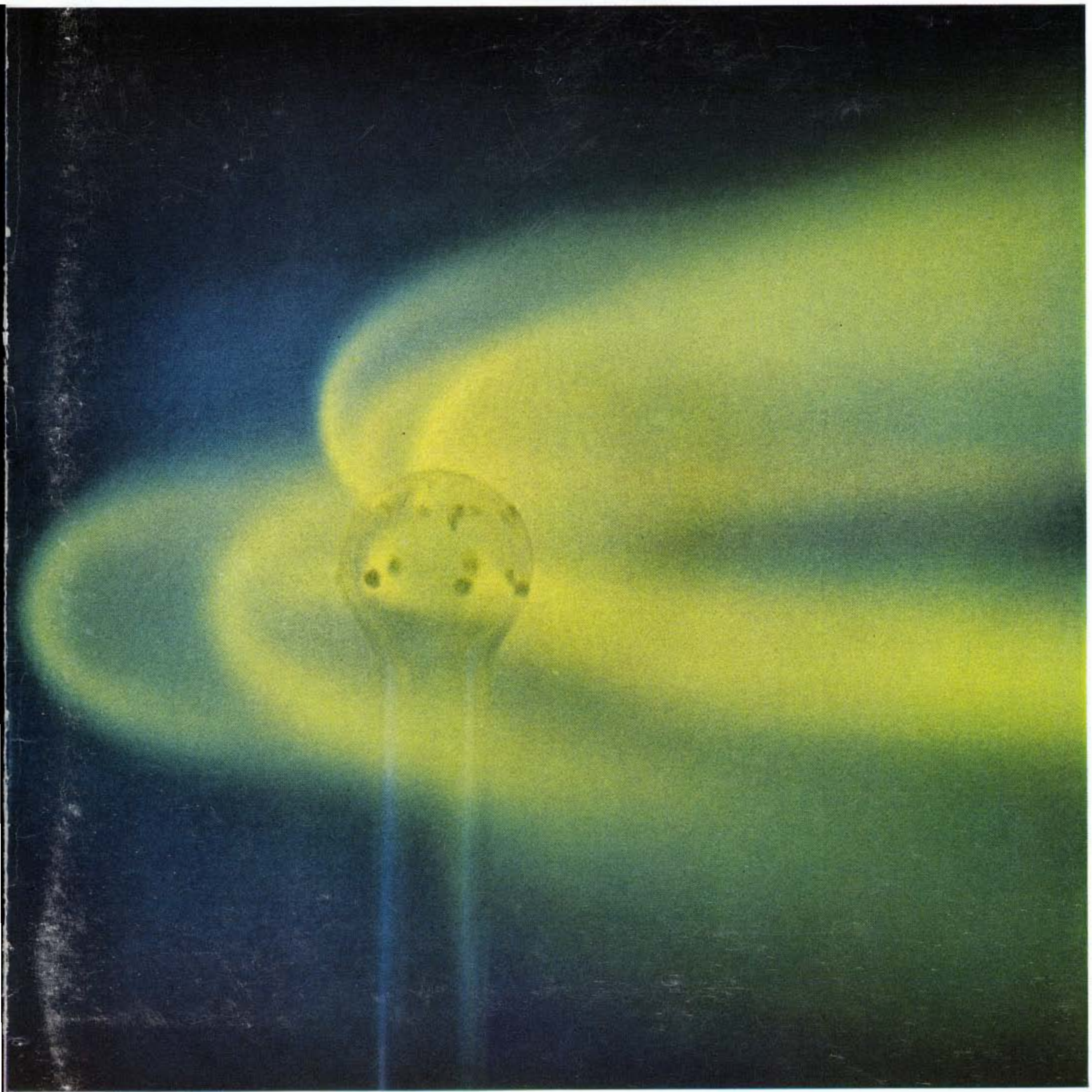
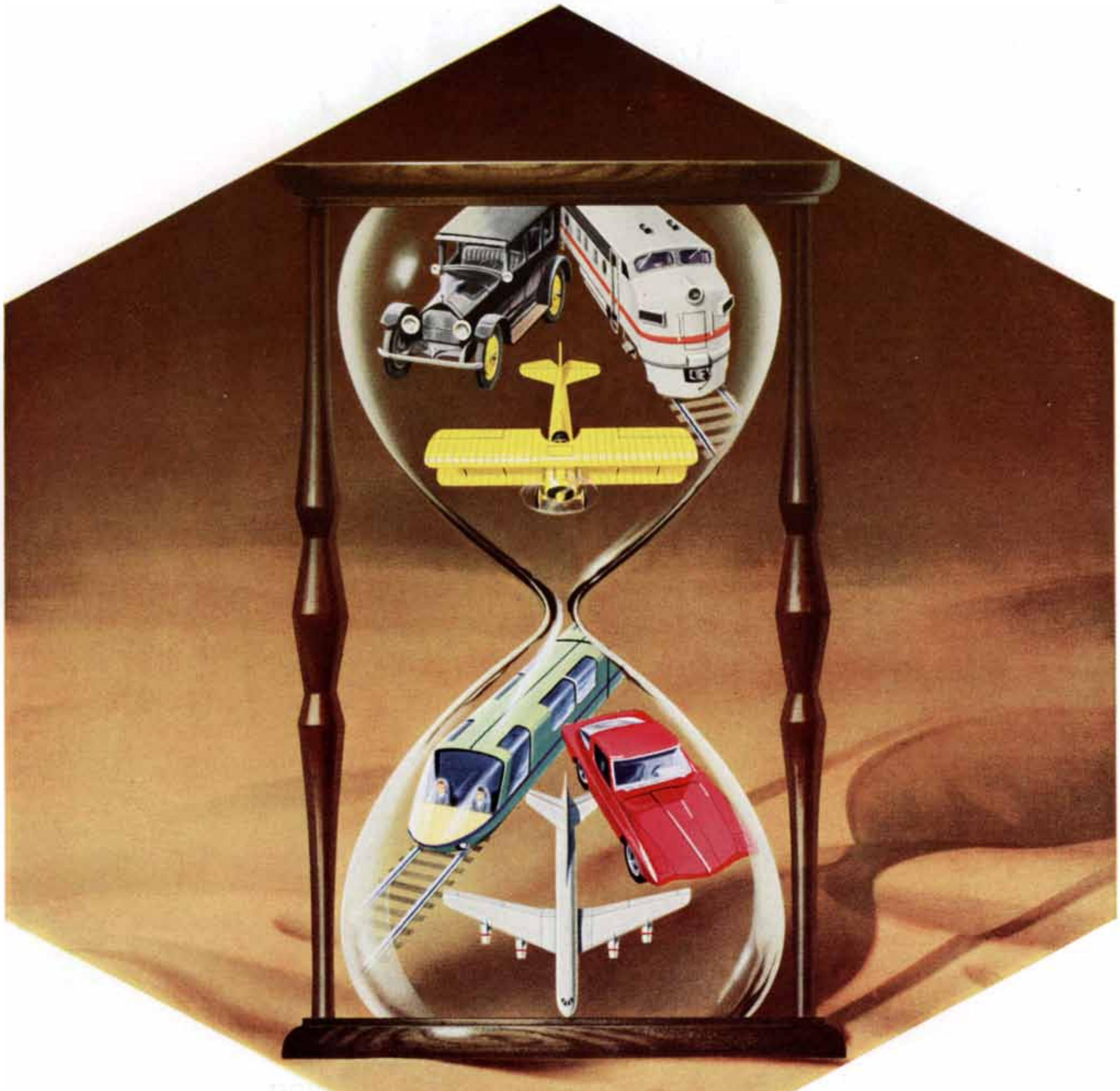
SCIENTIFIC AMERICAN



THE AIRGLOW

SIXTY CENTS

March 1966



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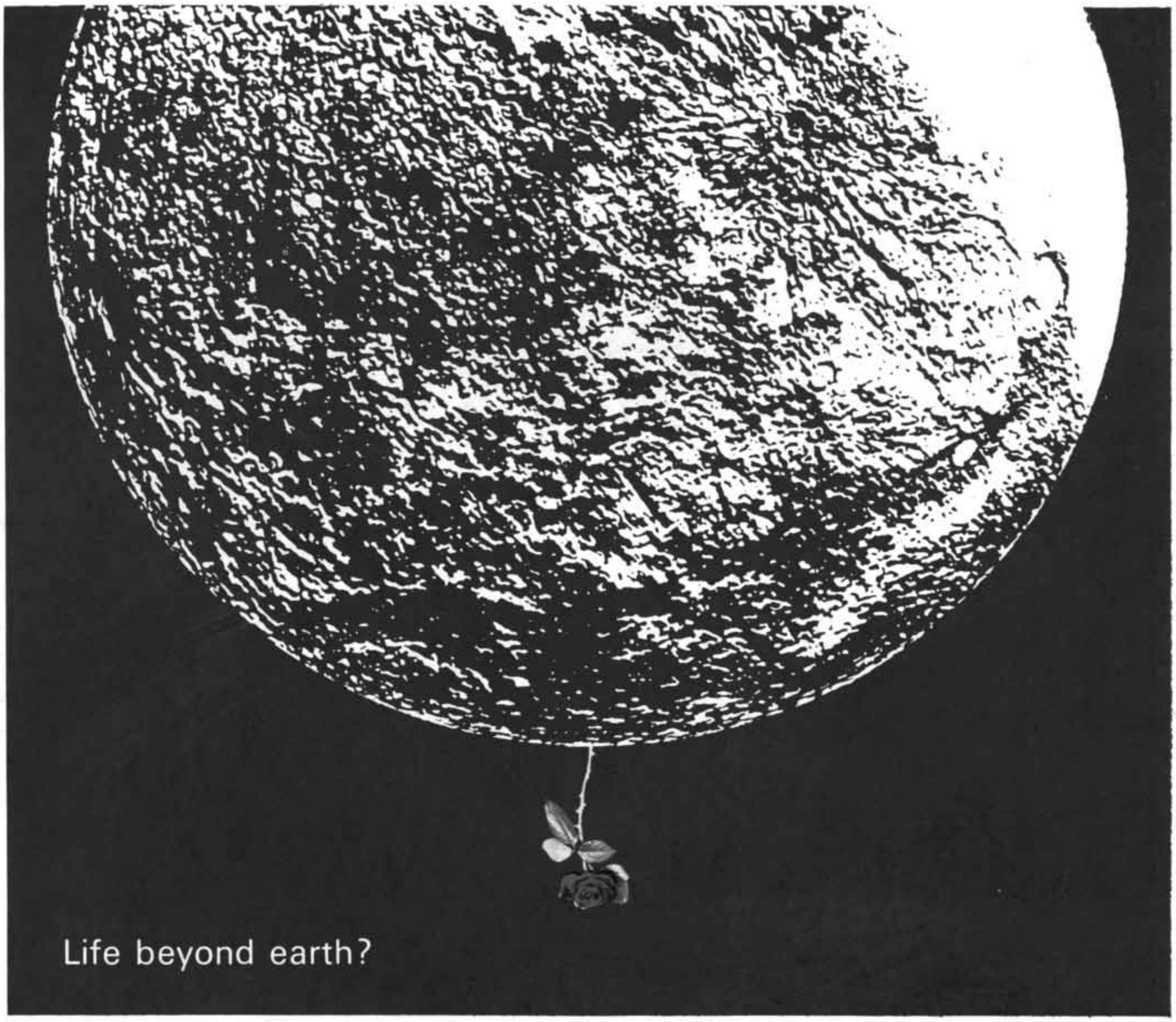

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ARTICLES

DEPARTMENTS

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THE COVER

The photograph on the cover shows part of an experiment designed to simulate the faint glow that is produced by excited atoms and molecules in the earth's upper atmosphere (see "The Airglow," page 102). Gas composed mostly of molecular nitrogen (N_2) is streaming from left to right across the entire area of the photograph. On the left side of the photograph the gas emits a blue light because of chemiluminescent reactions between small amounts of atomic oxygen (O) and atomic nitrogen (N) contained in the main gas stream. The spherical gas inlet in the center of the photograph introduces nitric oxide (NO) to the system, which reacts with the nitrogen atoms in the main stream to yield more molecular nitrogen and atomic oxygen. The oxygen atoms then react with excess nitric oxide molecules to produce nitrogen dioxide molecules (NO_2), some of which are excited and emit light, which here has a predominantly greenish cast. At a later stage in the experiment, when neither the blue nor the green light is seen, all the nitrogen atoms in the stream have been replaced by oxygen atoms, which then interact to produce a much weaker and monochromatic green light with a wavelength of 5,577 angstrom units. The green line at this wavelength is a conspicuous feature of airglow spectrograms.

THE ILLUSTRATIONS

Cover photograph by Jon Brenneis

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LETTERS

Sirs:

In your department "Mathematical Games" for January the good Dr. Matrix states: "Four is the only number, among all the infinity of numbers, that states correctly the number of letters in its English name."

How about two cubed?

MORRIS H. WOSKOW

University of California
Davis, Calif.

Sirs:

Marvin Shinbrot, in his fascinating article "Fixed-Point Theorems" [SCIENTIFIC AMERICAN, January] made the following statement concerning classical physics and determinism: "In any mechanical system, whether it is the moon and the earth or a swinging pendulum, the motions of the system are completely determined by its initial displacements and velocities." I feel compelled to underline his assumption that the interaction forces are known. The importance of this assumption is brought out in the glancing collision of two perfectly elastic spheres whose exact initial displacements and velocities only are known. Neither the displacements nor the velocities of these spheres after collision can be predicted by calculations!

Even if the interaction force is given with the initial conditions, small perturbations of the system limit prediction of positions and velocities through successive collisions to the immediate future. Émile Borel has shown that a perturbation as small as a one-centimeter displacement of a one-gram mass on the star Sirius makes predictions about molecular collisions on earth invalid after microseconds!

JOHN B. HART

Xavier University
Cincinnati, Ohio

Sirs:

Musicians are always thankful for any attention their art receives from the world of science, and I hope it will not be taken for ingratitude if I correct some minor inaccuracies in E. Donnell Blackham's interesting discussion of

"The Physics of the Piano" [SCIENTIFIC AMERICAN, December, 1965].

"Each octave has eight white keys," he writes on page 92, "for playing the diatonic scale (whole notes)." Mr. Blackham probably meant to say "whole *steps*" or "whole *tones*." The whole note, called in England the semibreve, has nothing to do with scales; it is a symbol for the duration of a tone that lasts four times as long as a quarter note.

Even when this slip has been corrected, however, the sentence remains inaccurate, for the term "diatonic scale" is not to be equated with or explained as "whole steps" any more than as "whole notes." The very essence of the diatonic scale is its mixture of whole steps with certain half steps, as in all major and minor scales.

It must also be pointed out that the introductory sentences of the same paragraph, although separately true, become misleading in juxtaposition. "The keyboard of the modern piano is constructed on essentially the same principles that had been fully developed before the 15th century. The standard keyboard has 88 keys divided into seven and a third octaves...." This seems to say (although I am sure it was not the author's intention) that the 15th-century keyboard, like the modern one, already had 88 keys and more than seven octaves. The facts are that as late as the 17th century most keyboards had only four octaves, Bach's harpsichord in the 18th century had something over five, the pianos of the time of Mozart ordinarily had five (61 keys), and Beethoven's Broadwood in the early 19th century had six; seven-octave keyboards, although heard in the 1820's, became common only after 1840.

One final point. In connection with the impact noise of the hammer, Mr. Blackham says: "Some composers even write with this specific quality in mind. An example can be found in *Piano Concerto No. 2* of the American composer Edward MacDowell, in which certain passages are marked *martellato,* presumably to indicate that as much hammer noise as possible should be introduced into the passage." Nothing of the sort is to be presumed. *Martellato,* meaning "hammered" in Italian, is a standard direction, occurring more often in music for bowed stringed instruments than in piano music, and indicating a staccato style with strong accents—obtained in the stringed instruments by a special method of bowing and on the piano by a sort of hammering technique of the player's hands. In both cases it refers to a style or manner of

playing and not to actual hammers on an instrument.

HOWARD SHANET

Associate Professor of Music
Conductor of the University Orchestra
Columbia University
New York, N.Y.

Sirs:

Permit a minor correction to the historical section of E. Donnell Blackham's interesting paper on the physics of the piano in *Scientific American:*

The last of the important steps in the development of the modern piano was not, as is generally believed, first taken by the justly famous Henry Steinway (Steinweg). In August, 1842, the Danish piano maker C. C. Hornung applied for a Danish patent for a one-piece cast-iron frame for all kinds of pianos. At the same time manufacturing started—13 years ahead of Steinway. The patent was granted in 1843.

C. C. Hornung and a fellow craftsman, Møller, began making pianos in 1827, and the firm Hornung & Møller is still very much alive.

CHR. BERGSØE, M.SC.

Copenhagen-Lyngby
Denmark

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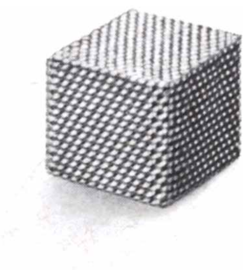

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50 AND 100 YEARS AGO

SCIENTIFIC AMERICAN

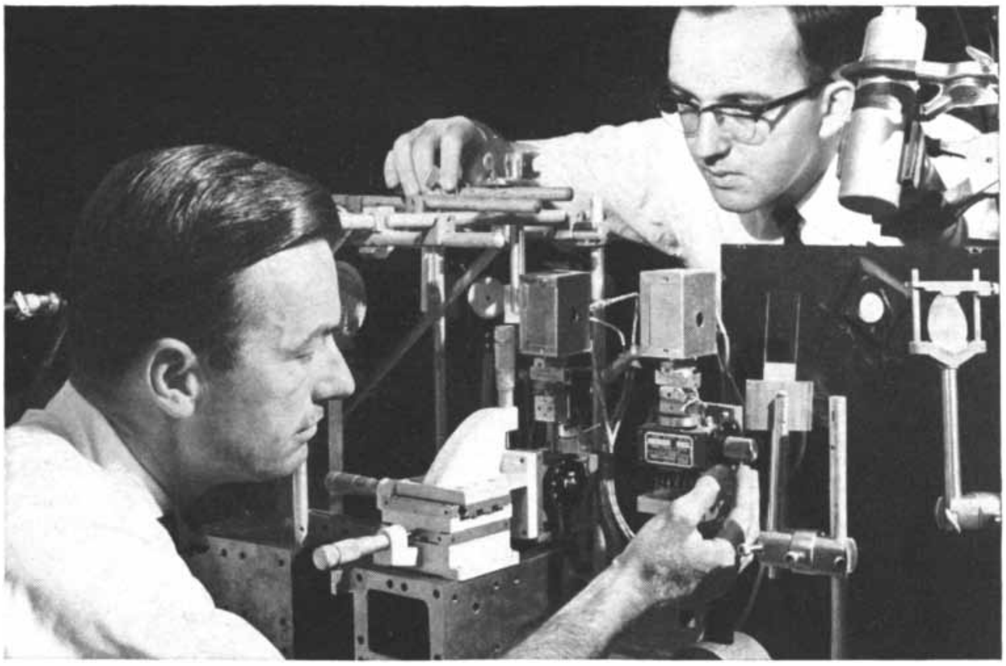
MARCH, 1916: "In the years preceding the war we were told that so murderous would be the combined artillery, machine-gun and rifle fire from an entrenched position that no troops in the world would attempt a frontal attack. The war has proved the very contrary. Indeed, the present gigantic struggle for the capture of Verdun has consisted, so far as the German infantry is concerned, in little else but frontal attacks against what are probably the most completely defended positions in the world. It is probable that since the early history of warfare troops have never been subjected to such a frightful ordeal as the dense masses of German infantry which have stormed, or attempted to storm, the outlying Verdun positions. The High Command of the German army knows what it is about, but the average layman is beginning to ask himself how long the German army can last if its numbers are to be reduced through the coming spring and summer at the rate of wastage in the Verdun battle."

"Director Hale of the Mount Wilson Observatory reports in a recent note that at the close of the 1915 construction season the steel dome for the 100-inch reflector—the world's largest telescope—was completely enclosed and in working order. The shipment of the tube, constructed at the Fore River Ship Yards, has been delayed by the suspension of traffic via the Panama Canal. The parabolizing of the 100-inch mirror is now 85 per cent complete. It is thought that the great telescope cannot be ready for use before the summer of 1917."

"According to information secured from an authentic source, it is learned that during the recent Zeppelin raid on Paris 40 bombs were dropped by the aircraft, part of them incendiary and the remainder explosive. Since a number of the bombs failed to explode, the authorities have been given an opportunity of examining their construction.

Report from

BELL LABORATORIES



R. C. Miller (left) and J. A. Giordmaine check the alignment of the crystal in which variable-frequency, laser-type light is generated.

A Tunable Source of "Laser" Light

A narrow beam of light, as generated by a laser, appears to offer many desirable qualities as a possible medium of communication. Individual lasers, however, operate at separate, discrete frequencies. For communications, tunable sources of light comparable to the variable-frequency oscillators used in radio work are useful.

Recently, Bell Telephone Laboratories scientists J. A. Giordmaine and R. C. Miller demonstrated an experimental tunable source of this type. Operating on parametric oscillation principles at optical frequencies (see illustration below), the device uses a crystal of lithium metaniobate, which is "pumped" by a laser beam. The device emits two beams, each of which is tuned by changing the temperature of the crystal. With the present model an 11° C temperature change produces a 6 percent change in output wavelength of each of the beams.

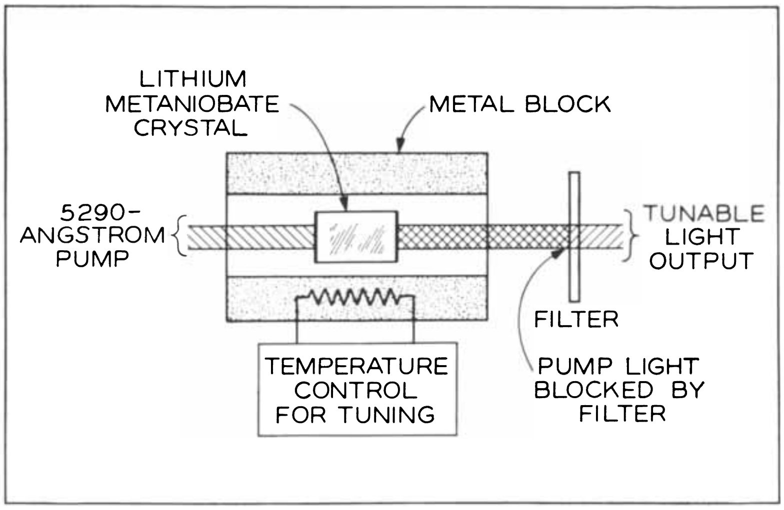
Tunable, coherent sources represent a versatile scientific tool of importance for optical spectroscopy. In other applications, they could function as local oscillators in optical-frequency superheterodyne receivers.

Operating features of tunable source based on parametric oscillation at optical frequencies: "pump" light from laser enters lithium metaniobate crystal at left, and, as a consequence of parametric oscillation, two additional beams are produced in the crystal. End surfaces of crystal, to which dielectric coatings have been applied, are partially reflecting. From right end emerge the two beams, plus the pump light, which is blocked by the filter.

The principles governing parametric oscillation include the conservation of the energy and momentum of the interacting photons. As a consequence of energy conservation, the sum of the two output frequencies equals that of the pump. These output frequencies vary with temperature since the crystal's temperature-dependent index of refraction controls photon momentum in the beams.

In current work, the second harmonic of a pulsed calcium tungstate/neodymium-doped laser provides the required 7 kilowatts of pump power. Pump frequency of 5.7 x 10^5 gigacycles (5290A wavelength) produces output frequencies ranging from about 2.6 x 10^5 gigacycles (11,500A) to 3.1 x 10^5 gigacycles (9700A), depending on temperature.

Lithium metaniobate, whose unique optical properties are essential to this effect, was first investigated in detail at Bell Laboratories where, also, large optical-quality crystals for this experiment were grown.



Bell Telephone Laboratories

Research and Development Unit of the Bell System

They are ordinary steel spheres without handles; two of them weigh 136 pounds each and measure 12½ inches in diameter. The shell is 5/16 inch thick and contains 46 pounds of trinitrotoluene. The third bomb weighs 224 pounds and measures 20 inches in diameter."

"Periodicities in various meteorological and magnetic phenomena on the earth have been more or less conclusively linked up with the sunspot period and other periods of solar activity; hence it is natural to look for analogous correspondences between the phenomena of the sun and of planets other than our own. In this connection *Nature* cites some recent researches by T. Köhl, who finds that Jupiter's northern cloud belts appear to be especially weak at times of sunspot maxima, and to become broader and more conspicuous during sunspot minima. Observations of the 'secondary light' on the dark side of Venus suggest coincidence in time with auroral displays on the earth, and the latter, of course, coincide with periods of solar activity."

Scientific American.

MARCH, 1866: "All the facts of geology tend to indicate an antiquity of which we are beginning to form but a dim idea. Take, for instance, a single example—England's well-known chalk. This consists entirely of shells and fragments of shells deposited at the bottom of an ancient sea far away from any continent. Such a process as this must be very slow; probably we should be much above the mark if we were to assume a rate of deposition of 10 inches in a century. Now the chalk is more than 1,000 feet in thickness and would therefore have required more than 120,000 years for its formation. The fossiliferous beds of England as a whole are more than 7,000 feet in thickness, and many which measure only a few inches expand on the Continent into strata of immense depth, while others of great importance elsewhere are wholly absent in England. Moreover, we must remember that many of the strata now existing have been formed at the expense of older ones; thus all the flint gravels in the south-east of England have been produced by the destruction of chalk. This again is a very slow process. It has been estimated that a cliff 500 feet high will be worn away at the rate of an inch in a century. The Wealden Valley is 22

miles in breadth, and on these data it has been calculated that the denudation of the Weald must have required more than 150,000,000 of years."

"The fatal truth connected with the *vaisseaux blindes,* or French iron-clads, about which so much fustian has been talked and written, is at last made clear. These vessels are of no use whatever. The iron-plated vessels, having made a hole in the budget through which has passed 100 million francs of the public money, are declared only fit to remain stationary in port and will never be able to use their artillery at sea, the slightest motion of the sea paralyzing the action of the guns. This unexpected check to the dream of maritime power indulged in by the Emperor has given a terrible blow to his *amour propre.* The mania for creating a monster navy and possessing those three great elements of power—ships, colonies and commerce—which, according to the great Louis Napoleon, must be regarded as the very souls of national greatness in modern times, is increasing with the Emperor's old age."

"The *Spectator* insists that to secure house room for the working class their dwellings in great cities must be built into the air. The cost of the site must be distributed among many floors. Inside corridors can be superseded by broad, continuous outside balconies. Each tenant would thus possess a separate house, and the sense of living in a barrack, which workmen so much dislike, would be obviated. Such balcony streets, moreover, would be thoroughfares and allow of supervision much more easily than corridors, and they would also allow the hardworking poor to open little shops above the ground floor—an impossibility with existing architecture."

"The Russian chemist M. Mendelejeff has just published the results of a series of very laborious researches with respect to the specific gravity of absolute alcohol, and of the various compounds of alcohol with water. Curiously enough, these results go to show that of all previous determinations of the specific gravity of alcohol and its hydrates the oldest, being those made by Gilpin in 1794, are the most accurate. M. Mendelejeff's experiments far transcend in accuracy all previous ones upon the same subject, their author having taken into account every possible source of error and having bestowed the utmost pains upon ascertaining the magnitude of each."

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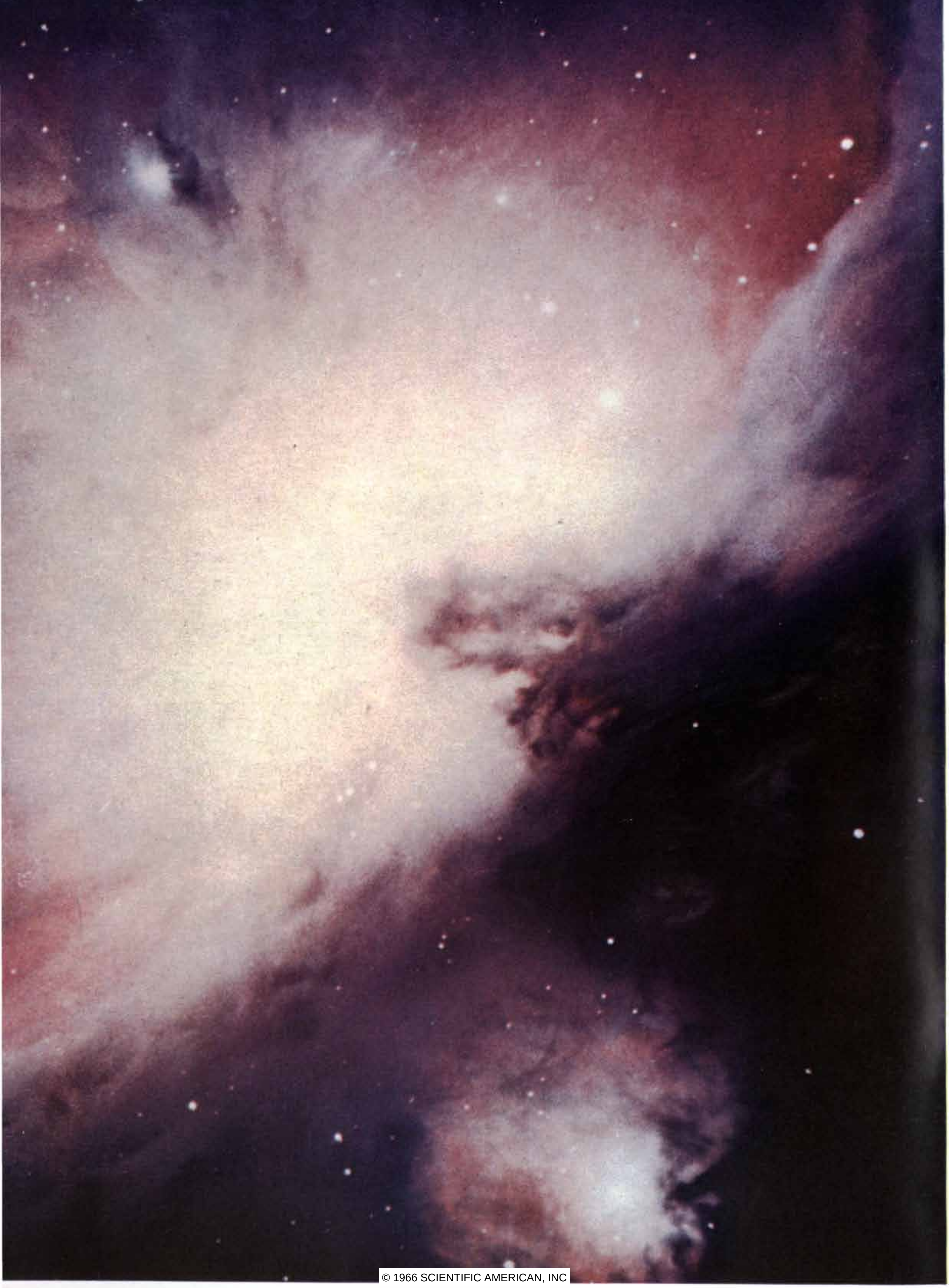


THE AUTHORS

JOSEPH B. MACINNIS ("Living under the Sea") is director of diving research for Ocean Systems, Inc. He is a Canadian who obtained a medical degree at the University of Toronto in 1962, turned to the subject on which he writes after having studied high-pressure physiology at the University of Pennsylvania School of Medicine. His chief interests are the psychological and physiological responses of humans to diving, living and working in the deep sea and the development of life-support systems that facilitate human activity under the sea. MacInnis, who held three Canadian swimming records in 1956 and 1957, has himself taken part in dives as deep as 600 feet, made several underwater films, conducted research on diving problems and written extensively about the pleasures and problems of undersea activity by humans.

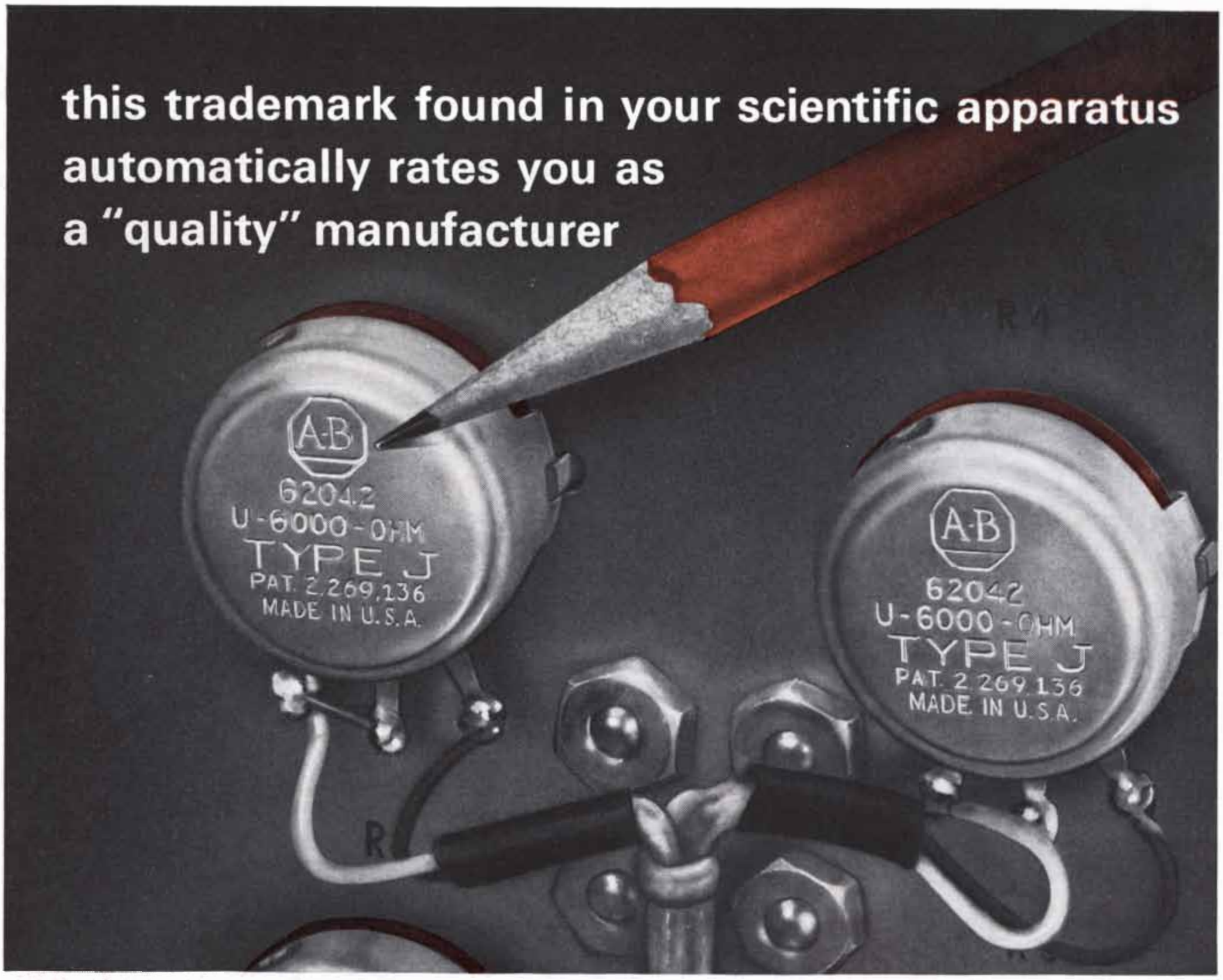
FRED RAPP and JOSEPH L. MELNICK ("The Footprints of Tumor Viruses") work at the Baylor University College of Medicine, where Rapp is associate professor of virology and Melnick is professor of virology and epidemiology and chairman of his department. Rapp, a graduate of Brooklyn College, received a Ph.D. at the University of Southern California at Berkeley in 1958. He taught there for a year and at the Cornell University Medical College for three years before going to Baylor in 1962. Melnick, who was graduated from Wesleyan University, obtained a Ph.D. at Yale University in 1939 and remained there for 18 years as a member of the faculty of the School of Medicine. He was chief of the virus laboratories in the division of biologics standards at the National Institutes of Health for a year before going to Baylor in 1958. Rapp and Melnick, who have begun investigating cancer after years of research in virology, have learned how to confer a tumor-causing potential on viruses that do not normally have that property; they write that they "are interested in learning how to make the converse operational."

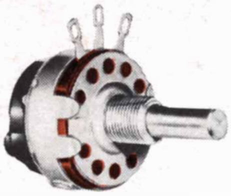
J. N. JAMES ("The Voyage of *Mariner IV*") is a member of the Jet Propulsion Laboratory of the California Institute of Technology. He was project manager for the Mariner flights to Venus and Mars; last year he received the Exceptional Scientific Achievement Medal of the National Aeronautics and Space Administration for that work. Recently he became acting assistant director of lunar and planetary projects at the Jet Propulsion Laboratory. James, who received a bachelor's degree in electrical engineering from Southern Methodist University in 1942 and a master's degree from Union College in 1948, did radar work with the Navy during World War II. He spent five years in private industry before joining the Jet Propulsion Laboratory in 1950.

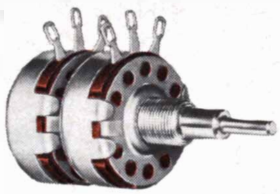
E. A. MUYDERMAN ("Bearings") is a research engineer at the Philips Research Laboratory in the Netherlands. He did his undergraduate work at the Technical University of Delft, where he studied the road-holding properties of vehicles. Joining Philips in 1958, he was given a choice of activities and chose bearings. That work introduced him to the subject of air-lubricated bearings. He concentrated on an investigation of their properties and was led to the development of the spiral-groove bearings that he describes in his article. The theoretical work on these bearings formed the basis for the Ph.D. he obtained at the Technical University of Delft in 1964.

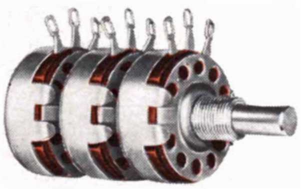
PETER F. BAKER ("The Nerve Axon") is a university demonstrator in physiology at the University of Cambridge and also a fellow of the university's Emmanuel College. He did his undergraduate work at the university and stayed on to obtain a Ph.D., which involved him in work on the phosphorus metabolism of nerve. During that work he became interested in axons. In 1964 he visited the U.S. as a guest investigator at Rockefeller University.

D. J. MULVANEY ("The Prehistory of the Australian Aborigine") is senior fellow in prehistory at the Australian National University. As an undergraduate at the University of Melbourne, concentrating on Greek and Roman history, he made a special study of Roman Britain and by that means became interested in archaeology. "I realized," he writes, "that archaeological methods could be applied in Australia, but no formal instruction in prehistoric archaeology was then offered in Australia." He went to the University of Cambridge to study prehistoric archaeology from 1951 to 1953. Thereafter for 10 years he lectured in ancient world history at the University of Mel-

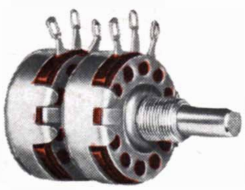


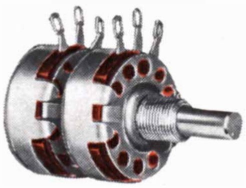

Type JS single unit with line switch


Type JJC dual unit with concentric shaft


Type JJJ triple unit


Type JL single unit with lock bushing


Type JJ dual unit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bourne "but spent much of my vacation time excavating aboriginal sites." In some of this work he had the sponsorship of the Nuffield Foundation. Last year he moved to the Australian National University in Canberra, where he devotes full time to research in prehistory.

THEODORE H. SAVORY ("False Scorpions") is vice-principal of Stafford House, a tutorial college in Kensington, England. Although he describes himself as having spent most of his adult years leading "the typical life of a British public school master," he is in fact one of the world's foremost authorities on arachnids, the group of animals that includes spiders, daddy longlegs and scorpions. His interest was kindled when he was 16 and "contemplating the idea of specializing in some sort of animal." One day he was reading outdoors when a spider dropped from an oak tree onto his book. Savory "said to companion, casually, 'What about spiders?' 'Why not?' replied he, and so it was." After graduating from the University of Cambridge in 1918, Savory spent 31 years teaching science at Malvern College and seven years as senior biology master at the Haberdashers' School in Hampstead. He took his present position in 1958. The article on false scorpions is his third in SCIENTIFIC AMERICAN; the others were "Spider Webs" in April, 1960, and "Daddy Longlegs" in October, 1962.

ROBERT A. YOUNG ("The Airglow") is senior physicist in the department of atmospheric sciences at the Stanford Research Institute. After receiving a Ph.D. in 1959 at the University of Washington, where he also did his undergraduate work, he spent a year with the Boeing Airplane Company before joining the Stanford Research Institute. There he is involved in work on chemiluminescence, which he describes in his article and in a book he is writing, and on laser research, ionization phenomena, vacuum ultraviolet photolysis and rocket exploration of the upper atmosphere. He writes that photography was one of his main hobbies until he learned to fly. "Now," he adds, "I own a Cessna 210, which I have flown 30,000 miles in the past eight months."

N. W. PIRIE, who in this issue reviews *Science in History,* by J. D. Bernal, is head of the biochemistry department of the Rothamsted Experimental Station in Britain.

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Living under the Sea

To learn more about the ocean and harvest its resources, men must be able to live and work as free divers on the continental shelf. Several research programs are currently developing this ability

by Joseph B. MacInnis

It is one thing to glimpse a new world and quite another to establish permanent outposts in it, to explore it and to work and live in it. In recent years parts of the ocean floor have been studied in considerable detail, but almost entirely by surface-bound investigators. They have sounded the oceans with electronic devices, dangled instruments thousands of feet below the surface and secured samples of the bottom, and a few have undertaken brief expeditions in submersible vehicles to the greatest depths. Now, however, men are beginning to try to live underwater—to remain on the bottom exposed to the ocean's pressure for long periods and to move about and work there as free divers.

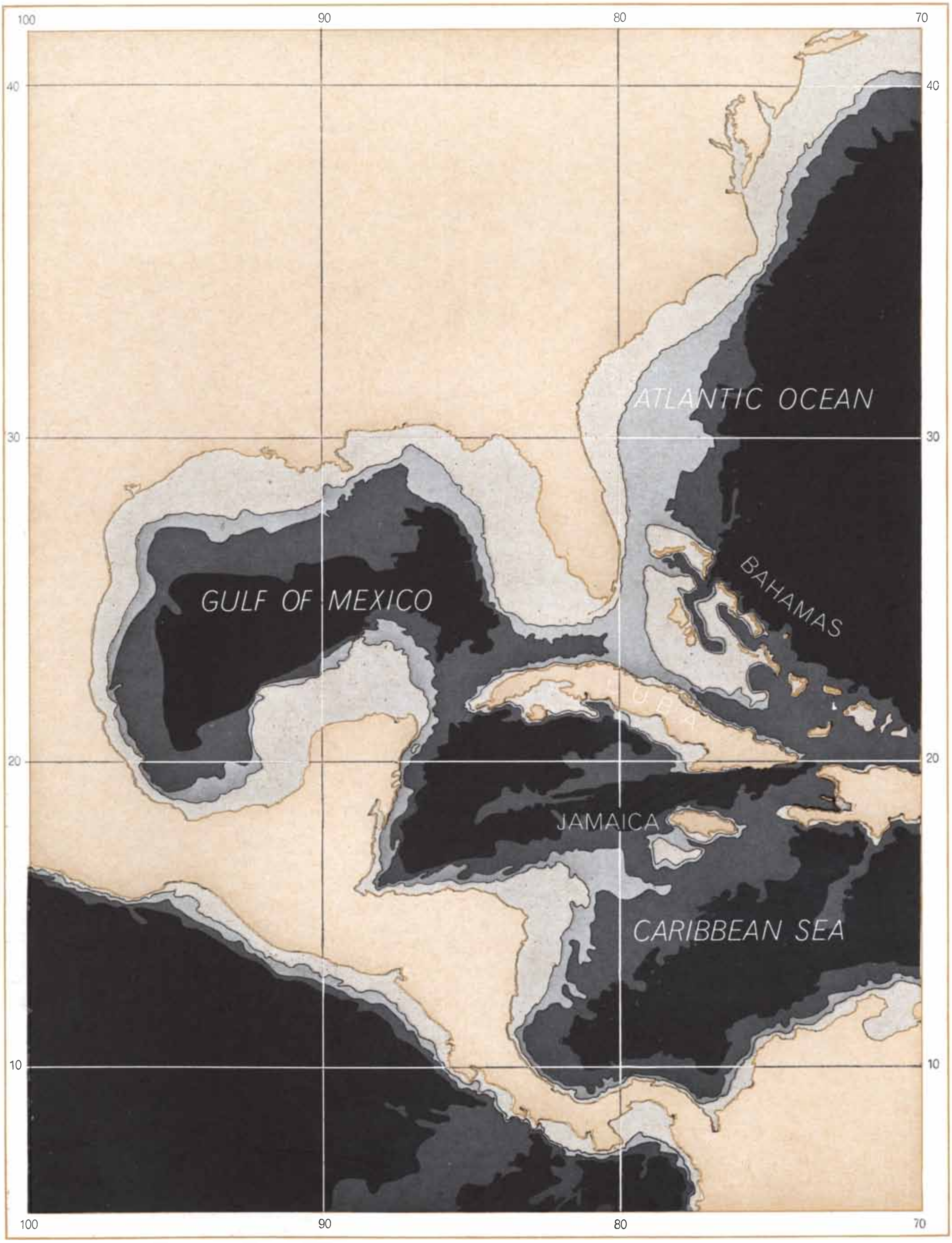
The submerged domain potentially available to man for firsthand investigation and eventual exploitation can be regarded as a new continent with an area of about 11,500,000 square miles—the size of Africa. It comprises the gently sloping shoulders of the continents, the continental shelves that rim the ocean basins. The shelves range up to several hundred miles in width and are generally covered by 600 feet of water or less. That they are submerged at all is an accident of this epoch's sea level: the ocean basins are filled to overflowing and the sea has spilled over, making ocean floor of what is really a seaward extension of the coastal topography. Geologically the shelf belongs more to the continents than to the oceans. Its basement rock is continental granite rather than oceanic basalt and is covered largely with continental sediments rather than abyssal ooze.

Not surprisingly, mineral deposits similar to those under dry land lie under the shelf. Oil and natural gas are the foremost examples. In 1964 alone the petroleum industry spent $5 billion to find and recover offshore oil; only recently the continental shelf in the North Sea has become the site of extensive exploration for oil and gas. Drilling and capping a well from the surface is not easy. The prospect of more efficient oil and gas operations in deeper water by men working on the floor of the shelf is one of the primary reasons for the surge of activity directed toward living under the sea. There are other reasons. One is the increasing interest in all aspects of oceanography, coupled with an awareness of the geological, biological and meteorological information to be gained in direct undersea investigations. Another is the advance in free-diving techniques that began with the invention of "self-contained underwater breathing apparatus" (SCUBA) in the 1940's. Finally, there is a need for improved methods of underwater salvage and submarine rescue.

The reasons for going underwater are balanced by an impressive list of potential hazards. Most of them stem from the effects of pressure, which increases at the rate of one atmosphere (14.7 pounds per square inch, or 760 millimeters of mercury) with every 33 feet of depth in seawater.

The best-known hazard and one of the most dangerous is decompression sickness—"the bends." Under pressure the inert gas in a breathing mixture (nitrogen or helium) diffuses into the blood and other tissues. If the pressure is relieved too quickly, bubbles form in the tissues much as they do in a bottle of carbonated water when it is opened. Sudden decompression from a long, deep dive can be fatal; even a slight miscalculation of decompression requirements can cause serious injury to the joints or the central nervous system. A diver must therefore be decompressed slowly, according to a careful schedule, so that the inert gas can be washed out of the tissues by the blood and then exhaled by the lungs. Whereas the demands of decompression become more stringent with depth, with time they increase only up to a point. After about 24 hours at a given depth the tissues become essentially saturated with inert gas

UNDERWATER DWELLING called the SPID (for "submerged, portable, inflatable dwelling") was designed by Edwin A. Link as a base of operations for long dives to the continental shelf. In the photograph on the opposite page the SPID is undergoing a pressure test at 70 feet. In the summer of 1964 two divers occupied the SPID for two days at 432 feet below the surface.



CONTINENTAL SHELF (*lightest areas*) off part of North America is shown. It is less a part of the ocean basin than it is an extension of the continental land mass. As in most parts of the world, the shelf slopes gently to about 600 feet below sea level; then the continental slope plunges toward the floor of the ocean basin. On this map, based on charts of the International Hydrographic Bureau, the contour intervals are in meters rather than feet. The lightest tone shows the bottom from sea level down to 200 meters (655 feet); successively darker blacks indicate bottom from 200 to 1,000, 1,000 to 3,000 and deeper than 3,000 meters.

at a pressure equivalent to the depth; they do not take up significantly more gas no matter how long the diver stays at that level. Therefore if a diver must descend to a certain depth to accomplish a time-consuming underwater task, it is far more efficient for him to stay there than to return to the surface repeatedly, spending hours in decompression each time. Although this "saturation diving" is efficient, it imposes an extra technical burden, because the schedules for the ultimate decompression must be calculated and controlled with particular care.

Pressure also has significant effects on a diver's breathing requirements. For one thing, hyperoxia (too much oxygen) becomes almost as dangerous as hypoxia (too little). Acute hyperoxia can affect the central nervous system, causing localized muscular twitching and convulsions; chronic hyperoxia impairs the process of gas exchange in the alveoli, or air sacs, of the lung. Optimum oxygen levels are still under investigation; they vary with the duration, depth and phase of the dive and the muscular effort required of the diver. It is clear, however, that the "partial pressure" of oxygen should be kept between about 150 and 400 millimeters of mercury during the at-depth phase of a long saturation dive. The partial pressure of oxygen in the air we breathe at sea level is 160 millimeters of mercury (21 percent of 760). If oxygen is kept at 21 percent of the mixture, however, its partial pressure increases with depth—rising to 1,127 millimeters 200 feet down, for example. As a result the proportion of oxygen in the air or other breathing mixture must be cut back sharply from 21 percent. The band of permissible percentages narrows rapidly with depth [*see illustration on page 30*], calling for increasing accuracy in the systems that analyze and control the gas mixture.

Nitrogen, which is physiologically inert at sea level, has an anesthetic effect under pressure. At depths greater than 100 feet it begins to produce "nitrogen narcosis," an impairment in judgment and motor ability that can render a diver completely unable to cope with emergencies. Helium has been found to be much less narcotic and is currently used instead of nitrogen in almost all deep-sea dives. Being less dense, it also offers less breathing resistance under pressure; this can be important to a working diver. Helium has two disadvantages, however. Because its thermal conductivity is almost six times as great as nitrogen's, it accelerates the loss of body heat and makes a diver uncomfortably cold even at temperatures of 70 or 80 degrees. Helium also distorts the resonance of a diver's voice, making his speech almost unintelligible and thus giving rise to a serious communication problem.

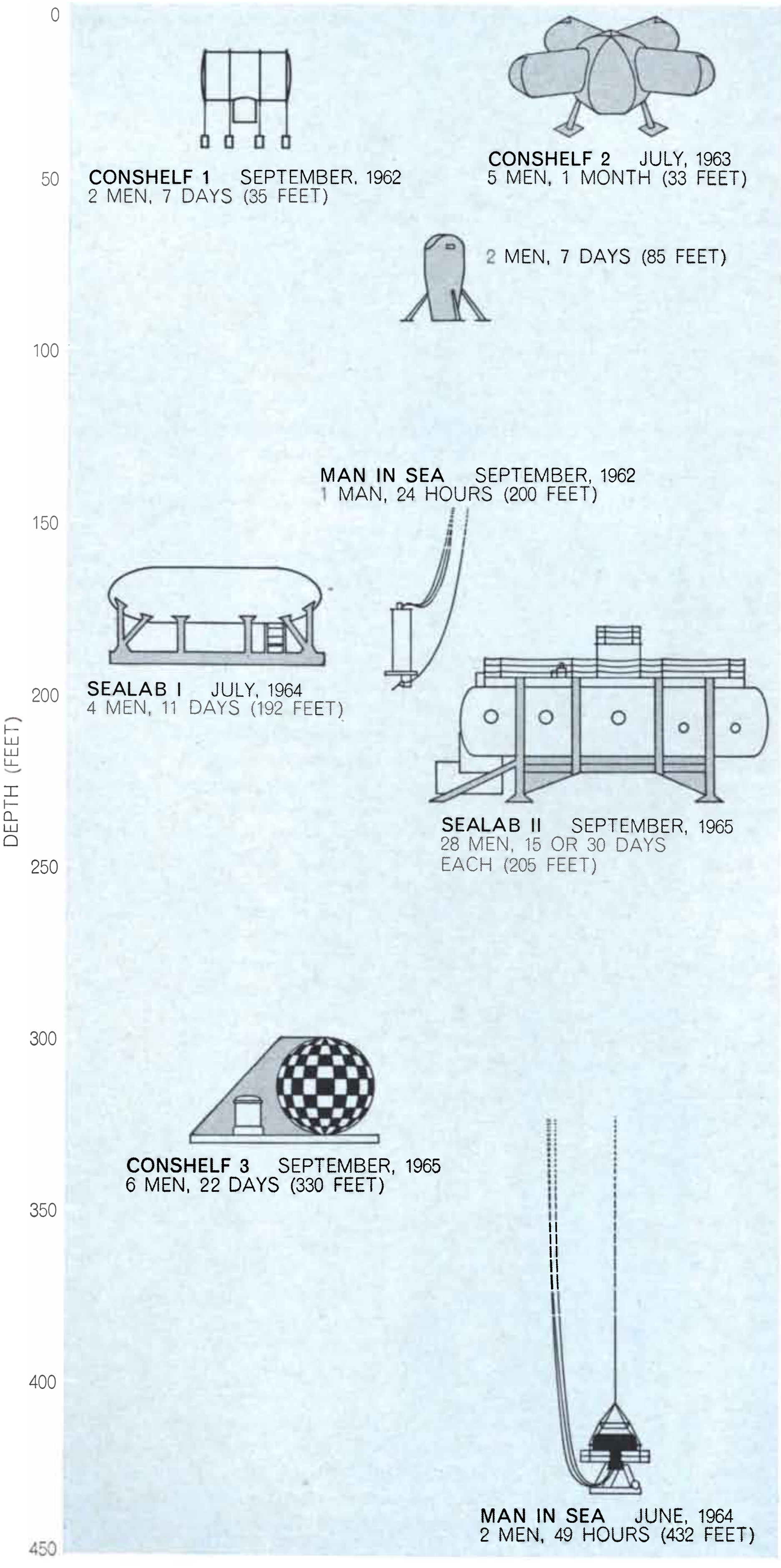
In any confined environment the buildup of exhaled carbon dioxide must be monitored carefully. In our diving experiments for Ocean Systems, Inc., we try to keep the partial pressure of this gas below seven millimeters of mercury (compared with the sea-level pressure in fresh air of .3 millimeter), but at the U.S. Naval Medical Research Laboratory in New London, Conn., Karl E. Schaefer has found that at sea level slightly higher levels are tolerable for several weeks. In any case, carbon dioxide accumulates rapidly in a small space and soon reaches a toxic level, causing dizziness, headache and an increase in the rate of breathing. It must therefore be continuously "scrubbed" out of the diver's atmosphere, usually by being passed through some chemical with which it will react. Other gases, such as carbon monoxide and certain volatile hydrocarbons, can also reach toxic levels quickly if they are allowed to concentrate in the diver's breathing mixture.

There are sometimes other obstacles to casual access to the ocean floor: a demoralizing lack of visibility, strong currents, uncertain bottom profiles. There are also dangerous marine animals, ranging in size from a unicellular infective fungus to the widely feared great white shark. Finally, the water of the continental shelf is cold. Temperatures average between 40 and 60 degrees, and without protective clothing a diver soon becomes totally ineffective.

Faced with these difficulties commercial divers and undersea investigators found it impossible to spend time and do useful work on the continental shelf. Those who went down in pressurized suits and thick-hulled submersible vehicles were held prisoner by their protective armor. Free divers, on the other hand, could not go very deep or stay very long. In 1956 Edwin A. Link, the inventor of the Link Trainer for simulated flight training, was engaged in undersea archaeological investigations. He recognized that a diver could work more effectively at substantial depths if he could live there for prolonged periods instead of having to be decompressed to the surface after each day's work. Link set out to build a vehicle that could operate as an underwater elevator, a diving bell and a decompression chamber. The "submersible decompression chamber" (SDC) he designed is an aluminum cylinder 11 feet long and three feet in diameter [*see illustration on page 29*]. With its outer hatches closed it is a sealed capsule in which a diver can be lowered to the bottom. On the bottom, with the internal gas pressure equal to ambient water pressure and the hatches open, the SDC serves as a dry refuge from which the occupant can operate as a free diver. Then, with the hatches again closed, it becomes a sealed chamber in which the diver can be decompressed safely and efficiently on shipboard or during his ascent to the surface. An inner hatch provides an air lock through which someone else can enter the chamber (or pass food and other supplies into it) during the decompression phase.

Before open-sea experiments with the SDC were possible some preliminary research was necessary. How deep could a man go as a free diver? How long could he stay down? What would be the acute and the long-term medical effects of the pressure itself and of the synthetic atmosphere? What would be the response to the cold, the confinement and the psychological hazards of deep submergence? Some early and significant answers were provided by Captain George F. Bond, a U.S. Navy physician who in 1957 conceived and carried out a series of simulated dives in a compression chamber on land at the Naval Medical Research Laboratory. Bond's group first exposed small animals, including some primates, to a pressure equivalent to a depth of 200 feet. Volunteer Navy divers then lived in the chamber under precisely controlled conditions of pressure, temperature and humidity. These experiments showed, among other things, that men could breathe helium instead of nitrogen for long periods without ill effects and encouraged Link to move ahead.

Early in September, 1962, the SDC underwent its critical test in the Mediterranean Sea off Villefranche on the French Riviera. A young Belgian diver, Robert Sténuit, descended in it to 200 feet and lived there for 24 hours, swimming out into the water to work and returning to rest in the warm safety of the pressurized chamber. When the time came to return to the surface, Sténuit did not have to face hours of dangling on a lifeline or perching on a platform, decompressing slowly in the cold water. Instead he sealed himself into the chamber, was hoisted to the deck of Link's research vessel, the *Sea Diver*, and there was decompressed in



SATURATION DIVING, in which the divers stay down for prolonged periods, is made possible by underwater shelters. The chart gives data for seven such dives. "Man in Sea" is the Link project, "Conshelf" is Jacques-Yves Cousteau's and "Sealab" is the U.S. Navy's.

safety and relative (although somewhat cramped) comfort.

Meanwhile, moving ahead independently, the French undersea investigator and inventor of the aqualung, Jacques-Yves Cousteau, had undertaken experiments aimed at establishing manned undersea stations on the continental shelf. The first experiment in his "Conshelf" program was carried out near Marseilles in mid-September, 1962, when two men lived under 35 feet of water for a week. The divers worked in the sea several hours a day, returning to a cylindrical cabin to eat and sleep.

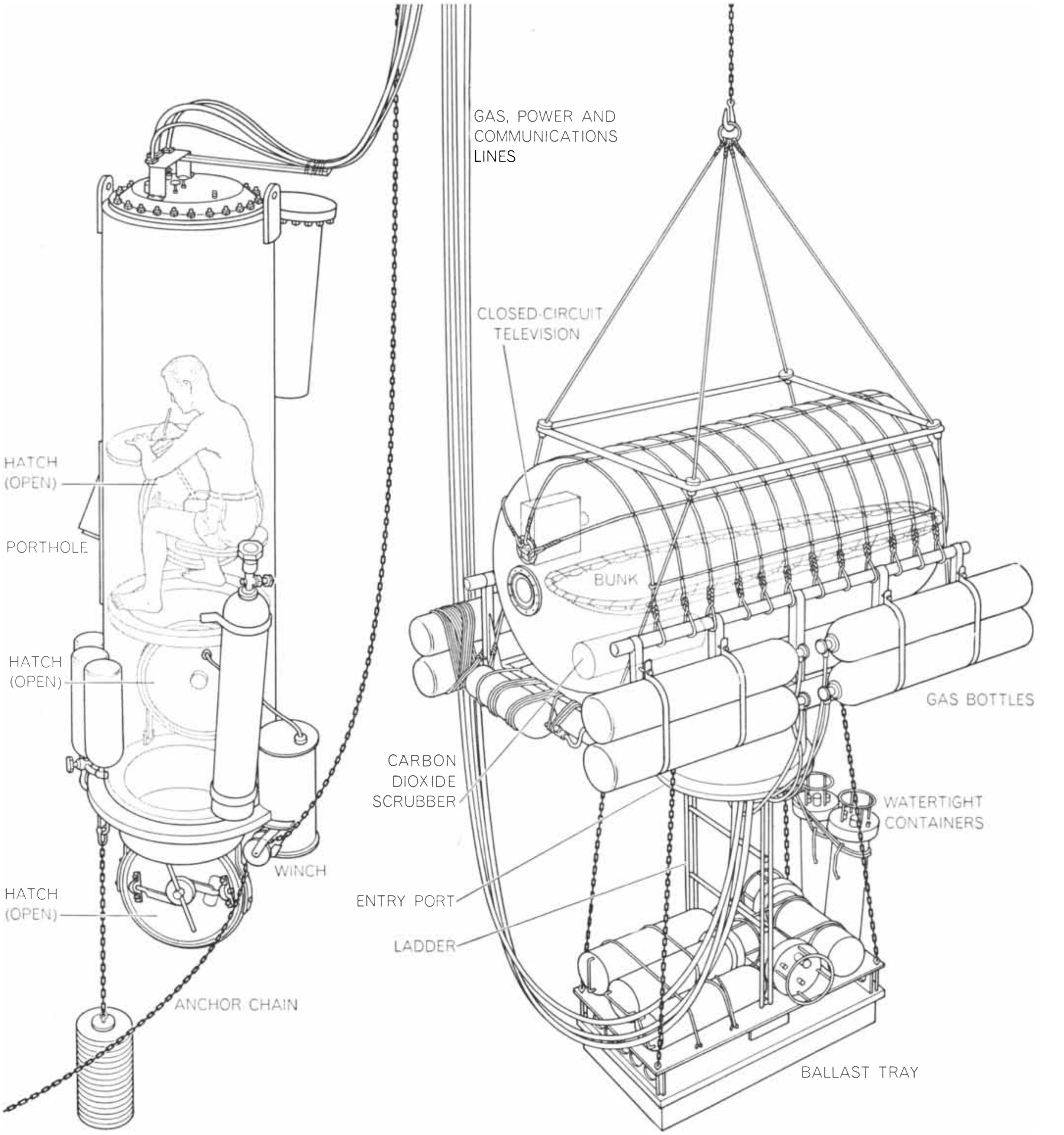
Cousteau's group went on, in the summer of 1963, to establish a complex underwater settlement at the remote Sha'ab Rumi Reef in the Red Sea. The hub of the settlement was "Starfish House," an assembly of cylindrical chambers in 33 feet of water. It housed five men and their eating, sleeping and laboratory facilities for a month. Nearby was a submarine "hangar" from which a two-man "diving saucer," with its pilot and passenger protected from the sea pressure, made a number of trips as deep as 1,000 feet to collect samples and make observations at the edge of the reef. Down the coral slope from Starfish House, at a depth of 85 feet, two men lived in the controlled oxygen-helium-nitrogen environment of a "deep cabin" for seven days, making short excursion dives as deep as 360 feet. One of the most interesting things about Conshelf II was its demonstration that—at least for relatively shallow depths—the participants did not have to be experienced divers or even young men in particularly good physical condition. Instead they were picked for their vocational ability as mechanics, scientific workers, cooks and so on. The experiment showed that biological investigations and submarine operations could be carried out from submerged stations.

In the U.S. meanwhile Link and Bond were designing pressure experiments and engineering diving systems that would enable free divers to reach greater depths safely. From late 1963 until March, 1964, a series of simulated saturation dives—the first such dives deeper than 200 feet—were carried out under the technical direction of Captain R. D. Workman at the Navy's Experimental Diving Unit in Washington. The tests showed that divers suffered no harmful effects when exposed to depths of 300 and 400 feet for 24 hours and that they could be decompressed successfully on a linear decompression schedule.

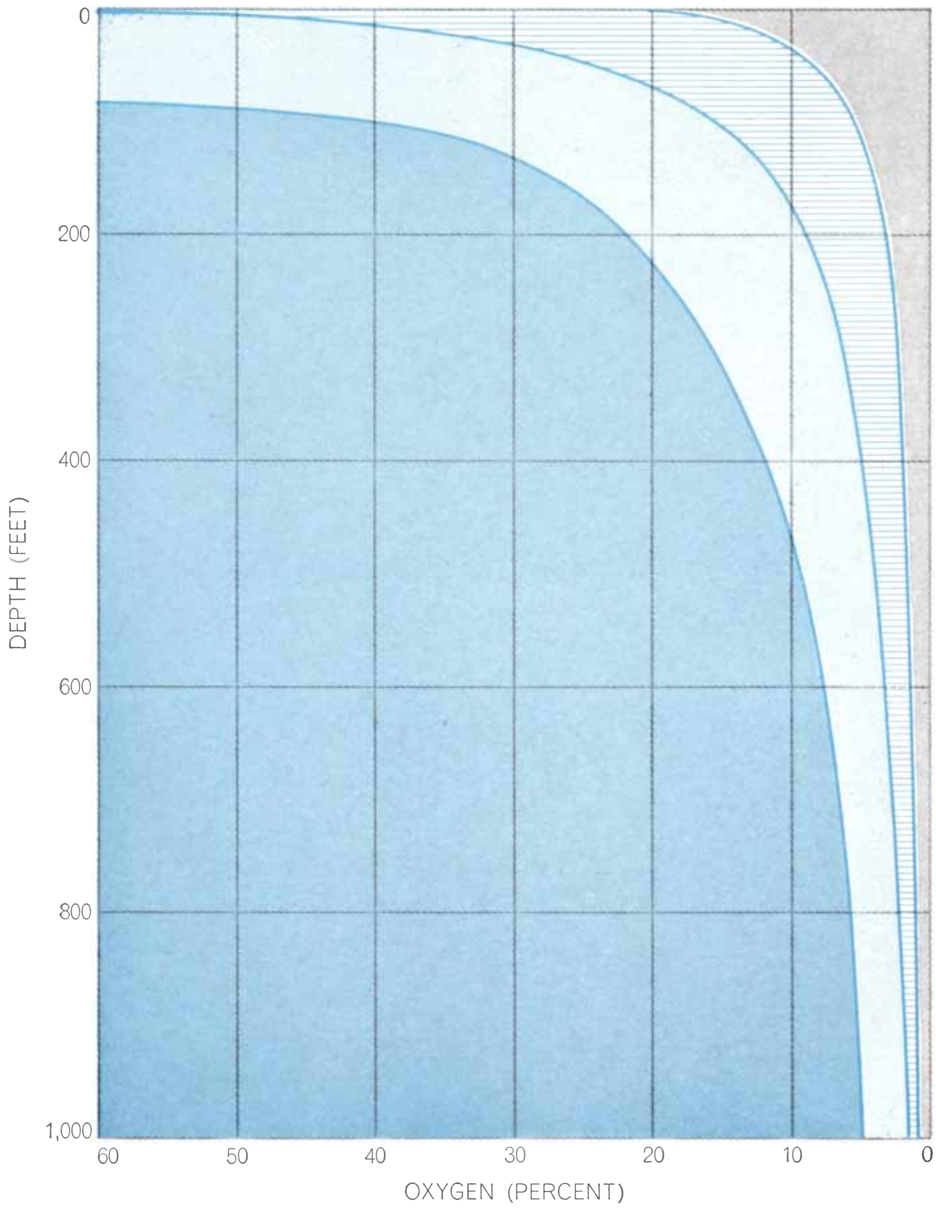
Link had decided that the second

phase of his "Man in Sea" project would attempt to demonstrate that men could work effectively at 400 feet for several days. He established a "life-support" team under the direction of Christian J. Lambertsen of the University of Pennsylvania School of Medicine to undertake preliminary research and supervise the medical aspects of the dive. Under Lambertsen's direction James G. Dickson and I first evaluated the accuracy and reliability of gas analyzers that would monitor the divers' breathing atmosphere. In addition to proving out the system, our experiments showed that mice could tolerate saturation at (and decompression from) pressures equivalent to 4,000 feet of seawater.

The 400-foot dive required the design of a larger and more comfortable "dwelling" on the ocean floor. Such a dwelling presents unusual engineering problems. It must provide shelter and warmth and be easy to enter and leave underwater, simple to operate and resistant to the corrosive effects of seawater. The dwelling must be heavy enough to settle on the bottom but not so heavy that it is hard to handle from the deck of a support ship. Link's unique solution was



TWO CHAMBERS used in the Man in Sea 432-foot, two-day dive are diagrammed. The "submersible decompression chamber," or SDC (*left*), is an aluminum cylinder 11 feet long and three feet in diameter. With the hatches open and the inside gas pressure equal to the external water pressure, the SDC serves as a diving bell. The SPID (*right and photograph on page 25*) is an eight-by-four-foot inflatable rubber dwelling with a steel frame and ballast tray. Access to it is through an open entry port at the bottom.



PERMISSIBLE RANGE (*barred white band*) of oxygen content in the breathing gas supplied to a diver narrows sharply with depth, requiring close control in order to avoid zones in which there is a danger of hypoxia (*gray*) or hyperoxia damaging to the lungs (*light color*) and the central nervous system (*dark color*). For several days' exposure at sea level a safe range is from 20 to 60 percent. With depth the "partial pressure" of oxygen increases, however; at 300 feet, or 10 atmospheres, 2 percent of the breathing mixture represents as much oxygen as 20 percent would represent under sea-level conditions.

in effect an underwater tent: a fat rubber sausage eight feet long and four feet in diameter, mounted on a rigid steel frame [*see illustration on preceding page*]. Deflated at the surface, the "submerged, portable, inflatable dwelling" (SPID) is remarkably easy to handle—an important advantage when undersea habitations are established in remote locations. As it is submerged, the tent is inflated so that its internal gas pressure is equal to the ambient water pressure. There are no hatches; an open, cufflike entry port in the floor of the SPID allows easy access and provides the necessary vertical latitude for variations in the pressure differential. Inside the SPID and in watertight containers on the frame and on the ballast tray below it are stored supplies and equipment: gas cylinders and the gas-circulating system, a closed-circuit television camera, communications equipment, food, water, tools and underwater breathing gear.

In the 400-foot dive the SPID was to be one of three major pressure chambers. The second was the proved SDC and the third was a new deck decompression chamber. This time the SDC was to serve as an elevator and also as a backup refuge on the bottom but not as the main decompression chamber. After a long, deep dive decompression takes several days, and it is important that the divers be as comfortable as possible. An eight-by-five-foot decompression chamber with a four-foot air lock was therefore secured to the deck of the *Sea Diver*. The SDC could be mated to it so that the divers could be transferred to it under deep-sea pressure. Decompression could then proceed under the direct supervision of life-support personnel.

Early in June, 1964, Link and his research group sailed to the Bahamas to test the three-chamber diving concept. We checked the chambers with dives to 40 and 70 feet, spending several weeks refining techniques for handling the SDC and the SPID and coping with potential emergencies. The exact site for the dive, chosen with the cooperation of Navy personnel using sonar and underwater television, was a gentle coral sand slope 432 feet deep, about three miles northwest of Great Stirrup Cay.

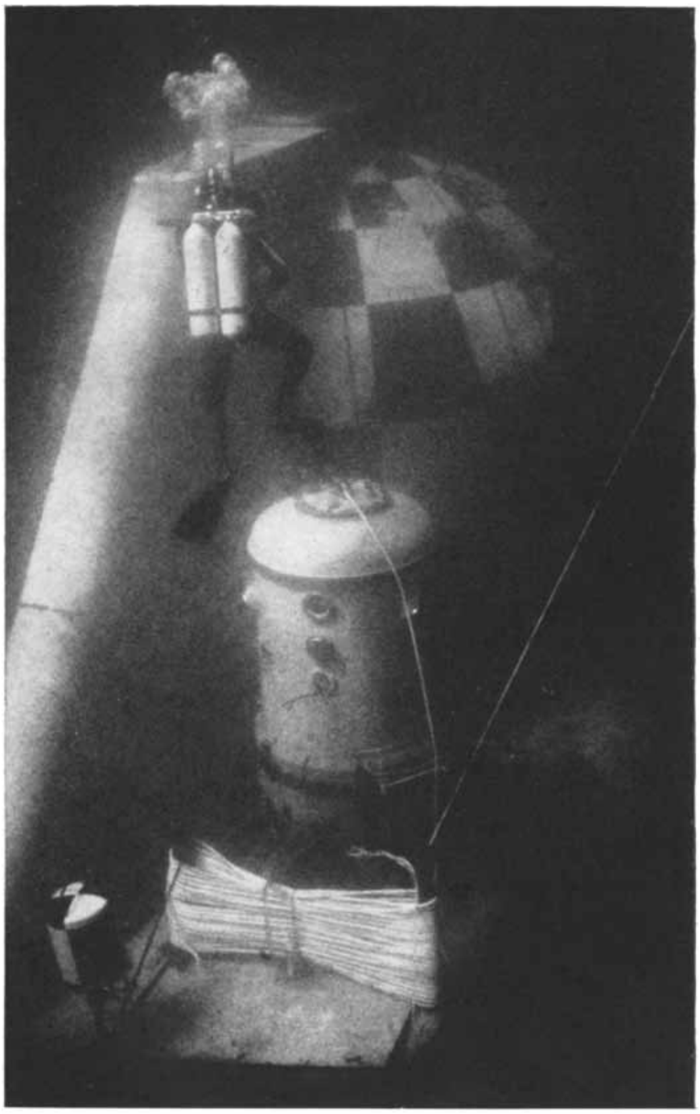
On June 28 the underwater dwelling, with its vital gear carefully stowed aboard, was lowered slowly to the ocean floor. When it had settled on the shelf, the oxygen level inside was adjusted to 3.8 percent, the equivalent of a sea-level partial pressure of 400 millimeters of mercury. The inert gas was helium with a trace of nitrogen (because there had been air in the tent to start with). Then the SPID was left, a habitable outpost autonomous except for communications, power and gas lines, ready for its occupants.

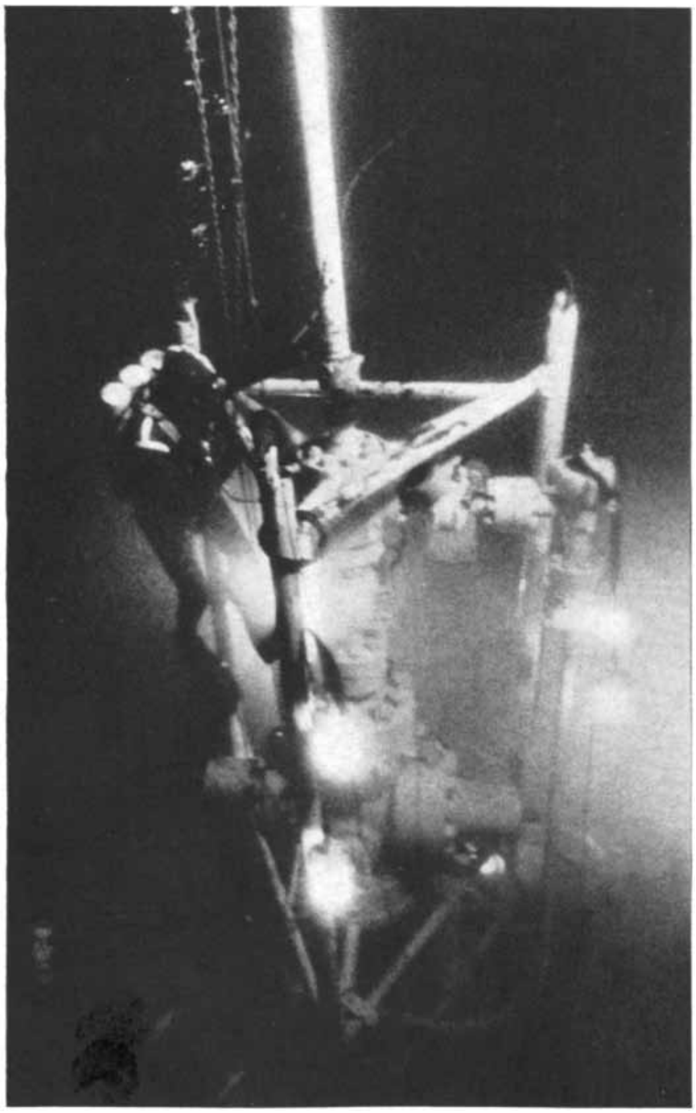
The next step was to transport Sténuit and Jon Lindbergh, another experienced diver, to the shelf. As usual, the SDC was placed in the water at the surface so that the divers could enter it from below. At 10:15 A.M. on June 30 Sténuit and Lindbergh went over the side and swam up into the chamber, closed the outer hatches and checked their instruments. At 10:45, still at the surface, the SDC was pressurized to the equivalent of 150 feet with oxygen and helium to check for leaks; one minor leak was discovered and repaired. At noon the chamber started down, slipping through the clear purple water toward the deep shelf. When it reached 300 feet, Lindbergh reported the bottom in sight. At 1:00 P.M. the anchor weights touched bottom and the chamber came to a stop five feet above the sand. It was just 15 feet from the waiting SPID. During the descent the SDC's internal pressure had been brought to 200 feet; now pressurization was completed. At 1:15 the bottom hatches were opened and Sténuit swam over and entered the dwelling. Lindbergh joined him and they began to arrange the SPID for their stay.

At that point Lindbergh reported that the carbon dioxide scrubber had been flooded and was not functioning. The divers found the backup scrubber in its watertight container and prepared to set it up as the carbon dioxide level rose to almost 20 millimeters of mercury. Then they found they could not get at the reserve scrubber: the pressure-equalizing valve that would make it possible to open the container was missing. With the carbon dioxide level rising rapidly as a result of their muscular exertion, they had to leave the dwelling and return to the SDC. We had hoped to maintain the diving team on the shelf with a minimum of support from the surface, but it now became necessary to send a spare scrubber down on a line from the *Sea Diver*. The divers installed it in the SPID and the dwelling was soon habitable.

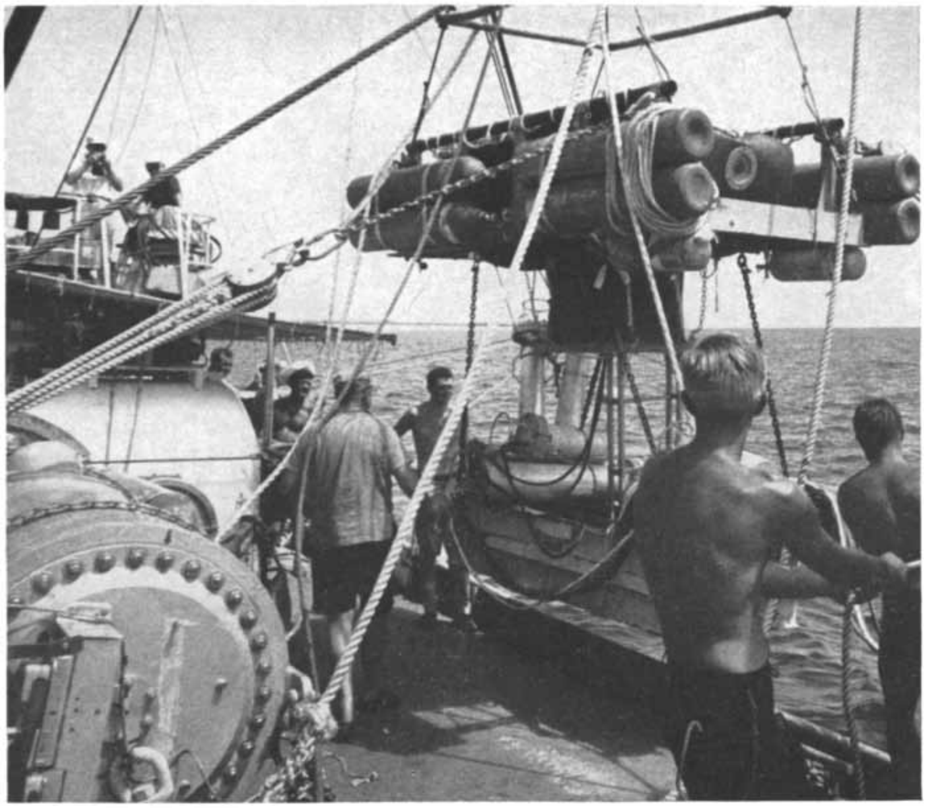
Later that evening the divers took over control of the dwelling's atmosphere, monitoring it with their own high-pressure gas analyzer and adding makeup oxygen as required. We kept watch from the surface by closed-circuit television as Sténuit and Lindbergh settled down for the night. While one slept the other kept watch, checking instruments and communications (a procedure that, as confidence in the system increases, should not be necessary in the future). The water temperature that night was 72 degrees and the dwelling was at 76 degrees, yet both divers later reported that the helium atmosphere was too cold for comfortable sleeping.

In the morning the divers swam over to check the SDC, making sure that it was available as a refuge in case of trouble in the SPID. For the rest of the day both men worked out of the dwelling, observing, photographing and collecting samples of the local marine life. While they were in the water the divers breathed from a "closed" rebreathing system connected to the SPID rather than from an "open" SCUBA system. An open apparatus spills exhaled gas into the sea. At 432 feet, under 14 atmospheres of pressure, each exhalation expends gas equal to a sea-level volume of some seven liters, which would be prohibitively wasteful. Link had designed a system that pumped the dwelling atmosphere through a long hose to a breathing bag worn by the diver. Exhaled gases were drawn back to the dwelling through a second hose to be purified and recirculated. The apparatus worked well except that the breathing mixture was so dense under 14 atmospheres that the pumps could





"CONSHELF III" station at 330 feet was occupied by six divers of Cousteau's group last fall. The spherical dwelling in which they lived is shown at the left in a photograph made from Cousteau's diving saucer. The elongated shape (*far left*) is a fin for stability under tow; the turret-shaped structure is a compression chamber for emergency escape to the surface. The major task accomplished by the divers was the installation and repair of an oil-well head (*right*). They were able to manipulate repair tools to handle emergency breakdown situations met in actual production. In the photograph a diver is guiding a tool pipe into the wellhead.



DEFLATED SPID is hoisted over the side of Link's vessel *Sea Diver*. One end of the SDC is visible in the left foreground, with part of the deck decompression chamber beyond it.

reached 30 feet. Still, we had one period of concern about decompression sickness. At about 20 feet Sténuit reported a vague "sawdust feeling" in his fingers that seemed to progress to the wrists. I examined him under pressure in the chamber. There were no abnormal neurological findings, but decompression sickness is so diverse in its manifestations that almost any symptom has to be taken seriously. Dickson and I therefore recompressed the chamber one atmosphere and then resumed decompression at the slower rate of four feet per hour. Finally, at noon on July 6, Sténuit and Lindbergh emerged from the chamber in excellent condition after 92 hours of decompression. The important point about saturation diving is that their decompression time would have been the same if they had stayed down 49 days instead of 49 hours.

Their dive had shown that men could live and work effectively more than 400 feet below the surface for a substantial period, protected by an almost autonomous undersea dwelling, and be successfully recovered from such depths and decompressed on the surface at sea. More specifically, it demonstrated the flexibility and mobility of the three-chamber concept. It also emphasized some problems, including the voice distortion caused by helium and the need for a larger breathing-gas supply to support muscular exertion. It showed that the control of humidity in an atmosphere in direct contact with the sea is extraordinarily difficult. The relative humidity in the chamber was close to 100 percent and both divers complained of softened skin and rashes. Temperature was a problem too. Both men preferred having the chamber temperature between 82 and 85 degrees. In the water, we realized, heated suits are required to keep divers comfortable even in the Caribbean Sea.

There have been a number of other recent saturation diving experiments, two of them conducted by Bond's Navy group. The first, "Sealab I," took place off Bermuda later in July, 1964. Four men lived for 10 days in a large cylindrical chamber 192 feet below the surface. Last summer the Navy conducted "Sealab II," a massive 45-day effort involving three teams of 10 men, each of which spent 15 days underwater. (One man went down for two nonconsecutive 15-day periods and one, the astronaut Scott Carpenter, stayed for 30 consecutive days.) The base of operations was a cabin 57 by 12 feet in size submerged in 205 feet of water near the Scripps In-

not move quite enough of it to meet the divers' maximum respiratory demand.

During the second evening we carried out and recorded voice-communication tests with the divers breathing either pure air or a mixture of 75 percent air and 25 percent helium. Voice quality was considerably better than in a helium atmosphere, but even 30 seconds of breathing air caused a noticeable degree of nitrogen narcosis. At 11:00 P.M. the two men bedded down for the second night. They were disturbed from time to time by heavy thumps against the outside of the dwelling. It developed that large groupers, attracted by the small fishes that swarmed into the shaft of light spilling from the open port of the SPID, were charging the swarm and hitting the dark bulk of the dwelling.

The next day the divers measured the visibility in the remarkably clear water; they could see almost 150 feet in the horizontal plane and 200 feet vertically. Then they took more photographs and collected animal and plant specimens. At 1:30 P.M. on July 2 both men were back in the SDC with the hatches secured. At 2:20, after 49 hours on the deep shelf, the SDC began its ascent. The internal pressure was maintained at 432 feet; although the divers were being lifted toward the surface, they were not yet being decompressed. At 3:15 the dripping SDC was hoisted onto its cradle aboard the *Sea Diver*. Now the internal pressure was decreased to 400 feet to establish a one-atmosphere differential between the divers' tissues and the chamber environment and make it possible for helium to begin escaping effectively from their tissues. Then, at 4:00, the SDC was mated to the deck decompression chamber, which was also at a pressure of 400 feet. Sténuit and Lindbergh, transferred to the deck chamber and we began to advance them to surface pressure at the rate of five feet, or about .15 atmosphere, per hour. With the divers safe in their chamber another advantage of deck decompression became evident: mobility. While decompression proceeded the *Sea Diver* weighed anchor and steamed for Florida. By the time it moored in Miami on the afternoon of July 5 the pressure had been reduced to 35 feet.

During the shallow stages of decompression, breathing pure oxygen establishes a larger outward pressure gradient in the lung for the inert gas one is anxious to flush out of the diver's tissues and thus helps to prevent the bends. Since breathing pure oxygen under pressure for a sustained period can cause lung damage, Lambertsen had in the past suggested alternating between pure oxygen and compressed air. We instituted this interrupted oxygen-breathing schedule when the divers stitution of Oceanography at La Jolla, Calif. The Sealab "aquanauts" salvaged an airplane hulk, did biological and oceanographic research and conducted psychological and physiological tests. Electrically heated suits made it possible for them to work comfortably in the 55-degree water.

In the Mediterranean off Cap Ferrat, Cousteau's group last fall made another significant advance in underwater living. Six men lived in a spherical dwelling 330 feet below the surface for almost 22 days, linked to the surface only by an electrical and communications cable. Cousteau's "oceanauts" concentrated on difficult underwater work, including the successful emplacement and operation at 370 feet of a five-ton oil-well head in which oil under pressure was simulated by compressed air.

As men go deeper and stay longer the hazards increase and safety margins narrow. New questions arise. At what depth will even helium become too narcotic or too dense to breathe? Can hydrogen serve as an acceptable substitute? At what depth will pressure effects cause unacceptable changes in tissue structure? What will be the decompression obligation after saturation at 1,000 feet or more? And what are the residual effects of repeated exposure to great depths?

Again, the answers are beginning to come from dry-land experimentation. Last fall two Ocean Systems divers simulated a dive to 650 feet in our test chamber. They stayed at that pressure for 48 hours, becoming completely saturated with 20 atmospheres of helium. Our results indicated that helium is safe—at least at the depth and for the length of time involved in the test—and suggested that it may be possible to continue with helium as the inert gas even beyond 1,000 feet. We found that breathing an oxygen-neon mixture for 30 minutes at 650 feet caused no measurable narcotic or other detrimental effects and that it markedly improved voice quality. Heart and lung function, exercise tolerance, psychomotor performance and blood and urine characteristics were all within normal limits. I think the most significant result of this longest deep-pressure experiment to date was our impression that divers will be able to perform physical and mental work almost as effectively at 650 feet as at the surface.

There do not, then, seem to be any physiological or psychological barriers that will prevent the occupation of any part of the continental shelf. Nonetheless, it is important to recognize that so far all efforts to live under the sea have been investigations or demonstrations of man's ability to do so. In the last analysis men will live underwater only when specific tasks, with economic or other motivations, present themselves. At this point, however, the gates of the deep shelf have been opened.

"SEALAB II" CHAMBER housed 10 Navy divers at a time during a 45-day test at 205 feet last summer. The chamber is a 57-by-12-foot cylinder with several interior compartments. Entry is through the port at the lower left, which is protected by a wire shark barrier.

THE FOOTPRINTS OF TUMOR VIRUSES

Some viruses that cause tumors in animals are not found in the tumors but their antigens are. This suggests that the absence of a virus in a human cancer does not necessarily mean that a virus did not cause it

by Fred Rapp and Joseph L. Melnick

The viruses that cause cancers, like viruses in general, can be divided into two broad classes: those that contain ribonucleic acid (RNA) as their genetic material and those that contain deoxyribonucleic acid (DNA). The two classes exhibit a curious difference in behavior. The RNA-containing viruses can readily be found in the tumors to which they give rise; in tumors induced by DNA-containing viruses, on the other hand, the virus itself disappears. Because the DNA viruses cannot be recovered from tumors for study, most of the investigations of virus-induced cancer in animals have concentrated on RNA viruses, specifically those that cause leukemia. A recent discovery, however, has opened a way to investigation of tumor-causing viruses of the DNA class. It was ascertained that, although the viruses themselves cannot be found in the tumors, they leave footprints, in the form of antigens, that identify them. This finding has made possible a broad study of the transformation of normal cells into tumorous ones by DNA-containing viruses.

In our laboratory at the Baylor University College of Medicine we have been studying two types of DNA-containing virus: the adenoviruses (responsible for colds and other respiratory diseases) and the papovavirus of monkeys known as SV 40 (simian virus 40). The two types of virus differ from each other in many respects, but they are alike in two significant properties: they often generate latent infections that produce no sign of illness, and they can produce cancer when inoculated into newborn hamsters (and in the case of some of the adenoviruses, in newborn mice as well).

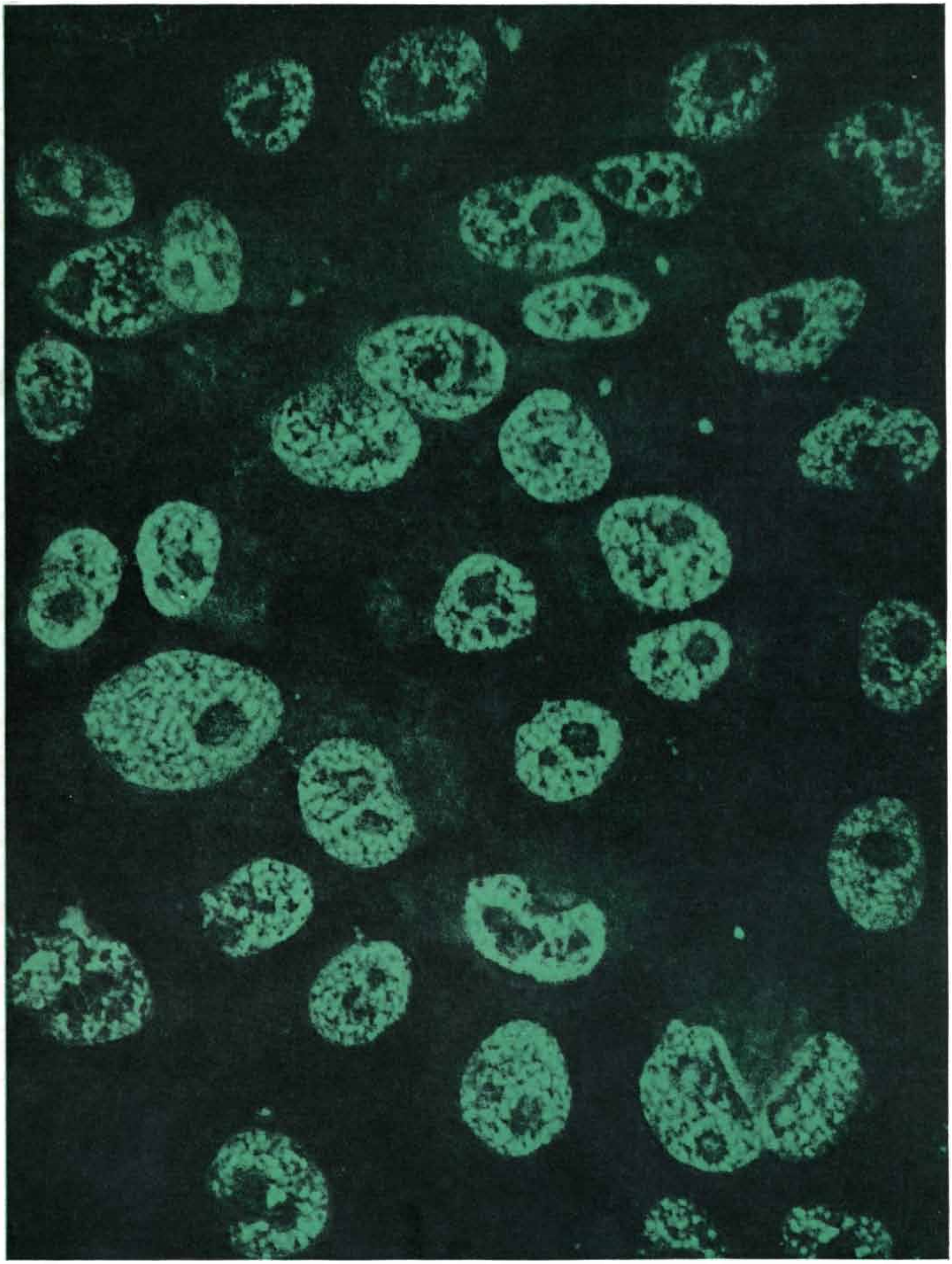
Adenoviruses were first recognized and isolated in 1953 in cell cultures derived from the adenoids of infected children. Since then more than 30 antigenic types of this virus have been found in man and other types in monkeys, cattle, dogs, mice and chickens. At least five types are now known to be capable of producing tumors in newborn hamsters. The simian virus was discovered in cultures of monkey kidney cells that were used to prepare vaccines against poliomyelitis and other virus diseases, including those caused by adenoviruses. There is no evidence that SV 40 produces any disease in man; nevertheless, after its discovery all virus vaccines were carefully tested to make sure that this contaminant was excluded, and experimental studies were undertaken to examine the virus's fundamental properties and effects on various cells. These studies quickly showed that in cell cultures SV 40 was capable of transforming normal cells into abnormal forms; it could produce these inheritable changes not only in hamster cells but also in human cells. The transformed hamster cells proved to be capable of producing tumors when they were implanted in young adult hamsters.

Grown in a glass dish, the cells transformed by the simian virus multiply into colonies that look like molds to the unaided eye. Under the microscope they show various abnormalities, such as aberrations in the chromosomes (breaks, translocations and deletions) and a three-dimensional piling up of the cells [*see top illustration on page 41*]. This last marks them as abnormal because normal cells grown in a dish are inhibited from climbing over one another and hence form only a single sheet of cells.

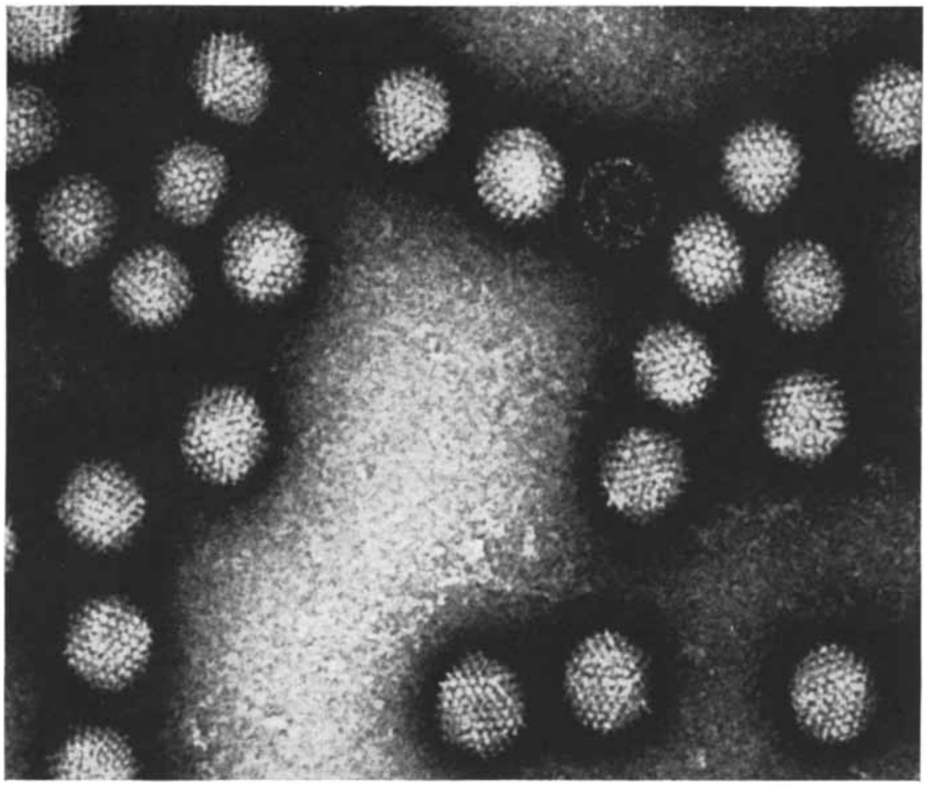
No virus could be found in the tumors arising from cells transformed by either the adenoviruses or SV 40. In the adenotumors particles with some resemblances to the virus were found, but the particles were plainly not complete viruses. The SV 40 tumors yielded no such particles at all. What, then, could account for the permanently changed behavior of the transformed cells, persisting in generation after generation of these cells? Did the virus endow the cells with a portion of its set of genes—a portion that, although not sufficient for reproduction of the virus itself, imposed a control over certain properties of the cell? If so, could evidence of the presence of these genes—footprints of the virus, so to speak—be found among the chemical products of the transformed cell? Did the transformed cell, for instance, produce a substance that was not produced by normal cells and that could be linked to the virus?

The first indication that such footprints might indeed be found was reported by Robert J. Huebner and his colleagues at the National Institutes of Health. They applied the well-known complement-fixation test for detecting antigen-antibody reactions to the serum of hamsters bearing tumors that had been initiated by an adenovirus, and they observed that the animals produced specific antibodies that reacted with antigens obtained from the tumor. Huebner, and Albert B. Sabin of the University of Cincinnati College of Medicine as well, went on to apply the same test to hamsters with tumors induced by SV 40, with the same result: these animals synthesized antibodies capable of reacting with a specific antigen derived from the tumor extracts containing the simian virus but not with the virus itself. The antigen was therefore called a "tumor" antigen.

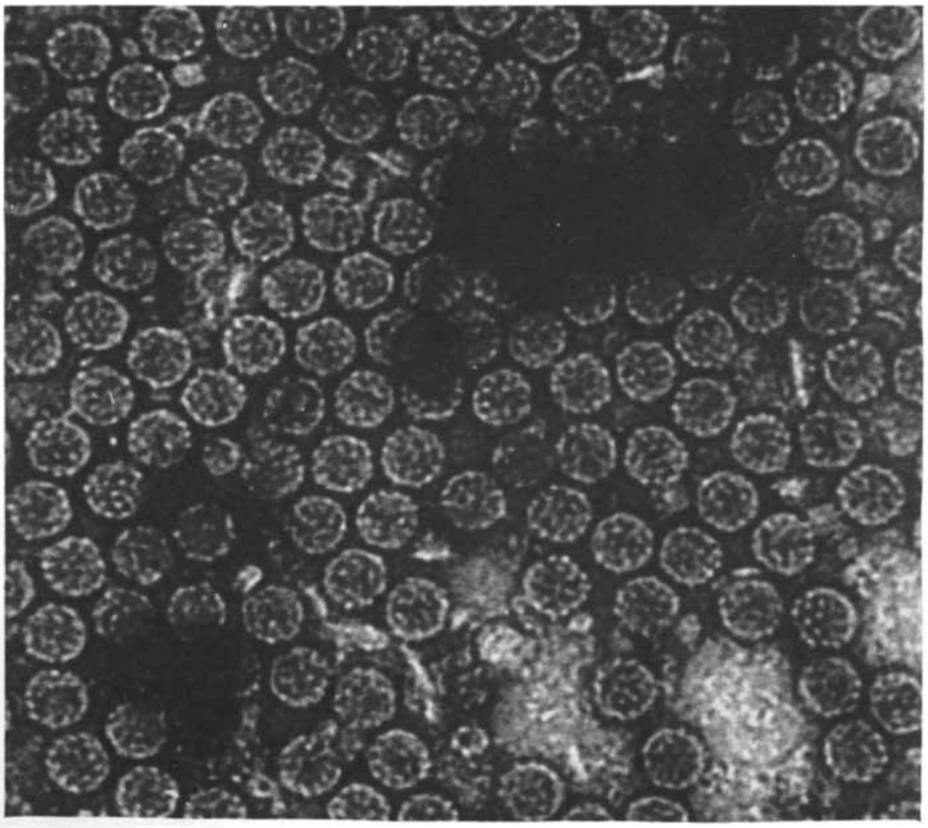
Other evidence for the existence of virus-induced cellular antigens emerged from studies in our laboratory. If hamsters were first inoculated with the SV 40 virus, they later rejected implants of SV 40 tumor cells. Evidently infection with



TUMOR-VIRUS ANTIGEN appears as the luminous granular green areas in this photomicrograph of hamster cells made by the technique of immunofluorescence. The cells had originally been infected with the tumor virus SV 40 (simian virus 40), which then disappeared. Later the cells were treated with antibody to the SV 40 virus; this antibody had been labeled with the fluorescent dye fluorescein isothiocyanate. When the cells were illuminated with ultraviolet radiation, the dye fluoresced, revealing the presence of the antibody. This indicated that, even though the virus itself was absent, it had left behind antigen that could react with antibody.



ADENOVIRUSES appear in this electron micrograph. Each has an actual diameter of about 70 millimicrons and a capsid, or outer protein coat, with 252 subunits. These viruses and those in the electron micrograph at the bottom of this page contain DNA as their genetic material. Such viruses disappear from the tumors they cause, unlike tumor-causing RNA viruses, and so have been difficult to investigate as tumor-causing agents. The discovery that some DNA viruses leave "footprints" in the form of antigen facilitates the study of them.



SV 40 VIRUSES, which are a type of papovavirus, also contain DNA. Each virus is about 45 millimicrons in diameter and has a capsid with either 42 or 72 subunits. These and various adenoviruses figured in the investigation by the authors of tumor-causing DNA viruses.

the virus caused the animals to synthesize an antigen eliciting the synthesis of antibodies that in turn attacked any graft of cells containing the same antigen; in short, the viral-induced antigen, which we call transplantation antigen, acted as a vaccine, making the animal immune to inoculation with the tumor cells. Recent studies suggest that the transplantation antigen is synthesized on the surface of the transformed cells. The surface of such cells will react with serum from a hamster that is immune to inoculation with an SV 40 tumor but not with the serum of a hamster hospitable to the tumor. The surface antigen seems to be the transplantation antigen, which stimulates the production of antibodies directed against the tumor cell. Because of these antibodies such an animal will resist the transplantation of tumor cells [*see illustration on opposite page*]. Such antibodies are not present in susceptible animals in which the tumor is able to grow. Antibodies in the tumor-bearing animals are able to react with the complement-fixing tumor antigen but not with the surface antigen.

There was every evidence that the tumor antigen was a new creation—a substance not present in the virus itself or in the original cell but one that the virus caused the transformed cells to manufacture. What could be learned about this antigen? Using serum from hamsters with SV 40 tumors and applying the immunofluorescence technique to observe reactions between the serum and the cells, Janet S. Butel of our laboratory found that every hamster cell transformed by SV 40 (either in vivo or in vitro) synthesized the tumor antigen, or *T* antigen, as it came to be called. The same antigen was also found in cells of other animals (including man) that were transformed by SV 40, but not in cells transformed by other viruses. The antigen was located in small particles within the nucleus of the cell. What was more, it showed up in the nuclei that had been newly formed as the cell divided. Obviously, therefore, the parent cell passed on information for the synthesis of this antigen to its daughter cells.

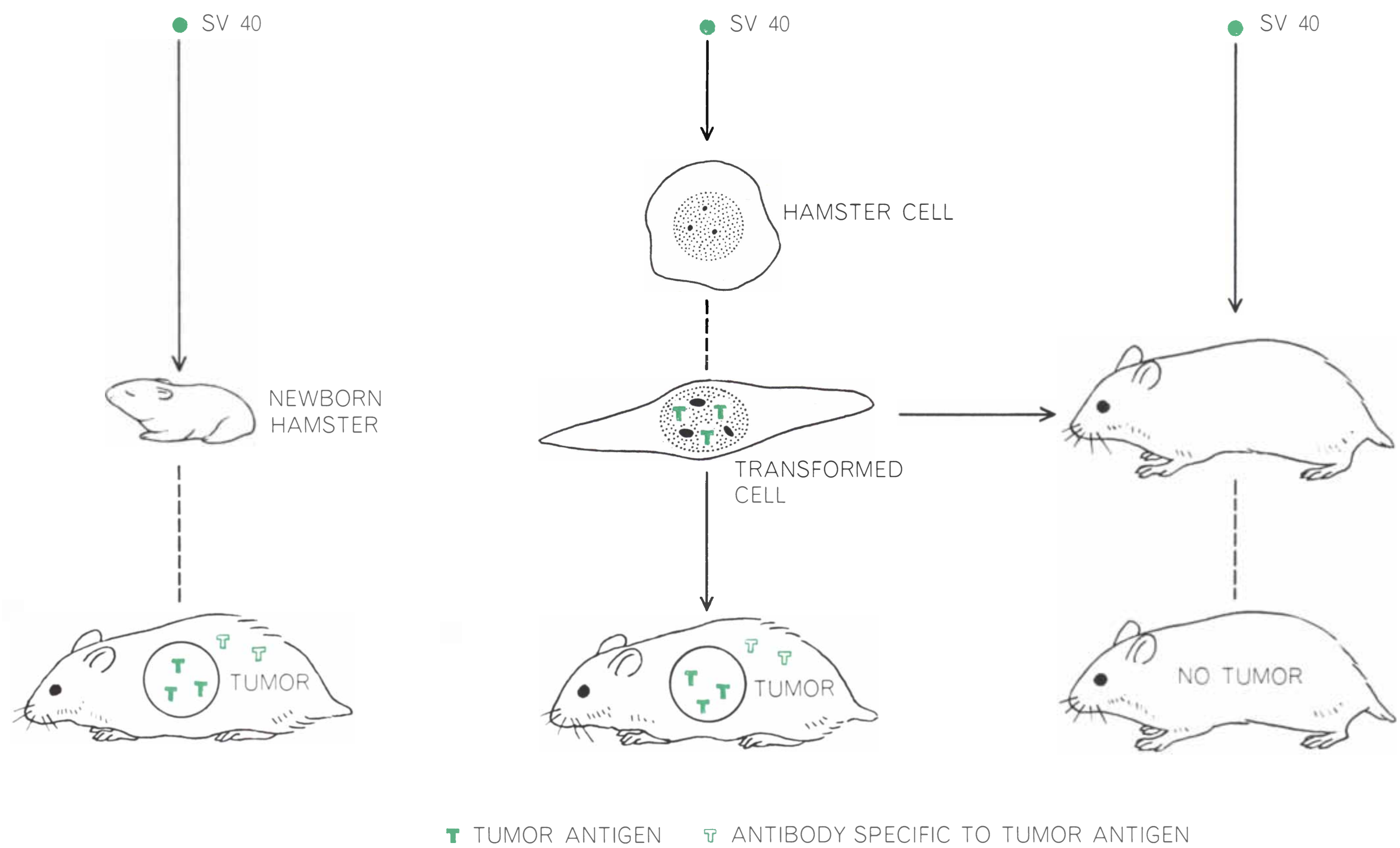
The fact that transformed cells of various species of animals synthesized the same antigen clearly indicated that this synthesis must be controlled by genes from the virus, incorporated in the transformed cells. This conclusion was strengthened by a discovery that directly linked the virus itself to the synthesis of the antigen. It was found, by investigators in Huebner's and Sabin's laboratories and in ours, that cells infected with SV 40 (but not transformed) synthesized the same antigen that transformed cells did. They produced the antigen during the early phases of the reproduction of the virus in the cell. This antigen production, not previously observed, was now found to occur in cells of monkeys, hamsters, rabbits and man when the cells were inoculated with SV 40 and then tested with serum from hamsters containing the specific antibody.

When the SV 40 virus invades a monkey kidney cell, it takes about 30 to 36 hours to produce a new crop of virus progeny. The protein coats of the new viruses are not synthesized until near the end of that period. Long before that happens—as early as 18 hours after the infection—the cell begins to produce the tumor antigen. We soon learned that poisons that inhibit the synthesis of the virus's DNA do not inhibit the production of the tumor antigen. This indicated that replication of the viral DNA is not necessary for the synthesis of the antigen. We found, however, that the synthesis does depend on messenger RNA in the cell, because production of the antigen could be suppressed by the administration of the antibiotic actinomycin D, which blocks the formation of messenger RNA [*see illustration on next page*].

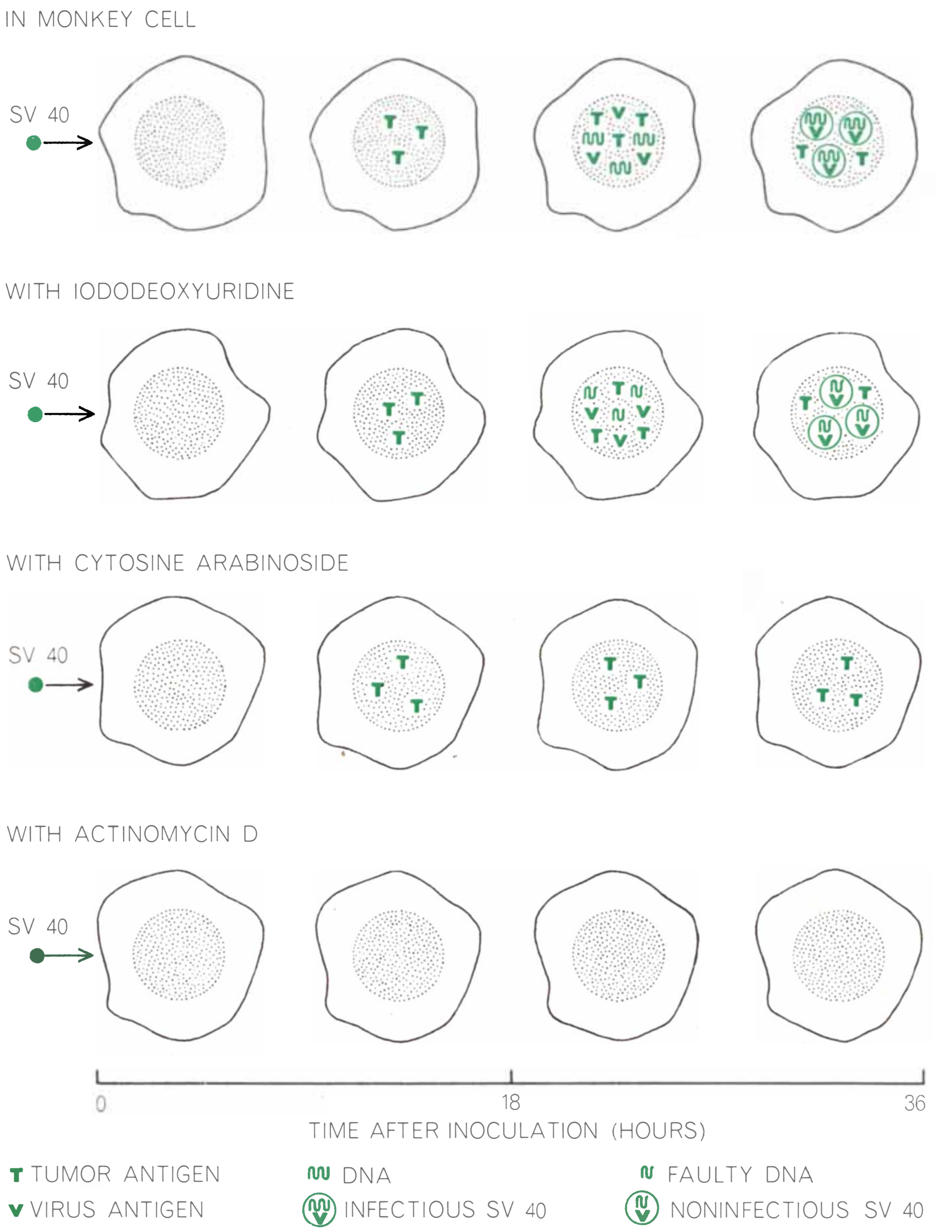
Experiments with adenoviruses, carried out in a number of laboratories, have yielded results similar to those with SV 40. We can conclude in general that cells transformed to malignancy by DNA-containing viruses have as one of their distinctive properties the production of a specific antigen in the nucleus. The nature of this antigen is not yet known. There are indications that it may be an enzyme; similarities have been found between the induction of the tumor antigen and a step-up in the production of thymidine kinase, one of the enzymes involved in the synthesis of DNA, during the early stages of the replication of SV 40 in monkey cells. It seems from the work of Saul Kit at the Baylor College of Medicine that the thymidine kinase produced by infected cells is somewhat different from that in normal cells. No precise information has been obtained so far, however, on what relation, if any, exists between the tumor antigen and thymidine kinase.

The tumor antigen, then, furnished a clear and definite link connecting the viruses in question with cancers. As events turned out, the antigen also became something more than that: it proved to be a marker capable of providing information about the genetic transformation of cells. Within a few months after the discovery of the SV 40 tumor antigen a genetic relation between SV 40 and certain adenoviruses was discovered, both in our laboratory and at the National Institutes of Health. It developed that these adenoviruses could cause cells to produce precisely the same antigen the SV 40 virus did!

The first adenovirus involved was one called Type 7, which was used as the seed for the preparation of an adenovirus vaccine. It had been found that during the culture of this virus, involving many passages in monkey kidney cells, the strain became contaminated with SV 40. The cultures in which it was growing were therefore treated with a specific antiserum to eliminate SV 40. The treatment succeeded in yielding a culture that was free of any SV 40 capable of producing infection;



SECOND ANTIGEN became evident in a series of experiments. Newborn hamsters inoculated with SV 40 (*left*) often developed tumors and a tumor antigen. Similarly, hamster cells grown in culture and transformed by SV 40 (given permanent inheritable changes) often induced tumors when transplanted to young adult hamsters (*center*). However, hamsters to which transformed cells were transplanted after inoculation of the animals with SV 40 resisted the tumor-causing effect of the transformed cells (*right*). The virus acts as a vaccine: it induces an antigen, evidently the same as the "transplantation antigen" carried by the transformed cells.



DEVELOPMENT OF SV 40 VIRUS under various conditions is depicted. Injected into a monkey cell, it replicates in about 36 hours. Tumor antigen and viral DNA appear earlier. The use of inhibitors shows the sequence of events. For example, the synthesis of tumor antigen does not require new viral DNA, whose formation is blocked by cytosine arabinoside.

yet, when the decontaminated Type 7 adenovirus culture was tested by inoculation into newborn hamsters, it induced tumors. When added to cells in culture, the virus caused them to produce the SV 40 tumor antigen. Apparently the adenovirus was now carrying some genetic determinants that had belonged to SV 40. Had the two viruses combined their genetic material and formed a hybrid virus?

Further experiments showed that a serum against SV 40 or against its tumor antigen did not prevent induction of the antigen by the "hybrid" virus, but a serum against the adenovirus did block synthesis of the antigen [*see illustration on opposite page*]. These results strongly suggested that SV 40 genetic material responsible for the antigen synthesis was encased in a protein coat of Type 7 adenovirus; the serum against Type 7 adenovirus would prevent the virus from entering the cells, since the protein coat is the agency by which a virus attaches to and invades cells. On the other hand, we were able to show that when the "hybrid" virus was allowed to gain entry into cells, the production of the antigen (and of thymidine kinase) followed the pattern exhibited by cells inoculated with SV 40.

The picture became complicated, however, when the effect on human cells was examined. We purified the "hybrid" virus both in monkey kidney cells and in human kidney cells (from embryos) by growing clones, or colonies, from single cells containing the virus. It turned out that in the monkey cells the virus progeny induced synthesis of the SV 40 tumor antigen; in the human cells they did not. Was the viral particle that carried the antigen determinant unable to reproduce itself in human cells? Was the "hybrid" population of viruses perhaps made up of two separate particles, one capable of replicating itself in human cells and the other (carrying the antigen determinant) reproducing only in monkey cells? Or might some combination of these ideas explain the results? These hypotheses were tested experimentally in our laboratory.

The reproduction of viruses in a cell culture can be measured by counting the number of "plaques," or areas of cell destruction, each representing a focus of infection, that are produced in the culture by inoculation with a given dose of the virus. If only one virus particle is required to generate reproduction of the virus (and hence formation of a plaque), then dilution of the inoculated dose should result in a directly proportional reduction in the number of plaques. For example, if the inoculation is diluted tenfold, only a tenth as many plaques should be produced. If two virus particles are required to infect a cell and initiate a plaque, the reduction in plaques should be proportional to the square of the dilution: a tenfold dilution of the dose should result in reducing the number of plaques to a hundredth, instead of a tenth, of the former number.

When this test was applied to the "hybrid" combination of Type 7 adenovirus and SV 40, the results indicated that in human cells the required ratio was one virus particle for one plaque; in monkey cells, however, two particles seemed to be necessary to initiate a plaque. What was missing in the latter case? If single virus particles were sufficient for infection and replication in the human cells, why not in the monkey cells? It was clear that the population of virus progeny generated in the human cells was not the same as that produced in the monkey cells; the purified virus from the human cells no longer contained SV 40 determinants, and when it was inoculated alone into monkey cells, it produced no plaques. In all likelihood, however, this form of the virus (let us for the moment call it Virus *A*, for adenovirus) was one of the two virus particles required for infection and virus replication in the monkey cells. Perhaps the other particle carrying the SV 40 genes was not produced in human cells. If that was the case, we should find that the production of plaques in cultures of monkey cells could be greatly increased by adding purified Virus *A* to the inoculation of "hybrid" virus into the monkey cells.

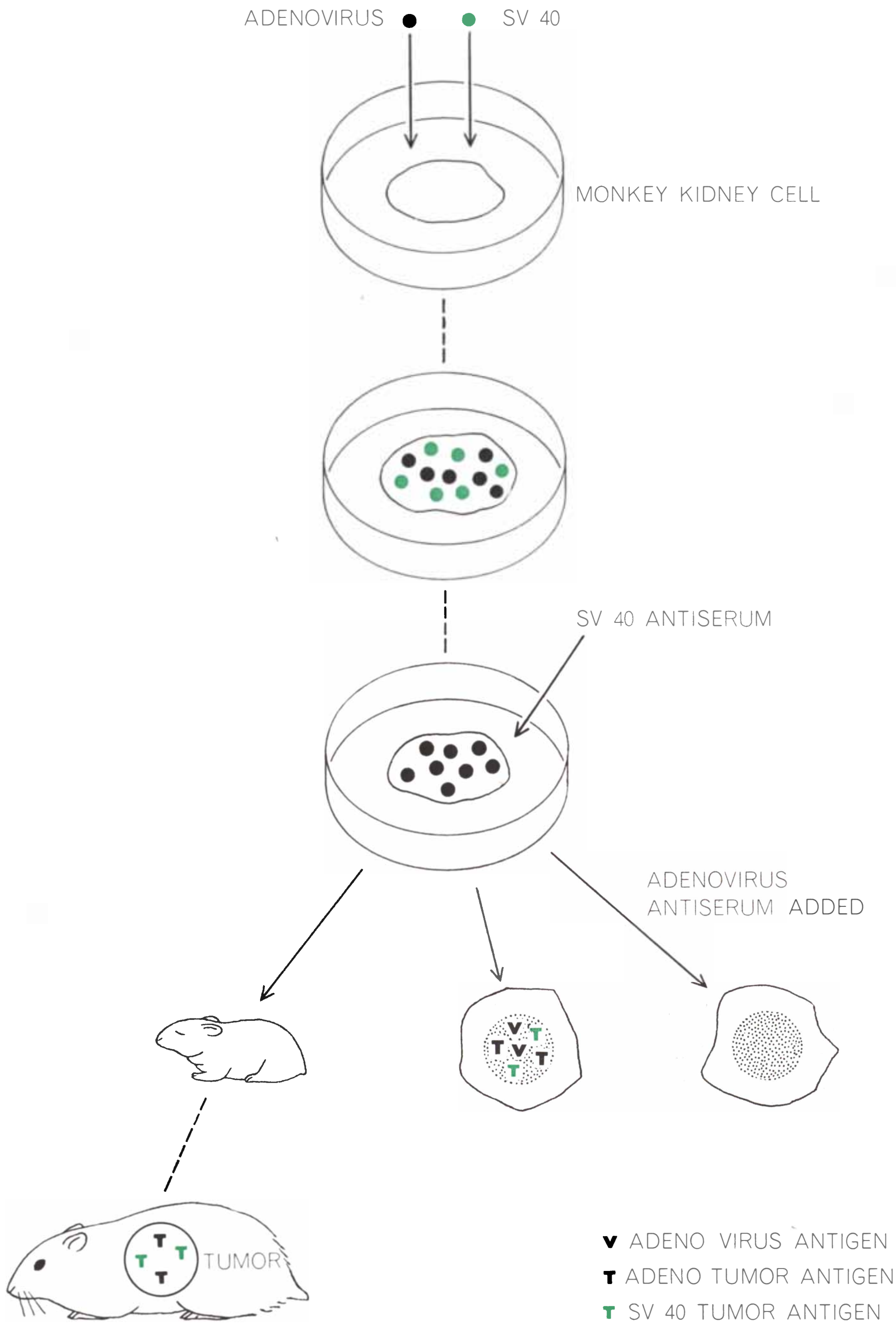
We performed that test. Monkey cells were first saturated with a concentration of Virus *A* (obtained from human-cell cultures) that was sufficient to infect every cell in the culture; then the cells were inoculated with the "hybrid" virus in various dilutions. The results bore out the hypothesis. In these cultures the number of plaques produced was more than 100 times greater than that in comparable cultures without the Virus *A* booster.

Taking all the findings together, we were able to conclude that the hybrid population of virus produced in monkey cells does indeed consist of two distinct viruses. One (the particle that replicates by itself in human cells) is an adenovirus. The other (carrying the SV 40 genetic material that induces the tumor antigen) we have named PARA, for "particle aiding replication of adenovirus." In monkey cells the two viruses are mutually dependent. Only by combined action can they produce the plaques that signal reproduction of these viruses. We confirmed by further experiments that the PARA virus cannot replicate itself without the help of the adenovirus, and that the adenovirus requires the aid of either PARA or SV 40 to multiply in monkey cells.

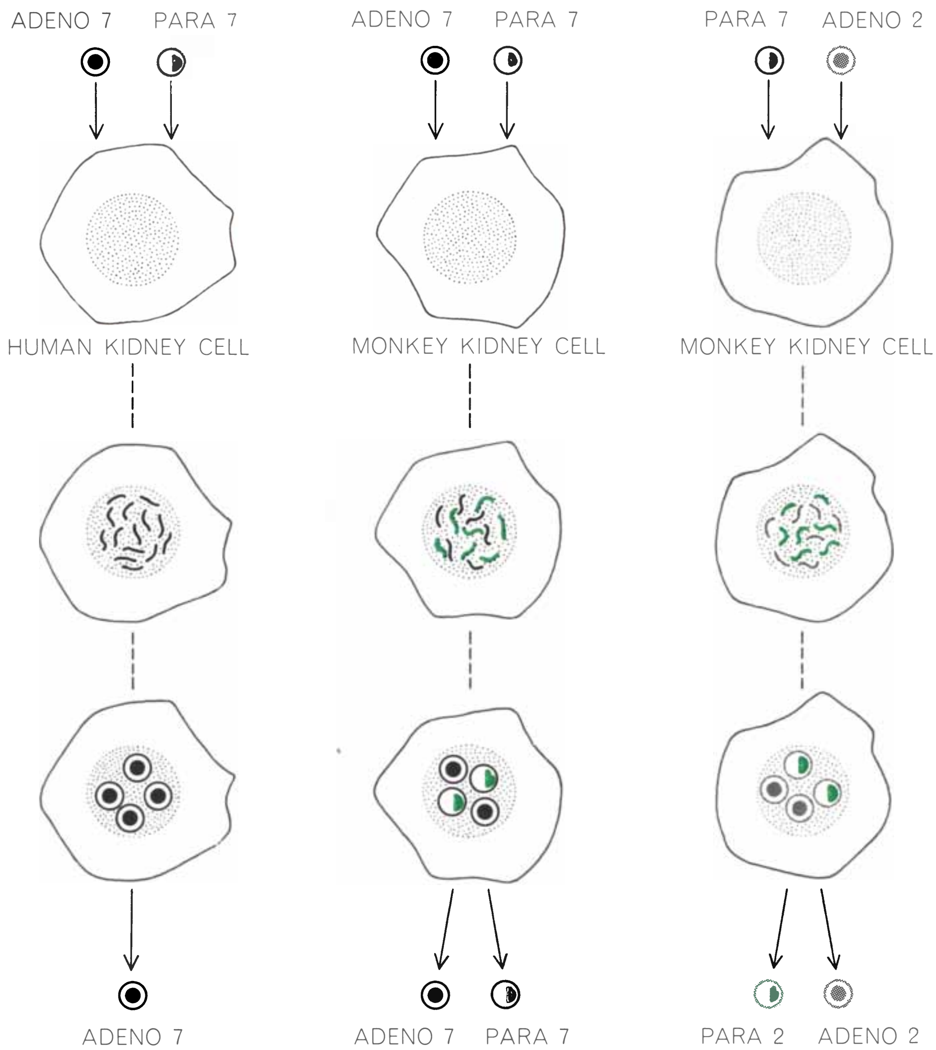
A series of highly interesting observations followed from this germinal finding. It developed that at least five other types of adenovirus in addition to Type 7 could act as helpers for the replication of PARA. In each case the progeny carried the specific protein coat of the adenovirus that had taken part in the reproduction of the PARA virus. The change in the coat of PARA from one type of adenovirus to another is now called transcapsidation, and it is of major significance for the oncogenic, or tumor-causing, potential of viruses. By means of the transcapsidation reaction a nononcogenic population of viruses can be converted into an oncogenic one. For example, an oncogenic PARA virus with a Type 7 coat can be made to undergo a transcapsidation reaction with a nononcogenic Type 2 adenovirus to yield a PARA virus with a Type 2 coat, and this virus is now oncogenic [*see top illustration on next page*]. The oncogenicity of the original PARA virus could be blocked only by Type 7 adeno-antiserum; that of the new PARA virus can be blocked only by Type 2 adeno-antiserum.

All the experimental evidence now points clearly to the conclusion that we are dealing with a partnership in which PARA supplies information that enables the adenovirus to reproduce and the adenovirus supplies information for the synthesis of the protein coat that endows PARA with the power of infection. Since PARA evidently plays a key role in the transformation of cells to malignancy, investigators are naturally eager to learn more about the nucleic acid—the genetic constitution—of this virus. Efforts to isolate PARA (that is, separate it from the adenovirus in the hybrid population) have been unsuccessful so far, but some pieces of genetic information have emerged from recent experiments in our laboratory. It appears that the PARA virus contains determinants not only for the tumor antigen but also for other antigens (mentioned earlier) that are found on the surface of transformed cells. Presumably for this reason hamsters that have been inoculated with the "hybrid" virus show an immune reaction to attempted implants of the cells that have been transformed by SV 40 virus.

The findings reviewed in this article



GENETIC RELATION between an adenovirus and SV 40 appeared when a strain of adenovirus being cultured by repeated passages through monkey kidney cells became contaminated with SV 40. Addition of an antiserum designed to eliminate SV 40 yielded a culture that had no SV 40 able to cause infection; yet on injection into newborn hamsters the purified adenovirus caused tumors; added to hamster cells in culture, it induced SV 40 tumor antigen. Evidently the adenovirus had acquired some of SV 40's genetic determinants.



MUTUAL DEPENDENCE of two viruses was demonstrated after it became apparent that a purified adenovirus carried SV 40 genetic determinants. In human kidney cells (*left*) this presumed "hybrid" did not reproduce itself; only adenovirus was reproduced. In monkey kidney cells (*center*) the adenovirus reproduced with SV 40 DNA (*color*). Evidently this requires two viruses: an adenovirus and PARA ("particle aiding replication of adenovirus"). The phenomenon occurs with other adenoviruses (*right*). The process involved is transcapsidation: the change in PARA's protein coat from one type of adenovirus to another.

	ADENOVIRUS	PARA	PARA-ADENO	SV 40 VIRUS
INDUCTION OF SV 40 TUMOR ANTIGEN			YES	YES
INDUCTION OF SV 40 VIRUS ANTIGEN				YES
INDUCTION OF ADENO 7 TUMOR ANTIGEN	YES		YES	
INDUCTION OF ADENO 7 VIRUS ANTIGEN	YES		YES	
REPLICATION IN MONKEY KIDNEY CELLS			YES	YES
REPLICATION IN HUMAN KIDNEY CELLS	YES		YES	SLIGHTLY

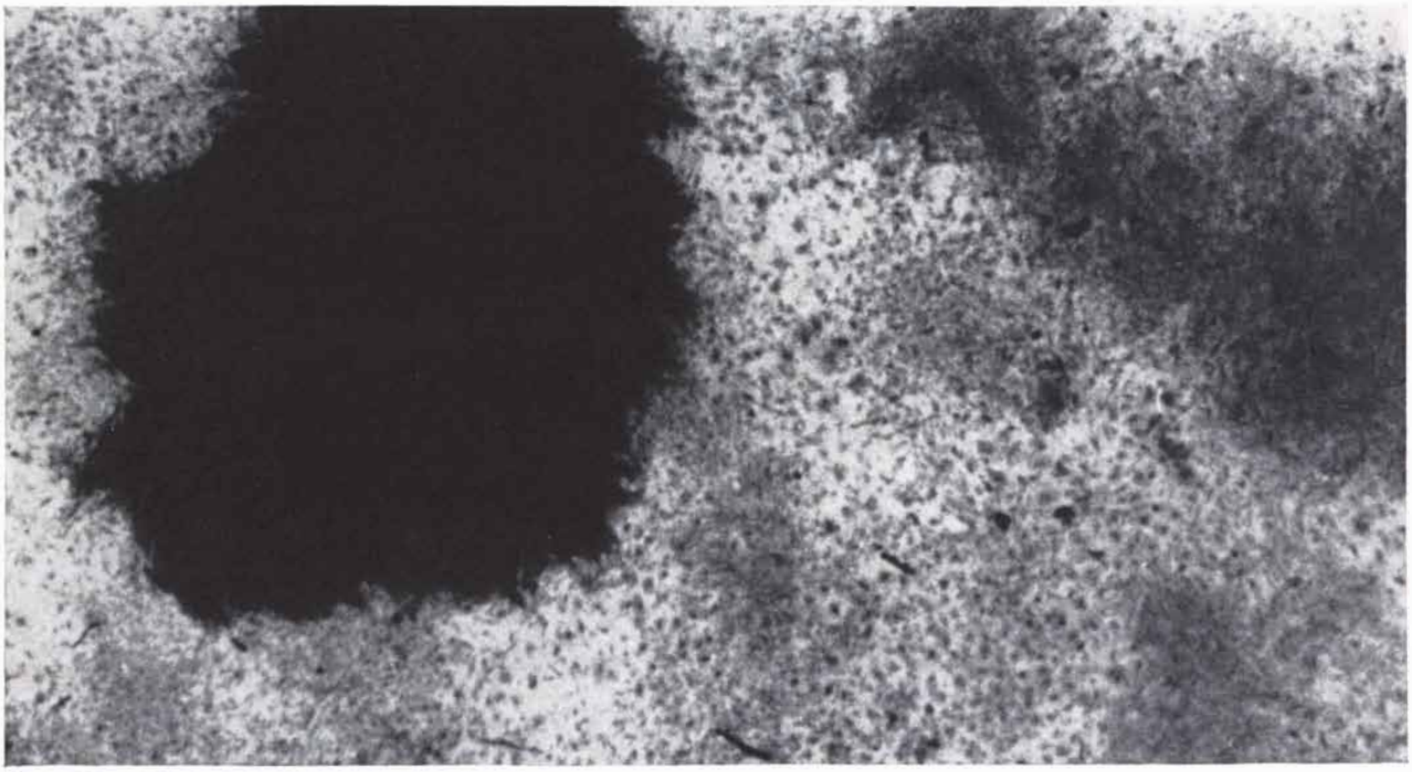
CHARACTERISTICS OF VIRUSES involved in such phenomena as transcapsidation and production of tumor antigen are summarized. A blank space means the event does not occur.

have raised many new questions and seem certain to change many ideas about the relation of viruses to cancer. For one thing, they suggest that the failure to find viruses in human cancers does not necessarily argue against the possibility of viruses being the cause of these cancers. It is apparent that DNA viruses, in the process of transforming cells, may surrender the power to reproduce themselves but leave in the cells a portion of their set of genes that will induce malignant growth or remain associated with such growth. Now that techniques have been developed for recognizing the footprints of such viruses (of the adenovirus and papovavirus SV 40 breeds) in the tumors of experimental animals such as the hamster, techniques may be found for detecting viral footprints in human tumors as well.

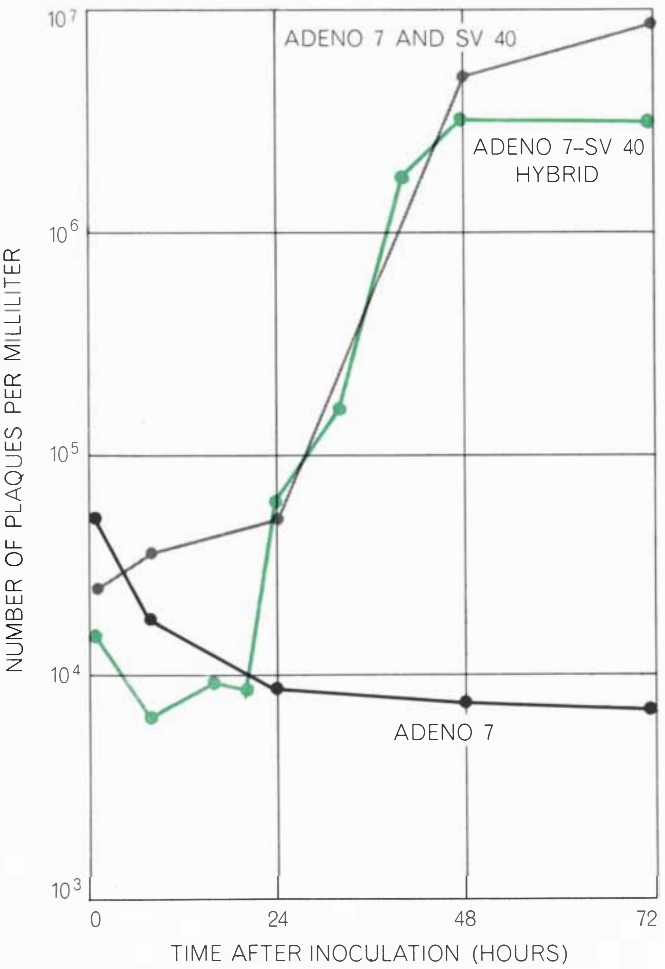
The adenovirus-PARA partnership is another surprising discovery. Other cases of mutual relations between viruses have been reported [see "A Defective Cancer Virus," by Harry Rubin; Scientific American, June, 1964], but they involve viruses that are mutants of each other or otherwise closely related. The adenovirus-PARA collaboration is the first that has been found between viruses of completely different genetic lineage.

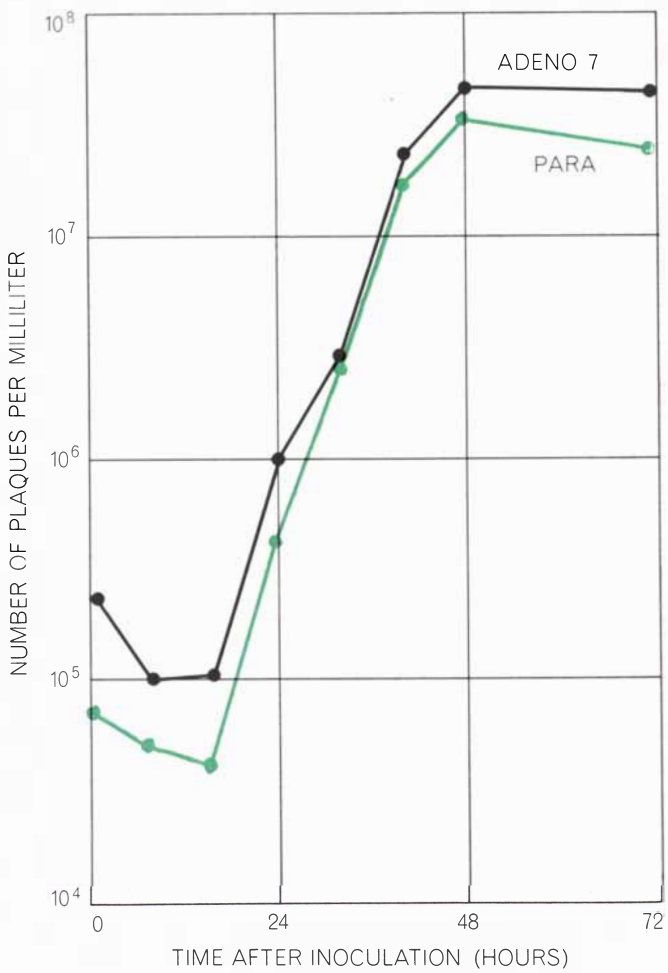
It is startling to find that one virus can usurp the protein coat of another and thereby endow a previously nononcogenic adenovirus with new powers—which can include the power to produce cancer. PARA is the result of such a transfer of viral constituents and is like a wolf in sheep's clothing. Particles like it may be much more widespread than we now suppose. PARA itself was discovered because it contains an identifying marker (induction of the tumor antigen); perhaps the footprints of other viruses of this kind will be discovered. It is conceivable that all cancer-causing virus populations of the class containing DNA include such particles.

If, as we have seen, viruses can carry pieces of genetic information donated by other viruses, perhaps they can also carry pieces of DNA picked up from the cells in which they are synthesized. The finding that animal viruses are not restricted to carrying genes for their own replication but can act as carriers of foreign genetic information—particularly determinants that transform cells to malignant growth—is an intriguing development that opens new perspectives in virology and cell pathology.



TRANSFORMATION OF CELLS by SV 40 is evident in the dark area of this photograph. It is a colony of hamster cells that are growing in a three-dimensional heap in a glass plate. In contrast, the surrounding normal cells grow in a two-dimensional sheet.





GROWTH RATES of the adenovirus and the PARA virus in the hybrid population are measured separately by means of plaque formation. A plaque is an area of cell destruction representing a focus of infection. The replication of adenovirus in the hybrid population (*left*) resembled that of adenovirus grown in the presence of SV 40; replication of PARA (*right*) was almost identical. These curves contributed to the evidence that the adenovirus did not reproduce in the absence of either PARA or added SV 40, and that PARA could not multiply without the helper adenovirus. Hence the curves showed the mutual dependence of adenovirus and PARA.

The Voyage of Mariner IV

In which numerous obstacles are overcome to put a 575-pound spacecraft within 6,118 miles of Mars after a trip of 228 days. The first of a three-part series on the mission and its results

by J. N. James

On October 1 of last year the mission of *Mariner IV*, the spacecraft that had taken the first closeup pictures of Mars two and a half months earlier, was formally ended. The termination of the successful voyage was initiated by a radio command that switched the spacecraft's transmitter from a high-gain antenna to an omnidirectional antenna. In a few days the radio beam of the high-gain antenna would have swept off the earth and the communication link to *Mariner IV* would have been broken. Switching the transmitter output to the omnidirectional antenna provided the most favorable chance of again receiving useful data from *Mariner IV* sometime in the future. "Sometime" will be in September, 1967, when *Mariner IV*, if it remains in operation, will approach to within 30 million miles of the earth.

Mariner IV responded perfectly to this final command, as it had to 79 other commands during its historic flight. When last heard from, it was transmitting good data on interplanetary fields and particles at a rate of 8⅓ "bits" per second from the unprecedented range of 191,060,000 miles from the earth—out beyond the orbit of Mars. *Mariner IV* had operated for 7,375 hours since its launching on November 28, 1964, and had traveled 418,749,000 miles on its first orbit around the sun.

At 3:05 P.M. on October 1 the signal from *Mariner IV* dropped below the threshold of sensitivity of the maser receiver at the Goldstone tracking station (near Barstow, Calif.) of the Deep Space Network. The final signal had a strength of only 10^{-18} (a billion-billionth) watt.

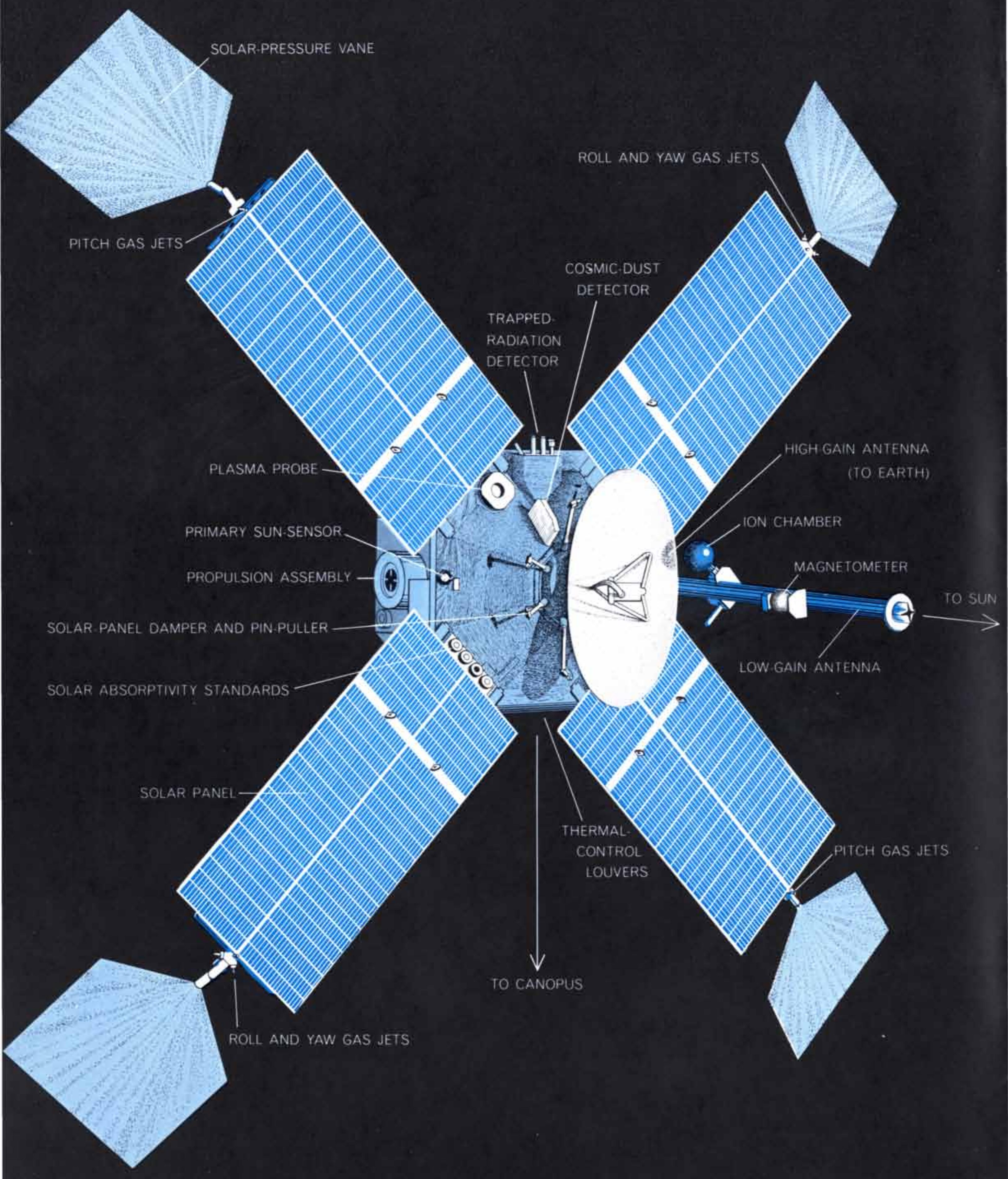
The members of the Mariner-project team who were gathered that afternoon in the Mission Operations Center at the Jet Propulsion Laboratory in Pasadena, Calif., represented only a handful of the thousands of people who had been involved in the project: members of the National Aeronautics and Space Administration, industrial contractors and workers from many academic institutions, including the California Institute of Technology, which established the Jet Propulsion Laboratory and now operates it for NASA. Looking back over the three years of intensive work that had gone into *Mariner IV*, one had to be impressed by the narrow margin of success of such a mission and by the difficulties that had had to be overcome.

In many ways *Mariner IV* resembled *Mariner II*, which had made the first successful mission to Venus in 1962 [see "The Voyage of Mariner II," by J. N. James; SCIENTIFIC AMERICAN, July, 1963]. Both were the result of "fall back" efforts that had to be quickly but carefully planned when projects based on a launch vehicle with an Atlas first stage and a liquid-hydrogen-fueled Centaur second stage had to be abandoned. The Atlas/Centaur vehicle could have sent a spacecraft weighing some 1,500 pounds to Mars. The payload-lifting performance of the substitute vehicle, which consisted of an Atlas first stage and an Agena second stage, was originally about 350 pounds for an interplanetary mission; this performance had to be increased by some 100 pounds to make possible the flight of *Mariner II* and by another 125 pounds to make possible the flight of *Mariner IV*. Both spacecraft were therefore severely limited in weight. Both were preceded by unsuccessful counterparts launched in the U.S.S.R. Both of the initial launchings—*Mariner I* to Venus and *Mariner III to* Mars—also ended in failure. Both of the successful craft developed roll-stabilization difficulties in flight that caused delays in conducting the mid-course maneuvers. In the end, however, both completed their mission successfully.

Because *Mariner IV* had the more difficult mission its design was necessarily more complex than that of *Mariner II*. *Mariner IV* had an allowable weight only 30 percent greater than that of *Mariner II* (575 pounds to 447), but it contained 2.6 times as many parts (138,000 to 54,000).

In November, 1962, after it had become clear that the Atlas/Centaur vehicle would not be ready for a Mars mission during the favorable opportunity presented in 1964, NASA authorized the Jet Propulsion Laboratory to proceed with a Mariner-class mission using an Atlas/Agena launch vehicle. Good opportunities for launching a spacecraft to Mars occur only at 25-month intervals. For the revised mission the Jet Propulsion Laboratory used an extension of the technology first developed for the Ranger lunar missions [see "The Ranger Missions to the Moon," by H. M. Schurmeier, R. L. Heacock and A. E. Wolfe; SCIENTIFIC AMERICAN, January]. Many of the personnel who were involved in the encounter of *Mariner II* with Venus on December 14, 1962, were working on the Mars project the next day. The responsibility of the Jet Propulsion

HISTORIC JOURNEY BEGINS with lift-off of *Mariner IV* from Pad 12 at Cape Kennedy at 9:22 A.M., November 28, 1964. The spacecraft, encased in a lightweight metal shroud, sits atop an Agena second-stage rocket, which in turn sits atop an Atlas first stage. The mission of *Mariner IV* was formally concluded 10 months later on October 1, 1965, after the spacecraft had traveled 418,749,000 miles on its first circumnavigation of the sun.


SOLAR-PRESSURE VANE
ROLL AND YAW GAS JETS
PITCH GAS JETS
COSMIC-DUST DETECTOR
TRAPPED-RADIATION DETECTOR
PLASMA PROBE
HIGH-GAIN ANTENNA (TO EARTH)
ION CHAMBER
PRIMARY SUN-SENSOR
MAGNETOMETER
PROPULSION ASSEMBLY
TO SUN
SOLAR-PANEL DAMPER AND PIN-PULLER
LOW-GAIN ANTENNA
SOLAR ABSORPTIVITY STANDARDS
SOLAR PANEL
THERMAL-CONTROL LOUVERS
PITCH GAS JETS
TO CANOPUS
ROLL AND YAW GAS JETS

Laboratory, as before, was to manage the entire project: to carry out and control the system design of the spacecraft and its operations, with the help of subcontractors who would fabricate the subsystems and assist in their design and testing.

Because of the large uncertainties involved, NASA authorized two launchings. Reliability studies showed that an extraordinary effort would be needed to extend the mean life expectancy of such a complicated spacecraft to the 6,000 hours required to reach Mars. To accomplish this it was deemed necessary to test and screen every part to be used in the spacecraft, to subject designs to "worst case" analysis, to build a complete set of spacecraft assemblies and require them to pass "type approval" tests over environmental extremes reasonably greater than anything one might expect them to experience in flight, to build and thoroughly test a complete "proof test" model spacecraft in simulated environments of space, and to maintain thorough records of, and take corrective action on, every failure or problem experienced in all tests and operations. Many of these actions had not been carried out on the Venus Mariners because of the limited time available for development. The Mars Mariners had these advantages, plus another important one. The Jet Propulsion Laboratory now had in operation a large test chamber in which the spacecraft could be subjected to high vacuum and extreme cold, together with a level of illumination that simulated solar radiation. With the aid of this facility we felt sure we could eliminate the temperature problems that had plagued the flight of *Mariner II*.

Three fully equipped flight spacecraft were prepared in order to ensure that two of them would be ready for launching in the limited period when the earth and Mars were in a suitable relationship. In spite of an intensive testing program, it was possible to accumulate a total of only 3,500 hours of testing on the three flight spacecraft and the proof-test model, compared with the 6,000 hours needed for the actual flight to Mars.

In order to achieve a mission with the Atlas/Agena vehicle, everyone concerned with weight and performance had to make sacrifices. The factor of weight was so critical that as little as three pounds added to the spacecraft would have reduced the launch period by one day, from 29 to 28 days. (The complete space vehicle, fueled for launching, weighed some 258,000 pounds.) The three completed spacecraft were all within five pounds of their specification weight.

The launch period is established by computations that involve the weight of the spacecraft, the performance of the launch vehicle and the trajectories selected [*see bottom illustration on next page*]. The trajectories are of two basic types, designated Type I and Type II. The first requires the spacecraft to travel less than halfway around the sun to reach its target; the second calls for a flight more than halfway around the sun. *Mariner IV* was launched on a Type I trajectory.

When it was evident that 29 days was the longest launch period that could be achieved, plans were made to reserve and modify two launch pads at Cape Kennedy—Pad 12 and Pad 13—and to prepare *Mariner III* and *Mariner IV* simultaneously. The launch period was too short to allow sequential preparation and launch from a single pad. After two years of intensive schedule discipline *Mariner III* was sent aloft on November 5, 1964, one day late.

The Mars Mariners were designed to conduct eight experiments. Each spacecraft was equipped with (1) a television camera, (2) a magnetometer to measure interplanetary magnetic fields and to determine whether or not Mars has a magnetic field, (3) an ion chamber to measure the density and energy of cosmic rays, (4) a trapped-radiation detector to measure cosmic rays and electrons and to search for Martian radiation belts, (5) a cosmic ray telescope to measure cosmic ray protons and alpha particles, their energy levels and direction of motion, (6) a cosmic-dust detector and (7) a plasma probe to measure the low-energy protons that stream outward from the sun as the "solar wind."

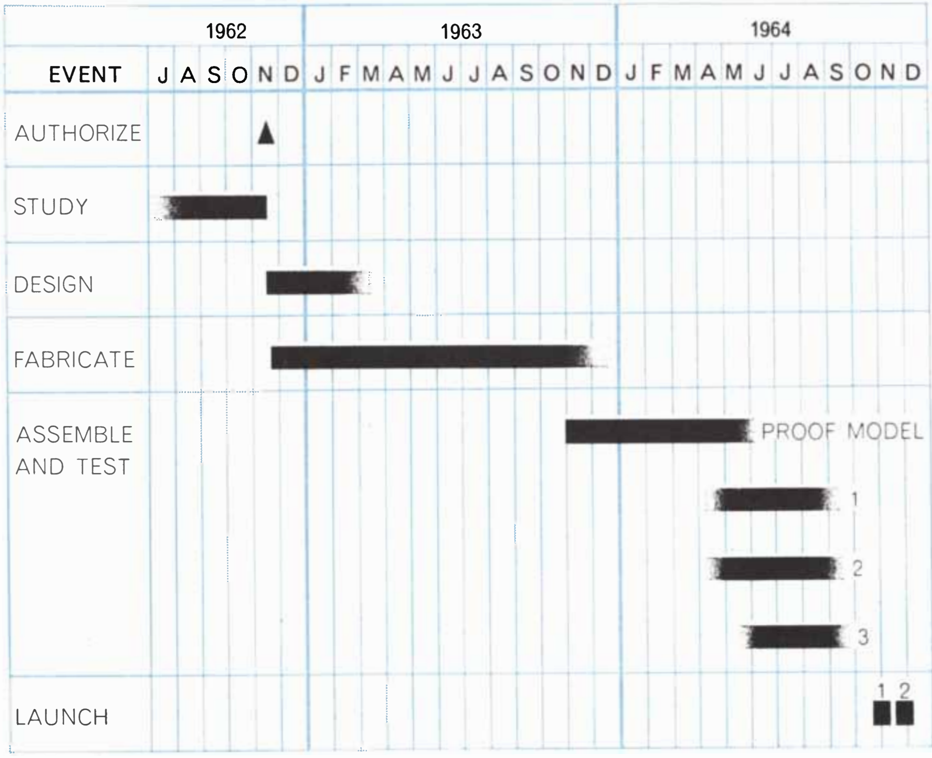
The eighth experiment—the occultation experiment—involved no special equipment on the spacecraft. It required, however, a trajectory precisely selected so that the spacecraft would disappear behind Mars for a brief period (slightly less than an hour). As the Mariner disappeared from radio view and emerged from behind the planet, its radio signals would have to pass through the Martian atmosphere. Depending on its density and composition, the atmosphere would change the apparent frequency, phase and amplitude of the signal.

One of the most complicated devices on board was the "data-automation subsystem," which controlled and synchronized the seven instruments, converted their various outputs into a common digital form and rate and provided these data to the communication subsystem. This device alone accounted for a third of the electronic parts on each Mariner.

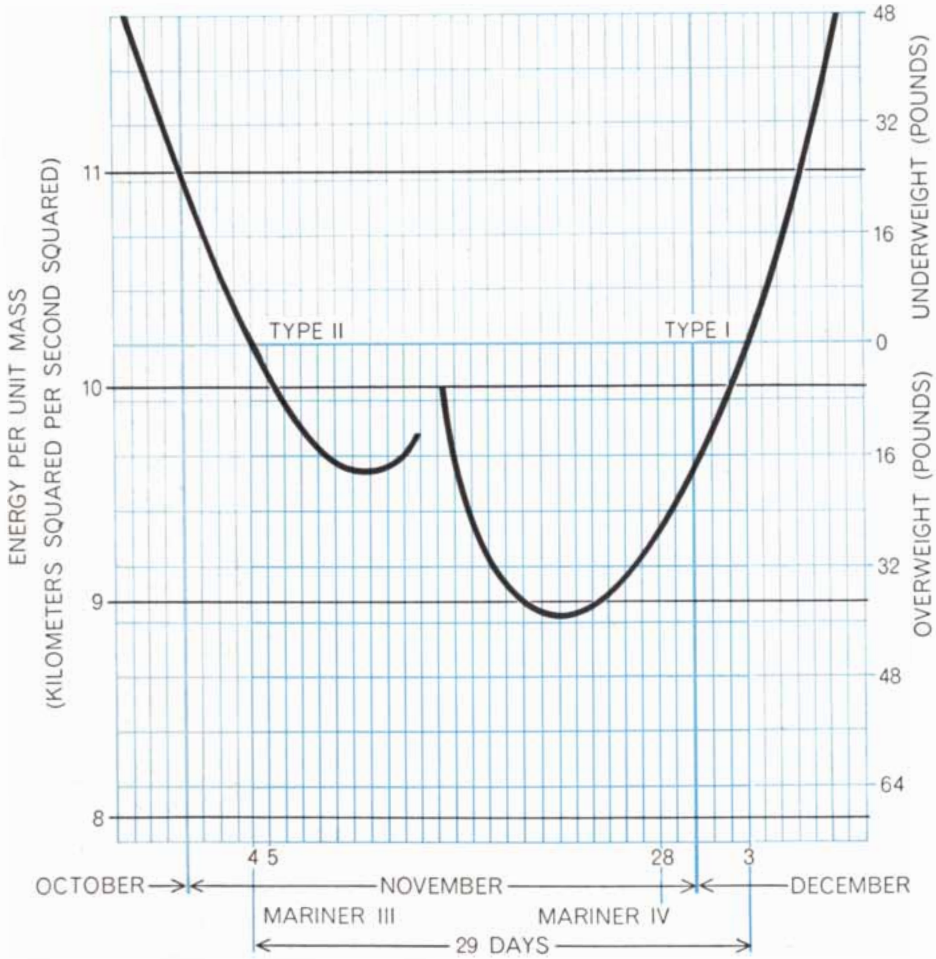
As the intensive development program got under way, problems were exposed by the hundreds. They were of three general kinds. Lightweight structures or devices failed to survive the vibration of simulated launchings. A few critical components deteriorated badly when operated for some thousands of hours. Gases trapped in components leaked when subjected to a vacuum for long periods. Although most of the problems could be solved readily, some required "crash" efforts by large task groups of engineers.

One such difficulty involved the data-automation subsystem. The spacecraft as a whole is packaged with a density of about 240 parts per pound; the data-automation subsystem has a density of 1,000 parts per pound, achieved by employing miniature "pellet components" to save weight. The components were potted in a compound that appeared to be acceptable until it was subjected to type-approval tests. Then it was found that at high temperatures the potting compound would react with the resistors in the potted circuits; after about 2,000 hours of testing, more than 50 percent of the resistors failed to meet specifications. After analyzing the cause of the failures, a task group devised a glass coating to protect the resistors. By the time new components could be assembled into new data-automation

MARINER IV followed the general design developed by the Jet Propulsion Laboratory for the Ranger lunar spacecraft and *Mariner II*, which traveled to Venus. *Mariner IV* is oriented here as it would have appeared from the direction of the earth as it approached Mars. In traveling outward to the orbit of Mars it fell behind the earth; therefore its path is carrying it across the page from right to left. Sun-sensors kept the long axis of the craft pointed at the sun so that the 28,224 solar cells on the four solar panels could collect a maximum amount of energy. The Canopus-sensor was locked onto the star Canopus, which is near the south ecliptic pole almost directly below the spacecraft's line of flight. When the spacecraft was fixed on the sun and Canopus, its high-gain antenna was pointed toward the earth throughout the last 70 percent of the flight to Mars, as shown in the illustration on page 47.



MARINER SCHEDULE for the Mars mission began with authorization in November, 1962, based on prior studies. The chart shows how it was necessary to compress design, fabrication and testing to have three flight-qualified spacecraft ready for launching by November, 1964.



MARINER LAUNCH PERIOD was fixed by interplanetary geometry that allowed maximum spacecraft weight for a given amount of rocket energy. Since this energy is fixed for a given launch vehicle, the launch period depends critically on the weight of the spacecraft. In Type I trajectories the spacecraft reaches Mars before traveling halfway around the sun; in Type II trajectories, after traveling halfway. *Mariner IV* followed a Type I trajectory.

subsystems, however, there was not enough time for type-approval tests. We had to take the gamble of installing the subsystems on the basis of standard flight-acceptance tests.

The general scheme for controlling the attitude, or orientation, of the Mars Mariners followed the one developed earlier for the Rangers and the Venus Mariner. The long axis of the spacecraft is kept pointing at the sun by a sun-sensor whose output signals are used to operate gas jets that can change pitch and yaw along this axis. The purpose of this control scheme is to keep the power-generating panels of solar cells—four panels in the case of the Mars Mariners—at right angles to the rays of the sun. If there were not another control mechanism, however, the panels would be free to roll around the spacecraft's long axis like the vanes of a windmill.

To stabilize the roll axis one would like to have a sensor that responded to some bright object either directly above or directly below the plane of the spacecraft's orbit. In the northern celestial hemisphere there is no suitably bright star in the appropriate location. In the southern hemisphere Canopus, the second-brightest star in the sky, is located within about 15 degrees of the south ecliptic pole—a point directly south of the sun as viewed from the plane of the earth's orbit [*see illustration on page 48*].

Accordingly the Mars Mariners were equipped with a star-sensor to stabilize the roll axis on Canopus and keep the solar panels from windmilling. Because Canopus is not precisely at the south ecliptic pole it was necessary to provide an automatic program that adjusted the angle between the Canopus-sensor and the roll axis four times during the long flight to Mars.

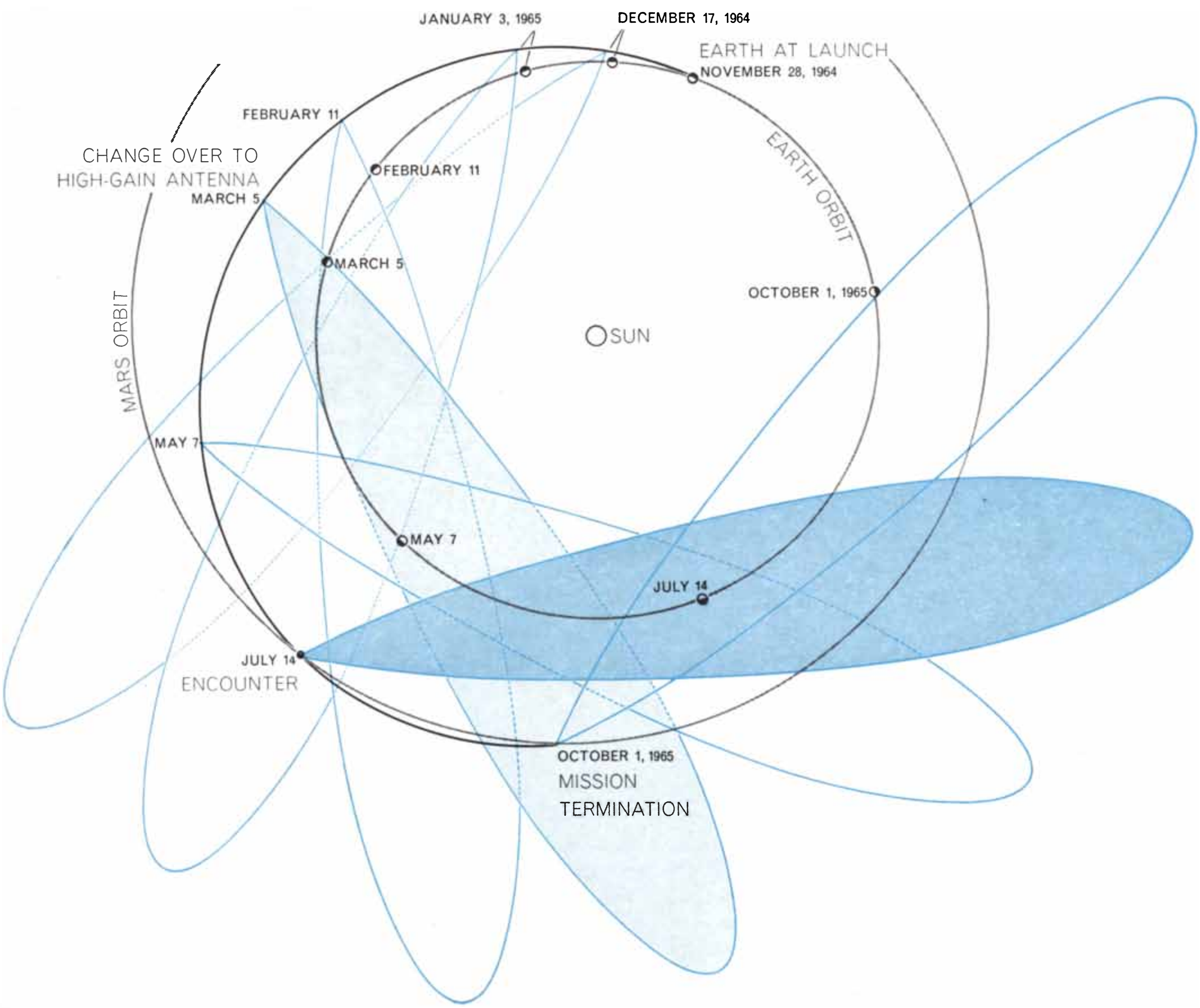
As auxiliary devices for pitch and yaw control, the Mars Mariners were equipped with four "sails," or vanes—seven square feet of aluminized plastic film made of Mylar; these vanes unfurled like Oriental fans at the end of each solar panel. The sun's electromagnetic radiation (as distinct from the solar wind) exerted a pressure of about a millionth of a pound on each vane. The vanes adjusted themselves automatically to balance the spacecraft on the sun and thereby reduce the frequency of operation of the gas jets. The total solar pressure on the spacecraft must be accurately taken into account in computing a trajectory. It was predicted that the pressure of sunlight would push *Mariner IV* 12,000 miles toward the orbit of Mars during the seven-month flight.

The Mariners were designed so that their functions and changes in them were specified by the stored program of a computer aboard the spacecraft. If necessary, this program could be altered by radio command from the earth. The on-board "central computer and sequencer" is the device that provides for most of the timing, sequencing and computing operations. The only commands that must be sent to the spacecraft are those needed to conduct a mid-course maneuver, if tracking data show that a change in course is necessary. Four commands are required for the maneuver. In addition, the capacity to respond to 28 other commands is designed into the spacecraft to back up the programmed instructions.

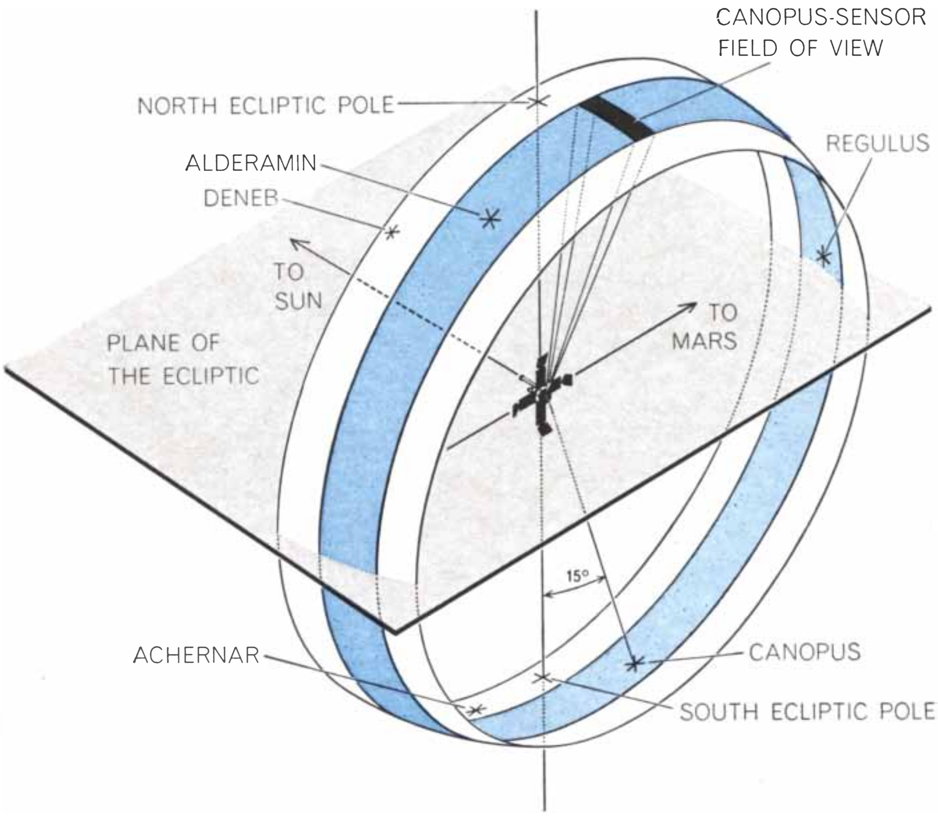
There were two 10-watt transmitters aboard: a triode cavity amplifier and a traveling-wave tube. The triode was used for the early part of the flight because it could be programmed to operate at a low power level during the ascent through the earth's atmosphere, when corona effects would occur and cause electric arcing. We switched to the traveling-wave-tube transmitter early in the flight because we knew it was more durable. If it had failed, the triode would have switched on automatically and vice versa. The transmitter alternated in sending 280 bits of data from the scientific instruments and then 140 bits from 90 different telemetry points that registered the condition of the spacecraft: temperatures, gas pressures, vane angles, voltages and so on. Data were sent at 33⅓ bits per second for the early part of the flight and were later switched to 8⅓ bits per second; at the lower rate about a second was needed for sending a single measurement.

Two antennas projected from the spacecraft; it could receive or transmit over either. The omnidirectional antenna was used for transmission until completion of the mid-course maneuver. It was not necessary to have a movable high-gain antenna for the Mars mission, as it had been for the Ranger moon missions and for the *Mariner II* Venus mission. Thanks to the fortunate geometry of the Mars mission, the spacecraft was so designed that when it was properly aligned on the sun and Canopus, the beam of the high-gain antenna was adequately aimed at the earth from about three months after launching until it was switched off on October 1 [*see illustration below*].

The mid-course motor carried on the Mars mission used a storable liquid monopropellant, anhydrous hydrazine,



HIGH-GAIN ANTENNA on *Mariner IV* was fixed with respect to the spacecraft. The antenna was aligned so that its beam would be pointed toward the earth throughout most of the flight. The omnidirectional antenna was used to transmit signals from the spacecraft until the earth fell within the high-gain beam on March 5, 1965, and it has again been used for transmission since October 1.



STAR-TRACKING SYSTEM of *Mariner IV* employed a sensing device that locked onto Canopus, which is the second-brightest star in the sky. Canopus lies within 15 degrees of the south ecliptic pole. As the spacecraft rolls slowly around its long axis, the Canopus-sensor scans the region of the sky shown. A brightness signal above a certain level brings the roll to a stop. Early in the flight the sensor locked onto Alderamin and other bright stars.

and was an advance over that employed for the Venus mission. It could be fired twice in flight; only a single firing was necessary to bring *Mariner IV* into the desired target zone.

Successful design and operation of the temperature-control system undoubtedly contributed to the long life of *Mariner IV*. Without such a system parts of the spacecraft would have been 125 degrees Fahrenheit colder at Mars's distance from the sun than at the earth's distance. The temperature in the instrument bays of *Mariner IV* was held in a range between 55 and 85 degrees F. by a combination of aluminized sheets of insulation, louvers actuated by coiled bimetallic strips, and paint patterns and reflecting surfaces.

All the equipment was scheduled to be shipped to Cape Kennedy in August, 1964, but because of problems uncovered in testing, the last items did not arrive until September. It had been recognized that the assembly and preparation period at Cape Kennedy would coincide with the peak of the hurricane season. During one hurricane threat Cape Kennedy was completely evacuated. One of the Atlas launch vehicles had to be removed from the launch pad and returned to its hangar, and the spacecraft had to be stored. Fortunately such contingencies had been allowed for in the schedule of Cape Kennedy operations.

Preparations for the launches proceeded well, and an attempt was made to launch *Mariner III* on the first day of the launch period: November 4, 1964. The attempt had to be abandoned because of minor problems with ground equipment. At about noon the next day *Mariner III* was launched from Pad 13. The minor difficulties had been corrected; the countdown was smooth; the weather was good, and we were all confident that the craft was on its way to Mars. Within minutes the mission was doomed. When the time came to free the shroud that protected the spacecraft during ascent, it could not be ejected.

Early indications of trouble came at the end of powered flight, when earth-based tracking indicated that the velocity of the spacecraft was too low by about 670 miles per hour. It soon became clear that the reason was the extra weight of 290 pounds contributed by the shroud. Five and a half minutes after the spacecraft and the Agena had separated, data telemetered from the spacecraft showed an unsuccessful attempt to deploy the solar panels. Without its panels deployed the spacecraft had no solar power and was running on its battery. After studying the telemetered data and building up piece by piece a picture of the trouble, the operations team took a series of steps to save the mission from failure. It commanded *Mariner III* to conserve power by switching off the scientific equipment, repeatedly commanded deployment of the solar panels, and at the same time developed a plan to ignite the rocket motor to put the spacecraft into a cartwheel spin to fling off the shroud.

How long should the motor burn? If it burned too long, either the spinning spacecraft would break up or its attitude-control system would be unable to recover. If it burned too briefly, the shroud would of course stay on.

There was little time left, since a series of extra commands had to be sent to ignite the motor at this early stage of flight. Furthermore, no one could tell how much charge remained in the battery under these unexpected flight conditions. Calculations were made and the commands were being sent to ignite the rocket motor when, eight hours and 43 minutes after launch, the battery ran out of power.

We were stunned that the labors of two years had come to this end. The task before us was to try to understand what had happened and why, correct the problem and carry out the launch operations of *Mariner IV*—all within 27 days. Fewer than 27 days would be available if our corrective measures increased the weight of the overall space vehicle.

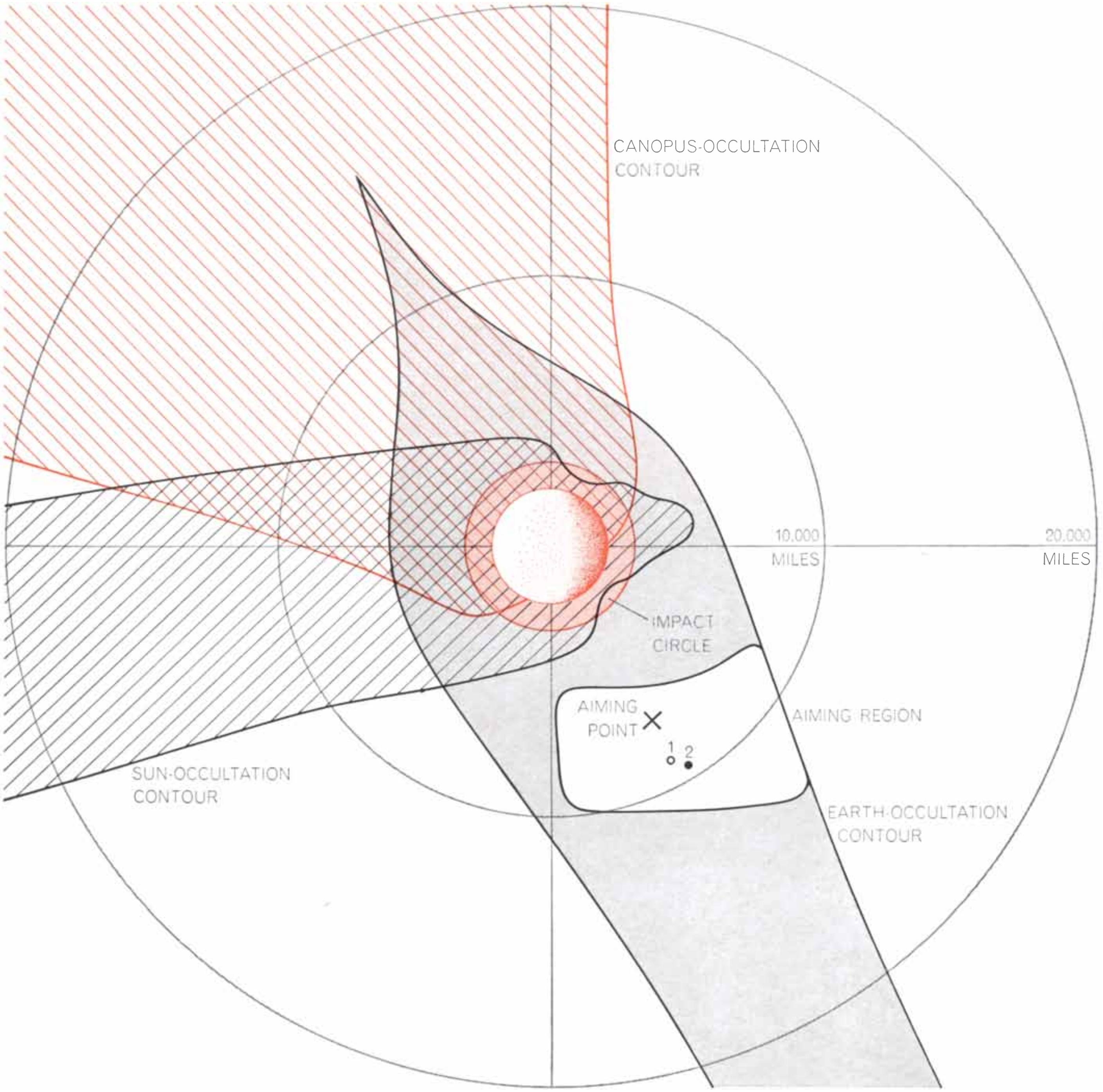
The task was completed in 23 days. The intensity of that effort and the determination of the men who worked around the clock to correct the trouble can only be touched on here.

The shroud consisted of a fiber-glass skin over a honeycomb core. After five days of data analysis and special testing it was determined that the shroud had ruptured early in the flight, most likely for the following reasons. During fabrication, air was trapped in the honeycomb cells. Subsequently, as a result of handling, the skin separated imperceptibly from the honeycomb in a few places. During the thrust up through the atmosphere, heating caused the air trapped in the honeycomb to expand and led to further separation of the skin. Finally, buffeted by aerodynamic forces, the shroud ruptured and collapsed. When the time came for it to separate from the Agena, it moved forward a few inches and jammed.

The design, fabrication and testing of a new shroud made of magnesium-thorium was begun immediately, and on November 22 the new shroud was flown to Cape Kennedy. Because the new shroud was 47 pounds heavier than the old one, the launch period was shortened by a few days. To regain those precious days we took certain risks and removed some important equipment from the space vehicle in order to lighten it; we also made plans to remove other equipment if it should be necessary to extend the launch period still further. Fortunately these actions were successful.

On November 27 an attempt was made to launch *Mariner IV*, but it was halted because of a momentary variation in the radio signal from the spacecraft while it was on the ground. The problem proved to be transitory, and at 1:37 A.M. Eastern Standard Time on November 28 the long countdown began. Not quite eight hours later, at 9:22 A.M., *Mariner IV* rose smoothly from Pad 12 [*see illustration on page 43*]. The Atlas performed perfectly and dropped away. Just before the ignition of the Agena engine, telemeter signals reported that the shroud had been jettisoned forward according to plan.

The Agena, with the spacecraft attached, ignited briefly and went into a 41-minute coasting orbit at an altitude of 100 miles. The coasting orbit carried the Agena and its payload over the Indian Ocean, where the Agena ig-



MARINER IV **AIMING REGION was selected to satisfy several constraints. The contours shown here are drawn on the "impact plane," a plane perpendicular to the trajectory of the spacecraft before it is pulled toward Mars by the planet's gravity. If the craft enters the impact circle, it will hit the planet. If it enters the Canopus-occultation contour, it will lose its fix on Canopus. If it enters the sun-occultation contour, it will lose its fix on the sun. Earth occultation is desired to interrupt the craft's radio signals. *X* marks the aiming point of the mid-course correction. Initial tracking data indicated that *Mariner IV* would enter the impact plane at *1;* later data established that it actually entered at *2*, about 500 miles away. The miss distance before mid-course correction was 151,000 miles.**

nited for the second time and provided the thrust needed to achieve an escape velocity of 25,598 miles per hour. Tracking data soon showed that *Mariner IV*, now separated from the Agena, would come within 151,000 miles of Mars—a distance that was well within the correction capability of the mid-course motor.

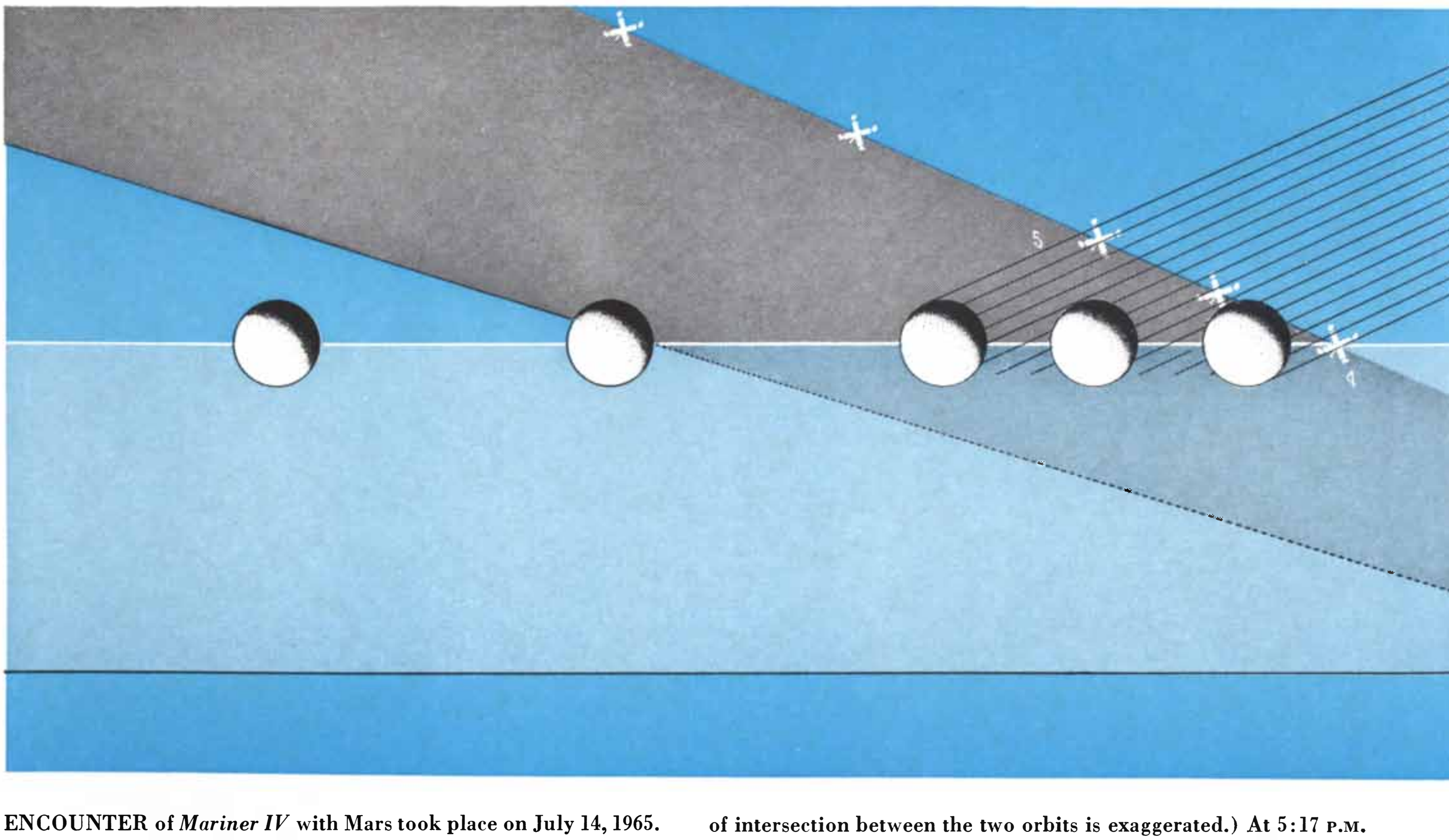
After *Mariner IV* and the Agena had separated, the power of the spacecraft's radio could be turned up because the craft was now in the vacuum of interplanetary space; the interplanetary scientific instruments were turned on. A three-minute timer was also started, and at the end of the three minutes small explosive charges pulled pins that unlatched the solar panels and set them in position. The spacecraft searched for the sun and "locked" onto it. After that, still following programmed instructions, the spacecraft went into a slow roll about its solar-pointing axis. This roll, which continued for the next 15 hours, made it possible—after analysis of the telemetry data—to distinguish between interplanetary magnetic fields measured by the magnetometer and the permanent field of the spacecraft itself.

The roll was stopped when the attitude-control system instructed the Canopus-sensor to lock onto Canopus. We were not altogether surprised to learn that the sensor had locked onto another bright star, Alderamin. This problem had been anticipated; radio commands were issued to achieve the proper fix on Canopus.

We had not anticipated, however, that the problem of staying fixed on Canopus would absorb our attention continuously for the next 19 days. The trouble evidently was that tiny dust particles shaken from the spacecraft glinted in the sunlight as they drifted in front of the Canopus-sensor. A dust particle a third the diameter of a human hair and located two feet in front of the Canopus-sensor would provide a brightness signal eight times the intensity of Canopus. When such a signal occurred in the Canopus-sensor, the spacecraft logic became confused and responded as if the sensor had detected a very bright signal other than Canopus. The spacecraft automatically began to roll, searching for Canopus, and would eventually come to rest on yet another star. The problem was finally corrected on the 19th day of flight at a time when it was certain that the spacecraft was fixed on Canopus: a command was sent out that in effect disconnected the sensing circuits that activated the roll search. From that time on *Mariner IV* never lost its fix on Canopus.

Previously, however, on the seventh day of flight, December 4, a maneuver was attempted to set the spacecraft on a course that would place it in the target zone. When all the commands had been stored in the spacecraft for the maneuver, *Mariner IV* lost its lock on Canopus and a command had to be hastily sent to erase the maneuver sequence. The maneuver was successfully carried out the next day. Commands were transmitted to the spacecraft instructing it to pitch 39 degrees, roll 156 degrees and burn its rocket motor for 20 seconds. The maneuver changed the course of the craft about a quarter of a degree and increased its velocity some 37 miles per hour. *Mariner IV* was now securely headed for the target.

The diagram on the preceding page shows the target area and certain "forbidden" zones. Mars is in the center. The band closest to Mars shows the region within which the spacecraft would be captured and pulled into the planet. In the shaded region above Mars the spacecraft would lose its fix on Canopus and its roll stabilization. In the region to



ENCOUNTER of *Mariner IV* with Mars took place on July 14, 1965. The scene is pictured here from inside the orbit of Mars, looking away from the sun. *Mariner IV* is approaching Mars from the right, about 8,000 miles below the plane of the planet's orbit. (The angle of intersection between the two orbits is exaggerated.) At 5:17 P.M. Pacific Daylight Time, when *Mariner IV* is about 10,500 miles from Mars (*1*), the television camera begins tape-recording views of the planet. The last of 22 pictures is taken 26 minutes later (*2*),

the left of Mars the spacecraft would enter the planet's shadow, where it would lose its solar power and its fix on the sun. The ideal target zone was chosen to match the sensitivity requirements of the instruments and to achieve radio occultation; the aiming point in that zone and the actual point at which the spacecraft passed Mars are identified in the illustration. With the mid-course maneuver successfully completed, the space-flight-operations team at the Jet Propulsion Laboratory and the stations of the Deep Space Net settled down to the job of tracking *Mariner IV* 24 hours a day and recording data for analysis. There are three stations in addition to the one at Goldstone: Woomera and Canberra in Australia, Johannesburg in South Africa and Madrid.

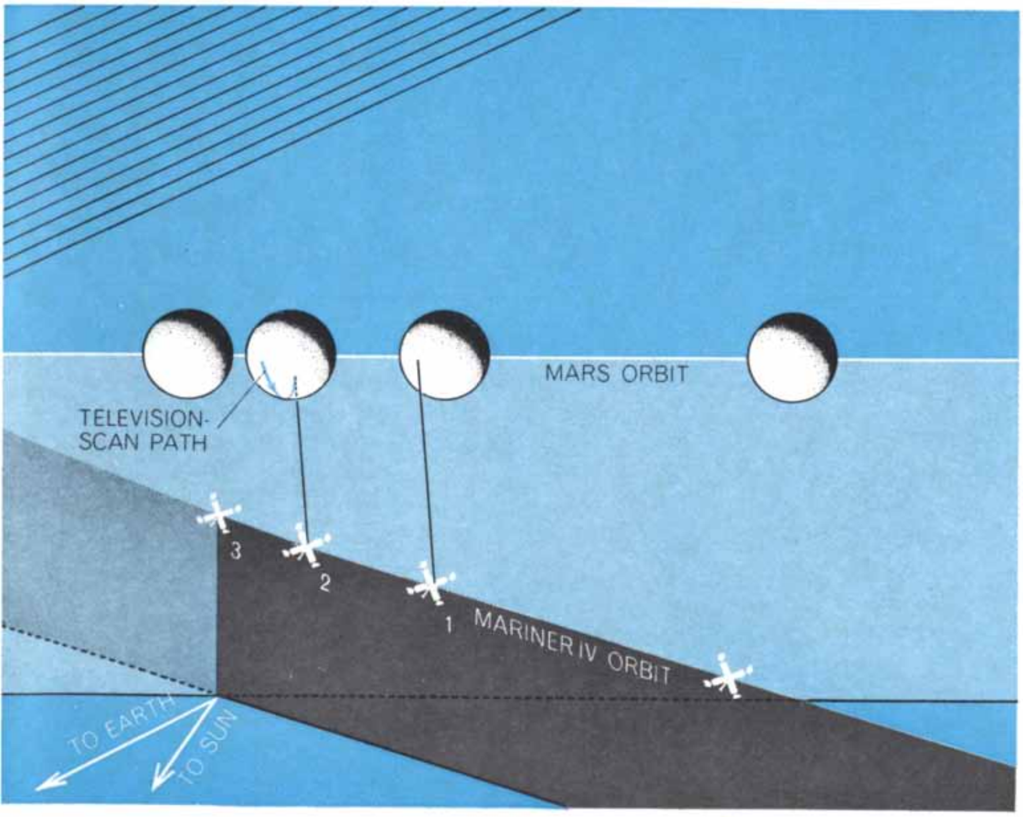
On the 78th day of flight some precautionary measures were taken to enhance the probability of success at the encounter with Mars. The cover of the television camera lens was scheduled to be removed automatically in the vicinity of the planet. Because of the difficulties we had met with the Canopus-sensor, we were worried that removing the cover might release a small cloud of dust particles and cause problems at a time when there would be little opportunity for recovery. We issued a command for the cover to drop away.

Still another precaution was taken. We knew how successful the mid-course maneuver had been and thus we could compute quite accurately the relation of the spacecraft to the planet at the time of encounter. We were concerned lest during the long flight the bearing system on the scanning platform that held the television camera freeze and refuse to move when commanded to at the end of the trip. We also knew that the position of the platform was such that unless it could be moved we would not be able to photograph the planet. Accordingly, in addition to sending a command for the television lens cover to drop away, we dispatched a command for the platform to go into its scanning motion. After we had timed the scanning motion via the telemetry system for a number of cycles, we sent out a command that stopped the scanning platform in such a way that the television camera would be aimed directly at Mars at the time of encounter. The precaution proved unnecessary: the scanning system did function when it was finally called on.

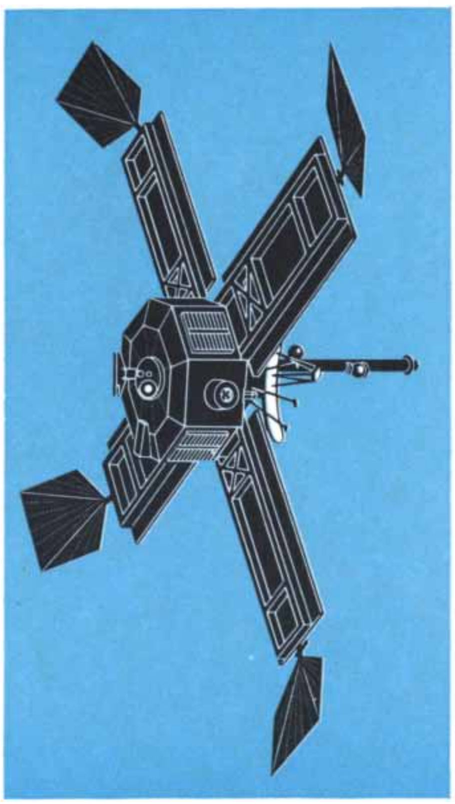
Throughout the first four months of flight the telemetry data showed that *Mariner IV* was running into a steadily increasing amount of cosmic dust. This caused considerable apprehension, particularly since there had been reports that Soviet spacecraft aimed toward Mars had encountered meteor showers. On about the 145th day of flight the bombardment observed by the cosmic-dust detector reached a peak and thenceforth decreased, relieving our fears.

Much thought and planning went into the preparations for the 228th day of flight—the encounter with Mars. We wondered what to do, for example, if the narrow-angle sensor, on detecting the planet, did not successfully initiate the operation of the tape recorder aboard the spacecraft. Information about such a failure would take 12 minutes to traverse the 134 million miles that would then separate *Mariner IV* and the earth. It would require a few more minutes to analyze the situation, make a decision and send a corrective command, which would then take another 12 minutes to get back to the spacecraft. In all, some 30 minutes would have elapsed before the tape recorder could be turned on. By then it would be about four minutes too late to photograph the planet.

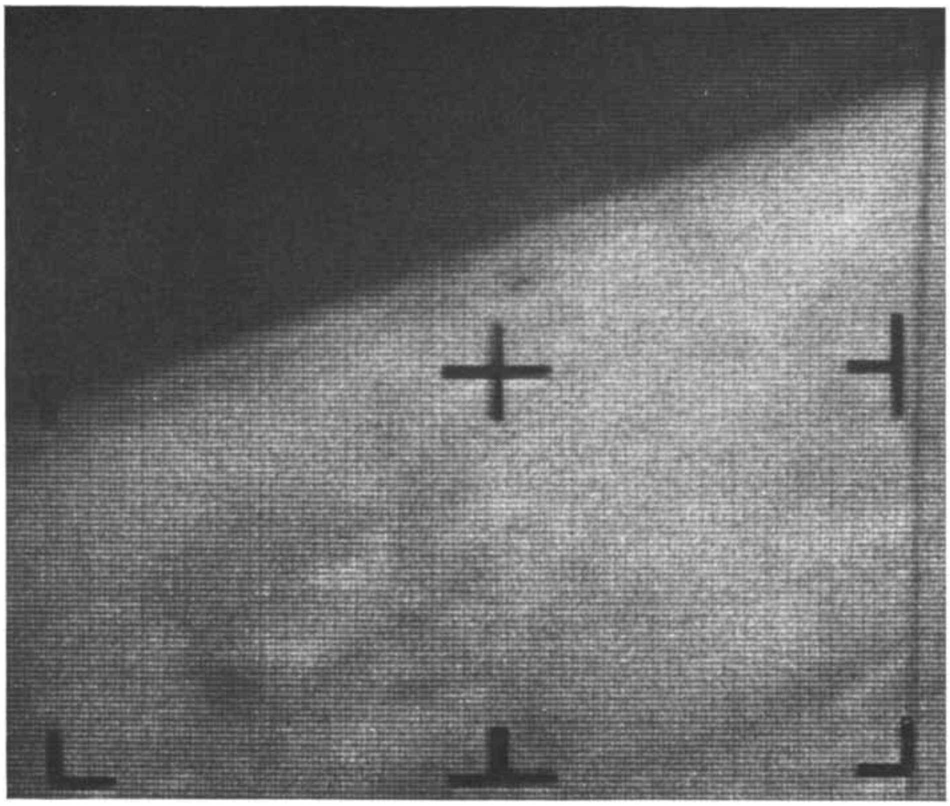
Once again a plan was developed to make use of backup commands by send-



when Mars is about 7,400 miles away. At 6:01 P.M. *Mariner IV* passes within 6,118 miles of the planet's surface, the point of closest approach (*3*). About an hour later (*4*) Mars comes between the earth and the spacecraft, blanking out its signals. The occultation ends 54 minutes later (*5*). About 10 hours later playback of the recorded television pictures begins.



MARTIAN VIEW of *Mariner IV* at the time it was taking pictures would have looked like this. The camera is mounted at the base of the spacecraft, in the shadow of the sun.



FIRST CLOSE-UP OF MARS was received at the Goldstone tracking station of the Deep Space Net on July 15, 1965. Because the sun was shining almost straight down on the planet there were no strong shadows to bring out surface details. In later pictures, which will be discussed in a forthcoming article in SCIENTIFIC AMERICAN, numerous craters are visible.

ing them in advance of any evidence indicating a need for the commands. In some cases the commands were organized in such a way that the on-board program would provide immediate alternative measures.

About 10 hours before the time of closest approach to Mars the activity began. The spacecraft was placed in the encounter mode with the television system turned on. The platform holding the television camera began its scanning motion. The television subsystem consisted of a single Vidicon tube attached to a small reflecting telescope that provided a focal length of 12 inches. The system was capable of taking about 22 pictures in 25 minutes, which were stored on a 330-foot loop of tape.

The picture-taking sequence began at 5:17 P.M. Pacific Daylight Time on July 14 of last year and ended 26 minutes later. During this period the distance between the spacecraft and Mars was reduced from 10,500 to 7,400 miles. There were some worrisome indications that something had gone wrong with the tape-recording mechanism, but these later proved false. Eighteen minutes after the picture-taking had been completed, or at 6:01 P.M., the spacecraft came within 6,118 miles of the surface of the planet Mars—the point of closest approach.

Until a few days before encounter, the tracking data had indicated that the point of closest approach would be about 500 miles nearer Mars. The discrepancy is still being studied. It might have been caused by fluctuations in the response of the spacecraft to solar radiation pressure, by a slight error in the measured value of the astronomical unit (the mean distance from the center of the earth to the center of the sun, from which all other planetary distances are computed) or by errors in the predicted position of Mars in its orbit.

About an hour after the time of closest approach *Mariner IV* disappeared behind Mars. Fifty-four minutes later it emerged from behind the planet and its signal was quickly picked up by the tracking stations at Goldstone and Canberra; shortly thereafter the instruments concerned only with Mars itself were turned off. Some 11 hours after the time of closest approach the spacecraft began the slow playback of the information it had captured on its tape during its brief encounter with the planet.

The historic first picture of Mars taken by *Mariner IV* is shown above. At the slow playback rate of 8⅓ bits per second, 8⅔ hours were required to play back each picture. When the complete series of 22 pictures had been received, the tape was played a second time to fill in any data that might have been lost in the first transmission.

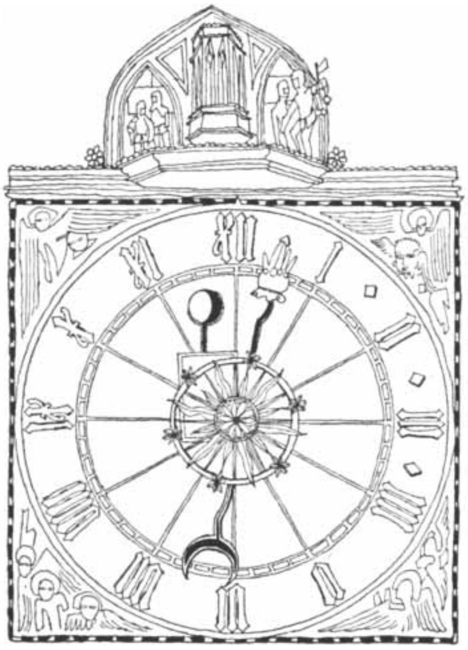
The results of all the experiments conducted on the *Mariner IV* mission will be reviewed in subsequent articles in *Scientific American*. I shall summarize briefly by noting that the *Mariner IV* instruments detected no evidence that Mars possesses either a significant magnetic field or radiation belts. The occultation experiment showed that the atmosphere of Mars is even thinner than most earlier studies had indicated. The 22 photographs are still being intensively studied. They show that the surface of Mars is heavily cratered, presumably because it has been subjected to very little erosion of the kind experienced by the earth's surface. Nearly everyone agrees that the photographs leave open the question of whether or not Mars has ever supported life.

We have evidence that *Mariner IV*, now on the other side of the sun, is at this writing still working well. By integrating the radio signal from the spacecraft over a period of several minutes, the stations of the Deep Space Net have been able to detect the signal even though its power is only a tenth that of the background noise level. It is not possible, however, to extract information from such a faint signal.

This spring a new high-precision 210-foot dish antenna is scheduled to go into operation at Goldstone. If *Mariner IV* is still working, the new dish may be able to recover some significant data from the spacecraft's signals. Meanwhile the spacecraft is continuing in its orbit around the sun. On June 6 of this year, 556 days after launching, it will have made one solar revolution. Barring failure, it has enough attitude-control gas in its tanks to remain stabilized on the sun for at least another three years.

If *Mariner IV* is still operating when it makes its next close approach to the earth during September, 1967, we shall be able to communicate with it and receive good data from its omnidirectional antenna. Such data will be very valuable indeed, because they will come from a region of space some 10 million miles above the orbital plane of the earth, which has not yet been explored. *Mariner IV* was lofted out of the earth's orbital plane by the gravitational attraction of Mars, which changed the spacecraft's direction by some 15 degrees. But perhaps it is expecting too much to wish to hear from *Mariner IV* again. It has already fulfilled every hope and expectation of all those who were associated with its mission.

SCIENCE AND



The Impact of Automation

Early in February the National Commission on Technology, Automation and Economic Progress submitted a 210-page report, the result of a year-long study, to the President. Created by Congress, the commission had been asked to evaluate the effect of technological change on the U.S. since World War II, to forecast the rate of change and its impact over the next 10 years and to recommend measures to help workers meet that impact. The commission was also asked to define areas of unmet social needs that would benefit from the application of new technologies and to recommend ways in which technological change could be used to promote the continued economic growth and well-being of the American people.

The commission reported:

Output per man-hour in the private economy increased at an overall rate of 2 percent per year from 1909 to 1947. From 1947 to 1964 the increase has been at the rate of 3.1 percent per year. If agriculture is excluded, the prewar increase remains at 2 percent but the postwar increase is reduced to 2.5 percent. The commission concluded that "a sharp break in the continuity of technical progress has not occurred, nor is it likely to occur in the next decade."

The high unemployment rates following the Korean war, which reached a peak in 1961 when 7.1 percent of the labor force was unemployed, were due primarily to an inadequate economic growth rate rather than to automation and other technological changes.

To absorb the expected increase of more than 18 million workers in the labor force over the next decade (a 26 percent increase from 70.4 million in 1964 to 88.7 in 1975) the economy will have to grow at an average rate of more than 4 percent per year "just to prevent the unemployment rate from rising, and even faster if the unemployment rate is to fall further, as we believe it should." As evidence that there is no cause for complacency, the 1947–1965 average rate of increase in the gross national product was only 3.5 percent per year.

Over the next 10 years the number of unskilled jobs will remain at about 3.7 million but will then represent only 4.2 percent of the total labor force, compared with 5.2 percent in 1964. In 1975 the sector of the economy producing "goods" will provide only 36 percent of all jobs, compared with 41 percent in 1964. The sector producing "services" will show a proportionate increase from 59 to 64 percent. The largest increases in employment will come in state and local government (five million more jobs, a 69 percent increase), wholesale and retail trade (four million more jobs, a 33 percent increase), services and miscellaneous (3.7 million more jobs, a 43 percent increase), all manufacturing (2.5 million more jobs, a 14 percent increase) and contract construction (1.1 million more jobs, a 37 percent increase). The biggest decrease in employment will be in agriculture, which will provide 1.1 million fewer jobs in 1975, a decrease of 21 percent.

The commission made the following major recommendations:

The Federal Government should use fiscal policies (tax reduction and increased public spending) to stimulate the economic growth that will be needed in the decade ahead "for successful adjustment to technological change."

The Government should act as an "employer of last resort" to provide jobs for those "unsuccessful in the competition for existing jobs." Such public service employment should be directed toward satisfying community and social needs that are currently unfulfilled in medical institutions, schools, national parks and so on.

The Government should establish a program of income maintenance, perhaps through a "negative income tax," to provide a floor of minimum income for individuals and families. It is estimated that between $10 billion and $16 billion a year would be required to raise the income of the 35 million people now living in poverty to the level of $3,000 a year.

Free public education should be provided for 14 years instead of the present 12 years.

No one should be deprived of a college education for lack of funds.

The Government should create a nationwide computerized service to help match unemployed workers with available jobs.

New technology should be applied to such problems as improvement of medical care, abatement of air pollution and water pollution, development of new transportation systems and improvement of the quality and lowering of the cost of housing.

The chairman of the commission was Howard R. Bowen, president of Grinnell College. The other members were Benjamin Aaron, Joseph A. Beirne, Daniel Bell, Patrick E. Haggerty, Albert J. Hayes, Anna Rosenberg Hoffman, Edwin H. Land, Walter P. Reuther, Robert H. Ryan, Robert M. Solow, Philip Sporn, Thomas J. Watson, Jr., and Whitney M. Young, Jr.

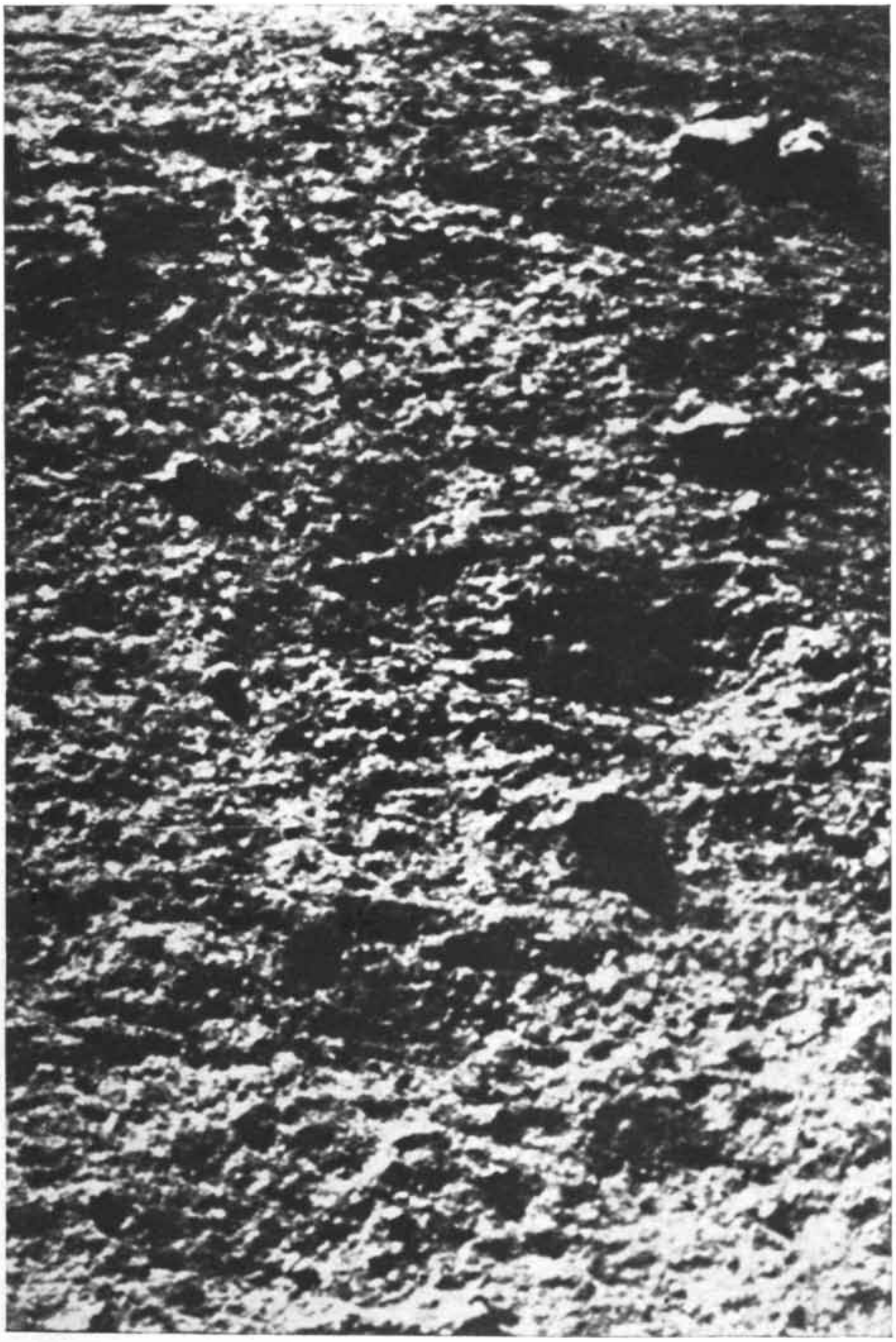
Antivivisection and Congress

With some 30 bills concerning laboratory animals now before Congress, a major effort to pass restrictive legislation is expected during this session. None of the bills explicitly opposes animal experimentation; most of them concede its value. According to the National Society for Medical Research, most would nonetheless hamper biological investigation; they are based on the premise that such investigation often involves unnecessary cruelty to animals and are designed to limit and control the amount and nature of animal experimentation.

One group of bills deals with the care of laboratory animals, the other with their procurement. The most restrictive bills in the first group would establish a Federal "coordinator" or "commissioner" who would license investigators or institutions to experiment with animals. In most cases applicants would have to describe their research plans and expectations in advance in order to get approval; under some bills they would have to certify that no method of animal experimentation but the kind proposed could possibly obtain the desired information. The National Society for Medical Research states that such bills would impose "censorship" of scientific investigation and opposes them. It opposes another "moderately restrictive" bill (S. 1087) primarily because, like most of the others, it aims at "preventing needless duplication of research" and could therefore make experiments to verify new findings virtually impossible.

The bills dealing with procurement would make "pet-stealing" for medical research purposes a Federal offense. The society is concerned that such legislation would interfere with legal arrangements under which animals in public pounds that would otherwise be put to death are released to approved institutions. It points out that local, state and Federal laws already cover larceny and the transportation of stolen property across state lines.

The society is backing H.R. 5191, introduced by Representative Edward R. Roybal of California. This bill would provide Federal support for research, training, dissemination of information and facilities for improved animal care. Its provisions would be administered by a coordinating committee headed by the Surgeon General with guidance from an advisory committee of the National Academy of Sciences.



SURFACE OF THE MOON appears in this photograph made by the Soviet space station *Luna 9* and radioed back to earth. The camera has a wide-angle lens and is mounted about two feet off the ground. The small crater at right center is probably less than a foot across.

Luna Firma

The surface of the moon appears to be quite solid and unexpectedly low in radioactivity. These are the two chief scientific findings to come out of the successful Soviet "soft landing" of an unmanned spacecraft on the moon on February 3. According to Academician Nikolai Barabashov, a leading Soviet selenologist, the *Luna 9* photographs "proved beyond doubt that the upper layer of the lunar soil is a spongelike, rough-textured mass scattered with individual sharp-edged fragments of various sizes." The long-standing question of whether or not this layer is strong enough to support heavy objects appears to have been settled. Soviet workers charged with analyzing the photographs point out that the 220-pound *Luna 9* instrument package "did not sink into the soil to any substantial degree."

Since *Luna 9* is believed to have landed in a region known as a mare, or "sea," the photographs seem to contradict the widespread apprehension that the surface of such maria might be covered by a thick blanket of loose dust into which any spacecraft would sink as though in quicksand. Nonetheless, the Soviet analysts emphasized that the landing point might not be typical, and that other areas could be dusty.

The resemblance of the lunar surface material in the photographs to a slaggy form of quick-cooled terrestrial lava (*see illustration at left*) has been interpreted by some Soviet and U.S. workers as supporting the view that the lunar maria are filled with volcanic material. Other workers in this country (notably Thomas Gold of Cornell University) feel that the photographs alone do not prove this hypothesis. Gold points out that the same appearance might be presented by a layer of dust produced in a vacuum by meteorite collisions and subsequently cemented. The small-scale roughness evident in the photographs, he adds, might also be the result of many tiny meteorite collisions.

Radiation detectors aboard *Luna 9* registered a very low surface measurement of 30 millirads per day, which Soviet physicists attributed largely to cosmic rays. The level of radioactivity of the lunar surface material indicated by this measurement is much lower than the level of radioactivity of the earth's surface material. This finding may have a considerable bearing on theories of the moon's origin. It seems to support the view that the moon is an alien body that has been captured by the earth's gravitational field rather than a chunk of material torn from the surface of the earth. Both Soviet and U.S. workers were inclined to reserve judgment on this question, however, until more information becomes available.

Among the many technical achievements of the *Luna* 9 mission, the procedure for detaching the instrument package from the rocket vehicle just before impact seems particularly noteworthy. After retro-rockets had slowed the vehicle's speed from some 6,000 miles per hour to less than 15 miles per hour, an undisclosed device ejected the roughly spherical instrument package, which landed some distance from the rocket's point of impact. This procedure guaranteed that the area observed by the instrument package was a "virgin" lunar landscape and had not been altered by the blast of the retro-rockets.

Where the Jobs Are

A survey of 10,000 Ph.D.'s in the sciences has recently been published by the National Academy of Sciences–National Research Council. The 10,000 were selected from 200,000 persons granted science Ph.D.'s since 1920; the study covered the period 1935–1960. The majority–59 percent–are employed by colleges and universities; 8 percent work for the Federal Government; 15 percent work in business and industry, and the remainder–18 percent–are distributed among all other categories of employers. The distribution is remarkably constant for all age groups.

The Middle West produced 40 percent of the Ph.D.'s and now employs only 25 percent. The other chief exporter is New England, which produced 13 percent and employs 7 percent. The chief importers are the South and the West, which both produced 12 percent and now respectively employ 24 percent and 18 percent. The Middle Atlantic states produced 22 percent and employ 20 percent.

In the 25-year period the following shift took place in the origin of the

Ph.D.'s. Output in New England declined from 15 percent to 11 percent, in the Middle West from 42 percent to 39 percent and in the Middle Atlantic states from 23 to 21 percent. The South increased its output from 10 to 15 percent and the West from 9 to 15 percent.

A separate study prepared for the House Committee on Government Operations shows that in 1964 the following institutions, listed in order, were the 15 leaders in producing science Ph.D.'s: the University of California, the Massachusetts Institute of Technology, Purdue University, the University of Wisconsin, the University of Illinois, the University of Michigan, Stanford University, Harvard University, Iowa State University, Columbia University, the University of Texas, Ohio State University, Princeton University, New York University and Cornell University. One surprising finding is that the percentage of Ph.D.'s awarded in the physical and biological sciences has fluctuated in a narrow band between 45 percent and 53 percent of all Ph.D.'s awarded annually since 1920. In other words, the huge influx of Federal funds in support of the natural sciences has not altered the historical ratio of Ph.D.'s in science to all other Ph.D.'s.

In accord with this finding, the major producers of science Ph.D.'s are not necessarily the major recipients of Federal funds. When one lists separately the 15 major recipients of funds from the Department of Defense, the National Aeronautics and Space Administration, the Atomic Energy Commission and the National Institutes of Health, one finds that each list contains only seven or eight of the top producers of science doctorates. Only the list of 15 for the National Science Foundation contains as many as 10 of the 15 leading Ph.D. producers.

The National Academy survey shows that the average time spent in research has been rising and that the time spent in teaching has been declining for each new generation of doctorates. In 1935 the new Ph.D. spent 47 percent of his time teaching and 36 percent in research; in 1960 the percentages were respectively 33 percent and 49 percent. Administrative chores consume an increasing amount of the Ph.D.'s time as he gets older. Twenty-five years after he has earned his degree administrative duties occupy 32 percent of his time, compared with 8 percent at the start of his career.

The survey also shows the geometric mean of present salaries according to field. The top 10 fields, in order, are engineering ($16,970), physics ($15,860), the medical sciences ($14,860), chemistry ($14,660), economics ($14,280), mathematics ($14,170), pharmacology ($13,850), psychology ($13,200), geology ($12,890) and political science ($12,870).

North Sea Petroleum

The most active area of oil and gas exploration in the world is now the North Sea and its environs. The special inducement is the prospect of finding large fuel deposits in immediate proximity to major markets and in a region of relative political stability. Writing in *The Geographical Review,* Trevor M. Thomas points out that in recent years petroleum producers have been turning increasingly to secondary-recovery methods and unconventional sources [see "Tar Sands and Oil Shales," by Noel de Nevers; SCIENTIFIC AMERICAN, February], and that exploration for new conventional sources of petroleum is shifting more and more to the submerged continental shelves [see "Living under the Sea," by Joseph B. MacInnis, page 24]. Offshore production is estimated to account for about 16 percent of the total petroleum output of the non-Communist countries today and may reach 25 percent by 1990.

The major stimulus to North Sea exploration was the discovery in 1959 of natural gas in the province of Groningen in the northeastern Netherlands. The Groningen field, together with its extension into West Germany, now appears to cover more than 200 square miles and to contain enormous reserves. In Britain, where a number of oil and gas fields have been in production for some years, there has been increased exploration in the past decade. Some of the gas discoveries have been in the same geological system (the Permian) as the Groningen field.

It has long been suspected, Thomas writes, that there are in the North Sea salt domes of the type that provide the structural basis of much of the oil entrapment in the Gulf of Mexico region. Since the Groningen discovery extensive magnetic and seismic surveys have been carried out at sea by a large number of companies. Concessions have been granted by the United Kingdom, the Netherlands, West Germany and Denmark; some wells have been drilled and some oil- and gas-bearing strata have been discovered. According to Thomas, the work is extremely speculative, but it seems likely that valuable deposits will be found. In view of the serious financial burden that oil imports have imposed on European countries, even moderate oil or gas production from the North Sea would have a significant effect on the economy of western Europe.

Rh Incompatibility Controlled?

Investigators in several countries are evaluating a technique that may eliminate the hazard arising when a woman with Rh-negative blood bears the children of a man with Rh-positive blood. Some 85 percent of the people in the U.S. are Rh-positive: on the surface of their red blood cells is a substance that is not present on the red cells of the Rh-negative 15 percent. When an Rh-negative woman gives birth to an infant that has inherited its father's Rh-positive blood, Rh-positive red cells leak through the placenta and enter her bloodstream. She may then produce antibodies to remove the cells, a response known as active immunization. When she becomes pregnant with a second Rh-positive child, the response is intensified, that is, she produces antibodies in much larger quantity. These antibodies can destroy Rh-positive red cells in the unborn child and give rise to the serious disorder erythroblastosis fetalis. About one in every 200 infants born in the U.S. suffers from this disorder; three out of four recover completely but one out of four dies.

Several years ago Vincent J. Freda and John G. Gorman of the Columbia University College of Physicians and Surgeons and William Pollack of the Ortho Research Foundation in Raritan, N.J., undertook an attempt to prevent active immunization from arising in pregnant Rh-negative women. Independently R. Finn and his colleagues at the University of Liverpool began a similar effort. Each group injected its subjects with a serum rich in an antibody (sometimes called "anti-D") that is known to destroy Rh-positive red cells. By administering anti-D immediately after delivery the investigators were able to induce a short-lived "passive immunization" that sufficed to destroy the Rh-positive red cells that had entered the woman's blood. This passive immunization can be induced repeatedly without being intensified. Theoretically a woman supplied with anti-D will not produce any antibody of her own, and samples of blood taken from subjects months after delivery contain neither self-produced antibody nor anti-D. The investigators are awaiting the next pregnancies of their subjects.

Research Submersibles: A report from General Dynamics

New breed of vessel:

A hundred and thirty feet down in the Aegean Sea, a Byzantine galley had hidden its secrets for almost fifteen centuries. Then in 1964, University of Pennsylvania Museum archeologists mounted paired cameras on a new research submarine, Asherah, and learned more from the three-dimensional photographs obtained in one "flight" over the wreck than had been possible from weeks of scuba diving.

This was the first of dozens of undersea jobs already done by Asherah. The Asherah is the 339th—and at 17 feet long, the smallest—submarine built by General Dynamics. For comparison, the Holland, the very first submarine we delivered to the Navy in 1900, was 54 feet long. Over the years, we have built the prototypes of most classes of United States Navy submarines, including its nuclear-powered undersea ships.

But the true manned research submarines are really a new breed of boat. Less than a score now exist.

Depth and mobility:

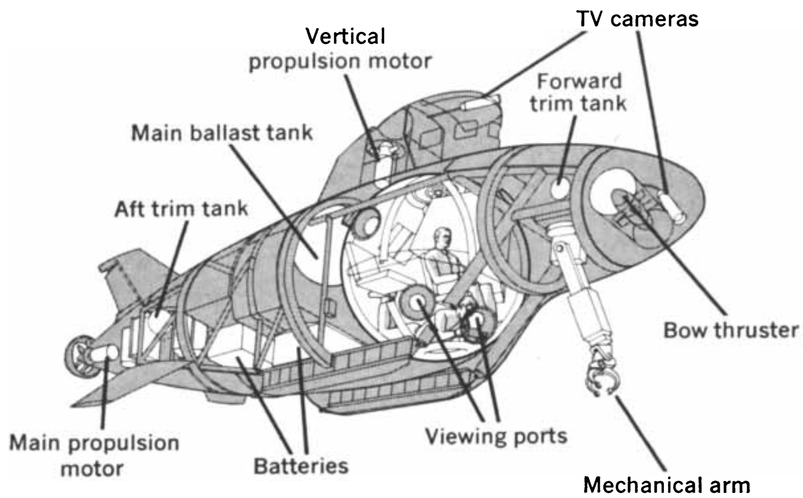
Unlike bathyscaphes, designed to drop to great depths but remain relatively immobile for passive observation, the new research submarines must have depth capability, the ability to perform useful work, and the mobility to survey extended areas at a reasonable speed.

Asherah is one of the first true research submarines. It can dive to 600 feet (World War II subs rarely dived much below 300 feet), stay submerged for ten hours, cruise at three to four knots, move in all directions. An improved sister ship, Star II, is made of the same HY-80 steel that goes into nuclear submarines; it has depth capability to 1,200 feet.

A larger boat we call Star III (see cutaway drawing below) is built of even tougher HY-100 steel. It has a cruising depth of 2,000 feet, and is equipped with an external mechanical arm that has interchangeable "hands"—a clamshell grip, a wire cutter, and a "three-finger" which can pick up a pencil or a 200-pound weight, or manipulate a valve.

The Asherah beneath the Aegean Sea.

CUTAWAY OF STAR III



The Aluminaut, the largest research sub so far, was built by General Dynamics for Reynolds International to prove, among other things, the feasibility of aluminum as a hull metal. The 51-foot Aluminaut is designed to operate at depths up to 15,000 feet, under pressures up to more than 7,000 pounds per square inch. Aluminaut, in early sea trials, has cruised as deep as 6,250 feet, and remained submerged for over 30 continuous hours. A World War II military submarine rarely remained submerged for more than 24 hours.

Problems and needs:

These early research subs still have many limitations of speed, range and submerged endurance. They require back-up by a mother ship and have to be carried or towed to a job location.

This last "limitation" can sometimes be an advantage. Asherah and Star II, for example, are small enough to be rushed by air for a rescue operation.

But subs with many special characteristics will be needed for exploring—and for exploiting—the sea.

Some vessels will have to withstand pressures up to 10,000 pounds per square inch, to allow them to penetrate into mid-ocean abysses four miles deep. Work subs for, say, mining will have to be stable enough in a buoyant environment not to be whipped about in reaction to the force of their own tools.

We have already done a study for the Bureau of Fisheries showing feasibility of a submarine to track oceanic fish. It would be 160 feet long, carry 31 persons at speeds up to 20 knots, and could cruise submerged for up to 90 days.

Right now, we don't think there will ever be one single all-purpose type of research-work submarine. Just as land vehicles range from motor scooters to 20-ton earthmovers, so will most manned submersibles be designed and built for special purposes.

General Dynamics is a company of scientists, engineers and skilled workers whose interests cover every major field of technology, and who produce: aircraft; marine, space and missile systems; tactical support equipment; nuclear, electronic and communication systems; machinery; building materials; coal and gases.

GENERAL DYNAMICS

BEARINGS

Basically they enable two parts of a machine to move with respect to each other. They have evolved rapidly in response to increased demands in terms of load, speed, lifetime, efficiency and economy

by E. A. Muyderman

Bearings are among the simplest and most familiar components of machines. Even someone who knows very little about machinery has heard of, say, a ball bearing and can describe how it works. Yet bearings, like practically every other component of modern machines, are in a phase of rapid evolution that has carried them beyond everyday familiarity. Under such circumstances it is useful to examine bearings from the basic aspects of design and function, and to consider how they have evolved in these aspects to meet the needs of technology.

Fundamentally bearings have only two purposes: to let one mechanical part move while a related part stands still (or moves at a different speed) and to minimize friction between the two parts. From the standpoint of design bearings can be divided into the broad categories of sliding bearings and rolling bearings. In a sliding bearing the moving surface slides in more or less direct contact with the stationary surface. (In most modern machines, of course, the two surfaces are slightly separated by a film of lubricant.) A typical member of the sliding group is the journal bearing, such as the bearing commonly used for the axle of a railroad car. In a journal bearing a shaft rotates within a cylindrical shell. To be precise about terminology, the part of the shaft that is inside the shell is the journal and the shell is the bearing; the lining of the shell is often called the bushing. Ordinarily, however, the entire assembly is regarded as the bearing. Another widely used sliding bearing is the type known in the U.S. as the Kingsbury bearing and in Europe as the Michell bearing. I shall describe this bearing in more detail later.

Rolling bearings, which are also known as antifriction bearings, interpose between the moving and the stationary surfaces an assembly of devices that roll. These devices can be spherical, whence the name "ball bearing," or they can be more or less cylindrical and so be classed as roller bearings. In any case the basic idea is the same: the devices bring about a reduction of friction because they change the sliding motion into a rolling one.

A more recent development is the spiral-groove bearing. In configuration and mode of operation this bearing most closely resembles a sliding bearing. Our work at the Philips Research Laboratories in the Netherlands has shown, however, that in some applications a grease-lubricated spiral-groove bearing can replace a rolling bearing at a favorable comparison in cost.

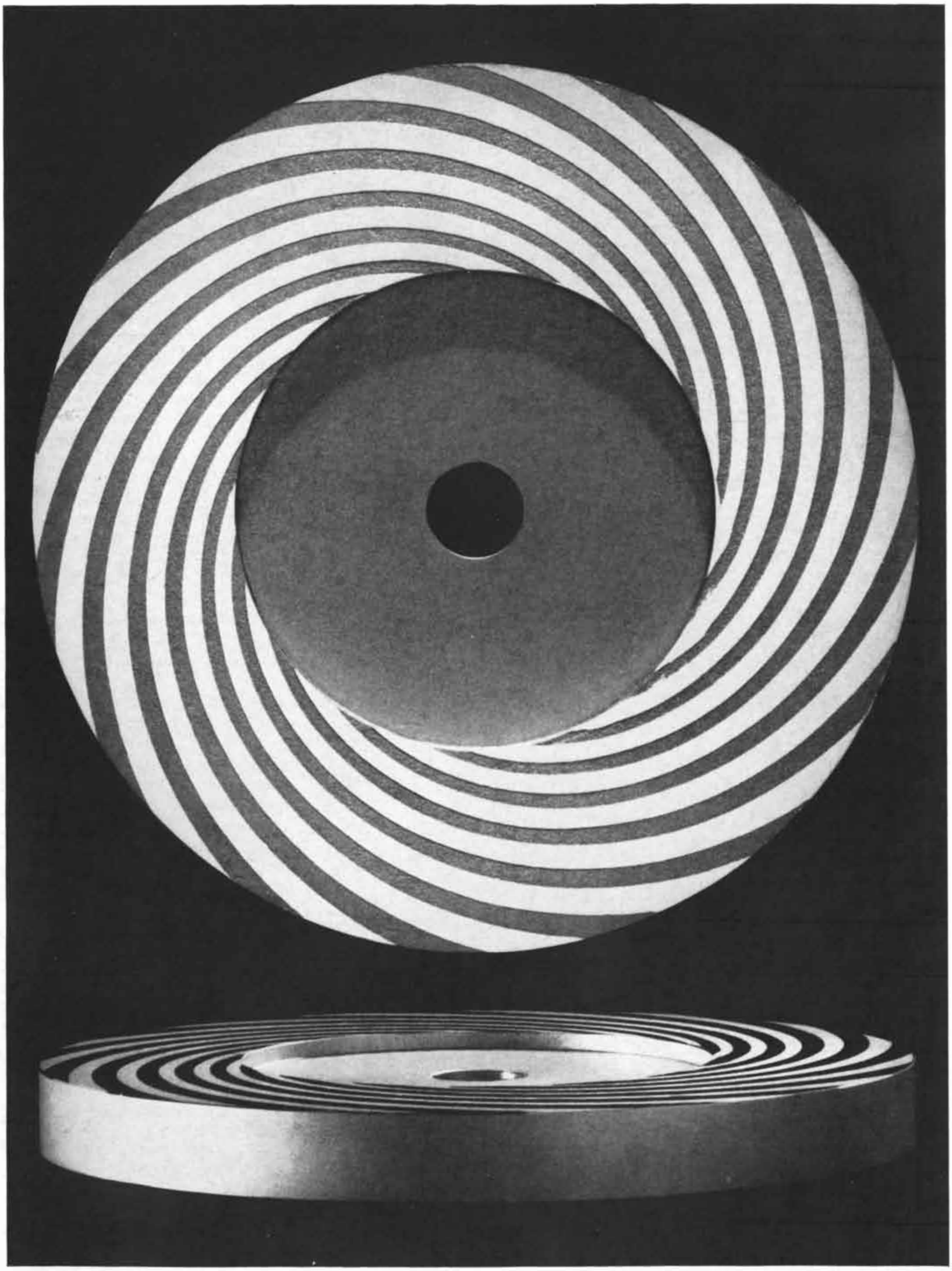
From the standpoint of function bearings can be classified according to the kind of load they carry. A thrust bearing carries a load that is parallel to the axis of the shaft, as is the case with the bearing supporting a merry-go-round. A radial bearing carries a load that is perpendicular to the axis of the shaft, as is the case with the bearings that support the crankshaft of an automobile. An angular bearing is designed to carry loads in which both thrust and radial forces are at work; such bearings are needed, for example, in the wheels of automobiles. I trust it will be evident to the reader that both sliding and rolling bearings can be designed to carry any of these kinds of loads. The same is true of the spiral-groove bearing.

The Friction Problem

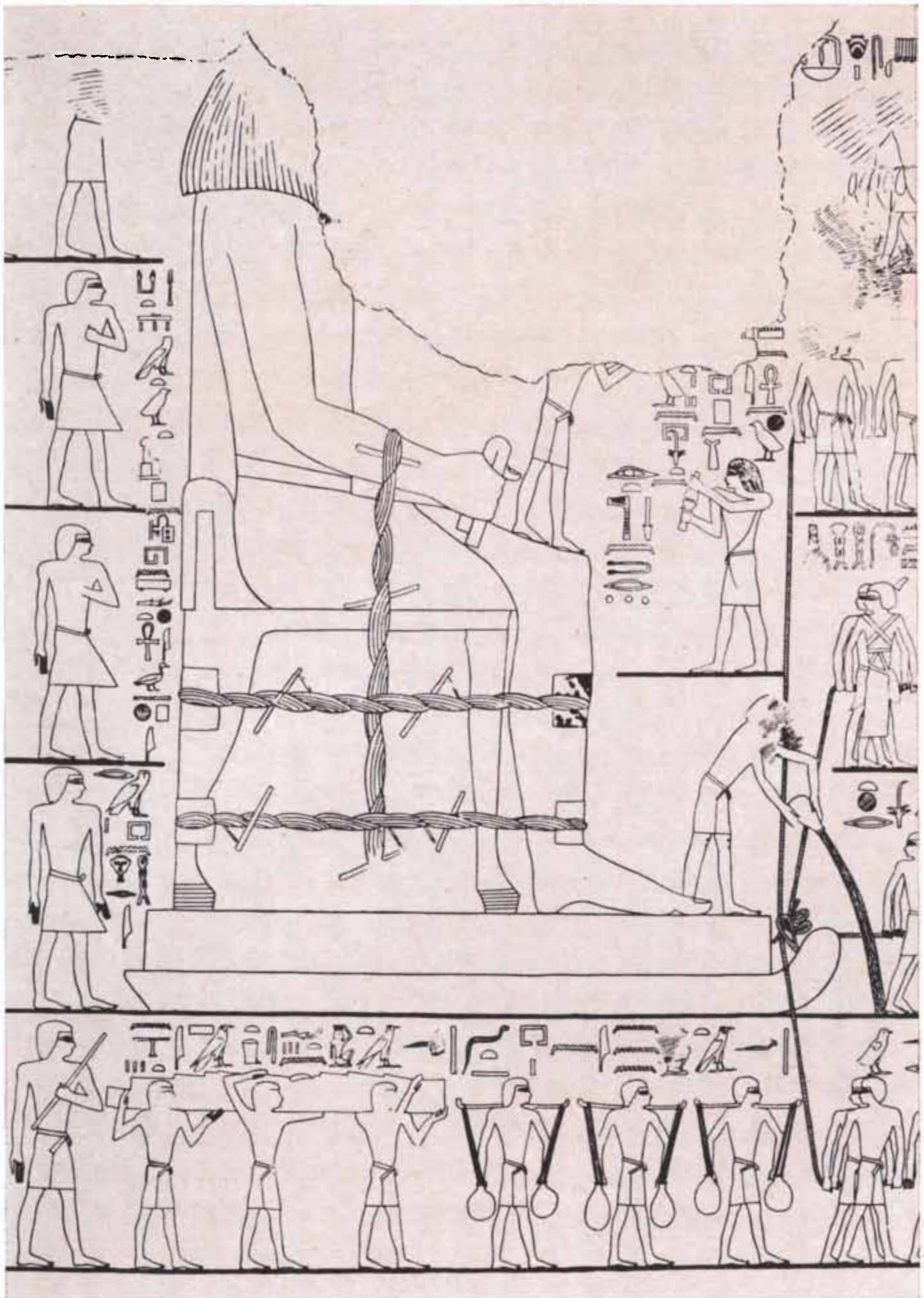
The origins of bearings extend too far back into antiquity to be traced with certainty, but it seems reasonable to assume that the bearing principle arose from efforts to cope with friction. One of the earliest representations of such an effort is a painting on the wall of an Egyptian tomb; it shows men pouring oil under the runners of a sledge carrying a large statue. The invention of the wheel was a crucial step in reducing friction. By shifting the problem to an axle this invention gave rise to the first radial bearings. An arrangement of devices that could be called the forerunner of the ball bearing was found in the remains of an ancient Roman ship raised from a lake in Italy some 35 years ago; the devices evidently were used to help support a turntable while facilitating its movement [*see bottom illustration on page 62*]. In each of these cases, as in most modern instances where one part moves in relation to another, the central aim of the designer was to reduce friction.

It is convenient to consider the fundamental problems of friction associated with bearings under the headings of dry sliding friction and dry rolling friction. Measurements of electrical resistance show that in dry sliding friction two objects seemingly pressed close together actually make contact over an extremely small percentage of their surfaces. Even surfaces specially machined for smoothness touch at only a few microscopic peaks. At these points of contact the pressure is very high, and there the phenomenon of adhesion can give rise to welds between the two surfaces. If the surfaces then move past each other, the welds are broken and re-formed elsewhere.

The amount of surface that is in actual contact depends on two factors: the normal force, which is the amount of load carried by the bearing, and the pressure at which the softer of the materials involved gives way. The frictional force is equal to the amount of surface area in actual contact times the shear strength of the welds. Hence the



SPIRAL-GROOVE BEARING, shown here in bottom and oblique views, is a recent development in bearing technology. This spiral-groove bearing is a thrust bearing: it rotates against a flat circular plate with a raised rim. The hole in the center of the bearing is for mounting a shaft. The function of the grooves is to pump the lubricant, in this case air, toward the center of the bearing and keep the two plates apart. This bearing is 12 inches in diameter. The grooves, which are the dark areas, were made by etching.



EARLY ATTEMPT to cope with friction was depicted in this fragment of a painting in an Egyptian tomb dating from about 1650 B.C. A slave is pouring oil in front of the runners of a sledge carrying a large statue. Presumably it was frictional problems of this kind that gave rise to the idea of bearings, which play a vital role in the reduction of friction.

FORERUNNERS of the ball bearing were found in a Roman ship raised from a lake some 35 years ago. Apparently the balls, which were about two inches in diameter, helped in the support and rotation of a turntable. They embody the basic idea of the ball bearing, which is to reduce friction between two surfaces that must move in relation to each other by putting between them a set of devices that roll. These balls differ from true ball bearings, however, because they have shafts that actually carried the load of the rotating turntable.

frictional force is directly proportional to the normal force.

It has been found in practice that the two surfaces of a sliding bearing should never be made of the same material. If they are, serious wear results from a chain of circumstances. The welds formed at the points of contact are then of the same material as the two surfaces. The strong deformation at the points of contact actually strengthens the material there, so that the breaking and re-forming no longer occur at the welds but in the underlying bulk material; hence the excessive wear. Accordingly the designer of a sliding bearing will make one surface of a weaker material than the other. A typical combination is steel for one surface and a white metal (an alloy of tin, antimony and copper, for example, or lead, antimony and tin) for the other.

Dry rolling friction is of course the phenomenon that occurs in a rolling bearing. The amount of friction involved is notably low, and the exact cause of the friction that does arise has long been a subject of speculation. The question was first investigated by the British physicist and engineer Osborne Reynolds, who in 1875 carried out experiments with a steel ball rolling over a rubber membrane. He concluded that the source of friction was the fact that in moving over a stationary surface a rolling object slips slightly. In his "microslip theory" the slippage resulted from elastic deformations in the zone of contact between the stationary surface and the rolling object.

Closer study has shown that this theory is quite unable to account for the frictional losses actually observed in rolling bearings. In 1954 David Tabor of the University of Cambridge put forward a theory that gave a satisfactory explanation of the losses and that is now generally accepted. The theory is that the losses arise from hysteresis: the fact that when a load is put on a ball or roller and the ball or roller is then moved, the metal does not behave quite elastically. The force is larger in the zone of compression than in the zone of relaxation, and a frictional effect results. Even the best ball-bearing steel exhibits hysteresis.

In dry rolling friction, welds are formed as they are in dry sliding friction, but they are not broken in the same way because in dry rolling friction there is no tangential movement of the touching surfaces. Therefore the kind of wear that can arise in dry sliding friction does not occur Another form of wear, however, is found in dry rolling friction. The loading and unloading of the ball or roller will give rise to metal fatigue. After much rolling there will be a tendency for flakes to peel from the rolling object at points of load, and then the object no longer rolls satisfactorily. This phenomenon is the reason for the finite life of even well-lubricated rolling bearings.

The Role of Lubrication

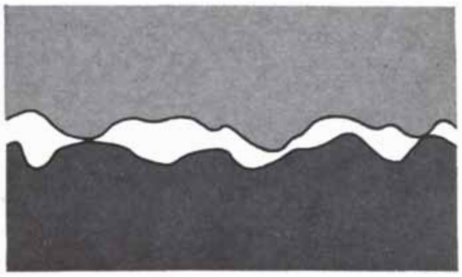
It must have been evident quite early in the evolution of sliding bearings that an application of lard, tallow or some other lubricant to the bearing surfaces made for far easier (and far quieter) operation. It was not until some 80 years ago, however, that the lubrication of bearings proceeded beyond the trial-and-error stage and began to be based on an understanding of the physical principles involved. This development arose after the Institution of Mechanical Engineers in Britain hired Beauchamp Tower to study friction in the journal bearings of railroad cars. As part of his investigation he decided to run a journal in a bath of oil, and he drilled a hole through the outer housing so that oil could reach the journal. He found that when the wheel was turned, oil flowed from the hole. When he attempted to contain the oil by driving a wooden plug into the hole, the plug was continually forced out as the wheel ran.

Tower then substituted a pressure gauge for the plug and measured the pressure generated in the oil. He reported his observations to a meeting of the Institution of Mechanical Engineers in 1883. Osborne Reynolds undertook to analyze the effect, and in 1886 he presented to the group the theory of self-acting lubrication, which is also known as hydrodynamic lubrication. The essence of the theory is that the pressure is generated by the motion itself because the viscous lubricating medium of the bearing is forced through a constriction. In the case of the railroad journal bearing the constriction is the wedge-shaped passage between the surface of the bearing and the bottom of the journal. As a result the journal, which is in contact with the outer housing when the wheel is at rest, is lifted from the housing by this wedge effect when the wheel turns [*see upper illustration on page 65*].

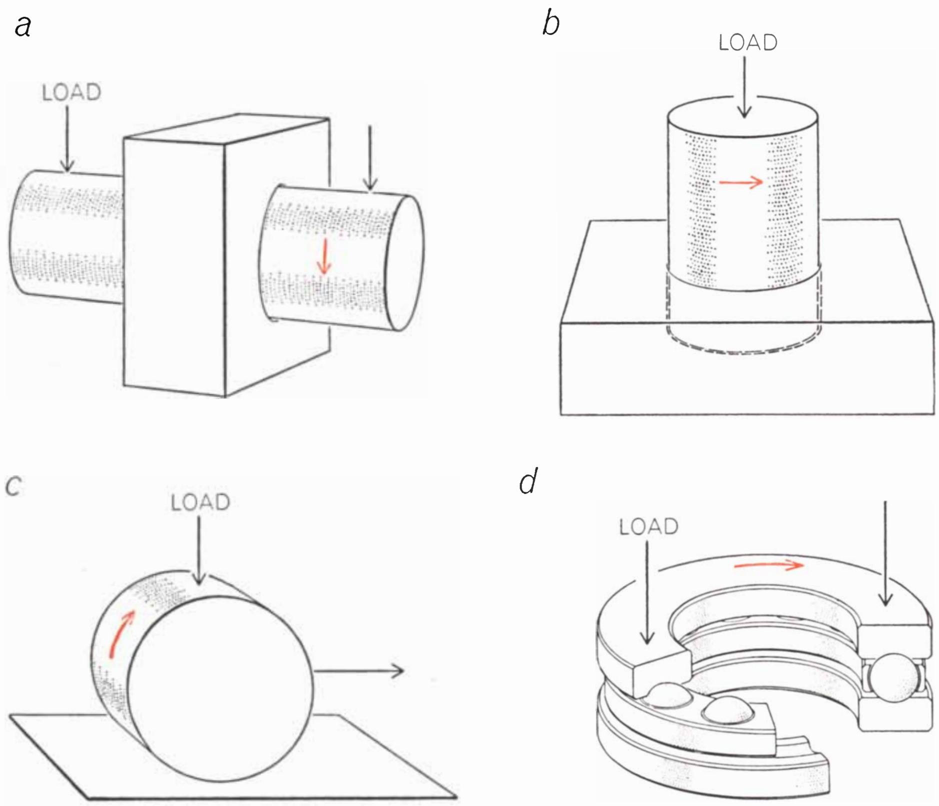
These findings explained why journal bearings, which had been run in oil for many years without any detailed understanding of how the oil reduced friction, were so much more successful than the thrust bearings of the time. The reason was that in the journal bearings there was no contact between bearing surfaces once the journal began to turn rapidly enough to produce the wedge effect. When this was realized, efforts were made to apply the constriction principle to thrust bearings.

The problem of thrust bearings was particularly troublesome on ships, where the propeller shaft takes a large thrust from the force of the propeller pushing against the water. At that time the usual arrangement was essentially a flat rotating plate parallel to and bearing on a similar fixed plate. The rubbing surfaces were lubricated with oil, but under the circumstances there was no wedge effect and wear was severe.

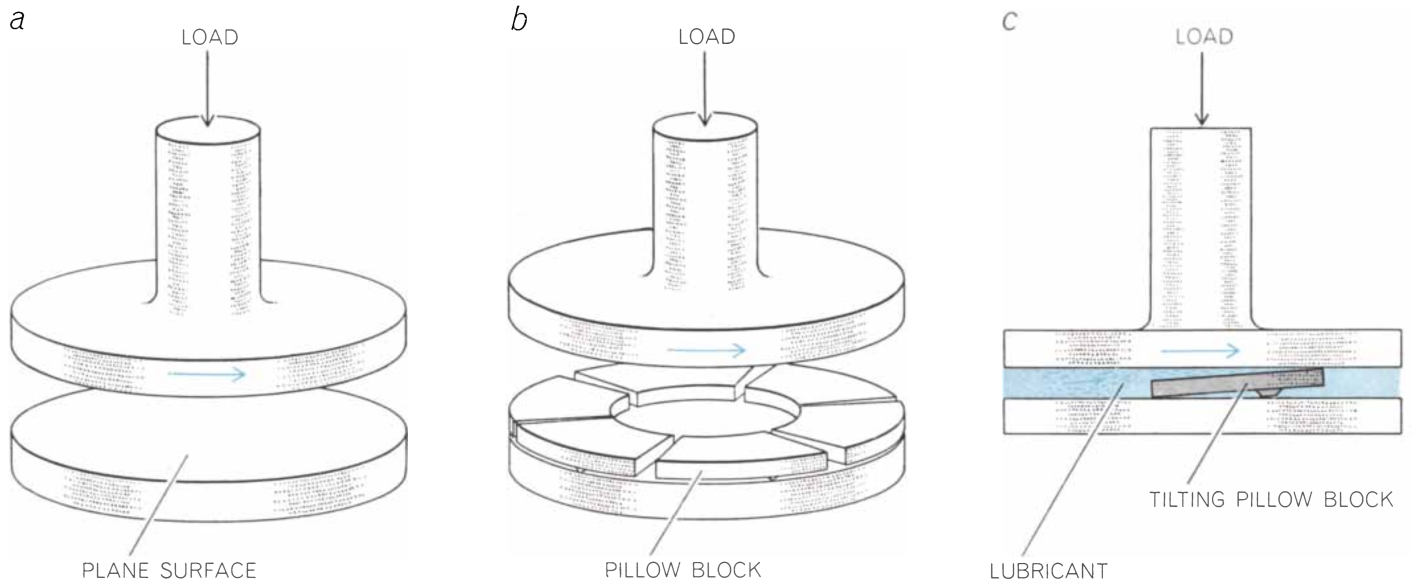
Albert Kingsbury of the U.S. and A. G. M. Michell of Australia addressed themselves to the thrust problem independently and each arrived at the idea of fitting one of the circular plates with a ring of pivoted blocks [*see illustration on next page*]. Each block introduces a wedge-shaped constriction into the space between the plates. If the ridge on which a block pivots is correctly placed, the block will automatically take up under load a position that yields the optimum wedge effect. In this way it is possible to create a complete clearance between the plates. This "tilting-pad principle," as it is often called, underlies the Kingsbury and Michell bearings. A similar but simpler thrust bearing that was developed later is the step bearing. Here rectangular ridges serve



SLIDING FRICTION that occurs between two surfaces even when they are carefully machined for smoothness is depicted. The actual contact occurs at microscopic peaks, here represented with considerable exaggeration. Pressure is high at these points, giving rise to welds between the surfaces.



BASIC PRINCIPLES of sliding and rolling bearings are outlined. At top are two sliding bearings, a type so named because the two surfaces involved have a sliding relation. At *a* is a journal bearing; in terms of load it is a radial bearing because its load is perpendicular to the rotating shaft. At *b* is a thrust bearing, so called because its load is parallel to the shaft. Rolling bearings are based on the small amount of friction that occurs as an object rolls along a surface (*c*); as applied in a typical rolling bearing (*d*), the principle involves placing between the two surfaces an assembly of devices that roll. Such bearings are also widely known as antifriction bearings; this one is a thrust-load ball bearing.



KINGSBURY-MICHELL PRINCIPLE is named for Albert Kingsbury of the U.S. and A. G. M. Michell of Australia, who worked it out independently near the beginning of this century. The problem they confronted (*a*) was that thrust bearings, which usually consisted of two smooth plates, exhibited severe wear even though a lubricant was used. Kingsbury and Michell put tilting blocks (*b*) on one of the surfaces; by this means the lubricant (*color*) was repeatedly forced through a series of wedge shapes, as shown at *c*. This wedge effect built up high pressure in the lubricant, and the pressure in turn achieved a full separation of the two bearing surfaces.

to bring about the constriction that is the essential feature of self-acting bearings.

In some bearings self-generated pressure is unsuitable. A bearing operated at low speed, for example, is unlikely to develop enough pressure in the lubricant to achieve a separation of the bearing surfaces. The starting and stopping of bearings that are under heavy load presents a similar problem; at such times there will be either no pressure—and hence no separation—or insufficient pressure to maintain a separation. The bearing surfaces come in contact under conditions of movement, and again excessive wear results.

Externally Pressurized Bearings

Situations of this kind gave rise to the externally pressurized bearing, in which separation of the bearing surfaces is achieved by means of pressure built up in the lubricant from an outside pump. One of the first devices to make use of this hydrostatic principle was an apparatus exhibited at the Paris Industrial Exposition of 1878. The apparatus was called a *chemin de fer sur glace*—a railroad on ice. It consisted of a heavy block mounted on four lion-like feet that rested on a smooth steel surface. Without lubrication the apparatus was virtually immovable, but when the pump was turned on, pressure built up in the lubricant, a separation was achieved between the feet and the steel surface, and the apparatus slid easily.

This principle of external pressurization has many modern applications. A heavy rotating machine such as a turbine, for example, will have pumps that supply pressure to the bearing lubricant as the turbine starts; when the speed of rotation is sufficient to maintain the separation of bearing surfaces the pumps can be shut off. They are started again as the turbine slows to a stop. A notable example of externally pressurized bearings is provided by the 200-inch Hale telescope on Palomar Mountain: equipment weighing more than a million pounds is turned easily by a small motor because externally pressurized pad bearings virtually eliminate friction.

The reader has probably noticed that in this discussion of the separation effect achieved by a viscous medium I have referred only to sliding bearings. I should not like to give the impression that rolling bearings operate without lubrication. They usually are lubricated, but for other reasons. One reason is the need to counteract attrition of the surfaces. Another reason is the fact that the lubricant helps to dissipate heat. The primary reason, however, is that in a rolling bearing it is necessary to have some means of separating the rolling elements from one another, and inevitably there is sliding friction between the separator (commonly called the cage) and the rolling elements. Lubrication is needed to reduce the friction and wear resulting from the interaction of the rolling elements and the cage.

Rolling bearings are of enormous importance in modern machinery, and I do not mean to slight them. Most of the recent developments relating to bearings, however, involve the kind of bearing I prefer to call contactless. Such bearings operate with a complete separation of the bearing surfaces.

Contactless Bearings

The concept embodied in contactless bearings is often called full-film lubrication because the separation of bearing surfaces is achieved in most cases by the pressure in a viscous medium between the surfaces. I use "contactless" to refer to the full-separation principle not only because that word seems more descriptive but also because it takes into account situations where there is no viscous medium. For example, some specialized applications require magnetic bearings, in which the spinning rotor is supported by a magnetic field.

One of the major developments of the past 15 years has been the use of viscous mediums other than oil in contactless bearings. Several laboratories are working on applications where the lubricant is the liquid or gas being processed by the machine. The viscous medium in these cases can be almost anything; examples that come to mind are steam, molten steel and even applesauce. Such lubricants are advantageous when oil is unsuitable, either because it breaks down under operating conditions or because it can create problems by leaking.

The use of a gas (usually air) as a lubricant in contactless bearings has progressed well beyond the laboratory. One convenient feature of air as a lubricant is that it can be allowed to escape into the surrounding atmosphere; it does not need to be collected for recirculation and so it does not gradually become dirty as recirculating oil does. Moreover, there is seldom the danger that air will contaminate the product. Because oil can leak from bearings no matter how diligently the designer seeks to make them leakproof, oil is undesirable in machinery used for processing food or for manufacturing materials (such as textiles) that would be spoiled by oil. In such cases air lubrication is clearly preferable.

There are still other cases in which lubrication with oil is unsuitable for purely practical reasons, and in which the gas bearing offers a way out of the difficulty. Examples are machines with bearings that must work at unusually high or unusually low temperatures. At high temperatures oil may break down chemically; at low temperatures it may congeal. Another example is a machine, such as a nuclear reactor, that operates in the presence of ionizing radiation; oil decomposes under the influence of radiation.

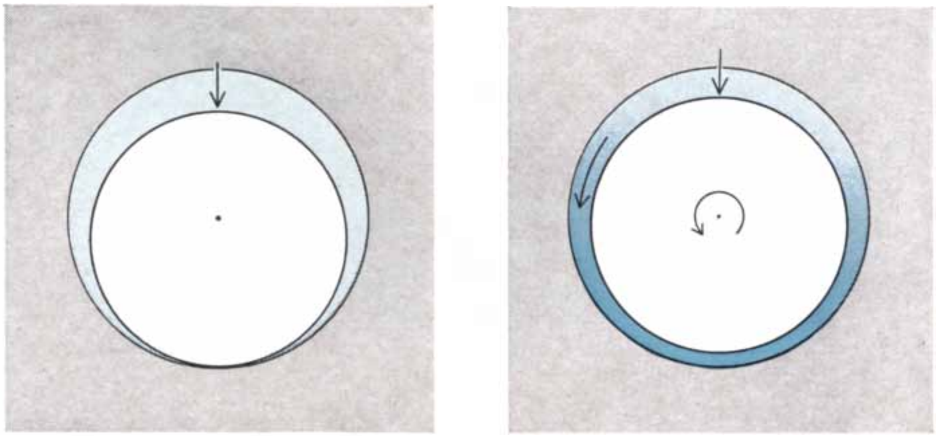
Air is particularly suitable for the bearings of shafts that have a large diameter and operate at high speed. The combination of large diameter and high angular velocity produces high relative velocities in the bearing surfaces, which in turn creates a problem in getting rid of heat generated in the viscous medium by the shearing effect. Air, with a viscosity three or four orders of magnitude lower than that of oil, gives rise to less heat than oil in this shearing process.

A small experimental rotor designed in the Philips laboratory provides an indication of the high-speed possibilities of air-lubricated bearings. This rotor, which is driven and lubricated by compressed air, can reach speeds of about 450,000 revolutions per minute without overheating the bearing. The latest types of dental drill are based on the same principle. Similarly, some gyrocompasses and machines of the turbine type are provided with gas bearings because of the high speeds at which they operate.

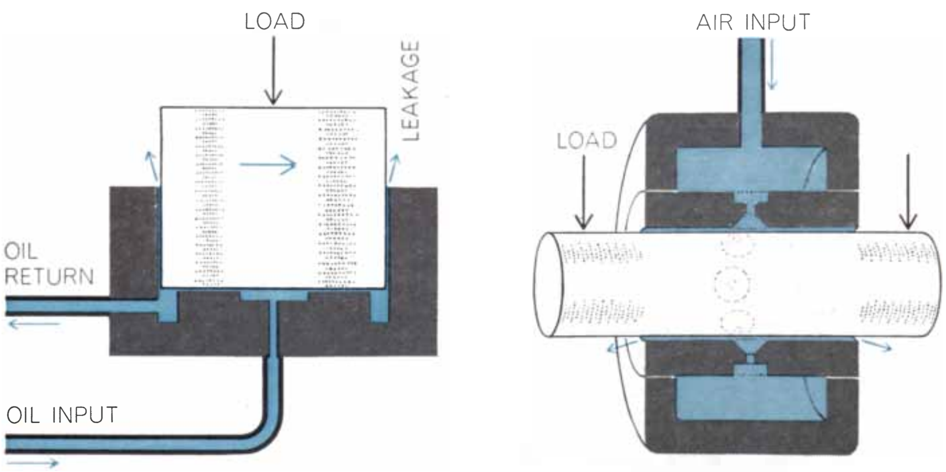
An interesting class of applications for gas bearings is that in which the need is not so much low friction as it is constant friction. Such needs arise particularly when sensitive measurements are desired; variations in friction will produce variations in the measurements. In our laboratory we have built a device to measure the torque exerted by a motor of fractional horsepower. The torque to be measured is very small compared with the frictional torque that would be produced if the rotating brake of the measuring device were mounted on ball bearings, and the inevitable small variations in this frictional torque would make the measurements highly unreliable. With air lubrication the friction in the bearing is constant and the problem is overcome.

This same principle is applied for quite a different purpose in computers. Here the problem is to maintain a constant and exceedingly small separation (one to five microns) between a magnetic head and the memory drum from which the head takes information (or to which it delivers information). Essentially the moving drum is one surface of a bearing and the head is the other. The separation is maintained by air, with the wedge effect achieved between the round surface of the drum and the flat surface of the head. In the air-lubricated bearing that I described earlier the maintenance of a given separation between bearing surfaces was only a consequence of achieving contactless operation; in the computer the constant clearance is the objective. It was for this application, incidentally, that large numbers of gas bearings (air-lubricated, self-acting Michell bearings) were used for the first time.

Of all the various bearings the kind with full-film lubrication performs best in terms of low wear and freedom from vibration. Often, however, there are



WEDGE EFFECT was discovered by the British engineer Beauchamp Tower when he ran a railroad journal bearing in an oil bath. The situation is shown here in exaggerated form. When the journal, or shaft, is at rest (*left*), it is in contact with the surface on which it bears. When the shaft turns (*right*), the lubricant (*color*) is forced into the wedge shape below the shaft and a pressure sufficient to lift the shaft from the bearing is thus generated.

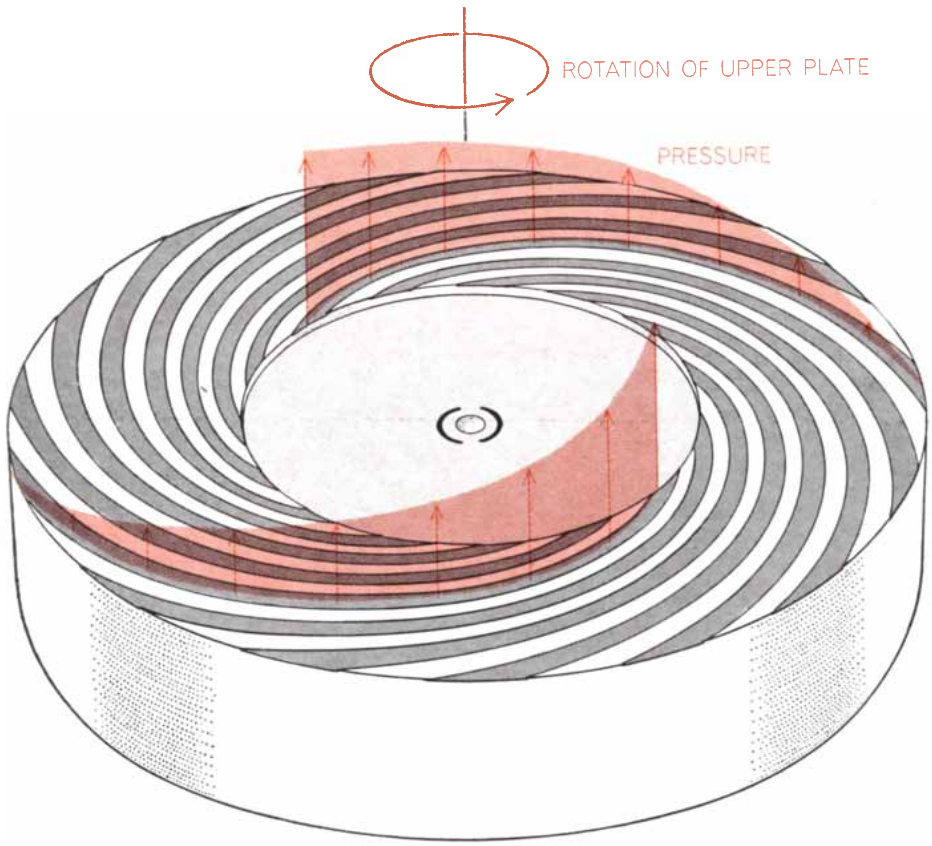


EXTERNAL PRESSURE is used to achieve a separation of bearing surfaces when the wedge effect is inoperative because of such factors as heavy load and low speed. The lubricant is given pressure by an outside pump. At left is an externally pressurized bearing in which oil is the lubricant; at right, one in which air supplied through a series of orifices in the shell around the bearing provides the separating effect. The broken ellipses represent three orifices behind the shaft. Two orifices are shown in section at top and bottom.

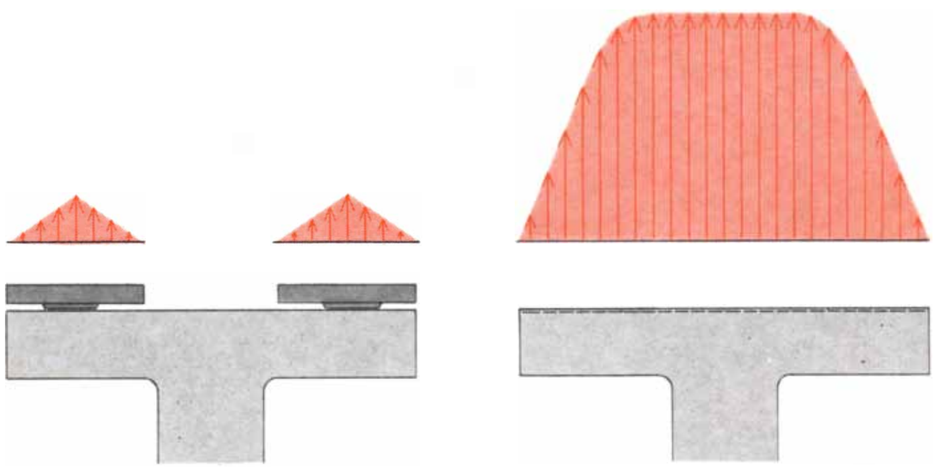
reasons why such bearings—in particular thrust bearings of the Michell and the step type—cannot be used. The coefficient of friction may be too high or the load-carrying capacity too low, or the cost of providing the seal that maintains a bath of oil around the bearing may be too high. Moreover, it is difficult to make Michell bearings with an external diameter of less than 20 millimeters. This in general terms was the situation that caused us in 1960 to begin the investigations that led to the spiral-groove bearing.

The specific problem we confronted was whether or not it would be possible to make a wear-resistant thrust bearing able to take up to about one kilogram of thrust from a shaft rotating at a speed of 50,000 revolutions per minute. If possible, the power consumption was to be held under 1.5 watts. The bearing was to support the end of the shaft and had to be suitable for operation in a vacuum.

Rolling bearings were out of the question, both because of the wear they undergo and because the frictional losses would certainly be excessive. Dry-sliding friction had to be excluded for the same reasons. This narrowed the field to contactless bearings, in particular to the self-acting kind, because in an externally pressurized bearing the pump alone would account for more than two watts of power consumption. A self-acting Michell bearing was a possibility, but in the circumstances it would have had to operate with unusually small clearance between the bearing surfaces. We were doubtful about the possibility of building a Michell bearing with such a small clearance.



PRESSURE BUILDUP in a spiral-groove bearing is from the perimeter to the center. This effect accounts for a significant difference in performance between a spiral-groove bearing and a Kingsbury-Michell bearing; the difference is shown in the illustration below.

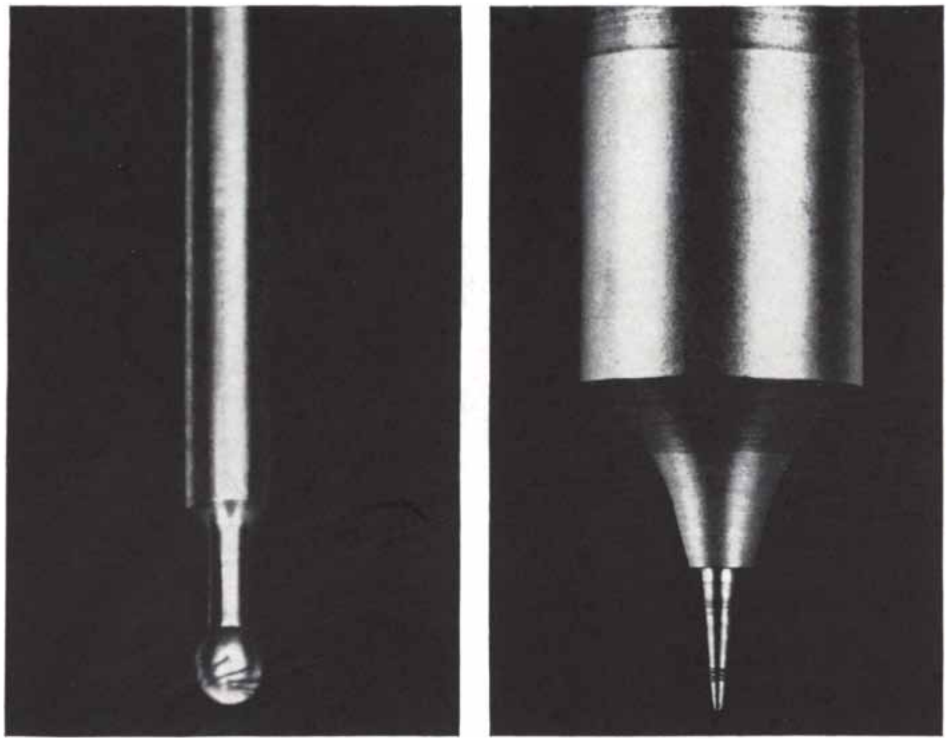


PRESSURE DISTRIBUTION in a Michell bearing and a spiral-groove bearing are compared. In a Michell bearing (*left*) the pressure builds up over the center of each pad; in a spiral-groove bearing (*right*) it builds up over the entire face. By this means the spiral-groove bearing achieves a larger load-carrying capacity than a Michell bearing of the same size; in the Michell bearing there is no pressure at the center because there is no relative velocity between bearing surfaces there. To put the matter another way, the spiral-groove bearing has proportionately lower friction than the Michell bearing for a given load.

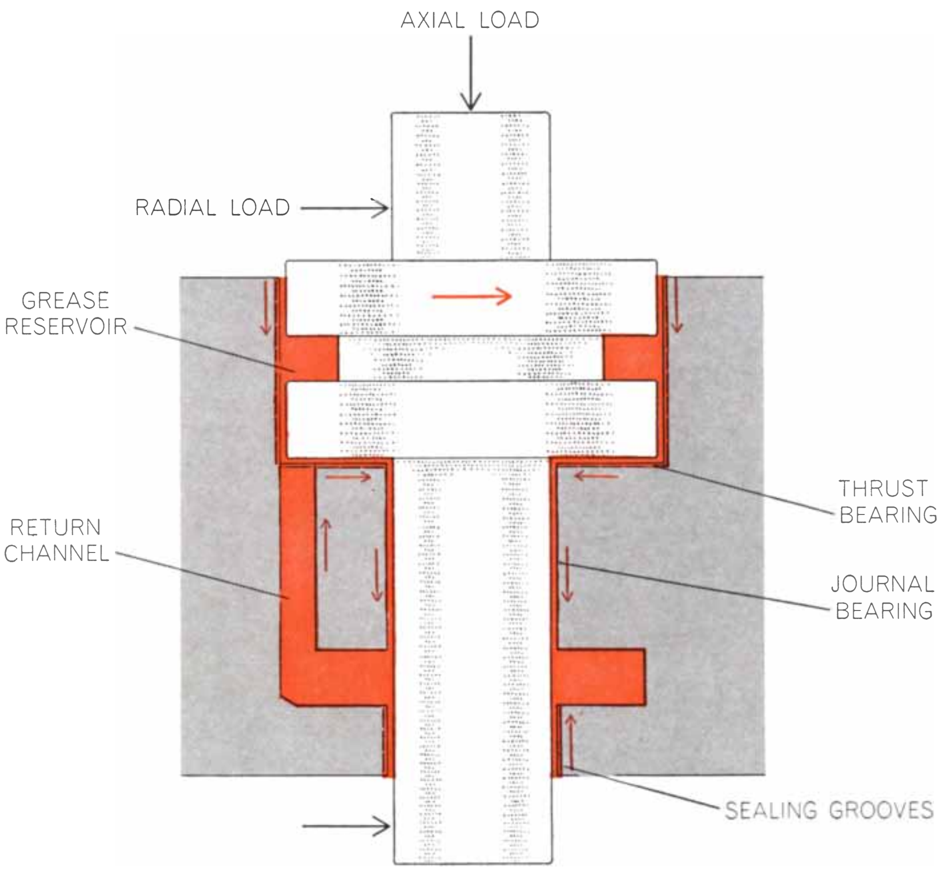
Spiral-Groove Bearings

Accordingly we had to look for a bearing with a different kind of geometry. It was in this situation that H. Rinia of our laboratory suggested an arrangement whereby a smooth circular plate would rotate in relation to a similar plate in which "a recurrent pattern of shallow grooves" had been cut. Pursuing this suggestion, I began to investigate the matter. I soon found that we were involved in something of a rediscovery; earlier investigators had suggested the theoretical possibilities of such a design. None, however, had carried the idea through. We have done that by working out the theoretical calculations and building a variety of bearings. As it turns out, the form of groove that works best is a spiral [*see illustration on page 61*]. We have found that spiral-groove bearings have an advantage over other bearings not only when gas is the viscous medium but also with oil and particularly with grease.

The basic idea was that the grooves would have a pumping effect on the viscous medium. The effect would arise as the plates rotated in relation to one another, and it should pressurize the entire area inside the groove region from the perimeter to the center. The pumping effect would be balanced by a leaking effect: from the center of the bearing the viscous medium would escape around the shaft, in the case of



FURTHER DEVELOPMENT of the spiral-groove bearing is represented by the spherical design (*left*) and a conical design (*right*). The former is three millimeters in diameter; the latter, one millimeter. Kingsbury-Michell bearings cannot be made that small; the spiral-groove bearings also carry larger loads with lower friction and can take some radial load.



GREASE-LUBRICATED BEARING, designed by the author and his colleagues, is able to take both axial and radial loads in a situation where the shaft passes through the bearing. The axial load is carried by a spiral-groove thrust bearing and the radial load mainly by a standard journal bearing. In operation the spiral grooves pump grease inward; it leaks down through the journal bearing to the chamber at bottom. Groove patterns in the thrust and journal bearings provide pressure for return of the grease. No relubrication is needed.

oil and grease to a collecting chamber and in the case of air to the outside. The balance of pumping and leaking effects would result in a separation of the two plates.

The situation can be summed up by pointing out two important differences between a Michell bearing and a spiral-groove bearing. In the Michell bearing the pressure at the center is zero because the relative velocity is zero at that point. In the spiral-groove bearing the pumping action results in a pressure distribution that shows a sharp increase along a line drawn from the perimeter to the center [*see lower illustration on page 66*]. Moreover, the spiral bearing produces an uninterrupted pressure pattern whereas the pressure pattern in the Michell bearing is essentially interrupted. For these reasons the spiral-groove bearing has a much larger area contributing to the load-carrying capacity—and yet the increase in area does not entail any large increase in frictional loss. In short, the spiral-groove bearing has a lower coefficient of friction.

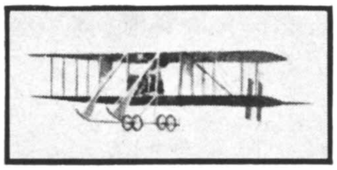
In time we built a spiral-groove bearing; it was about four inches in diameter. We found that turning the upper plate by hand at a speed of only one revolution per minute sufficed to build up a layer of air that completely separated the two bearing surfaces. This bearing demonstrated the practicality of the spiral-groove principle, but it left us with much work to do to achieve an optimum performance.

One of our tasks was to find the best geometry for the grooves. Specifically that meant finding the optimum values of the angle the logarithmic-spiral configuration of the grooves makes with the radius of the plate, together with the optimum ratios of groove width to ridge width, film height to groove depth, and sealing region (the ungrooved area) to grooved region. We have calculated these values for a large variety of spiral-groove bearings.

Evolution of the Principle

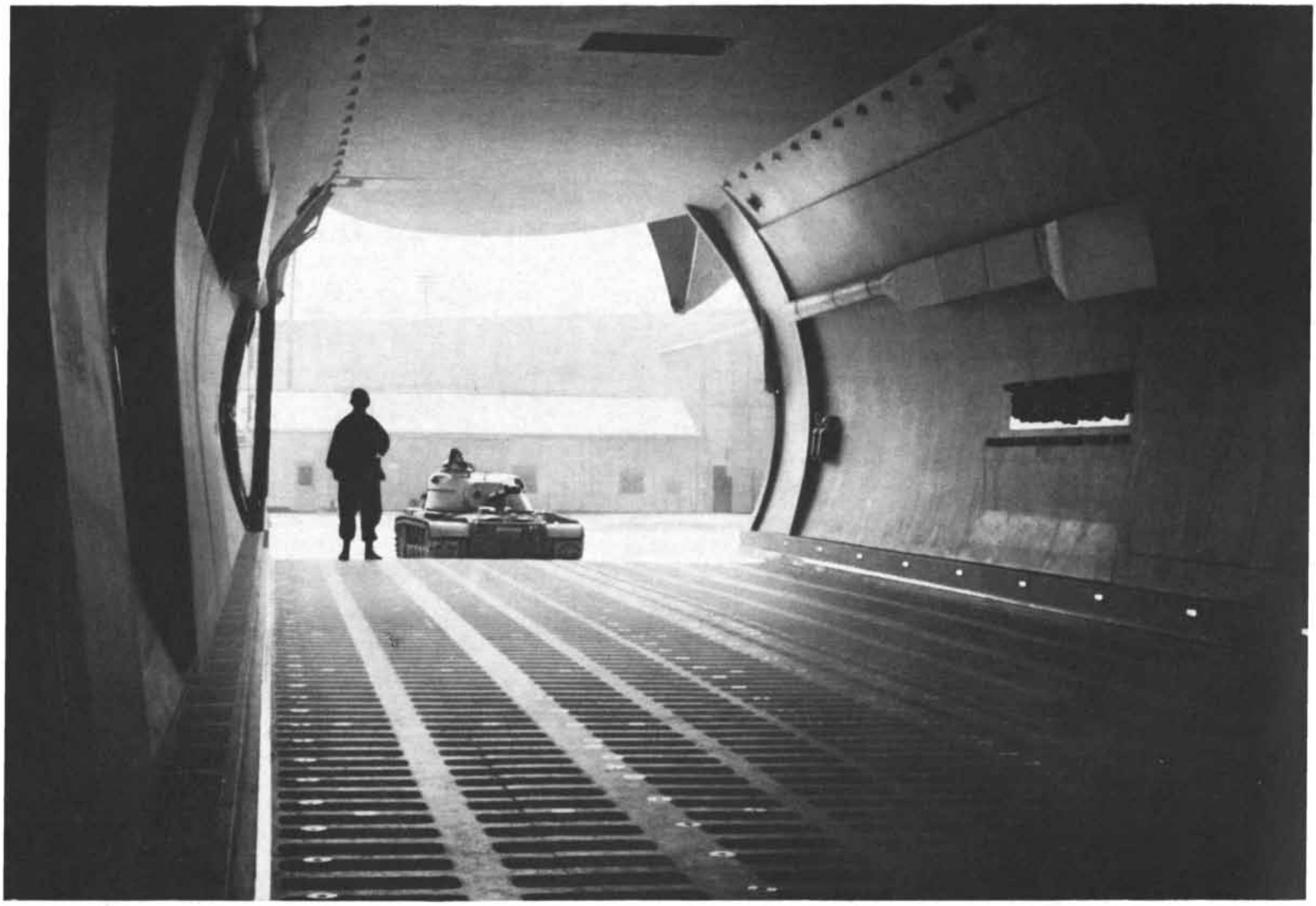
The spiral-groove bearing as it has evolved in these investigations has all the properties of other contactless bearings embodying the concept of full-film lubrication. In addition it has a low coefficient of friction, a high load-carrying capacity and notably simple construction. Moreover, it can be made in very small sizes.

We have achieved a further increase in the load-carrying capacity of the



YOU HAD TO MISS THE FIRST COAST-TO-COAST FLIGHT

BUT YOU DON'T HAVE TO MISS THE C-5A



In 1911, a young man named Calbraith Rodgers opened a new era in transcontinental aviation. By successfully completing a 3,220 mile flight from New York to Pasadena, with 69 stops en route, Rodgers became the first man to fly coast-to-coast. Rodgers' flight opened one era in aviation, the C-5A opens another—the new era in global aviation. ☐ Challenges were great in these early days of flight. Opportunities were big. They are even bigger today. ☐ USAF's C-5A transport offers opportunities unmatched in the aerospace industry. C-5A is the biggest thing to come along in years. An aviation giant, this new transport will fly coast-to-coast with a 220,000 pound payload—almost three times the carrying ability of present airlifters. ☐ To engineers, the C-5A offers long term careers, challenging assignments, and plenty of room to grow. Don't let an opportunity this big pass you by. Find out for yourself. Get in at the start of aviation's new era. Send your resumé to Charles E. Storm, Professional Employment Manager, Lockheed-Georgia Company, 834 West Peachtree Street, Atlanta, Georgia, Department 3-SA. Lockheed is an equal opportunity employer.

C-5A

LOCKHEED-GEORGIA

GIVE YOUR CAREER THE BIGGEST LIFT OF ALL

Lockheed-Georgia Company, Marietta, Georgia: A Division of Lockheed Aircraft Corporation spiral-groove bearing by redesigning the bearing surface in spherical form [*see left part of top illustration on page 68*]. Indeed, the load can be as much as 25 times that of a Michell bearing with the same diameter. Spherical spiral-groove bearings have several advantages apart from being able to sustain higher loads. They are simple to make and so lend themselves to mass-production methods. They are self-adjusting. They can be remarkably small; we have made one with a diameter of about three millimeters. Finally, they can to some extent take up radial as well as axial loads. This last characteristic has been provided in even greater degree by making spiral-groove bearings in conical form. With this development, the possibility arises that air-lubricated spiral-groove bearings can replace the ball bearings in gyroscopes, where ball bearings present problems of wear and slight variation in frictional torque. Attempts toward such a substitution are now under way.

An unexpected and interesting possibility that has engaged much of our attention in recent years is that there is much to be gained by lubricating spiral-groove bearings with grease. The reason is that the spiral-groove bearing forces the grease into the space between the bearing surfaces instead of forcing it out, as all other bearings do. I was led to the discovery of the grease-lubricated spiral-groove bearing by repeatedly being confronted with the limitations of "porous" bearings, a kind widely used for journals with a diameter of a quarter-inch or less. A porous bearing is made of a porous metal that serves two functions: it retains oil like a sponge and it has high resistance to wear during starting and stopping. The purpose of these bearings, which are used by the millions in machines such as domestic appliances, is to avoid the necessity of relubrication and to allow the bearing to function on the full-film principle without special provisions for maintaining the lubricant bath.

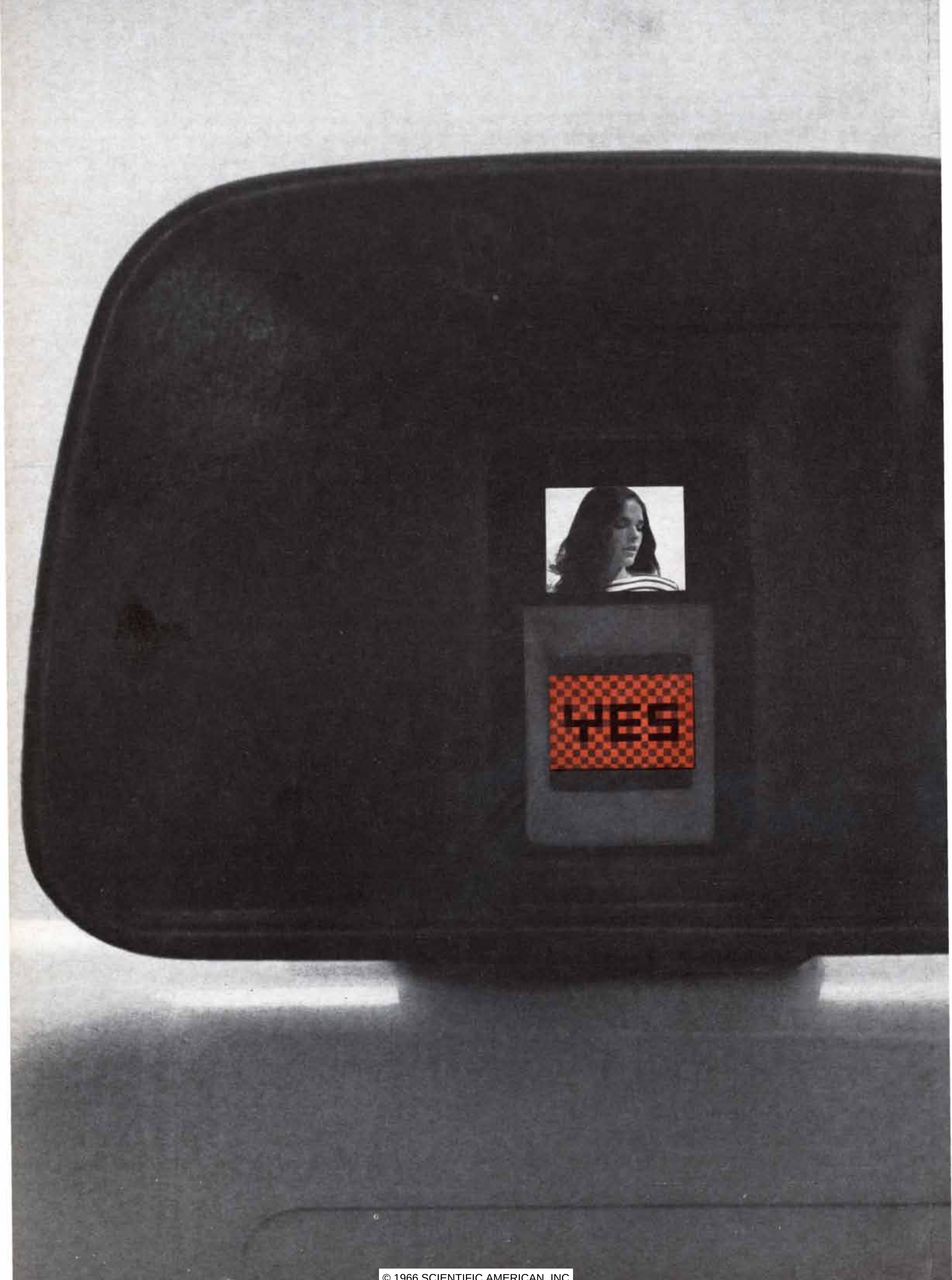
There is no doubt that porous bearings perform their function well, but they do have some limitations. The desirable pressure built up by the wedge effect is reduced by leakage of the oil through the porous material. Moreover, the space between the bearing surfaces does not remain filled with lubricant; the oil leaks sideways out of the slit at the site of maximum pressure and is flung off by the rotating spindle. Eventually such a bearing goes dry and becomes useless.

The grease-lubricated spiral-groove bearing overcomes these problems. Moreover, since it uses grease instead of oil, only simple sealing measures are needed. We expect that the grease-lubricated spiral-groove bearing, designed to take both axial and radial loads, will avoid all the disadvantages of the porous bearing while being competitive in price. Indeed, I have obtained excellent results testing five-millimeter grease-lubricated spiral-groove bearings in electric motors originally fitted with porous bearings. The motors have run some 13,000 hours, and the bearings have withstood 200,000 starts and stops under load. The bearings continue to operate under conditions of full-film lubrication.

These results have convinced me that grease-lubricated spiral bearings can replace ball bearings in more heavily loaded motors where porous bearings are out of the question because of the limits on their load-carrying capacity. We have tested this idea with good results on a motor that runs at 20,000 r.p.m.; its two ball bearings were replaced by two five-millimeter conical grease-lubricated spiral bearings. In such an application spiral bearings offer the advantage over ball bearings of long life without the need for periodic inspection.

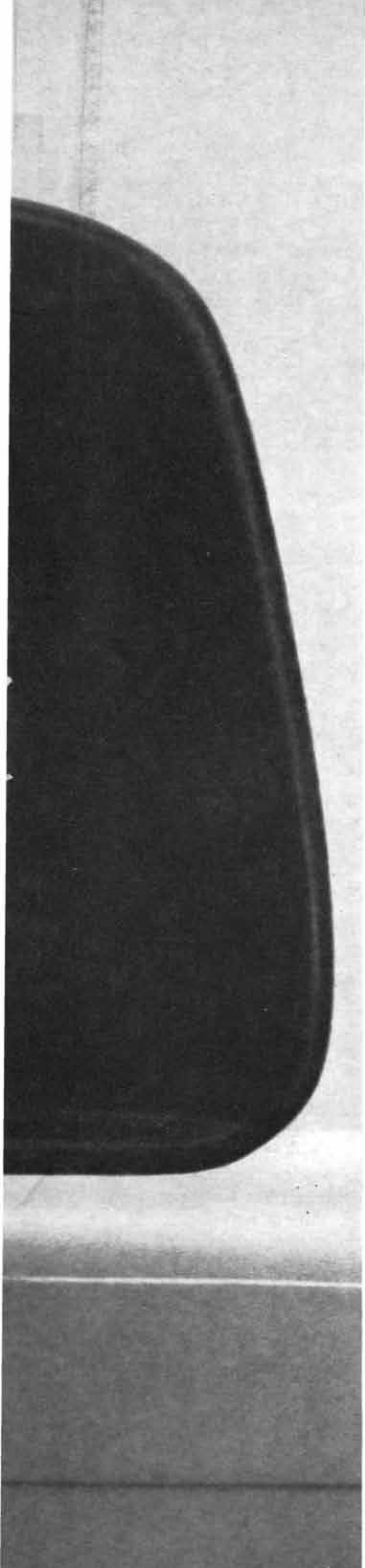
Thus far I have spoken only of spiral-groove thrust bearings. We have also found that a spiral-groove bearing can be used in a situation where the shaft passes through the bearing. The arrangement we devised for this situation involves a combination of a spiral-groove bearing and a standard radial bearing [*see bottom illustration on page 68*].

There will always be a wide range of applications for rolling bearings, particularly when heavy loads have to be carried with a low coefficient of friction at low speeds and the bearing must accommodate both clockwise and counterclockwise rotation. I think, however, that grease-lubricated spiral-groove bearings will prove to be better and cheaper than rolling bearings in many applications. I can also see the possibility, arising from the noiseless and contactless operation of the grease-lubricated spiral bearing, that it will replace certain jeweled bearings—not in watches but in larger and higher-speed devices where the noise and material-to-material contact of jeweled bearings can be troublesome. These and other ideas await further scrutiny and the test of experience.


YES

Polaroid ®

Polaroid has a camera that can talk.



This is The Swinger, the new low priced Polaroid Land camera.

For twenty years we have dreamed of making a camera that would not only deliver a picture instantaneously, but would also be in focus without focusing, that would stop the motion of the subject and be nearly indifferent to the motion of the photographer, that would weigh not much more than a pound and that would give perfect exposure.

The Swinger is that camera.

One of the elements of the dream was precise exposure control. The electronic photometer that controls the automatic shutters on our Color Pack cameras was far too costly for this camera.

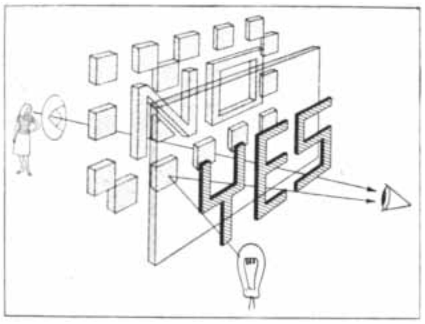
Split-field photometers, with one side of an edge illuminated by a standard light source, and the other side of the edge illuminated by the light to be measured, are inherently accurate. But their large batteries, rheostats and voltmeters make these professional instruments too heavy.

Their cost is ten or twenty times the cost of our camera.

Nevertheless, we were impressed by the inherent simplicity and reliability of this kind of photometry. It occurred to us that progress in design has led to batteries which maintain their voltage accurately over long periods, and to minute electric bulbs imposing on the batteries so little demand for current that in combination the bulbs and batteries could be used without a voltmeter or rheostat as a standard of brightness.

If we used this constant source to illuminate half of the photometric field, we could vary the brightness of the other half of the field by an aperture that was an integral part of the diaphragm blades. We found that while technically trained people could use this photometer readily, the classical matching operation is difficult for the untrained and strenuous in the environment of ordinary picture taking. We then assigned ourselves the task of going on from this type of photometer to one which would give an affirmative response, the actual word YES, when the correct setting was made.

In this photometer, the light from the tiny bulb goes on when you squeeze the exposure-control knob. The amount of subject light admitted through the photometer aperture can be regulated by turning the knob. Until the two intensities are matched, you read NO in the viewfinder window. When they're made equal—by turning the knob—you read YES. And when you see YES, the correct lens aperture has been set for you (the photometer aperture and lens aperture are linked so that when one opens, the other opens proportionally).



The YES/NO target resulted from our studies on the nature of lightness and our application of camouflage technique. By printing the word YES lightly on the NO, we reached the following situation. When the NO is present, illuminated by the light from the bulb, the YES although physically "there" does not appear. When the NO is balanced out, as the scene light is increased by opening the aperture, the YES though lightly printed is the lowest member present on the lightness scale and therefore *becomes dark for the eye.*

For the photographer, the operation is reduced to—aim, turn the knob until YES appears, snap the shutter, and wait ten seconds for a perfectly exposed picture.

Price of The Swinger...$19.95.

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THE NERVE AXON

The fiber that conducts the nerve impulse is a tubelike structure. Its operation can be studied by squeezing the contents out of the giant axon of the squid and replacing them with various solutions

by Peter F. Baker

Axons are the communication lines of the nervous system, and along them message-bearing electrical impulses travel from one part of the body to another. They are sometimes compared to electric cables, but they do not carry an electric current the way a wire does. Whereas in a copper wire electricity travels at a speed approaching the velocity of light, in an axon an impulse moves at only about 100 meters per second at best. The interior of an axon is about 100 million times more resistant to the flow of electricity than a copper wire is. Moreover, the membrane sheathing the axon is about a million times leakier to electric current than the sheath of a good cable. If the propagation of electricity in an axon depended on conduction alone, a current fed into it would die out within a few millimeters. The fact is that the axon propagates a current not by simple conduction but by means of a built-in amplifying and relay system.

How does the system work? This is a central question in the investigation of the nervous system, and it has long intrigued physiologists. This article is primarily an account of one of the new techniques developed for investigating the process of nerve-impulse conduction. The technique can be described briefly as experimenting with perfused axons.

The electrical activity of an axon is based on the interaction of three elements: (1) the fluid contents of the axon, called the axoplasm, (2) the membrane that encloses these contents and (3) the outside fluid that bathes the axon. The key to the axon's propagation of an impulse lies in the membrane. Essentially our technique consists in emptying the axon of its contents and perfusing the emptied tube with various experimental solutions, the object being to determine just what factors are required to make an electric current travel along the axon.

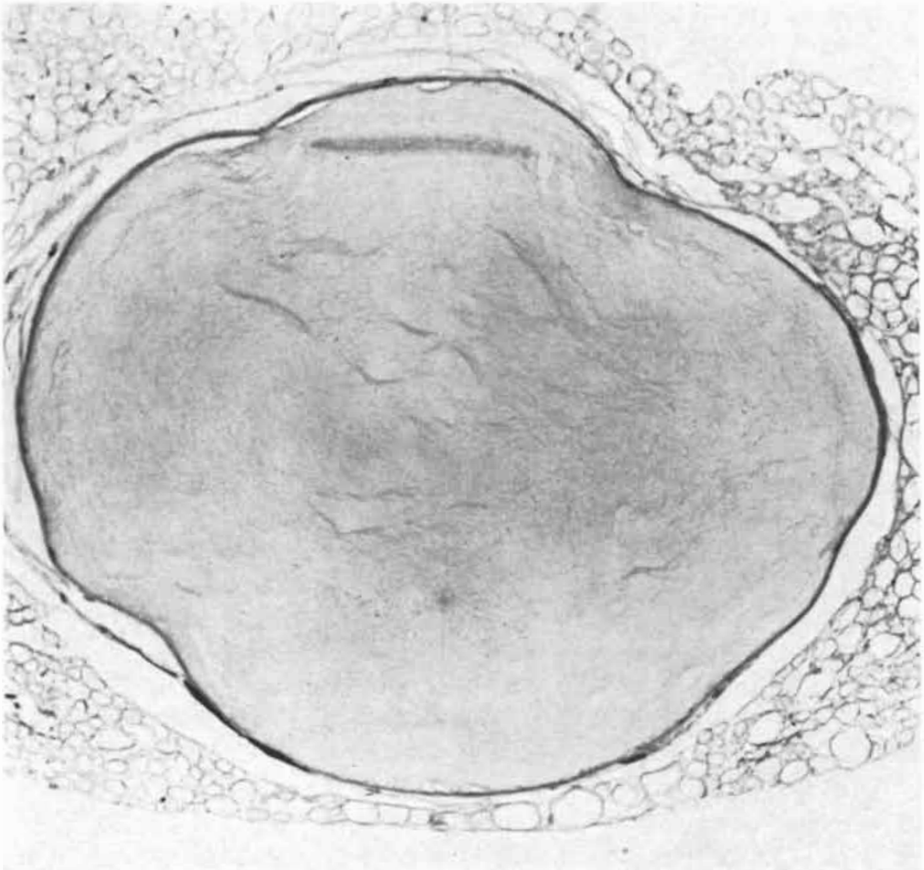
These studies, like many others that have been done on the transmission of nerve impulses, are made possible by that remarkably convenient gift of nature: the giant axon of the squid. This axon measures up to a millimeter in diameter. The axons of human nerve cells, in contrast, are only about a hundredth of a millimeter in diameter. For the squid the giant axon represents an adaptation to a vital need in its particular way of life. For the physiologist this axon provides an ideal experimental preparation. All the available evidence suggests that experimental results obtained with squid axons are applicable to all other nerve fibers.

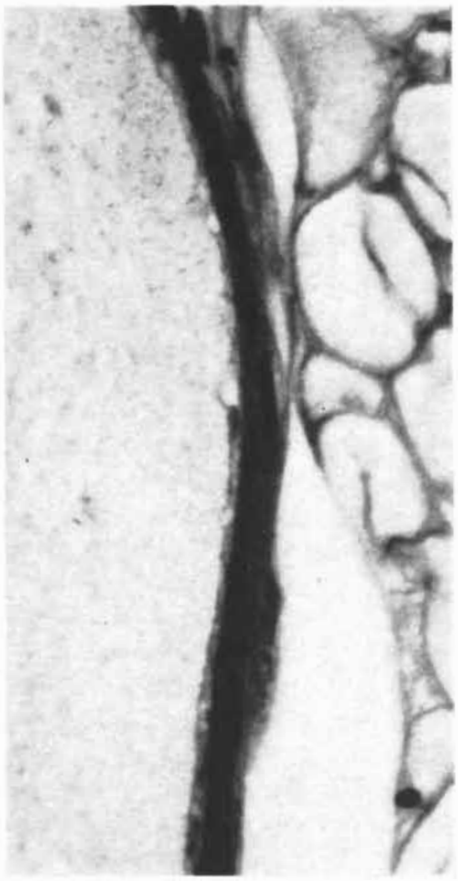
Why some animals and not others have evolved giant axons is not fully understood, but such axons appear to be involved in escape responses. The giant axon of the squid is part of the mechanism by which the animal flees its enemies in the water. In order to dart away rapidly the squid uses a jet-propulsion system, squirting water out of a tube at one end of its body. This calls for the synchronous contraction of muscles located throughout its body mantle, and therefore all those muscles must receive the message from the brain simultaneously. The device that takes care of this timing is a variation in the size of the axons radiating from the brain to the various muscles. The farther the muscle is from the brain, the thicker is the axon leading to it, and experiments have shown that the thicker the axon, the faster it conducts impulses. Hence the diameter of the axon is adjusted to the length of the route to be covered, and this ensures that the signal will reach all the muscles at the same time.

The giant axon is easily dissected out of the squid. It is by probing the isolated axon with microelectrodes and by other means that Kenneth S. Cole in the U.S., A. L. Hodgkin, A. F. Huxley and Bernhard Katz in Britain and other investigators have since 1939 developed an outline of the main events that take place when an electrical impulse passes along a nerve fiber. Much of this story has already been related in *Scientific American*. I shall only briefly review the principal features of the picture.

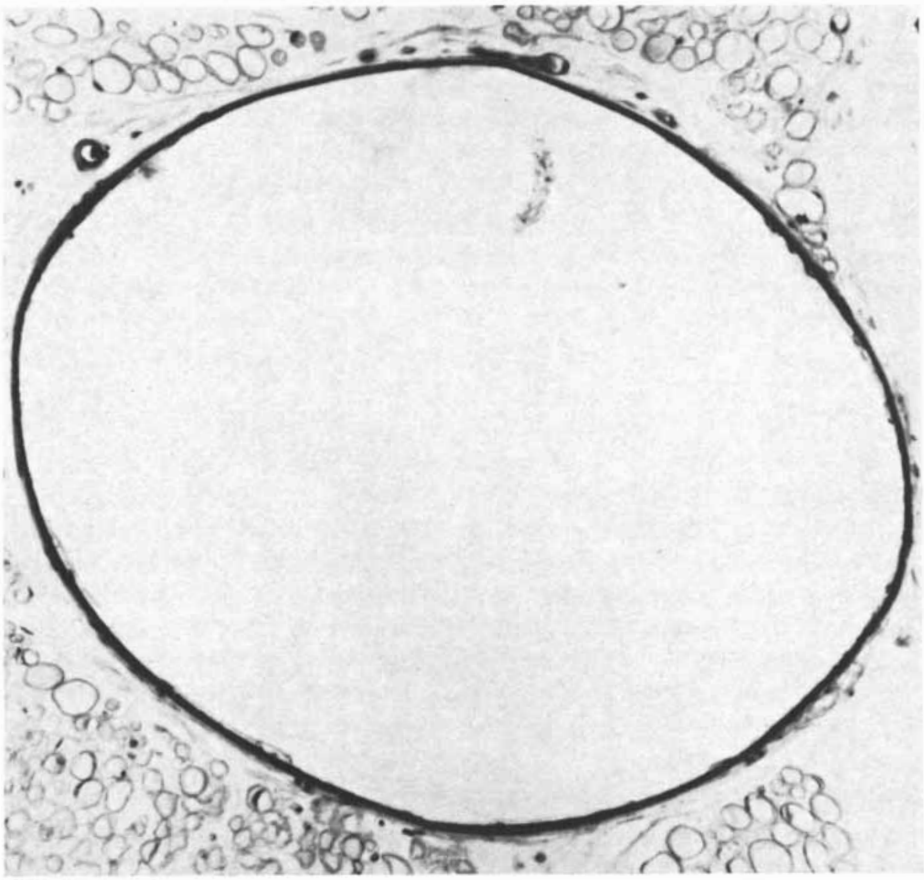
Between the axoplasm inside the axon and the fluid bathing the axon on the outside there are distinct chemical and electrical differences. Chemically the axoplasm is distinguished by the presence of various organic molecules and a comparatively high content of potassium. The outside fluid, on the other hand, is quite similar to seawater: it principally contains sodium ions and chloride ions. The concentration of potassium is about 30 times higher inside the axon than outside; the concentration of sodium is about 10 times higher outside than inside. Because of these differences potassium ions tend to leak out of the axon (by diffusion) and sodium ions to leak in, to the extent that the membrane will allow the ions to pass. (We can disregard the axoplasm's organic molecules and the outside fluid's chloride in this connection, because the membrane is highly impermeable to them.)

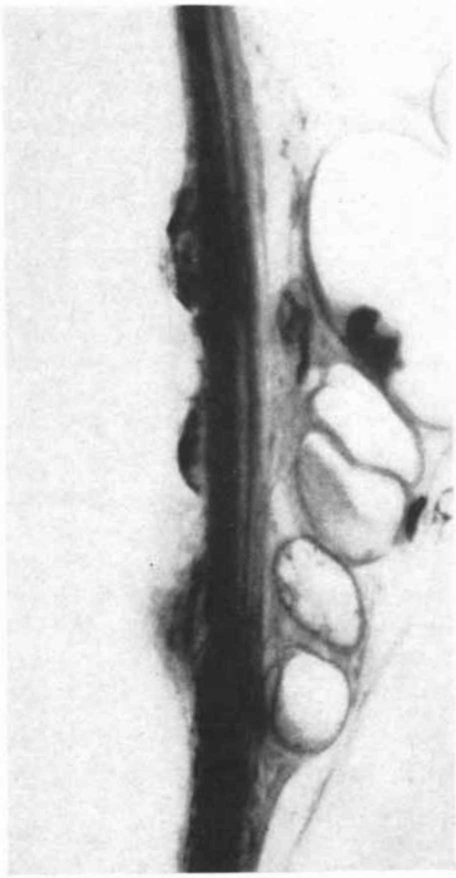
The electric-charge situation complicates the picture. Inside the axon we have a high concentration of positively charged potassium ions; outside, a high concentration of positively charged so-



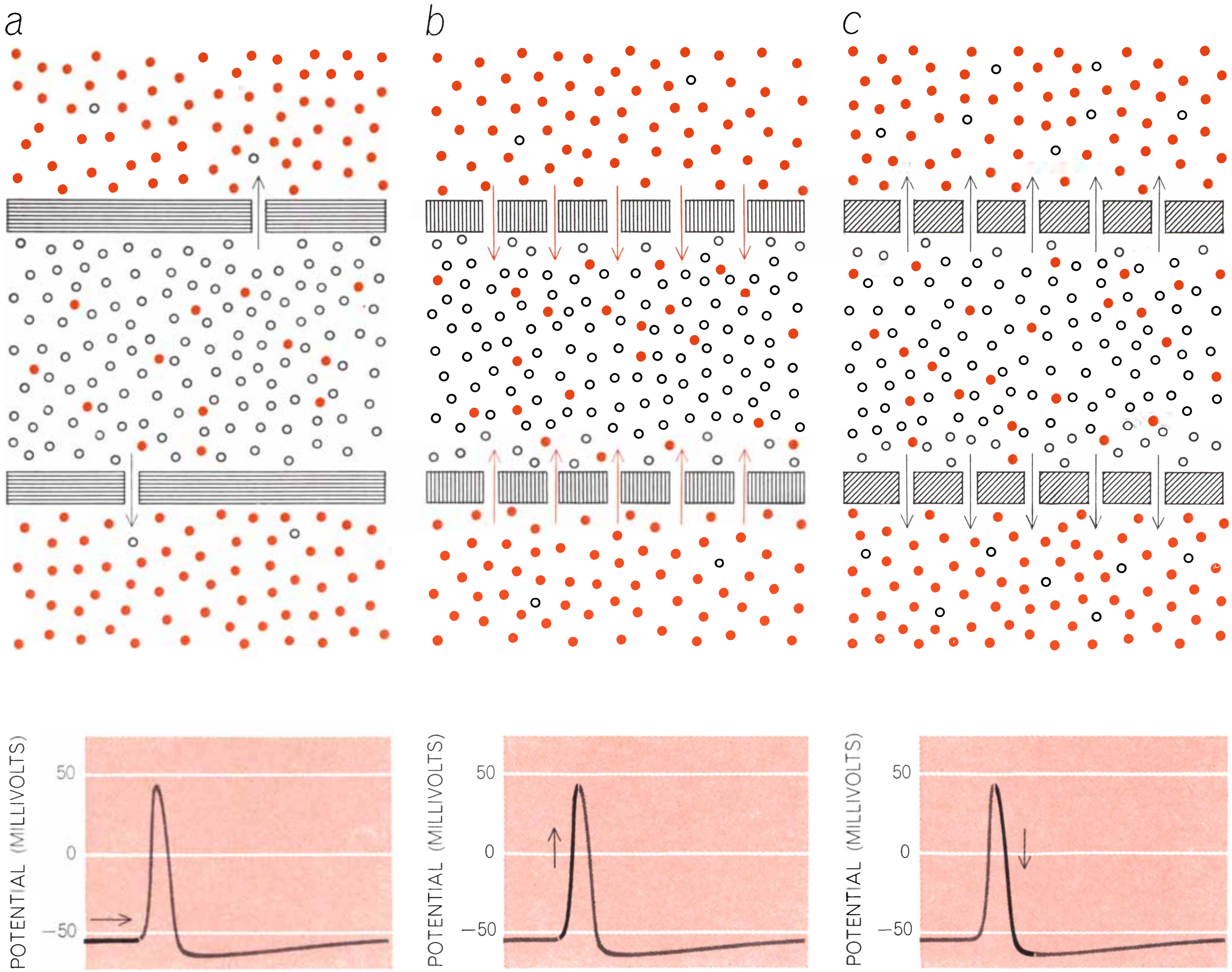


INTACT SQUID AXON is seen in transverse section in these photomicrographs made by J. S. Alexandrowicz of the Plymouth Marine Laboratory. The section is enlarged 140 diameters (*left*). A segment of the axon's perimeter is enlarged 1,150 diameters (*right*). The gray material inside the axon is the axoplasm. The dark boundary of the axon is composed primarily of a layer of Schwann cells.





PERFUSED AXON (*left*) and part of its perimeter (*right*) are enlarged as in the top micrographs. The axoplasm has been replaced with a potassium sulfate solution. The small amount of grayish residual axoplasm is thickest near a nucleus of a Schwann cell (*right*).



INTERNAL POTENTIAL of an axon is established by the concentration gradients of potassium and sodium ions and by the axon membrane's changing permeability to these two positive ions. Because it is more concentrated inside the axon than outside, potassium (*open circles*) tends to diffuse out; sodium (*colored dots*), more concentrated outside than inside, diffuses in. A normal resting axon membrane is more permeable to potassium than to sodium. The resulting net outward diffusion of potassium establishes the inside-negative resting potential (*a*). An action potential occurs when the nerve is depolarized (by a nerve impulse or artificial stimulation). The action potential has two phases. First the membrane becomes very permeable to sodium, which enters the axon and makes the inside positive (*b*). Then the sodium "gates" close; the membrane thereupon becomes very permeable to potassium, which moves out, making the interior negative again (*c*). A reduction in potassium permeability reestablishes the resting condition.

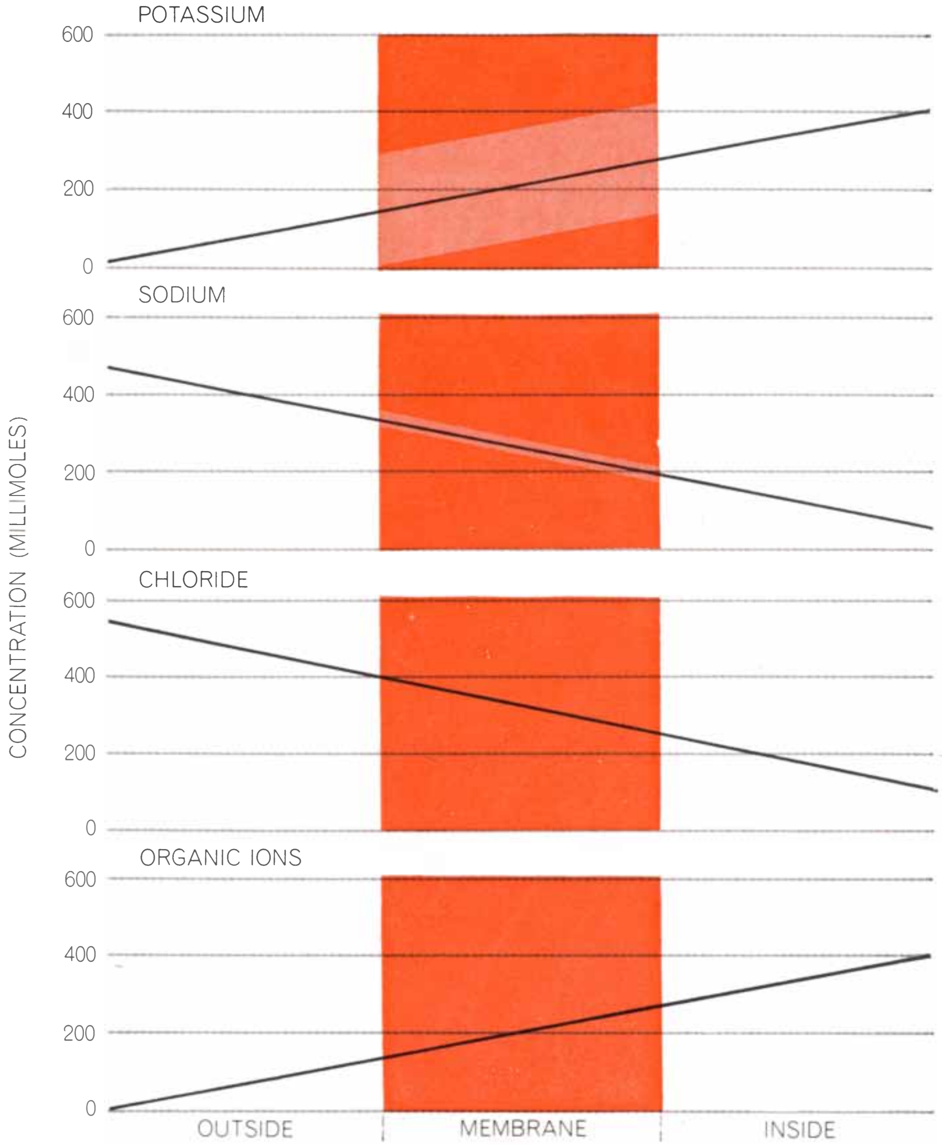
dium ions. When the membrane is in its unstimulated resting condition, it is highly permeable to potassium but only slightly permeable to sodium. Because of potassium's relative freedom to pass through the membrane and its steep concentration gradient, it tends to leak out of the axon at a high rate. The result is that the inside of the axon becomes electrically negative with respect to the outside. There is a limit to this process; an equilibrium is reached when the tendency for potassium to diffuse out is balanced by the electric field that has been set up. At this point the interior of the axon is about 60 millivolts (thousandths of a volt) negative in relation to the external fluid. That difference—a negative potential of 60 millivolts—is called the resting potential of the nerve cell. It is created, in effect, by a potassium battery.

It is relevant to inquire what would happen to the potential if the membrane were highly permeable not to potassium but to one of the other ions. The concentration gradient of the positively charged sodium ions is from outside to inside, acting to make the inside of the axon positive. The gradient of the negatively charged organic molecules is from inside to outside and will also act to make the inside positive. The negatively charged chloride ions are more concentrated outside the axon than inside and will act to make the inside negative. Altering the permeability of the membrane to these ions can establish a wide range of potentials, the inside being negative (because of potassium or chloride) or positive (because of sodium or organic ions) or anything in between (because of a mixture). The possibilities this variability offers for the control of membrane potential have been thoroughly exploited by the cells of the central nervous system.

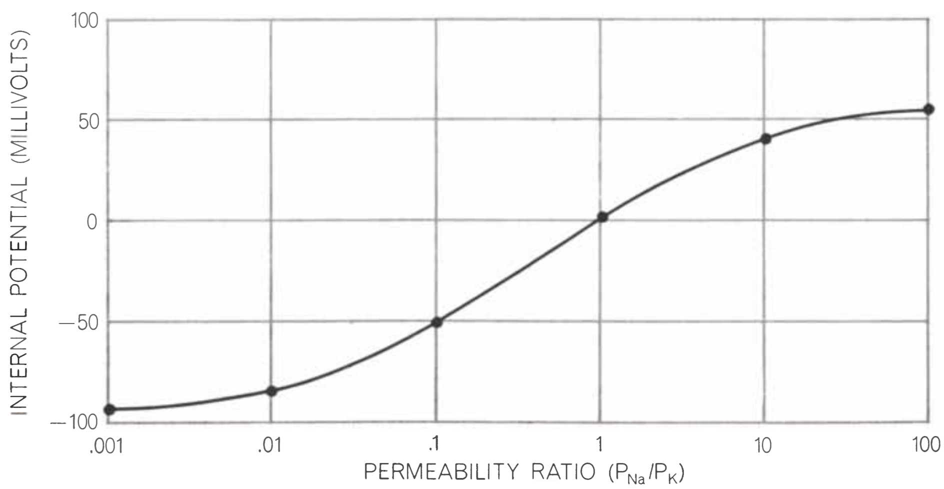
Obviously any change in the membrane's permeability to one of the ions can change the potential. This is precisely what happens when an electrical impulse passes along the axon. The current reduces the potential at the point of its arrival, and the resting potential there drops toward zero; the membrane is said to be depolarized. In response to the drop in potential the membrane's permeability to sodium suddenly increases; this further reduces the potential, which in turn makes the membrane still more permeable to sodium. As if a door were suddenly opened wide, sodium ions from the surrounding fluid rush into the axon. The result is that within a small fraction of a second the interior of the axon switches from a *negative* potential of about 60 millivolts to a *positive* potential of about 50 millivolts. The new condition is the first phase of what is called an action potential.

The local region within the axon is now positive, whereas the next adjacent section, which still has a normal resting potential, is negative. Consequently a current flows from the positive to the negative region, completing the circuit by returning to the positive region through the conducting solution outside the axon. The current arriving in the region of normal resting potential opens the membrane door to sodium and thus triggers the generation of an action potential like that in the region it has just left. In this manner the impulse is regenerated from point to point along the axon and flows from one end of it to the other. In each region, shortly after an action potential has been generated, the membrane's permeability to sodium is switched off and its permeability to potassium increases, and as a result that section of the axon returns to the resting potential. The entire local action potential lasts only about a millisecond.

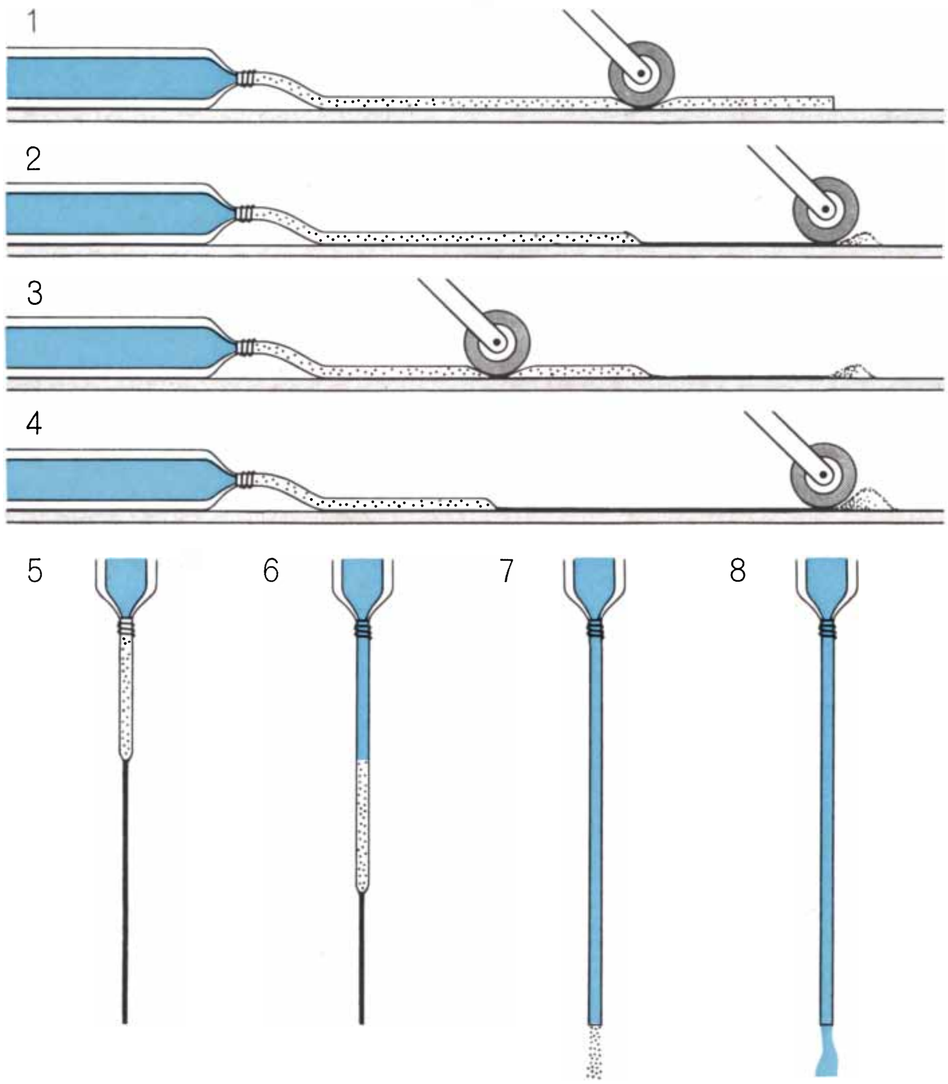
For many years physiologists have been looking into this process experimentally. What would happen if the concentration of ions on one side of the membrane or the other was changed artificially? Isolated axons remain functional for many hours when they are immersed in a simple salt solution containing the major ions present in seawater. It is an easy matter to vary the concentration of these ions in order to study their influence on the process of impulse conduction. Experimenters found that when they added potassium to the medium, thereby reducing the potassium gradient between the inside of the axon and the outside, the resting potential dropped. When they removed sodium from the medium (replacing it with an osmotically equivalent amount of sugar or the positive organic molecule choline), the axon became incapable of propagating an electrical impulse. These results supported the general view of the critical roles normally played by the sodium and potassium



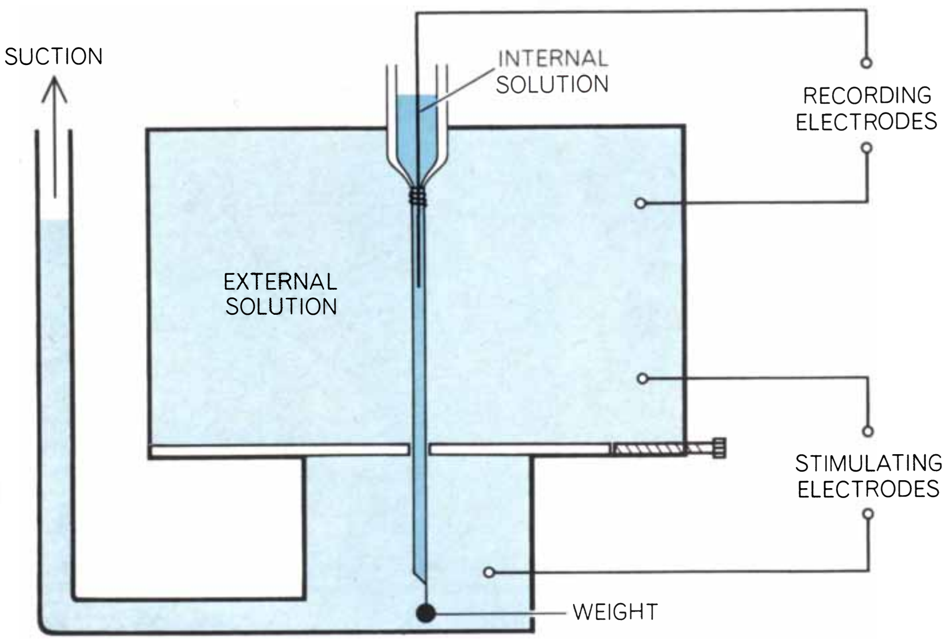
CONCENTRATION GRADIENTS for the major nerve-axon ions are shown. The concentration of each ion on the outside (*left*) and inside (*right*) of the membrane (*color*) is given; the slope shows the direction of the concentration gradient, down which the ions have a tendency to diffuse. This diffusion is blocked by the membrane, which resists mixing of internal and external ions except as it is permeable to one or another of them. The permeability of the membrane during the normal resting phase is suggested here: it is 10 times as permeable to sodium as to organic ions or chloride, 10 times as permeable to potassium as to sodium. Sodium and potassium ions are positive, chloride and organic ions negative.



MEMBRANE PERMEABILITY to potassium and sodium ions can be varied. The curve shows, for the concentrations given in the chart at the top of the page, the effect on the internal potential of changes in the membrane's relative permeability to sodium and potassium.



AXOPLASM is removed from a piece of squid axon and replaced with an experimental solution. A piece of axon with a cannula fixed to its smaller end is placed on a rubber pad and axoplasm is squeezed out of it by successive passes with a rubber roller (*1–4*). Then the nearly empty sheath is suspended vertically in seawater (*5*) and the perfusion fluid is forced through it, removing the remaining plug of axoplasm and refilling the axon (*6–8*).



PERFUSED AXON is mounted so as to allow solutions to be changed with an internal electrode in position. To fill it with a new solution, suction is applied to the external fluid.

gradients in the generation of the action potential and the resting potential.

Experiments were also performed in changing the composition of the axoplasm within the axon. To inject extra sodium or potassium into the intact fiber a special microsyringe was used by Harry Grundfest, C. Y. Kao and M. Altamirano at the Columbia University College of Physicians and Surgeons, and also by Hodgkin and Richard D. Keynes at the University of Cambridge. It is not possible to change the inside-outside gradients of ions by any great amount in this way, but when extra sodium was injected into the axon, raising its concentration closer to the level of the sodium outside, the action potential produced by an electrical impulse was smaller than normal, presumably because the usual inrush of sodium ions was checked.

Could one find a way to remove the core of the axon and substitute a strictly experimental solution inside the fiber? Recently two different techniques for doing this were developed almost simultaneously, one by Ichiji Tasaki and his co-workers at the Marine Biological Laboratory in Woods Hole, Mass., and the other by T. I. Shaw and the author at the Plymouth Marine Laboratory in Britain. In Tasaki's method a fine capillary tube is used to ream out some of the axoplasm, and then a fluid is run through the tube to wash out as much of the remaining material as possible. It is an extremely tricky operation, particularly on the comparatively small "giant" axons of the squid found in North American waters, and a considerable amount of axoplasm is left in the fiber.

Our technique is crude by comparison, but we have found that it effectively removes most of the axoplasm. It is based on an old stratagem: in 1937 Richard S. Bear, Francis O. Schmitt and J. Z. Young, working at the Marine Biological Laboratory in Woods Hole, discovered that if an end of an axon was cut, they could squeeze the axoplasm out of it much as one squeezes toothpaste out of a tube. We were led to wonder about the casing left by this operation. Did the squeezing spoil the membrane's properties or could it still conduct an electrical impulse? We undertook an experiment to answer the question. We laid a section of axon about eight centimeters long (a little more than three inches) on a rubber pad and squeezed axoplasm out of the wider end by a series of fairly firm strokes with a rubber-covered roller. When the axoplasm had been extruded from half of the axon's length, leaving that part a flattened sheath, we rolled the other half to push some of its axoplasm into the emptied section. To our surprise we found that the roughly handled membrane could still conduct and boost electrical impulses. This experiment showed that the excitable properties of the membrane are not destroyed by extrusion and encouraged us to try to replace the axoplasm with artificial solutions.

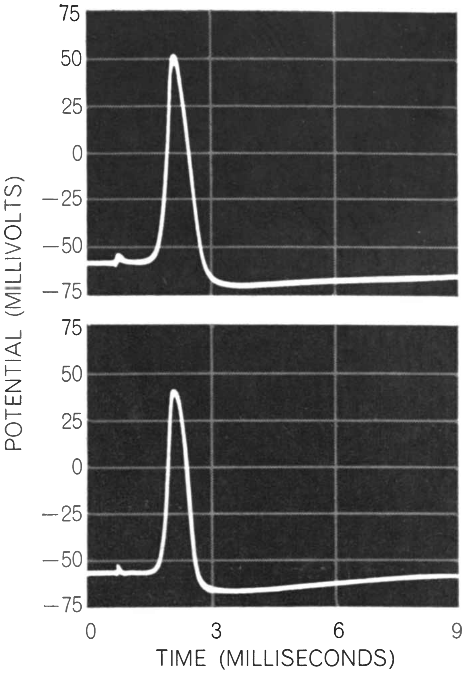
Our procedure was to insert a small glass cannula into the narrower end of the axon and squeeze out as much of the axoplasm as possible; then, attaching the cannula to a motor-driven syringe, we suspended the nerve in seawater and forced "artificial axoplasm" through the flattened sheath. When the nerve was refilled, its excitability was tested, and in about 75 percent of the experiments the preparation was found to function like a normal nerve. With this method about 95 percent of the axoplasm is extruded and a long length of axon can be perfused. Once perfused, the axon can be tied off at both ends and handled like a normal axon. It is more convenient, however, to mount it in such a way that a microelectrode can be inserted into the axon and, with the electrode in place, the perfusion fluid can be changed repeatedly [*see illustrations on opposite page*]. It is also possible to change the external solution.

We now undertook a series of perfusion experiments in collaboration with Hodgkin. The first question we asked was: What substances must the axoplasm contain in order to generate a resting potential and an action potential? The requirements turned out to be simple indeed. The only essentials are that the solution be rich in potassium ions and poor in sodium ions, and that it have about the same osmotic pressure and concentration of hydrogen ions as normal axoplasm. As for the negatively charged ions (which normally are ionized organic molecules), their nature is not critical; we have used a wide variety of negative ions successfully. A particularly convenient solution is buffered potassium sulfate. When axons are perfused with this solution, they produce resting and action potentials that are almost identical with those generated by intact fibers [*see top illustration at right*].

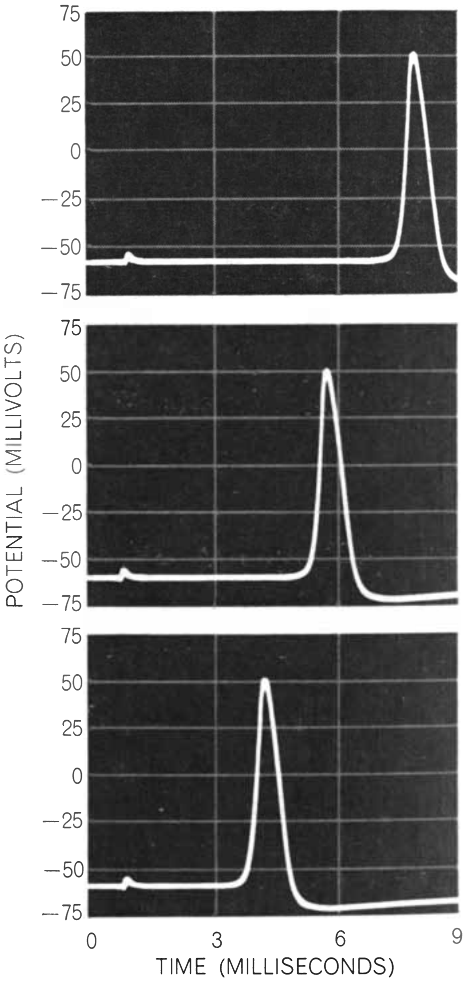
It appears, then, that the bulk of the natural axoplasm is not necessary for the propagation of impulses. Are crucial remnants of the original material left in our axons—substances that could not be dispensed with and that make conduction possible? It is not easy to answer categorically; all that can be said is that the axons are still fully functional and can conduct up to half a million impulses, even after they have been washed by a flow of artificial solution amounting to 100 times the volume of the original fluid. If essential molecules were diffusing from the remaining axoplasm to the membrane, they should have been completely washed out by such a massive flow. There remains the possibility that something essential for impulse conduction is supplied by a layer of cells that surrounds the giant axon. The function of these cells, called Schwann cells, is obscure. Various kinds of evidence suggest, however, that they are not directly involved in electrical conduction. For instance, when the Venezuelan workers R. Villegas, Maximo Gimenez and L. Villegas inserted two microelectrodes into a squid nerve—one into a Schwann cell and the other into the axon—they were unable to detect any electrical change in the Schwann cell when an action potential passed along the axon.

Perhaps the most remarkable finding is the fact that apparently no source of energy other than the difference in ion concentrations on the two sides of the membrane is needed to amplify an elecfical impulse and propagate it along the axon. Our artificial axoplasm contained no sugar, adenosine triphosphate (ATP) or other chemical source of energy, and it is unlikely that any ATP was produced by the traces of original axoplasm left in the sheath. Yet the axon could generate both resting and action potentials whenever the inside and outside solutions had the right concentrations of potassium ions and sodium ions.

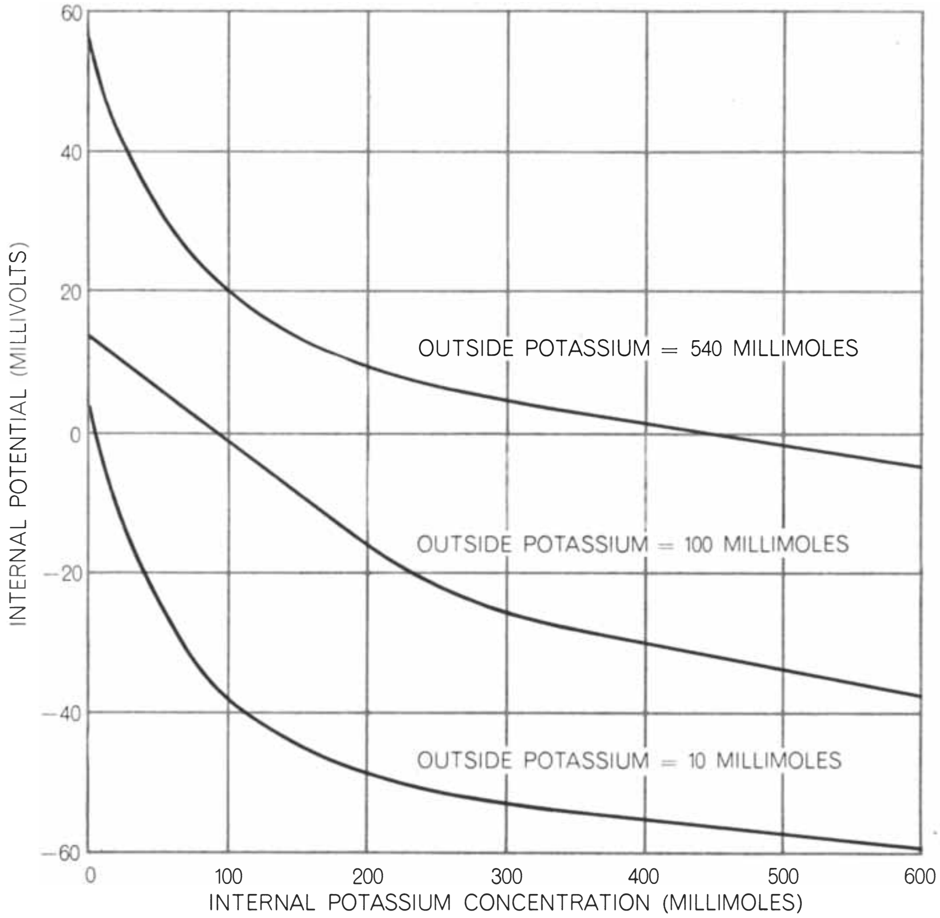
To produce the resting potential we need only make sure that the fluid perfusing the experimental axon is primed with a sufficiently high concentration of potassium. If we substitute sodium for potassium in the potassium sulfate perfusion fluid, the resting potential drops, and the amount of this drop depends on the extent of the substitution; when the concentration of potassium inside the axon is reduced to the same level as that outside, the potential drops to zero. If we make the potassium concentration outside much higher than that inside, the inside of the axon becomes positive,



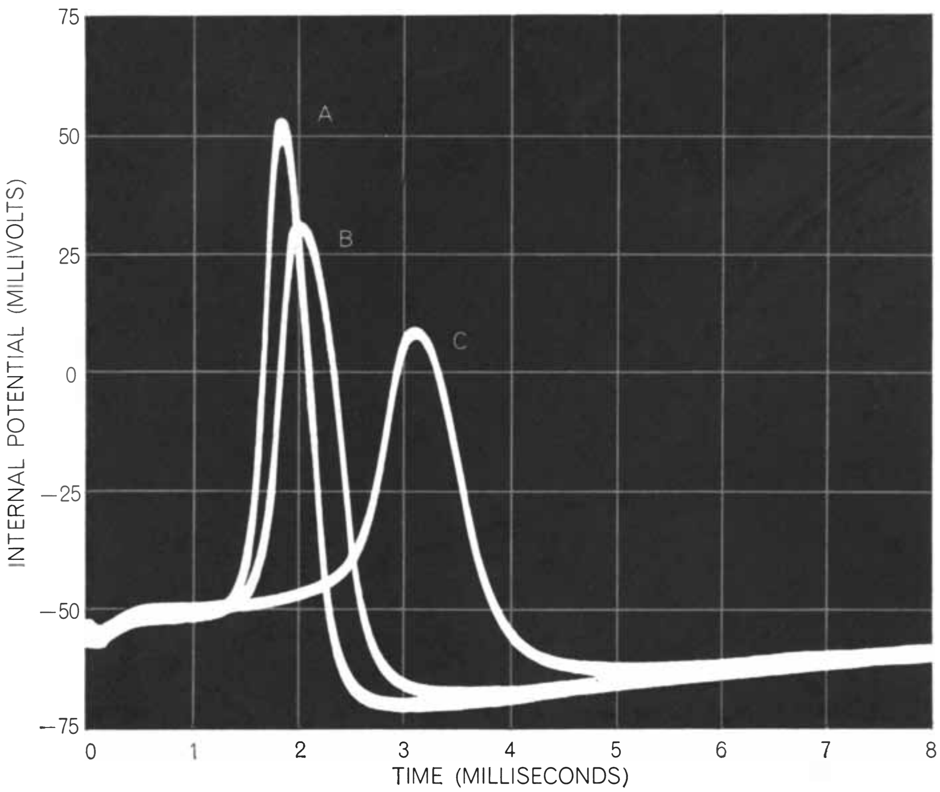
ACTION POTENTIALS from an axon perfused with potassium sulfate (*top*) and from an intact axon (*bottom*) are quite similar.



DEGREE OF INFLATION with perfusion fluid does not change the action potential but does increase the conduction velocity. These potentials were recorded from an empty axon (*top*), a partly inflated one (*middle*) and a fully inflated one (*bottom*).



RESTING POTENTIALS of perfused axons depend on the potassium concentration gradient. The internal potassium content was changed by substituting sodium chloride for potassium chloride inside the axons. The lower the potassium gradient, the less negative the internal potential became. With the gradient reversed the internal potential became positive.



"OVERSHOOT," the amount by which the action potential rises above zero, decreases as the sodium gradient is reduced. These potentials are from a normally perfused axon (*A*) and axons in which a quarter (*B*) and half (*C*) of the potassium was replaced with sodium.

that is, the sign of the resting potential is plus instead of minus. These observations are what would be expected if the resting nerve membrane is freely permeable to potassium ions but not to other ions. The magnitude and the sign of the potential generated are dependent on the steepness and the direction of the potassium concentration gradient.

These experiments also show that the inward diffusion of the negatively charged chloride ions contributes very little to the resting potential. When there is no potassium concentration gradient, there is still a steep gradient for chloride, and yet the measured resting potential is close to zero. This type of experiment suggests that the resting membrane is highly impermeable to negative ions but freely permeable to potassium. A similar argument applied to sodium indicates that the resting membrane is also impermeable to this ion.

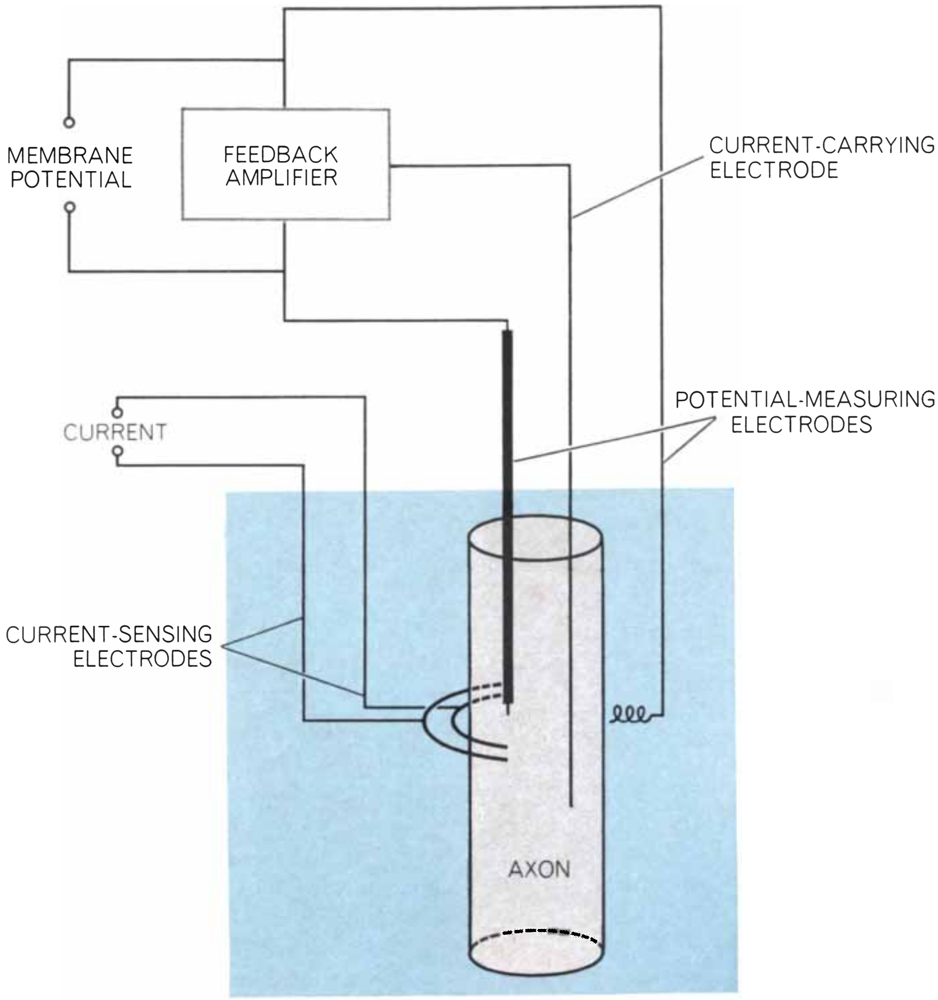
During the action potential, on the other hand, the permeability to sodium ions is markedly increased, but the amount by which the action potential overshoots zero and becomes positive is dependent on the sodium concentration gradient. The progressive replacement of potassium inside the axon by sodium reduces the overshoot and finally abolishes the axon's ability to conduct impulses; the ability is restored as the sodium is washed out and replaced with potassium. A rather interesting corollary is the change in action potential that occurs when most of the potassium sulfate in the perfusion fluid is replaced by an osmotically equivalent amount of sugar. Under these conditions the overshoot of the action potential is increased, only to fall again when the axon is refilled with potassium sulfate. This suggests that when potassium is present within the axon, it acts to some extent as a barrier to the inflow of sodium. That is to say, the potassium ions serve in some degree as if they were sodium ions, and when they are absent, the sodium gradient from outside to inside is steepened.

In performing these experiments we noticed that, although the substitution of sugar for potassium sulfate enhanced the action potential, it slowed the axon's conduction of impulses. Presumably this could be attributed to the low electrical conductivity of sugar, which would shorten the range of each stage of propagation (that is, each local circuit) and thereby slow the overall rate of travel. Indeed, we found by experiment that the higher the electrical conductivity of the material we used for perfusing the axon, the faster the speed of impulse propagation.

Recent experiments on perfused axons have been directed toward a detailed analysis of the properties of the action-potential mechanism. These investigations have depended to a large extent on the device known as the voltage clamp. This technique was devised almost 20 years ago by Cole and his coworkers and by Hodgkin, Huxley and Katz, and in their hands it provided almost all the detailed evidence for the sequence of changes in membrane permeability that occurs during the action potential.

The technique is simple in principle but often very difficult in practice. The idea is to produce a sudden displacement of the membrane potential from its resting value and to hold the potential at this new fixed level by means of a feedback amplifier. The current that flows through a definite area of membrane under the influence of the impressed voltage is measured with a separate amplifier [*see illustration at right*].

When the membrane of an intact axon is depolarized and the potential is held at some value close to zero, the current that flows is at first directed inward, but it rapidly reverses its direction and continues to flow outward as long as the membrane is kept depolarized. There is every indication that the initial inward current is carried by a rapid inflow of sodium ions resulting from a transient increase in the membrane's permeability to sodium, and that the delayed outward current results from a prolonged increase in its permeability to potassium. For one thing, experiments with the same technique show that if sodium is absent from the outside medium, so that the downward gradient of sodium concentration is from inside to outside instead of the other way around, there is a small initial outward flow of current instead of an inward one. Sodium ions now move from inside to outside through the door of increased permeability opened by depolarization of the membrane. The same result can be obtained, even when the axon is bathed on the outside by seawater, by reversing the resting potential, that is, by making the inside of the axon positive instead of negative and holding the potential at the positive value. The potential difference then drives sodium ions out of the axon against the chemical gradient, and a current flows outward. At some intermediate value the applied potential just balances the tendency of sodium ions to enter the axon and there is no detectable early current. This potential is called the sodium equilibrium potential.



"VOLTAGE CLAMP" is set up as shown in this schematic diagram. A change in membrane potential is produced by the current-carrying electrode. Sensed by two other electrodes, the potential is thereafter maintained by a small current regulated by feedback. The current that flows through a known area of membrane at this fixed potential is then measured.

The technique of perfusing axons allows the voltage clamp to be applied to situations in which the interior of the axon, as well as the external environment, can be varied. What will happen if sodium is completely absent both outside and inside the axon? In such a case we might expect that, if the axon contains only potassium, depolarization of the membrane should simply bring about a lasting outward current, carried by potassium ions diffusing out of the axon. When Knox Chandler and Hans Meves of our laboratory applied the voltage clamp, they found that depolarization always resulted promptly in a small outward current that was identical in its properties with the one normally carried by sodium ions. Were potassium ions in the axon acting like sodium ions in this case, passing out through the same membrane channel that opened up for sodium ions during depolarization?

Chandler and Meves tested this surmise by adding sodium to the formerly sodium-free outside medium. If the potassium ions within the axon did not behave like sodium, the downward gradient of sodium concentration from outside to inside would be very steep; hence a large inside-positive potential should be required to prevent a flow of current across the membrane. Actually the experiments showed that the equilibrium potential (the potential required to abolish the early inward current) had the value that would be expected if the potassium inside the axon acted like a small amount of sodium. These results indicate that the channel by which sodium ions enter the axon during an action potential is not completely specific for sodium ions. Chandler and

Meves were able to calculate from their measurements that the "sodium channel" enables sodium ions to pass about 12 times more easily than potassium ions.

This conclusion has recently been strengthened by a different approach. The fish known as the puffer manufactures a potent nerve poison called tetrodotoxin; analysis by means of the voltage clamp of how this poison acts has shown that it very specifically blocks the increase in permeability to sodium that occurs immediately following depolarization. It has no effect on the delayed increase in permeability to potassium. When tetrodotoxin was applied to perfused axons, Chandler and Meves found that it also blocks the early current carried by potassium ions, thus confirming the view that the potassium is passing through the "sodium channel."

The axon membrane loses its capacity for increasing its permeability to sodium when its resting potential is kept at progressively less negative levels. Hence as the resting potential is reduced toward zero (for instance by the replacement of potassium ions inside the axon with sodium or choline) the mechanism for admitting sodium is progressively inactivated, and therefore the axon's capacity for producing an action potential becomes progressively smaller. If, however, the potassium in the perfusion fluid is replaced by sugar instead of another ion, this loss is not so sharp; the membrane maintains its activity, or ability to increase its permeability to sodium, at much lower levels of resting potential. Chandler, Hodgkin and Meves have proposed a possible explanation for this puzzling result. They suggest that the reduction or elimination of ions in the perfusion fluid uncovers negatively charged groups of atoms on the inner face of the membrane. This process would increase the electric field within the membrane without altering the total potential difference between the internal and external solutions. Accordingly a charged molecule within the membrane will experience an imposed electric field identical with that which it experiences in an intact axon at the resting potential.

Experiments with perfused axons have yielded results that would have been impossible to obtain with intact axons and thus represent a considerable advance, but in general they have not produced any revolutionary changes in ideas about the mechanism of propagation of the nerve impulse. They have, however, served to define more sharply the questions to be answered about the basic chemistry of the process. How do ions pass through the membrane? What makes the resting membrane so much more permeable to potassium than to sodium? What specific change in the membrane (brought about by a drop in potential) causes it suddenly to open its doors to sodium?

There are many hypotheses on these questions but so far no convincing items of evidence. The difficulties facing those who are trying to solve such problems in chemical terms are immense. Many of the unique properties of living membranes are dependent on the potential that normally exists across them. The application of the routine biochemical technique of homogenizing cells in order to isolate the cell membrane would break up the membrane and destroy the potential across it; this might so alter the architecture of the molecular groups involved in nerve activity that they would be unrecognizable. Moreover, the relevant groups are probably quite thinly scattered through the membrane material and hence much diluted by less interesting molecules. Perhaps artificial membranes, synthesized from substances extracted from natural ones, will yield some clues, but a complete explanation of the behavior of the nerve membrane in molecular terms is probably still a long way off.

Further experiments with perfused axons may tell us something about the process of recovery from nerve activity. During the passage of an impulse in an intact nerve there is a small net gain of sodium and a small net loss of potassium. If this were to continue unchecked, the nerve would lose potassium and gain sodium, and the concentration gradients on which nerve activity depends would be destroyed. The tendency is counteracted by the mechanism known as the "sodium pump," which uses metabolic energy in the form of ATP to extrude sodium ions from the axon in exchange for potassium ions. Although an intact nerve can function for some time without an operating sodium pump, the pump is essential for the long-term maintenance of nerve activity. In perfused axons this is not the case, since fresh fluid can be constantly passed through the axon. There is evidence, however, that the pump mechanism survives perfusion and that it can be activated by adding ATP and small amounts of magnesium and sodium to the perfusion fluid.

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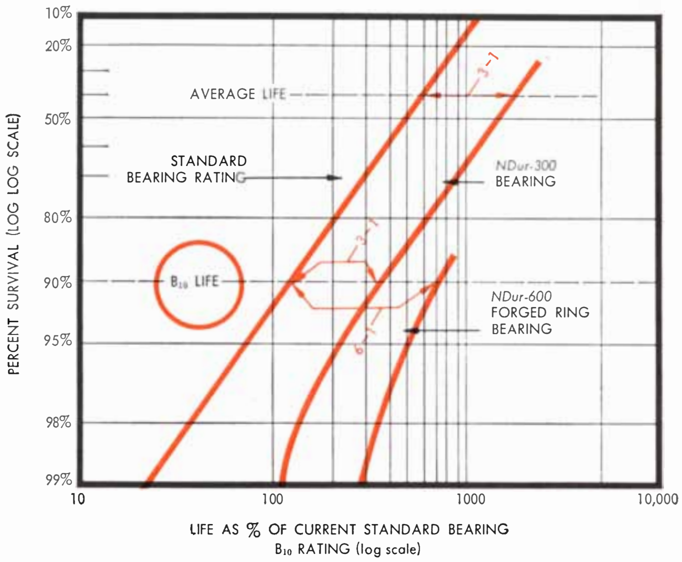

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The Prehistory of the Australian Aborigine

The native inhabitants of Australia were long thought to be relatively recent immigrants. In the past decade archaeologists have discovered that man first reached the remote continent at least 16,000 years ago

by D. J. Mulvaney

The prehistory of Australia ended in 1788, when the British landed at the site of modern Sydney. How many millenniums before that the continent's aboriginal inhabitants arrived has not been precisely established. Only a decade ago their prehistoric period was widely believed to have been no more than a brief prelude to the European colonization. Today it seems certain that the initial migration took place in Pleistocene times—no less than 16,000 years ago and probably much earlier. Here I shall review the archaeological findings that shed some light on the prehistory of the aborigines and then describe recent field studies that, in my opinion, quite drastically alter earlier views.

Sundered from Asia before *Homo sapiens* evolved, Australia is a land mass almost the size of the U.S. It has 12,000 miles of coastline and extends from 43 degrees South latitude to within 11 degrees of the Equator. A third of its area lies north of the Tropic of Capricorn. An equally extensive area annually receives less than 10 inches of rainfall. Only 7 percent of the land mass rises above 2,000 feet; indeed, the continent can be traversed from the Gulf of Carpentaria in the northeast to the southern coast without climbing higher than 600 feet. Australia's major mountain and river systems are restricted to its eastern and southeastern parts. These topographic realities must be reckoned with in considering prehistoric patterns of human settlement. Of equal significance are the usually dry watercourses and salt pans of the arid "outback," which testify to a more congenial climate in late Pleistocene or early postglacial times.

MAJOR PREHISTORIC SITES are located on a map of Australia. Devon Downs, in the Murray River valley, was the first stratified site to be found on the continent. Excavated in 1929, its lower levels are 4,500 years old. Kenniff Cave, a site the author found 1,000 miles north of Devon Downs in 1960, contains strata that span some 16,000 years of prehistory.

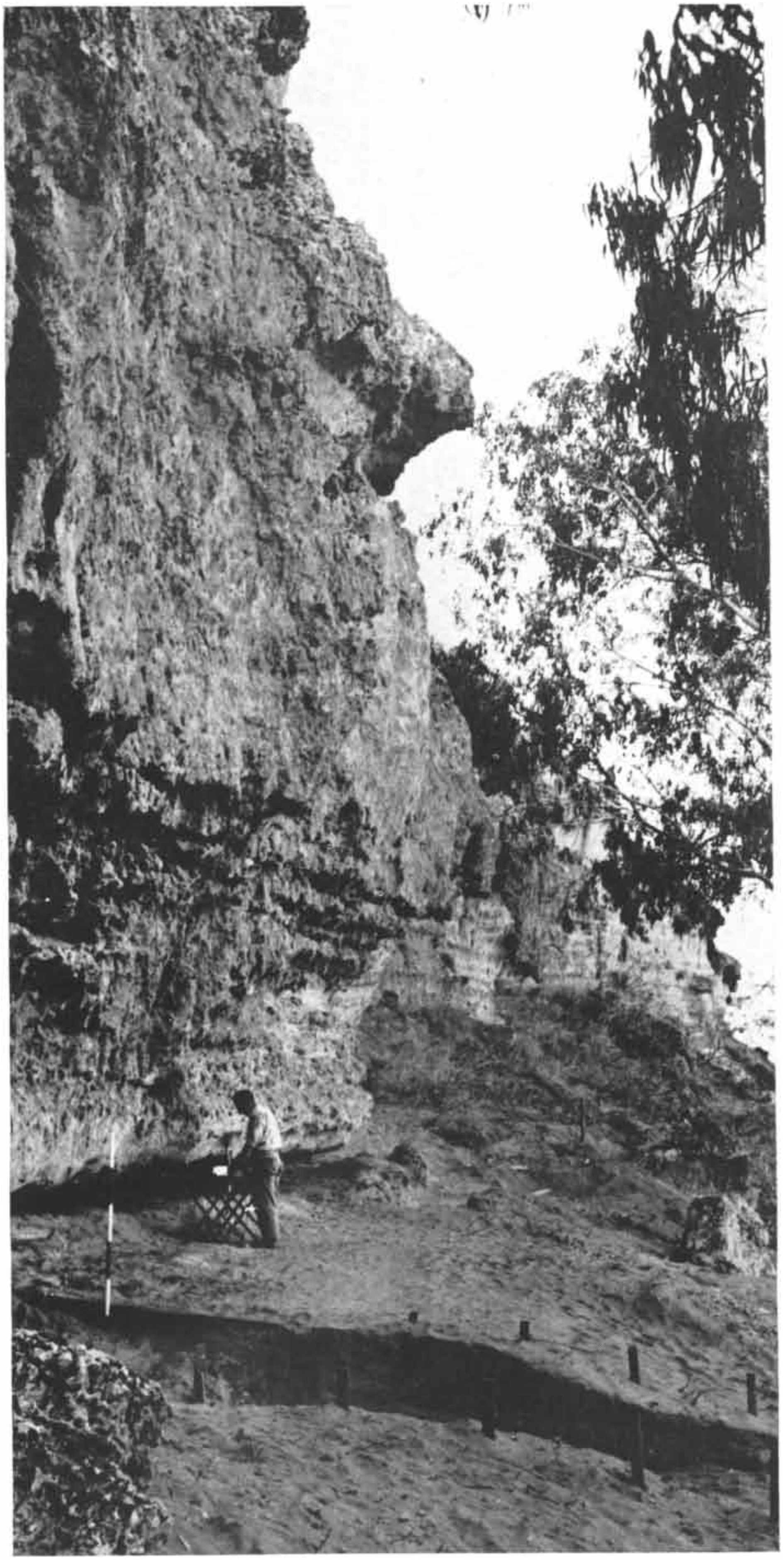
In 1788 Australia was inhabited by perhaps 500 aboriginal tribes; they probably mustered a total population of some 300,000. In coastal or river-valley environments, where the population density was comparatively high, there were one or two individuals per square mile. Elsewhere immense tracts supported no more than one person every 30 or 40 square miles. In so large an area one might expect considerable variety in the inhabitants' ecological and technological adaptations, and reports by early European observers and the scantier evidence of archaeology document such differences. For all its variety, however, the prehistoric period had an underlying unity. Over the entire continent the aborigines habitually hunted, fished and gathered wild plants; they had not learned agriculture. The extent of nomadism in each tribal area was largely determined by the local availability of food.

Regional variations in the Australian environment are actually more apparent than real. In other lands differences in topography, rainfall or latitude give rise to diversity in plant and animal species; in Australia these factors may result merely in the substitution of one member of the same plant or animal family for another. The eucalyptus and the kangaroo are ubiquitous. Another phenomenon, universal in Australia and unique to it, is the absence in prehistoric times of formidable predatory animals competing with man for the same game. Such factors must have encouraged a degree of human standardization, both in the character of weapons and in the techniques of hunting and foraging. At the same time it can be postulated that these factors discouraged a dynamic experimental attitude within prehistoric Australian societies.

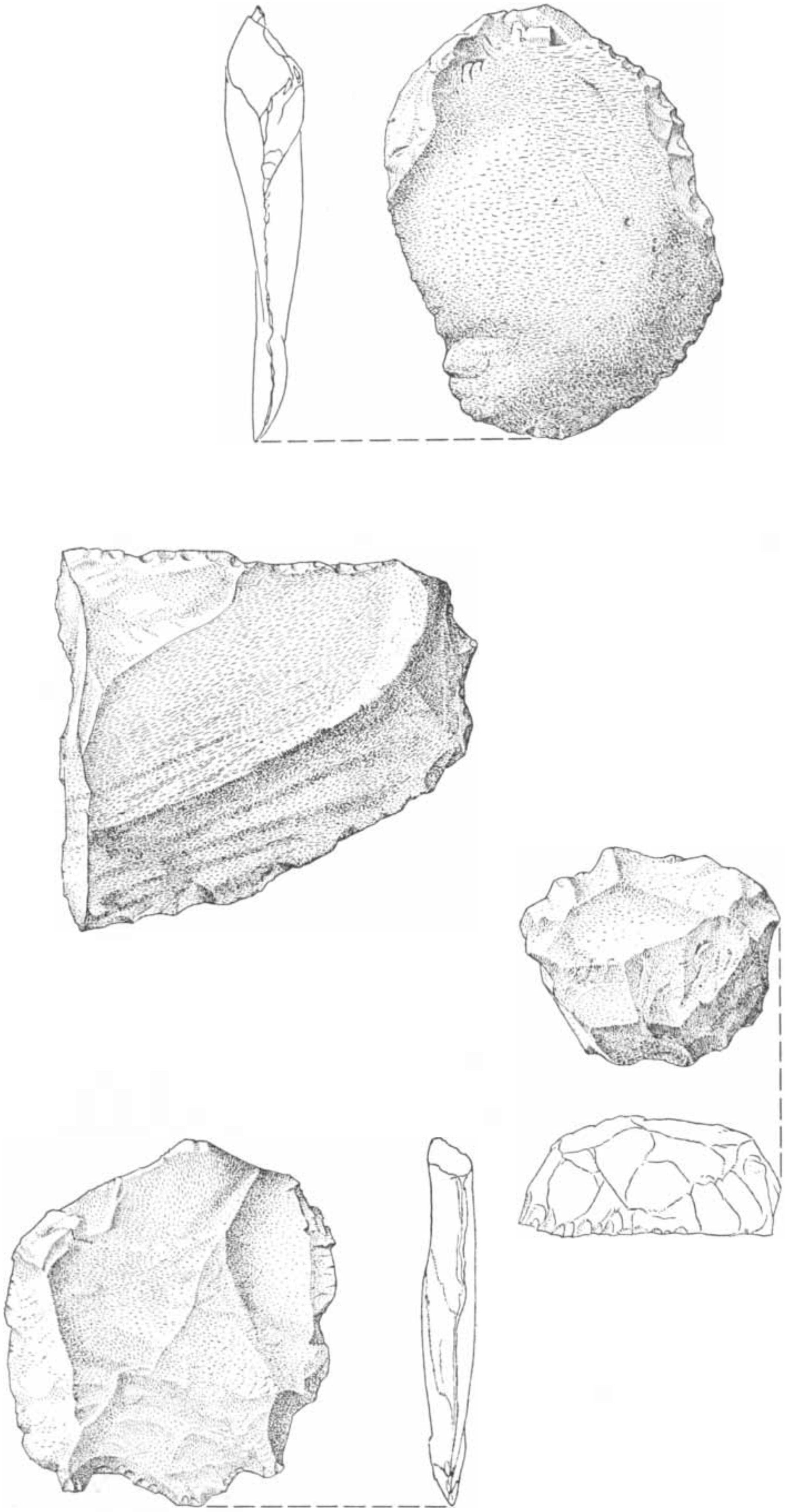
In terms of material culture few Paleolithic peoples were more impoverished than the aboriginal Australians. There were no horned, antlered or tusked species of animals to provide the raw material for the artifacts so valued in hunting societies elsewhere. Flint and similar fine-grained rocks were rare; instead the aborigines made most of their implements out of quartz or quartzite, materials from which even the most skilled knapper has difficulty producing elegant objects. As for the elements of culture beloved of so many writers on archaeology—dwellings, tombs, grave goods, ceramics, metals, precious stones, cultivated crops and domesticated animals (with the exception of the dog)—the aborigines had none.

Accordingly under the best of circumstances the investigator of Australian prehistory is faced with a paucity of archaeological evidence and a limited range of diagnostic cultural traits. To make matters worse, where bone artifacts might have survived, the high acidity of the soil has often eaten them away, and where desert dryness might have preserved wooden artifacts the voracious termite has destroyed them. These disadvantages are partly offset by the rich store of information on living aborigines, beginning with the first European descriptions and extending to the fieldwork of contemporary ethnographers. The prehistorian must guard against an anachronistic fallacy, however; he must not assume that customs and technologies recorded during the past 100 years constitute unambiguous evidence when it comes to interpreting prehistoric remains.

In 1929 a landmark in Pacific archaeology was established. In the valley of the Murray River east of Adelaide, Her-



BASE OF THE CLIFFS bordering the Murray River in South Australia was a popular prehistoric camping ground. Here, at Fromm's Landing rock-shelter No. 2, the excavation of a deep stratified deposit threw new light on earlier discoveries at nearby Devon Downs.

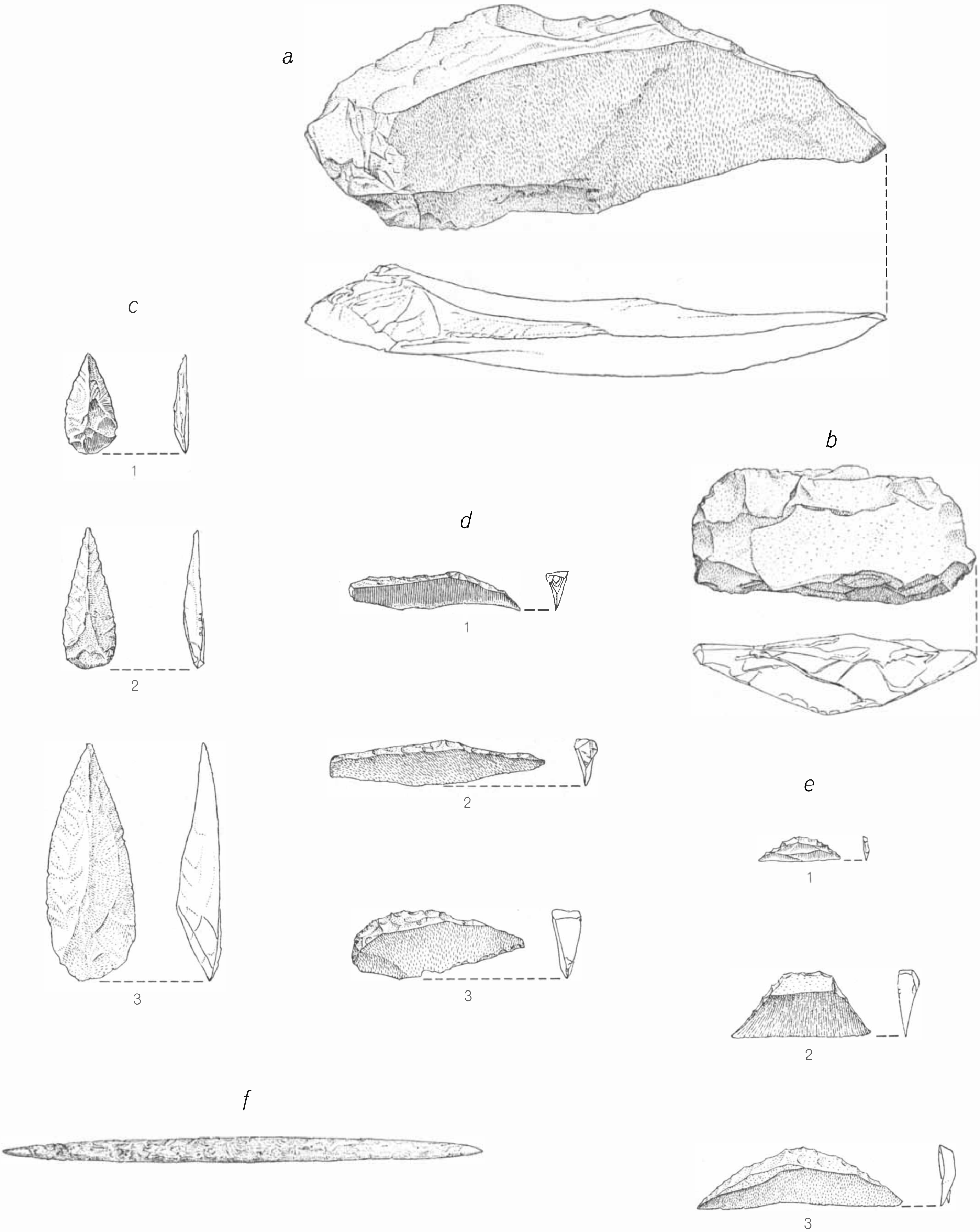


EARLIEST TOOLS found at Kenniff Cave were scrapers such as these, shown actual size. Made by trimming the edges of stone flakes, they were hand-held rather than hafted. They are (*top to bottom*) a quartzite side scraper, trimmed along its right edge; a scraper made from a broken quartzite flake, finely trimmed along its top edge; a round scraper, made from chert, that was trimmed very steeply (*see profile*), and a quartzite flake with trimmed projections and concavities on its bottom edge that suggest use in the manner of a spokeshave to work wood. Eleven thousand years passed before more elaborate tools were made.

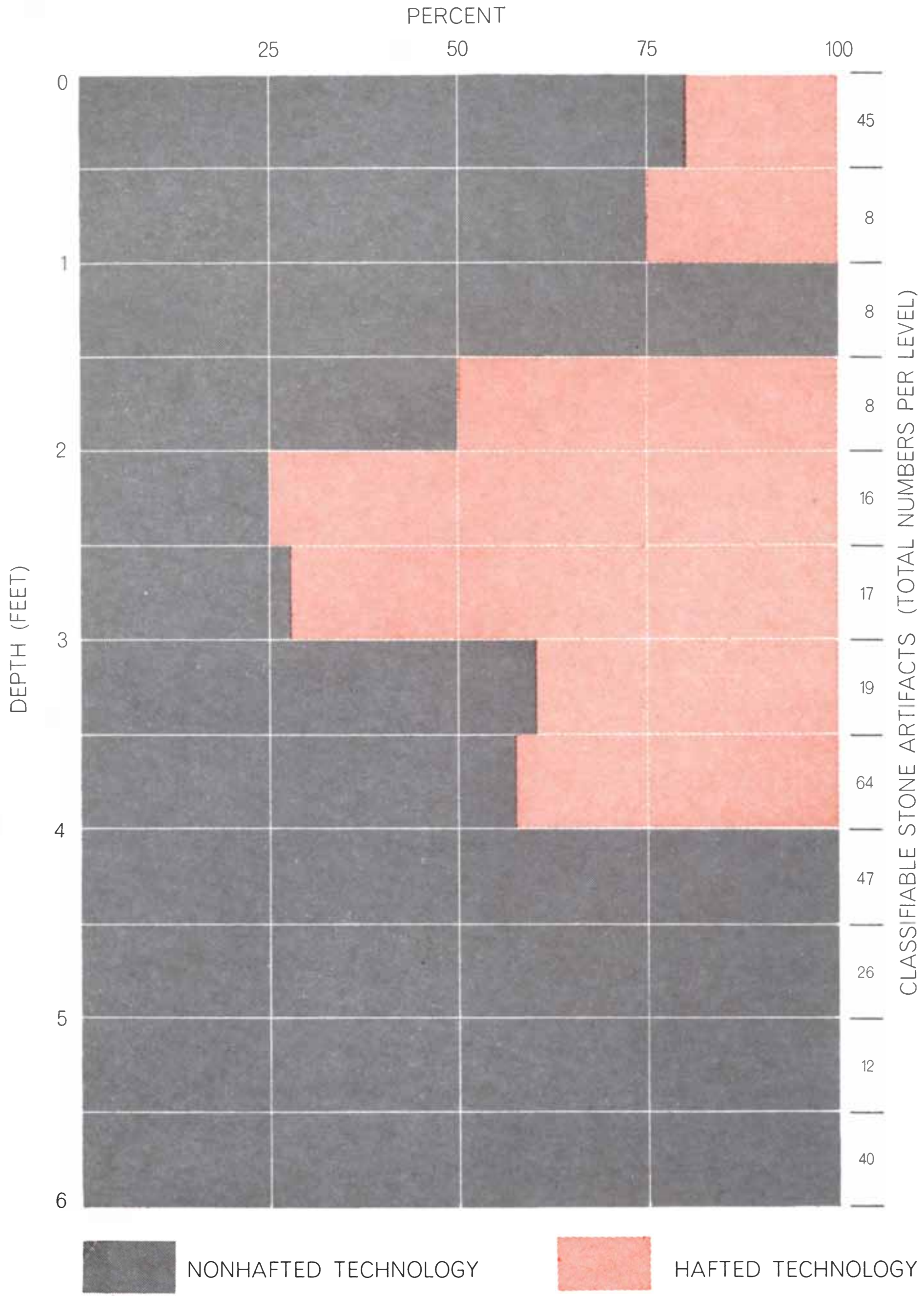
bert M. Hale and Norman B. Tindale of the South Australian Museum excavated a rock-shelter site, known as Devon Downs, that contained 20 feet of stratified deposits of human occupation. Hale and Tindale divided the occupation layers into three successive cultural stages on the basis of the presence or absence of stone or bone implements they believed possessed diagnostic significance. They called the earliest culture Pirrian, because the *pirri*, a symmetrical stone projectile point flaked only on one side, was restricted to the lower layers of the site. Pirris are aesthetically perhaps the most pleasing of all the prehistoric aboriginal artifacts [*"c-1" to "c-3" in illustration on opposite page*]. Many of them are found on the surface at sites in the interior of Australia, but it was at Devon Downs that they were first discovered in a stratigraphic context.

The excavators called the second culture Mudukian; *muduk* is the word used by the Murray River aborigines for a short length of bone, pointed at both ends, that resembles the simple kind of fishhook called a gorge [*"f" in illustration on opposite page*]. At Devon Downs muduks were found in the occupation strata above those that contained pirris. Hale and Tindale gave the uppermost occupation layers the label Murundian; this was derived from the subtribal name of the local aborigines. The Murundian layers contain no distinctive objects of either stone or bone; in effect the archaeological definition of the culture is negative. Carbon-14 dating techniques were not available in those days, but charcoal from a sample of earth preserved in the South Australian Museum since 1929 was recently analyzed and yielded a date for one of the Pirrian strata of about the middle of the third millennium B.C.

Within the past decade I have excavated two rock-shelters at Fromm's Landing, a point only 10 miles downstream from Devon Downs. Carbon-14 dating indicates that the lowest levels in these deposits were occupied early in the third millennium B.C. Fromm's Landing and Devon Downs are thus very close, both geographically and temporally. The sequence of three cultures identified by Hale and Tindale at Devon Downs, however, was not evident at Fromm's Landing. This is a matter of more than casual importance because Tindale has asserted—and the assertion has received wide acceptance—that the three cultures are distributed



ARTIFACTS FOR HAFTING show a wide variety of forms. All are shown actual size. The knife *a* is from the topmost stratum at Kenniff Cave; stone blades like this, with resin or skin handgrips, were still used by Queensland aborigines early in the 19th century. The step-trimmed adze flake *b* was mounted on the end of a stick and served as a chisel or gouge. The neatly trimmed *pirri* points (*c-1 to c-3*) probably were projectile tips; *c-1* was excavated near Kenniff Cave and is 3,500 years old. The other two are surface finds from South Australia. The three blades (*d-1 to d-3*) are called Bondi points; their backs have been blunted by steep but delicate trimming. The three microliths (*e-1 to e-3*) show similar fine trimming on their backs. All presumably formed the working edges of various composite tools. The pointed bone (*f*) is a 400-year-old *muduk;* it may have served as a fishhook, a spear tip or even a nose ornament.



ADVANCE IN TECHNOLOGY at Kenniff Cave took place about 3000 B.C., when the concept of hafting was introduced. At four feet and deeper only hand-held scrapers (*gray*) were found; above that, entirely new kinds of stone artifact (*color*) appear among the scrapers (*see illustration on preceding page*). These artifacts are evidently parts of composite tools.

across the entire continent. I found the pirris at Fromm's Landing in association with microliths, tiny stone blades of the kind that in Old World cultures are sometimes set in a haft of bone or wood to form a composite tool. The association contradicts Tindale's belief that such microliths are artifacts typical not of the Pirrian but of the later Mudukian culture.

This finding and others at Fromm's Landing led me to question the basis of Tindale's cultural diagnosis at Devon Downs. It seemed more useful to consider the elements the three Devon Downs assemblages had in common than to isolate discrete traits as Tindale did. Flakes of stone used as adzes, for example, were found in all three Devon Downs culture levels; there was no apparent break in the tradition of stoneworking from the earliest time to the latest. In reporting my findings at Fromm's Landing I suggested that the differences in the inventory of various Devon Downs strata might be attributable to changes in artifact preference on the part of an aboriginal population that was becoming increasingly adapted to life beside a river, with its varied and rich organic resources. Such an interpretation eliminates any need to imagine the successive arrival of separate culture groups, and all the elements of discontinuity that such a succession implies.

By the standards of archaeology in the Old World neither the Devon Downs nor the Fromm's Landing site is particularly ancient. Between 1960 and 1964, however, my associates and I excavated a rock-shelter in southern Queensland that contained 11 feet of stratified deposits; carbon-14 dating showed that the lowest levels at this site—Kenniff Cave on the Mount Moffatt cattle station—are at least 16,000 years old. Troweling and sieving 85 cubic yards of sand and ash, we recovered more than 21,000 stone flakes and waste fragments, most of them quartzite. Among them were some 850 deliberately shaped and retouched artifacts. About 60 percent of the artifacts were either broken or only lightly worked. As a result the total number of artifacts available for rigorous classification and measurement was little more than 350.

The sequence of stone artifacts at Kenniff Cave constitutes the point of departure for any current discussion of Australian prehistory. Two facts give the Kenniff Cave collection its special significance. First, it includes examples of most of the prehistoric implement types known in Australia, arranged in stratigraphic order. Second, the age of the collection ranges from the immediate past back to the late Pleistocene.

For those accustomed to the richness of Old World Paleolithic sites the Kenniff Cave assemblage will seem a small sample, but it is one of the largest Australian collections to be analyzed. Recent work in the vicinity of Sydney and in the Northern Territory (where one excavation has uncovered more than 2,000 worked projectile points) has been more productive, but the results have not yet been published. My experience elsewhere has often been daunting. An excavation in Victoria yielded 2,300 waste pieces and only eight artifacts; the strata of a South Australian site yielded an average of three artifacts per 1,000 years of occupation.

The Kenniff Cave assemblage includes 261 specimens of the generalized tool termed a scraper. Their distribution fluctuated with depth [*see illustration on this page*]. In the uppermost four feet of the deposit scrapers were relatively rare compared with other types of implement. Throughout the lower layers, however, they constituted 100 percent of all classifiable stone artifacts.

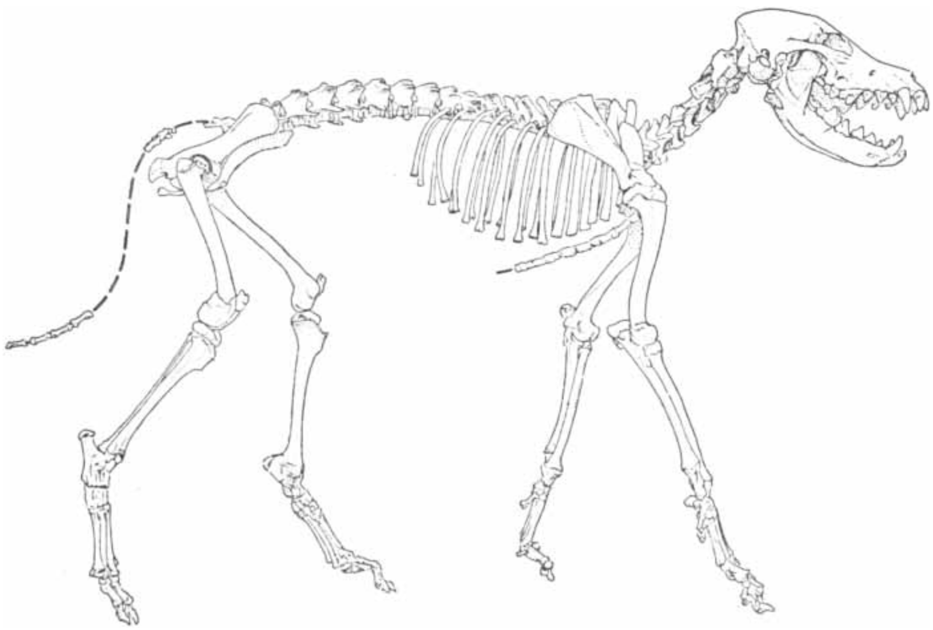
An implicit assumption underlies the use of a descriptive label such as "scraper." In this case the assumption is that scrapers were normally held in the user's hand. Scrapers are thus members of the family of nonhafted stone tools. They are technologically distinct from hafted, composite tools of the kind that presumably made use of the microliths unearthed at the Murray River sites in South Australia. Such tools possess handles or other extensions, together with fixatives such as resin or gum or lashings such as hair, vegetable fiber or sinew.

The sandy soil of Kenniff Cave is so acid that, if any such organic constituents of composite tools had once been present, no evidence of them now remains. The distinction between nonhafted and hafted artifacts at Kenniff Cave is therefore a subjective one. Nonetheless, the difference between the percentage of scrapers in the upper strata and in the lower led me to make a basic assumption about the place of nonhafted and hafted tools in Australian prehistory. I postulated that the apparent ignorance of hafting techniques on the part of the early inhabitants of Kenniff Cave was genuine ignorance, and that an extensive phase in the prehistory of the site occupied a period during which this advanced technology was literally unknown. Carbon-14 estimates indicate that this prehafting phase lasted for at least 11,000 years, or from about 14,000 to about 3000 B.C.

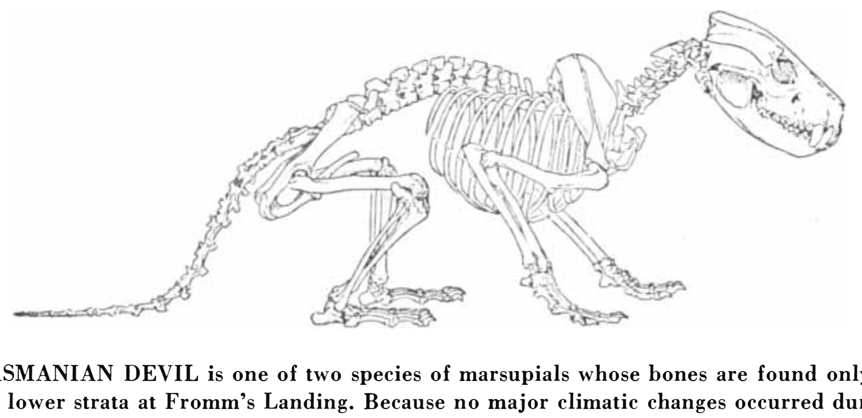
When the Kenniff Cave scrapers were set in their stratigraphic sequence and subjected to careful measurement and analysis, the results of the study confirmed that, as postulated, a continuity of tradition had existed during this period of 11 millenniums; the scrapers showed no significant change either in production technique or in size. Such technological stability—or lack of invention—may be relevant when one considers the social dynamics of a prehistoric culture.

In contrast to the long period of stability attested by the lower levels at Kenniff Cave, the upper levels told a sharply different story. Scrapers appear in diminishing percentages, and accompanying them are various types of small, delicately worked stone artifact. Both the size and the shape of these objects—one specimen measuring only 1.7 centimeters by .5 by .1 centimeter is shown at *e-1* on page 87—imply their diverse functions as components of hafted implements. Such is certainly the case with the long stone knives and with the artifacts identifiable as adze flakes [*"a" and "b" in illustration on page 87*]. The latter objects, which have a characteristic stepped appearance, were the stone working edges of gouges or chisels, composite tools that are widely represented in Australian ethnographic collections. It is probably the case with the pirri points (which may have been mounted on projectile shafts) and geometric microliths (a type of artifact whose purpose is debated on other continents but that is everywhere assumed to be part of a hafted tool).

To judge from the level at which these tool types are first found in the stratigraphic sequence at Kenniff Cave, the technology of hafting materialized there about 5,000 years ago. The impact of the new technology was fundamental. During the 2,000 years that followed—until about 1000 B.C.—most of the characteristic types of stone implement that have been unearthed or collected on the surface by prehistorians in southern Australia were deposited in the upper strata of Kenniff Cave. It is of course conjectural that the acquired knowledge of hafting techniques was the factor that enabled the aboriginal populations of this period to develop greater flexibility in the design of tools, but it is unmistakable in the stratigraphic record at Kenniff Cave that the rate of technological advance accelerated during



OLDEST KNOWN DINGO, the Australian dog, was unearthed at a depth of six feet at Fromm's Landing rock-shelter No. 6. Dating from 1000 B.C., the animal shows no morphological differences from the dingos of today. Man and the dingo were virtually the only predators in prehistoric Australia; they apparently killed off some species of marsupials.



TASMANIAN DEVIL is one of two species of marsupials whose bones are found only in the lower strata at Fromm's Landing. Because no major climatic changes occurred during the 5,000 years the site was occupied, the disappearance of this animal and the Tasmanian wolf from the area in the second millennium B.C. may be attributable to man and the dingo.

FOSSIL MAN in Australia may be most authentically represented by the skull unearthed at Keilor, near Melbourne, in 1940 (*left*); recent additional findings at Keilor suggest that the skull is as much as 18,000 years old. From the time of its discovery the Keilor skull has been considered nearly a twin of a fossil skull from Wadjak in Java (*center*), discovered in 1890 and of unknown age. The discovery of an adolescent skull, possibly 40,000 years old (*right*), in the Niah Caves of Sarawak in 1959 has added another possible precursor to the aboriginal family tree. The scarcity of Australian fossil evidence, however, renders all these conclusions tentative.

the period from 3000 B.C. to 1000 B.C.

Bearing in mind the postulate that a long period of nonhafted-tool technology was followed by a briefer period of the more variable hafted-tool technology, it seems appropriate to review the findings of other archaeologists at a number of sites across the continent. Let us begin with the sites that seem, on the basis of established or assumed chronology, to belong to the nonhafted-tool phase. At the Tombs Shelter in Queensland, near Kenniff Cave, a few artifacts have been found that appear to date back to the eighth millennium B.C. They belong to the nonhafted tradition, and worked stones suitable for hafting are found in the strata above them. At Ingaladdi in the Northern Territory a stone-tool industry that consists of scrapers (and "cores" that were reworked as nonhafted scraping or chopping tools after flakes had been struck from them) yields a date in the fifth millennium B.C. Three sites in New South Wales—Seelands, Curracurrang and Capertee—cover a period from the sixth millennium to early in the second millennium B.C. All three evince nonhafted phases and all are overlain by layers containing artifacts suitable for hafting. These lower-level tool kits contain other nonhafted implements in addition to the ubiquitous scrapers; at least that is how I interpret saw-edged flakes at Capertee and core tools common to all three sites. The cores, flaked on one or both faces, suggest that they were held in the fist for some battering or chopping function. Specimens from Seelands have been measured by the prehistorian J. M. Matthews; he finds them comparable to the Southeast Asian pebble industry named Hoabhinian, after a Vietnamese site.

R. V. S. Wright of the University of Sydney has recently undertaken an important excavation at Laura, on Cape York in northeastern Queensland. The technological sequence he has uncovered may be comparable to that at Kenniff Cave. The same may be true of a ninth-millennium-B.C. occupation site Tindale has found at Noola, near Capertee in New South Wales. The Noola material has not yet been formally described, but Tindale's brief published note allows the relevant finds to be attributed to a nonhafted phase. Some years ago Tindale isolated a stone-tool industry on Kangaroo Island, off the coast of South Australia. No varieties of artifact associated with hafted implements were found there, but flaked pebbles, scrapers and massive core tools were numerous. Tindale postulated a Pleistocene date for the occupation; so far neither excavation nor carbon dating has been attempted. This is an intriguing field for further investigation.

Still another early date is proposed by Tindale and Richard Tedford of the University of California at Riverside for "nondescript" stone flakes found at a surface site at Lake Menindee in New South Wales. Charcoal samples at the site have yielded carbon-14 dates of 17,000 and 22,000 B.C., but detailed evidence for a direct association between the charcoal and the artifacts has not yet been published.

The reconstruction of the most recent 5,000 years of Australian prehistory is further along than the work on the earlier phases, although there is no better agreement on how the evidence from these five millenniums should be interpreted. It is nonetheless possible to relate my hypothesis concerning a significant technological change involving hafting techniques during this period to the culturally oriented descriptions of findings by other workers. Much of this work is in progress, and since it would be improper to anticipate the results here the survey that follows—like those that have preceded it—draws primarily on published reports and my own experience.

The Kenniff Cave site in Queensland is more than 1,000 miles from the Devon Downs and Fromm's Landing sites in the Murray River valley of South Australia. In comparing the equivalent upper and later levels of the three sites, however, it is worth noting that Kenniff Cave lies in the same system of river valleys as Devon Downs and Fromm's Landing. The upper levels at Kenniff Cave, like those at the South Australian sites, contain pirri points, microliths, adze flakes and other stone artifacts representative of the hafted-tool tradition. There are no bone muduks at Kenniff Cave, but the acid sands of the site would have destroyed any that had been buried. Significantly there is considerable contrast between the size and finish of similar artifacts from the Queensland and the South Australian sites. This tends to confirm my view that artifacts of the same general type were subject to a process of differentiation.

Although many more excavations are needed to bridge existing gaps in knowledge and lay the foundation for valid generalizations, some hint of the rate at which the concept of hafting diffused among the aboriginal populations is provided by the following carbon-14 results. As I have noted, the Fromm's Landing site and the upper strata at Kenniff Cave belong to the early third millennium B.C., and a layer containing pirri points at Devon Downs dates from later in the same millennium. This parallels the age of the oldest hafted types of stone artifact found in New South Wales, those unearthed at Seelands. The Ingaladdi site in the Northern Territory has yielded pirris and other points, some finished on one side and some on both. These points appear with relative suddenness in a layer that has not been dated. A stratum a few inches below the points, however, contains an assemblage of nonhafted artifacts that has been dated to the latter part of the fifth millennium B.C. It begins to look as if the period centered on the fourth millennium B.C. will prove to be a crucial one in the reconstruction of Australian prehistory.

For many years a key figure in the documentation of the prehistory of New South Wales has been Frederick D. McCarthy, principal of the Australian Institute of Aboriginal Studies; his excavations at Lapstone Creek and Capertee have provided a working basis for systematizing evidence from elsewhere in the state. Recent investigations in the Sydney and New England areas of New South Wales have served to test and elaborate McCarthy's pioneering studies. Under the sponsorship of the institute Wright and J. V. S. Megaw of the University of Sydney have carried out important excavations, and Isabel McBryde of the University of New England has undertaken an all-inclusive field survey.

McCarthy believed he had isolated two cultures at Lapstone Creek. The earlier of these he called Bondaian; its characteristic tool is a small, asymmetric, pointed blade reminiscent of a penknife, with a sharp cutting edge and a blunt back edge [*"d" in illustration on page 87*]. The later culture he named Eloueran; the characteristic tool is a flake shaped like a segment of an orange, with its back heavily blunted and its cutting edge often polished by use. Also found in the Bondaian levels are geometric microliths. This is scarcely surprising when one considers that the skills needed to make microliths and small blades are essentially the same. Both require as their raw material thin blades, one edge of which is then artificially blunted.

Carbon-14 dates from Curracurrang, Capertee and Seelands place the characteristic artifacts of the Bondaian and the Eloueran cultures in the second and first millenniums B.C. Both could be assigned to my hafted phase. This emphasis on technology rather than on any cultural context accords with McCarthy's own observation that, in spite of fluctuations in the fortunes of specific traits, there is an underlying similarity in the stoneworking techniques of the two cultures.

The present rub in classifying Australian artifacts is how to decide where to draw boundaries and what degree of emphasis to give single culture traits. Within each group of artifacts there are variations in size, shape and trimming. What criteria, for example, distinguish a thin Eloueran flake from a large, crescent-shaped microlith? It is unsatisfactory to lump all the pirris together, and there is diversity even among the bone muduks. The truth is that early workers, myself included, selected an ideal type and then blurred the edges of distinction by treating deviations from this ideal as atypical, even though the deviations possessed inherent definable characteristics. Today, in common with the trend in archaeology around the world, the analysis of artifacts in Australia has shifted away from the subjective methods of the past toward laborious quantitative definition. Most assemblages that have been excavated recently are undergoing rigorous statistical investigation. The Bondaian-Eloueran problem should benefit from this objective approach.

I have presented evidence in support of the view that Australian prehistory can be divided into two phases, distinguishable by a change in technology from the exclusive use of nonhafted, hand-held stone artifacts to the employment of many specialized stone artifacts that were hafted to form composite implements. What of the people who used these nonhafted and hafted tools? Some of the most interesting evidence for the antiquity of man in Australia comes from gravel quarries at Keilor on the outskirts of Melbourne, where a creek has cut a series of terraces. A human skull was unearthed there in 1940, but its exact origin became a matter of dispute. In attempting to resolve the controversy, Edmund D. Gill of the National Museum of Victoria has obtained a series of carbon-14 dates for the Keilor quarries; his earliest date, centered around 16,000 B.C., is for charcoal taken from a point some feet below the 1940 level of the quarry floor. The crucial issue is whether this charcoal is from an aboriginal campfire or from some natural conflagration. The fact that a number of stone artifacts have been found in the creek bed and embedded in the banks of the terraces does not automatically mean that the charcoal is also the work of men. These objects lack the authority of artifacts excavated from undisturbed stratified de-

EXCAVATION AT KENNIFF CAVE revealed evidence of human occupation from about the 14th millennium B.C. until the present. The paler strata along the walls of the 11-foot pit represent periods when the shelter was virtually deserted by prehistoric hunters; the darker strata are rich in organic material. The outline paintings of human hands on the overhanging rock were made by aboriginal tribesmen who have used the shelter during recent years.

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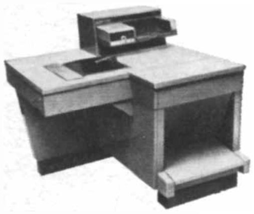

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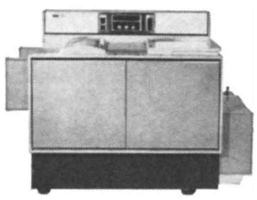

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TOP PHOTO: LDX (Long Distance Xerography) scans, transmits and reproduces images over distances limited only by the transmission medium. Something like it may print the morning paper at your breakfast table some day, or help you manage a global business.

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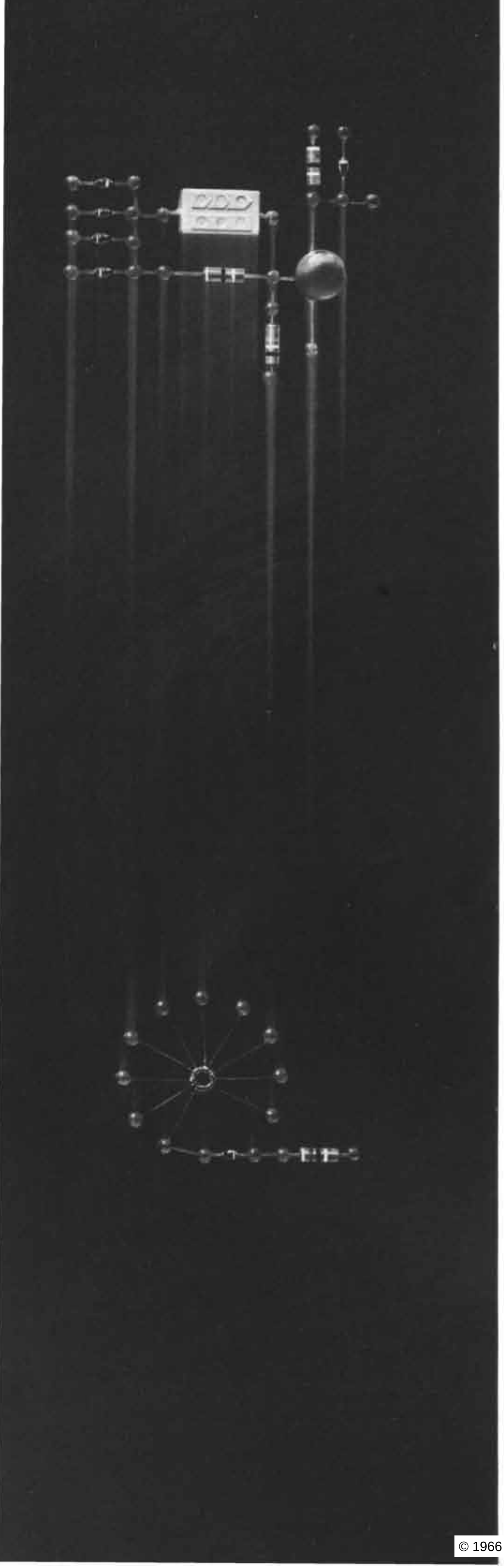
In 1965 the evidence favoring the authentic antiquity of the Keilor skull suddenly multiplied. Two miles from the scene of the 1940 discovery earth-moving equipment exposed a human skeleton that was in a fair state of preservation. Preliminary indications are that the skeleton belongs to the same level of terrace as that proposed for the Keilor skull. Charcoal is plentiful at the site, although some of it—tree roots that were burned where they had grown—is clearly the work not of man but of nature. Artifacts are also present. Obviously men lived here during the period of terrace formation, and carbon-14 determinations should establish the age of the site. The National Museum of Victoria is coordinating the investigations now under way at Keilor.

Man may have arrived in Australia at the time of the continent's climatic climax, when inland rivers flowed, lakes brimmed and the giant herbivorous marsupials flourished. In any case he almost certainly played a role in altering the ecological character of the continent (and, less directly, the soil) through selective hunting activities and frequent burning of vegetation. One ecological effect resulted from man's introduction of the dog. Man and the dingo together represented a scourge to the prehistoric fauna; the two were virtually the sole predatory carnivores on the continent. What caused the extermination of numerous marsupial species: man and dog or climatic change? The findings at Fromm's Landing indicate that the two carnivores played their part. There is nothing in the evidence to imply that, during the 5,000 years spanned by the deposits at the site, there were important fluctuations in climate that might have exerted an ecological influence. Mammal bones identified at Fromm's Landing represent 685 individuals of 31 species. In two cases—*Sarcophilus*, the Tasmanian devil, and *Thylacinus*, the Tasmanian wolf—there are indications that the species became extinct there during the second millennium B.C. It is relevant that a 3,000-year-old stratum at the site has yielded the skeleton of a dingo; this is the earliest authenticated occurrence of the dingo in Australia.

What were the racial origins of the prehistoric Australians? This question has been much debated, but the debate is conducted virtually in a vacuum because of the scarcity of early human fossils in Australia. Until the discovery of the Keilor skeleton there was not one such fossil whose authenticity was unchallengeable. Now the Keilor skull found in 1940—and perhaps a badly crushed skull from Talgai in Queensland—may gain a more respectable status. Still, two or three specimens make a small sample for determining the origin of a race. Two fossil skulls found at Wadjak in Java, to the northwest of Australia, have been proposed as a link in aboriginal evolution; these fossils, however, remain undated. A skull from the Niah Caves in Sarawak, to the north of Java, is possibly 40,000 years old and has also been compared with prehistoric Australian remains. Caution must be the keynote when there are such wide spatial gaps in the fieldwork and so few fossils.

The origin of the prehistoric inhabitants of Tasmania, the large island to the south of Australia, also remains an open subject. The last Tasmanians (none survived the 19th century) were an ethnographic rarity: a society using stone tools without hafts of any kind. Studies of changes in the sea level during Pleistocene times have made it a tenable theory that the Tasmanians walked to Tasmania from Australia when the intervening strait was dry land; carbon-14 estimates have established their presence in Tasmania 8,000 years ago. During the past two years Rhys Jones of the University of Sydney has achieved striking success in fieldwork in northern Tasmania. When his carbon samples are dated and his human skeletal material is analyzed, Tasmanian archaeology will have entered an objective era.

Now that it seems certain that Australia was colonized in Pleistocene times, the inadequacy of evidence on this period not only in Australia but also in its northern neighbors such as New Guinea is painfully apparent. If we are to retrace the steps of Australia's first colonists, detailed studies of changes in the sea level are required. If we are to seek out their early patterns of settlement, we need far more precise dating of environmental changes in the continent's interior. With much of Australia archaeologically unexplored, with increasing numbers of investigators undertaking fieldwork and with carbon-14 chronologies providing new perspectives, these are exciting times for the study of the continent's prehistory. It is certain that during the next few years the nearly blank outline map of that prehistory will come to be filled with detail.



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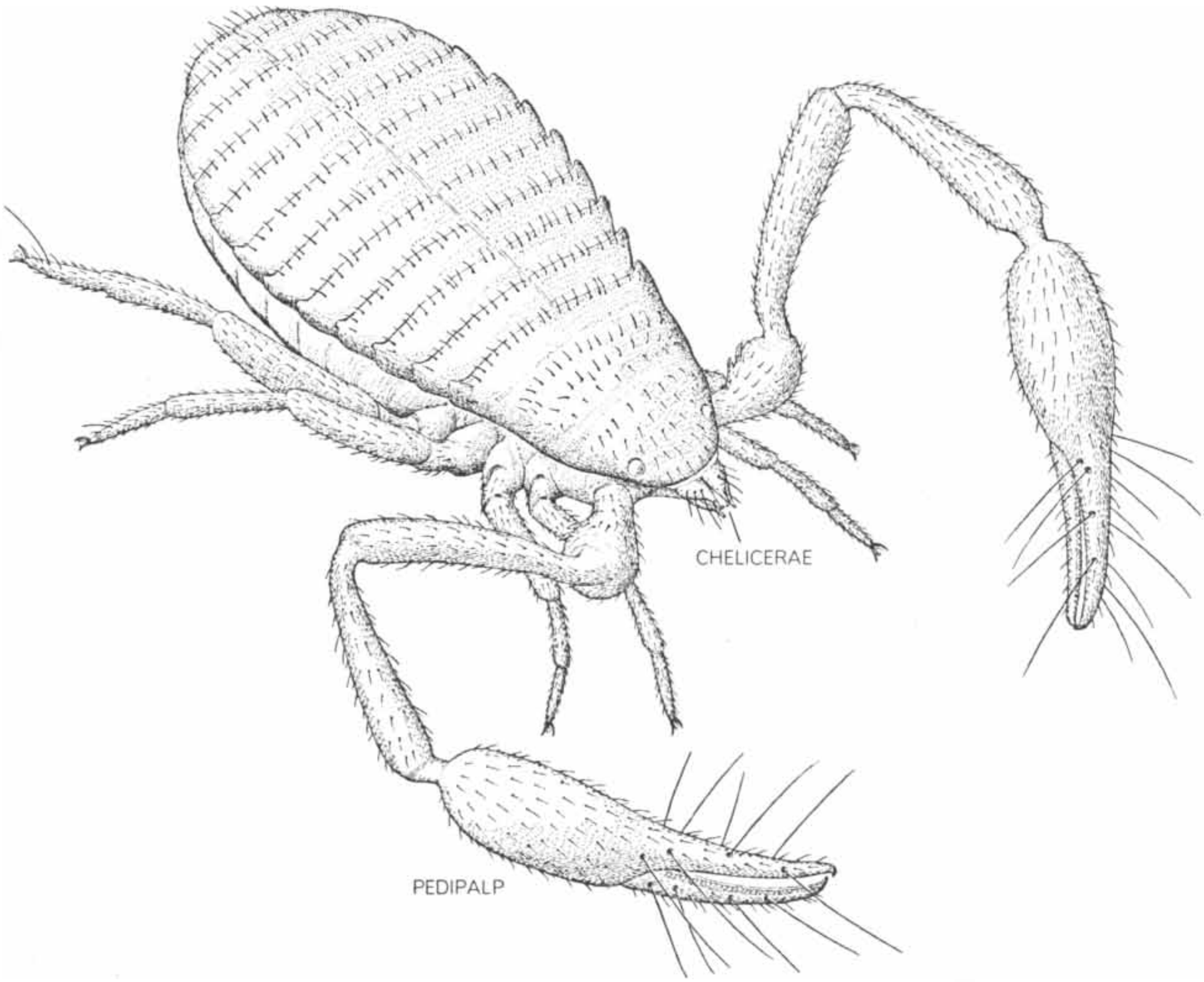
FALSE SCORPIONS

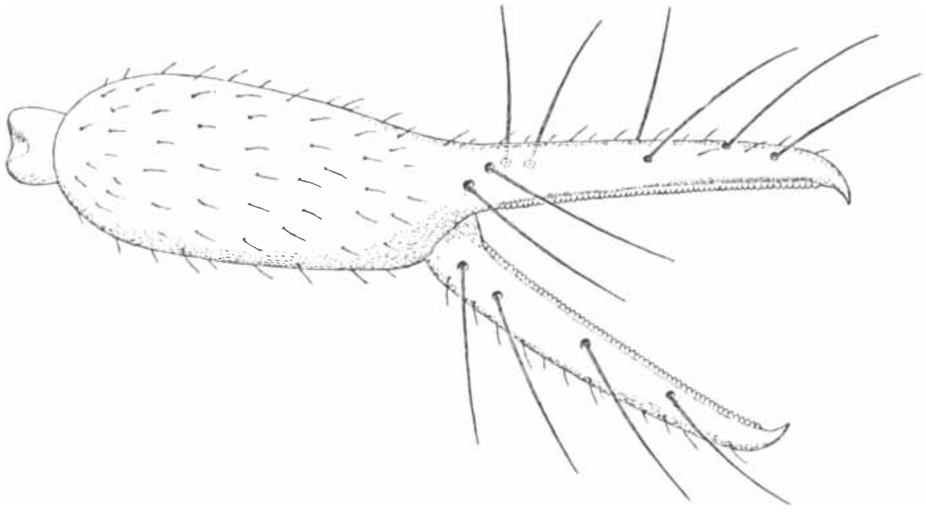
These small, harmless relatives of the true scorpions are surprisingly abundant in woodland regions. Their retiring way of life conceals some fascinating patterns of behavior

by Theodore H. Savory

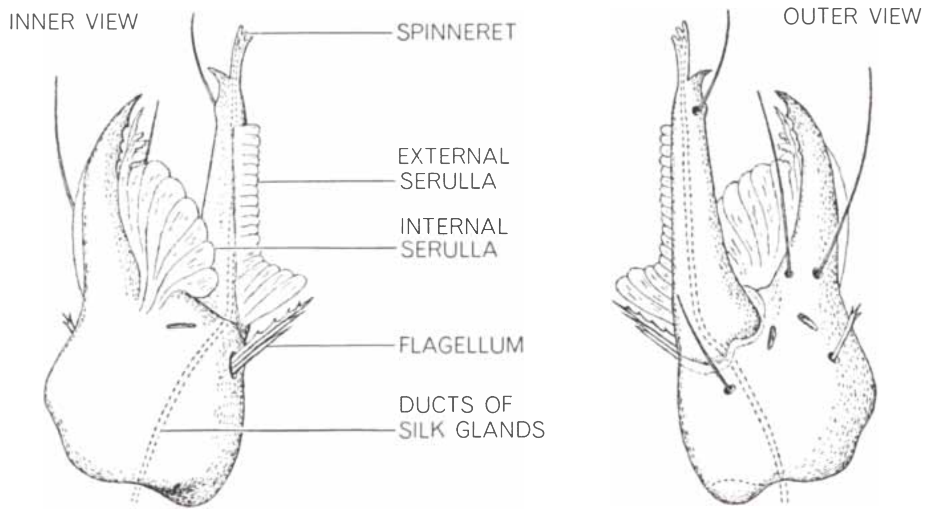
If a group of animals delights a naturalist—and the animals happen to be numerous, easy to observe and yet unfamiliar to most people—he feels compelled to share his pleasure with others. Such is my feeling about the false scorpions, harmless relatives of the venomous true scorpions that are well known and feared in hot countries. The false scorpions look like tiny tailless scorpions less than a quarter of an inch long. There are nearly 2,000 species of them, constituting an order in the class Arachnida (which includes the spiders, the harvestmen, or "daddy longlegs," and the mites). False scorpions can be found in wooded regions throughout the world. The fact that many people never see one is due to the animals' retiring way of life, not to their scarcity. In the woods or a subur-



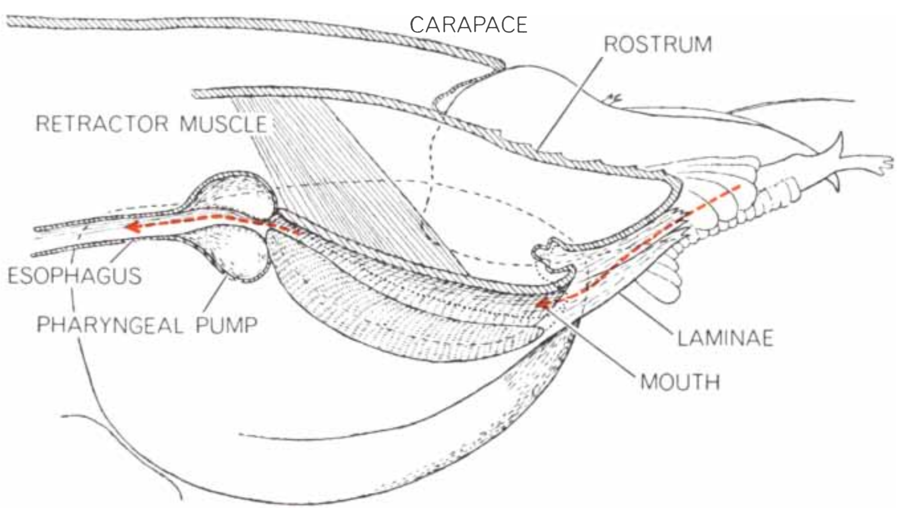
FALSE SCORPION of the species *Chelifer cancroides* is enlarged some 30 diameters in this drawing. The nearly 2,000 species of false scorpion make up an order in the class Arachnida. The large pedipalpi that resemble a crab's claws are used mainly as weapons in predation. The small organs between the pedipalpi, the chelicerae, contain extensions of the mouth that flood the prey with digestive juices and later imbibe it in dissolved form. The chelicerae also contain glands that secrete a silk similar to that of spiders.



PEDIPALP is used by the false scorpion as a pincer for attack and defense. The animal's two pincers are huge in proportion to its body; when extended, they about double its length The long hairs on the pincers (and chelicerae) are thought to be sensitive organs of touch.



CHELICERA, one of two manipulative organs near the mouth, consists of a fixed finger (smaller extension) and a movable one. Ducts of the silk glands open onto the spinneret at tip of movable finger; other ducts open onto the serrulae. Role of the flagellum is unknown.



MOUTH PARTS of the chelicerae include a set of grooves and channels through which digestive juices flow outward and food is imbibed. Arrows show route of intake. Retractor muscle and pharyngeal pump are part of sucking apparatus. Laminae are extensions of mouth.

ban backyard 100 or more specimens can be taken from an area of 10 square yards in a month if one knows their likely whereabouts.

The best way to get to know false scorpions for oneself is to go into a wooded area with a wide-mesh strainer and sift handfuls of fallen leaves in it so that any small animals in the leaves will shower down on a sheet of newspaper. If one is reasonably lucky, there will be one or two false scorpions among them. An aid to identification is provided by Robert Hooke's description of three centuries ago: a "crab-like insect" with 10 legs, two of which resemble crab's claws, and "other hands, very near placed to its mouth." At first, after falling onto the paper, the animals will have drawn in their legs and will be still, but after perhaps half a minute they will begin to move. They stretch out their legs, spread their large pedipalpi (the crablike claws, or pincers) in front of them like the antennae of an insect and begin to walk slowly across the paper with an oddly dignified air that distinguishes them at once from any other living creature. As soon as one sees them thus one cannot help being struck by their character: here are small animals displaying in their general bearing the equivalent of human personality.

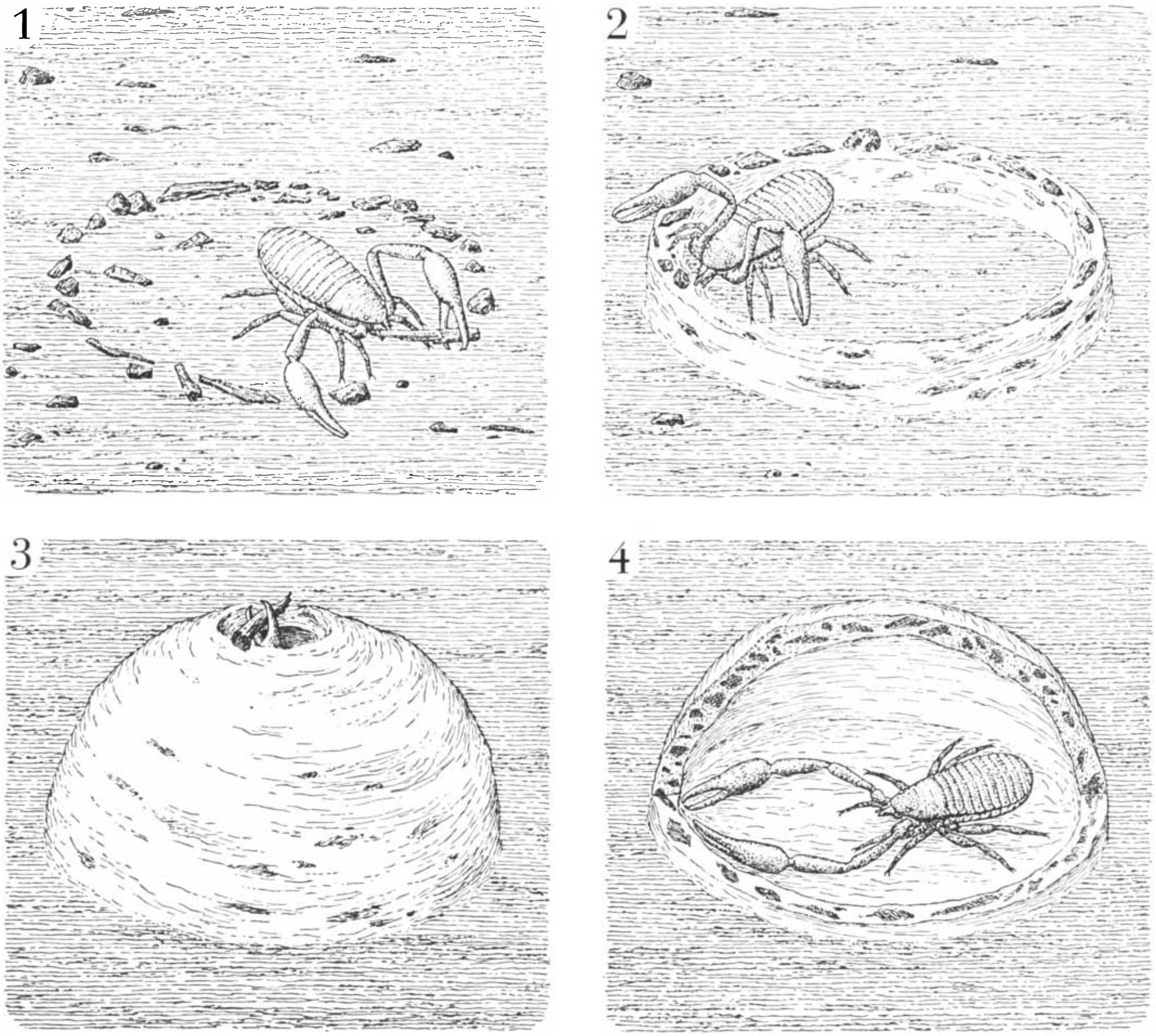
Perhaps as the false scorpion proceeds across the paper it meets some other animal and touches it lightly with one of the long, hairlike bristles that cover its limbs. The reaction is immediate. The pincers are withdrawn and the scorpion darts backward with a speed that is astonishing compared with its earlier majestic progress. This rapid retreat is characteristic of false scorpions. Not many animals can go backward as easily as forward and very few can go backward much more rapidly.

After such an intriguing introduction false scorpions invite closer acquaintance. To bring them home alive and install them in suitable cages does not require elaborate arrangements. Laboratory zoologists often follow the soulless procedure of putting the false scorpion in a plain glass tube with a loose cotton plug and a slip of paper on which the animal can rest. I have had good results using a glass dish, such as a Petri dish, with moist filter paper on the bottom. If one plans to keep the animals for long periods, it is well to cover the bottom of the dish with small stones and on top of this to sprinkle a layer of white sand. The gravel can be kept moist (an essential condition) and the sand makes a light background against which the inhabitants can be seen clearly. A few small stones will provide them with the shelter they seem to enjoy. The dish must be covered because the animals are able to climb the vertical sides.

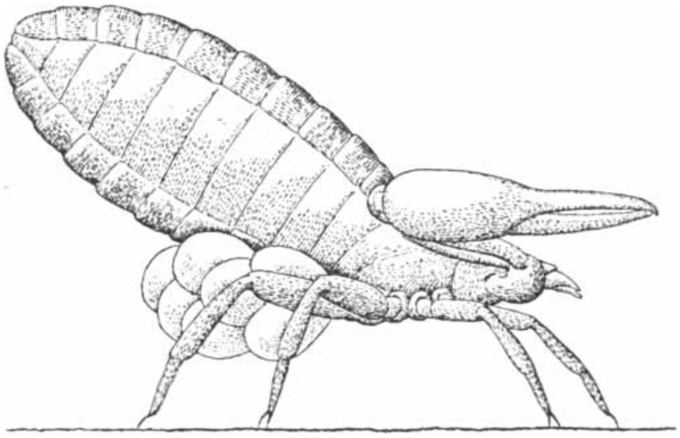
In captivity false scorpions will live for weeks or months in apparent comfort. They must, of course, be fed. False scorpions, like true scorpions and like spiders and daddy longlegs, are predators that normally catch and kill their prey. Unlike daddy longlegs, however, they refuse to eat anything that is not alive, and so living things must be supplied to them. An obvious source of food for a false scorpion would be the small animals that fell through the strainer in its company. Among these may be daddy longlegs, tiny spiders, mites, flies, the wingless insects known as springtails and other false scorpions. I have seen false scorpions respond to each of these potential meals. The ones I have kept would neglect the mites, flies and daddy longlegs, and would back away from spiders. Once I swung a small spider on its thread into the jaws of a false scorpion. The spider was killed instantly, but no attempt was made to eat it. The springtails were seized with avidity; a dozen or more would vanish in an hour, leaving no trace. Occasionally false scorpions (as well as true ones) will make a meal of a wounded or ailing member of their own species, but this is rare if other food is available.

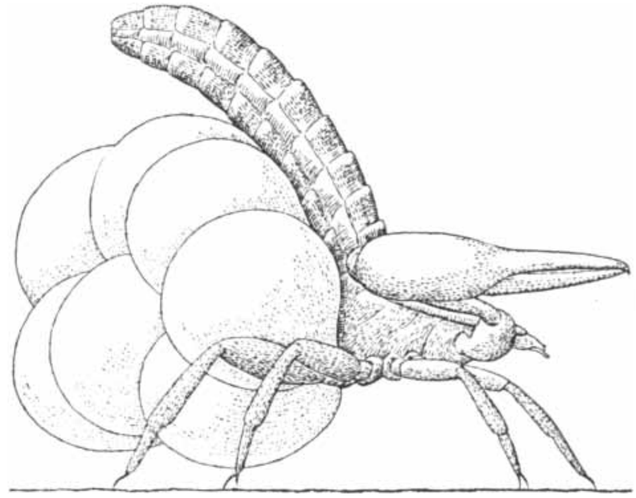
The way in which a false scorpion feeds is unusual, with the process of digestion beginning before the animal has taken its prey into its body. The victim is grasped by the pincers; then the chelicerae—the small organs Hooke described as hands located near the mouth—tear it open and are thrust into it. Glands in the chelicerae flood the prey with a saliva containing digestive ferments. These convert the victim into a nutrient solution that the false scorpion imbibes.

One of the surprising aspects of a false scorpion's feeding is the complete consumption of its food. Whereas spiders and daddy longlegs leave a debris of emptied legs and dried carapaces, false scorpions leave hardly any trace of their meal. In another respect, however, the feeding pattern of false scorpions is like that of other arachnids: they do not require frequent, regular meals and



BUILDING OF A COCOON in the open is depicted in four stages. The false scorpion surrounds itself with "bricks" of wood and stone (*1*). Brushing the bricks with its chelicerae to supply a silken mortar, it assembles them into a dome (*2 and 3*). Finishing the cocoon, which may be used for molting, hibernating or laying eggs, the animal rests (*4*). A cocoon in a crevice would be cylindrical.





INCUBATION CHAMBER of the mother false scorpion is secreted by glands in the walls of her oviduct and remains attached to her body (*left*). The eggs are fertilized as they descend into the chamber. The larvae come out of the individual egg membranes still attached by short beaks to the mother. After several days she forces yolk into their bodies, which become greatly inflated. The incubation chamber swells, flattening the mother's abdomen (*right*). For about two weeks the larvae absorb this meal; they then reach the first nymphal, or preadult, stage. At this point they break out of the chamber and leave the cocoon in which the process takes place.

seem indifferent to semistarvation. Zoologists who keep spiders know that they do not have to feed the animals often and that many a spider is overfed if given one fly a day. False scorpions may be fed daily, but Herbert W. Levi, who studied them at the University of Wisconsin, found that adults fed once a week seemed better off than those leading their natural lives in the woods.

At the start of a meal—often, in fact, when prey has only been sighted—a false scorpion begins to rub its chelicerae with its pincers. This procedure is not analogous to a man's washing his hands, although it looks as if the animal were scraping or sucking dirt from its limbs. Actually the pincers are cleaning the mouth parts of the chelicerae, a complex arrangement of grooves and channels through which digestive juices flow outward and digested solutions are sucked inward. It is essential that the chelicerae be kept clear for this dual purpose. The pincers are fundamentally weapons of attack and defense and can remain soiled.

The chelicerae have yet another important function. They contain silk glands that secrete a viscous fluid that hardens into fine thread. The silk is probably similar to that of spiders (no biochemist has chosen to analyze it), but for false scorpions its use is limited. Spiders have so exploited the silk they produce that their lifelong behavior is dominated by it. False scorpions use their silk only on the occasions when they construct a cocoon-like shelter in which to pass the winter, lay their eggs and shed their skins.

False scorpions grow discontinuously by molting the hard outer skeleton that covers their body. They undertake this major operation in the safety of a cocoon built in one of two forms, depending on the physical surroundings. If a cocoon is to occupy a crevice, it will consist of a circular base or mat of silk with sides rising to the roof of the crevice and making a roughly cylindrical chamber. If the chamber is built in the open, it will resemble an igloo with a dome-shaped top [*see illustration on preceding page*].

Before construction begins the false scorpion can be seen wandering about as if it were searching for a suitable spot. Once this is found the animal starts to collect material such as small pieces of wood or bark, bits of stone and so on, which it arranges in a circle around itself. Joining the pieces with strands of silk, the animal lays foundations on which it places more wood and stone, brushing each addition to the rising wall with its chelicerae to provide a silken fixative. Soon it becomes necessary to go outside and collect more material. The false scorpion steps over the wall, picks up a splinter or two in its pincers, transfers the material to its chelicerae and carries it back. One is reminded of a bird flying back to its nest with straws in its beak.

As the wall rises and arches inward, the unfilled hole in the center gets smaller, yet the scorpion seems to have little difficulty forcing its way in and out. Finally the last brick is laid and the builder is imprisoned in the structure. This incarceration does not stop its activities; the animal continues to add silk to the wall of the cocoon, sweeping its chelicerae over the surface like a paintbrush. Silk threads emerge and coalesce, eventually to cover the entire inside with a continuous sheet of what may well be described as silk wallpaper. Several days may have passed before this last phase is complete.

Secure now from outside interference, the false scorpion will shed its skin. First it rests for a day or so with closed pincers and legs rigid and straight. Its abdomen, normally flattened, swells by becoming narrower and deeper. Suddenly the carapace breaks loose from the front part of the body, rising upward with an almost explosive force. Then the old skin begins to slide backward to the accompaniment of some twitching of the abdomen. The legs remain still. The whole affair, which is much more passive than the strenuous molting of a spider, may occupy the better part of a day. When it is over, the false scorpion moves its legs into their usual position and is able to walk about. It proceeds slowly for the first day or two; if it falls over, it will have some difficulty righting itself. In the course of their lives false scorpions, at least those that have been thoroughly observed, shed their skin four times.

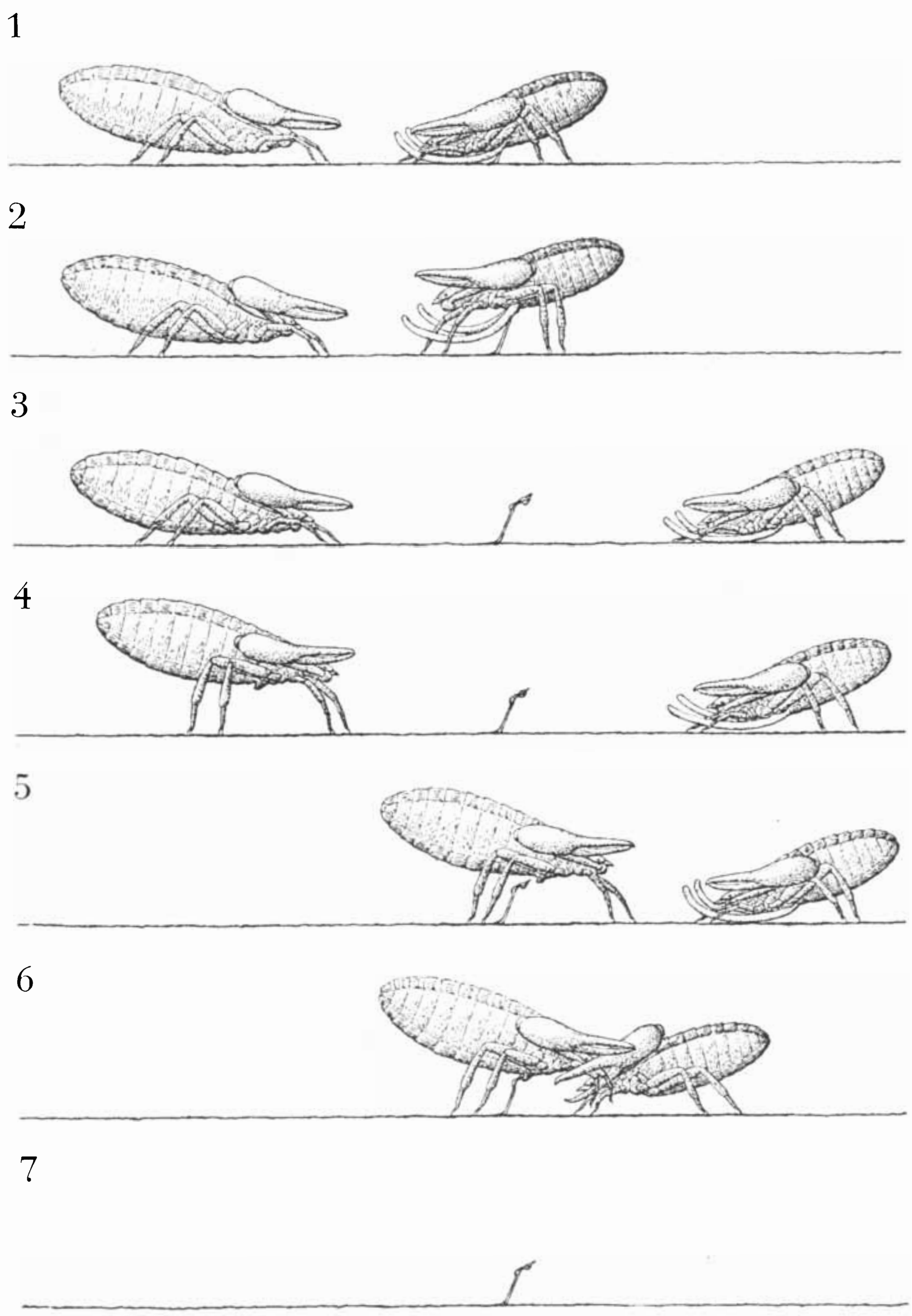
Clearly the chelicerae of a false scorpion are organs of great value and versatility, although to the casual observer they are overshadowed by the enormous pincers. Each chelicera consists of two parts [*see middle illustration on page 96*]. There is a broad, flat "hand," elongated at one point to form a fixed "finger." Close to the fixed finger there is a movable finger that forms a grasping organ. At the base of the fixed finger a group of spines forms a flagellum of uncertain function; on its inner edge is a comblike organ called the interior serrula. The fixed finger ends in a sharp point; at the tip of the movable one is a rounded spinneret onto which the ducts of the silk glands open; this spinneret takes a wide variety of forms in the different genera of false scorpions. A second comb, the exterior serrula, is found on the inner edge of the fixed finger. In addition to the silk and digestive glands there are two other glands in each chelicera; one of them secretes through a single duct near the tip of each finger, the other through many ducts onto the combs.

Normally the chelicerae are held horizontally and pointing forward, but they can be moved and in fact are very active. They serve as jaws in feeding, cutting open the prey. They also pick up, hold, carry and distribute silk. The combs are used in cleaning the rostrum (a beaklike part of the mouth) and other mouth parts. They are lined with hairlike spines that probably serve as highly sensitive organs of touch. It is interesting to note that these spines are not scattered haphazardly over the surface; they occupy specific positions, and students can identify each one.

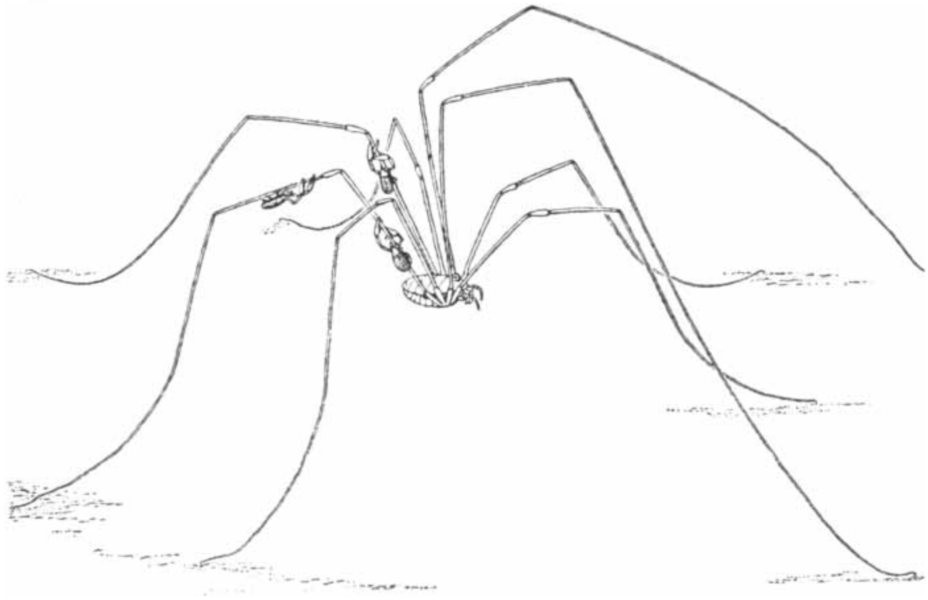
As I have indicated, false scorpions build cocoons for reasons other than molting. In the winter they build hibernation chambers (false scorpions may live for two or three years), which are shared by two or three individuals. Females build a variation of the cocoon, in the shelter of which they lay their eggs and bring up their young. The females also produce, as an extension of their bodies, an incubation chamber in which the eggs are actually laid. This chamber is made of a secretion from glands in the wall of the oviduct; when the secretion is extruded, it takes the form of an inverted mushroom with the stem attached to the oviduct opening [*see illustration on opposite page*]. The eggs are fertilized during their downward passage and about 20 fall one by one into the chamber. The chamber is not detached, as are most egg cocoons, but remains in position under the abdomen of the female. The eggs develop within it.

When the larvae break out of the egg membrane, they are still attached by short beaks to the body of the mother, absorbing a nourishment produced by the degeneration of her ovaries. A few days later a remarkable event takes place: the mother, by muscular contraction, forces yolk into the bodies of the young larvae. They become inflated to about three times their previous size; the incubation chamber is enlarged and pushes the shriveled abdomen of the female into an almost vertical position. For about two weeks thereafter the inflated larvae absorb their enormous meal and develop into the first nymphal, or preadult, stage. This development involves the first molting of the false scorpion's life. In the first nymphal stage the larvae break out of the cocoon and begin to lead independent lives. They feed themselves and molt three more times before becoming adults about a year later.

Adulthood is defined by mating, a step preceded by a complicated courtship that marks the end of the false scorpion's retiring style of life. The courtship begins with a form of dance in which the male takes the initiative. He stops close to a female, shaking his abdomen and waving his pincers. The female is quiet with a significant still-



COURTSHIP of false scorpions takes the form of a mating dance begun by the male. He approaches the female (*from the right in this drawing*), shaking his abdomen and pincers (*1*). If she moves, he halts the procedure; if not, he drops a vertical thread from his genital opening (*2*). This hardens into a pillar on which he leaves seminal fluid before backing away (*3*). The female advances (*4*) and straddles the pillar (*5*). The male then returns and shakes her by the legs (*6*) to ensure that the fluid is detached within her body. The drawing is based on studies by Max Vachon of the National Museum of Natural History in Paris.



PHORESY, a method of travel used occasionally by false scorpions, involves grasping the legs of an animal such as a "daddy longlegs" so that the scorpions can move long distances without expending energy. Phoresy seems advantageous when food in an area is scarce.

ness: if she moves, the male stops his maneuvers; if not, he draws closer until his forelegs almost touch her. At this point he suddenly lowers his body and on rising is seen to have left a viscous vertical thread, drawn from his genital opening. In a few seconds the thread has hardened into a tiny pillar known as a spermatophore. The male backs away, leaving a drop of seminal fluid on the pillar that stands between the two dancers. Signaled by a particular movement of the male's pincers, the female moves forward in such a way that the spermatophore enters her body. The male then grasps her by the legs and shakes her vigorously so that the drop of fluid is detached and remains within her to fertilize the eggs when they are laid. The two false scorpions then part. If they meet again, they show no particular interest in each other.

Courtship of this kind, which is widespread among arachnids and other invertebrates, is easier to describe than to explain. It may have two functions: first, the mutual stimulation of the two participants, and second, the exact placing of the female so that the spermatophore will be oriented properly. Other interpretations have been suggested; it cannot be said that zoologists agree on the best explanation.

The false scorpions that live among the fallen leaves in woods are the easiest to find, but many others live in quite different kinds of surrounding. Some are almost always to be found under the loose bark of dead or dying trees, and others abound among stones and slates. A few species remain close to the sea; some live among rocks that are regularly covered by the tide. Others come into human habitations and may be found among books in the library or clothes in the closet. (A common name for the animal is "book scorpion.") Some species live in ants' nests, but this comes as no surprise; these habitats are popular with animals of almost every class and order. It is surprising, however, to find false scorpions in beehives. There is even some evidence that they eat the pollen from the bees' legs.

Having a wide choice of possible habitats is a sign that an animal is a success in the evolutionary struggle. Insects and birds are obvious examples of successful types of animal life; so too are spiders and roundworms. The success of false scorpions involves their ability to adapt their bodies to various degrees of moisture in the surrounding air. Almost all invertebrates that live on land need some protection against the excessive loss of water. Indeed, the retention of water often determines their behavior and distribution. Some invertebrates, for example, may be active only after sunset; they lie in shelter during the day. It is therefore of considerable interest to learn that false scorpions can survive in wholly dry air for times varying from a few hours (for the species that frequent fallen leaves) to many days (for those that live in dry places such as houses).

As a conclusion to this brief account of the order of false scorpions, one of its unsolved mysteries should be mentioned. It has long been known that false scorpions will at times cling to the legs of flies, daddy longlegs and other creatures so that they can be transported for relatively long distances with no expenditure of energy. False scorpions are not parasites, and there is no question of their having got into such a situation by accident. This mode of travel, known as phoresy, seems to be a response to specific conditions of the environment. Only a few species of false scorpion have been observed to use it, and they do so only at certain times of the year. In the periods when phoresy is employed, however, it seems to be the preferred means of transportation. In 1945 Max Vachon of the National Museum of Natural History in Paris recorded 78 false scorpions on the legs of 57 daddy longlegs in one week in August. The largest load carried by any one daddy longlegs was eight individuals. All were of the same species and all but one were mature females.

The fact that the females were mature indicated that phoresy is not reserved for the dispersal of the young. Vachon found that all his specimens had either recently mated or had just produced a brood of young. Since all were in need of food, he concluded that hunger had induced them to seize the legs of flies or daddy longlegs and thus travel to a neighborhood where prey might be more plentiful.

The study of false scorpions is enriched by the beauty and precision revealed when their bodies are examined in the microscope. The intricate design in the construction of their tiny frames is breathtaking. For example, the number and position of the spinelike hairs near the animal's mouth change in a characteristic way during the various stages of its growth. The devout assure us with confidence that the number of hairs on our heads is ordained; in the same spirit the zoologist can state that the very hairs on a false scorpion's jaws are arranged systematically.

Is it useful to know and record such facts? A professor of biology has recently written, "No merit attaches to the pursuit of useless knowledge," adding that the amount of useless information awaiting pursuit is greater than the united ability of men to follow it. If it can be said that any knowledge is useless, his dictum might cause some reflection among those who follow false scorpions. Useful or not, however, the pursuit of some types of knowledge brings an enjoyment unique in itself and a satisfaction one does not want to forgo.

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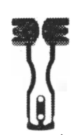

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THE AIRGLOW

A faint but constant light is emitted by a layer of excited atoms and molecules in the earth's upper atmosphere. The events that give rise to this radiation have now been reproduced in the laboratory

by Robert A. Young

The night sky is illuminated by a number of different sources, located both outside the atmosphere and within it. Among the major contributors to this collective illumination are the moon, the stars and the lights of cities. Yet if it were possible in some way to eliminate all the light from these familiar sources, the sky would still not be completely black. In the latitudes of the Temperate Zones a faint multicolored glow would remain, almost uniform in brightness over the entire sky. (In arctic and antarctic latitudes the glow is often overpowered by the light of the aurora.)

The general term for this phenomenon is the airglow; depending on the time of day it is observed, it is also called the nightglow, the twilightglow and the dayglow. Because the airglow is most difficult to observe during the day, studies of the phenomenon have usually concentrated on the nightglow, with the expectation that the processes that give rise to the nightglow are similar to those that give rise to some of the dayglow and the twilightglow (both of which are now under intensive study). Since some quite different processes may be initiated by the direct action of sunlight on the earth's atmosphere, however, considerable differences between the nightglow and the dayglow are to be expected.

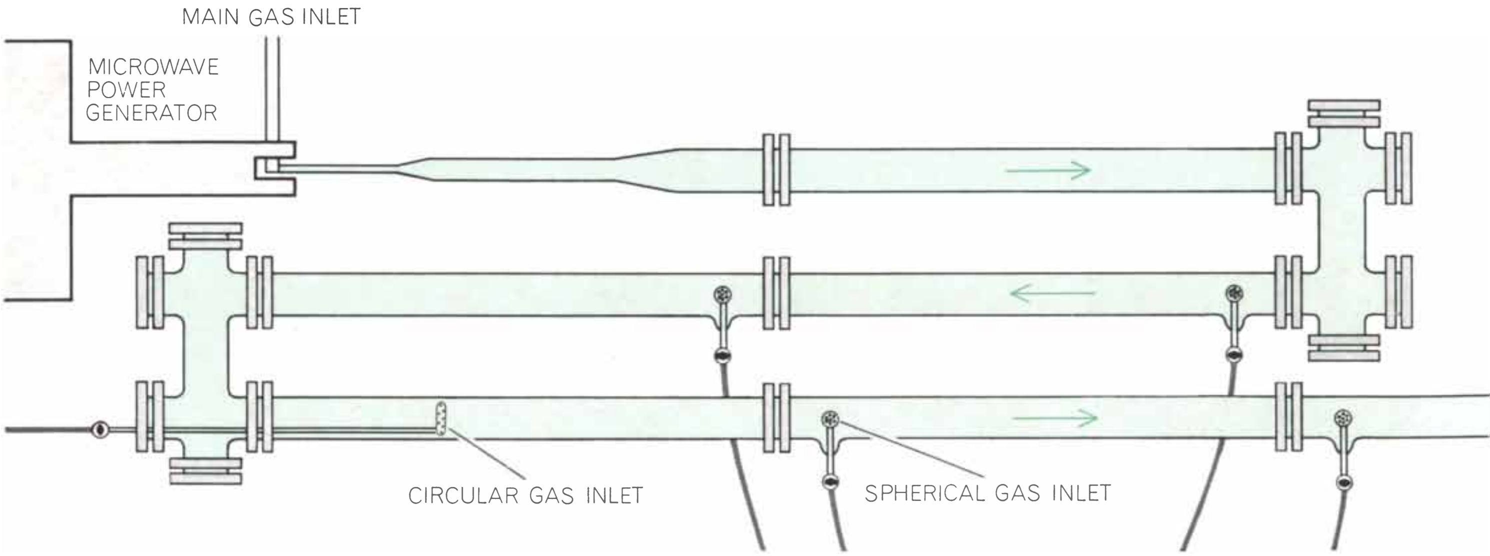
It has long been known that the airglow originates within the atmosphere. The first spectrogram of the airglow, obtained early in this century, was clearly distinguishable from spectrograms of stars or the aurora. Until 1927, however, none of the radiation from the airglow was associated with a specific atomic or molecular event. Then a distinct green line at a wavelength of 5,577 angstrom units in an airglow spectrogram was identified as originating with excited oxygen atoms. Later it was found that most of the airglow radiation in the blue region of the spectrum was associated with molecular oxygen (O_2), and that radiation in the infrared region was associated with the hydroxyl radical (OH). Airglow spectrograms have also revealed the existence of sodium in the upper atmosphere. So far, however, no emission from nitrogen, the most abundant gas in the atmosphere, has been reliably identified in such spectrograms.

For the past few years my colleagues and I in the department of atmospheric sciences at the Stanford Research Institute have been engaged in studying the processes that excite the airglow. We have concentrated particularly on the green spectral line of atomic oxygen at 5,577 angstroms. A large part of our research effort has gone into reproducing this and other airglow radiations in laboratory apparatus [*see illustration on opposite page*].

Before proceeding to a discussion of our experiments, it may be useful to review briefly how excited atoms and molecules emit light. As is often said, the electrons that surround the nuclei in atoms and molecules are in a very general way analogous to planets traveling in orbit around the sun. When an electron is farther from its "sun" than it is ordinarily, the atom or molecule is electronically excited. When the electron returns to its lowest energy level, or smallest orbit, its excess energy is emitted in the form of an electromagnetic wave. In the case of atoms this emission is at a specific wavelength and accounts for a single bright line in a spectrogram. The atomic nuclei in molecules, on the other hand, both revolve around a common center of mass and vibrate back and forth; for each jump of an electron back to a smaller orbit the emission is fanned out into bands by the revolution of the nuclei and the bands are displaced by the vibration of the nuclei.

Most of the atomic lines and molecular bands in the spectrum of the airglow are seldom observed in the laboratory. One reason is that these lines and bands arise from changes in the internal structure of atoms and molecules that seldom happen; in the language of quantum mechanics such changes are said to be "forbidden." Time is an important factor in determining if a forbidden change will occur, and on the atomic scale events that can preempt the forbidden change are plentiful and occur with great rapidity. For example, excited atoms and molecules generally lose their excess energy by radiation approximately a hundred-millionth of a second after being excited. Molecules vibrate some 10 trillion times per second, and collisions between atoms and molecules begin and end in a trillionth of a second. It would appear, therefore, that when forbidden emissions of energy are observed that require a full second (as in the case of the green line of atomic oxygen), 10 seconds or 100 seconds (as in the case of other forbidden emissions of atomic and molecular oxygen), these events must be occurring in extreme slow motion in the atomic world.

ARTIFICIAL AIRGLOW was produced in the author's laboratory at the Stanford Research Institute. The predominantly green color inside the bottom pipe arises from the de-excitation of nitrogen dioxide molecules (NO_2) flowing through the system. A close-up of the spherical gas inlet at the head of the greenish stream appears on the cover. A schematic drawing of the entire apparatus appears on the next two pages.



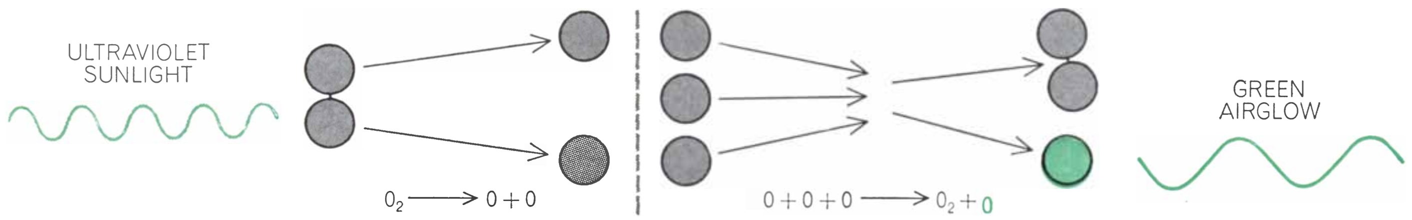
EXPERIMENTAL SYSTEM in which the author and his colleagues studied the excitation of atomic oxygen is depicted schematically on these two pages. Molecular nitrogen gas is admitted to the system at top left, where a microwave discharge dissociates some of the gas into atomic nitrogen. After the gas travels through the two upper sections of the glass pipe, nitric oxide is added to the system through the gas inlets located along the bottom section of pipe. The nitric oxide reacts with the nitrogen atoms to form oxygen atoms and more molecular nitrogen. The oxygen atoms in turn interact to produce excited oxygen atoms, which are de-excited both by emitting light and by colliding with other, nonexcited atoms. (These processes are studied in the large spherical bulb at the far right, where measurements of light intensity are made.) In order to separate these two effects another gas (called a quenching gas) that does not react with nonexcited atomic oxygen but does remove the excess energy of excited atomic oxygen is introduced to the system in measured amounts until it removes excited oxygen atoms as quickly as they are removed by collisions with nonexcited atomic oxygen plus radiation. The amount of quenching gas needed to reduce the light intensity by half verified the author's analysis that attributed the major loss of excited oxygen atoms in the experimental apparatus to reactions with nonexcited oxygen atoms.

Several forbidden emissions of oxygen have been recorded in the spectrum of the airglow. If one were to try to reproduce such emissions in a laboratory experiment at, say, a hundredth of atmospheric pressure at sea level, each excited atom and molecule would collide at least a million times per second with other atoms and molecules; thus there would be a great many opportunities for the excess energy to escape in the collisions before being radiated. Under such circumstances emission from the excited particles is said to be quenched. It is clear that at least part of the reason for the peculiar spectrum of the airglow is the low collision rate of atoms and molecules in the upper atmosphere, where the air pressure is on the order of a millionth of the pressure at sea level.

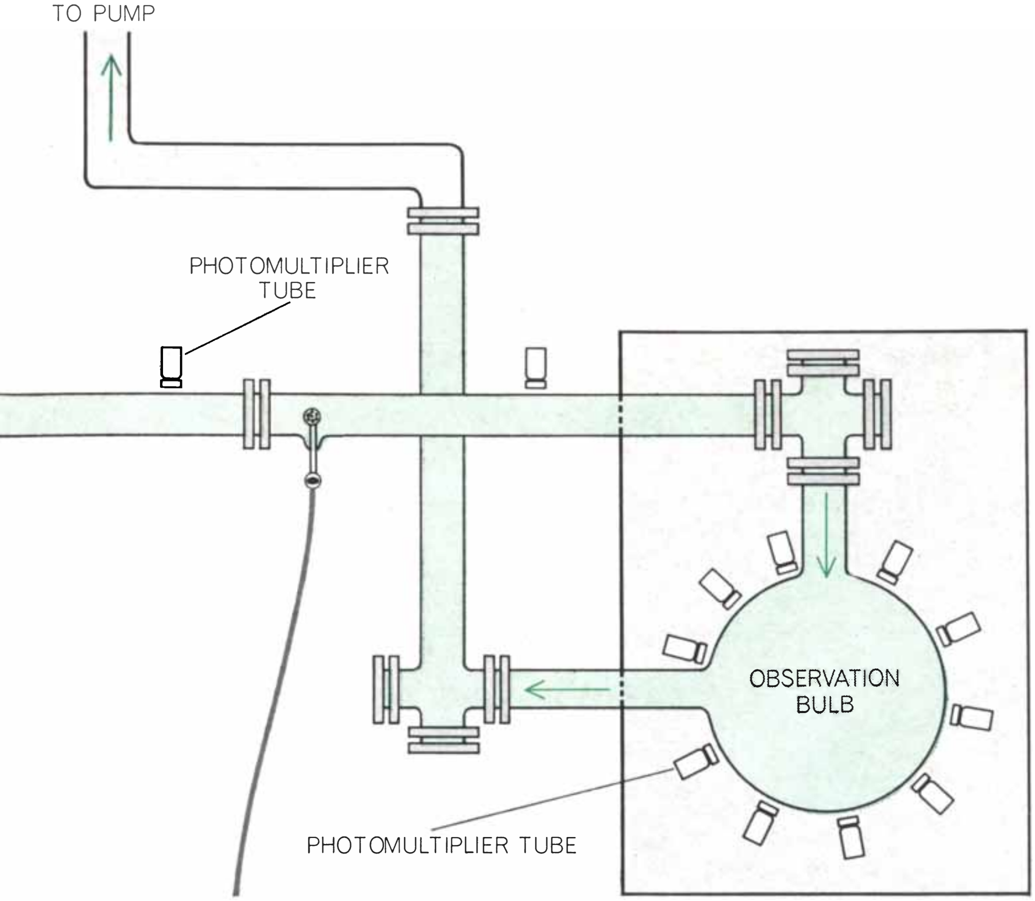
The best estimate of the location of the airglow in the atmosphere has been made with the aid of rocket-borne light detectors. According to these studies the green light from atomic oxygen and the blue light from molecular oxygen is produced in a rather narrow layer some six miles thick approximately 60 miles above the surface of the earth. Infrared radiation from hydroxyl radicals comes from a somewhat lower level, and red light from atomic oxygen comes from a much higher one [*see bottom illustration on page 107*]. It seems reasonable to expect some change in the condition of the atmosphere in the vicinity of these emission layers.

A suggestion as to what the change might be was made in 1931 by the British physicist Sydney Chapman, who was engaged at the time in analyzing the effect of the sun's radiation on the composition of the earth's atmosphere. Chapman's theory of how ozone (O_3) was formed in the atmosphere led him to surmise that some molecular oxygen would be dissociated into individual oxygen atoms at altitudes above 55 miles by the action of ultraviolet radiation from the sun. Molecular nitrogen (N_2) would be similarly affected only at much greater altitudes. Chapman further suggested that some of the oxygen atoms produced by ultraviolet radiation during the day would interact at night to produce the nightglow.

Since the nightglow persisted undiminished through the entire night, only a small fraction of the available oxygen atoms could be used up in this process, and a more or less constant



RADIATIVE MECHANISM responsible for the green spectral line at 5,577 angstroms was suggested by the British physicist Sydney Chapman in 1931. According to Chapman's hypothesis, some molecular oxygen (O_2) would be dissociated into individual oxygen atoms (O) at altitudes greater than 55 miles by the action of ultraviolet light from the sun. Some of the atoms produced in this way during the day would react in threes at night to produce molecular oxygen plus excited atomic oxygen (*color*). In about a second the excited oxygen atoms would emit their excess energy in the form of an electromagnetic radiation with a wavelength of 5,577 angstroms.



layer of atomic oxygen would be required. At the time Chapman formulated his hypothesis only the green line at 5,577 angstroms had been positively identified in the airglow spectrogram as originating from atomic oxygen; other lines and bands were observed, but their origin was unknown. Chapman proposed that the green emission from atomic oxygen was produced by the interaction of three oxygen atoms to produce an oxygen molecule plus an excited oxygen atom [*see bottom illustration on opposite page*].

Emission bands produced by excited oxygen molecules were later identified in the airglow spectrum. It was at first supposed that these bands were produced when pairs of oxygen atoms combined (with the aid of a stabilizing collision of each pair with a third atom or a molecule) to form excited oxygen molecules. Our laboratory experiments have confirmed this mechanism in the case of the ultraviolet bands produced by excited molecular oxygen, but it appears that some other mechanism involving ozone molecules is responsible for oxygen bands in the infrared. The intense infrared emission band produced by vibrationally excited hydroxyl radicals, first identified in the airglow spectrum in 1950, can also be achieved in the laboratory (and presumably in the airglow as well) by the reaction of a hydrogen atom (H) with an ozone molecule to form an excited hydroxyl radical plus an oxygen molecule. The process whereby atomic oxygen is excited to produce red emission lines at 6,300 and 6,364 angstroms is apparently quite complicated, and several mechanisms have been proposed.

Although in terms of the total light emitted by the airglow the green light of atomic oxygen at 5,577 angstroms is negligible, this single emission line in principle contains as much information about conditions in the airglow layer as any of the brighter emission lines or bands. Whether or not this potential information is received depends on the detectability of the emission. Two factors other than brightness determine this detectability: first, how distinct the emission is from other lines or bands, and second, how sensitive the light detector is. The green line of atomic oxygen at 5,577 angstroms is conspicuously isolated from other airglow emission lines, and modern photoelectric detectors are particularly efficient in the green region of the spectrum. Together these two factors more than compensate for the comparative weakness of the atomic-oxygen emission at this wavelength.

The same two factors also account for the fact that the green spectral line of excited atomic oxygen has been more fully studied than any other aspect of the airglow. The relative ease of observing this particular emission line of atomic oxygen in the airglow holds true for artificial airglows produced in the laboratory, and much of our present work is devoted to exploiting these advantages.

If the major fuel that is "burned" to power the airglow is atomic oxygen, as Chapman suggested, the largest quantum, or unit, of energy available for excitation by a single reaction is equal to the energy that would be given up if these atoms were to form molecules. The molecules most likely to be formed when atomic oxygen reacts with other constituents of the upper atmosphere are molecular oxygen and ozone. The energy given up by either of these reactions is less than five electron volts, which is quite small compared with the energy, say, of the charged particles that produce the aurora or excite radiation in laboratory discharges. This small quantum of energy available to excite the constituents of the upper atmosphere is another major factor in determining the unique spectral properties of the airglow.

There is still only indirect evidence of the existence of a layer of atomic oxygen in the upper atmosphere. Refined measurements of the intensity of the sun's ultraviolet radiation, the rate at which molecular oxygen can absorb this radiation, and the mean density and temperature of the upper atmosphere all strongly suggest, however, that an average of a trillion oxygen atoms per cubic centimeter are present in the airglow emission layer at an altitude of about 60 miles. Laboratory measurements indicate that comparatively few of these atoms are consumed during the night by chemical reactions. It follows that each day's accumulation of oxygen atoms must be removed by bulk transport to lower regions of the atmosphere. In sum, it appears very likely that the layer of atomic oxygen envisioned by Chapman does indeed exist.

In order to discuss in a quantitative way the chemiluminescent processes that may be responsible for the airglow it is necessary to know how the speed of a particular reaction is related to the concentrations of the reactants. The rate at which any two substances react to form other substances is proportional to their collision rate, and this rate in turn is proportional to the multiplication product of their concentrations. When more than two reactants must interact simultaneously, the rate of the reaction involves additional concentration factors. If an excited atom or molecule can lose its excess energy only by radiating a photon, or quantum of light, then the number of photons radiated per second must be equal to the number of atoms or molecules excited per second.

On this basis Chapman's mechanism for the production of the green spectral line of atomic oxygen implies that the intensity of the emission is proportional to the cube of the atomic-oxygen concentration. The "proportionality constant" in such a relation is called the rate coefficient; the speed at which the reaction takes place is found by multiplying the rate coefficient by the concentrations of the reactants. From estimates of the density of atomic oxygen in the airglow layer and from measurements of the rate of emission for the green line in airglow spectrograms, the rate coefficient can be computed. It is this rate coefficient, then, that must be measured in order to verify Chapman's excitation mechanism.

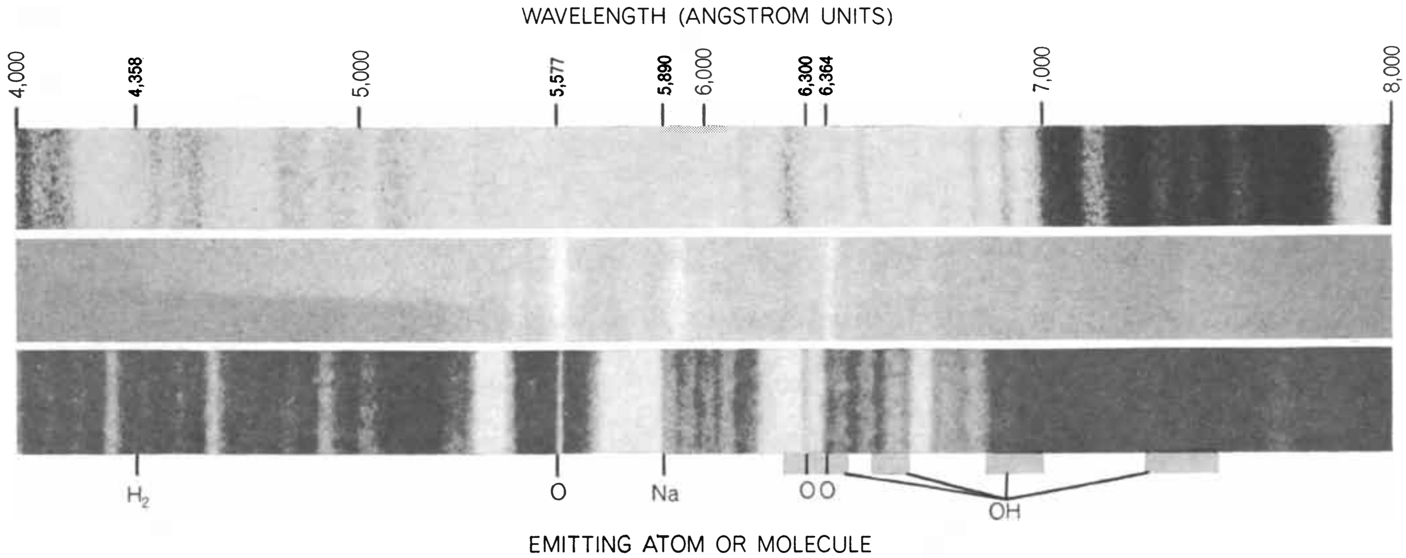
In attempting to validate Chapman's hypothesis experimentally, we have made use of a phenomenon first studied systematically by Lord Rayleigh in 1911. This phenomenon occurs after nitrogen at low pressure is subjected to an intense electrical discharge. When the discharge is stopped, the gas continues to glow—in some cases for hours. For the next 40 years many workers, including Rayleigh, attempted in vain to fully understand this "afterglow." Then in 1956 George B. Kistiakowsky and his colleagues at Harvard University applied the technique of mass spectroscopy to the nitrogen afterglow. They found that the major reactant responsible for the luminosity was atomic nitrogen. When nitric oxide (NO) was added to their gas mixture, one excited oxygen atom was produced for each nitrogen atom consumed.

Kistiakowsky's discovery made it possible to prepare measured amounts of atomic oxygen from atomic nitrogen and nitric oxide in an environment of molecular nitrogen to simulate very closely the actual condition of the earth's upper atmosphere. (Small amounts of molecular oxygen would improve the simulation.) Since molecular oxygen, in contrast to molecular nitrogen, rapidly de-excites atomic oxygen, some stratagem for producing excited oxygen atoms in the absence of oxygen molecules is essential to a laboratory study of the airglow emissions of atomic oxygen.

The first laboratory observation of the green spectral line of atomic oxygen in an oxygen-contaminated nitrogen afterglow was made in 1928 by Joseph Kaplan, who was then at Princeton University. In 1960 Charles A. Barth of the Jet Propulsion Laboratory of the California Institute of Technology and Kenneth C. Clark and I, then at the University of Washington, independently made the first measurements of the intensity of the green line in an afterglow produced by known amounts of oxygen atoms diluted with molecular nitrogen. At almost the same time William Schade, working with Kaplan at the University of California at Los Angeles, observed the green line in the presence of both atomic nitrogen and atomic oxygen.

Although it is fairly easy to duplicate the composition of the atmospheric airglow layer in the laboratory, the intensity of the green light produced is so faint that it is undetectable unless enormous volumes of gas are used. After all, even direct observations of the nightglow require extremely sensitive detectors. Instead the concentrations of the reactants believed necessary to produce selected airglow emissions were greatly increased in these experiments, thereby increasing the rate of excitation of the atoms and molecules responsible for some characteristic airglow light. Although this expedient facilitated the measurement of light and reactant concentrations—and hence the determination of the rate coefficient of the reaction producing the light—it was not without its drawbacks.

The most serious drawback was the concomitant increase in the collision rate, brought about partly by the higher concentrations of the reactants and partly by the requirement that diffusion to the boundaries of the experimental system be slow. As I have mentioned, the low collision rate of excited atoms and molecules in the upper atmosphere allows these systems to radiate light rather than exchanging their energy in collisions. It was not at all clear five years ago whether or not the same ab-



SPECTROGRAMS of the aurora (*top*), the airglow (*middle*) and an artificial airglow produced in the author's apparatus (*bottom*) are compared. The main difference between the aurora and the airglow spectrograms is the absence of molecular nitrogen (N_2) emission bands in the right half of the airglow spectrogram. The laboratory spectrogram represents an incomplete approach to simulated airglow, but it does contain the conspicuous green line of atomic oxygen (O) at 5,577 angstroms. It also contains molecular nitrogen bands, which are not present in the spectrogram of the complete simulation. Other atoms and molecules responsible for emission lines and bands in the airglow spectrogram are molecular hydrogen (H_2), sodium (Na) and the hydroxyl radical (OH).

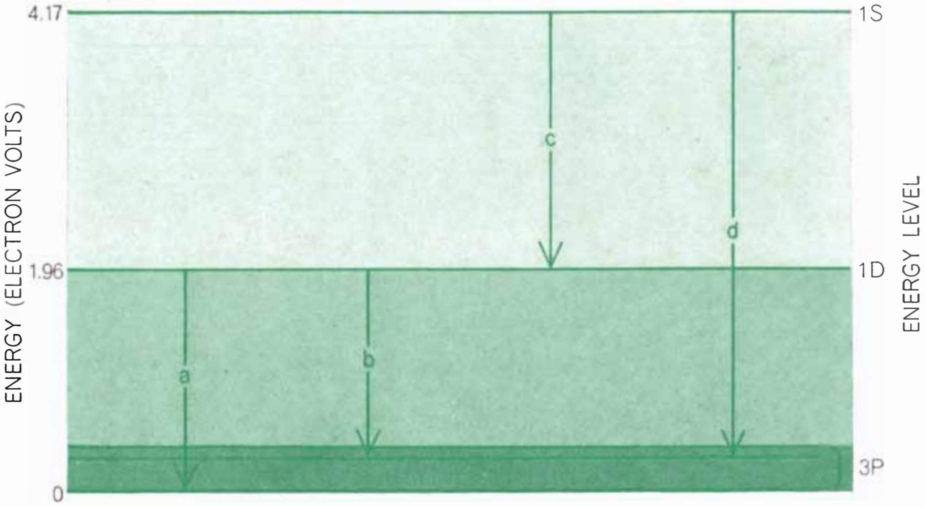
sence of quenching would obtain in a laboratory experiment, and a complete study of quenching under these circumstances had to be undertaken. This in itself was a difficult project, and it was combined with the necessity of making absolute measurements of both very faint light fluxes and highly reactive atomic fragments. It is not surprising that more than 35 years elapsed before Chapman's suggested airglow-excitation mechanism could be verified.

In 1963 Robert Sharpless and I conducted a series of experiments at the Stanford Research Institute that measured both the intensity of the green line at 5,577 angstroms and the concentration of atomic oxygen produced by the reaction of atomic nitrogen with nitric oxide in a low-pressure, flowing-gas system. We found that the light intensity was proportional to the square of the atomic-oxygen concentration. This finding is in conflict with the prediction that the light intensity would be proportional to the cube of the atomic-oxygen concentration, which was made on the assumptions that the Chapman reaction produced excited oxygen atoms and that all the atoms radiated.

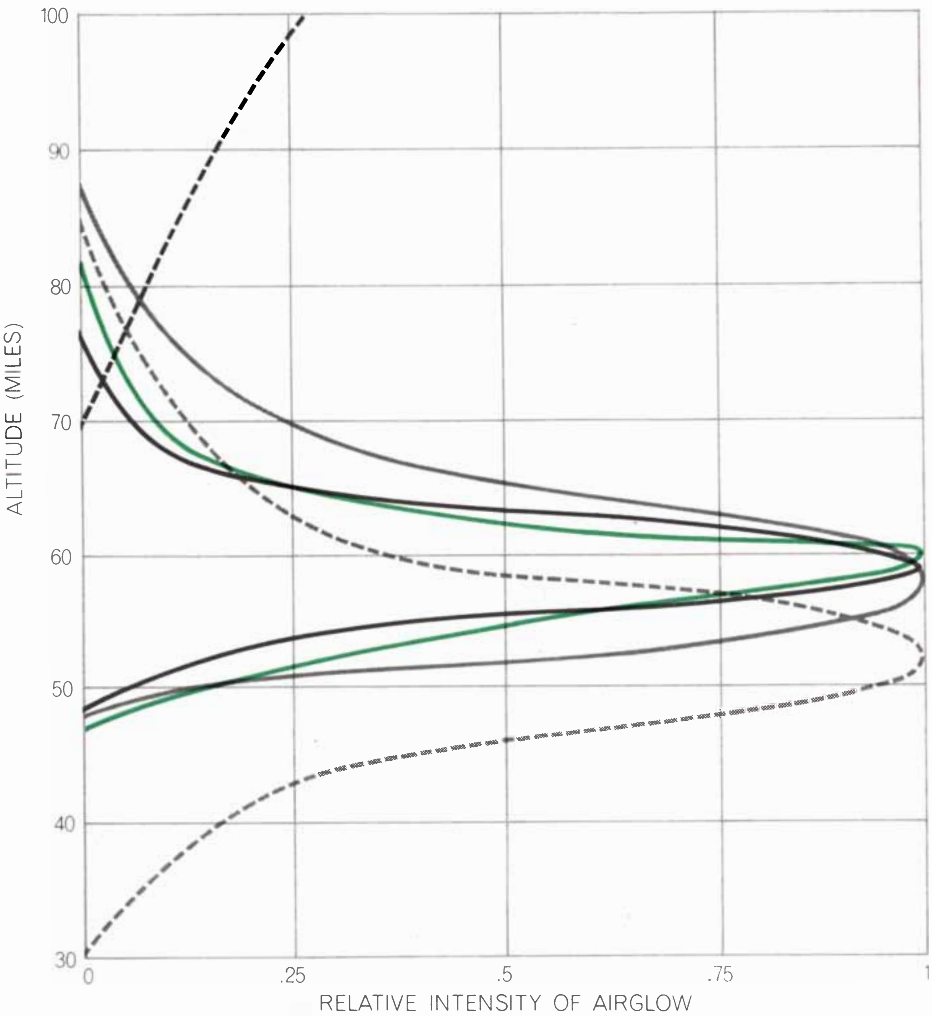
Clearly at least one of these assumptions is incorrect. This being the case, we must ask the question: What would be the relation between the light intensity and the concentration of atomic oxygen if the excited oxygen atoms that are responsible for the radiation we observe are indeed produced by the Chapman reaction but subsequently lose their energy by both emission and collision? It is necessary to bear in mind that the proportionality of light intensity to the concentration of excited atoms is fixed by the internal structure of the atoms and is not changed by other processes that may also de-excite these atoms.

If oxygen atoms are de-excited by radiation only, their rate of de-excitation equals the rate of emission, but if collisions also remove energy from excited atoms, their rate of de-excitation is larger than their rate of emission and consists of two components, one (radiation) independent of the environment and the other (collisions) linked to the density of the collision partners.

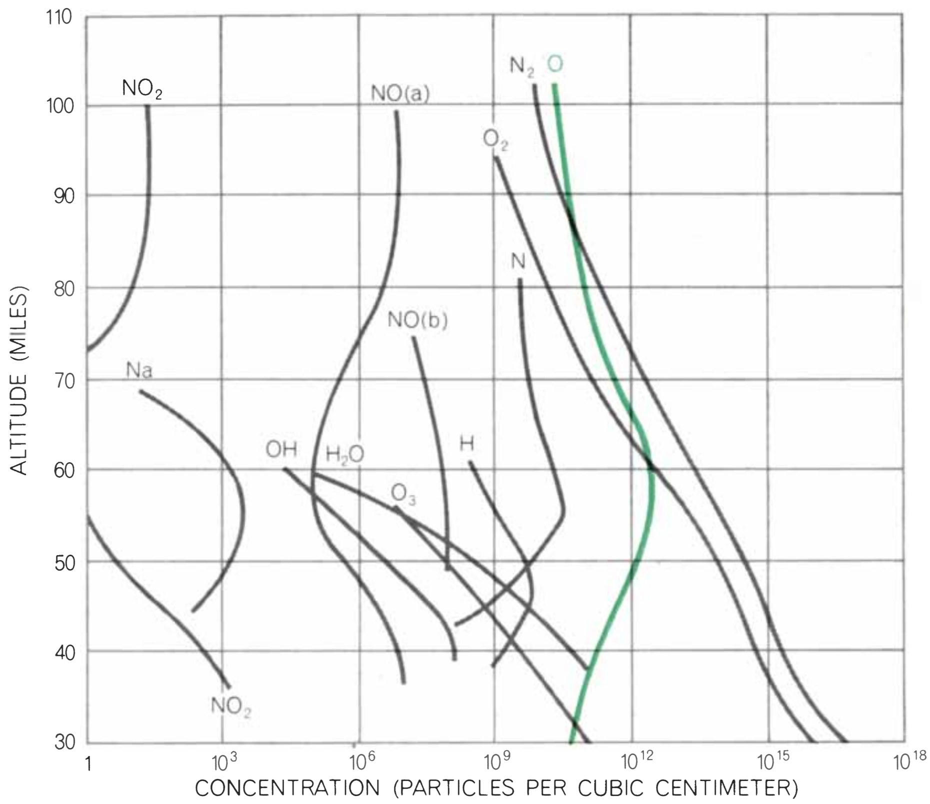
In a steady state (that is, one in which all the variables change slowly) the number of excited atoms produced per unit of time must equal the number lost per unit of time. The rate of loss is proportional to the number of atoms present only when one excited atom at a time is involved in the de-excitation



ELECTRON ENERGY LEVELS of atomic oxygen are shown in this diagram, together with four electron transitions between energy levels that account for characteristic emission lines in spectrograms of the airglow. Transitions *a* and *b* are responsible for red emission lines at 6,300 and 6,364 angstrom units respectively. Transition *c* produces the green spectral line at 5,577 angstroms, and transition *d* produces an ultraviolet line at 2,972 angstroms.



INTENSITY OF AIRGLOW produced by several different radiative mechanisms varies with altitude. Green light from atomic oxygen (*colored curve*) and violet and red light from molecular oxygen (*solid black curve and solid gray curve respectively*) is produced in a rather narrow layer some 60 miles above the surface of the earth. Infrared radiation from hydroxyl radicals (*broken gray curve*) comes from a somewhat lower level, and red light from atomic oxygen (*broken black curve*) comes from a much higher one. The data for the curves in this graph were obtained by D. M. Parker and his colleagues from rocket observations of the upper atmosphere conducted by the U.S. Naval Research Laboratory.



CONCENTRATIONS of various atoms and molecules in the upper atmosphere are represented here. The bulge in the colored curve for atomic oxygen (O) in the vicinity of the airglow layer was predicted in Chapman's hypothesis of the mechanism responsible for the green spectral line at 5,577 angstroms. The other curves represent nitrogen dioxide (NO_2), sodium (Na), two forms of nitric oxide (NO), the hydroxyl radical (OH), water (H_2O), ozone (O_3), hydrogen (H), atomic nitrogen (N), molecular oxygen (O_2) and molecular nitrogen (N_2). The graph was constructed from measurements and theoretical estimates by Charles A. Barth of the Jet Propulsion Laboratory of the California Institute of Technology.

process. (Because of the very small number of excited atoms compared with the total number of atoms—excited and nonexcited—available for collision, this is a good approximation.) In the Chapman excitation process the rate of production of excited atoms is proportional to the cube of the concentration of all oxygen atoms; it is also proportional to the rate of loss of excited atoms. The density of excited atoms divided by their radiative lifetime is equal to the light intensity. Consequently the rate of production of excited atoms divided by the proportionality constant that relates excited atoms to their rate of loss, and by the radiative lifetime of the excited state, will be proportional to the light intensity that results when the excited atoms radiate. If the rate of loss of excited oxygen atoms is dominated by collisions with nonexcited oxygen atoms, one of the oxygen-density factors in the production rate will be canceled by the oxygen-density factor in the loss rate, and the light intensity will be proportional to the square of the concentration of oxygen atoms.

Thus it appears that the proportionality constant that relates the light intensity to the square of the oxygen concentration is actually a ratio of constants. The numerator is the rate coefficient of the Chapman reaction (which produces excited atoms) and the denominator is the rate coefficient of another reaction (which produces nonexcited atoms) multiplied by the radiative lifetime of excited atomic oxygen. Since this lifetime has been calculated (it is approximately one second), it should be possible to obtain the rate coefficient of the Chapman reaction from our earlier measurements, if these are supplemented by a determination of the rate coefficient of collisional de-excitation of excited oxygen by nonexcited oxygen.

To make this measurement and to increase the precision of the earlier studies, a larger and much improved experimental system was built in 1964 at the Stanford Research Institute. In this apparatus Graham Black and I made extensive new observations of the quenching of excited atomic oxygen.

I shall briefly describe the operation of this new system and the findings it yielded. Since changes in the concentration of atomic oxygen affect both the production rate and the loss rate of excited oxygen atoms, it is difficult to separate these effects. If some gas could be found, however, that does not react with nonexcited atomic oxygen but does remove the excess energy of excited atomic oxygen, then this gas could be added to the mixture of excited and nonexcited oxygen until it removes excited oxygen atoms as quickly as they are removed by collisions with nonexcited atomic oxygen plus radiation. When this point is reached, the intensity of the emission from the excited oxygen atoms will be half what it was before the quenching gas was added.

We found that nitrous oxide (N_2O) and carbon dioxide (CO_2), which did not react with nonexcited oxygen atoms, were gases suitable for quenching the radiation from excited atomic oxygen. Since the rate at which these gases de-excite oxygen atoms is proportional to their concentration in the gas mixture, one would expect that the amount of quenching gas added to reduce the light intensity by half would be proportional to the density of nonexcited atomic oxygen. This was indeed true when the density of nonexcited atomic oxygen was large enough. The finding verified our analysis that attributed the major loss of excited oxygen atoms in the experimental apparatus to reactions with nonexcited oxygen atoms. In order to obtain the rate of the atomic-oxygen quenching reaction, however, the rate of the added-gas quenching reaction must be known.

If the concentration of atomic oxygen could be reduced sufficiently, radiation would be the predominant form of energy loss from excited oxygen atoms; if this radiation were detectable, the rate of excitation of oxygen atoms could be measured directly. As the concentration of atomic oxygen is decreased, however, the rate of excitation decreases much faster than the rate of quenching loss decreases, and as a result the light intensity is reduced to an undetectable level. If it were not for this complication, the rate of the Chapman reaction could be obtained directly.

These circumstances suggest an extrapolation procedure to obtain the rate coefficient for the quenching of excited oxygen atoms by the added gas. Once this quantity is found, the same procedure will also yield the rate coefficient for the de-excitation of oxygen atoms by nonexcited oxygen atoms. It is this latter parameter that is needed to convert our previously measured proportionality constant (relating light intensity to the square of the nonexcited-

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Ford Motor Company research in radiation cross-linking mechanisms leads to a new paint-curing process

Polymerization initiated by high-energy electrons is being explored by Ford Motor Company scientists. The kinetics of the copolymerization of unsaturated esters with styrene show that unusually rapid rate processes occur by mechanisms which do not follow classical concepts. These results reflect the unique mode of interaction between high-intensity, high-energy electrons and organic molecules.

Optimum reaction rates at a given radiation intensity are noted for solutions containing 65% ester (Figure 1). The overall rate depends both on the reactivity of the components and the steric constraints imposed on the system by the rigid network produced. Since the reaction occurs at room temperature, below the glass transition point of the network, the growing chains are not sufficiently mobile to accommodate the configuration predicted by the established copolymerization theory. The structure of the product depends instead on the concentration of double bonds at the instant of radiation.

As the beam intensity becomes greater (Figure 2), the rates increase linearly; network formation occurs within small, isolated volume elements swept out by the incident electrons. At still higher intensities, the volume elements overlap, so that the efficiency of the reaction now is reduced. A consequence of such fundamental studies was the development of a major innovation in paint-curing technology. Chemical structures exhibiting maximum sensitivity to radiation, and with rheological and weathering properties required for optimum performance, were designed and synthesized. The result is a coating that cures in seconds. And at room temperature.

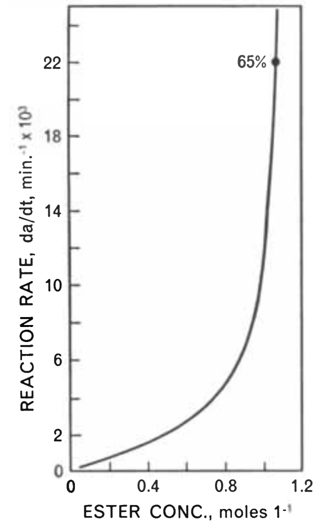

Figure 1. This graph shows the rate of polymerization as a function of ester concentration; commercially useful systems are in the 65% ester range [Burlant and Hinsch, J. Polymer Science, 2, 2135 (1964)].

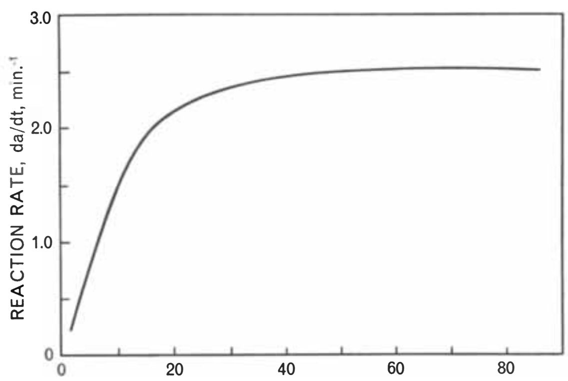

Figure 2. This graph shows the rate of polymerization as a function of radiation intensity for 65% ester solutions; optimum rate occurs with an electron beam intensity of 40 MRAD-min-1 [Burlant and Hinsch, J. Polymer Science, 3, 3587 (1965)].

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atomic-oxygen concentration) to the rate coefficient of the Chapman reaction. If the amount of quenching gas needed to reduce the light intensity by half is plotted on a graph against the density of atomic oxygen, a straight line results, since the densities are proportional to each other [*see illustration below*]. Because the excited oxygen atoms can radiate, however, this line does not pass through the point of origin but rather crosses the added-quenching-gas axis at a finite value. The amount of the quenching gas indicated by this crossing is then the amount needed to de-excite oxygen atoms at the same rate as that at which they radiate. Thus the rate coefficient of the added-gas quenching reaction is equal to the rate of light emission per excited atom divided by the density of the quenching gas. The slope of the line is the ratio of the added-gas quenching rate coefficient to the nonexcited atomic-oxygen quenching rate coefficient. By dividing the added-gas quenching rate coefficient by the slope of the line, the rate coefficient of quenching by nonexcited atomic oxygen can be obtained. Finally, the measured proportionality constant that relates light intensity to the square of the oxygen density is multiplied by the radiative lifetime of the excited oxygen atoms and by the rate coefficient of quenching for nonexcited atomic oxygen to yield the rate coefficient of the Chapman reaction.

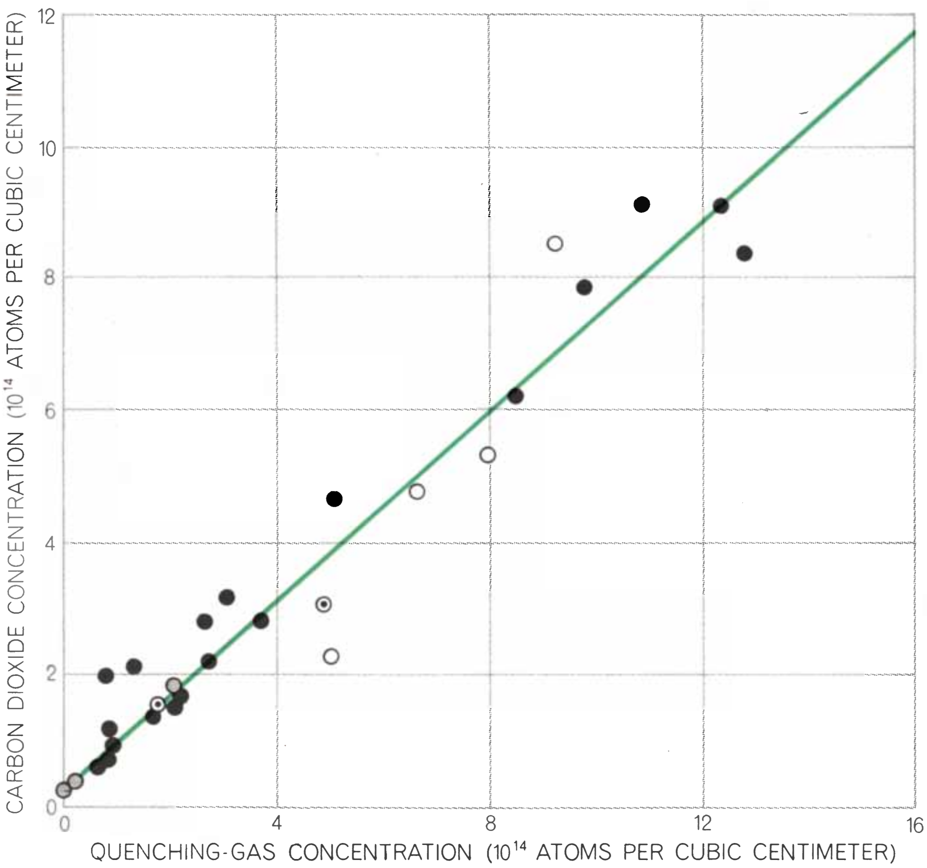
Amazingly, everything happened in our apparatus just as we had anticipated. Several gases that react slowly with oxygen and nitrogen—for example nitrous oxide, carbon dioxide and molecular oxygen—were used to quench the radiation from the excited oxygen atoms. All these quenching gases indirectly gave the same value for the quenching rate coefficient for nonexcited oxygen atoms. When combined with either our original measurements relating light intensity to the square of the atomic-oxygen density or the measurements made in our new system, the rate coefficient of the Chapman reaction turned out to be the same as that deduced from the direct measurements of the nightglow. Chapman's hypothesis was finally confirmed: three nonexcited oxygen atoms can combine in the upper atmosphere to form an oxygen molecule and an excited oxygen atom, which can then emit light at 5,577 angstroms.

The airglow still has some secrets. In the very near future, however, the other chemical processes that power the airglow will undoubtedly be identified and their rate coefficients measured. The phenomenon of the airglow has become a tool to be exploited in other investigations by physicists, chemists and meteorologists.

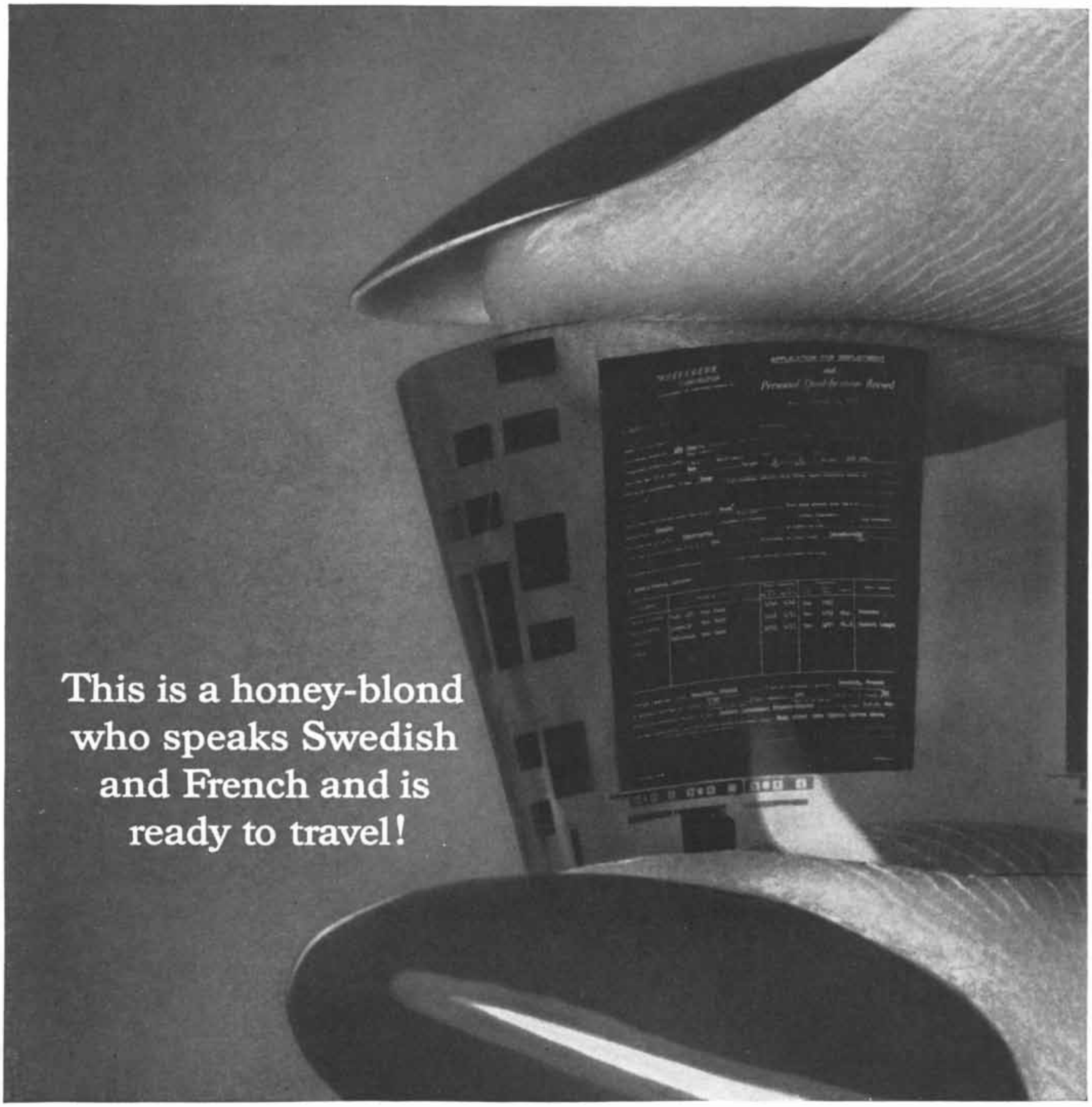
For example, the mass transport of air to and from the airglow region of the atmosphere may be part of an important global circulation system. By using atomic oxygen as a tracer of air motion this circulation system could be studied. In the polar regions the sun is below the horizon for several months and therefore cannot replenish excited atomic oxygen lost by radiation and by massive air movements. A study of the concentration of excited oxygen atoms under these conditions may lead to a determination of the speed of the air mass of which they are a part. Fortunately new observational techniques now make it possible to observe the polar airglow in spite of the interference of auroral light.

The ultraviolet radiation from the sun that powers the airglow does not reach the earth's surface. Hence the airglow is a potentially useful indicator of changes in a component of sunlight that are not measurable directly by conventional ground-based techniques. A reanalysis of airglow data for the past 50 years may reveal significant fluctuations in the amount of ultraviolet radiation from the sun. Because of the slow response of the airglow to solar excitation, however, it is not certain that brief solar disturbances can be monitored in this way.

It is difficult to predict how our new knowledge of the airglow will be used in the future. But since the airglow mirrors the chemical state of the upper atmosphere it cannot fail to attract increasing attention as long as man insists on traversing this boundary between the earth and the rest of the universe.



EXTRAPOLATION PROCEDURE is employed to find the amount of quenching gas needed to de-excite oxygen atoms at the same rate as that at which they radiate. If the amount of quenching gas needed to reduce the light intensity by half is plotted against the density of atomic oxygen, a straight line results, since their densities are proportional to each other. Because the excited oxygen atoms can radiate, however, this line does not pass through the point of origin but rather cuts the added-quenching-gas axis at a finite value. The amount of quenching gas indicated by this intercept is then the amount needed to de-excite oxygen atoms at the same rate as that at which they radiate. From the slope of the line the rate coefficient for de-excitation of atomic oxygen by nonexcited atomic oxygen can be obtained. The four different kinds of dots indicate readings for different mixtures of nitrogen and other gases in which the excited and nonexcited oxygen atoms were embedded.



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MATHEMATICAL GAMES

The hierarchy of infinities and the problems it spawns

by Martin Gardner

A graduate student at Trinity
Computed the square of infinity.
But it gave him the fidgets
To put down the digits,
So he dropped math and took up divinity.
—ANONYMOUS

In 1963 Paul J. Cohen, a 29-year-old mathematician at Stanford University, found a surprising answer to one of the great problems of modern set theory: Is there an order of infinity higher than the number of integers but lower than the number of points on a line? To make clear exactly what Cohen proved, something must first be said about those two lowest known levels of infinity.

It was Georg Ferdinand Ludwig Philipp Cantor who first discovered that beyond the infinity of the integers—an infinity to which he gave the name aleph-null—there are not only higher infinities but also an infinite number of them. Leading mathematicians were sharply divided in their reactions. Henri Poincaré called Cantorism a disease from which mathematics would have to recover, and Hermann Weyl spoke of Cantor's hierarchy of alephs as "fog on fog."

On the other hand, David Hilbert said, "From the paradise created for us by Cantor, no one will drive us out," and Bertrand Russell once praised Cantor's achievement as "probably the greatest of which the age can boast." Today only mathematicians of the intuitionist school and a few philosophers are still uneasy about the alephs. Most mathematicians long ago lost their fear of them, and the proofs by which Cantor established his "terrible dynasties" (as they have been called by the Argentine writer Jorge Luis Borges) are now universally honored as being among the most brilliant and beautiful in the history of mathematics.

Any infinite set of things that can be counted 1, 2, 3 . . . has the cardinal number $\aleph_0$ (aleph-null), the bottom rung of Cantor's aleph ladder. Of course, it is not possible actually to count such a set; one merely shows how it can be put into one-to-one correspondence with the counting numbers. Consider, for example, the infinite set of primes. It is easily put in one-to-one correspondence with the positive integers:

$$\begin{array}{cccccc} 1 & 2 & 3 & 4 & 5 & 6\ldots \\ \downarrow & \downarrow & \downarrow & \downarrow & \downarrow & \downarrow \\ 2 & 3 & 5 & 7 & 11 & 13\ldots \end{array}$$

The set of primes is therefore an aleph-null set. It is said to be "countable" or "denumerable." Here we encounter a basic paradox of all infinite sets. Unlike finite sets, they can be put in one-to-one correspondence with a *part* of themselves or, more technically, with one of their "proper subsets." Although the primes are only a small portion of the positive integers, as a completed set they have the same aleph number. Similarly, the integers are only a small portion of the rational numbers (the integers plus all integral fractions), but the rationals form an aleph-null set too.

There are all kinds of ways in which this can be proved by arranging the rationals in a countable order. The most familiar way is to attach them, as fractions, to an infinite square array of lattice points and then count the points by following a zigzag path, or a spiral path if the lattice includes the negative rationals. Here is another, intriguing method of ordering and counting the positive rationals that was proposed by the American logician Charles Sanders Peirce.

Start with the fractions 0/1 and 1/0. (The second fraction is meaningless, but that can be ignored.) Sum the two numerators and then the two denominators to get the new fraction 1/1, and place it between the previous pair: 0/1, 1/1, 1/0. Repeat this procedure with each pair of adjacent fractions to obtain two new fractions that go between them:

$$\frac{0}{1}\ \frac{1}{2}\ \frac{1}{1}\ \frac{2}{1}\ \frac{1}{0}.$$

The five fractions grow, by the same procedure, to nine:

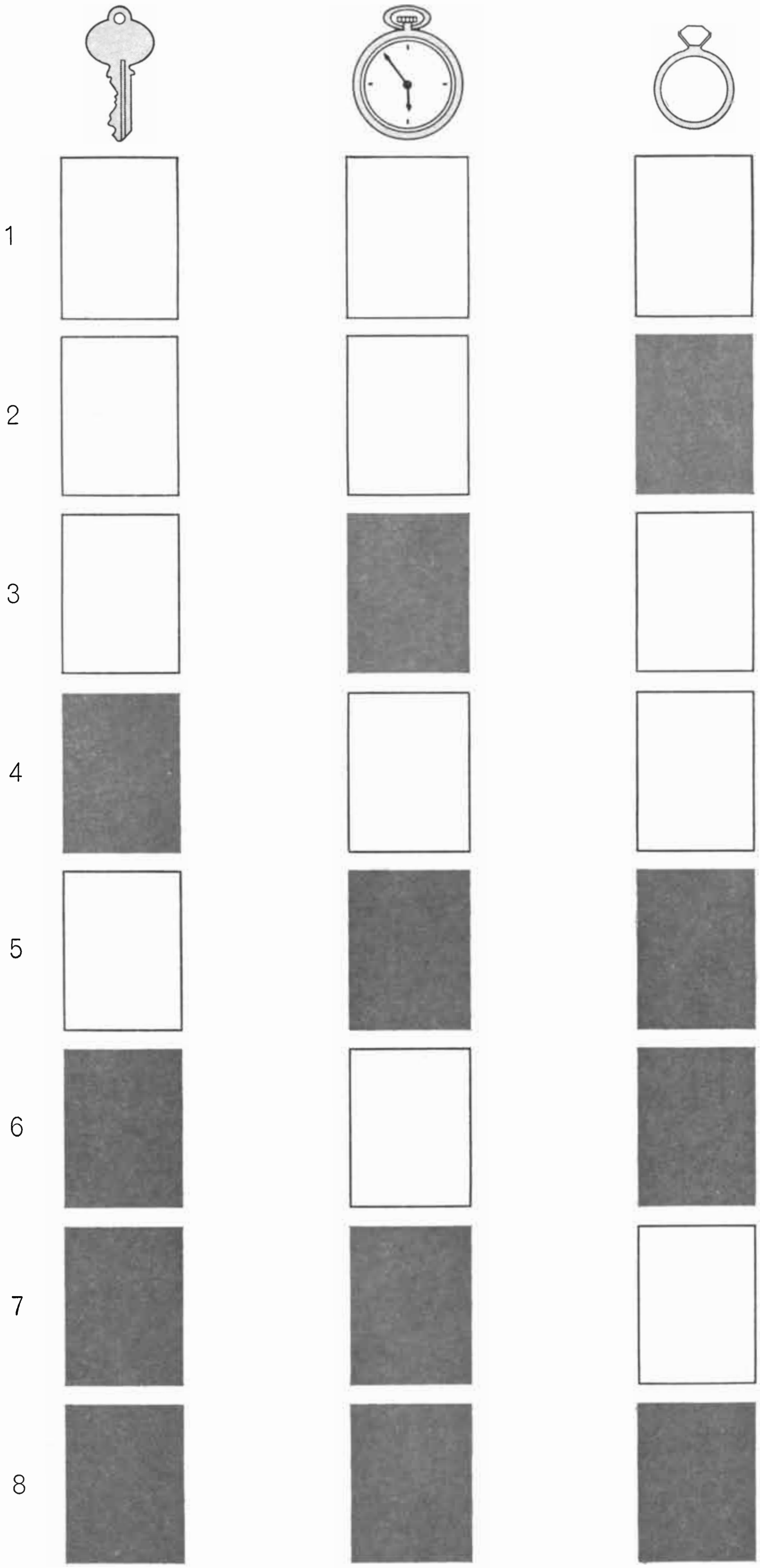
$$\frac{0}{1}\ \frac{1}{3}\ \frac{1}{2}\ \frac{2}{3}\ \frac{1}{1}\ \frac{3}{2}\ \frac{2}{1}\ \frac{3}{1}\ \frac{1}{0}.$$

In this continued series every rational number will appear once and only once, and always in its simplest fractional form. There is no need, as there is in other methods of ordering the rationals, to eliminate fractions, such as 10/20, that are equivalent to simpler fractions also on the list, because no reducible fraction ever appears. If at each step you fill the cracks, so to speak, from left to right, you can count the fractions simply by taking them in their order of appearance. This series, as Peirce said, has many curious properties. At each new step the digits above the lines, taken from left to right, begin by repeating the top digits of the previous step: 01, 011, 0112 and so on. And at each step the digits below the lines are the same as those above the lines but in reverse order. The series is closely related to what are called Farey numbers (after the English geologist John Farey, who first analyzed them), about which there is now a considerable literature.

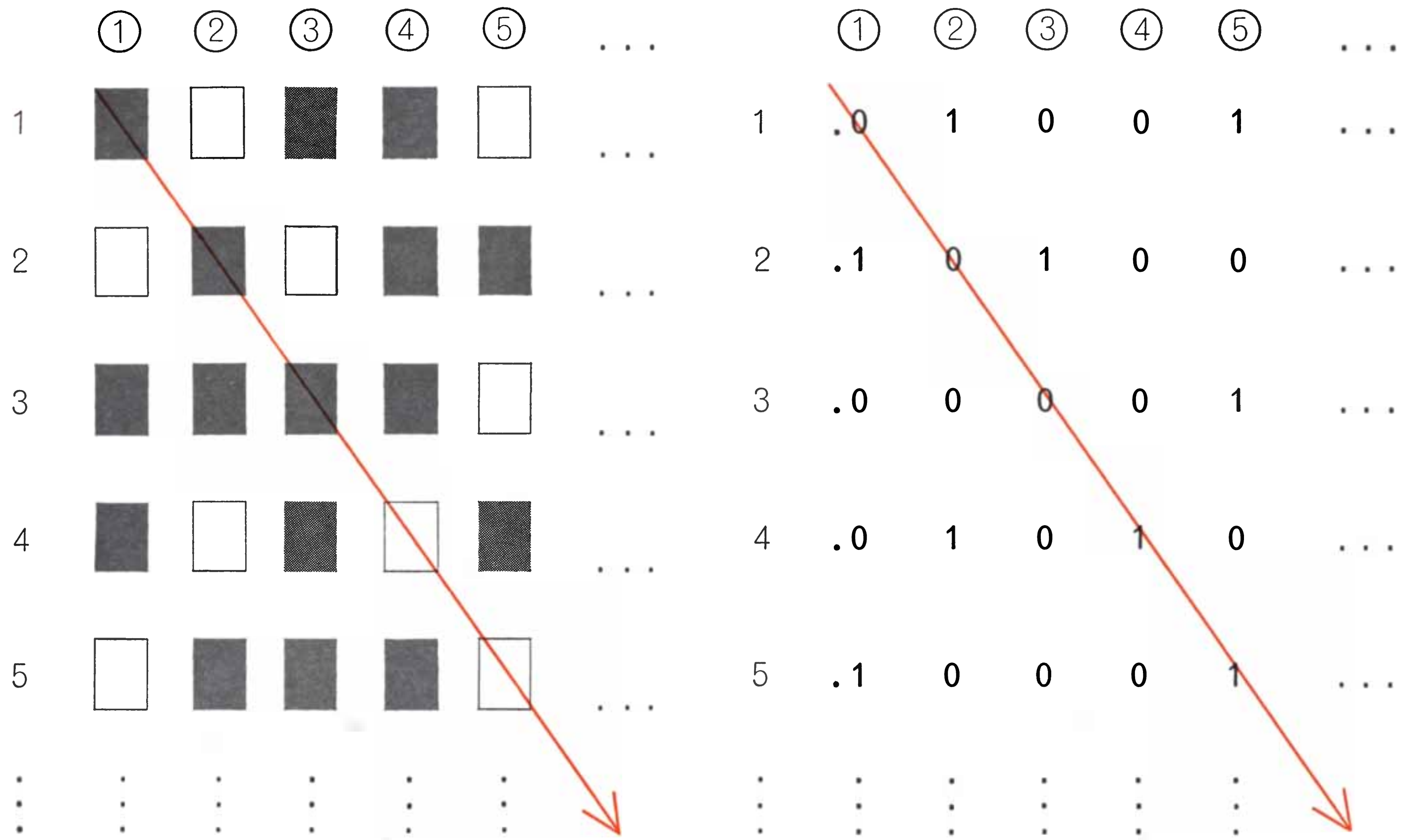
It is easy to show that there is a set with a higher infinite number of elements than aleph-null. To explain one of the best of such proofs a deck of cards is useful. First consider a finite set of three objects, say a key, a watch and a ring. Each subset of this set is symbolized by a row of three cards [*see illustration on opposite page*]; a face-up card [*white*] indicates that the object above it is in the subset, a face-down card [*gray*] indicates that it is not. The first subset consists of the original set itself. The next three rows indicate subsets that contain only two of the objects. They are followed by the three subsets of single objects and finally by the empty (or null) subset that contains none of the objects. For any set of n elements the number of subsets is 2^n. Note that this formula applies even to the empty set, since $2^0 = 1$ and the empty set has the empty set as its sole subset.

This procedure is applied to an infinite but countable (aleph-null) set of elements at the left in the illustration on the next page. Can the subsets of this infinite set be put into one-to-one correspondence with the counting integers? Assume that they can. Symbolize each subset with a row of cards, as before, only now each row continues endlessly to the right. Imagine these infinite rows listed in any order whatever and numbered 1, 2, 3... from the top down. If we continue forming such rows, will the list eventually catch all the subsets? No—because there is an infinite number of ways to produce a subset that cannot be on the list. The simplest way is to consider the diagonal set of cards indicated by the arrow and then suppose every card along this diagonal is turned over (that is, every face-down card is turned up, every face-up card is turned down). The new diagonal set cannot be the first subset because its first card differs from the first card of subset 1. It cannot be the second subset because its second card differs from the second card of subset 2. In general it cannot be the nth subset because its nth card differs from the nth card of subset n. Since we have produced a subset that cannot be on the list, even when the list is infinite, we are forced to conclude that the original assumption is false. The set of all subsets of an aleph-null set is a set with the cardinal number 2 raised to the power of aleph-null. This proof shows that such a set cannot be matched one to one with the counting integers. It is a higher aleph, an "uncountable" infinity.

Cantor's famous diagonal proof, in the form just given, conceals a startling bonus. It proves that the set of real numbers (the rationals plus the irrationals) is also uncountable. Consider a line segment, its ends numbered 0 and 1. Every rational fraction from 0 to 1 corresponds to a point on this line. Between any two rational points there is an infinity of other rational points; nevertheless, even after all rational points are identified, there remains an infinity of unidentified points—points that correspond to the unrepeating decimal fractions attached to such algebraic irrationals as the square root of 2, and to such transcendental irrationals as pi and e. Every point on the line segment, rational or irrational, can be represented by an endless decimal fraction. But these fractions need not be decimal; they can also be written in binary notation. Thus every point on the line segment can be represented by an endless pattern of 1's and 0's, and every possible endless pattern of 1's and



Subsets of a set of three elements



A countable infinity has an uncountable infinity of subsets (left) *that correspond to the real numbers* (right)

0's corresponds to exactly one point on the line segment.

Now, suppose each face-up card at the left in the illustration above is replaced by 1 and each face-down card by 0, as shown at the right in the illustration. We have only to put a binary point in front of each row and we have an infinite list of different binary fractions between 0 and 1. But the diagonal set of symbols, after each 1 is changed to 0 and each 0 to 1, is a binary fraction that cannot be on the list. From this we see that there is a one-to-one correspondence of three sets: the subsets of aleph-null, the real numbers (here represented by binary fractions) and the totality of points on a line segment. Cantor gave this higher infinity the cardinal number C, for the "power of the continuum." He believed it was also $\aleph_1$ (aleph-one), the first infinity greater than aleph-null.

By a variety of simple, elegant proofs Cantor showed that C was the number of such infinite sets as the transcendental irrationals (the algebraic irrationals, he proved, form a countable set), the number of points on a line of infinite length, the number of points on any plane figure or on the infinite plane, and the number of points in any solid figure or in all of three-space. Going into higher dimensions does not increase the number of points. The points on a line segment one inch long can be matched one to one with the points in any higher-dimensional solid, or with the points in the entire space of any higher dimension.

The distinction between aleph-null and aleph-one (we accept, for the moment, Cantor's identification of aleph-one with C) is important in geometry whenever infinite sets of figures are encountered. Imagine an infinite plane tessellated with hexagons. Is the total number of vertices aleph-one or aleph-null? The answer is aleph-null; they are easily counted along a spiral path [*see top illustration on page 116*]. On the other hand, the number of different circles of one-inch radius that can be placed on a sheet of typewriter paper is aleph-one because inside any small square near the center of the sheet there are aleph-one points, each the center of a different circle with a one-inch radius.

Consider in turn each of the five symbols J. B. Rhine uses in his "ESP" test cards [*see bottom illustration on page 116*]. Can it be drawn an aleph-one number of times on a sheet of paper, assuming that the symbol is drawn with ideal lines of no thickness and that there is no overlap or intersection of any lines? (The drawn symbols need not be the same size, but all must be similar in shape.) It turns out that all except one can be drawn an aleph-one number of times. Can the reader show, before the answer is given next month, which symbol is the exception?

The two alephs are also involved in recent cosmological speculation. Richard Schlegel, a physicist at Michigan State University, has called attention in several papers to a strange contradiction inherent in the "steady state" theory. According to that theory, the number of atoms in the cosmos at the present time is aleph-null. (The cosmos is regarded as infinite even though an "optical horizon" puts a limit on what can be seen.) Moreover, atoms are steadily increasing in number as the universe expands. Infinite space can easily accommodate any finite number of doublings of the quantity of atoms, for whenever aleph-null is multiplied by two, the result is aleph-null again. (If you have an aleph-null number of eggs in aleph-null boxes, one egg per box, you can accommodate another aleph-null set of eggs simply by shifting the egg in box 1 to box 2, the egg in box 2 to box 4, and so on, each egg going to a box whose number is twice the number of the egg's previous box. This empties all the odd-numbered boxes, which can then be filled with another aleph-null set of eggs.) But if the

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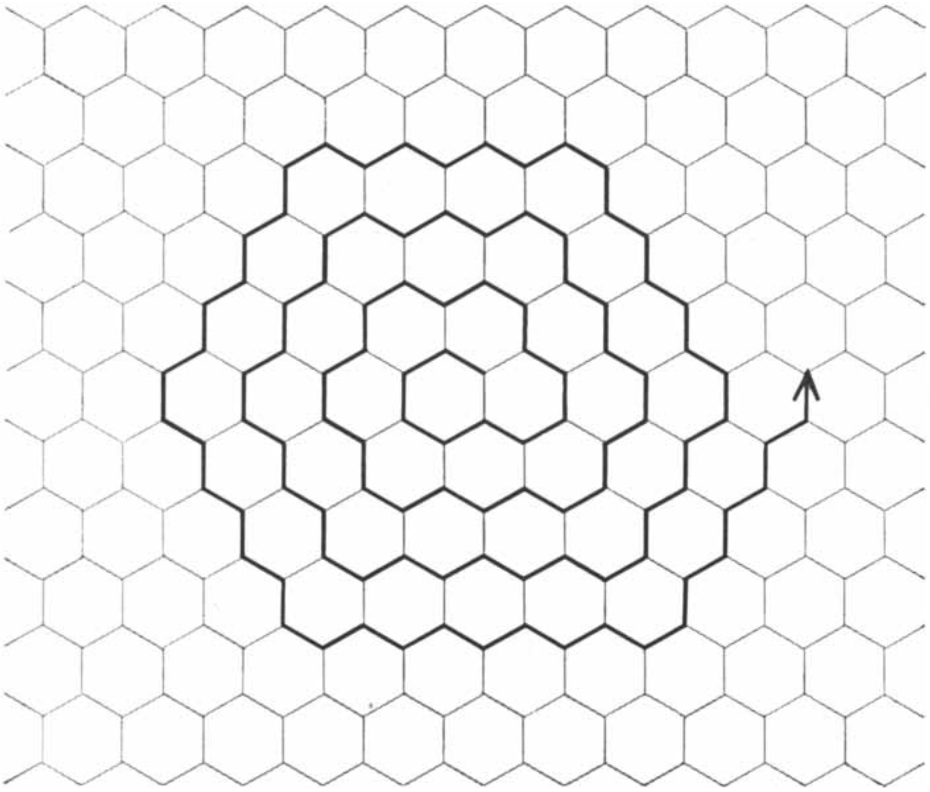

Spiral counts the vertices of a hexagonal tessellation

doubling goes on for an aleph-null number of times, we come up against the formula of 2 raised to the power of aleph-null—that is, $2 \times 2 \times 2 \ldots$ repeated aleph-null times. As we have seen, this produces an aleph-one set. Consider only two atoms at an infinitely remote time in the past. By now, after an aleph-null series of doublings, they would have grown to an aleph-one set. But the cosmos, at the moment, cannot contain an aleph-one set of atoms. Any collection of distinct physical entities (as opposed to the ideal entities of mathematics) is countable and therefore, *at the most,* aleph-null.

In his latest paper, "The Problem of Infinite Matter in Steady-State Cosmology" (*Philosophy of Science,* Vol. 32, January, 1965, pages 21–31), Schlegel finds a clever way out. Instead of regarding the past as a completed aleph-null set of finite time intervals (to be sure, ideal instants in time form an aleph-one continuum, but Schlegel is concerned with those finite time intervals during which doublings of atoms occur), we can view both the past and the future as infinite in the inferior sense of "becoming" rather than completed. Whatever date is suggested for the origin of the universe (remember, we are dealing with the steady-state model, not with a "big bang" or oscillating theory), we can always set an earlier date. In a sense there is a "beginning," but we can push it as far back as we please. There is also an "end," but we can push it as far forward as we please. As we go back in time, continually halving the number of atoms, we never halve them more than a finite number of times, with the result that their number never shrinks to less than aleph-null. As we go forward in time, doubling the number of atoms, we never double more than a finite number of times; therefore the set of atoms never grows larger than aleph-null. In either direction the leap is never made to a completed aleph-null set of time intervals. As a result the set of atoms never leaps to aleph-one and the disturbing contradiction does not arise.

Cantor was convinced that his endless hierarchy of alephs, each obtained by raising 2 to the power of the preceding aleph, represented all the alephs there are. There are none in between. Nor is there an Ultimate Aleph, such as certain Hegelian philosophers of the time identified with the Absolute. The endless hierarchy of infinities itself, Cantor argued, is a better symbol of the Absolute.

All his life Cantor tried to prove that there is no aleph between aleph-null and C, the power of the continuum, but he never found a proof. In 1938 Kurt Gödel showed that Cantor's conjecture, which became known as the "continuum hypothesis," could be assumed to be true, and that this could not conflict with the axioms of set theory.

What Cohen proved in 1963 was that the opposite could also be assumed. One can posit that C is *not* aleph-one; that there is at least one aleph between aleph-null and C, even though no one has the slightest notion of how to specify a set (for example a certain subset of the transcendental numbers) that would have such a cardinal number. This too is consistent with set theory. Cantor's hypothesis is undecidable. Like the parallel postulate of Euclidean geometry, it is an independent axiom that can be affirmed or denied. Just as the two assumptions about Euclid's parallel axiom divided geometry into Euclidean and non-Euclidean, so the two assumptions about Cantor's hypothesis now divide the theory of infinite sets into Cantorian and non-Cantorian. Set theory has been struck a gigantic blow with a cleaver, and exactly what will come of it no one can say.

Five "ESP" symbols

The answers to last month's penny problems follow:

1. To double the eight pennies in a row into four stacks of two coins each, number them from one to eight and move 4 to 7, 6 to 2, 1 to 3, 5 to 8. For 10 pennies, simply double the pennies at one end (for example, move 7 to 10), to leave a row of eight that can be solved as before. Clearly, a row of $2n$ pennies can be solved in n moves by


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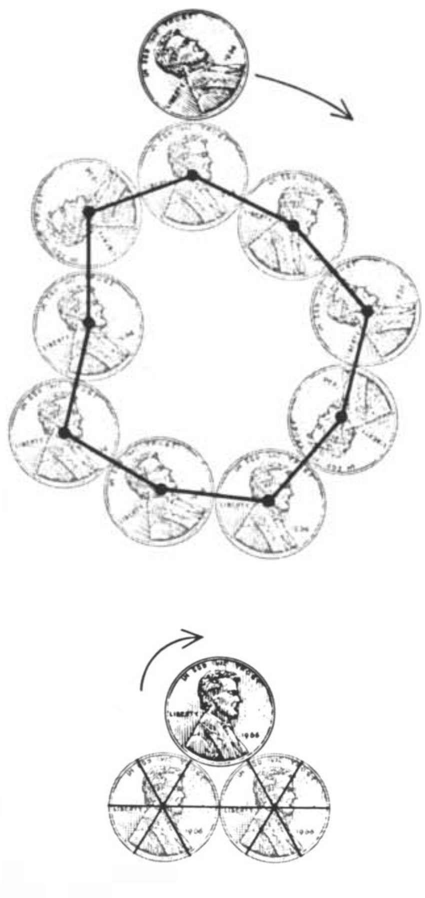
Rhombus-to-circle puzzle

Inverting the triangle



The rolling penny

doubling the coins at one end until eight remain, then solving for the eight.

2. Six pennies in rhombus formation [*top illustration at left*] can be formed into a circle, in accordance with the rules given, by numbering them as shown and moving 6 to touch 4 and 5, 5 to touch 2 and 3 from below, 3 to touch 5 and 6.

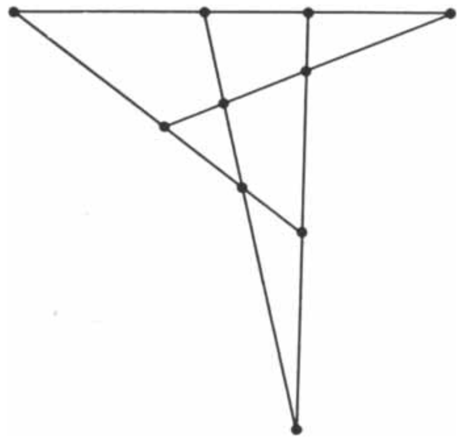
3. A triangle of 10 pennies is inverted by moving three coins as shown in the middle illustration at the left. In working on the general problem, for equilateral formations of any size, readers may have realized that the problem is one of drawing a bounding triangle (like the frame used to group the 15 balls for a game of billiards), inverting it and placing it over the figure so that it encloses a maximum number of coins. In every case the smallest number of coins that must be moved to invert the pattern is obtained by dividing the number of coins by 3 and ignoring the remainder.

4. The 10-coin triangle, with positions numbered as shown last month, is reduced to one coin in five moves by first removing the penny at spot 3, then jumping: 10–3, 1–6, 8–10–3, 4–6–1–4, 7–2. I shall report later on letters received about the 15-coin triangle.

5. Readers were asked to prove that if a penny is rolled once around a closed chain of pennies, touching every penny, the number of rotations made by the penny is a constant regardless of the chain's shape. We shall prove this first for a chain of nine pennies.

Join the centers of the coins by straight lines as shown at the top of the bottom illustration at the left to form a nine-sided polygon. The total length (in degrees of arc) of the perimeters of the pennies, outside the polygon, is the same as the total of the polygon's conjugate angles. (The conjugate of an angle is the difference between that angle and 360 degrees.) The sum of the conjugate angles of a polygon of n sides is always $\frac{1}{2}n + 1$ perigons (a perigon is an angle of 360 degrees).

As the penny rolls around the chain, however, for every pair of pennies it touches it fails to touch two arcs of 1/6 perigon each, which together are 1/3 perigon [*see bottom of illustration*]. For n pennies, it will fail to touch $n/3$ perigons. We subtract this from $\frac{1}{2}n + 1$ to obtain $\frac{1}{6}n + 1$ as the total perimeter over which the penny rolls in making one circuit around the chain. As explained last month, the penny rotates two degrees for each degree of arc over which it rolls, therefore the penny must make a total of $\frac{1}{3}n + 2$ rotations. This



Straight-line configuration

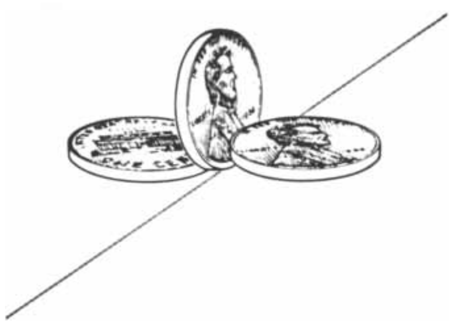
obviously is a constant, regardless of the number of pennies or the chain's shape, because the centers of any chain of pennies must mark the vertices of an n-sided polygon. (The formula also applies to a degenerate chain of two pennies, whose centers can be taken as the corners of a degenerate polygon of two sides.)

A similar argument establishes $\frac{1}{3}n - 2$ as the number of rotations made by a penny rolling once around the inside of a closed chain of six or more pennies and touching every penny. The formula gives zero rotations for the chain of six, inside which it fits snugly, touching all six coins. For an open chain of n pennies it is easy to show that the rolling penny makes $\frac{1}{3}(2n + 4)$ rotations in a complete circuit.

6. The sixth configuration of 10 pennies having five straight lines of four coins each is shown above.

7. To put penny C between two touching pennies A and B without touching A or moving B, place a fingertip firmly on B and then slide C against B. Be sure, however, to let go of C before it strikes B. The impact will propel A away from B, so that C can be placed between the two previously touching coins.

8. Three pennies can be placed with two heads on one side of a line and two tails on the other as shown below.



How to place the pennies

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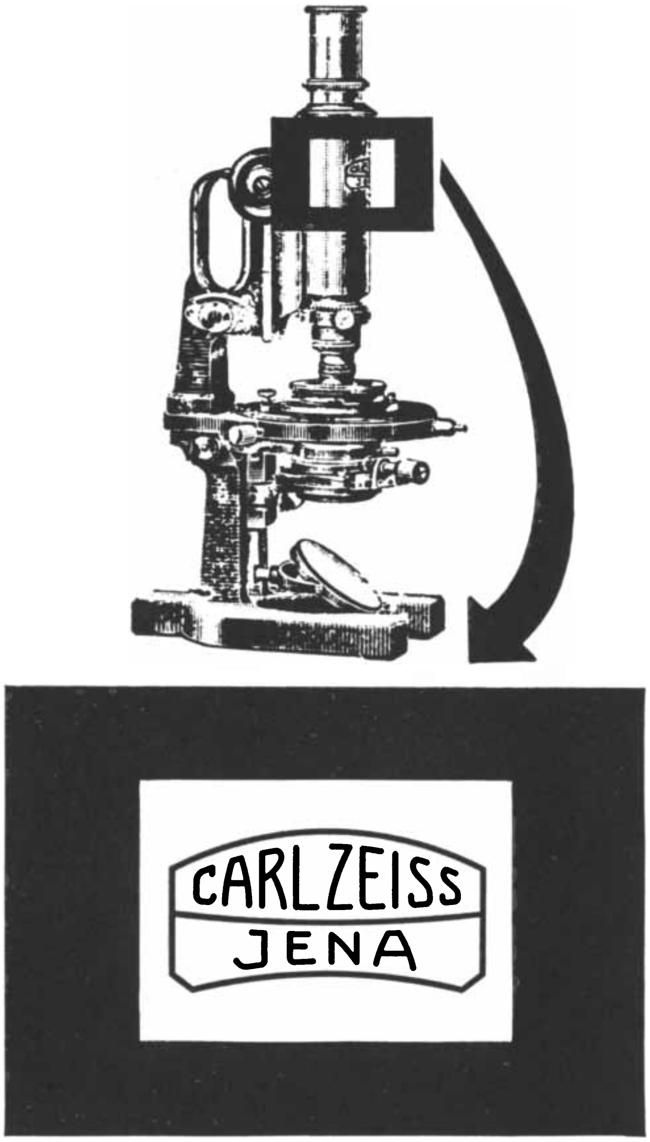

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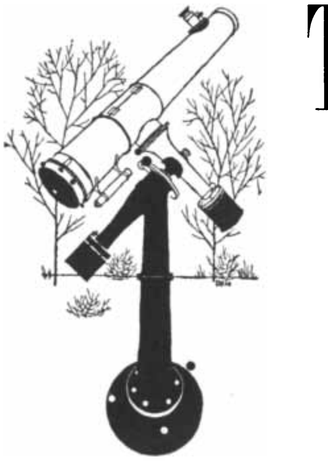


THE AMATEUR SCIENTIST

On preserving the shapes of snow crystals by catching snowflakes in dissolved plastic

Conducted by C. L. Stong

Preserving the shapes of snowflakes can be an engrossing wintertime avocation. One does this by catching the flakes in a thin film of plastic that has been dissolved in a solvent. Surface attraction spreads the dissolved plastic over the snowflake; when the solvent evaporates, the hardened plastic retains a permanent impression of the flake. A collection of these jewel-like forms not only is beautiful but also can shed light on the meteorological processes that account for the fall of snow.

Every snowflake carries in its structure a record of the atmospheric conditions responsible for its formation and growth. Because only one flake at a time can fall through a given path in the air the structure of each flake is unique. Of course gross similarities are observed in all crystalline substances, snowflakes not excepted. In snowflakes such similarities are explained by the fact that water habitually forms hexagonal crystals and by prominent meteorological factors such as the depth of the air through which the crystals fall, the temperature at various elevations and related differences in the ratio of water vapor to other gases in the atmosphere.

Investigators who specialize in deciphering the history of snow crystals must sometimes wish it were possible to make permanent collections of actual flakes. Unfortunately snow crystals are as perishable as they are beautiful. Usually the crystals either melt or sublime (evaporate from solid to vapor) promptly on reaching the ground. For centuries students of snowflakes had to content themselves with drawings of the characteristic forms. A partial solution to the problem of recording the geometric patterns of flakes came in the 19th century with the invention of photomicrography. One zealous amateur, Wilson A. Bentley of Jericho, Vt., took up the hobby in 1885 and eventually published a book of more than 5,000 pictures of snowflakes; the book is still prized as the classic reference in its field.

The most significant advance, however, came in 1940 with the replication of snowflakes in plastic. This technique was developed by Vincent J. Schaefer of Schenectady, N.Y., then a research associate at the General Electric Research Laboratories and now director of research at the Atmospheric Sciences Research Center of the State University of New York. "About 25 years ago," writes Schaefer, "while I was waiting for a bus on my way home from work, a snowfall occurred that consisted of unusually beautiful snow crystals of the dendritic, or branching, type. I wondered if it might not be possible to devise a way of preserving such beauty. I had recently been making castings of crystal patterns in aluminum, and the thought came to me that perhaps a modification of the procedure could successfully capture the impression of snow crystals and that of frost patterns.

"When I reached home, I placed in a bottle a small amount of a new plastic solution with which I had been experimenting and put it in the freezing compartment of the refrigerator. This substance was polyvinyl formal. It is obtainable under the brand name Formvar 15–95 from the Monsanto Company, Bircham Bend Plant, P.O. Box 2130, Springfield, Mass. 01101. The solution I refrigerated consisted of one gram of Formvar 15–95 in 100 milliliters of ethylene dichloride, a solvent.

"When the material had cooled below the freezing point of water, I spread a thin layer of it on an equally cold glass microscope slide, scraped a few frost crystals from the walls of the freezing compartment onto the wet slide and then—leaving the slide in the refrigerator—waited for the solvent to evaporate. After a few hours, when the slide was dry, I took it out of the compartment and examined the frost crystals with a hand lens. They were perfectly preserved and remained so long after the slide had become warm! My excitement can be imagined.

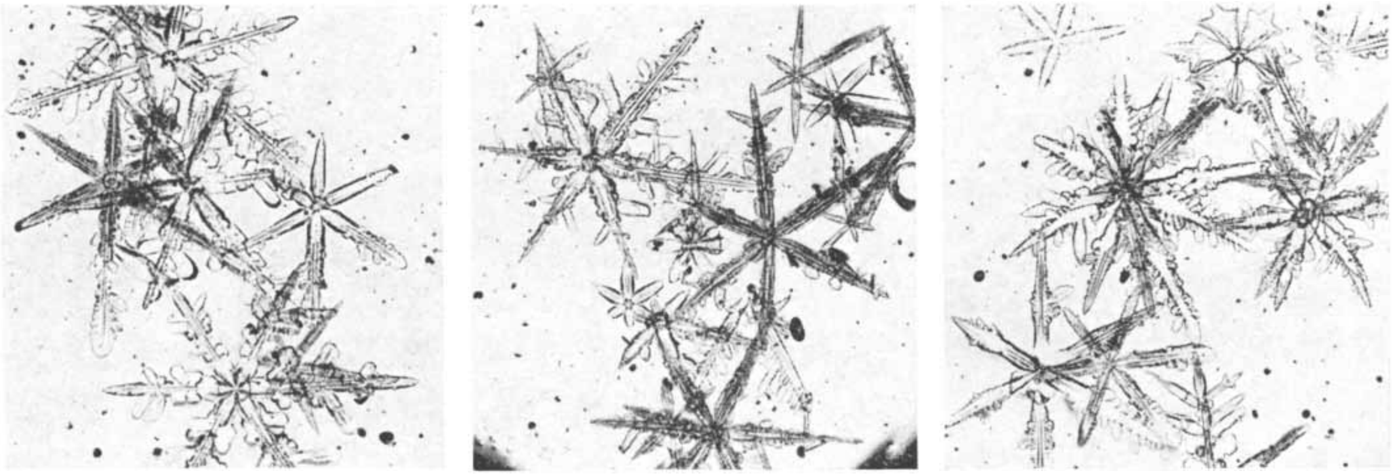
"During the rest of the winter I was out in many snowstorms making replicas of snow crystals by this method and investigating other methods. Naturally I learned that there are a few tricks to the technique. Not all my early replicas were equally good. Even so, the art of replicating snowflakes in plastic is fairly easy to master after the experimenter becomes familiar with the behavior of the plastic.

"The replica consists of a film of plastic with several properties. The film is thick enough (and hence strong enough) to retain its shape when the ice melts. Yet it is thin enough to make a faithful impression of sharp corners and other crystalline details and to permit rapid evaporation of the solvent.

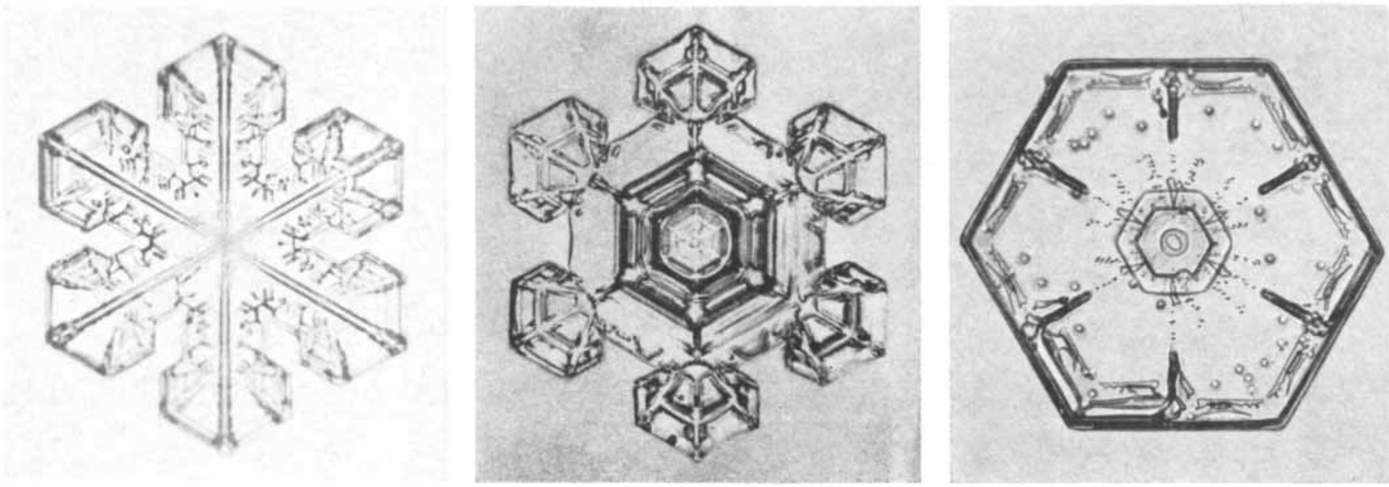
"One minor difficulty that can plague the beginner is the sublimation of the crystal as the solvent evaporates. Another is 'blooming,' the formation of a white, powder-like surface on the normally transparent plastic film. Blooming occurs during evaporation if the surface of the film cools below the dew point of the surrounding air so that moisture condenses on the drying surface in the form of tiny water droplets or frost. Ways to prevent blooming include maintaining the replica solution a bit colder than 0 degrees centigrade, reducing the rate at which the solvent evaporates (such as by immersing the replica in an atmosphere charged with enough solvent vapor to retard evaporation) or lowering the relative humidity.

"For optimum results the concentration of Formvar in solution must also be adjusted with respect to the size of the particles to be replicated. In the case of 'diamond dust'—ice crystals that rarely exceed .2 millimeter in diameter—and other crystals up to approximately one millimeter in diameter, the concentration of Formvar in solution should range between .1 and .5 percent when ethylene dichloride is used as the solvent. Such a proportion, for example, would be .1 to .5 gram of Formvar in 100 milliliters of ethylene dichloride. The concentration can be increased to 1 percent in the case of ordinary snow and frost crystals. For replicating snow on the ground, as well as rime and graupel (also known as soft hail), the concentration should be increased to 2 percent or more.

"Evidently luck was with me on the occasion of that first experiment. I have never hit on a more effective replicating material. Still, materials other than Formvar dissolved in ethylene dichloride can be used. Any solvent can be substituted that is not soluble in water. For example, chloroform can be used if more rapid evaporation is desired. Accelerated evaporation induces quicker cooling, however, thereby encouraging the condensation of moisture and the possibility of blooming. The experimenter should avoid such common solvents as acetone, amyl acetate and alcohol because they are soluble in water. Similarly, benzene is unsatisfac-

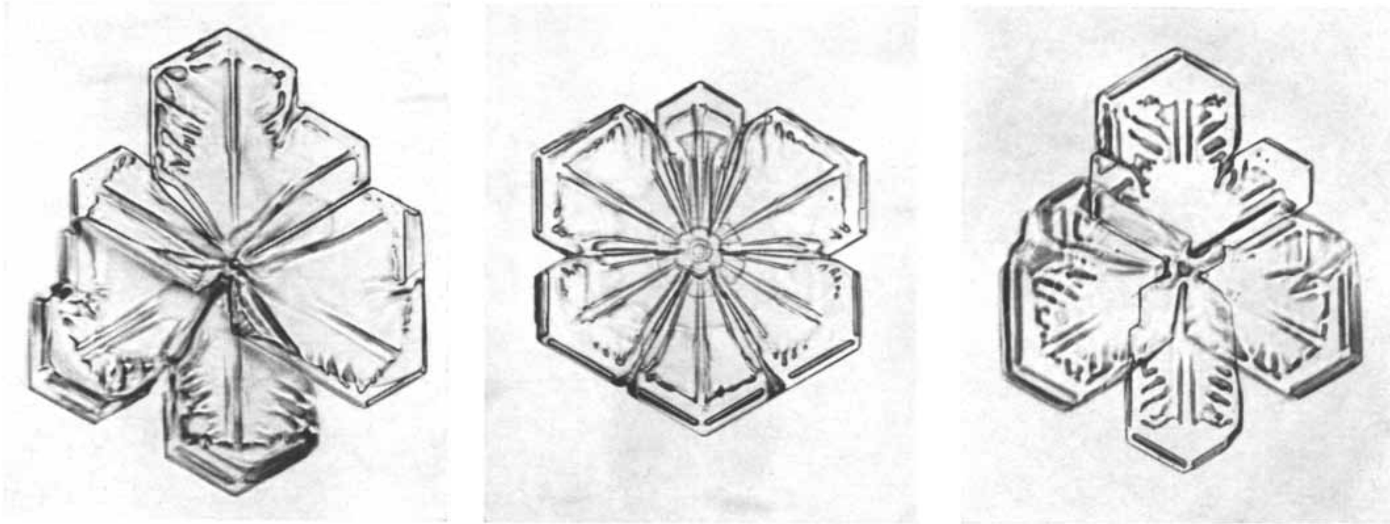


Shapes of snowflakes preserved in plastic

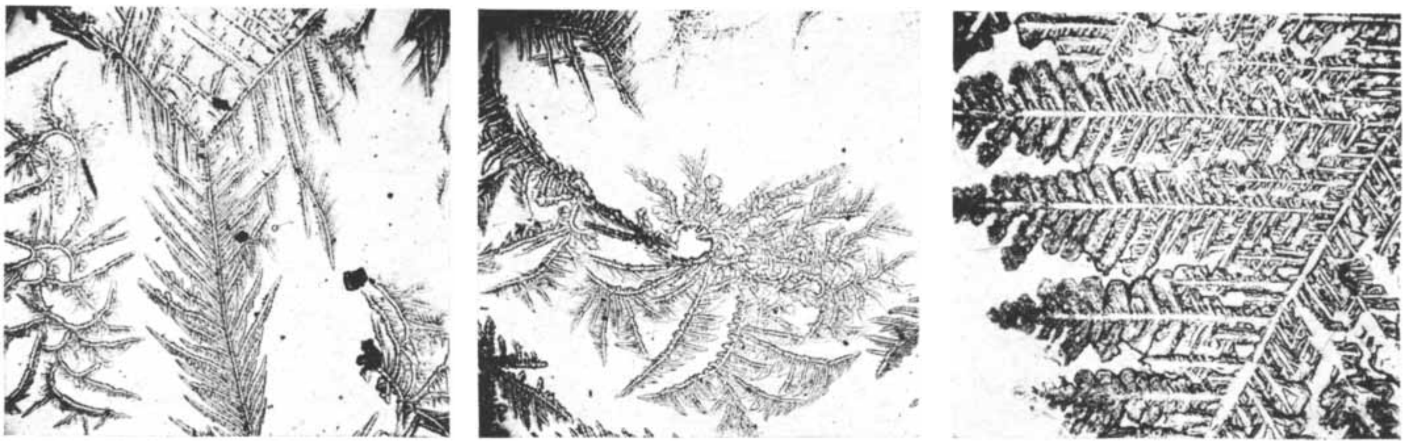


Snow crystals called hexagonal plates

Examples of stellar snow crystals



Some of the small crystals called "diamond dust"



Frost crystals

tory as a solvent because it solidifies before it reaches the freezing point of water.

"Just as it is possible to use solvents other than ethylene dichloride, so one can use plastics other than polyvinyl formal. Among the alternatives are polystyrene and methyl methacrylate. Polystyrene is readily available as Styrofoam. Polymerized methyl methacrylate is commonly known as Plexiglas. Scraps of Plexiglas reduced to granules by means of a wood rasp dissolve readily. Both materials are soluble in either ethylene dichloride or chloroform.

"A number of procedures have been devised for making replicas. One of the simplest calls for the plastic solution to be stored in a squeeze bottle made of polyethylene and kept below the freezing temperature of water. Enough solution is squirted onto a cooled glass slide to wet the upper surface. The slide is exposed to falling snow in order to collect specimens on the wet surface and is then placed in a cold, ventilated area for drying. The characteristic odor of the solvent becomes less apparent as the slide dries. When the odor can no longer be detected, the replica is ready for use. The best results are obtained when the slide is kept well below 0 degrees C. until the plastic hardens. Hardening may require some hours even in a well-ventilated area. Water molecules of the specimen migrate through the plastic film without difficulty.

"The most beautiful snowflakes occur in three basic forms: hexagonal plates imprinted with symmetrical designs, triangular plates and six-pointed stars with lacelike branches. Surprisingly, few storms produce perfect crystals of these types in substantial quantity. The dendritic stellar forms usually appear in large numbers during gentle snowfalls on cold, still nights.

"The platelike crystals, both hexagonal and triangular, appear to originate in high clouds. Six-pointed stars of simple design often form at altitudes of 1,000 feet or less. Elaborately ornamented crystals of the stellar type most often fall from clouds of intermediate height—the crystals of greatest complexity appearing when the weather is mild. The crystals comprising diamond dust are so small that they require many hours to fall a few thousand feet. Most of them sublime on the way. For this reason it appears that these minute particles form close to the ground. Snowstorms of the most common type produce clusters of flakes in random array that contain few symmetrical crystals and range in diameter from a few millimeters to several centimeters.

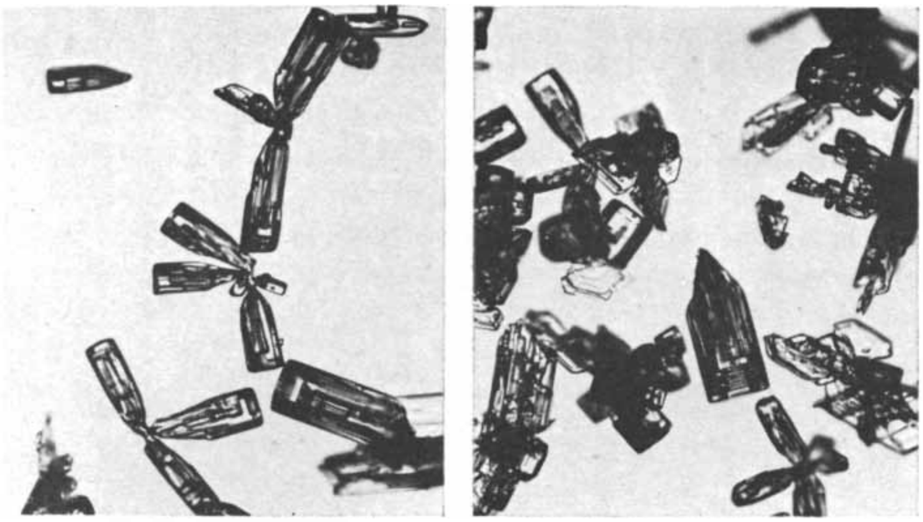
"Nicely formed crystals can be caught on a sheet of black velvet (black for ease of visibility and velvet for ease of handling) that is exposed to the fall for a few seconds and then examined in a cold, sheltered location such as an unheated shed, garage or tent. Specimens can be lifted from the velvet easily with a toothpick that has a small drop of 1 percent replica solution on the tip. The drop of solution can be collected on the tip by dipping the wood into the solution and withdrawing it quickly. The bottom of the suspended drop is then quickly but gently applied to the center of the specimen. Surface tension will lift the crystal from the velvet. Care must be taken not to press the wooden tip against the specimen or the crystal may be broken.

"The crystal is then transferred to the wet slide. Surface tension exerted by the large area of fluid now in contact with the bottom of the crystal will pull the specimen away from the wooden point. It will promptly sink into the film on the slide. As many as 20 crystals can be so arranged on a two-inch cover glass of the type used for making 35-millimeter projection slides. Once prepared, replicas can be covered by a second glass fastened in place with binding tape for optical projection, photographic enlargement or microscopic examination.

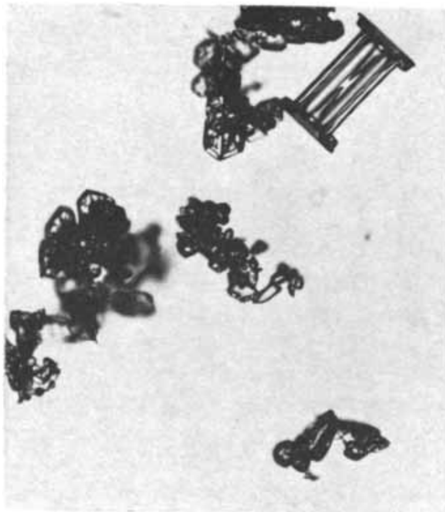
"Base materials other than glass can be used. Replicas show up nicely on black cardboard, for example. I have also used sheets of polyethylene, which are easy to cut into strips of the desired size. The thin transparent plastics such as Saran Wrap can serve as flexible mountings. They are easy to handle when they are supported by a frame such as an embroidery hoop.

"To speed up the drying of replicas I have frequently made precoated slides. The base material is dipped into or otherwise coated with a replica solution of the required concentration and dried. The precoated surface is then moistened uniformly with solvent and charged with specimens. The coating dissolves in the immediate vicinity of the snowflakes. Much of the excess solvent is soaked up by the surrounding film. Because of surface attraction every nook and cranny of the specimen acquires a thin plastic coating. Some of my most successful replicas have been made by this procedure.

"Crystals smaller than one millimeter in diameter, such as 'diamond dust,' can be replicated most successfully by a variation of the precoated-slide technique. After the crystals have come to rest on the dry coating the slide is exposed to the vapor of the solvent, either ethylene dichloride or chloroform. (A vapor chamber for exposing slides can be improvised from a small wooden box equipped with an airtight lid. A shallow dish of solvent placed on the bottom of the chamber provides the vapor; a slit in the side of the box that opens onto an inner shelf admits the



Crystals originating in cirrus clouds

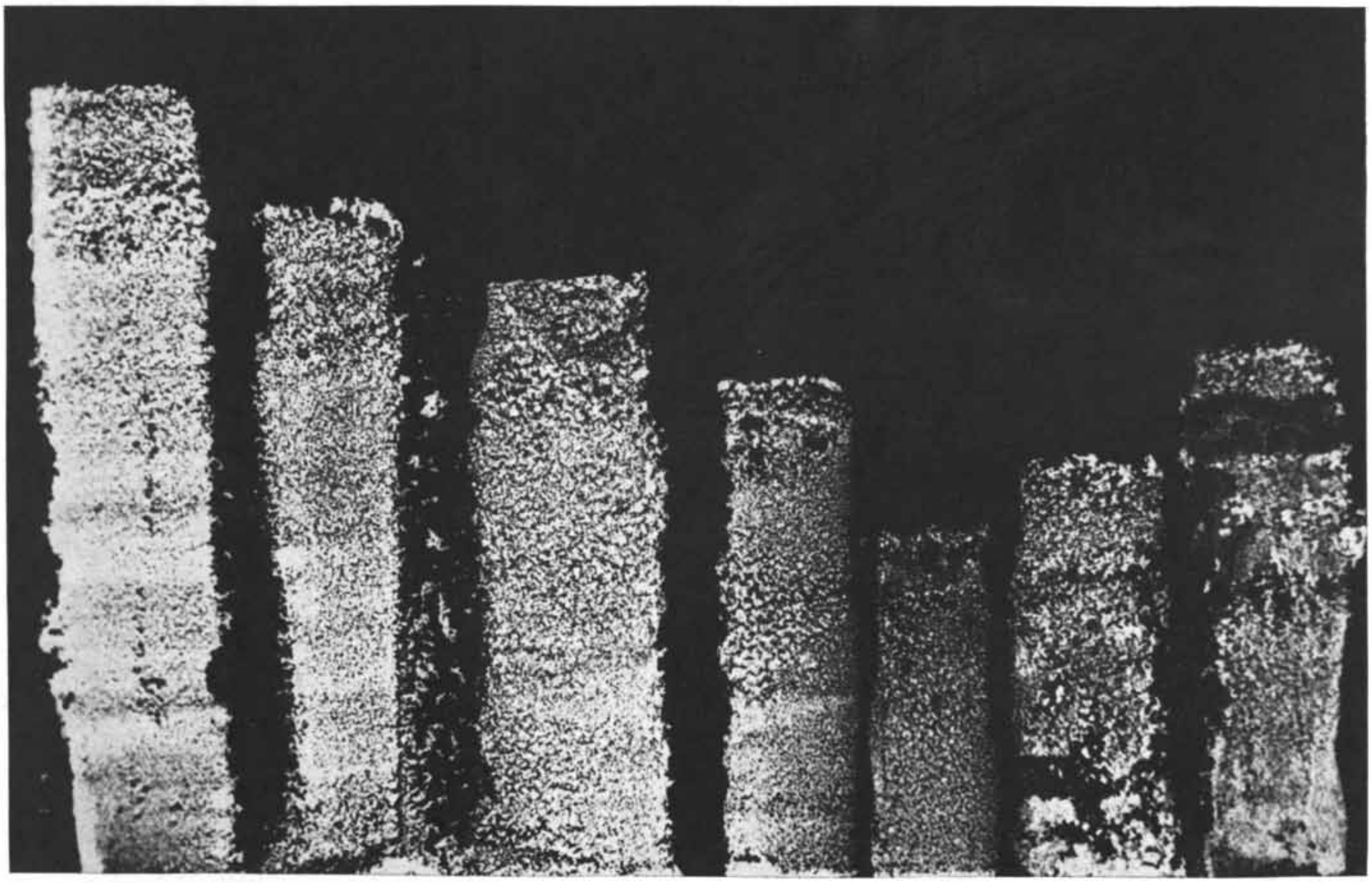


A "capped column" crystal

slide.) The vapor condenses as a thin film on the coating but tends to accumulate around each specimen. There it dissolves the plastic locally and by a combination of surface tension and capillary effects coats the snowflakes in three dimensions.

"Another convenient technique makes use of the clear lacquer solutions now commercially available in aerosol-spray cans. Snowflakes that have collected on a base material are simply sprayed with the solution. The method is particularly useful for replicating frost on windowpanes. For best results the spray can should be held far enough away so that the plastic is almost dry when it makes contact with the crystals. The optimum distance varies with the drying time of the spray and must be determined by experiment. Krylon, an aerosol lacquer available from dealers in paint and art supplies, is particularly effective for this technique.

"Replication experiments need not be limited to small crystals and frost. For example, interesting and useful records of snow accumulations on the ground can be compiled by making daily replicas during a period of a week or more. For the base I use a sheet of window glass as wide as the snow is deep and from 15 to 20 inches or more in length,



Sections obtained on successive days from a layer of snow

depending on the desired number of replicas. I apply a vertical strip of 5 percent solution about two inches wide at one end of the glass and press the glass gently against a vertical cut in the snow layer. The glass is then removed, along with the adhering film of snow, and placed with the replica side up in a cold, sheltered place. The plastic will dry within 24 hours and the film of snow will have sublimated. At approximately the same hour on succeeding days additional strips, each adjacent to its predecessor, are similarly applied to the glass and the replicating procedure is repeated. The series provides a sequential account of the gradual consolidation of snow by recrystallization even when the temperature remains well below freezing through the entire period of observation. Such effects as the gradual disappearance of layering that is always evident in fresh falls and the conversion of light flakes into 'corn' snow of relatively high density become strikingly evident in the record.

"Replicas can also be made of the structure of bulk ice, much as the crystalline pattern in metals is revealed by polishing and etching techniques. Experiments of this kind can be made even in summer in locations where hailstones fall. In fact, any kind of ice can be used: an icicle from the edge of a roof, a small piece of ice from a pond or stream or even an ice cube from the refrigerator. Whatever the specimen, the ice should be kept well below freezing temperature. If necessary, place the specimen in the freezing compartment of a refrigerator for a few hours. Begin the procedure by cutting a piece of plastic foam about the size of the specimen. This will be used as a handle to insulate the specimen from heat. To attach the handle, make a reasonably flat surface on the specimen by sawing or otherwise cutting off any roughness. Then spread a bit of water on the plastic foam and apply the wet surface to the flattened ice. The water will promptly freeze, cementing the handle to the specimen.

"Next, heat an electric iron or other flat metal object until it is warm to the touch. Press the specimen against the warm iron until the ice melts enough to form a flat surface. Immediately wipe the surface dry with a soft cloth. The temperature of the specimen will have risen substantially. Return it to the freezing compartment for an hour or so.

"Cover one surface of a flat board about 10 inches square with a piece of velvet stretched tight and securely tacked at the edges. The velvet will serve as a polishing buff. Remove the cooled ice from the freezing compartment and, manipulating the specimen by the handle, rub the flattened surface of the ice briskly across the velvet until it acquires a polished surface. The polishing operation requires only a minute or so. Then store the specimen in a cold, dry atmosphere for etching. The crystalline pattern will appear in a matter of minutes. Etch pits, marking crystal boundaries in the ice, are formed by sublimation. They stand out clearly as differences in the intensity of reflected light. Etching by sublimation requires that both the ice and the atmosphere be dry. Otherwise frost may form on the specimen and spoil the experiment.

"To replicate the etched surface, cover the polished area with a film of 2 percent Formvar solution and store the specimen in a freezing compartment until the plastic is dry. Then transfer the ice to a bowl of water. The replica will float free when the ice melts. It can then be maneuvered onto a glass slide and, when dry, covered by a second slide to make a permanent mount.

"Beautiful ice crystals can also be grown synthetically and replicated. For this experiment I use a water-soluble material such as polyvinyl alcohol or gelatin in concentrations ranging from 1 to 3 percent—one to three grams of alcohol or unflavored Knox gelatin in 100 milliliters of distilled water. A sheet of well-cleaned glass or other base material is flooded with a film of the solution and placed level in a cold, protected location until the film is supercooled—that is, has chilled below its normal freezing point. If a minute ice crystal or other appropriate freezing nucleus is now touched to the film, beautiful crystals will form immediately. As the crystals grow the alcohol or gelatin will precipitate out of the ice, marking the outlines of each crystalline feature. If the assembly is maintained at subzero temperature for several days, the ice will sublime, leaving a replica of great beauty.

"By careful seeding it is possible to grow huge single crystals of ice by this method. I have made some that measure up to 10 inches in diameter by restricting nucleation to a single site. Similar effects can be induced in soap bubbles that are either free-floating or held in a ring. They are difficult to replicate but useful for the detection of ice-forming nuclei in the air."

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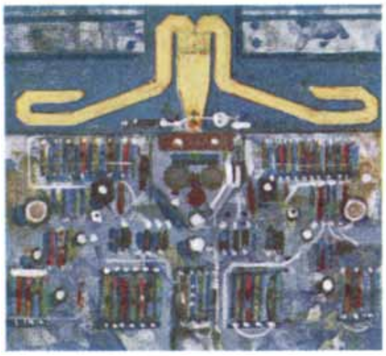
2. The oscillogram is an actual trace on the 140A Oscilloscope with the 1415A Time Domain Reflectometer plug-in. Two inductive discontinuities are shown at a BNC connector joining a pair of 50-ohm cables. The distance between the discontinuities represents the connector length itself.

3. TDR can be used to test impedance of coaxial cables and for checking cable assemblies to maximize signal transmission.

4. The electronic design lab finds broad use for TDR. In measuring transmission line characteristics, the engineer can locate, identify and measure the magnitude of reactive and resistive discontinuities. Thus, design can be altered to improve performance.

5. TDR is useful in liquid measurements. A slotted coax probe allows liquids to flow through freely, permits identification of liquids by their variation in dielectric constants. Typical applications would be locating different layers in a pressurized or high-temperature vessel, measuring liquid interfaces at low temperatures or in the presence of foam...even determining completion of a chemical reaction by displaying the disappearance of an interface.

6. Problems within inaccessible coax cable are being solved by Time Domain Reflectometry: construction and maintenance of aircraft, ships, broadcast facilities, missile ground-support systems and the like. Cable can be tested for damage during installation and for degradation with age, vibration, bending.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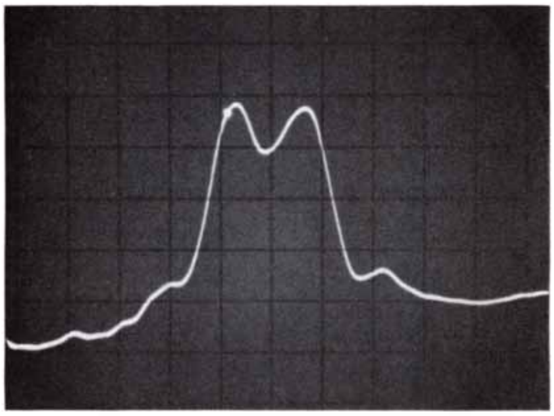
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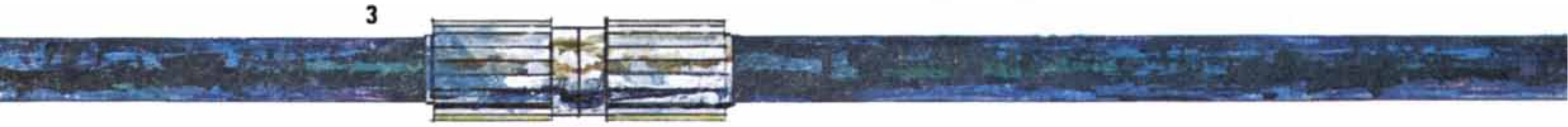

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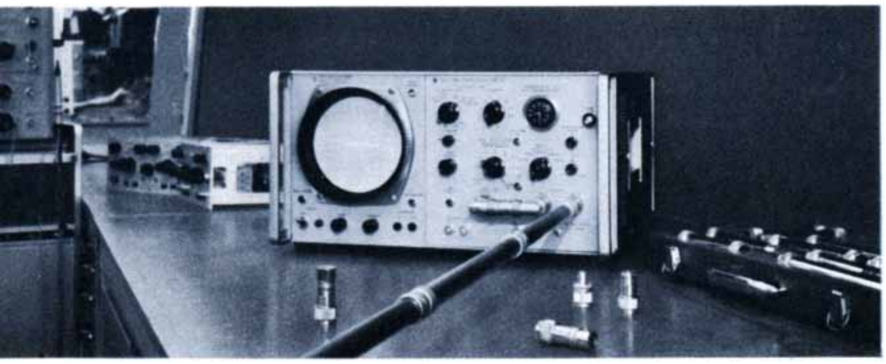

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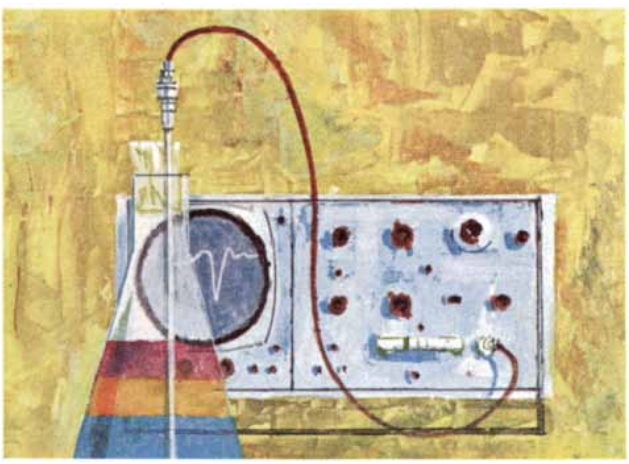


6

BOOKS

About the role of science in history, in contrast to the history of science

by N. W. Pirie

SCIENCE IN HISTORY, by J. D. Bernal. C. A. Watts & Co. (four pounds four shillings).

"The fox knows many things, but the hedgehog knows one big thing." That fragment from the writings of the early Greek poet Archilochus gave Isaiah Berlin the title for his book *The Hedgehog and the Fox,* which contrasted two types of historian: those who ranged, or even rambled, over all aspects of their subject and those who had a clear picture of what they were trying to demonstrate and made every facet of the story contribute to it. Marx is a hedgehog and Macaulay is a fox. Berlin would have trouble categorizing J. D. Bernal. He is an intriguing, if zoologically improbable, hybrid.

Bernal carefully explains that he is not writing a history of science. He is undoubtedly doing that, and doing it as effectively as many writers who have no other aim. He is also writing a universal history from the standpoint of the development of science and technology and is arguing that the long-term trends in history—the rise and fall of civilizations rather than dynasties—can be understood only when the technical competence and philosophical outlook of the participants are understood. Technical competence does not show itself merely in the improvement of weapons, whether in the familiar transition of materials from stone to bronze to iron or in the design of military machinery. He attaches importance to these things but attaches more to the stages by which communities were able to get away from a primitive subsistence economy and build up a surplus.

Bernal is a Marxist and analyzes the situation at each stage in Marxist terms. Those who do not share his philosophical and economic views—either because they understand and adhere to the capitalist philosophy or, more likely, because they have no coherent economic philosophy but, having been nurtured in a capitalist environment, find his terminology unfamiliar in a scientific work—will often wince at what looks like the obtrusion of politics. They will be mistaken. Words such as "exploitation," "parasitism," "imperialism" and so forth have a clear meaning in the context and save a great deal of space. Bernal points out that the very existence of an extensive parasitic class, that is to say, a class that consumed exuberantly while not making a material return (medieval churchmen, for instance), is the best evidence we have for the existence of a surplus and therefore of agricultural and mechanical competence. It is not necessary to agree with Bernal's strictures on the ways in which the surplus was used in order to accept the existence of these ways as evidence that the surplus was there.

The material in this book is based on the Charles A. Beard Lectures given at Ruskin College in Oxford in 1948. It has expanded steadily in successive editions and has not unexpectedly been subjected to active criticism in book reviews, letters and comments in other publications. In some of the notes in his book Bernal replies to criticism in a refreshingly combative manner; it is pleasant to find someone saying that something is nonsense when that is what he thinks it is.

Having explained that he is not writing a history of science, Bernal goes on to say that he does not intend to define science. He comments on its nature at many points, saying, for example, that it is "ordered technique," "rationalized mythology" and that it "implies socialism," but he is wise to avoid a rigid definition when the meaning that can usefully be applied to the term varies with the state of the art. It would be difficult to stretch a satisfactory up-to-date definition of science so as to make it cover the slow development of knowledge in prehistoric communities. This development is discussed briefly, and it is hard to see how anything much fuller could be managed in the absence of a written record. Nevertheless, Bernal seems to underrate the amount of accurate memorized information accumulated during the preliterate phase. No plant large enough to handle is now used on a significant scale as a source of food or drugs that was not known to primitive man. All we have done is move plants about in the world and breed better varieties.

Greek science is generally discussed by people who are not scientists and who have an entirely irrational respect for the verbal pitfalls the Greek philosophers dug for themselves. Commenting on Zeno's paradoxes, Bernal says: "These subtleties were taken to prove that the visible world cannot really exist; but they may serve as well to show that pure reason can be sillier and emptier than anything the senses can contrive." He is, however, fully aware of the abiding value of Greek mathematics. Not unexpectedly he agrees with most other commentators in attributing the inadequacy of Greek science and technology to the use of slaves; with abundant labor there was little stimulus urging people to think in concrete physical terms. The slave-based economy even generated a snobbery that made Plato, who was born wealthy, sneer at philosophers who took fees. Bernal is sympathetic to Hippocrates and points out that he came as near to a rational conception of disease, without magical or religious causes and cures, as was possible at the time. He also has a good word for Alexander, who, by setting up and financing the museum in Alexandria, started the tradition of government support for science on which most of us now depend. But he is oddly antagonistic to Aristotle, in spite of a note in this edition apologizing for his harshness in earlier ones. Aristotle cannot be blamed for having been adopted by Aquinas and run as a sort of Roman Catholic mascot for 1,000 years, nor can he be blamed for being an enthusiastic practical biologist who drifted at times into writing intellectually coherent but often ill-founded physics and philosophy. After all, Bernal has been known to be similarly mobile in the opposite direction.

"Knowledge that is not being used for the winning of further knowledge does not even remain—it decays and disappears." That is Bernal's accurate summing up of the end of Greek science and the sterile period of Roman domination, when "barbarians," who, on their wet lands, were better farmers than the Romans, adopted unsuitable methods and gained some roads in exchange. Gibbon attributed the decay to Christianity and other religions: "The various forms of worship, which prevailed in the Roman world, were all considered by the people as equally true; by the philosopher as equally false; and by the magistrate, as equally useful." Bernal does not agree and looks for other reasons; he does not quote Gibbon's familiar sentence and he may not have attached enough weight to the last phrase in it. An effective government with nothing to fear from its neighbors is likely to prefer religions that support the *status quo* to scientific efforts liable to upset it. China, which is considered very briefly here because Joseph Needham is recording its scientific history so thoroughly, is a case in point. Having shown unparalleled scientific and inventive capacity while Europe was barbarous, the Chinese lost the spirit of inquiry as they gained isolation and strong government. With the loss of isolation science is now returning.

The Arabs, as is well known, maintained the Greek texts during the Dark Ages. They also contributed magnificently to chemistry because their philosophers were practical pharmacists and workers in metal and glass. This attitude of mind reached medieval Europe and was well stated by Roger Bacon in praising Peter Peregrinus: "He knows natural science by experiment, and medicaments and alchemy and all things in the heavens or beneath them, and he would be ashamed if any layman ... should know anything about the soil that he was ignorant of." Few periods have been associated with so much technological change, partly because of local invention and partly because of information diffused by travelers. The horse collar, windmill, clockwork, compass, sternpost rudder, spectacles, papermaking, printing with movable type, Arabic numerals, gunpowder and alcohol all appeared at this time. The last two had a particular philosophical impact: they opened up new vistas in the properties of matter. John Mayow remarked later: "Nitre has made as much noise in philosophy as it has in war." The volatility, inflammability and pharmacological activity of alcohol excited equal wonder; Bernal asserts that its distillation was the first chemical industry. It was an aspect of technology of immediate interest to primitive people. Those who spoke Gaelic called it *uisge beatha,* the water of life, and so gave us the word "whisky," and Manhattan means "the place where we got drunk."

The Renaissance is a curious period. There was a genuine concern with practical observation and the work of artisans, as is shown by the development of naturalistic art and accurate anatomy, astronomy and geography. Skepticism flourished: Rabelais, outlining an educational regimen for Pantagruel, advised disregard for the alchemical ideas of Ramón Lull, and Copernicus forthrightly condemned the Ptolemaic system that 200 years earlier so wise a man as Alfonso of Castile had felt able to condemn only obliquely. ("If God had consulted me before embarking on the Creation, I would have suggested something simpler.") Nevertheless, it also tolerated the mystical biochemistry of Paracelsus and the magnificent undisciplined imagination of Da Vinci, of whom Bernal remarks: "He could invent machines for almost any purpose and could draw them incomparably well, but hardly any of them, and none of the most important, would have worked."

Science as we understand it, with controlled experiment complementing accurate observation, may be said to have started with William Harvey. He did not dispute about what the heart ought to do and where the blood ought to go; he measured the flow from the heart and demonstrated the impermeability of its septa to a flow of that magnitude. And he described the experiment, which a child can repeat, on the superficial veins that demonstrates centripetal flow in them. Harvey's contemporary Galileo was also an experimenter, and a very great one, but the Middle Ages still enmeshed him, as is shown by his remark that he did experiments to convince others on matters about which reason had already made him certain. This is the old outlook, as Boyle recognized when, in *The Sceptical Chymist,* he has the Aristotelian say: "For it is much more high and philosophical to discover things *a priori* than *a posteriori.* And therefore the peripatetics have not been very solicitous to gather experiments to prove their doctrines, contenting themselves with a few only, to satisfy those that are not capable of a nobler conviction. And indeed they employ experiments rather to illustrate than to demonstrate their doctrines." Two theorists stand out in this period: Bernardino Telesio and Francis Bacon. The former urged scientists to eschew Aristotle's "final causes"; that is to say, he got rid of "ought" as Harvey did. The latter advocated enormous schemes of planned experimentation but did not realize how much an actual physical contact with the experimental material contributes to a scientist's thinking. Like Antaeus in the Greek legend, scientists tend not to be productive for long when their feet are held off the ground! Bacon may have been prudent in sticking to theory; he died as a result of one of his few recorded experiments—stuffing a chicken with snow in the course of studying putrefaction.

Descartes realized that experimentation on the necessary scale would be an expensive business and so paved the way for endowment. Bernal points out that this was thoroughly in accord with the mercantile spirit of the 17th century but that local conditions controlled the form of subvention. In autocratic France, where Colbert was setting up nationalized industries, there was government support. Britain, with its pleasant habit of operating as a semianarchy, set up the private-enterprise Royal Society dedicated "to improve the knowledge of natural things, and all useful Arts, Manufactures, Mechanick practices, Engynes and Inventions by Experiment."

If human history followed a rational course—even such a limitedly rational course as the fossil record suggests for evolution—it should now have been possible to inaugurate the scientific revolution. Final causes were out, experiment was in, and it was generally agreed that a proper balance should be struck between fundamental and applied research. Bernal calls the Dark Ages the Age of Faith; the period from the end of the 17th to the beginning of the 18th century is often called the Age of Reason. It would be more accurate to call it the Age of Schizophrenia, and we still live in it. The beginning of the century was dominated by Newton; his *Philosophiae Naturalis Principia Mathematica* is an unmatched intellectual achievement. Most symposia on the history of science (certainly all those held in Britain) pay tribute to his unique powers, yet half a million words by him languish unpublished in London and Cambridge. His interest in the last phases of alchemy, and in the possibility of occult knowledge, is considered unseemly. It was no doubt mistaken, and

there is more than a grain of truth in the description of him as "the last of the Magi," but it would be interesting to know just what he thought—if only to comprehend William Blake's couplet "May God us keep/From Single vision and Newton's sleep!"

To a present-day reader even Newton's published writings show, if not the "fourfold Vision" called for by Blake, at least double vision. Boyle published as much theological matter as science. John Wilkins, in "An Essay towards a Real Character, and a Philosophical Language," analyzed the essential nature of language and was far ahead of his time in outlining an international symbolic notation based on concepts rather than words. He then went on to refute "some Atheistical scoffers" by devoting many pages to the size of Noah's ark. Even here his scientific acumen did not desert him: he became the forerunner of quantitative nutrition by expressing requirements in sheep units and so ultimately as hay. Joseph Priestley was as much a theologian as a scientist and, as Bernal records, surprised the French by his belief in God. The list can be extended up to the present day, and it raises a problem to which Bernal pays too little attention: How do some scientists contrive to live in two incompatible intellectual environments?

From the middle of the 18th century until the end of the 19th there was fairly uniform development in all the sciences and technologies and step by step they took on a modern aspect. Scientists increasingly became specialists and no longer tried to cover the whole domain single-handed. Recognizing this, Bernal changes the form of his survey and follows right through the period a few separate aspects of knowledge in isolation from the others. He speculates on the factors that led to the uneven regional development of knowledge. The idea of variations in the innate capacity of the people involved is quickly dismissed. James Watt is amusingly quoted as saying that "the Scots were naturally incapable of becoming engineers"; it would be salutary to have a comprehensive anthology of similar pontifical illusions as a warning to contemporary racists. Bernal argues that an important factor was the absence of an obstructive, complacent, landed group, perfectly content with things as they were. This satisfactorily explains the rise of the chemical industry in Scotland, engineering around Birmingham and in New England, and university research in Germany. Both here and later he comments on the relative ease with which financing can be got for meeting an existing need in a novel way, or for developing a new use for a process or material that already exists, in contrast to the tremendous opposition and delay that confront any attempt to do something novel using a novel technique. The mind seems to jib at coping with two new ideas at once. This is well illustrated by Bernal's masterly account of the development of the steam engine and electricity. With the former, practice kept ahead of theory for a century because pumping and hauling were all too obviously burdensome; the latter, insofar as it penetrated beyond the laboratory at all, did so for 50 years essentially as a toy and a sideshow in fairs.

This section of the book begins and ends with political analyses of the relationship of scientists to the community during the period when the style of research as a paid occupation was established. It seems unnecessarily gloomy. The cautious political conformism of those who are dependent on a government or an industry for their jobs is contrasted with the old unruliness of the scientific aristocrat or the scientist who earned his living on the side by peddling aphrodisiacs, horoscopes or something of the sort. The real contrast would seem to be between the period when only a few people per million were scientists in even the most developed countries and the period when the proportion had increased to a few per thousand. Conformity is always a pity, but all we are forced to recognize is that, with the steady increase in the scale of science, scientists increasingly are becoming an average cross section of the population, with a training rather than a mania.

Having reached the end of the 19th century, we are just halfway through this monumental book. The mode of approach is again changed. Contemporary politics are introduced, and with them value judgments on which there will be sharp differences of opinion. We are all at one in condemning slavery or poking fun at the feudal system and are prepared to agree that these systems were obstacles to progress. The protean forms "democracy" has taken, and now takes, raise more difficulty. My prejudice is to dislike all governments as unfortunate necessities but to recognize some variation in their degree of incompetence and in the out-of-dateness of their basic concepts. (It may be this impartiality that causes me similar trouble and delay from the visa-issuing officials of both the U.S. and the U.S.S.R.) Bernal devotes a great deal of space to attacking this attitude, which is common among scientists, and finding reasons for it. The reason is simpler than he thinks. Anyone who works in Communist-influenced organizations soon finds that Communists still have something, but not much, to learn from capitalists about fraud, evasion, special pleading and sudden switches of policy. Examples of the last three abound in Bernal's eulogy on the state of affairs in science and education in the U.S.S.R. and China. The increasing government control over the universities in Britain is condemned, whereas the total control in some other countries is commended on principle, and not just because it leads to much bigger universities; the Lysenko episode is handled tactfully, there is a justified jeer at logical positivism because it has resulted in little experimental advance but no similar jeer at dialectical materialism, which is not noticeably more productive. Bernal advocates what he calls "provisionalism," that "whatever we think now, people in a very short time from now will think differently and better." We could have done with more political provisionalism.

All of this arises in the course of discussing the growth and present state of the social sciences. Much of this section, unlike the rest of the book, has a hectoring and pamphleteering tone. Malthus is tediously called "Parson" Malthus, and Marx is referred to as the Galileo of the social sciences. Aristotle might have been a more suitable avatar. But the section is redeemed by some sensible strictures on advertising and opinion polls, and by some stories of the misuse and nonuse of operational surveys during World War II. Bernal discusses the extent to which some surveys (those advocated or carried out by the Fabians, for example) were simply means for postponing the time when something had to be done about an obvious scandal. More up-to-date examples of this could usefully have been given. This delay may be the motive behind many present-day nutritional surveys in developing countries. It is obvious that food is needed; which precise deficiency is paramount is a somewhat academic point. There is also a brief history of economic theory enlivened by many sardonic comments on the nonsense promulgated by distinguished economists. Here, as at other points in the book, Bernal may be too prone to attribute malice and corruption to those with whom he disagrees. Folly and shortsightedness, which is much easier to recognize many years later than it is at the time, is often an adequate expla-

nation. As with Bernal's account of Greek philosophy, this is an interesting section; the subject is not often discussed by an experimental scientist.

Economists, like scientists, have their imaginations trammeled by the environment in which they live. It is because Bernal recognizes that this is half of his theme that the book is called *Science in History* and not "The History of Science." At every period, what serves as a satisfactory explanation of a set of phenomena is a metaphor based on some aspect of the contemporary scene. Socrates used navigation, cooking and childbirth; 17th- and 18th-century physiologists used bellows, levers and clockwork, and so on. This reciprocity between technology and explanation is so obvious that social metaphors based on Darwinian evolution and the "state of the market" were natural enough at the end of the past century, and Bernal may be a little unfair in pillorying economists for their use. Furthermore, it is odd to find, in an earlier section, surprise being expressed that in current theory the gene resembles a computer tape. Those who framed the theory were familiar with computer technology.

A final chapter surveys the whole scene and outlines many possible improvements in the organization of science and the scientific literature whereby planning could be made tolerable to the temperamentally anarchic people who tend to be the most original thinkers. Bernal points out certain essential preliminaries to this desirable state: the transfer to research of some of the resources now wasted on preparations for war, the elimination of the secrecy and restriction that preparations for war entail and, most important of all, sufficient general improvement in scientific education to ensure that our rulers get a little of it and become aware of how things work and what the immediate possibilities are. Although Bernal realizes that biological problems are now dominant, political bias leads him to underrate their complexity. The defects of capitalism are so great that there is no need to exaggerate them. The statement that biological research in underdeveloped regions is undertaken only when "conditions have become so bad that they have interfered with profit making itself" is extreme. It is obvious that progress would be more rapid if there were much more money, but a number of honestly conceived and well-financed projects in underdeveloped areas have run into unforeseeable biological complications.

The attempt to encompass in one book the nature, history, philosophy, present state, probable future and most serious frustrations of science is heroic. There are few besides Bernal who would have made it and still fewer who would have been successful. The book undoubtedly is successful. I have commented on some matters of political emphasis and there are a few factual errors in the biological sections, but these do not affect the general sweep of the argument.

Short Reviews

COMING OF AGE IN AMERICA: GROWTH AND ACQUIESCENCE, by Edgar Z. Friedenberg. Random House, Inc. ($5.95). A study of adolescents based on observations, interviews and tests directed to more than 200 students randomly selected in nine secondary schools, with one exception coeducational and located in small towns or small cities in different parts of the U.S. The aim was to discover how the children grow up in the schools, how they interact with their fellows, teachers and school administrators, what their values are as evinced in reactions to fictional but typical school situations and "problems" (presented to them as episodes described on cards), what animates and moves them, which features of American culture they accept and which they reject, and how the complex of influences, demands, requirements and responsibilities of the educational system shapes their entire personality. "The highest function of education," Friedenberg says, "is to help people understand the meaning of their lives, and become more sensitive to the meaning of other peoples' lives and relate to them more fully. . . . I do not, of course, suggest that this is or has been the primary function of education in the United States or any other major industrial country." It is to this point—to the ways in which education subverts this function—that he devotes the greater part of his book. He emphasizes not only the shortcomings of teachers and administrators but also the difficulty that even a dedicated and trained staff would have today in discharging its duties properly because of the inherently antagonistic forces that operate so powerfully in our mass society. The ideals of education he sets forth are not to be achieved, in his view, because the schools themselves can do no more than express their actual position in society, and the policies they carry out necessarily reflect the compromises the staff must make to maintain that position.

In any case, education designed to help people understand themselves and relate more sensitively to the lives of others is bound to be education for a minority, "for only a minority will accept it or demand enough of themselves to take part in it." The problem, then, is not how to extend the appeal of such education but how to protect and support it against the hostility of people who are frightened or outraged by it.

The adolescents with whom Friedenberg worked exhibit in miniature almost all the foibles of our society. Their values are the conventional, most acceptable values, which mass media, Madison Avenue, ministers, clubs and other social organizations of the community, schools and of course government ceaselessly proclaim and attempt to inculcate. This is not, it needs scarcely be said, a surprising discovery, but its implications and consequences are far more disturbing than even most thoughtful persons recognize. Two outstanding merits of Friedenberg's books are the clarity and forcefulness of his presentation, supported by an imaginative scheme of experimental design, and his undisguised bias in favor of the youngsters themselves; they, he insists, are more the victims than the beneficiaries of our educational system. As long as our children are raised in such a system, required to acquiesce in its controls and adopt its values, the promise of a great society—of which they are presumably to be the executors—is likely to remain little more than the sloganeering of politicians. This is a deeply felt and powerfully argued book.

The Management of Wild Mammals in Captivity, by Lee S. Crandall. The University of Chicago Press ($13.50). Not less than 60 million persons annually visit the zoos and aquariums of the U.S. Many of them must feel that they would like to know more about the animals than can be gleaned from the tersely worded plaques usually attached to cages and enclosures. Their interest and that of a variety of others—administrators, zoo keepers, pet dealers, veterinarians—is admirably served in this excellent book by Lee S. Crandall, general curator emeritus of the New York Zoological Park ("the Bronx zoo") and for more than 50 years an observant, sympathetic and imaginative student of wild mammals. He gives exactly the kind of information one would like to have: where the mammals came from and how they were transported to the zoo, how they must be handled to keep them healthy and happy, what their dispositions are, the diets of the various species, their lifespan, their mating habits in captivity, and so on.

Crandall has written numerous scientific papers on the subject, is a master of its large literature and has collected information about the experiences of major zoological parks all over the world. He writes easily and pleasantly and embellishes his survey with anecdotes and odd facts. One learns, for example, that a macaque gets a coddled egg as part of its breakfast and that its afternoon repast includes vegetables, fruits, raisin bread, chopped meat, cod-liver oil and "five or six peanuts." Vampire bats require a daily supply of fresh blood, although not necessarily sucked from the neck of a maiden. An otter's food intake should average from 25 to 35 percent of its body weight per day, preferably given in four meals at six-hour intervals. Tigers show strong temperamental differences: of three cubs raised at the Bronx zoo, Raniganj (a male) was almost from birth recorded as being "definitely bad tempered," his brother Rajpur as being "fat and indolent" and their sister Dacca as "bright and friendly." Sea lions, who seem to have an appetite, when such objects are available, for bottle caps, rubber balls and the like—the effects of which are not infrequently fatal—require large doses of vitamin B_1. Curiously, some infant sea lions are unable to swim until they are a couple of months old and require not only careful instruction from their mothers but also ceaseless surveillance lest they fall into the water and drown. A young Atlantic walrus named Herbert who came to lodge at the Bronx zoo in 1951, weighing 240 pounds, presented many feeding problems, both as to content and as to schedule. Finally a formula was improvised of undiluted evaporated milk, Mazola oil and cod-liver oil, to which was added, with the aid of a Waring blendor, comminuted mackerel. On this diet his weight was brought up from 240 to 958 pounds in 17 months, at which time he died from the effects of eating a rubber ball. Crandall also has in his repertory a fine collection of elephant stories, one of which concerns a fabulous female named Cutie, who, among other extraordinary feats, succeeded in traversing a half-moat (called a ha-ha) and jumping sideways over a wall—an accomplishment almost every student of elephants will assure you is quite impossible.

The Collected Papers of Lord Rutherford of Nelson: Volume III. Published under the scientific direction of Sir James Chadwick. John Wiley & Sons, Inc. ($15). The third volume of Rutherford's papers covers his period as Cavendish Professor of Experimental Physics at Cambridge from 1919 to 1937. During this time almost the entire emphasis of his researches and those of his group was on the nuclear constitution of the atom, the disintegration of elements by the emission of alpha particles and the transformation of nuclei by bombardment. The first major paper is a Bakerian Lecture on the nucleus delivered in 1920; this is followed by a series of communications reporting the growth of knowledge of atomic structures, which was achieved by the use of increasingly sophisticated instruments and experiments, which were in turn guided and interpreted by increasingly sophisticated theories. One starts with a vision of the atomic world ruled by the mechanics of Galileo and Newton, only to find that these rules will not reach, that they must be replaced by the subtle and complex systems of quantum and wave mechanics invented by Werner Heisenberg, Erwin Schrödinger, Max Born, P. A. M. Dirac and others. Throughout, however, Rutherford's reports of his techniques and his interpretations are conveyed in his characteristically clear, concrete and intuitive manner. Mathematical formalisms concerned him little, although he had no difficulty following the most abstract flights of his pupils and disciples. He understood a phenomenon when he could *see* it, or at least imagine it in terms of experience—tiny particles, elasticity, mechanical, magnetic and electric forces, pushes, repulsions, attractions, collisions. In this he resembled Michael Faraday or perhaps even Enrico Fermi, and his accounts are a model of graphic discourse and deft extrapolation. Looking back on his work, one finds oneself wishing that contemporary physicists could explain what they are doing and thinking as well as he could. It is for these qualities, as well as for his immense contributions so vividly described in these pages, that all physicists—students as well as seasoned practitioners—can learn much from, and delight in, this volume and its predecessors.

MATHEMATICS AND LOGIC IN HISTORY AND IN CONTEMPORARY THOUGHT, by Ettore Carruccio. Aldine Publishing Company ($8.75). EVOLUTION OF MATHEMATICAL THOUGHT, by Herbert Meschkowski. Holden-Day, Inc. ($5.95). Carruccio's book, a translation from the Italian, is both more than and less than a history of mathematics. The object is to show the development of certain fundamental concepts and the contribution of mathematical thought to the evolution of logic. A good deal of space is devoted to Greek mathematics, often interwoven with philosophy and dialectics; close attention is paid to the rise of modern infinitesimal analysis and to non-Euclidean geometries, and there is a substantial section on metamathematics and logic. Some of the material is clearly presented and comprehensible to almost any serious reader even if he has no special knowledge of the subject; the major part, however, is fairly heavy going and will make sense only to the mathematician. One is struck by disproportions of emphasis. Probability, for example, has only a shabby little corner and number theory even less than that. Altogether a curious book—idiosyncratic, parochial in its sources (the bulk of the references in a fairly extensive bibliography are to Italian authors) and by no means as helpful in establishing the connections between mathematics and other aspects of culture as the author promises. Yet certainly it is not devoid of either value or interest in fields such as logic, where the author's enthusiasm and fullness of presentation match his competence.

Much less ambitious and more successful is Meschkowski's *Evolution of Mathematical Thought*. It grew out of lectures intended for a wide circle of students and sought, as he says, to awaken in them some understanding of mathematical thinking in the framework of the liberal arts. It discusses succinctly and lucidly a variety of topics, most of them related to the foundations of mathematics and logic: infinity, Georg Cantor's creation of the theory of sets, antinomies and paradoxes, intuitionism, geometry and experience, formalism, mathematical logic and decision problems. Meschkowski handles intricate problems in a lively, good-humored way. He does not burden the reader with unnecessary complexities, but he says enough to excite his wonder, perhaps even to engross him to the point at which he is unlikely to drop the story until he can find out—in other books—how it ends. This little volume of 157 pages, although grievously overpriced, is recommended without reservation.

AESTHETICS AND TECHNOLOGY IN BUILDING, by Pier Luigi Nervi. Harvard University Press ($9.95). Nervi, one of the world's most distinguished architects, was invited to share the Charles Eliot Norton Lectureship at

Harvard University for the year 1961–1962. This book, heavily illustrated, is based on his lectures. The text, although relatively brief, imparts valuable information about Nervi's highly original methods and techniques, such as his use of precast and cast-in-place concrete, and the advantages of reinforced concrete inherent in its economy, technical fitness and beauty—particularly when Nervi invents the shapes. The photographs of structures embodying Nervi's designs are as enlightening as they are attractive. They include such stunning creations as the Flaminio Stadium in Rome, the UNESCO buildings in Paris, the Pirelli skyscraper in Milan, the Burgo paper mill in Mantua, the Port Authority bus terminal of the George Washington Bridge in New York, the Corsa Francia elevated road in Rome and the Nathaniel Leverone field house at Dartmouth College.

The Love of Anxiety, by Charles Frankel. Harper & Row Publishers ($4.50). A collection of graceful and frequently penetrating essays on a variety of topics: John Dewey's legacy, illusions of foreign policy, George Santayana, the anti-intellectualism of intellectuals and the love of anxiety, which is called a present-day philosophical preoccupation. Anxiety has always had its uses and can be most fruitful when it spurs men to question and analyze, but in its current widespread form, Frankel says, it leads to blank despair based on the conviction that the more terrifying problems of our day represent a kind of conspiracy against humanity that is not amenable to the ways of reason.

Structure and Function of the Epiphysis Cerebri, edited by J. Ariëns Kappers and J. P. Schade. American Elsevier Publishing Company, Inc. ($37.50). This 10th volume in the series "Progress in Brain Research" contains some 37 papers on the pineal gland, a remarkable part of the nervous system that was first described, probably by Herophilus of Alexandria, more than 2,000 years ago, continued to attract the interest of physicians, physiologists and even philosophers for more than 20 centuries and has in recent years drawn increasing scientific attention as important facts about its structure and function have come to light. An impressive, handsomely made book.

The Psychoanalytic Study of the Child: Volume XIX, edited by Ruth S. Eissler, Anna Freud, Heinz Hartmann and Marianne Kris. International Universities Press, Inc. ($10). Dedicated to Heinz Hartmann on his 70th birthday, this 19th volume of an established series contains, among other things, the first English translation of the introductory chapters of a book by Hartmann on concept formation in psychoanalysis (published in German in 1927), nine articles on aspects of normal and pathological development (including one by Dorothy Burlingham on the role of hearing in the development of the blind) and a number of clinical contributions touching on such topics as children's reactions to the assassination of President Kennedy.

Logical Positivism, edited by A. J. Ayer. The Free Press ($2.95). A paperback republication of a collection of papers by the founders of logical positivism, among them Moritz Schlick, Rudolf Carnap, Otto Neurath, Frank D. Ramsey, Bertrand Russell, Friedrich Waismann, Hans Hahn and the editor himself. One of the most valuable features of the book is an exhaustive 55-page bibliography.

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Use of Computers in Biology and Medicine, by Robert S. Ledley. McGraw-Hill Book Company ($29.50). A 966-page view of past, present and proposed uses of computers in biology and medicine.

South: Man and Nature in Antarctica, by Graham Billing and Guy Mannering. University of Washington Press ($15). A lavishly illustrated account, with supporting text of moderate length, of the New Zealand Antarctic research program. Some of the photographs are stunning.

Ethics, by G. E. Moore. Oxford University Press ($1.25). A soft-cover edition of one of the late G. E. Moore's major essays.

Paideia: The Ideals of Greek Culture, by Werner Jaeger. Oxford University Press ($2.50). The first volume of Jaeger's profound study dealing with ancient Greece and the mind of Athens is here reissued in soft covers.

Biological Rhythm Research, by A. Sollberger. American Elsevier Publishing Company, Inc. ($25). An account of rhythmic phenomena in the living world: biological clocks, developmental rhythms, population cycles, rhythms in psychopathology, photoperiodism, lunar and seasonal rhythms and so on.

Advances in the Study of Behaviour, edited by Daniel S. Lehrman, Robert A. Hinde and Evelyn Shaw. Academic Press Inc. ($9.50). Beginning a new series that views current problems with emphasis on the study of the behavior of animals.

Science in the Nineteenth Century, edited by René Taton. Basic Books, Inc., Publishers ($17.50). The third volume of *History of Science*, translated from the French, contains articles on mathematics, astronomy, the physical, geological and biological sciences, and science and society. It has both the strong and the weak points of its predecessors, already described in these columns.

Race, Language and Culture, by Franz Boas. The Free Press ($3.95). A reissue in soft covers of a collection of 62 papers by a preeminent anthropologist, first published in 1940.

The Foundations of Intuitionistic Mathematics, by Stephen Cole Kleene and Richard Eugene Vesley. North-Holland Publishing Company ($7). A monograph based on almost a quarter-century of research that relates intuitionistic mathematics, closely associated with the ideas of L. E. J. Brouwer and Arend Heyting, to the theory of recursive functions.

United States Government Organization Manual: 1965–66. Government Printing Office ($1.75). A new edition, revised to June 1, 1965, of this official publication that offers a comprehensive and reliable description of the organization and functions of the agencies in the legislative, judicial and executive branches of the United States Government.

Cosmic Ray Physics, by A. E. Sandström. John Wiley & Sons, Inc. ($15). A monograph, addressed to specialists, on the present stage of development in cosmic ray physics.

Bibliography of Statistical Literature: 1940–1949, by Maurice G. Kendall and Alison G. Doig. Hafner Publishing Co., Inc. ($10.50). The second of three projected volumes covering the literature of statistics and probability from the 16th century, containing about 6,000 references to articles published in the years from 1940 through 1949.

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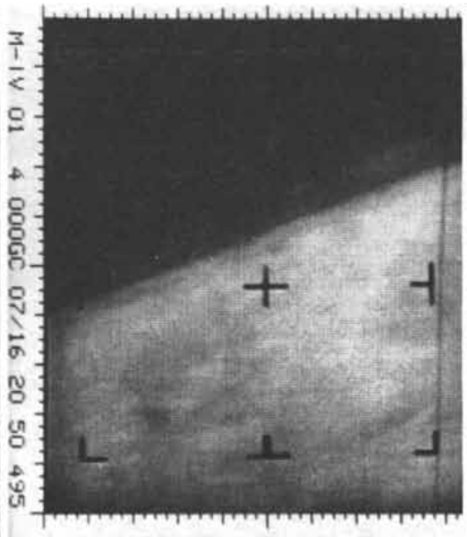

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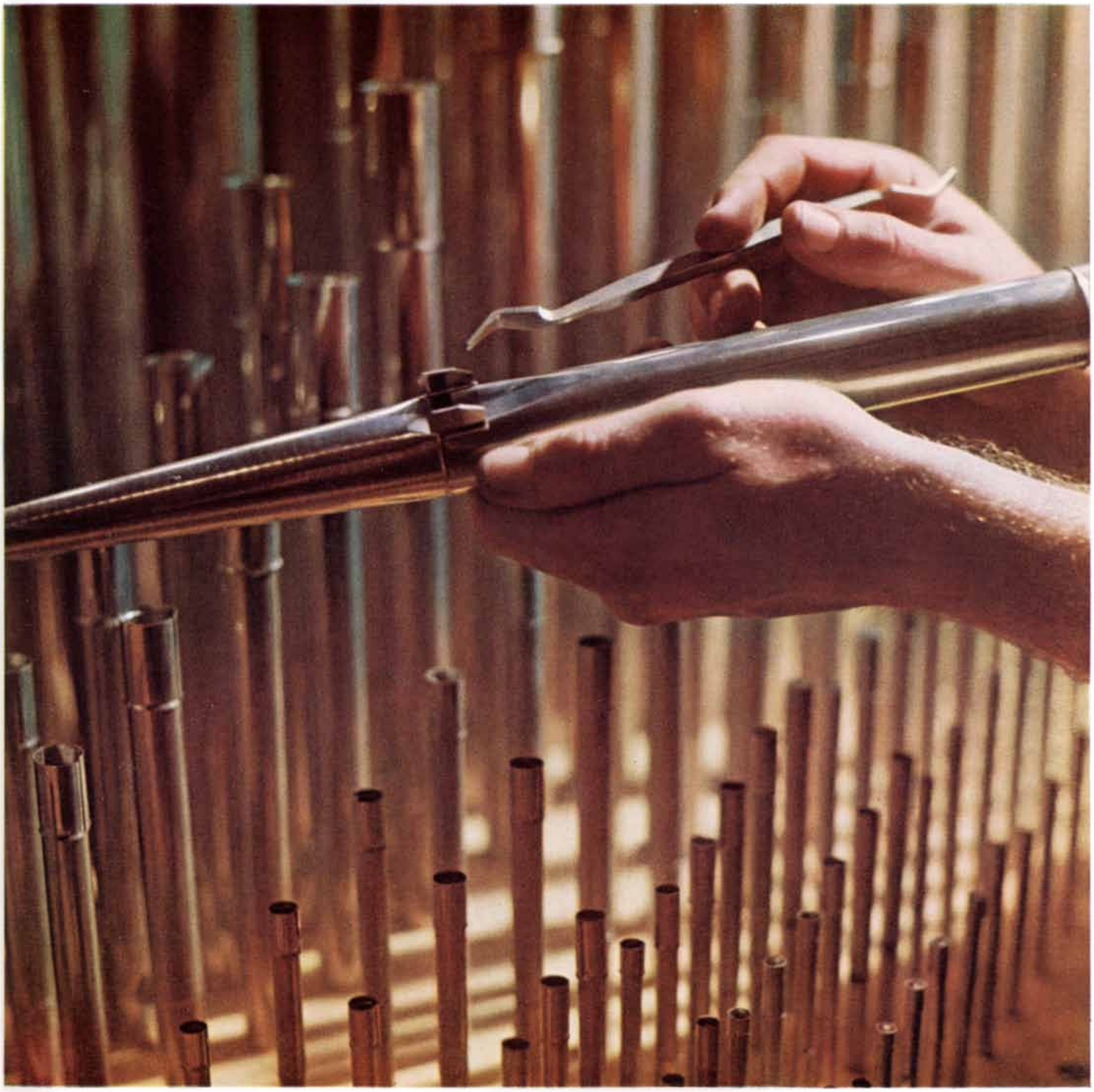

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