic effect would indicate synchronization between the two.

"Alternatively, the output of the phototube can be connected to the vertical plates of a cathode-ray oscilloscope while the horizontal plates are energized by a constant frequency of, say, 60 hertz. The display of two stationary pips on the face of the tube would indicate that the mirror is rotating at the rate of 7,200 r.p.m. I time the speed with the oscilloscope mainly because I happen to own one.

"The output of the phototube appears

as a pulse of voltage across the 27,000-ohm resistor [*see illustration on next page*]. It is fed to the vertical deflection plates of the oscilloscope through a double-pole, double-throw switch. The other side of the switch serves for applying the 60-hertz frequency of the power line to the plates, thus providing a means of checking the sweep frequency that is applied to the horizontal plates of the instrument. The 60-hertz frequency can be taken from any low-potential source in the circuit, such as the filament winding of the power transformer.

"If the slit-image screen is in the same plane as the slit and the rotating mirror, the rotating mirror will alternately reflect to the screen a direct image of the brilliantly lighted slit and its relatively dim image from the stationary mirrors. The dim image would be lost in the glare of the bright one. To avoid that effect I adjusted the second stationary mirror so that the image of the slit would be reflected from the reverse side of the rotating mirror to the second auxiliary mirror on the viewing platform at a small downward angle. The auxiliary mirror in turn reflected the light upward through an achromatic lens that focused the image of the slit on the translucent screen, where it could be viewed at a comfortable angle outside the plane of the direct image.

"The achromatic lens and a pointer made of sheet metal are mounted on a screw-operated carriage that can be displaced laterally about 15 centimeters with respect to the light beam [*see top illustration on page 125*]. The pointer slopes downward across the screen. In it there is a small aperture through which the image of the slit can be observed. A sharp bend below the aperture allows the pointer to traverse a small shelf or table that records the position of the observed image as indicated by the pointer.

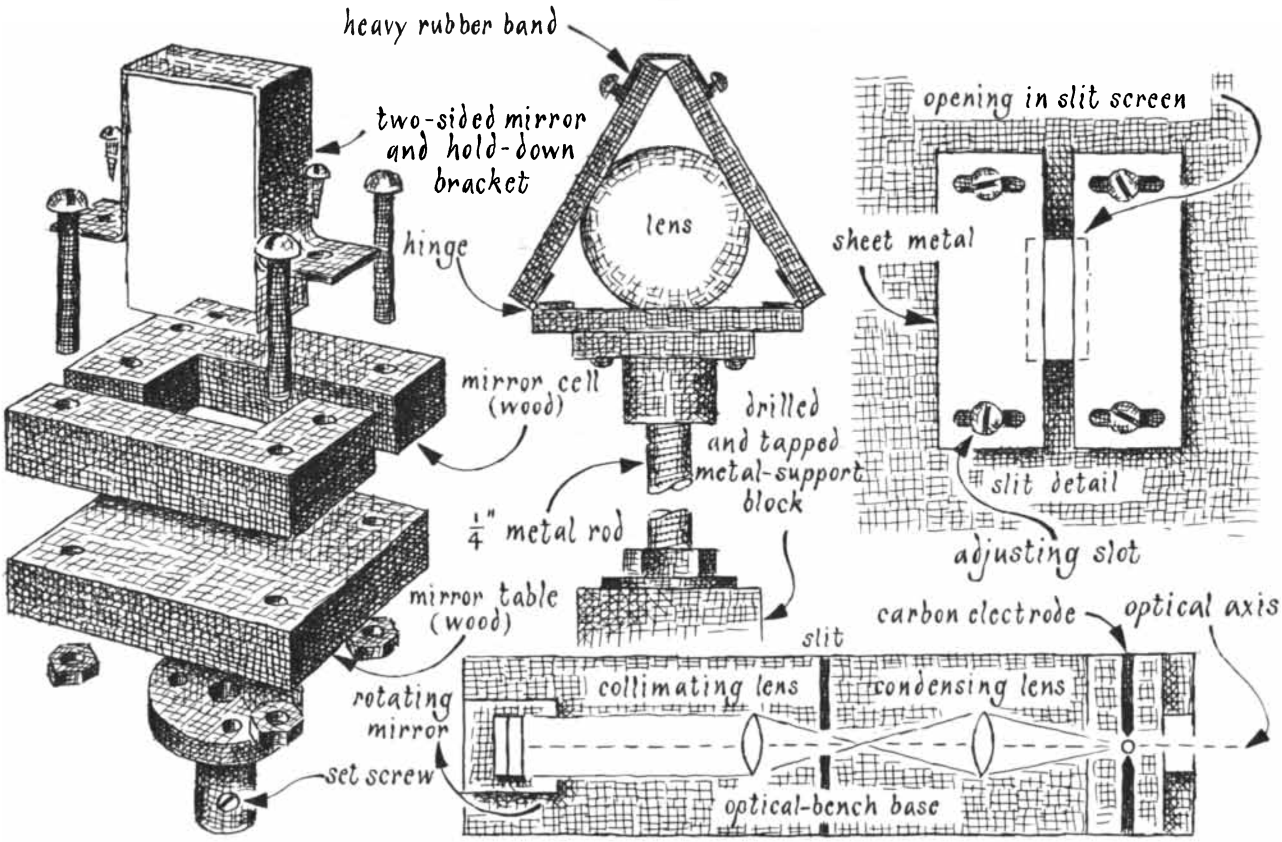
"The base of the assembly is slotted for lateral adjustment with respect to the beam. The lead screw that moves the carriage was made of stud-bolt stock, which is available from most dealers in hardware, as are the smooth rods that function as ways on which the carriage slides. The remainder of the construction is largely of wood and commonly available bolts.

"The assembled apparatus must be adjusted at night, and the experiments are of course done at night. At least two assistants are needed to adjust and operate the system. The team can communicate by prearranged flashlight signals or by citizen's-band radio telephones.

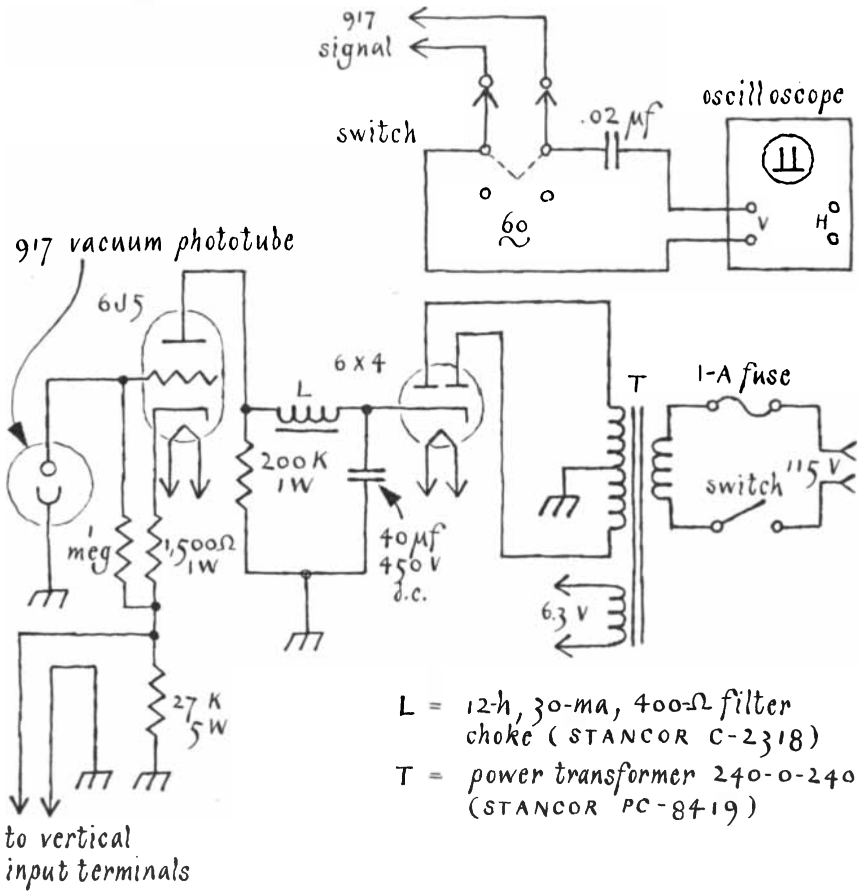
"The stationary mirrors must be installed on solid supports. The length of the path from the first surface of the rotating mirror through all stationary mirrors to the second surface of the rotating mirror must be measured as accurately as possible, preferably with a surveyor's transit. The technique of using the transit is explained in all elementary textbooks on surveying. Try to measure the path to within at least 30 centimeters. Put the observing screen at least 18 meters from the rotating mirror.

"Adjust the condenser lens to the position where the slit is filled with the image of the arc and the images of the tips of the glowing electrodes fall on the slit screen outside the jaws of the slit. For the initial trial the width of the slit can be set at about three millimeters. The collimating lens is then placed at a distance slightly greater than its focal length from the slit and in line with the rotating mirror.

"At the center of a six-inch square of white cardboard draw a circle equal in diameter to the diameter of the collimat-



Details of the optical bench



Circuitry for the oscilloscope

ing lens. With the arc lamp in operation and the rotating mirror blocked in any position at which it reflects light in a convenient direction, have an assistant intercept the collimated beam about a meter from the mirror with the screen of white cardboard. The diameter of the spot of light that falls on the cardboard must equal the diameter of the circle on the card. If it does not, move the collimating lens toward or away from the slit until the diameters match.

"Meanwhile a second assistant should monitor the arc lamp and adjust the electrodes to maintain the size of the arc. The first assistant then moves about 10 meters farther away from the mirror and checks the diameter of the beam again to see if the collimating lens needs further adjustment. The collimation adjustment is adequate when the diameter of the light beam remains unchanged through a distance of 20 meters.

"All my stationary mirrors were fitted with dust covers. To prevent dew from forming on the surfaces do not remove the covers in the evening until the temperature stabilizes. The experiment should be undertaken only during fair weather with little or no wind.

"The rotating mirror and the first auxiliary mirror are now adjusted to reflect the beam to the first stationary mirror, where an assistant sights on the beam and positions the mirror to intercept it. The rotating mirror should then be locked so that it cannot turn. (The slightest fraction of a degree of rotation will throw the beam off target.) The clamping screws of the auxiliary mirror are carefully tightened. Only the screws that control the horizontal translational adjustment of the first stationary mirror should be tightened at this point. The second stationary mirror is aligned with the first, and the vertical and horizontal rotational screws of the first mirror and the horizontal translational screws of the second are tightened.

"The second stationary mirror is now aligned with the reverse side of the rotating mirror. After the second auxiliary mirror on the image-viewing platform has been adjusted to reflect the beam through the focusing lens the slit should be visible on the viewing screen. The width of the slit is adjusted at the optical bench for the best quality of image, and a fine line is drawn on the recording table to indicate the zero position of the slit image. The rotating mirror is freed and the light is directed to the phototube housing, which is turned so that the cathode of the phototube intercepts the beam squarely.

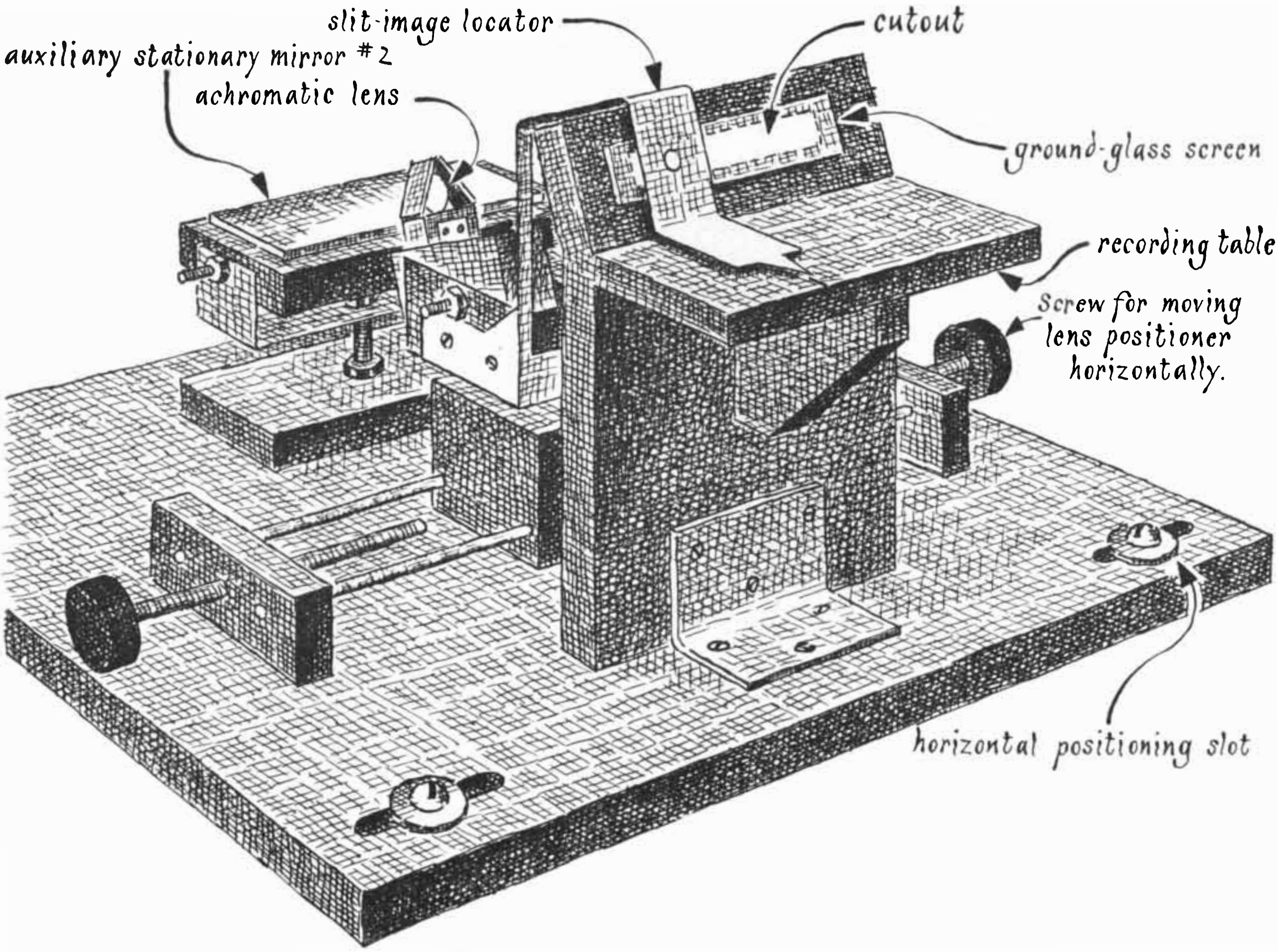
"A team of three people is also needed to operate the apparatus. One member observes the waveform on the oscilloscope and adjusts the autotransformer to maintain the speed of the rotating mirror at exactly 7,200 r.p.m. Another maintains the spacing between the electrodes of the arc to illuminate the slit uniformly. The third keeps the pointer aligned with the image of the slit and measures the excursion of the image from its zero position.

"After the apparatus has been turned on and has warmed up, the double-pole, double-throw switch is operated to display the 60-hertz wave of the power supply. The sweep rate of the oscilloscope is adjusted to form exactly one complete sine wave. The switch is thrown to its other position, and the autotransformer is adjusted to the point where the oscilloscope displays two needlelike spikes. If the amplitude of the spikes appears to exceed the height of the oscilloscope screen or the waveform is otherwise distorted, the width of the phototube window and the controls of the oscilloscope are adjusted experimentally until two distinct spikes are displayed. When they appear, stop the motor. Recheck the zero position of the slit image by adjusting the rotating mirror by hand.

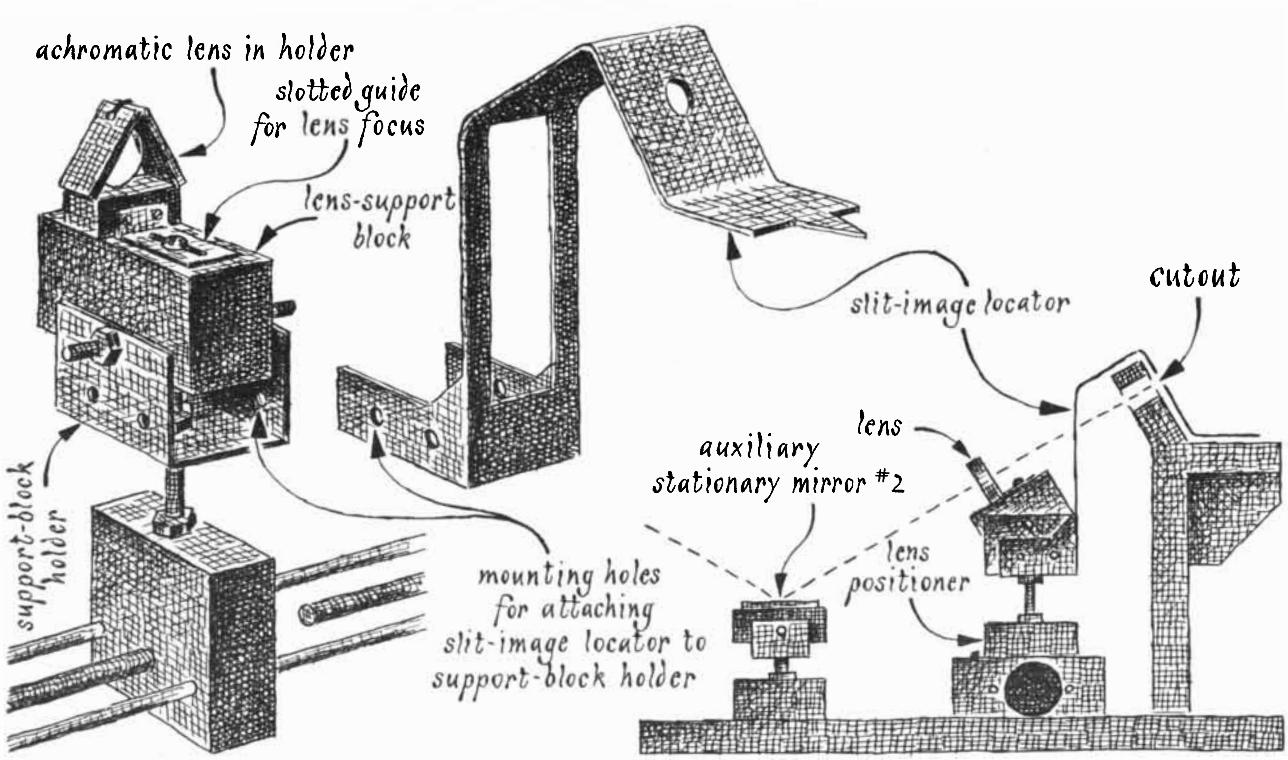
"The motor is started again and gradually brought up to speed (7,200 r.p.m.) by means of the autotransformer. The observer follows the displacement of the image of the slit by moving the pointer until the motor operator announces '7,200 r.p.m.' He then reports that he has recorded the position of the displaced image. The system can now be turned off.

"The observer measures the distance between the line on the recording table (zero) and the tip of the pointer at its displaced position to the nearest .01 centimeter, using a vernier caliper and a magnifying glass. Repeat the experiment at least 10 times. The average of 10 or more measurements of the displacement of the image is accepted as the final result.

"The velocity of light is now calculated as follows. The total distance traveled by light from the front of my rotating mirror to its reverse side was 1.131 kilometers. The distance between the rotating mirror and the image of the slit on the screen was 1,857 centimeters



Recording table of the apparatus



Features of the recording assembly

(60.93 feet). The displacement of the image on the screen between its zero position and its final position averaged (for 10 measurements) 10.55 centimeters. The average angle through which the rotating mirror turned from the position of zero image displacement to the average final position of image displacement is equal to arc tangent 10.55 / (2 × 1,857) = .1627 degree. The motor was turning the mirror at the rate of 7,200 / 60 = 120 r.p.m., or 120 × 360 = 43,200 angular degrees per second. Light traveled from the front surface to the reverse surface of the rotating mirror during the time required for the mirror to turn .1627 degree in .1627 / 43,200 or 3.766×10^{-6} second. According to these measurements, light would travel in one second 1.131 / (3.766×10^{-6}) = 300,318 kilometers per second (186,608 miles per second). As I have mentioned, the accepted value for the velocity of light in air is 299,792.5 kilometers per second.

"Obviously my measurements require refining. For example, with my apparatus an error of .01 centimeter in reading the displacement of the image of the slit leads to an error of about 200 kilometers per second in the calculated speed. The timing of the speed of the rotating mirror is equally critical. Although the 60-hertz frequency of public-utility power lines is carefully monitored so that electric clocks keep reasonably good time on the average, the rate at any specific instant may well be a few tenths of a hertz too high or too low. I recommend that the speed of the rotating mirror be checked against a frequency standard of known accuracy, such as the 500- or 600-hertz signals that are transmitted by radio stations WWV (Colorado) and WWVH (Hawaii) on the carrier frequencies of 2.5, 5, 10, 15 and 20 megahertz by the National Bureau of Standards. Other refinements that would improve the accuracy of my system would include a more accurate measurement of the optical path and indeed of all other dimensions.

"Investigators have been attempting to determine the value of c, the velocity of light, for more than 300 years. Its determination to within a few meters per second still eludes the best experiments. In fact, it has been suggested in recent years that nature does not impose an absolutely fixed speed limit on the universe. Does c change with the passage of time? Amateurs may not find it possible to build apparatus capable of checking that question, but it is both informative and fun to try."

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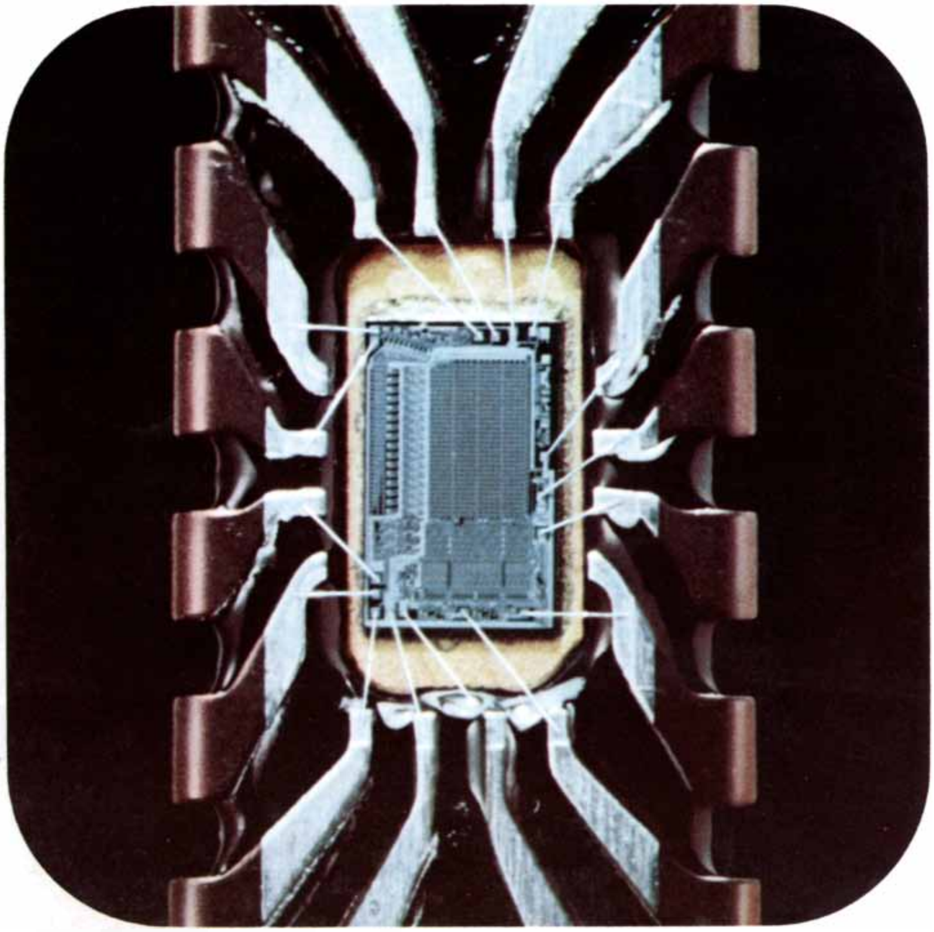

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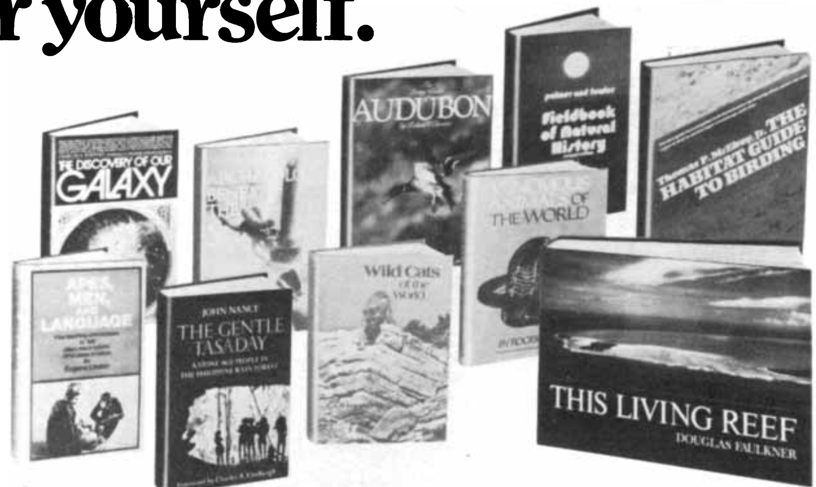

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BOOKS

A new synthesis of the principles that underlie all animal societies

by John Tyler Bonner

SOCIOBIOLOGY: THE NEW SYNTHESIS, by Edward O. Wilson. The Belknap Press of Harvard University Press ($20). This is a book of major importance. It is the product of a remarkable scholar who only four years ago produced an enormous and definitive book on the social insects. There is, however, a big difference between these two monuments of Edward Wilson's. *The Insect Societies* is the final and authoritative work on the subject of the social forms of insects. It totally supplants the classical studies of William Morton Wheeler and others, and it is unlikely that it will be superseded until Wilson himself writes a second edition. *The Insect Societies* caps a subject that has been one of active investigation for literally hundreds of years. Wilson's second monument, *Sociobiology*, is equally impressive in its simplicity and clarity, in its enormous intelligence, even in its size. Instead of capping the subject, however, it begins a new field that undoubtedly will hold a central position in biology, and perhaps in sociology too, for many years to come. It is quite an extraordinary beginning. I do not mean to suggest that Wilson has invented all the ideas that will form the basis of the work in the next 20 years, but rather that he has identified and brought together in one tome all those elements that will be the ingredients of the sociobiology of the future.

He himself recognizes the preliminary nature of these ideas. To the biologist he is saying, "You have masses of facts and a substantial body of integrating theory; do you not realize that this is a subject of prime significance in biology today?" To the social scientist he is saying, "This is what the biologist has to offer; surely this must be of consequence to you and to the future of your study of man." In spite of the fact that the subject of sociobiology is in its infancy, it is quite possible that these contentions are true.

In Wilson's view there is an important difference between sociology, as it is conceived today, and sociobiology. It is a difference that revolves around the fact that sociobiology, in contrast to sociology, is ruled by natural selection. He points out that sociology is still in a descriptive phase where correlation and "unaided intuition" play an important role. He compares this phase to the state of taxonomy before the enormous influence of population genetics and the modern synthesis of evolutionary theory. Sociobiology can, along with taxonomy, be understood in terms of neo-Darwinism. Indeed, it is this fact alone that gives sociobiology the guiding principle that will make it a significant and profound study. It is natural selection that shapes the groupings of animals, and from this vantage we can see how animal societies repeatedly evolved, why some societies differ and others are functionally very similar and how the differences or similarities bear no necessary relation to the animals' phylogenetic connections.

The key word is "functional," because in natural selection it is the function that is selected for. Animal societies are shaped by selection, so that certain functional or adaptive traits are achieved and maintained. This is quite a different proposition from the mere description of the integration of various social groups, and it is exactly such description that Wilson considers equivalent to pre-neo-Darwinian taxonomy.

To say that sociobiology is solidly based on the principle of natural selection is a sound beginning that will please, but not surprise, the modern biologist. The question is what to say next. How is one to build a theoretical scaffolding to encompass the entire large subject? In other words, how can one proceed from the anecdotal to the analytical?

Wilson makes a good beginning. It is true that before he is off and running he devotes much space to carefully defining a number of words so that he can use them unambiguously. I confess to a certain nervousness when I see a long string of definitions. I automatically suspect that the subject is opaque and that the author is going to make a valiant attempt to ignore the fact by establishing a set of consistent and logical propositions that are self-contained but give no insight into the scientific problem. Wilson does not, however, pour in philosophy when the science gets difficult; he sticks faithfully to the biological problems.

There are three major themes that are the foundation of Wilson's sociobiology: one is an analysis of those characters of organisms that make social groupings adaptive, the second is the genetic and population-biology basis of sociality and the third is the study of the ecological factors that shape particular kinds of social groups.

On the first of these themes Wilson calls those traits that make social groups adaptive the "prime movers" in social evolution, and he considers them in the category of natural history rather than theory. That is surely correct at this infant stage of sociobiology, but the adaptations he describes, or better, the selective forces that give rise to the adaptations, are the basic raw material for any future theory. Let me give some of his examples. He points out, for instance, that social grouping provides defense against predators. The instances of this among vertebrates are numerous. In gulls there is the famous (and at one time controversial) study of F. Fraser Darling, who showed that large sea-gull colonies were self-stimulating and that their egg production was better synchronized than it was in small colonies. The chicks hatched at the same time and therefore were vulnerable to predators only for a limited period, which improved their chance of survival. Other strategies that make social grouping adaptively advantageous are increased feeding efficiency, penetration of new adaptive zones, increased reproductive efficiency, increased survival at birth, increased population stability and greater ability to modify the environment.

The power of Wilson's work is in the richness of the examples he brings to bear on these points. To give one, he shows that honeybees have been able to modify their environment solely by virtue of their social groupings. Specifically, they can to a remarkable degree control the internal temperature of the hive. By fanning water droplets the bees can cool the hive through evaporation, and they can raise the temperature through the heat of muscle contraction. In this way they keep the hive between 34.5 and 35.5 degrees Celsius all through the foraging season from very cold nights to extremely hot days (up to 70 degrees C., or 158 degrees Fahrenheit). Such homeothermy is impossible for solitary bees.

Turning to the genetic basis of a social existence, it is remarkable how clearly Darwin understood the difficulties social insects presented to his theory of natural selection. All the worker insects are sterile and therefore are not directly responsible for reproduction, and reproductive success is the ultimate goal in Darwinian selection. The answer is that somehow the success of the queen in procreating must be enhanced by the presence of the workers so that they do contribute to the ultimate reproductive success of their species. There is something in the social existence of these insects that ensures this reproductive success of the colony; the cost of all the neuter workers must be more than paid by the success of reproduction; otherwise they would never have arisen by selection. As Darwin understood, the same restriction of the reproductive powers can be applied to multicellular organisms, where the majority of the cells are not capable of reproduction in the sense of giving rise to a new individual; that property is confined to the sex or germ cells. In both cases the cost of making these nonreproductive individuals or cells must be less than the strategic advantages of having the sterile tissue to improve reproductive success.

The revolution in our understanding of this process came in the early 1960's with an extraordinary paper by William D. Hamilton of the Imperial College of Science and Technology, who showed that because of a genetic peculiarity of the Hymenoptera (bees, wasps and ants) the sterile workers are more closely related to one another than brothers and sisters are among most other organisms, such as ourselves. We share, on the average, half of our genes with our siblings, and an ant worker shares three-quarters of its genes with its sisters. It is well known that advanced sociality arose independently many times among the insects for which this is true, but it arose only once (with the termites) among insects whose brothers and sisters share half of their genes.

Because of the multiple social successes of the Hymenoptera, Hamilton suggested that by being closely related they lower the cost of becoming social. To a greater extent they are saving their own genes by their industry and sacrifice. Ultimately it is the genes that are selected, and therefore altruism results in the preservation of those genes. In an individual multicellular organism the cells are usually genetically identical, so that the same argument can be made there. Indeed, it is an even stronger argument, since the cells of the body share all the genes of the sex cells. This phenomenon where altruism pays in terms of conserving genes is known as kin selection. It has become a pillar of our understanding of how sociality might have arisen by natural selection.

Related to kin selection is the whole matter of group selection. Does selection always act on individuals or can it act on entire groups in a population that is spread into discrete small pockets? In this case kinship need play no part, but certain altruistic traits could arise simply because those groups that included altruistic individuals might have an advantage (perhaps by chance) even though some individuals within the group would be at a disadvantage. Group selection and kin selection are such rapidly moving subjects that already Wilson's careful analysis is somewhat dated. There has been intense controversy over group selection, but as Robert M. May of Princeton University has pointed out in a persuasive essay (*Nature*, Vol. 254, page 485; 1975), there are in fact a variety of situations where group selection could operate.

Wilson goes beyond these major factors in the population genetics of social groups to provide all of what he believes to be the relevant principles of population biology. Indeed, he gives a brief course in the entire subject. The points he makes are too numerous to list, but he leaves no doubt that the population biology of animal societies has an important future.

Still another way of looking at the basis of sociobiology is to find the ecological correlates of sociality: What environmental conditions have promoted a social existence? This is the least developed of all the foundations of sociobiology, and perhaps for that reason it is one of the most interesting. As Wilson points out, in some instances there is a correlation with habitat. For example, those animals that have moved from the forest to the grasslands have in general increased their social integration. There are, however, many exceptions. In the case of the primates correlations with the environment are particularly tenuous, yet because of these animals' large brain size their social organization can be particularly complex.

This leads to the subject of animal behavior, which has been the main approach to the study of animal societies in the past. The subjects of communication, aggression, territories, parental care, social symbiosis and many others are considered in detail and with impressive care by Wilson. Indeed, he brings a special kind of balance to the discussion that is admirable. He makes it abundantly clear that even though these studies are fascinating and important, they do not address themselves (as the points discussed here do) to the question of why animal societies exist in the first place but rather to how they work. Wilson calls them the "social mechanisms."

One of the reasons behavior is a subject of such importance is that as one goes up the social scale from insects to primates it is clear that the brain itself is undergoing enormous change. This has meant a proliferation of signals and a corresponding increase in the subtlety of responses to the signals. In the course of evolution the genetically and ecologically determined social group takes on a life of its own with the higher vertebrates. And when one reaches man, the force of this idea becomes overwhelming.

All sensible biologists are apprehensive about human sociology. It seems to be a quagmire, a murky mixture of the obvious and the obscure. It is prolix and wrapped in a jargon that repels the hardiest reader. The more primitive the society, the more anthropological the sociology, the happier the biologist. Yet at the same time this does not represent any lack of interest in the subject. There is a certain satisfaction in seeing Wilson sneak up on sociology through the social animals. One expects to be shown that our social actions do, after all, have a biological basis, and to a considerable extent that expectation is justified. One cannot forget, however, how different from all other animals man really is, and how all these differences seem to be associated with the extraordinary tumorlike growth of the brain starting only a few million years ago.

The question I cannot answer is how this book will affect the sociologist. Will it give him a perspective that he will find valuable and useful? I should think that the answer must be yes. I doubt that it will have an immediate influence, simply because the subject of sociobiology is itself in a fetal stage. As it matures it will develop its own rigorous theory, and

then it will help to point the way to a deeper understanding of man and his social organization.

What Wilson has done is to make it possible to begin seriously the study of animal societies in a way that was not possible before. He thinks of his book as being a new synthesis, and clearly it is: by virtue of its putting together, with perception and clarity, with imagination and even with beautiful illustrations, an elegant compendium of all the significant things we now know about animal societies. Rarely has the world been offered such a splendid stepping-stone to the exciting future of a new science.

Shorter Reviews

by Philip Morrison

THE INTERNATIONAL BUREAU OF WEIGHTS AND MEASURES 1875–1975, edited by Chester H. Page and Paul Vigoureux. National Bureau of Standards Special Publication 420. U.S. Government Printing Office ($3). FRANCIS PLACE AND THE EARLY HISTORY OF THE GREENWICH OBSERVATORY, by Derek Howse. Science History Publications ($12.95). One way to distribute the load of our bicentennial recall of General Howe's search-and-destroy mission and its consequences is to seek other centenary events. Here are two engaging celebratory scientific books that bracket the epoch of the Minute Men and their leaders, the one commemorating 1875 and the other 1675.

A hundred years ago, in May of 1875, the envoys of 17 nations signed in Paris the Treaty of the Metre, binding their states to "establish and maintain, at their common expense, [an] international bureau of weights and measures, the location of which shall be at Paris." The first of these two books, well illustrated by an agreeable mixture of historical and scientific images, is a translation of the official centennial volume prepared by the Bureau International des Poids et Mesures that was set up by the treaty. The U.S. was a founding signatory and belongs still. (A few signatories, such as "His Majesty the Emperor of the Ottomans," withdrew later; Turkey came back in 1933; even the United Kingdom soon adhered.) Today some 43 states belong, none contributing less than half of 1 percent or more than 10 percent of the four million gold francs the BIPM budgets this year (up fortyfold from 1878). Plainly the nations did not adopt the metric system solely by treaty; they have separately made their way year after year, the U.S. being the last major holdout. The BIPM, in the lovely woods at Sèvres (the site of the Trianon de Saint-Cloud), was created to provide the preconditions for an international system, to develop the techniques and hold the prototypes that fix the metric units. The metric units were of course the achievement of revolutionary France, although the system became compulsory in France itself only in 1840. The BIPM authors express "a hope that the world will be 100 percent metric at the end of the 20th century."

Their book chronicles not the common use of the metric system but the work of the international laboratory of scientific metrology in the elegant Pavillon de Breteuil. That laboratory is first among equals, a center uniting the various national standards laboratories now spread from Tokyo and Bombay to Leningrad and Pretoria. Here is the international kilogram under its triple glass dome, a platinum-iridium cylinder 39 millimeters high, made by the London firm of Johnson, Matthey and Company around 1880 and defined as the unit of mass since 1901. (Among the three made then it was the closest to the mass of the platinum kilogram "of the Archives" deposited as the prototype on the "4 Messidor an 7" of the Revolution.) The prime standard is used sparingly, having been involved in weighings only some 60 times in a century. Its many stand-ins, in Paris and around the world, do the work (a few hundred weighings per year each) and take the wear. A mirror polish, special tweezers "furnished with velvet or chamois skin" and absolute avoidance of dragging a weight across its support keep the wear down, which means no serious loss of mass. Gain of mass is the result of pollution from the air, which brings dust, stain and tarnish. The costly alloy is a noble one, and a fine hair brush, solvent cleaning and a delicate jet of distilled-water steam fight the mass gain. Secondary prototypes show a fairly steady gain in mass from exposure to the laboratory air: about three micrograms per year, which cleaning removes. Great care cuts mass loss, and triple-sealed storage in a partial vacuum reduces the deposit of dirt. The standard is surely good to a few micrograms per kilogram, still 1,000 times better than the ingenious X-ray-interferometry measures of Avogadro's number, which someday may enable us to tie macroscopic mass directly to the atomic-mass unit.

Weighing with the equal-arm balance is capable of an accuracy better than 10 micrograms per kilogram, with the interchanging scheme invented by Carl

Friedrich Gauss to take care of the inescapable inequality of the balance arms. The balance delivered in 1878 by Rueprecht of Vienna remained the best available to the BIPM into the 1970's! Only in the past few years has it been challenged by a remotely operated one-pan counterweighted balance, developed first at our own National Bureau of Standards. This apparatus, used in an airtight dome, makes possible the comparison of kilograms to within one microgram. One key element is the clever design of stops to ensure that the knife-edges are always loaded by about the same amount even while weights are being substituted. The disturbance of the contacts between the knife-edges and their flat bearing surfaces is the main source of the inconstancy of balance arms..

Almost everyone knows the classical length unit: one meter between the scratches on a polished X beam of platinum-iridium. Those are some scratches too. Since 1937 they have been elegantly trimmed and dressed lines, very neat in photomicrographs, so that one can compare meter scales "with good lines" to about one part in 10 million. Thus far can we go with Newton and geometric optics. A. A. Michelson began the use of interferometry to specify the meter by an optical wavelength as early as the 1890's. By 1960 the official meter definition was given in terms of the orange line in the spectrum of krypton 86. The pure isotope, the absence of hyperfine structure in the spectral line, the use of a capillary glow tube and looking both with and against the ion drift to allow interpolation for Doppler broadening—all of this belonged to what one might call the classical period of atomic physics, without quantum mechanics. It still works, but the krypton line remains intrinsically widened by about one part in two million; these experts can split the spectral line perhaps fiftyfold, depending on its very symmetrical shape, but that is their limit. Various krypton lamps differ from one another by several parts in a billion. The helium-neon laser locked to iodine absorption lines (with the newer quantum physics fully employed) promises improvement by one or two powers of 10. Even now the speed of light is known to an accuracy mainly limited by the definition of the meter. It is a good bet that within a decade the BIPM will seek to redefine the meter in terms of the speed of light and the atomic frequency standard that fixes the second, using laser beats and lasers locked to saturated atomic absorption lines.

Auxiliary quantities, such as the acceleration of gravity and the atmospheric pressure, are measured with virtuosity nowadays. Simple basic systems are used—a corner reflector tossed in a vacuum, mercury-barometer tubes 40 millimeters across—but data are taken by tricky electronic interferometry. The electrical units are discussed rather briefly; it is interesting that the farad is now the best-known absolute unit. An electrostatic theorem discovered by two Australian workers in 1956 makes it possible to build real capacitors with a capacity calculable in terms of one interferometrically measured length and the known speed of light.

There is much more here. The authenticity and detail of this small, generally readable book commend it to teachers of physics at any level. The precise realization of units has an intrinsic fascination, and the working out of the details is an illuminating test of comprehension. More advanced techniques, such as the Josephson junction and the proton magnetic resonances, are described rather telegraphically, but a complete set of up-to-date references is given.

In August of 1675 John Flamsteed, already appointed "astronomical observator" to Charles II at a salary of 100 pounds per annum, laid the foundation stone of the Royal Observatory on its beautiful grassy hill in Greenwich Park. He cast its horoscope for its moment of birth ("derisively," we are told) and added the Latin phrase "May this keep you laughing, my friends." The second of these books describes the history of the founding mainly in terms of a dozen contemporary etchings by the well-known Francis Place, after original drawings by one Robert Thacker. The plates are a delight to those who admire London or who take some interest in either the art or the science of King Charles's glorious Newtonian days. They show a map, many views of Sir Christopher Wren's splendid architecture ("It was for the Observator's habitation and a little for pomp," he wrote) inside and out and in plan, and views of the original instruments, both overall and in detail. (These are already deservedly popular postcards from Greenwich; collectors will particularly value this bound set.) The etchings were commissioned by Flamsteed's patron, the Surveyor General of Ordnance, but were never published because of a complicated series of mischances. Only two sets of prints survive, both bound in incomplete copies of Flamsteed's final work of 1712.

This was relevant science, all right. It was Louise, the Duchess of Portsmouth, then Charles's favorite, who recommended to the king "a bold and indigent Frenchman" who purported to know how to find the longitude from easy celestial observations. He required certain positional input data, and the king appointed an expert committee—including Wren, Robert Hooke and later Flamsteed—to examine the matter. They thought little of the scheme, although "his Majesty [was] so daily imperturbed" that a final answer had to be given. Flamsteed showed the Frenchman's proposal to be impractical: "We know better methods, upon which he huffed a little and disappeared."

Once Charles learned that years of observation with telescopic sights to prepare star catalogues would have been a necessary condition for finding longitude by lunar position, he held that "the work must be carried out in a royal fashion... whereby navigation could be made safer." Then Wren suggested the site, went to the spot and drew up the plans. The excellent account does not omit a photograph of the present state of the observatory, now only a museum under London's no longer dark night sky. Wren's charming conception still reminds one of some giant baroque musical instrument against the sky, only slightly marred in its curving symmetry by some low added rooms and the famous time ball on the western tower.

Even three centuries ago the worthies of the Royal Society knew that both relevant and philosophical purposes had to be served. The Royal Observatory had dedicated an instrument of large size—built in a vertical shaft 100 feet deep—intended by Flamsteed solely to measure the parallax of the star Gamma Draconis. It passed almost at the zenith of London, so that any refractive error was small. Flamsteed fixed a long-focus objective at the top of the shaft and observed from a couch at the bottom through an eyepiece hanging on a plumb line. The device could not give the split-second accuracy needed for stellar parallax. The scheme never worked, and only two observations with it were recorded. Engineering difficulties were blamed, and "the damp of the place." It is clear, however, that although such an expensive measurement might place the sun and the earth in the scale of space, it could in no way improve navigation. Three hundred years of science have not relaxed the essential tension of those dual purposes.

ULTRASONIC COMMUNICATION BY ANIMALS, by Gillian Sales and David Pye. Halsted Press Division of John Wiley & Sons ($15.75). A cacophony of clicks, chirps and songs of every kind vibrates through sea and air, the two acoustic mediums in which animal forms are immersed. At the low-frequency end the acoustic band is limited mainly by the inefficiency with which small radiators generate long waves (a good bass speaker needs a big baffle); at the high-frequency end it is slowly cut off by the molecular absorptions of the medium. In dry air the half-distance of a 150-kilohertz (150,000-cycle-per-second) ultrasonic beam is only a meter, and water vapor greatly increases the loss. In air a one-kilohertz tone falls to half-strength in 60 meters. Seawater opens a long-distance channel: an audio tone is absorbed there only after a transoceanic path, and even up at 100 kilohertz the half-path reaches 100 meters. Directionality is more easily gained at shorter wavelengths, where a postcard can bounce an ultrasonic echo that it takes a cliff to match at the wave band of a human shout.

Our ears cover the sonic band from a couple of tens of cycles per second to some 20 kilohertz, but living forms from crickets to whales extend the upper limit to a couple of hundred kilohertz, and not all of them have to pay for that in lowered bass sensitivity. The ultrasonic world is quieter than the sonic, mainly because its sounds are more local. A dense fog is as impenetrably dark to ultrasound as it is to light—more so, since its droplets are absorbent and hence "black" to sound, rather than scattering ("white") as they are to light.

By 1960 we knew well that many bats operate an elaborate system of location by ultrasound. Capacitance microphones are still standard for picking up bat voices; cathode-ray oscilloscopes with cameras were the main tools for entry into this world, an entry pioneered before World War II by Donald R. Griffin. A variety of more modern radio techniques, briefly described in this admirable review volume by two London investigators, now give fieldworkers a great deal more facility in detecting and recording. By 1970 Kenneth D. Roeder and his school had found adaptations in nocturnal insects to counter the sonar of their cunning bat predators, urgent countermeasures in the Fifty Million Years' War.

Two major chapters of the book review what we now know of this protracted acoustic struggle. Bat sonar is an advanced technology. There are bats that emit swept-frequency pulses whose echoes provide excellent range information as well as direction. Other bats emit long constant-frequency pulses and seek

Doppler returns to search out moving targets; they can compensate for their own airspeed. Some bats obtain good directional data by being virtuoso ear-wigglers. One unusual genus of fruit bat, with large eyes and a proved "night vision that is probably not surpassed by any other animals," has a broad-band click sonar system as well, and so it can fly skillfully in the pitch-dark caves where it roosts.

Many night-flying moths and lacewings (the "other side") have special ultrasonic detectors, and they take sudden evasive action when the pulse emitted by the bat arrives. Other moths emit clicks of their own, which jam the bat's reception or warn it of inedibility, either truly or falsely; electrical engineers would call it an IFF (identification of friend or foe) system. The evidence is strong that all of these were wartime countermeasures, evolved independently in many unrelated insect forms, since they have been hastily rigged in unlikely places and styles. Pye cites his own ingenious explanatory ballad:

In days of old and insects bold
(Before bats were invented),
No sonar cries disturbed the skies—
Moths flew uninstrumented.

The Eocene brought mammals mean
And bats began to sing;
Their food they found by ultrasound
And chased it on the wing.

Now deafness was unsafe because
The loud high-pitched vibration
Came in advance and gave a chance
To beat echo-location.

Some found a place on wings of lace
To make an ear in haste;
Some thought it best upon the chest
And some below the waist. . . .

Most of this action is heard in the wave band between 30 and 100 kilohertz. Plenty of puzzles remain, such as the full acoustic function of the bizarre facial features of many bats, which are presented here in photographs along with many sounds of various animals, both the hunters and the hunted.

Katydids sing ultrasound songs made by rubbing a hard edge across a file. We can often hear the call as it is modulated into the sonic region. Most small rodents make ultrasound squeaks and calls, probably using whistlelike structures in the larynx. Some crickets know how to make tiny tuned horns and baffles out of burrows and leaves in order to augment their ultrasound signal's effective radiated power; the porpoises and some other cetaceans surely mount elaborate high-resolution ultrasound location systems, perhaps using them for communication as well. The authors summarize all these results and methods up to about 1973, but the biological meaning of most of it is not yet nearly as well understood as that ancient conflict in the sounding night air.

TRAVELS IN COMPUTERLAND; OR, INCOMPATIBILITIES AND INTERFACES, by Ben Ross Schneider, Jr. Addison-Wesley Publishing Company ($5.95). Droll, laconic, candid and genuinely understanding, this small personal narrative by a Middle Western professor of English bears in 18th-century manner the subtitle "A Full and True Account of the IMPLEMENTATION of the *LONDON STAGE* INFORMATION BANK." That true account meaningfully connects the Burlington Mall, the Dartmouth Outing Club, the Widener Library, the works of Norbert Wiener, irrevocable letters of credit, the Bakerloo Line, the OCRB typeface on IBM "bouncing balls," a radial arm saw and Mr. Handel's music for a Covent Garden performance of *Semele* in 1762. Out of a few years spent moving within the marvelously complex context sampled in the list above, the once naïve author has spun a story both amusing and full of gain for the general reader who wants to learn what computers and computer people do. The chronicle is not limited to the textbook's algorithmic purity or the artful commercial assertion but sums up a real task in a tangled world of human beings, their tools and their intricate relations. If anyone is laying a cornerstone in these hard times out along Route 128 or on the Peninsula, this book, once it has been carefully read by all the principals, should be sealed inside it. It is a user's vivid record of the digital industry of the 1970's (this side of Moscow). Freehand flowcharts and sketches, sample data and London railway maps—mainly the work of the versatile author—enliven it visually.

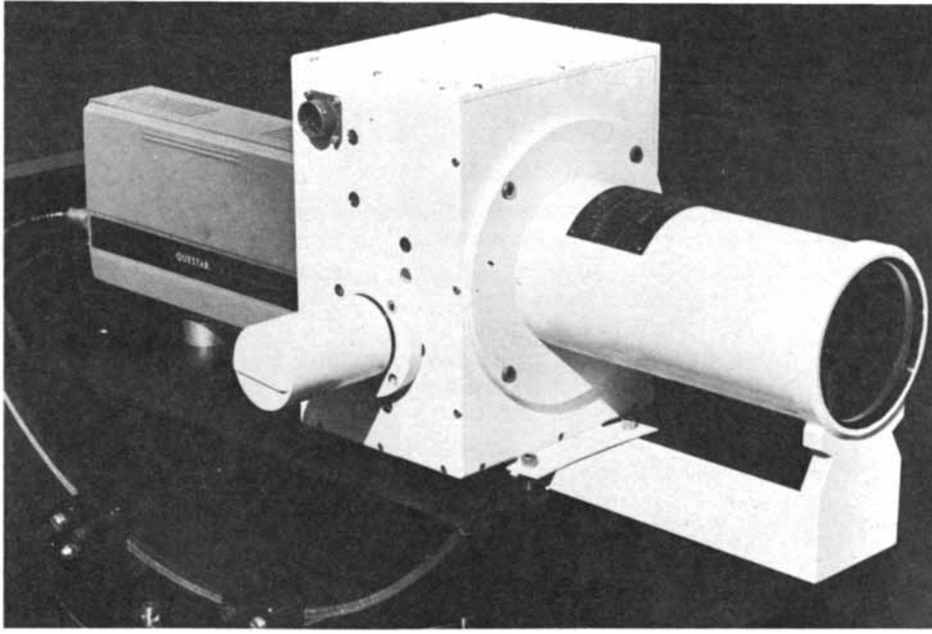
The task was set by a big book, 11 volumes long, called *The London Stage, 1660–1800.* Its 8,000 pages list, season by season and night by night, every play on the London boards, with casts, box-office receipts and comments, all compiled from the "playbills, newspapers, and theatrical diaries of the period" by five learned editors. They spent some 35 scholar-years at it, with a multivolume assist from a pioneer who published in the early 19th century.

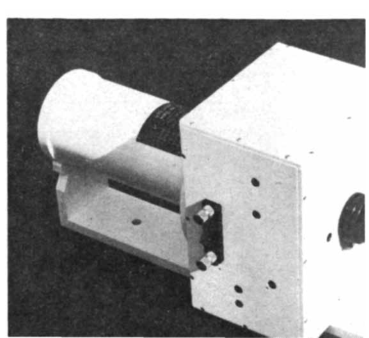
This giant store of information has no

index. It will not easily disclose even the names of the players or the play titles, let alone the role of Mrs. Mirthwit, the fat years for David Garrick or trends such as the rise of pantomime. The quest is now evident: Professor Schneider, who is a literary scholar (at Lawrence University) but with a difference, undertook to make *The London Stage* flexibly consultable by computer, yielding as a clearly desirable by-product a printed index of names and titles with a million entries.

Twenty-one million characters had to be transferred to disk memory, where they could be manipulated by programs that can exploit the "natural syntax" of two centuries of theater nights artfully enough to satisfy needs not yet expressed. An ingenious programmer was put to work; the entire 11-volume text was elaborately typed on the 21st floor of a Hong Kong building by a string of hardworking young Chinese women, and their output (except when tails appeared on characters because too thin paper had shifted a little under the impact of the bouncing ball) was optically scanned to make digital tapes, in part by a doggedly euphemistic firm in Kansas City and then by a devoted one in North London. A lengthy tape-editing by means of visual display is now under way; maybe by the end of next year an eager student of the London stage will be able to put almost any theatrical question to the "data base," which is held in a System 360 machine in Appleton, Wis.

How that was done and by whom—what friendships and misunderstandings, what a blizzard of alternatives, what boasts and promises and logical missteps, what little maneuvers and clever solutions were encountered on the way—is the delicious content of the book. No mere library worm, Professor Schneider himself ground small nicks into the plastic O's of the type balls to cure the scanner's confusion. No guileless consumer with other people's money to spend, he soon came to understand by direct experience that "a full-page ad in a magazine could well be 90% of a corporation's total substance." So writes a man who at the beginning shrank from telephoning Dallas for an estimate but at the end flew from London to Wisconsin for the weekend to clear up some trouble. The book holds a mirror up to computer society around "the terraqueous globe" and gives a humanist's shrewd appraisal of the image therein, both present and potential. The somewhat tired main title is inadequate for this witty book.





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LYNDA J. WELLS

HOME: Cambridge, Massachusetts

AGE: 34

PROFESSION: Neurobiologist

HOBBIES: Yoga, classical guitar, hiking, cooking.

MOST MEMORABLE BOOK: Jung's "Memories, Dreams, Reflections"

LAST ACCOMPLISHMENT: Awarded a post-doctoral traineeship in experimental neurobiology.

QUOTE: "One of the most fascinating subjects for the scientist is the study of the human brain and human behavior. Neurobiological research will lead to an understanding of the basis of behaviors such as emotional illness."

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