SCIENTIFIC AMERICAN

CHERRY HARVESTER

SIXTY CENTS

August 1967

Almost everybody knows Rolls-Royce builds an inimitable motor car.

But did you know that one-fifth of all the commercial turbine-powered aircraft in the U. S. use Rolls-Royce engines?

That Rolls-Royce diesel-powered railcars are helping move commuters in and out of large cities around the world?

That Rolls-Royce builds the world's only military engine in production that can run on kerosene, jet fuel, diesel oil or gasoline?

Now read how Rolls-Royce is powering a new kind of ship—a hydrofoil that will "fly" passengers over the waves in speed and comfort never before possible.

Suppose a 75-foot ship could cruise at 60 mph, carry 88 passengers, go more than 230 miles on a tank of fuel—and all of it with a smooth ride even in *rough water.*

Think of the things such a boat could do. Open up new waterways. Turn rivers into commuting routes. Service off shore oil wells. Make fast delivery of high-priority cargo. Even serve as sight-seeing boats for excursion trips.

Does all this sound like a whale of a tale? It was Grumman's idea. The Grumman Aircraft Corporation of Bethpage, Long Island—a company long known for its innovations in the aviation business.

They wanted to build a hydrofoil boat. Not just any hydrofoil. But a *rough water* hydrofoil.

Hydrofoil boats ride *over* the water—rather than *through* it— by means of small metal fins called foils. The foils pass through the water, getting lift from it in the same way an airplane wing presses against the air. As the ship picks up speed, the foils lift the hull clear out of the water where it flies along free of wave disturbance.

Grumman put the foils beneath the waves and added a sensing device to detect and adjust the foils to changing wave patterns. Result: The hydrofoil could fly in waves up to six feet without so much as knocking a teacup off a passenger's knee.

Next came the engine. Grumman had already used Rolls-Royce Dart engines for their Gulfstream I—one of the most successful twin-engine corporate aircraft ever built. And they had picked the Rolls-Royce Spey engine for Gulfstream II—a jet version of Gulfstream I.

Grumman now looked at a third Rolls-Royce engine. The Tyne.

The Tyne is the most advanced turboprop made. And has a remarkably low fuel consumption.

The Tyne was right.

Right now, this British-powered hydrofoil is being built to Grumman's design in Germany. The first one to go into service has been bought by a Spanish shipping concern for the Barcelona to Majorca run.

Next time you're in Barcelona, you might want to hop over to Majorca in a Rolls-Royce.

Rolls-Royce Limited,
Derby, England

Look who's driving a Rolls-Royce.

Rolls-Royce Tyne engine powers this new hydrofoil designed by Grumman.

Established 1845 **SCIENTIFIC AMERICAN** August 1967 Volume 217 Number 2

ARTICLES

DEPARTMENTS

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THE COVER

The painting on the cover shows a portion of a machine that harvests cherries (see "Mechanical Harvesting," page 50). The view is from the top of the tree toward the ground. A mechanical arm grips a branch of the tree and shakes it gently, causing the cherries to fall into the white fabric collector at the base of the trunk. The collector is sloped so that the cherries roll onto a conveyor, which is covered to keep cherries coming out of the tree from hitting cherries already in the conveyor. This precaution is designed to prevent the fruit from being bruised. The cover of the conveyor is the dark vertical band near the center of the painting. The machine in the painting is made by the Friday Tractor Company of Hartford, Mich.

THE ILLUSTRATIONS

Cover painting by Philip Hays

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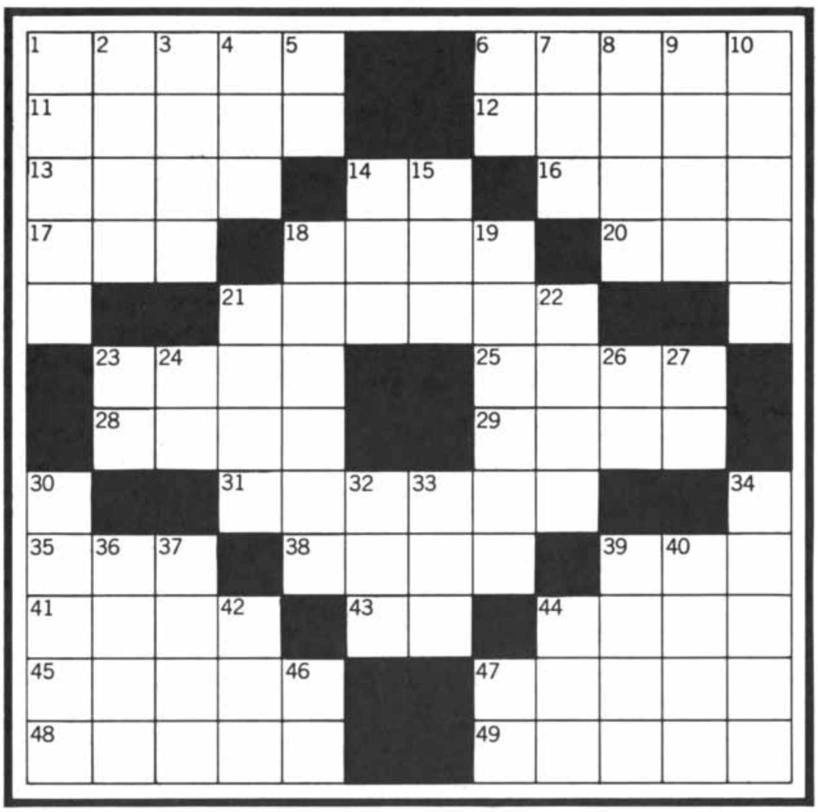
Kill some time while waiting in line at No.1. Compliments of Avis.



Or rent a shiny new Plymouth from Avis. (The line at our counter is shorter.)

ACROSS

1. Rabbit or knockout
6. Florida city
11. Hollywood statue
12. Command
13. Man's first name, Ponce's last name
14. The spirit of____.
16. Kiss Me____.
17. Printed persuaders
18. Couples
20. Non-women
21. Railway stations
23. Sherlock Holmes' Baker St. address
25. Girl's name
28. How many Arabian nights?
29. Metal
31. Bends over
35. A limb
38. Hurt
39. Female deer
41. To judge
43. LXX
44. The Jones and the Sawyer boy
45. Mr. Stevenson
47. A flat cap for men or women
48. Cowboy circus
49. Baked, lima, or jelly____.

DOWN

1. White bear
2. Second-hand
3. Sergeants
4. Tin container
5. Sixty minutes (Abbr.)
6. U. S. State (Abbr.)
7. Annoy
8. First man
9. To allot
10. Girl's name
14. Soft drink
15. Into the valley of death rode the____.
18. Entries of debt
19. Privates have one
21. God (Spanish)
22. Gentlemen
23. Voting age
24. XX
26. Preposition
27. In grammar, an article
30. Electronic eye
32. Killer's license number
33. Gold (Spanish)
34. Lies down
36. Do over
37. Canasta term
39. The dumb girl
40. A portent
42. Girl's name
44. Golf term
46. Downing St. address
47. Ammunition for toy gun

LETTERS

Sirs:

I have read the fascinating article by Ruth Hubbard and Allen Kropf entitled "Molecular Isomers in Vision" [SCIENTIFIC AMERICAN, June]. Since I share the authors' admiration for J. H. van't Hoff, and since van't Hoff occupies a prominent position in this article, I shall confine myself to pointing out some inaccuracies concerning him and his *La chimie dans l'espace.*

The assumption of three-dimensional arrangements of molecules and the atoms in them was not "a speculative leap of great audacity" by van't Hoff. "Three-dimensional" thinking was familiar to Gmelin (1848), Kekulé, Butlerov and Paternó, Rosenstiehl, Wislicenus and Pasteur before Le Bel and van't Hoff. Wislicenus stated in 1869: "If molecules can be structurally identical and yet possess dissimilar properties, then this difference is explainable only by the assumption that it is due to a different arrangement of the atoms in space." However, the concept of a *regular* tetrahedron, which is not stressed by the authors, and especially the consequent elaboration of the possibilities for the stereochemistry of the carbon atom ensuing from it, is largely his own contribution. (Aleksandr Mikhailovich Butlerov envisaged in 1862 the carbon atom as "a tetrahedron, each of the four surfaces of which is capable of binding an atom of hydrogen," and the former student of architecture Friedrich August Kekulé similarly postulated in 1867 that "the four valency units of carbon . . . terminate in tetrahedral planes.") The development by van't Hoff of the constitutional and configurational formula was not the result of a desire to speculate but necessitated by facts, namely the existence of a larger number of isomers than can be shown by structural formulas. . . .

His original and essential contribution to the chemistry in space ("Voorstel tot Uitbreiding . . . der Structuur-Formules in de Ruimte . . . ," 1874) was an 11-page brochure in Dutch. Van't Hoff made this contribution as a student before enrolling for his doctoral thesis and while not employed anywhere. In fact, it was not easy for him to find employment. His subsequent applications for the position of high school teacher were rejected because of his "sloppy appearance and brooding."

It is certainly correct that van't Hoff was the first recipient of the Nobel prize in chemistry. He was awarded it over the following additional leading candidates: Henri Moissan, William Ramsay, Lord Rayleigh, Adolf von Baeyer, Emil Fischer, Wilhelm Ostwald and Svante Arrhenius, but this was on December 10, 1901, and not in 1900. Also, he received the Nobel prize for his "discovery of the laws of chemical dynamics and the osmotic pressure in solutions" rather than for his contributions to stereochemistry, as the authors imply. . . .

GEORG F. SPRINGER, M.D.

Evanston Hospital
Northwestern University
Evanston, Ill.

Sirs:

We should like to thank Dr. Springer for amplifying our brief summary of the early history of structural organic chemistry. We might only add that it does not detract from van't Hoff's reputation to recognize that great scientific theories—"speculative leaps of great audacity"—are often in the air at the time of their formulation. Nevertheless, special recognition usually goes to the man who arranges the facts and first states the ideas boldly and succinctly. This was true of Einstein's special theory of relativity, as well as of some of the insights that initiated the current series of advances in the molecular biology of the gene. The "innovators" are usually aware of their debt to the past and to their contemporaries. We might recall Newton's remark, "If I have seen a little farther than others, it is because I have stood on the shoulders of giants."

RUTH HUBBARD

The Biological Laboratories
Harvard University
Cambridge, Mass.

ALLEN KROPF

Amherst College
Amherst, Mass.

Sirs:

The article by Harald Esch, "The Evolution of Bee Language" [SCIENTIFIC AMERICAN, April], requires some comment with respect to some of our research during the past three years. As a result of the "answers" furnished by bees in the course of a series of experiments we have been forced to abandon the notion that bees have a "language" as discussed by Dr. Esch and his predecessors. This also amounts to a complete reversal of opinion on our part from that which was contained in an earlier article in *Scientific American* [see "Sound Communication in Honeybees," April, 1964]. The evidence on which we base our present stand requires considerably more space than could be allowed in a letter to *Scientific American*. Those interested in these recent developments should refer to the following publications (listed in the order in which they were done):

1. Sound Production during the Waggle Dance of the Honey Bee. A. M. Wenner in *Animal Behaviour,* Vol. 10, pages 79–95; 1962.
2. An Analysis of the Waggle Dance and Recruitment in Honey Bees. A. M. Wenner, P. H. Wells and F. J. Rohlf in *Physiological Zoology,* in press.
3. Simple Conditioning in Honey Bees. A. M. Wenner and D. L. Johnson in *Animal Behaviour,* Vol. 14, pages 149–155; 1967.
4. A Relationship between Conditioning and Communication in Honey Bees. D. L. Johnson and A. M. Wenner in *Animal Behaviour,* Vol. 14, pages 261–266; 1966.
5. Communication between Honey Bees Which Have Had Field Experience. D. L. Johnson in *Animal Behaviour,* in press.
6. Honey Bees: Do They Use the Direction Information Contained in Their Dance Maneuver? D. L. Johnson in *Sci-*

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ence, Vol. 155, pages 844–847; Feb. 17, 1967.

7. Honey Bees: Do They Use the Distance Information Contained in Their Dance Maneuver? A. M. Wenner in *Science*, Vol. 155, pages 847–849; Feb. 17, 1967.

ADRIAN M. WENNER

DENNIS L. JOHNSON

University of California, Santa Barbara
Santa Barbara, Calif.

PATRICK H. WELLS

Occidental College
Los Angeles, Calif.

Sirs:

Dr. Wenner's statement that he and his co-workers "have been forced to abandon the notion that bees have a 'language' as discussed by Dr. Esch and his predecessors" is based on experiments that only prove that bees use the more primitive method of finding food sources by their specific scents if they have a chance to do so. But this fact was already recognized by Karl von Frisch soon after his discovery of the "bee language." In the crucial experiments, therefore, von Frisch tried to exclude this source of errors carefully. The bees, being forced to use communication by dances, showed that they were able to do so. Dr. Wenner cited some of von Frisch's publications concerned with this problem in his work but evidently did not read them carefully. If he had repeated von Frisch's deliberations and completed all necessary experiments, he would have regained his belief in "a 'language' [of bees] as discussed by Dr. Esch and his predecessors."

HARALD ESCH

Department of Biology
University of Notre Dame
Notre Dame, Ind.

ERRATUM

In the article "Butterflies and Plants" (SCIENTIFIC AMERICAN, June) the source of the illustration on pages 110 and 111 was not given. The illustration is based on the work of Verne Grant of the Rancho Santa Ana Botanic Garden in Claremont, Calif.

50 AND 100 YEARS AGO

SCIENTIFIC AMERICAN

AUGUST, 1917: "On the Western front what is officially admitted as the great artillery battle of the war has been taking place in Flanders in the last day or two; it follows two weeks of almost unprecedented bombardment and has been termed 'the highest degree of massed effect in the war.' So great has been the artillery fire that it has been heard in London. Numerous raids and tentative thrusts all along the Western front are highly significant and show a nervous tension in the lines."

"Solving the problem of tele-vision has long been one of the outstanding dreams of the inventor. What the telephone is to the ear the tele-vision apparatus would be to the eye—a means of bridging space and receiving sense impressions from practically unlimited distances. While we are as yet far removed from the solution to this problem, another task of similar nature has been solved in late years. Telegraphic transmission of photographs, handwriting, drawings, etc., has reached a point where, until war interrupted international traffic and took over control of the world's telegraph and telephone lines, some of the foremost European papers made a feature of the publication of photographs received by wire."

"We carry on another page of this issue a communication from a correspondent whose position in the technical world makes it necessary to take his remarks seriously. This gentleman criticizes the proposed Hudson River vehicular tunnel on the ground that adequate evacuation of the carbon monoxide generated by the operation of automobiles through it would be out of the question. In reaching this conclusion he is at direct variance with the engineers engaged by the Public Service Corporation to report upon the ventilation question. We have checked his arguments and figures carefully and find in them no error. The findings of our correspondent can only mean that these engineers have attempted to consider the subject of monoxide pollution with unbelievable cynicism or incredible ignorance of the working of this king among poisons."

"Since Professor Bragg lectured before the Institute of Metals on X rays and crystal structure a year ago further striking developments have become known in X-ray studies, not only of crystals but also of amorphous substances, liquid crystals, liquids and gases. The dimensions of the hydrogen molecule and of the benzene ring have been deduced, and it seems possible to determine the number and grouping of the electrons in the atom."

"The chairman of the National Research Council, Dr. George E. Hale, has addressed a letter to the several state councils of defense recommending the formation, under those bodies, of state committees of research. These committees would undertake, according to local needs and circumstances, the development or investigation of natural resources, the promotion of research methods in industries, the correlation of industries with research agencies already existing, etc. Dr. Hale urges that such committees be organized for permanent service to the community and not merely for emergency service during the war. The general expenses should be provided by the several states, and the committees should be representative of the leading educational and research institutions as well as of industries in which science plays an important part."

"The mysterious behavior of the so-called Cepheid variables—variable stars of the type of Delta Cephei—is at present one of the leading topics of discussion in the astronomical journals. These stars increase very rapidly in brightness from minimum to maximum and then decline much more slowly and irregularly. Most remarkable is the fact that these changes in brightness are accompanied by changes in spectral type. The changes in spectrum do not, however, proceed at the same rate as the changes in brightness. These curious phenomena open up new possibilities in celestial physics."

SCIENTIFIC AMERICAN

AUGUST, 1867: "Reports of gold deposits in our new Russian possessions are still coming in. The latest is contained in a letter to Secretary Seward from Mr. Berry of Oregon, who relates that a party of prospectors found in the Stickteen River, 300 miles from its mouth, gold and silver deposits of great wealth, also rubies and agates, and on the Bristol River, copper and coal indications."

"In M. de Louvrié's system of æronautics, which the Academy of Sciences have seen fit to disapprove, the recoil caused by a sudden expansion of gases, as in the sky rocket, seems to have been made use of as a motor. This inventor provides a hollow cylinder which contains an explosive compound generated by the mixture of air with a highly inflammable gas formed from some volatile hydro-carbon, such as benzine or petroleum. The combined gases are lighted as they escape from a small orifice at the lower end of the cylinder, and the resistance at the closed end from this explosion causes the ascent. Of these explosions there are from 30 to 40 per minute."

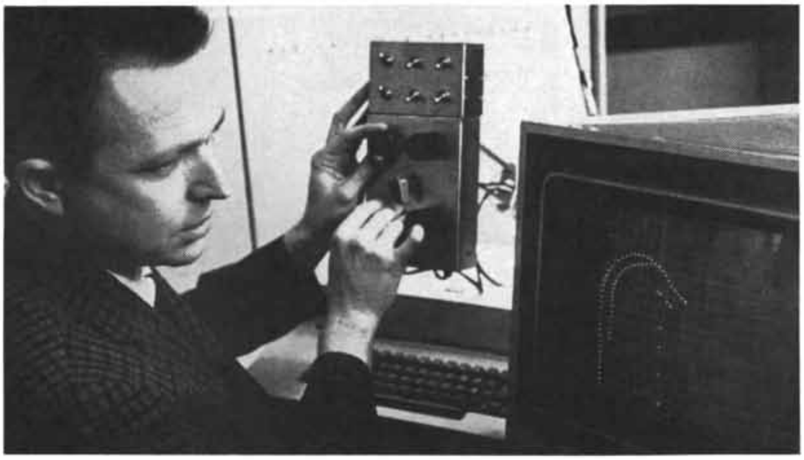
"The University of Oxford has made an appropriation of the sum of $2,500 for the purpose of equipping an expedition for scientific investigation in Palestine. Cambridge University will probably contribute an equal amount."

"At the Salmon Fishery Congress recently held in Kensington, England, the secretary of the River Dee Fishery Board testified that since the establishment of a petroleum refinery on the banks of that river every fish in the entire length of the stream, from salmon of 20 pounds downward, has been killed by a poisonous refuse matter which floats out from the refinery. The water supply for the town of Chester had been drawn from this river, but a skillful analyst has examined the fluid and declares that no filtration can purify water polluted by a poison so subtle and powerful as this."

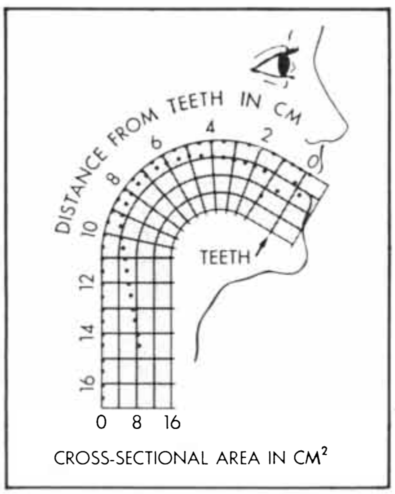
"Prof. Safford, who is in charge of the famous Clark telescope mounted in the Dearborn Observatory at Chicago, in reply to an English astronomer's inquiries as to what had been done with this great telescope, writes that he has discovered about 70 new nebulæ, mostly small, with distinct nuclei; of these one is triple, three in a row; one a large, rather diffuse nebula of singular shape in Perseus; another is an irregular ring, known previously as a nebula, not as a ring. The nebula of Orion astonished him by its brightness and distinctness, and he has discovered a branch preceding the main nebula which has a roundish opening in its center. He is preparing to publish a report of his observations in full."

Report from
BELL LABORATORIES

See a computer talk

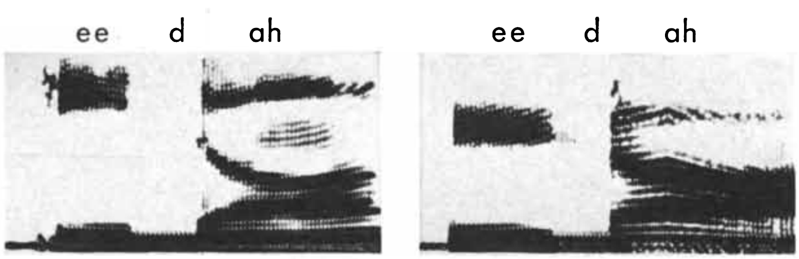


C. H. Coker adjusts controls which change the outline of the "vocal tract" simulated on the oscilloscope. At the same time, he hears the sound corresponding to the displayed shape. Desired vocal-tract shapes (representing sounds) can be stored in the computer memory.



Bell Laboratories' computerized vocal-tract model. (Head outline added.) The various parts can be positioned to imitate any speech sound. The model displays tract length versus cross-sectional area. It is based on anatomical measurements of the vocal tract made by a number of acousticians.

A feature of the model is that it reproduces the transition sounds between word fragments. The nonsense word eedah, for example, consists of ee plus d plus ah. But the d is not the same as in, say, eedee. That is, the d is noticeably affected by context. Coker handles this by storing dynamic properties of the vocal articulators (the tongue, lips and jaw). The program automatically incorporates these properties in assembling word fragments.



Comparison of nonsense word "eedah," pronounced by a human (left) and by Coker's program. These speech spectrographic patterns represent time (horizontal scale), frequency (vertical), and intensity (line density). The dark bars are called "formants" and are characteristic of speech sounds. The technique for making these diagrams was conceived and developed in the early 40's at Bell Telephone Laboratories.

Speech, one of the most complex of human activities, is studied as part of the continuing communications research at Bell Telephone Laboratories. But the speech mechanism has always been difficult to analyze: vocal-tract movements—crucial to the formation of meaningful acoustic signals—are mostly obscured from sight and are not easily measured. Now our understanding of speech is being advanced through a computerized simulation of the vocal tract devised by Cecil H. Coker of Bell Laboratories and Osamu Fujimura of the University of Tokyo, who worked at Bell Labs as a consultant.

The model (displayed on an oscilloscope, left) resembles the actual vocal tract and shows its principal parts. The parts can be moved either automatically by the computer program or by manual controls on the computer panel. The program calculates speech data corresponding to the displayed vocal-tract shape and delivers these data to an electronic speech synthesizer, designed by Coker. The synthesizer then generates a sound corresponding to the tract shape. Hence the researcher can hear the synthetic output at the same time he sees the tract motion.

The model accurately reproduces not only individual speech sounds but, for the first time, the subtle transitions that connect these sounds. It also demonstrates that these transitions are vital to clarity and realism.

The system produces patterns of frequency and energy (spectrograms) very like a human's (left). And it passes a more difficult test: pronouncing speech sounds which are understandable even when taken out of context.

Bell Telephone Laboratories
Research and Development Unit of the Bell System

THE AUTHORS

WILLIAM P. LOWRY ("The Climate of Cities") is assistant professor of biometeorology at Oregon State University. With a bachelor's degree in mathematics, which he obtained at the University of Cincinnati in 1950, he went to the University of Wisconsin, where he received a master's degree in meteorology in 1955, and to Oregon State University, where in 1962 he was awarded an interdisciplinary doctorate for work done in meteorology, statistics and the physiology and ecology of plants. Lowry was a physicist with the Snow, Ice and Permafrost Research Establishment of the U.S. Army Corps of Engineers from 1951 to 1953, a research associate in meteorology at the University of Wisconsin from 1953 to 1955, a research meteorologist with the Oregon State Board of Forestry from 1955 to 1957 and a research meteorologist with the Oregon Forest Research Center from 1957 to 1961. His first assignment at Oregon State University was as assistant professor of forest meteorology.

MICHAEL S. GAZZANIGA ("The Split Brain in Man") is assistant professor of psychology at the University of California at Santa Barbara. After his graduation from Dartmouth College in 1961 he did graduate work in psychobiology at the California Institute of Technology, receiving a doctorate there in 1964. He remained at Cal Tech as a research fellow until 1966 and then was a research fellow for several months at the University of Pisa. His research activities at Cal Tech and Santa Barbara have been supported by the U.S. Public Health Service; they entail some work with human beings and also a number of physiological and neuropsychological studies on monkeys.

GEORGE H. HERBIG ("The Youngest Stars") is an astronomer at the Lick Observatory, where he has been a member of the staff since 1949. Herbig was graduated from the University of California at Los Angeles in 1943 and received a Ph.D. from the University of California at Berkeley in 1948. He is a member of the International Astronomical Union and has served since 1964 as president of its Commission on Variable Stars. He is also a member of the National Academy of Sciences.

CLARENCE F. KELLY ("Mechanical Harvesting") is director of the University of California's Agricultural Experiment Station at Berkeley. Having received bachelor's and master's degrees in agricultural engineering from North Dakota State University, he went to work for the U.S. Department of Agriculture in 1935 and remained for 15 years, except for three years of naval service during World War II. From 1950 to 1963 he was a member of the agricultural engineering staff of the University of California at Davis, ending as professor and chairman of the department. He became associate director of the university's agricultural experiment station, which is an institution of statewide scope, in 1963 and director in 1965.

FREDERICK A. FUHRMAN ("Tetrodotoxin") is professor of experimental medicine and director of the Max C. Fleischmann Laboratories of the Medical Sciences at Stanford University. The laboratories are the multidiscipline teaching laboratories of the university's School of Medicine. Fuhrman is a physiologist, having received a Ph.D. in physiology from Stanford in 1943 after study at Oregon State University, the University of Freiburg and the University of Washington. He began his work on the Stanford faculty in 1945 as an instructor in physiology. His research activities have dealt with the effects of drugs and toxins on the metabolism of tissues; the effect of temperature on metabolism; the pathophysiology of frostbite, and the mechanism of active transport of sodium and the effect of drugs and toxins on the process. Fuhrman's present research, in which he has the collaboration of his wife, Geraldine Jackson Fuhrman, who is also a physiologist, is on the effects of various toxins from poisonous animals.

ADOLF SEILACHER ("Fossil Behavior") is professor at the University of Tübingen. He was a student at that university from 1945 to 1951, when he obtained a Ph.D. From 1951 to 1957 he was an assistant at the university. He was successively docent at the University of Frankfurt am Main, assistant professor at the University of Baghdad, professor at the University of Göttingen and visiting lecturer at Johns Hopkins University before taking his present position. Seilacher's research in paleontology has taken him to India, El Salvador, Jordan, Lebanon, Libya, Iraq and several states in the U.S.

BERNARD BERTMAN and ROBERT A. GUYER ("Solid Helium") are respectively a member of the staff at the Brookhaven National Laboratory and assistant professor of physics at Duke University. Bertman, who received a bachelor's degree in physics from the University of California at Los Angeles in 1959, went to Yale University for graduate work, studying biophysics for a year and obtaining a master's degree in physics in 1961. He then went to Duke, where he received a Ph.D. in physics in 1965. In September he will become assistant professor of physics at Wesleyan University. He writes that in addition to work on liquid and solid helium he has studied superconductivity and other problems in solid-state physics and is now "working on an apparatus capable of experimentation at ultralow temperatures." He hopes the apparatus will function in the range of .001 degree Kelvin, or .001 degree centigrade above absolute zero. Guyer was graduated from New Mexico State University in 1959 and received a Ph.D. from Cornell University in 1966. He has been at Duke since 1964. He writes: "Although my primary interests are in theoretical topics (transport properties of dielectric solids, ground-state properties of solid helium), I continue to be very closely associated with the experimentalists."

MARIE BOAS HALL ("Robert Boyle") is reader in the history of science at the Imperial College of Science and Technology of the University of London. Born in Massachusetts, she obtained bachelor's and master's degrees in chemistry at Radcliffe College and a Ph.D. from Cornell University. Her first work was as a technical writer with the U.S. Army Signal Corps from 1942 to 1944; she also served as a technical writer at the Radiation Laboratories of the Massachusetts Institute of Technology from 1944 to 1946. Among the institutions where she has taught the history of science are the University of Massachusetts, Indiana University and the University of California at Los Angeles. She is the author of *Robert Boyle and 17th-Century Chemistry*, published in 1958, *Robert Boyle on Natural Philosophy*, published in 1965, and other books and articles dealing mainly with 17th-century science. With her husband, A. Rupert Hall, a professor at Imperial College, she is now editing *Correspondence of Henry Oldenburg*, three volumes of which have been published to date.

KENT V. FLANNERY, who in this issue reviews *An Introduction to American Archaeology*, by Gordon R. Willey, is associate curator of archaeology at the Smithsonian Institution.

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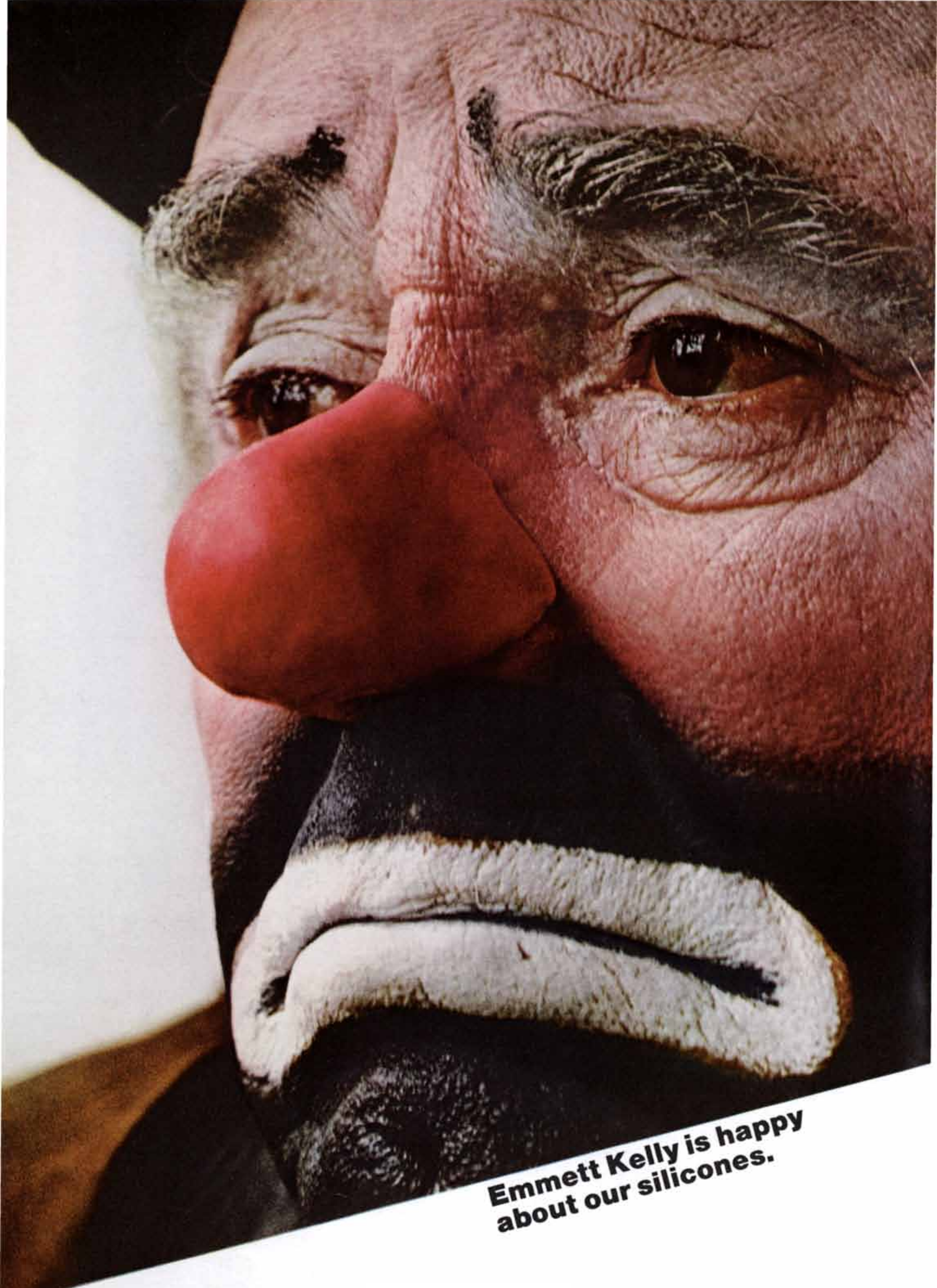
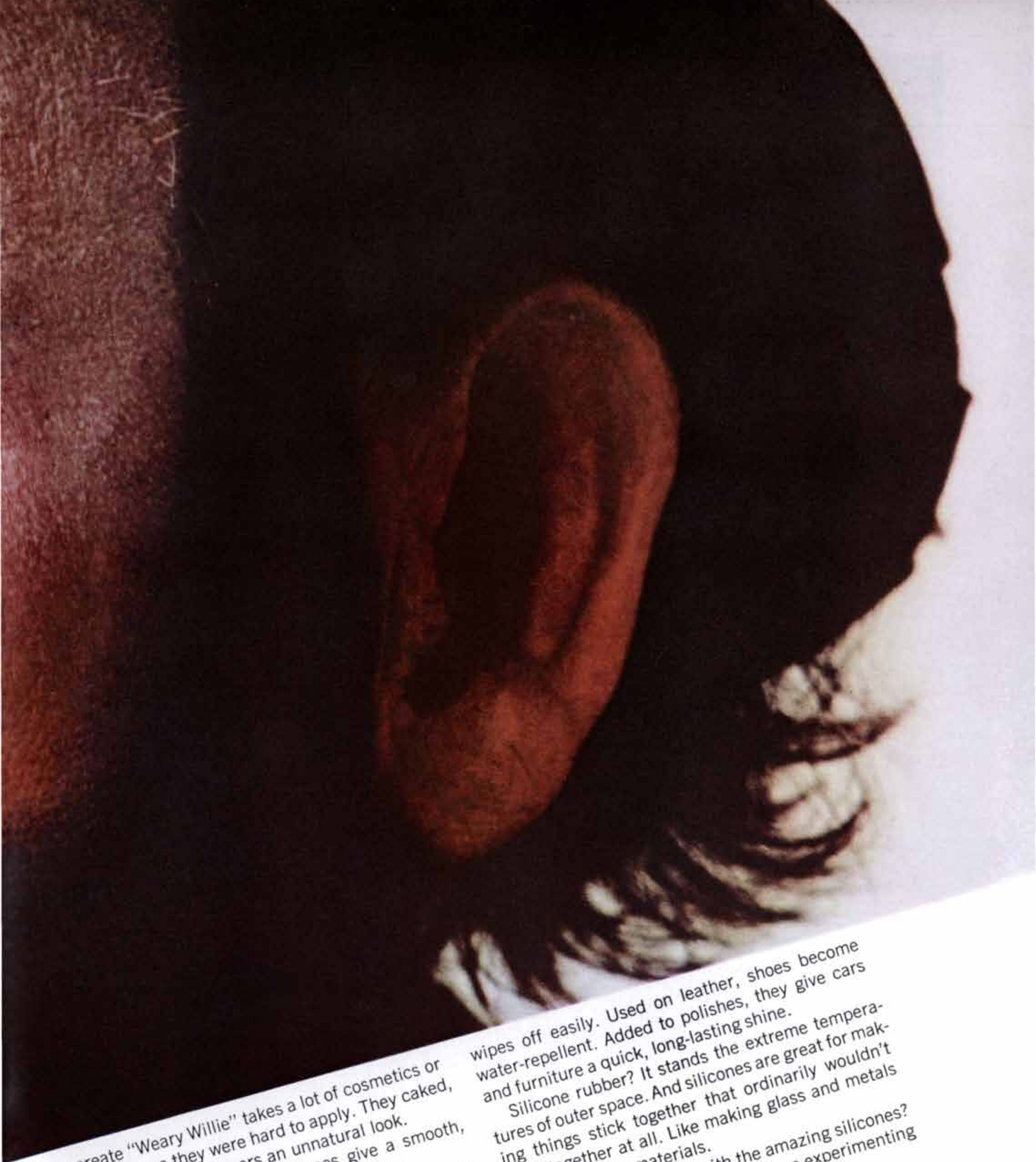

Emmett Kelly is happy
about our silicones.



To create "Weary Willie" takes a lot of cosmetics or makeup. Once they were hard to apply. They caked, crinkled, gave performers an unnatural look.

Today, cosmetics with silicones give a smooth, natural look. Easy to apply, easy to remove.

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Examples: add silicones to paints and they become easier to apply; cover surfaces evenly, uniformly. Put them in spray-on starch and irons won't stick to cloth. Coat ovens with them and grease wipes off easily. Used on leather, shoes become water-repellent. Added to polishes, they give cars and furniture a quick, long-lasting shine.

Silicone rubber? It stands the extreme temperatures of outer space. And silicones are great for making things stick together that ordinarily wouldn't stick together at all. Like making glass and metals adhere to many materials.

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RADIATION-INDUCED POLYMERIZATION

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.

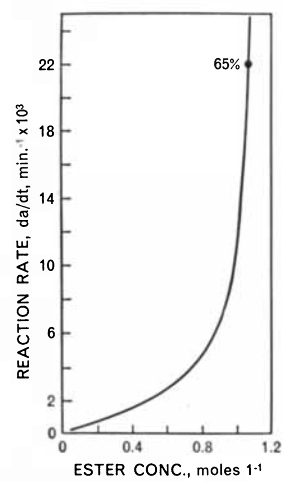

Figure 1. This graph shows the rate of polymerization as a function of ester concentration; commercially useful systems are in the 65% ester range [Burlant and Hinsch, J. Polymer Science, 2, 2135 (1964)].

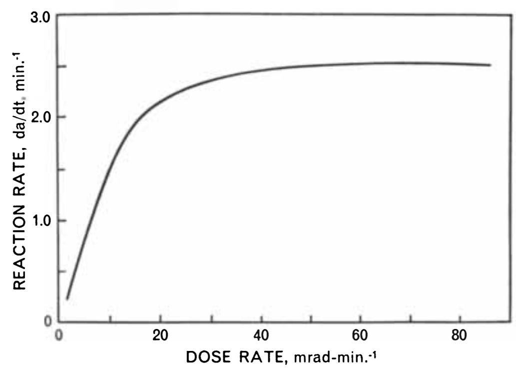

Figure 2. This graph shows the rate of polymerization as a function of radiation intensity for 65% ester solutions; optimum rate occurs with an electron beam intensity of 40 MRAD-min-1 [Burlant and Hinsch, J. Polymer Science, 3, 3587 (1965)].

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The Climate of Cities

The variables of climate are profoundly affected by the physical characteristics and human activities of a city. Knowledge of such effects may make it possible to predict and even to control them

by William P. Lowry

It is widely recognized that cities tend to be warmer than the surrounding countryside, and one is reminded almost daily by weather forecasts such as "low tonight 75 in the city and 65 to 70 in the suburbs." Exactly what accounts for the difference? Meteorological studies designed to answer such questions have now been made in a number of cities. Much work remains to be done, but one thing is clear. Cities differ from the countryside not only in their temperature but also in all other aspects of climate.

By climate is meant the net result of several interacting variables, including temperature, the amount of water vapor in the air, the speed of the wind, the amount of solar radiation and the amount of precipitation. The fact that the variables do not usually change in the same way in a city as they do in the open country nearby can often be measured directly in differences of temperature, humidity, precipitation, fog and wind speed between a city and its environs. It is also apparent in such urban phenomena as persistent smog, the earlier blooming of flowering plants and longer periods free of frost.

The city itself is the cause of these differences. Its compact mass of buildings and pavement obviously constitutes a profound alteration of the natural landscape, and the activities of its inhabitants are a considerable source of heat. Together these factors account for five basic influences that set a city's climate apart from that of the surrounding area.

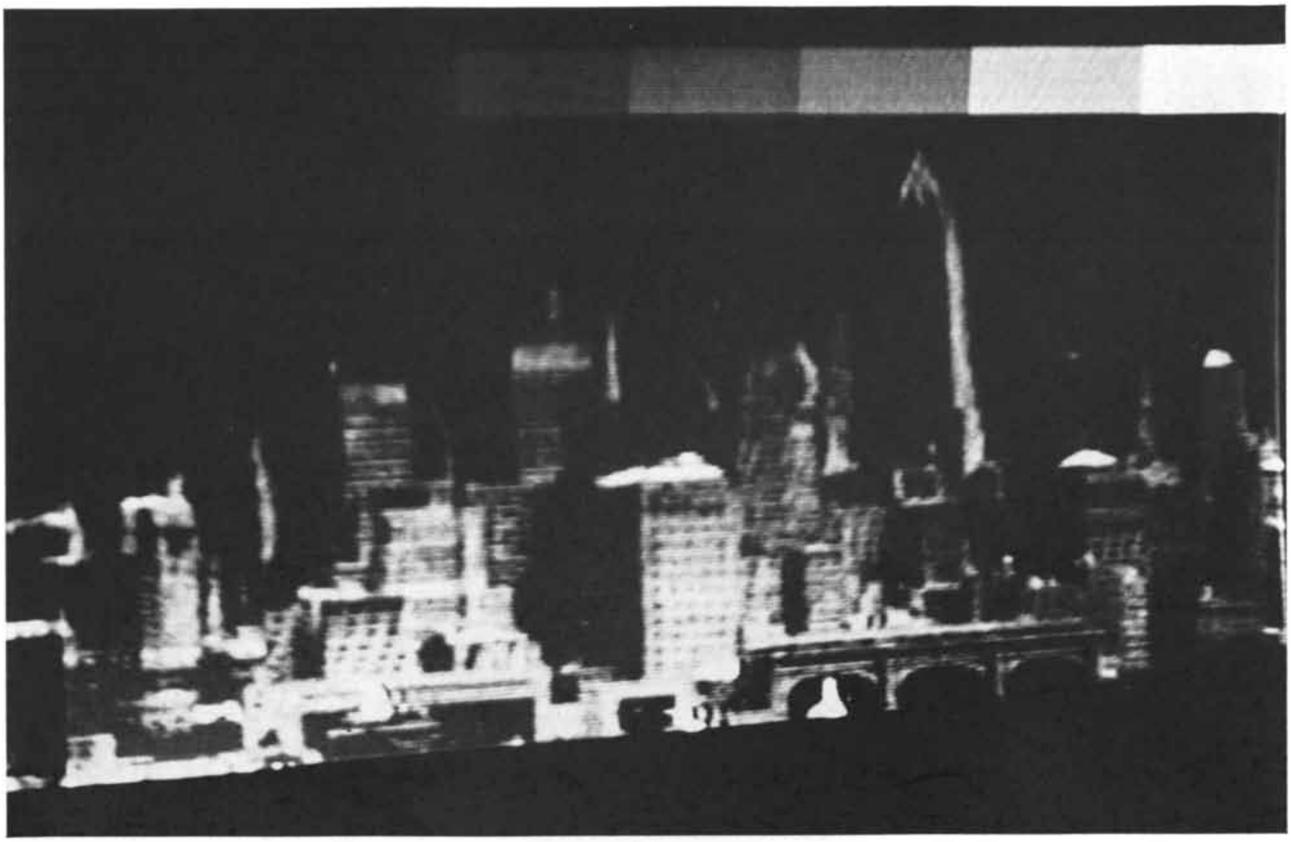
The first influence is the difference between surface materials in the city and in the countryside. The predominantly rocklike materials of the city's buildings and streets can conduct heat about three times as fast as it is conducted by wet, sandy soil. This means that the city's materials can accept more heat energy in less time, even though it takes roughly a third more energy to heat a given amount of rock, brick or concrete to a certain temperature than to heat an equal amount of soil. The temperature of soil at the warmest time of the day may be higher than that of a south-facing rock wall, but the temperature three or four inches below the surface will probably be higher in the wall. At the end of a day the rocky material will have stored more heat than an equal volume of soil.

Second, the city's structures have a far greater variety of shapes and orientations than the features of the natural landscape. The walls, roofs and streets of a city function like a maze of reflectors, absorbing some of the energy they receive and directing much of the rest to other absorbing surfaces [*see top illustration on page 17*]. In this way almost the entire surface of a city is used for accepting and storing heat, whereas in a wooded or open area the heat tends to be stored in the upper parts of plants. Since air is heated almost entirely by contact with warmer surfaces rather than by direct radiation, a city provides a highly efficient system for using sunlight to heat large volumes of air. In addition, the city's many structures have a braking effect on the wind, thereby increasing its turbulence and reducing the amount of heat it carries away.

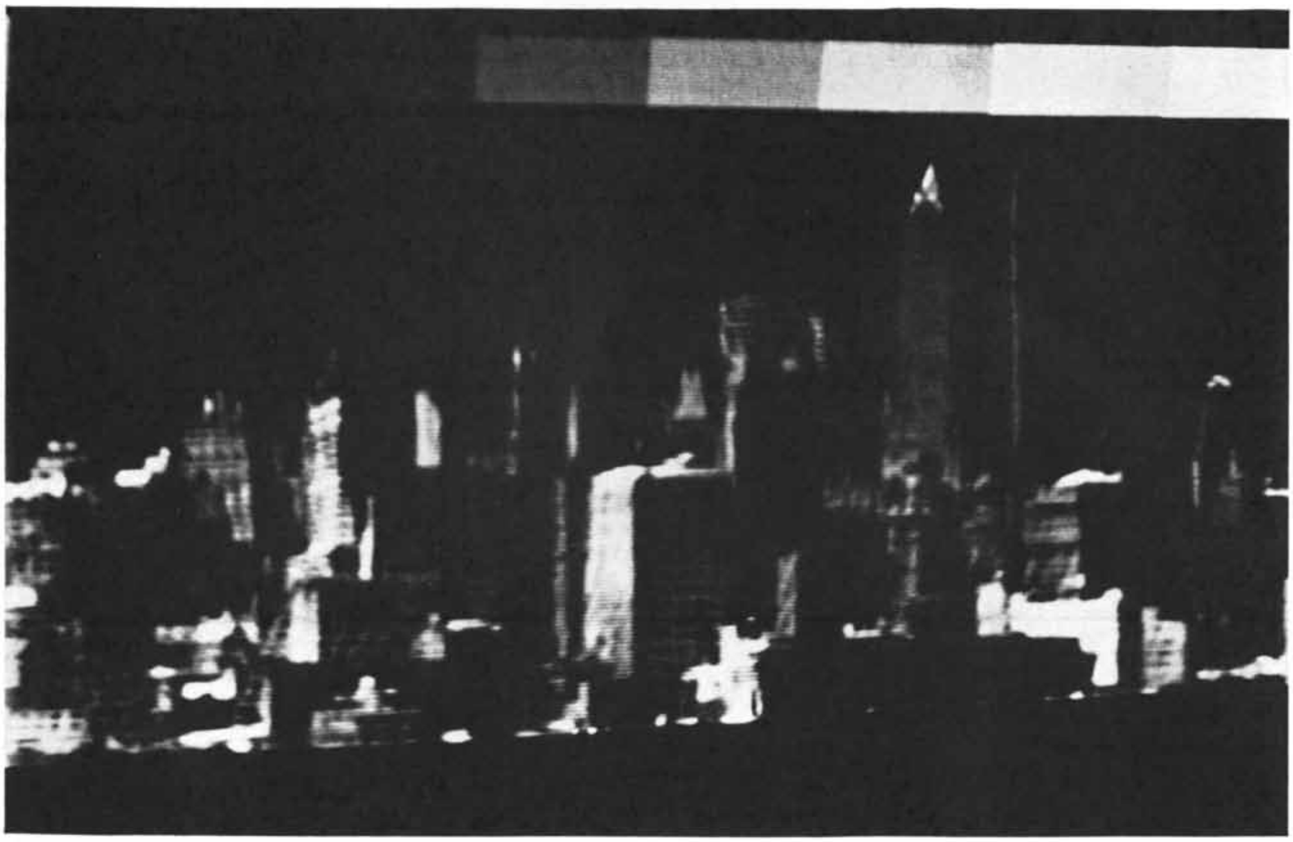
Third, the city is a prodigious generator of heat, particularly in winter, when heating systems are in operation. Even in summer, however, the city has many sources of heat that the countryside either lacks or has in far smaller numbers. Among them are factories, vehicles and even air conditioners, which of course must pump out hot air in order to produce their cooling effect.

Fourth, the city has distinctive ways of disposing of precipitation. If the precipitation is in the form of rain, it is quickly removed from the surface by drainpipes, gutters and sewers. If it is snow, much of it is cleared from the surface by plows and shovels, and significant amounts are carried away. In the country much precipitation remains on the surface or immediately below it; the water is thus available for evaporation, which is of course a cooling process powered by heat energy. Because there is less opportunity for evaporation in the city, the heat energy that would have gone into the process is available for heating the air.

Finally, the air in the city is different



HEAT PATTERNS in the lower Manhattan area of New York City on a summer day are shown by infrared photography. In the photographs, which were made with a Barnes thermograph, the lightest areas are the warmest and the darkest are the coolest. The view above shows the buildings at about 11:00 A.M. and the view below at about 3:30 P.M. The day was sunny but hazy; the temperature in the city during the time covered by the photographs was about 75 degrees Fahrenheit. The storage of heat by buildings affects a city's climate.



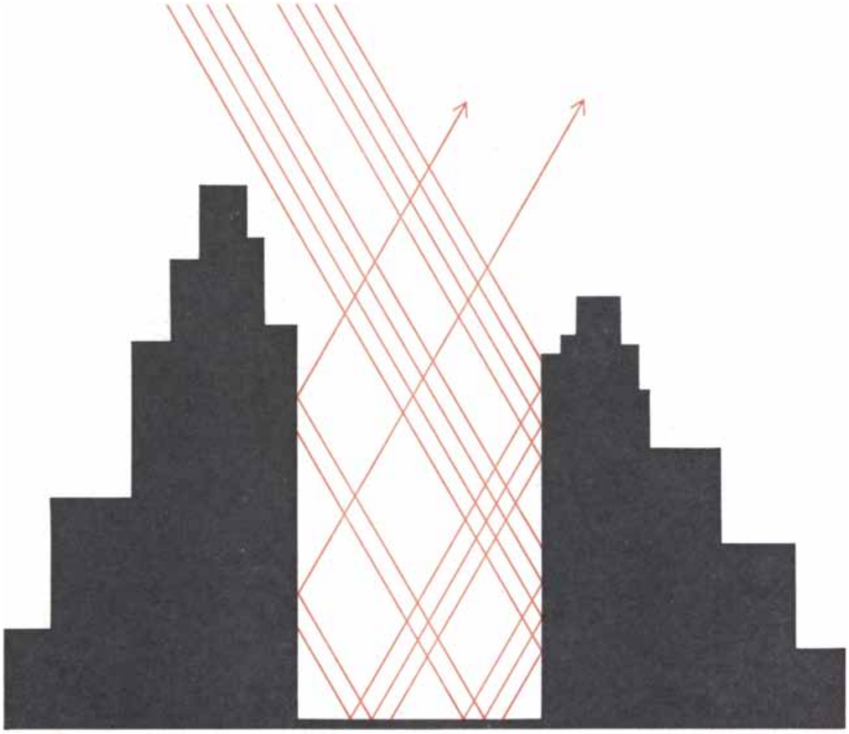
in that it carries a heavy load of solid, liquid and gaseous contaminants. About 80 percent of the solid contaminants are in the form of particles that are small enough to remain suspended for several days in still air. Although these particles collectively tend to reflect sunlight, thereby reducing the amount of heat reaching the surfaces, they also retard the outflow of heat. The gaseous contaminants, which usually have a greater total mass than the solid ones, come primarily from the incomplete combustion of fuels. One of the principal gases in many cities is sulfur dioxide; when this gas is dissolved under the appropriate meteorological conditions in cloud droplets or raindrops, it is oxidized to form dilute sulfuric acid.

Let us consider how these five influences act over a period of time on the climate of a large city. Our hypothetical city lies in an area of flat or gently rolling countryside and has no large bodies of water nearby. The day is a Sunday, so that no substantial amounts of fuel are being used for industrial purposes. It is a summer day, with clear skies and light winds.

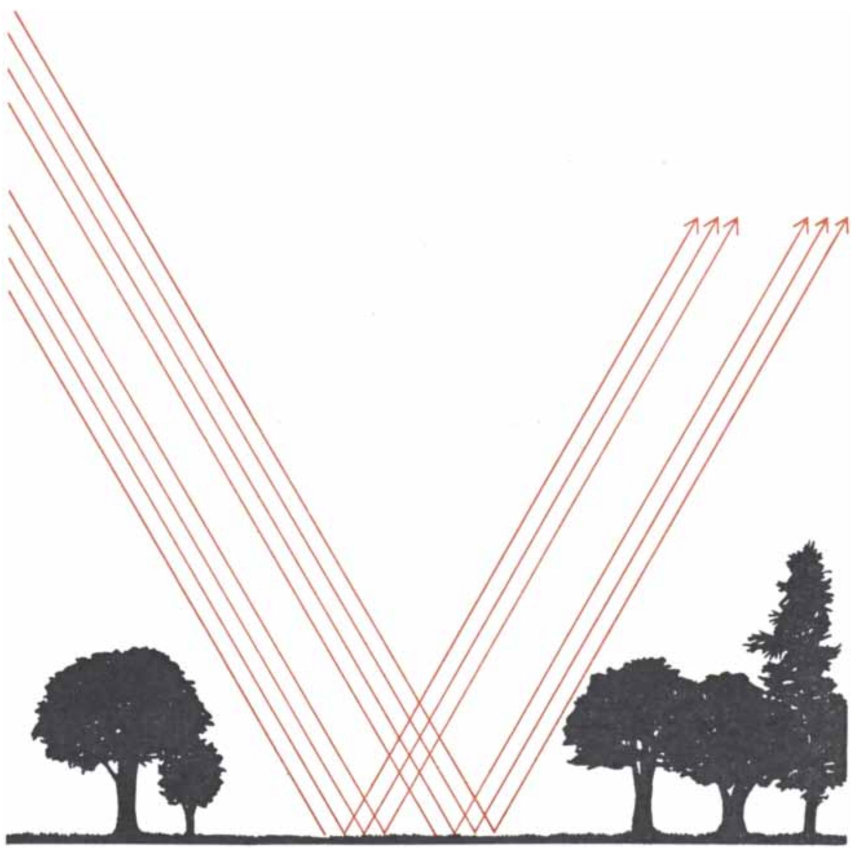
As the sun rises it shines equally on city and country. The sunlight strikes the flat, open country at a low angle; much of it is reflected from the surface. The many vertical walls of the city, however, are almost perpendicular to the sun's rays. In spite of the fact that when the sun is low in the sky its rays are less intense because they must pass through more of the earth's atmosphere, the walls begin almost at once to absorb heat. In the country little heat is being absorbed, even in the sunlit areas.

Later in the day the rural areas begin to respond more like the city. The sun has risen high enough for its radiation to impinge on the surface more directly and with less reflection. The air outside the city begins to warm rapidly. The city has already been warming for some time, however, and so it has a large lead toward the day's maximum temperature.

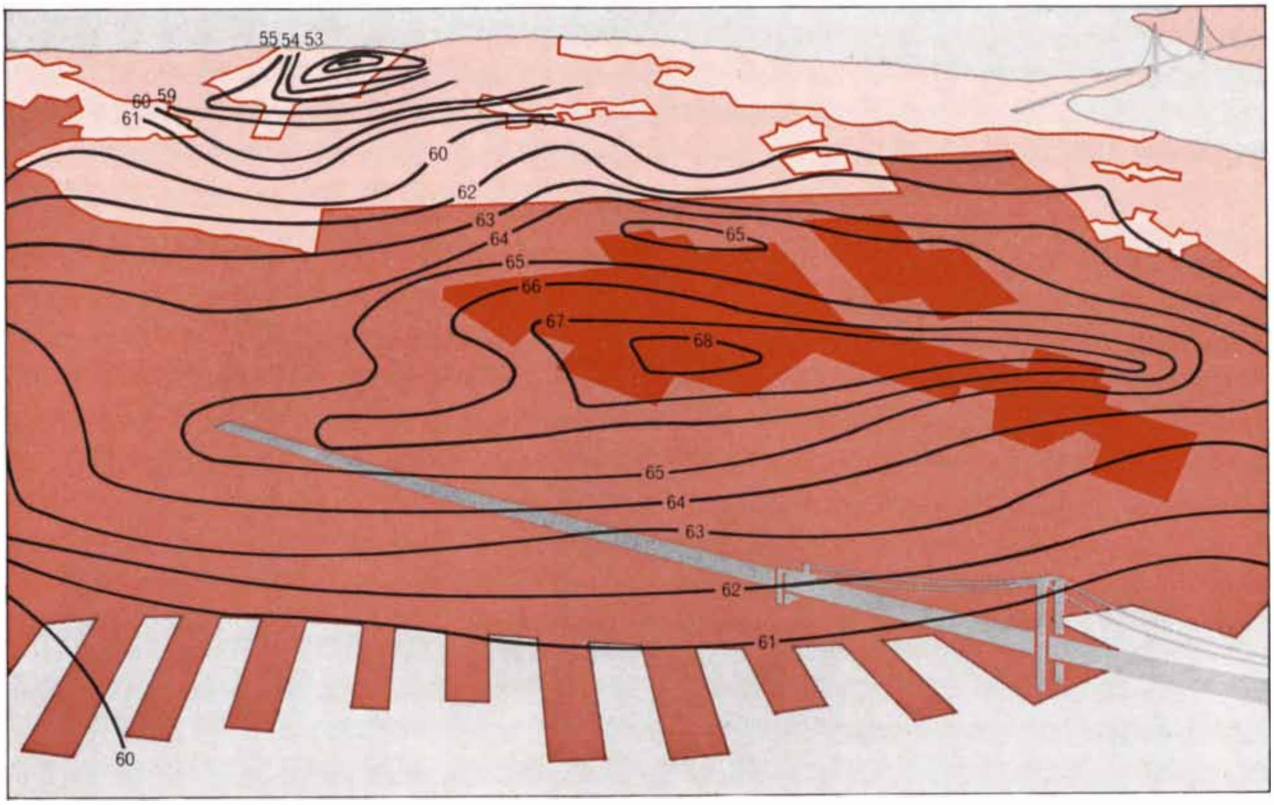
The warm air in the city concentrates near the city's center of mass. Toward midmorning the air in the center begins to rise. Being warmer at each level than the air at the same level in the surrounding countryside, the city air continues to rise in a gentle stream flowing upward from the center. The air that rises must be replaced; hence a flow from the rural areas into the city begins in the layers near the ground. The air from the country must also be replaced, and gradually a slow circulation is established. Air



SHAPE AND ORIENTATION OF SURFACES in a city have a strong bearing on the climate. Vertical walls tend to reflect solar radiation toward the ground instead of the sky. Rocklike materials also store heat, so that the city often becomes warmer than its environs.



RADIATION IN COUNTRYSIDE tends to be reflected back to the sky because the countryside has fewer vertical surfaces than the city. Toward midday, however, when the sun's rays are perpendicular to the ground, city and country temperatures may be about the same.



TEMPERATURE DISTRIBUTION in San Francisco on a spring evening is depicted by means of isotherms, which are lines of equal temperature. The shading ranges from the most densely built-up areas (*dark*) through less dense sections to open country (*light*).

EMISSIONS OF HEAT from an automobile with its engine idling appear in an infrared photograph made with a Barnes thermograph. Bright area below the car is pavement, which was in direct sunlight. Vehicles are a major source of heat production in a city.

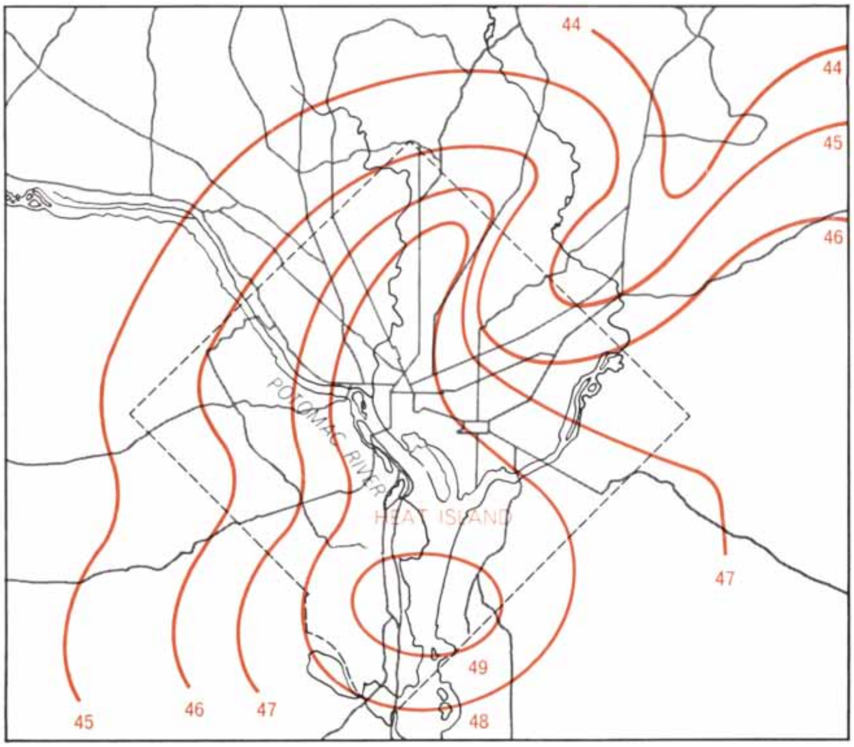
moves into the center of the city in the lower layers, rises in the central core, flows outward again at a higher altitude and as it cools settles down over the open country to complete the cycle.

Near midday the sunlight strikes the open country still more directly, and the difference in temperature between city and country becomes quite small. Now the air rising over the city is not appreciably warmer than the surrounding air, so that in the early afternoon the cycle of circulation is considerably weakened. As the afternoon progresses, however, a situation similar to that of the early morning develops. The sun sinks, its rays striking the open country at a lower and lower angle; an increasing proportion of its radiation is reflected. During this time the walls in the city are still intercepting the sun's radiation quite directly. The difference in temperature between city and country begins to increase again, and the circulation of air rising over the city and sinking outside it is reinvigorated. Just before sunset the circulation is fairly strong, but it weakens again as darkness falls. At about this time one would be likely to find the temperature at a weather station outside the city (such as at an airport) lower than the temperature at the downtown weather office.

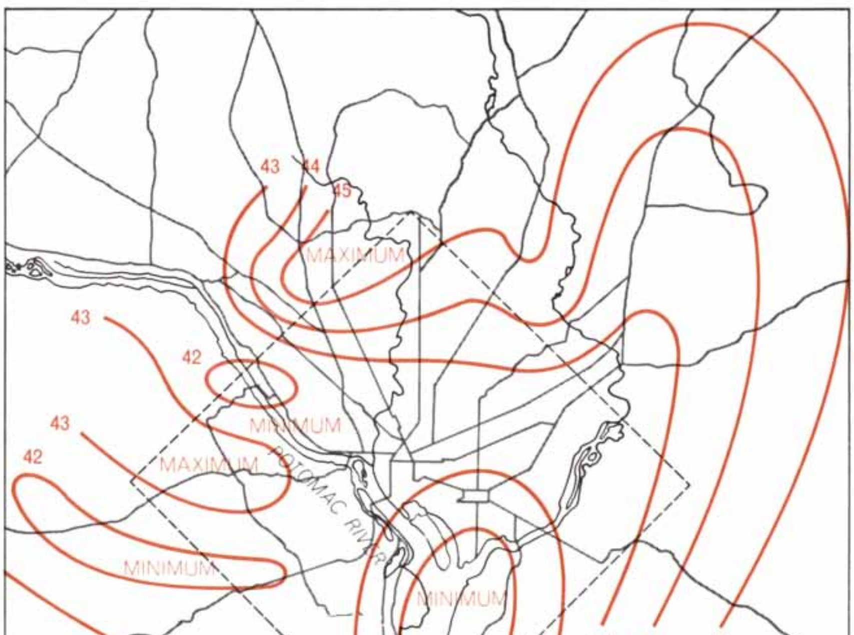
During the night the surfaces that radiate their warmth to the sky most rapidly are the streets and the rooftops. If much of the rooftop area of the city is at about the same height, there will be a strong tendency for a cool layer of air to be formed at that level. With cool air at the rooftops now lying below warmer air just above it, a rather stable stratification of air develops, and any tendency for upward movement of warm air in the spaces between buildings is inhibited.

The overall situation now is that the rural area is cooling rapidly and the city area is cooling slowly. Heat is being removed from the fields by light winds and by almost unobstructed radiation to the night sky. In the city, however, pockets of air are trapped. They cannot move upward, and they are still receiving heat from the release of energy stored in the walls of the buildings during the day. Through the night both the city and the countryside will continue to cool, but by dawn the city is still likely to be four or five degrees warmer than its surroundings.

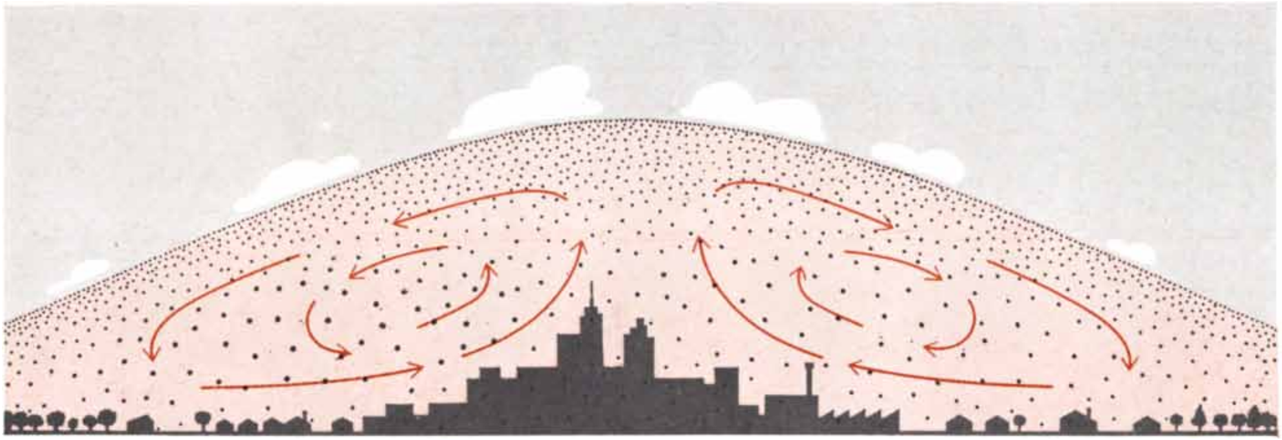
Early Monday morning the factories in the city begin to put forth heat, smoke and gases. Automobiles, trucks and buses start to emit large quantities



ANNUAL TEMPERATURE RECORD of Washington, D.C., and its environs gives the average of annual minimum temperatures for the period 1946–1960. The areas inside closed isotherms constitute what is known as the heat island. Here as in other cities the island is associated with the most densely built-up part of the urban complex. This map and the one below are based on data obtained by Clarence A. Woollum of the U.S. Weather Bureau.



RECORD OF PRECIPITATION in the Washington area covers the same 15 years as the temperature record. Both topography and the existence of the city affect precipitation.

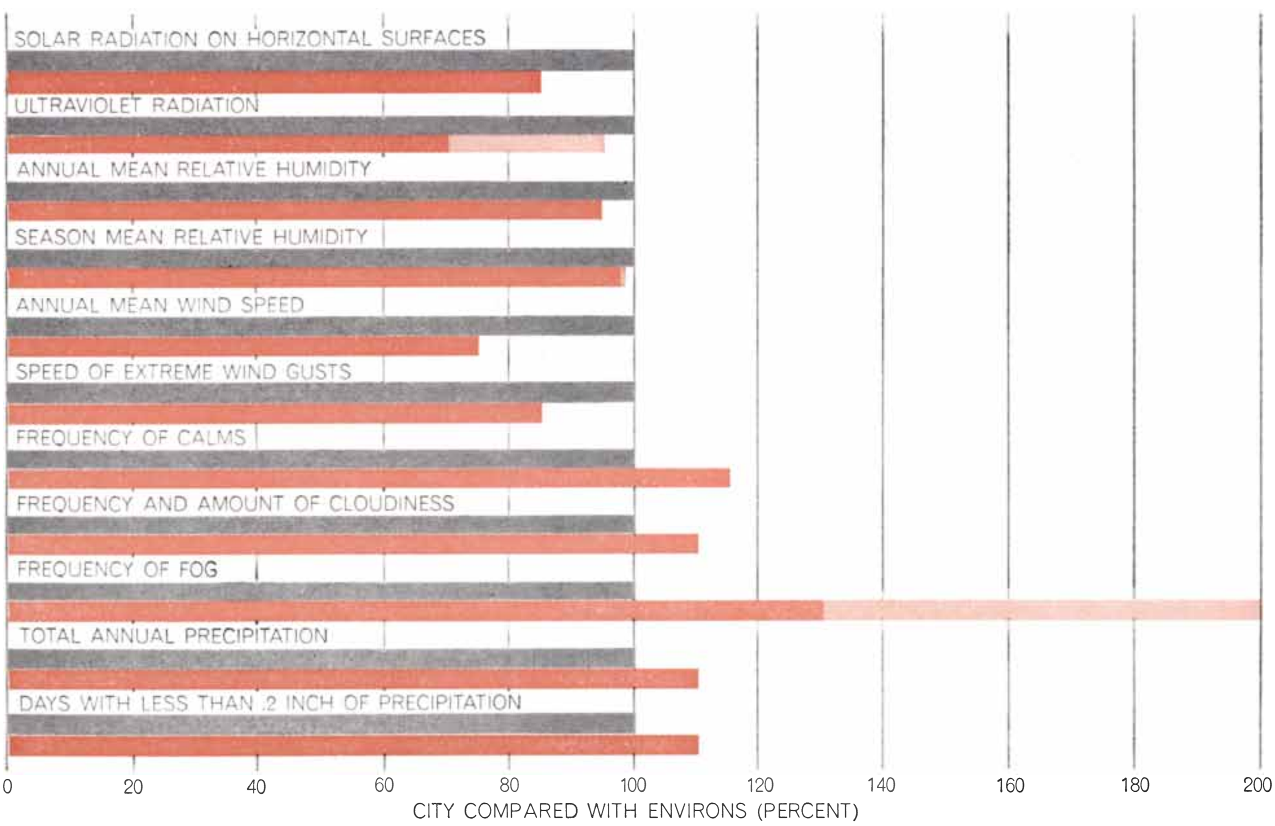


DUST DOME takes shape periodically over large cities because of the particles of dust and smoke that enter the air as a result of activities in the city. Air tends to rise over the warmer central part of the city and to settle over the cooler environs, so that a circulatory system develops. Dome is likely to persist, significantly affecting the city's climate, until a strong wind or a heavy rain carries it away.

of heat and fumes. Even stoves in kitchens constitute a source of heat that cannot be neglected. Artificial heating and air pollution thus become meteorologically significant as the day begins.

As before, the early sun starts to warm the city's walls and streets, and heat begins to accumulate in the downtown area. Today, however, there is a difference because of the heat being added to the system by the tall chimneys of factories. Ordinarily air rising to the height of the chimney tops would have had a chance to cool, but now it receives more heat at that level and will probably rise higher above the city than it did on Sunday. Moreover, the column of air now carries a freight of particles of dust and smoke. The smallest particles will fall only after they have been carried away from the rising column of air and out over the suburbs. Other particles will remain suspended over the city all day.

Over a long period of time the con-

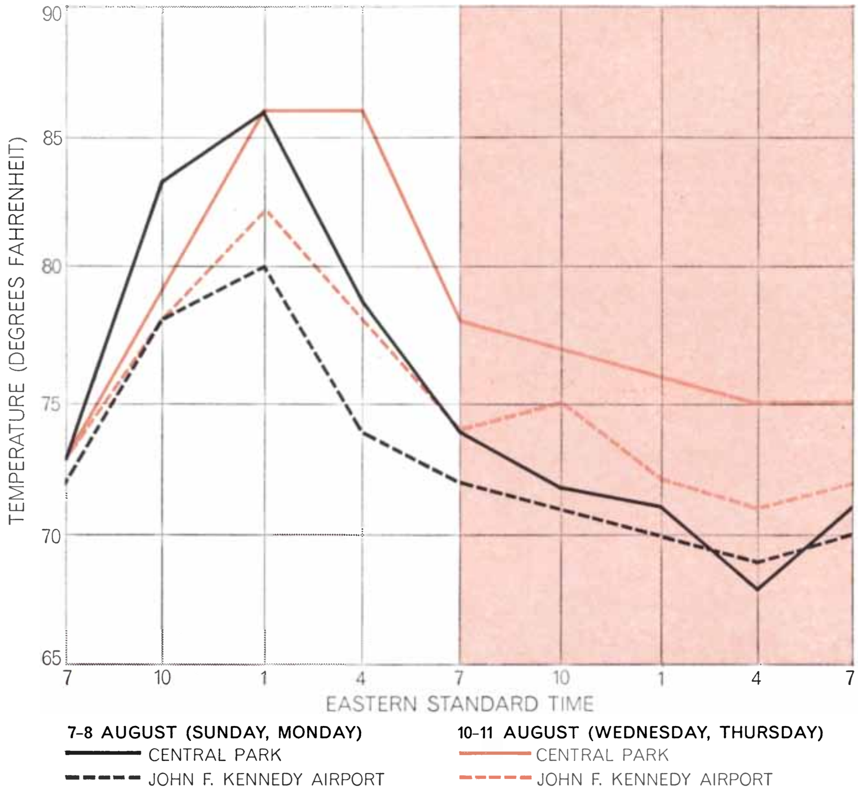


MAJOR DIFFERENCES IN CLIMATE between a city (*color*) and its environs (*gray*) are set out in terms of the percentage by which the city has more or less of each climatic variable during a year than is experienced in the countryside. For example, the city receives 5 percent less ultraviolet radiation than the countryside in summer, 30 percent less in winter; frequency of fog in city is 30 percent higher in summer and 100 percent higher in winter. Findings were made by Helmut E. Landsberg of the University of Maryland.

tinuous introduction and movement of particles creates a dome-shaped layer of haze over the city. This structure, variously called the "dust dome" and the "haze hood," has long been characteristic of large cities, although in recent years the general dirtiness of the air has made the dome harder to distinguish from its surroundings than it was several decades ago. Nonetheless, it still has a marked effect on the city's climate.

At night, as the particles in the dome cool, they can become nuclei on which the moisture in the air condenses as fog. The phenomenon occurs over cities in the middle latitudes when conditions are precisely right. The first layers of fog will usually form near the top of the dome, where the particles cool most rapidly by radiation; the blanket becomes thicker by downward growth until it reaches the ground as smog. This extra covering of water droplets over the city further retards nighttime cooling. Fog helps to perpetuate the dust dome by preventing the suspended particles from moving upward out of the system. Thus one day's contribution of solid contaminants will remain in the air over the city to be added to the next day's.

In the absence of a strong wind or a heavy rain to clear away the dust dome, the haze becomes denser each day. In winter, since less and less sunshine penetrates the dome to warm the city naturally, more and more fuel is burned to make up the difference. The combustion contributes further to the processes that build up smog. It is in this gradual but inexorable way that the smog problem has attained serious dimensions in many large cities.



TEMPERATURE DIFFERENCES appear in readings at a weather station in New York City and one at an airport in the environs for two 24-hour periods in August, 1966. The graph begins at 7:00 A.M. for each period. Temperature differences are often less pronounced on weekends than on weekdays because fewer of a city's heat sources are operating on weekends.

In sum, a city's effect on its own climate is complex and far-reaching. Helmut E. Landsberg of the University of Maryland, who until recently was director of climatology in the U.S. Weather Bureau, has drawn up a balance sheet showing the net effect of the variables [*see bottom illustration on opposite page*]. Among other things, he has concluded that cities in the middle latitudes receive 15 percent less sunshine on horizontal surfaces than is received in surrounding rural areas and that they receive 5 percent less ultraviolet radiation in summer and 30 percent less in winter. Landsberg's figures also show that the city, compared with the countryside, has a 6 percent lower annual mean relative humidity, 10 percent more precipitation, 10 percent more cloudiness, 25 percent lower mean annual wind speed and 30 percent more fog in summer and 100 percent more in winter.

T. J. Chandler, director of the London Climatic Survey, has compiled a number of records for the London area. He has found that over a period of 30 years the average maximum temperatures in the city, the suburbs and the surrounding countryside were respectively 58.3, 57.6 and 57.2 degrees and the average minimums 45.2, 43.1 and 41.8 degrees. His figures also show that over the period the city had consistently less sunshine than its environs did.

Some of these broad findings merit closer consideration. The patterns of temperature in a city can be shown on maps by drawing isotherms, or lines of equal temperature, for various times. Under a great variety of wind, cloud and sunshine conditions isotherm maps all show the highest temperatures clustered near the center of the city, with lower temperatures appearing radially toward the suburbs and the countryside. The resulting pattern of isotherms suggests the term "heat island" for the warmest area [*see top illustration on page 19*]. The term is used regularly by meteorologists to describe this major feature of a city's climate.

The heat island has been observed in many cities, some large and some small, some near water and some not, some with hills and some with none. How, then, can one be sure that the heat island, and thus the city climate itself, is really attributable to the works of man? J. Murray Mitchell, urban climatologist in the U.S. Weather Bureau, has considered the question and found three kinds of evidence that the city climate is caused by the city itself.

First, cities exhibit the heat island whether they are flat like Indianapolis or built on hills like San Francisco. Hence topography cannot explain the heat-island pattern. Second, temperature records averaged by day of the week show marked differences between Sundays and other days. Since many of the heat-creating processes distinctive to cities are inactive on Sundays, it is evident that those man-made processes account for the heat island. Finally, Mitchell has carefully examined the population and temperature records of a number of cities and found that the size of the heat island and the difference in temperature between it and surrounding areas increase as population does.

Another fact to be noted about tem-

LOSS OF BRIGHT SUNSHINE in London compared with areas surrounding the city is expressed in terms of minutes per day for each month. The figures show the city's average loss during the period 1921 to 1950. London area's districts are represented by the dark line at top, the inner suburbs by the middle line and the outer suburbs by the bottom line.

peratures is that the maximum difference between city and countryside appears to be about 10 to 15 degrees Fahrenheit, regardless of the size of the city. Chandler has found this to be the case in London, which has a population of eight million; my colleagues and I have found the same in Corvallis, Ore., which has a population of about 20,000.

Chandler's figures for the loss of sunlight in London show larger losses in winter, when the sun is low, than in summer, when sunlight takes a shorter path through the atmosphere. The amount of reduction increases markedly toward the center of the city, showing both the greater depth of the dust dome and the greater density of pollutants there. Part of the reduction of sunlight in London and other cities can be laid to the fact that a city tends to be more cloudy than its environs. Warm air rising over the center of the city provides a mechanism for the formation of clouds on many days when clouds fail to form in the country.

The frequency of fogs during the winter has to do with the greater relative reductions in sunshine during the winter months. One cannot simply say, however, that the greater frequency of fog explains the reduced total of sunshine. A feedback process is involved. Once fog forms, a weak sun has most of its energy reflected from the top of the fog layer. Little of the energy penetrates the fog to warm the city, and so the fog tends to perpetuate itself until the climatic situation changes.

Another connection between winter and the higher frequency of fog arises from the low temperatures. After an incursion of cold arctic air the residents of the city increase their rate of fuel consumption. The higher consumption of fuel produces more particulate pollutants and more water vapor. The air above a city is usually quite stagnant following the arrival of a cold wave, and thus the stage is set for the generation of fog. Lacking ventilation, the city's atmosphere fills with smoke, dirt and water vapor. The particles of smoke and dirt act as nuclei for the condensation of the water vapor. Because the water is shared among a large number of nuclei, the air contains a large number of small water droplets. Such a size distribution of water droplets forms a persistent fog, and the fog retards warming of the city. Retarded warming prolongs the need for extra heating. Only another change of air mass will relieve the situation. This chain of events has been associated with nearly every major disaster resulting from air pollution.

Reduction of visual range by smoke alone is not regularly recorded in cities. It is recorded at airports, however, and Landsberg has been able to use data from the Detroit City Airport, which is near the center of the city, and Wayne County Airport, which is in a more rural area, to deduce something about climatic differences between a city and the nearby countryside. The records indicate that a city will have, in the course of a year, 10 times more hours in which smoke restricts visibility to a mile or less than will be experienced in rural areas.

Contrary to what one might think, this situation may be improving somewhat. Robert Beebe of the U.S. Weather Bureau recently studied records of the visual range at the major municipal airports that did not change either their location or their schedule of weather observation between 1945 and 1965. He found that the number of times when smoke reduces horizontal visibility at the airports is less now than it was in 1945. The change might be explained by efforts to control air pollution, resulting in reduced concentrations of smoke and in changes in the size and character of smoke particles.

The differences in moisture and precipitation between a city and its environs are somewhat contradictory. During periods without rain the relative

FOG IN PARIS has cut visibility more in the city than in the surrounding areas. Data are for winter and show the percent of time when visibility was reduced to between one mile and a quarter-mile by light fog (*light*), a quarter-mile to 300 feet by moderate fog (*medium*) and less than 300 feet by dense fog (*dark*). In the summer there were far fewer days of fog.

scarcity of water for evaporation in the city results in a reduced concentration of water vapor in the air. Expressed as relative humidity, the difference gives the city a reduction of 6 percent in the annual average of the countryside, of 2 percent in the winter average and of 8 percent in the summer.

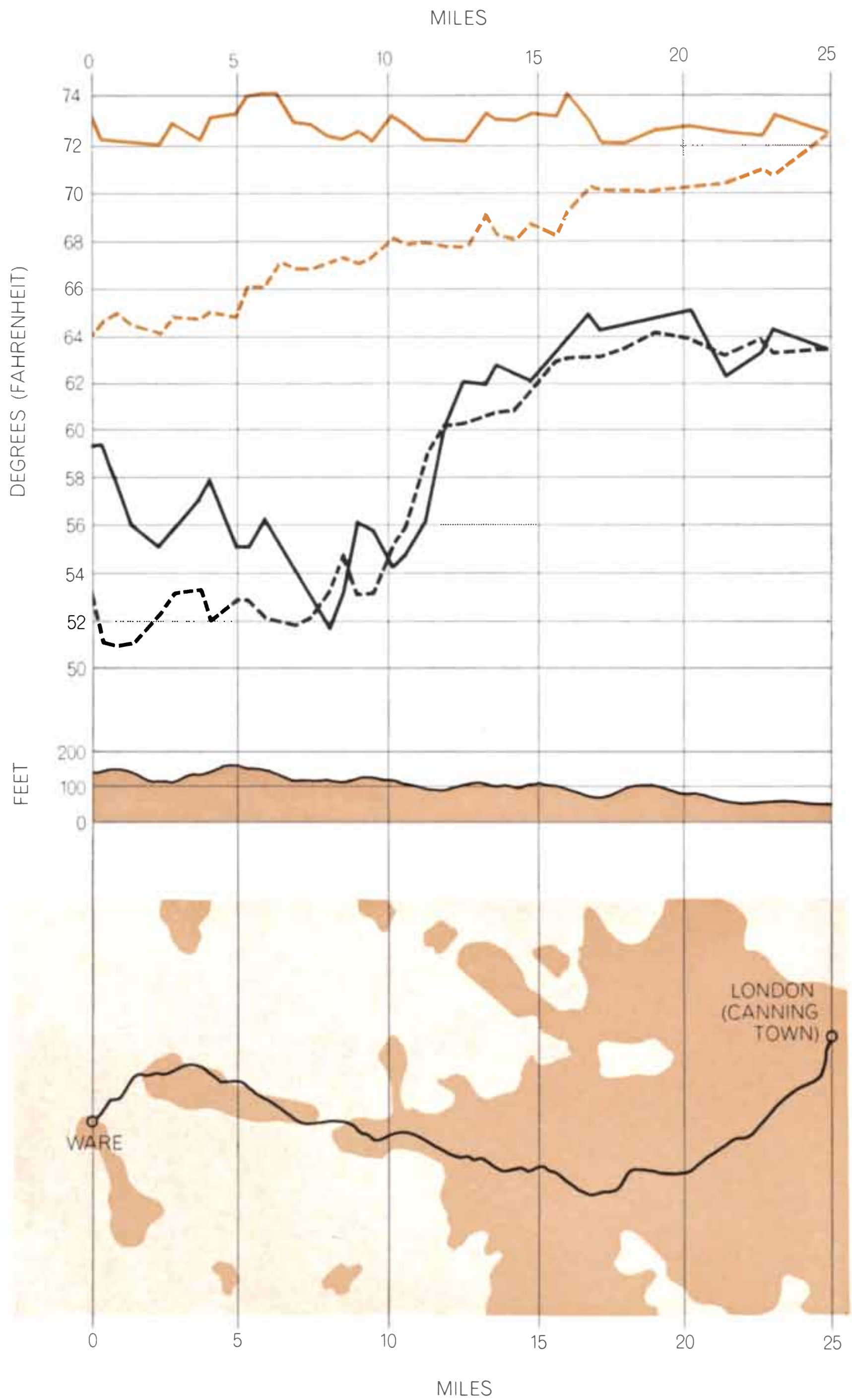
Even though the city is somewhat drier than its environs, on the days when rain or snow falls there is likely to be more in the city than in the countryside. The difference amounts to 10 percent in a year. It builds up mostly as an accumulation of small increments on drizzly days, when not much precipitation falls anywhere in the area. On such days the updrafts over the warm city provide enough extra lift so that the clouds there produce a slightly higher amount of precipitation.

Perhaps the catalogue of differences I have cited will leave the reader thinking that the city climate offers no advantages over the country climate. Actually there are several, including lower heating bills, fewer days with snow and a longer gardening season. Landsberg has estimated that a city has about 14 percent fewer days with snow than the countryside. The season between the last freeze in the spring and the first freeze in the fall may be three or four weeks longer in the city than in the countryside.

Both the advantages and the disadvantages of city climate testify to the fact that the city's climate is distinctly different from the countryside's. Every major aspect of climate is changed, if only slightly, by an urban complex. The differences in a small city may be only occasional; in a large city every day is different climatically from what it would have been if the city were not there.

Fuller understanding of the climatic changes created by a city may make it possible to manage city growth in such a way that the effect of troublesome changes will be minimal. Perhaps the changes can even be made beneficial. Several organizations are accumulating climatological data on cities. I have already mentioned the London Climatic Survey. Similar work is in progress in the U.S. Environmental Science Services Administration, at the University of California at Los Angeles, at New York University and in the research laboratories of the Travelers Insurance Company. Meteorologists in those organizations are driving instrumented automobiles, flying instrumented aircraft and operating hundreds of ground stations to obtain weather data. Although the studies are aimed primarily at understanding the meteorological problems of air pollution, other aspects of the local modification of climate by cities will be better understood as a result.

What may be even more important is the possibility of ascertaining the potential of extensive urbanization for causing large-scale changes of climate over entire continents. The evidence is not yet substantial enough to show that urbanization does cause such changes, but it is sufficient to indicate that the possibility cannot be ignored. The acquisition of more knowledge about the climate of cities may in the long run be one of the keys to man's survival.



TEMPERATURE TRAVERSES between the Canning Town section of London and the community of Ware 25 miles north were made on a June day (*color*) and night (*black*) by T. J. Chandler of the London Climatic Survey. In each case he made an outbound trip (*solid line*) and an inbound one (*broken line*). Dark shading at bottom shows heavily built-up areas.

The Split Brain in Man

The human brain is actually two brains, each capable of advanced mental functions. When the cerebrum is divided surgically, it is as if the cranium contained two separate spheres of consciousness

by Michael S. Gazzaniga

The brain of the higher animals, including man, is a double organ, consisting of right and left hemispheres connected by an isthmus of nerve tissue called the corpus callosum. Some 15 years ago Ronald E. Myers and R. W. Sperry, then at the University of Chicago, made a surprising discovery: When this connection between the two halves of the cerebrum was cut, each hemisphere functioned independently as if it were a complete brain. The phenomenon was first investigated in a cat in which not only the brain but also the optic chiasm, the crossover of the optic nerves, was divided, so that visual information from the left eye was dispatched only to the left brain and information from the right eye only to the right brain. Working on a problem with one eye, the animal could respond normally and learn to perform a task; when that eye was covered and the same problem was presented to the other eye, the animal evinced no recognition of the problem and had to learn it again from the beginning with the other half of the brain.

The finding introduced entirely new questions in the study of brain mechanisms. Was the corpus callosum responsible for integration of the operations of the two cerebral hemispheres in the intact brain? Did it serve to keep each hemisphere informed about what was going on in the other? To put the question another way, would cutting the corpus callosum literally result in the right hand not knowing what the left was doing? To what extent were the two half-brains actually independent when they were separated? Could they have separate thoughts, even separate emotions?

Such questions have been pursued by Sperry and his co-workers in a wide-ranging series of animal studies at the California Institute of Technology over the past decade [see "The Great Cerebral Commissure," by R. W. Sperry; Scientific American, January, 1964]. Recently these questions have been investigated in human patients who underwent the brain-splitting operation for medical reasons. The demonstration in experimental animals that sectioning of the corpus callosum did not seriously impair mental faculties had encouraged surgeons to resort to this operation for people afflicted with uncontrollable epilepsy. The hope was to confine a seizure to one hemisphere. The operation proved to be remarkably successful; curiously there is an almost total elimination of all attacks, including unilateral ones. It is as if the intact callosum had served in these patients to facilitate seizure activity.

This article is a brief survey of investigations Sperry and I have carried out at Cal Tech over the past five years with some of these patients. The operations were performed by P. J. Vogel and J. E. Bogen of the California College of Medicine. Our studies date back to 1961, when the first patient, a 48-year-old war veteran, underwent the operation: cutting of the corpus callosum and other commissure structures connecting the two halves of the cerebral cortex [*see illustration on page 26*]. As of today 10 patients have had the operation, and we have examined four thoroughly over a long period with many tests.

From the beginning one of the most striking observations was that the operation produced no noticeable change in the patients' temperament, personality or general intelligence. In the first case the patient could not speak for 30 days after the operation, but he then recovered his speech. More typical was the third case: on awaking from the surgery the patient quipped that he had a "splitting headache," and in his still drowsy state he was able to repeat the tongue twister "Peter Piper picked a peck of pickled peppers."

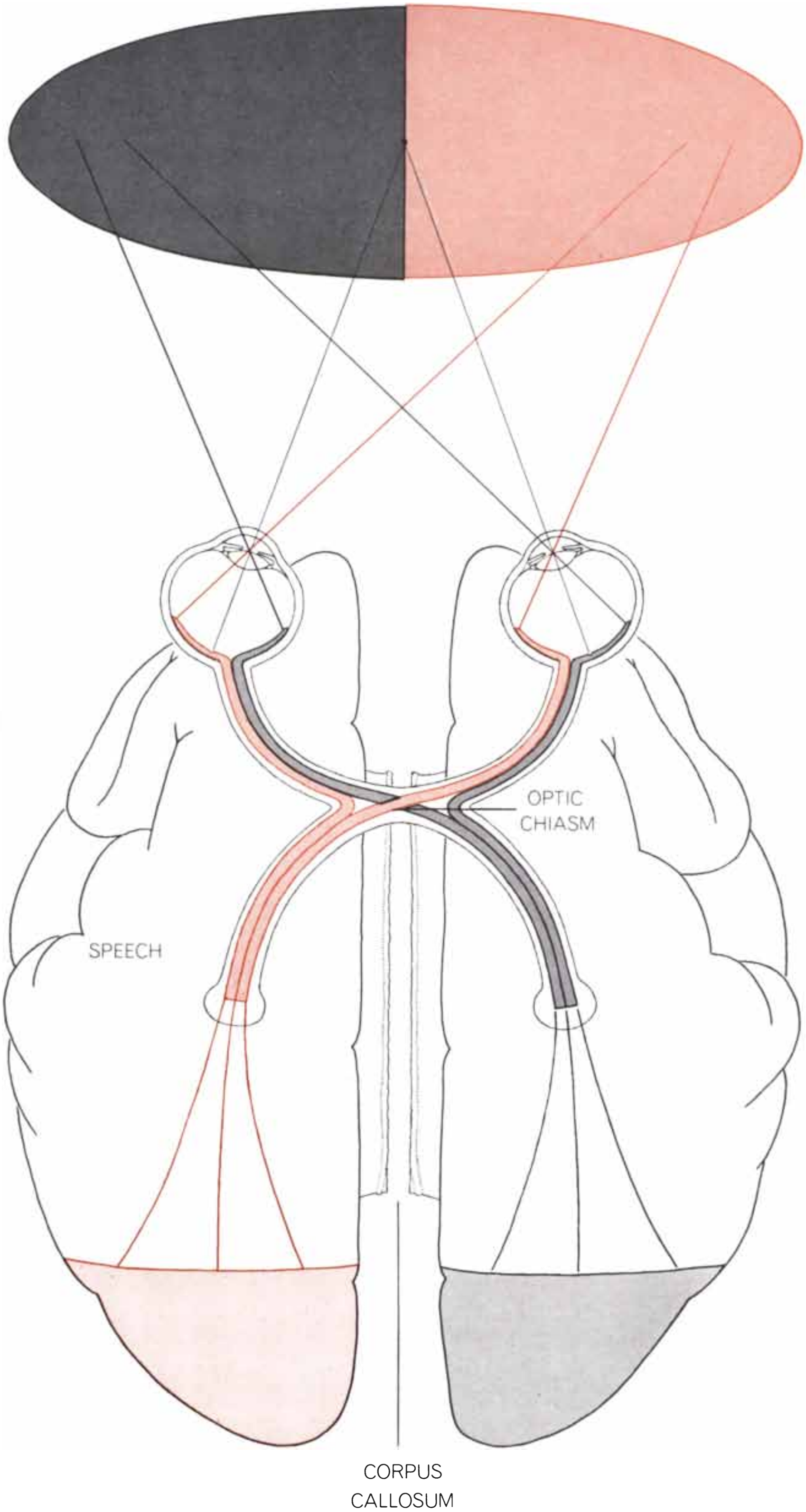
Close observation, however, soon revealed some changes in the patients' everyday behavior. For example, it could be seen that in moving about and responding to sensory stimuli the patients favored the right side of the body, which is controlled by the dominant left half of the brain. For a considerable period after the operation the left side of the body rarely showed spontaneous activity, and the patient generally did not respond to stimulation of that side: when he brushed against something with his left side he did not notice that he had done so, and when an object was placed in his left hand he generally denied its presence.

More specific tests identified the main features of the bisected-brain syndrome. One of these tests examined responses to visual stimulation. While the patient fixed his gaze on a central point on a board, spots of light were flashed (for a tenth of a second) in a row across the board that spanned both the left and the right half of his visual field. The patient was asked to tell what he had seen. Each patient reported that lights had been flashed in the right half of the visual field. When lights were flashed only in the left half of the field, however, the patients generally denied having seen any lights. Since the right side of the visual field is normally projected to the left hemisphere of the brain and the left field to the right hemisphere, one might have concluded that in these patients with divided brains the right hemisphere was in effect blind. We found, however, that this was not the case when the patients were directed to point to the lights that had flashed instead of giving a verbal report. With this manual response they were able to indicate when lights had been flashed in the left visual field, and perception with the brain's right hemisphere proved to be almost equal to perception with the left. Clearly, then, the patients' failure to report the right hemisphere's perception verbally was due to the fact that the speech centers of the brain are located in the left hemisphere.

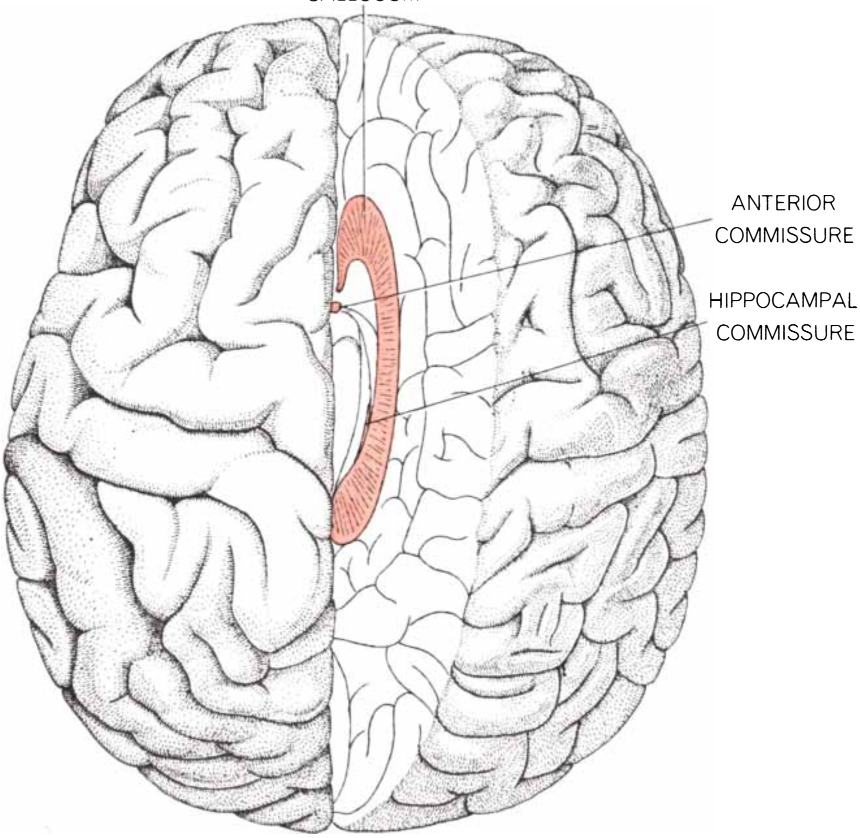
Our tests of the patients' ability to recognize objects by touch at first resulted in the same general finding. When the object was held in the right hand, from which sensory information is sent to the left hemisphere, the patient was able to name and describe the object. When it was held in the left hand (from which information goes primarily to the right hemisphere), the patient could not describe the object verbally but was able to identify it in a nonverbal test—matching it, for example, to the same object in a varied collection of things. We soon realized, however, that each hemisphere receives, in addition to the main input from the opposite side of the body, some input from the same side. This "ipsilateral" input is crude; it is apparently good mainly for "cuing in" the hemisphere as to the presence or absence of stimulation and relaying fairly gross information about the location of a stimulus on the surface of the body. It is unable, as a rule, to relay information concerning the qualitative nature of an object.

Tests of motor control in these split-brain patients revealed that the left hemisphere of the brain exercised normal control over the right hand but had less than full control of the left hand (for instance, it was poor at directing individual movements of the fingers). Similarly, the right hemisphere had full control of the left hand but not of the right hand. When the two hemispheres were in conflict, dictating different movements for the same hand, the hemisphere on the side opposite the hand generally took charge and overruled the orders of the side of the brain with the weaker control. In general the motor findings in the human patients were much the same as those in split-brain monkeys.

We come now to the main question on which we centered our studies, namely how the separation of the hemispheres affects the mental capacities of the human brain. For these psychological tests we used two different devices. One was visual: a picture or written information was flashed (for a tenth of a second) in either the right or the left visual field, so that the information was transmitted only to the left or to the right brain hemisphere [*see illustration on page 27*]. The other type of test was



VISUAL INPUT to bisected brain was limited to one hemisphere by presenting information only in one visual field. The right and left fields of view are projected, via the optic chiasm, to the left and right hemispheres of the brain respectively. If a person fixes his gaze on a point, therefore, information to the left of the point goes only to the right hemisphere and information to the right of the point goes to the left hemisphere. Stimuli in the left visual field cannot be described by a split-brain patient because of the disconnection between the right hemisphere and the speech center, which is in the left hemisphere.



TWO HEMISPHERES of the human brain are divided by neurosurgeons to control epileptic seizures. In this top view of the brain the right hemisphere is retracted and the corpus callosum and other commissures, or connectors, that are generally cut are shown in color.

tactile: an object was placed out of view in the patient's right or left hand, again for the purpose of conveying the information to just one hemisphere—the hemisphere on the side opposite the hand.

When the information (visual or tactile) was presented to the dominant left hemisphere, the patients were able to deal with and describe it quite normally, both orally and in writing. For example, when a picture of a spoon was shown in the right visual field or a spoon was placed in the right hand, all the patients readily identified and described it. They were able to read out written messages and to perform problems in calculation that were presented to the left hemisphere.

In contrast, when the same information was presented to the right hemisphere, it failed to elicit such spoken or written responses. A picture transmitted to the right hemisphere evoked either a haphazard guess or no verbal response at all. Similarly, a pencil placed in the left hand (behind a screen that cut off vision) might be called a can opener or a cigarette lighter, or the patient might not even attempt to describe it. The verbal guesses presumably came not from the right hemisphere but from the left, which had no perception of the object but might attempt to identify it from indirect clues.

Did this impotence of the right hemisphere mean that its surgical separation from the left had reduced its mental powers to an imbecilic level? The earlier tests of its nonverbal capacities suggested that this was almost certainly not so. Indeed, when we switched to asking for nonverbal answers to the visual and tactile information presented in our new psychological tests, the right hemisphere in several patients showed considerable capacity for accurate performance. For example, when a picture of a spoon was presented to the right hemisphere, the patients were able to feel around with the left hand among a varied group of objects (screened from sight) and select a spoon as a match for the picture. Furthermore, when they were shown a picture of a cigarette they succeeded in selecting an ashtray, from a group of 10 objects that did not include a cigarette, as the article most closely related to the picture. Oddly enough, however, even after their correct response, and while they were holding the spoon or the ashtray in their left hand, they were unable to name or describe the object or the picture. Evidently the left hemisphere was completely divorced, in perception and knowledge, from the right.

Other tests showed that the right hemisphere did possess a certain amount of language comprehension. For example, when the word "pencil" was flashed to the right hemisphere, the patients were able to pick out a pencil from a group of unseen objects with the left hand. And when a patient held an object in the left hand (out of view), although he could not say its name or describe it, he was later able to point to a card on which the name of the object was written.

In one particularly interesting test the word "heart" was flashed across the center of the visual field, with the "he" portion to the left of the center and "art" to the right. Asked to tell what the word was, the patients would say they had seen "art"—the portion projected to the left brain hemisphere (which is responsible for speech). Curiously when, after "heart" had been flashed in the same way, the patients were asked to point with the left hand to one of two cards—"art" or "he"—to identify the word they had seen, they invariably pointed to "he." The experiment showed clearly that both hemispheres had simultaneously observed the portions of the word available to them and that in this particular case the right hemisphere, when it had had the opportunity to express itself, had prevailed over the left.

Because an auditory input to one ear goes to both sides of the brain, we conducted tests for the comprehension of words presented audibly to the right hemisphere not by trying to limit the original input but by limiting the ability to answer to the right hemisphere. This was done most easily by having a patient use his left hand to retrieve, from a grab bag held out of view, an object named by the examiner. We found that the patients could easily retrieve such objects as a watch, comb, marble or coin. The object to be retrieved did not even have to be named; it might simply be described or alluded to. For example, the command "Retrieve the fruit monkeys like best" results in the patients' pulling out a banana from a grab bag full of plastic fruit; at the command "Sunkist sells a lot of them" the patients retrieve an orange. We knew that touch information from the left hand was going exclusively to the right hemisphere because moments later, when the patients were asked to name various pieces of fruit placed in the left hand, they were unable to score above a chance level.

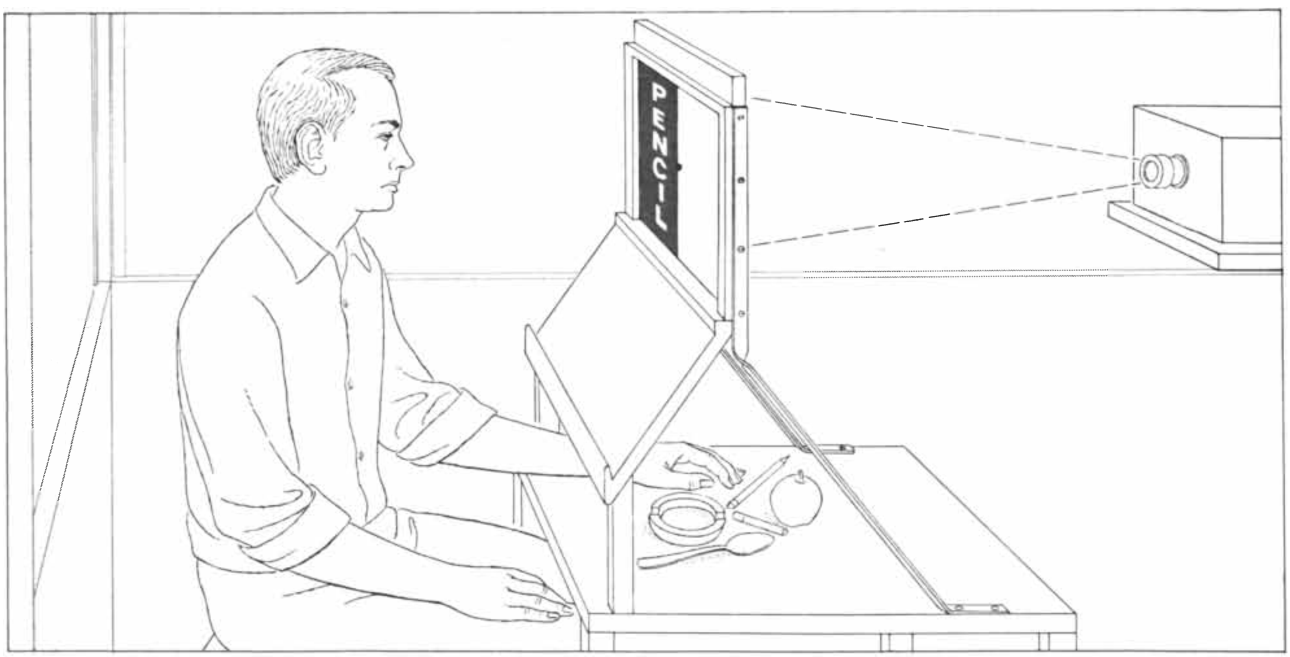
The upper limit of linguistic abilities in each hemisphere varies from subject to subject. In one case there was little or no evidence for language abilities in the right hemisphere, whereas in the other three the amount and extent of the capacities varied. The most adept patient showed some evidence of even being able to spell simple words by placing plastic letters on a table with his left hand. The subject was told to spell a word such as "pie," and the examiner then placed the three appropriate letters, one at a time in a random order, in his left hand to be arranged on the table. The patient was able to spell even more abstract words such as "how," "what" and "the." In another test three or four letters were placed in a pile, again out of view, to be felt with the left hand. The letters available in each trial would spell only one word, and the instructions to the subject were "Spell a word." The patient was able to spell such words as "cup" and "love." Yet after he had completed this task, the patient was unable to name the word he had just spelled!

The possibility that the right hemisphere has not only some language but even some speech capabilities cannot be ruled out, although at present there is no firm evidence for this. It would not be surprising to discover that the patients are capable of a few simple exclamatory remarks, particularly when under emotional stress. The possibility also remains, of course, that speech of some type could be trained into the right hemisphere. Tests aimed at this question, however, would have to be closely scrutinized and controlled.

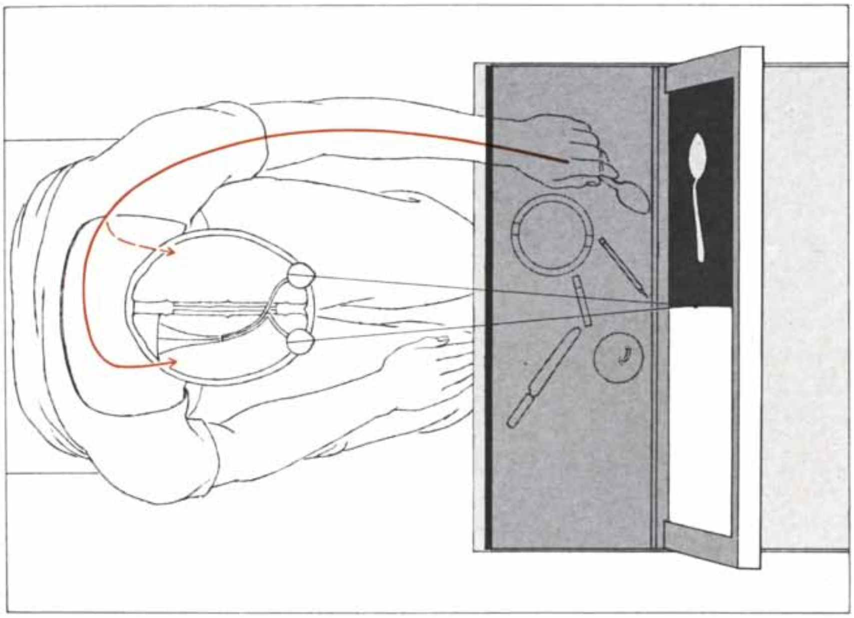
The reason is that here, as in many of the tests, "cross-cuing" from one hemisphere to the other could be held responsible for any positive findings. We had a case of such cross-cuing during a series of tests of whether the right hemisphere could respond verbally to simple red or green stimuli. At first, after either a red or a green light was flashed to the right hemisphere, the patient would guess the color at a chance level, as might be expected if the speech mechanism is solely represented in the left hemisphere. After a few trials, however, the score improved whenever the examiner allowed a second guess.

We soon caught on to the strategy the patient used. If a red light was flashed and the patient by chance guessed red, he would stick with that answer. If the flashed light was red and the patient by chance guessed green, he would frown, shake his head and then say, "Oh no, I meant red." What was happening was that the right hemisphere saw the red light and heard the left hemisphere make the guess "green." Knowing that the answer was wrong, the right hemisphere precipitated a frown and a shake of the head, which in turn cued in the left hemisphere to the fact that the answer was wrong and that it had better correct itself! We have learned that this cross-cuing mechanism can become extremely refined. The realization that the neurological patient has various strategies at his command emphasizes how difficult it is to obtain a clear neurological description of a human being with brain damage.

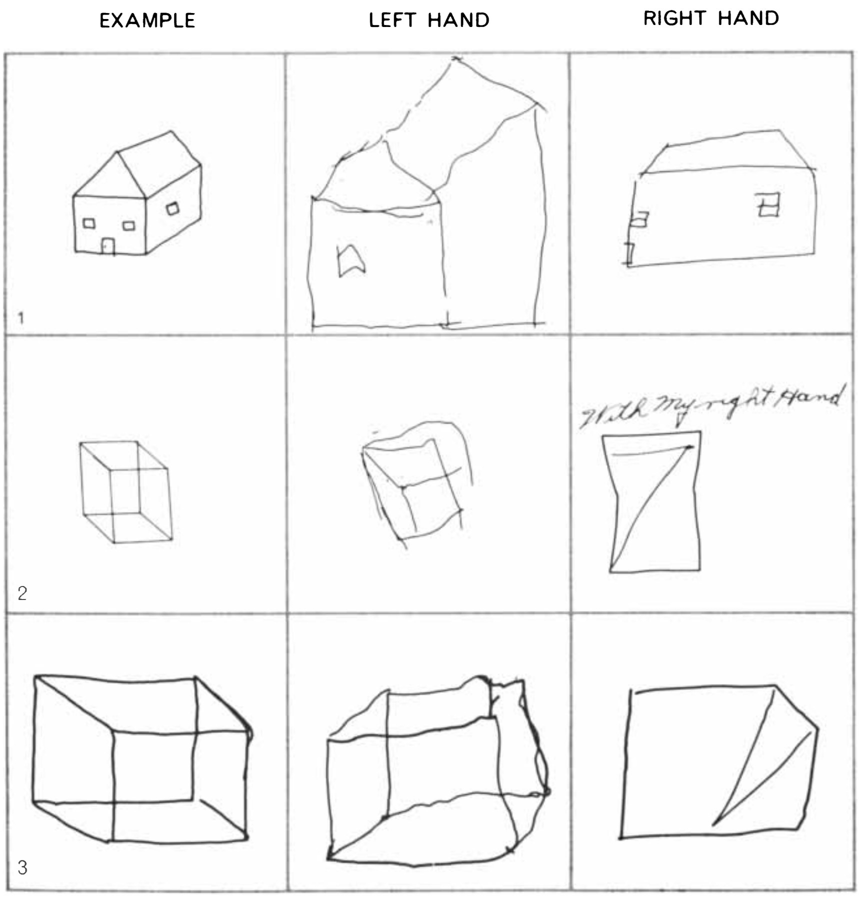
Is the language comprehension by the right hemisphere that the patients exhibited in these tests a normal capability of that hemisphere or was it acquired by learning after their operation, perhaps during the course of the experiments themselves? The issue is difficult to decide. We must remember that we are examining a half of the human brain, a system easily capable of learning from a single trial in a test. We do know that the right hemisphere is decidedly inferior to the left in its overall command of language. We have established, for instance, that although the right hemisphere can respond to a concrete noun such as "pencil," it cannot do as well with verbs; patients are unable to re-



RESPONSE TO VISUAL STIMULUS is tested by flashing a word or a picture of an object on a translucent screen. The examiner first checks the subject's gaze to be sure it is fixed on a dot that marks the center of the visual field. The examiner may call for a verbal response—reading the flashed word, for example—or for a nonverbal one, such as picking up the object that is named from among a number of things spread on the table. The objects are hidden from the subject's view so that they can be identified only by touch.



VISUAL-TACTILE ASSOCIATION is performed by a split-brain patient. A picture of a spoon is flashed to the right hemisphere; with the left hand he retrieves a spoon from behind the screen. The touch information from the left hand projects (*color*) mainly to the right hemisphere, but a weak "ipsilateral" component goes to the left hemisphere. This is usually not enough to enable him to say (using the left hemisphere) what he has picked up.



"VISUAL-CONSTRUCTIONAL" tasks are handled better by the right hemisphere. This was seen most clearly in the first patient, who had poor ipsilateral control of his right hand. Although right-handed, he could copy the examples only with his left hand.

spond appropriately to simple printed instructions, such as "smile" or "frown," when these words are flashed to the right hemisphere, nor can they point to a picture that corresponds to a flashed verb. Some of our recent studies at the University of California at Santa Barbara also indicate that the right hemisphere has a very poorly developed grammar; it seems to be incapable of forming the plural of a given word, for example.

In general, then, the extent of language present in the adult right hemisphere in no way compares with that present in the left hemisphere or, for that matter, with the extent of language present in the child's right hemisphere. Up to the age of four or so, it would appear from a variety of neurological observations, the right hemisphere is about as proficient in handling language as the left. Moreover, studies of the child's development of language, particularly with respect to grammar, strongly suggest that the foundations of grammar—a ground plan for language, so to speak—are somehow inherent in the human organism and are fully realized between the ages of two and three. In other words, in the young child each hemisphere is about equally developed with respect to language and speech function. We are thus faced with the interesting question of why the right hemisphere at an early age and stage of development possesses substantial language capacity whereas at a more adult stage it possesses a rather poor capacity. It is difficult indeed to conceive of the underlying neurological mechanism that would allow for the establishment of a capacity of a high order in a particular hemisphere on a temporary basis. The implication is that during maturation the processes and systems active in making this capacity manifest are somehow inhibited and dismantled in the right hemisphere and allowed to reside only in the dominant left hemisphere.

Yet the right hemisphere is not in all respects inferior or subordinate to the left. Tests have demonstrated that it excels the left in some specialized functions. As an example, tests by us and by Bogen have shown that in these patients the left hand is capable of arranging blocks to match a pictured design and of drawing a cube in three dimensions, whereas the right hand, deprived of instructions from the right hemisphere, could not perform either of these tasks.

It is of interest to note, however, that although the patients (our first subject in particular) could not execute such tasks with the right hand, they were capable of matching a test stimulus to the correct design when it appeared among five related patterns presented in their right visual field. This showed that the dominant left hemisphere is capable of discriminating between correct and incorrect stimuli. Since it is also true that the patients have no motor problems with their right hand, the patients' inability to perform these tasks must reflect a breakdown of an integrative process somewhere between the sensory system and the motor system.

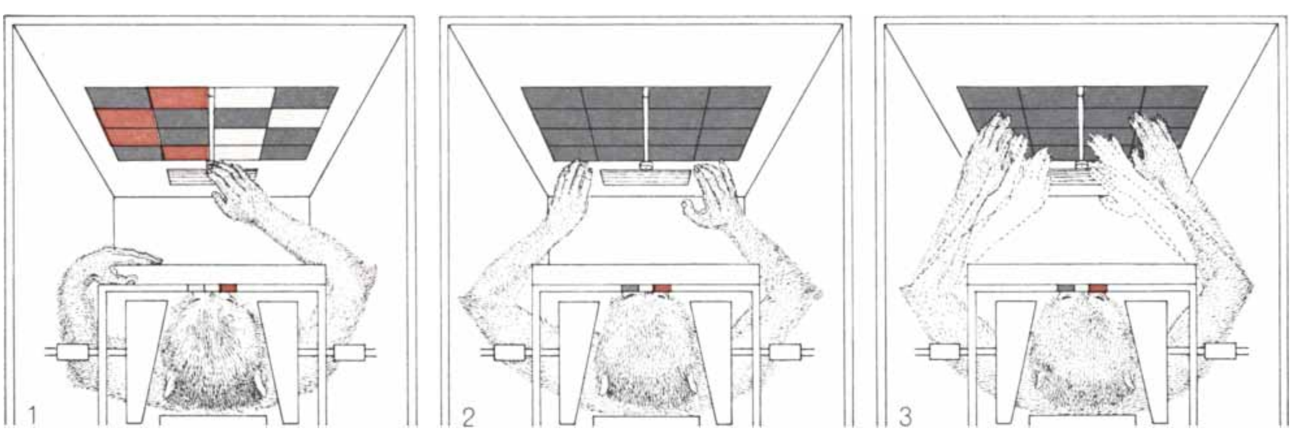
We found that in certain other mental processes the right hemisphere is on a par with the left. In particular, it can independently generate an emotional reaction. In one of our experiments exploring the matter we would present a series of ordinary objects and then suddenly flash a picture of a nude woman. This evoked an amused reaction regardless of whether the picture was presented to the left hemisphere or to the right. When the picture was flashed to the left hemisphere of a female patient, she laughed and verbally identified the picture as a nude. When it was later presented to the right hemisphere, she said in reply to a question that she saw nothing, but almost immediately a sly smile spread over her face and she began to chuckle. Asked what she was laughing at, she said: "I don't know... nothing... oh—that funny machine." Although the right hemisphere could not describe what it had seen, the sight nevertheless elicited an emotional response like the one evoked from the left hemisphere.

Taken together, our studies seem to demonstrate conclusively that in a split-brain situation we are really dealing with two brains, each separately capable of mental functions of a high order. This implies that the two brains should have twice as large a span of attention—that is, should be able to handle twice as much information—as a normal whole brain. We have not yet tested this precisely in human patients, but E. D. Young and I have found that a split-brain monkey can indeed deal with nearly twice as much information as a normal animal [*see illustration below*]. We have so far determined also that brain-bisected patients can carry out two tasks as fast as a normal person can do one.

Just how does the corpus callosum of the intact brain combine and integrate the perceptions and knowledge of the two cerebral hemispheres? This has been investigated recently by Giovanni Berlucchi, Giacomo Rizzolati and me at the Istituto di Fisiologia Umana in Pisa. We made recordings of neural activity in the posterior part of the callosum of the cat with the hope of relating the responses of that structure to stimulation of the animal's visual fields. The kinds of responses recorded turned out to be similar to those observed in the visual cortex of the cat. In other words, the results suggest that visual pattern information can be transmitted through the callosum. This finding militates against the notion that learning and memory are transferred across the callosum, as has usually been suggested. Instead, it looks as though in animals with an intact callosum a copy of the visual world as seen in one hemisphere is sent over to the other, with the result that both hemispheres can learn together a discrimination presented to just one hemisphere. In the split-brain animal this extension of the visual pathway is cut off; this would explain rather simply why no learning proceeds in the visually isolated hemisphere and why it has to learn the discrimination from scratch.

Curiously, however, the neural activity in the callosum came only in response to stimuli at the midline of the visual field. This finding raises difficult questions. How can it be reconciled with the well-established observation that the left hemisphere of a normal person can give a running description of all the visual information presented throughout the entire half-field projected to the right hemisphere? For this reason alone one is wearily driven back to the conclusion that somewhere and somehow all or part of the callosum transmits not only a visual scene but also a complicated neural code of a higher order.

All the evidence indicates that separation of the hemispheres creates two independent spheres of consciousness within a single cranium, that is to say, within a single organism. This conclusion is disturbing to some people who view consciousness as an indivisible property of the human brain. It seems premature to others, who insist that the capacities revealed thus far for the right hemisphere are at the level of an automaton. There is, to be sure, hemispheric inequality in the present cases, but it may well be a characteristic of the individuals we have studied. It is entirely possible that if a human brain were divided in a very young person, both hemispheres could as a result separately and independently develop mental functions of a high order at the level attained only in the left hemisphere of normal individuals.



SPLIT-BRAIN MONKEYS can handle more visual information than normal animals. When the monkey pulls a knob (*1*), eight of the 16 panels light momentarily. The monkey must then start at the bottom and punch the lights that were lit and no others (*2*). With the panels lit for 600 milliseconds normal monkeys get up to the third row from the bottom before forgetting which panels were lit (*3*). Split-brain monkeys complete the entire task with the panels lit only 200 milliseconds. The monkeys look at the panels through filters; since the optic chiasm is cut in these animals, the filters allow each hemisphere to see the colored panels on one side only.

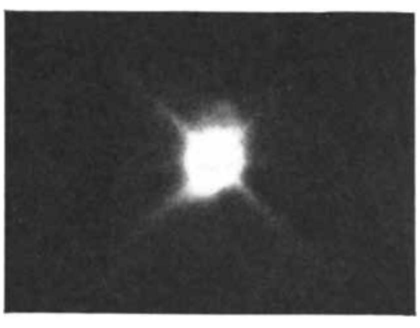
THE YOUNGEST STARS

The T Tauri variables are highly unstable stars that are found in regions rich in dust and gas. Their behavior suggests that they have recently condensed out of the interstellar material

by George H. Herbig

Until less than a quarter-century ago the stars appeared to astronomers as a bewildering collection of species among which it was difficult to find any systematic interrelations. The sky was filled with an almost zoological diversity of objects whose characteristics could be measured and described but hardly understood. Today, although some of the phenomena remain mysterious and new species keep turning up, the apparent chaos of the sky is being resolved into order. We are at last supplied with a clearly formulated philosophy of stellar evolution that now runs as a powerful theme through every aspect of astronomy and is beginning to fit the structural elements of the universe together into a coherent picture.

The basic concept for understanding the nature and history of stars grew from the discovery (barely 30 years ago) of how they generate their energy. They do so by thermonuclear processes that "burn" hydrogen and convert it into helium. The energy yield is 6×10^{18} ergs per gram of hydrogen converted. On the basis of the known energy output of our sun, this means that the sun must be burning hydrogen at the rate of 700 million tons per second. At that rate the sun's available supply of hydrogen fuel should give it a lifetime of about 10 billion years. When we examine more massive stars, we come to a rather startling conclusion about their life-span. The most massive stars (50 to 100 times greater in mass than the sun) have luminosities that indicate they are burning their hydrogen a million times faster than the sun. Furthermore, in stars of such size only about half of the hydrogen content is accessible for conversion in the nuclear furnace. The inexorable result is that these stars must effectively burn themselves out within a few million years and in a relatively short time thereafter vanish from the scene.



T TAURI, namesake of the class of very young, unstable stars discussed in this article, is shown in this photograph, made with the 120-inch reflecting telescope at the Lick Observatory. Two wisps of the small luminous nebula (called a Herbig-Haro object) in which the star is embedded can be seen protruding above and below the overexposed image of the star. The four sharp spikes are produced by the diffraction of light around vanes inside the telescope tube.

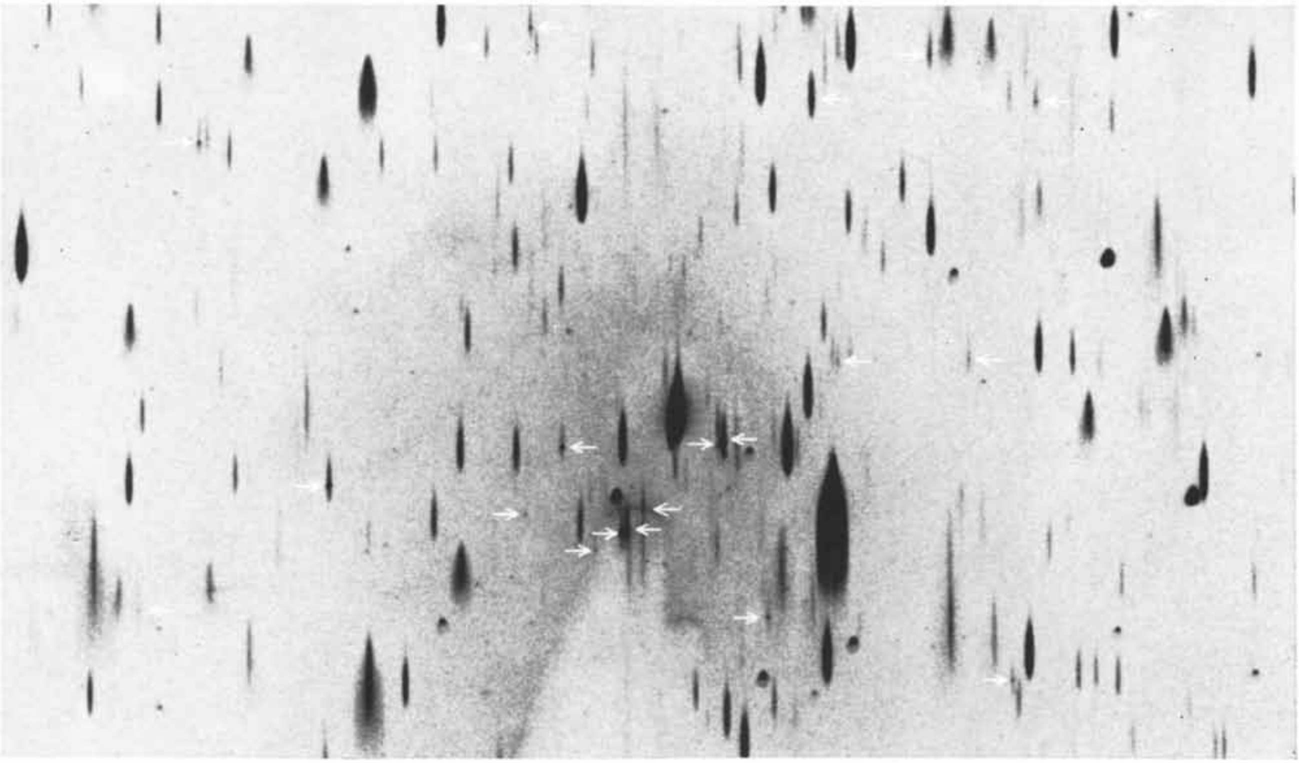
Accustomed as we are to thinking of the stars as permanent fixtures, it is surprising to realize that the brightest present stars—such as Rigel in Orion—cannot have been shining as such when the first men walked the earth. Even more striking are the implications of such a short stellar lifetime. If the population of high-luminosity stars in our galaxy remains approximately constant, as appears to be the case at present, old stars must continuously be replaced by new ones. Considering that there are about 6,000 high-luminosity stars in our galaxy, such replacements must occur at what in astronomical terms is an almost feverish pace—about one bright new star, on the average, somewhere in the galaxy every 500 to 1,000 years. In short, the formation of new stars must be going on *now*—and not so far from us. In the brief lifetime of the very young stars they cannot have traveled far from their places of origin—probably no more than about 100 light-years in the case of a star such as Rigel.

The argument we have just pursued does not, of course, apply only to the massive stars of high luminosity; the numbers are simply more compelling. All the smaller stars, however long-lived, have finite lifetimes, and in the long run they too must die. Star formation must be going on today at all stellar masses, great and small, because of the observed fact that bright stars are usually found together with a large number of less massive but physically related objects formed at about the same time, and always in a region filled with interstellar dust.

Whence comes the raw material that is continuously forming such massive stars or, for that matter, that produces a system such as the Pleiades, a cluster of some 300 stars with a total mass amounting to about 500 times that of the sun? The only known sources that could furnish such huge quantities of matter are the diffuse clouds of dust and gas that lie along the spiral arms of the galaxy. (Another possibility has been suggested: that there may be present in the galaxy very dense and invisible bodies that fission spontaneously and whose fragments become stars. But there is no direct evidence for the existence of such bodies, and there are other serious objections to the proposal.)

The question of the formation of new stars brings us to the particulars of our story. In the early 1940's Alfred H. Joy, the distinguished astronomical spectroscopist of the Mount Wilson and Palomar Observatories, began a systematic study of certain variable stars that exhibited rather peculiar properties. Joy named them the T Tauri stars, after an

T TAURI STARS are among the members of this young star cluster, designated NGC 2264, which was photographed in red light with the 120-inch telescope at the Lick Observatory. The cluster is still involved in the dense cloud of interstellar matter from which it was formed. The cool, dark dusty matter is visible only in silhouette against the background star field; this accounts for the apparent vacancies over part of the area. The hot, bright star just above the center of the photograph ionizes some of the gas in its vicinity, creating the "silver lining" effect on the upper edge of the long finger of dark matter pointing upward from below.



T TAURI STARS ARE DISCOVERED by means of a slitless spectrogram such as the one shown here, which covers the same star field as the photograph of NGC 2264 at the top of the page. In this negative print the thin vertical black streaks are first-order diffraction-grating images corresponding to the red regions of the spectra of the stars in the field. For the T Tauri stars (*arrows*) these streaks represent the red alpha emission line of hydrogen. The extraneous circular spots and hazy streaks are other-order grating images of stars and are to be disregarded. The plate was made with the 36-inch reflecting telescope at the Lick Observatory.

example of this type that had been known for nearly a century. One of their peculiarities was that in spectroscopic examination they showed a spectrum dominated by very intense emission lines. Usually in the spectra of ordinary, well-behaved stars only the dark absorption lines show up, corresponding to wavelengths of light from the star that have been absorbed by the cooler gases of the star's outer atmosphere. We see emission lines in the spectrum of the sun, for example, when a total eclipse by the moon covers the bright solar surface and allows us to detect the light from a thin rim of the solar atmosphere that is not hidden by the moon. This outer fringe of the solar atmosphere was named the "chromosphere" by the early eclipse observers. Now, the distinctive oddity of the T Tauri stars is that they show a fantastic exaggeration of this situation. Their chromospheres are thick, highly active regions whose output of energy is often more than that of the underlying main body of the star; in fact, the emission is often so strong that it masks the ordinary absorption-line spectrum.

The T Tauri stars have other strange properties that we shall consider shortly, but let us note here one circumstance in particular that can be taken as a sign that these stars must be very young. All of them lie in the midst of patches of interstellar dust; they show a striking preference for dust-filled regions in the galaxy, which, as we have seen, are the only places where the raw material for star formation is available in quantity.

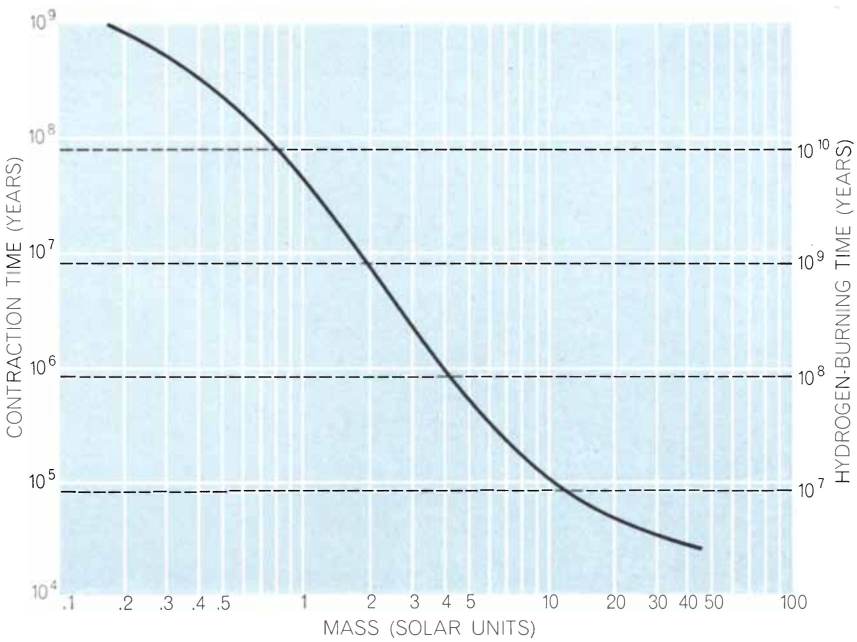
When Joy first reported his observations on these stars in the *Astrophysical Journal* in 1945, noting their peculiarities, no thrill of recognition ran through the astronomical world. Astronomy was simply not ready to appreciate the significance of the discovery. It is fair to say, however, that a decade later it would have been necessary to invent the T Tauri stars if Joy had not already discovered them. In the interval it had become clear not only that the formation of new stars should be a fairly common phenomenon in our own vicinity but also that the T Tauris fulfilled the specifications for very young stars.

To begin with, the T Tauri stars are found in precisely those regions, and only in those regions, where one would expect to find young, newly formed stars: in dense clouds of interstellar material. Their motions show that they are not just normal stars that happen to be wandering through those regions, and there are too many for them to have been gravitationally trapped there. Furthermore, their behavior and strange physical characteristics mark them as intrinsically unusual. Another strong piece of evidence is the fact that they are generally clustered with high-luminosity, short-lived stars whose extreme youth is beyond question on the basis of energy-generation arguments. Moreover, the number of T Tauri stars in the neighborhood of our sun is found to be roughly in agreement with the expected birthrate of new stars, on the assumption that the stellar population is to be maintained at approximately a constant level.

The observed size and surface brightness of the T Tauri stars give physical evidence of their youth, according to the present theoretical concepts of the process of star formation and development. To see why, let us briefly discuss the later stages of the formation process as current theory views it.

Consider a cloud of gas and dust, having about the mass of our sun, that is condensing into a star. When the object has over a rather long time shrunk to about the diameter of our solar system, it is still a comparatively cool, dark cloud. But at that point, as gravitational attraction shrinks it further, a new phenomenon appears: some of the energy released by the contraction begins to go not into heating the gas but into internal work such as breaking up hydrogen molecules and ionizing atoms. This diversion of energy results in a reduction of internal gas pressure below the point required to support the outer layers of the cloud. Consequently the cloud quickly collapses. It shrinks from the size of the solar system to a ball with a radius about equal to the distance from the sun to Mercury's orbit—that is, a size about 100 times the size of our present sun. This collapse, occurring with the velocity of free fall, takes less than half a year. It is finally halted by a buildup of heat and pressure within the cloud that restores the system to structural equilibrium. At that point the embryo star has become visible, with a surface temperature of perhaps 4,000 degrees Kelvin and a luminosity about 100 times that of our present sun. In short, the effect is that a newborn star suddenly appears in the sky. It was A. G. W. Cameron, then at the Goddard Institute for Space Studies in New York, who first recognized that this rapid collapse would take place and, so to speak, culminate in the "ignition" of the star. His suggestion has since been supported by the more detailed studies of Chushiro Hayashi and T. Nakano of Kyoto University.

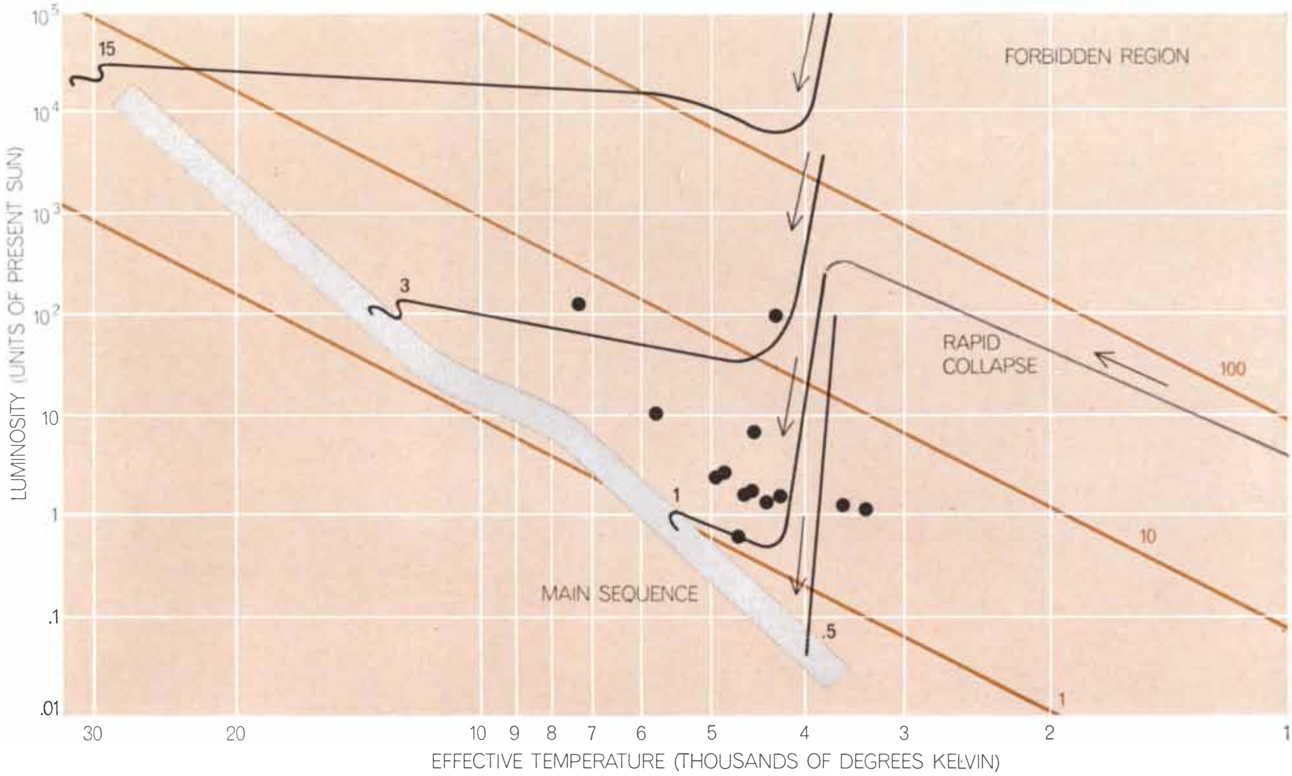
An event that meets the description of a collapse of this kind has actually been observed. In 1936 a new star, since named FU Orionis, made a sudden appearance in a concentration of gas and dust in Orion. A single example cannot, of course, be taken as complete verification of a theory. On statistical grounds we can consider ourselves fortunate to



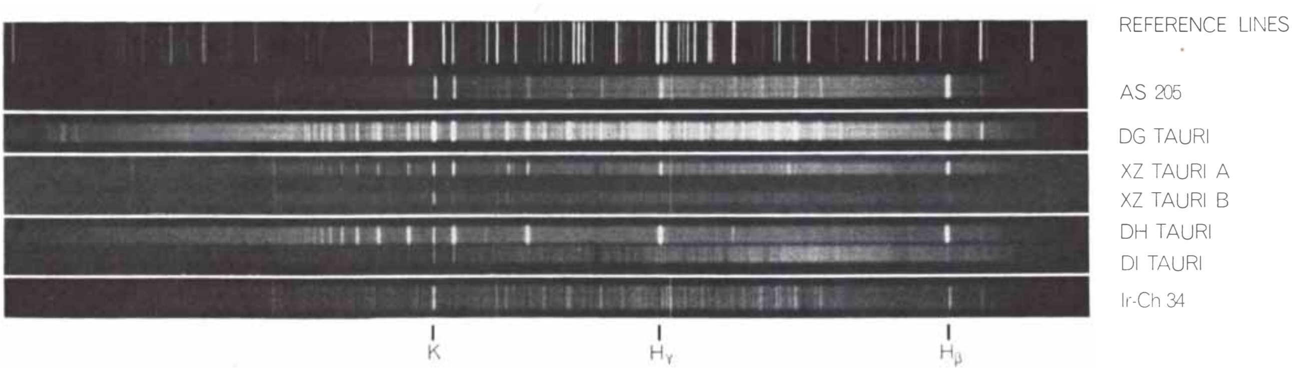
THEORETICAL DEPENDENCE of the time it takes for a star to contract (*scale at left*) on the mass of the star is given by this curve. The same curve is approximately correct also for a reasonable range of hydrogen-burning times (*scale at right*), but these are harder to define precisely, because of the gradual onset of the effects of hydrogen-exhaustion.

be able to witness even one such event with modern equipment, because we expect them to take place in our vicinity on the average of only about once in every 500 to 1,000 years.

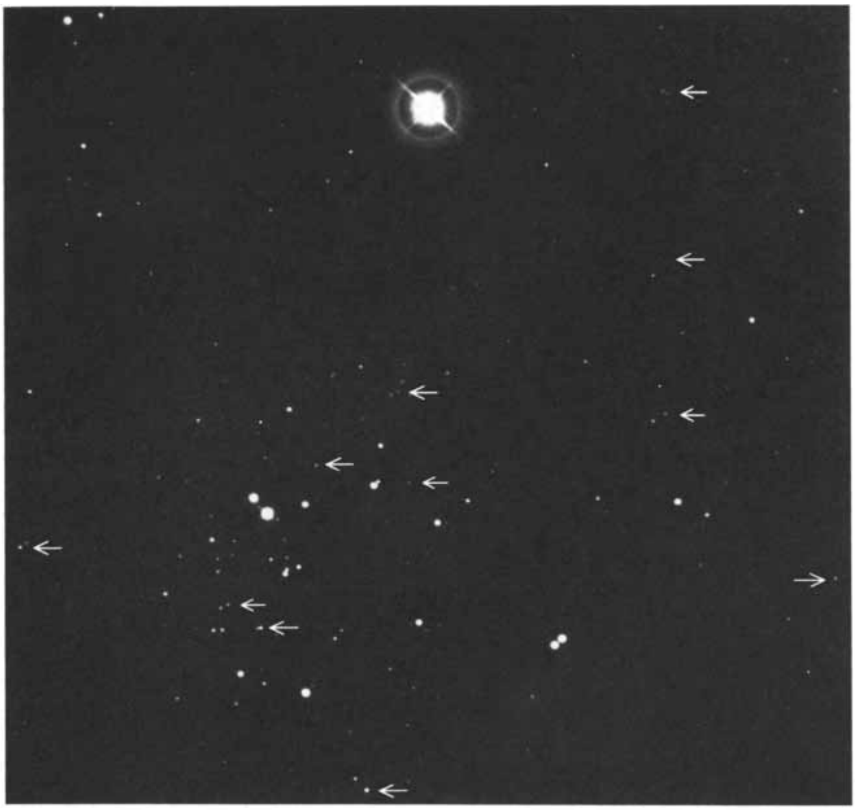
After the star's "birth" its initial development proceeds fairly rapidly, although not nearly so rapidly as the transformation from a dark cloud to a visible star. The young star gradually contracts and for a time diminishes in luminosity. Hayashi has pointed out that in very young stars the thermal energy of the interior must be transported to the surface mainly by the convective movement of rising hot gas. The surface temperature remains nearly constant, but the interior grows hotter as the star contracts. Eventually, when the energy supplied by contraction has raised the temperature at the center of the star to about 10 million degrees K., thermonuclear reactions converting hydrogen to helium become an important contribu-



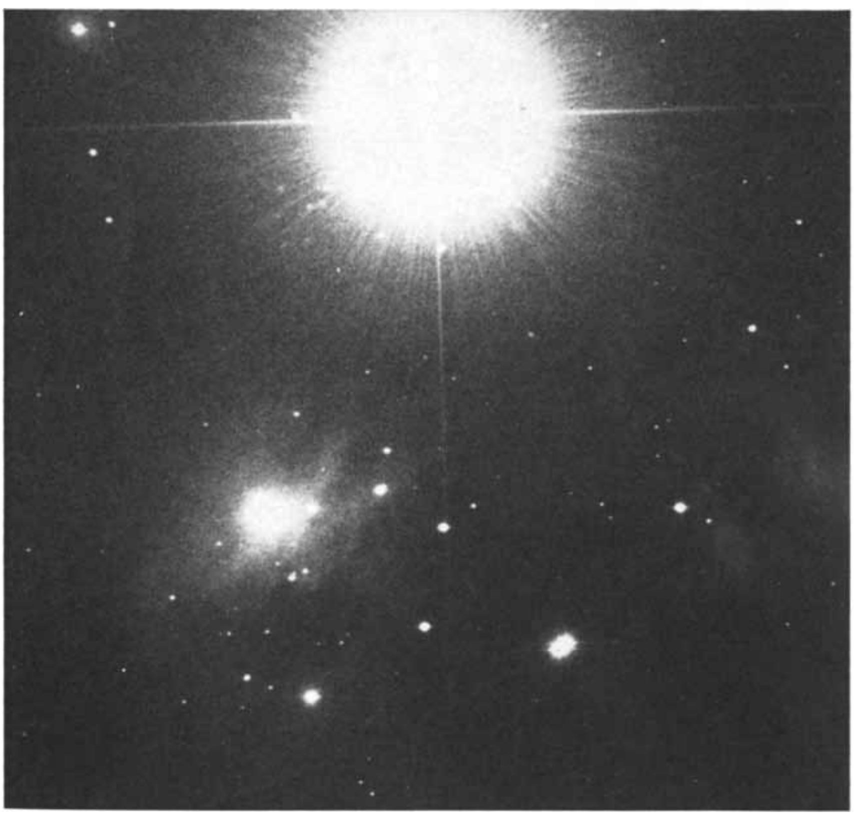
HERTZSPRUNG-RUSSELL DIAGRAM relates the surface temperatures and luminosities of the stars. The black dots give the approximate locations of a number of representative T Tauri stars, which congregate in a region of the diagram populated by contracting stars. The main sequence (*gray band*) is defined by chemically homogeneous stars that have completed their contraction and are now burning hydrogen in their interiors. Along this pathway stars are arranged in order of mass, from small masses at the lower right to large masses at the upper left. The black curves are the tracks followed by stars of various masses as they contract toward the main sequence; the black numbers on these curves indicate the masses in multiples of the sun's mass. The gray line shows the rapid collapse phase of a star of one solar mass during the final 100 days of its roughly 20-year formation period. The slanting colored lines are lines of equal stellar radius; the colored numbers on these lines indicate the radii in multiples of the sun's radius.



SPECTRA of a number of representative T Tauri stars show the bright emission lines that are produced in the active regions of the stars' chromospheres. In one case (DI Tauri) the emission lines are weak and the dark absorption lines of the underlying star can be seen. The "Hβ" and "Hγ" lines are produced by hydrogen; the "K" line is produced by ionized calcium. In several spectra a few extraneous bright lines run across the spectrogram from top to bottom (for example just to the right of the Hγ line). These lines are caused by illumination of the night sky above the Lick Observatory by the mercury-vapor street lighting of the city of San Jose. The bright reference lines across the top are produced by a helium-argon lamp inside the spectrograph.



VERY YOUNG STAR CLUSTER, designated IC 348, contains a number of T Tauri stars (*arrows*). This infrared photograph suppresses the blue light scattered from dust around the bright stars of the cluster and also brings out many cool, faint members of the cluster.



SAME CLUSTER, IC 348, appears in this photograph made in blue light. The very bright blue star at the top, called Omicron Persei, was quite inconspicuous in the infrared photograph. IC 348 is part of the Zeta Persei association, which is only a few million years old.

tor to the star's energy. Thereafter the star still more slowly approaches its final equilibrium state, in which contraction stops and the star's only source of energy is the burning of hydrogen. In a star such as our sun the time required for this maturation—that is, the "contraction time"—is about 50 million years. For the very massive stars the time is much shorter, only about 30,000 years for a star of 30 solar masses, for example.

The course of this development, tracing the slow structural changes and successive equilibrium states of stars of various masses, is now being calculated with increasing sophistication and attention to detail with the aid of large computers. The results are usually plotted on the well-known Hertzsprung-Russell diagram relating the temperatures and luminosities of the stars [*see upper illustration on preceding page*]; some of the most recent work has been done by Icko Iben, Jr., of the Massachusetts Institute of Technology. Such calculations have produced a chronology of stellar evolution that makes it possible to determine the ages of the stars from their observed location in the diagram. It turns out that the T Tauri variables fall in the domain of the diagram that again identifies them as very young stars: in the age range where they must still be contracting and have not yet begun to produce any significant amount of energy by burning hydrogen. They are indeed stars seen shortly after birth, in this case "birth" meaning the act of condensation from interstellar matter. They are stars in transit, so to speak, between interstellar matter and the settled state of a mature, hydrogen-burning star such as our sun.

Joy had observed in 1945 that the T Tauri stars lay in a region of the temperature-luminosity diagram that was nearly empty of normal stars, but at that time the point was given little attention, being regarded as just one of the oddities of these peculiar objects. In the 1950's E. E. Salpeter of Cornell University recognized, in a rough theoretical exploration of the problem, that just such an effect should exist. Meanwhile a number of astronomers—among them Guillermo Haro in Mexico, M. Dolidze in the U.S.S.R. and Pik-Sin The in Indonesia—undertook a diligent spectroscopic search for T Tauri stars, and today more than 1,000 of these objects have been found in the galaxy. Investigations are now focused on attempts to understand their extraordinary properties and behavior.

We have already mentioned one of these peculiarities: the T Tauri stars' unusually thick and highly active outer atmospheres, in contrast to the thin, faint chromospheres of ordinary stars. A second oddity is the fact that, in the case of those T Tauri stars that show absorption spectra, the spectral lines are abnormally wide, because of Doppler-effect broadening. There is no certain way of determining whether the motion producing the line-broadening is vertical mass movement of portions of the star's atmosphere or rapid rotation of the star as a whole.

A third peculiarity of the T Tauri stars is that they are all rapidly ejecting material into space. Apparently gases rising from the surface of these stars are somehow accelerated and forced beyond the domain of the star's gravitational control. (The force responsible for the expulsion is evidently not centrifugal, because the stars cannot be rotating rapidly enough for that.) In the spectra of many T Tauri stars one can actually see absorption lines of this material that are displaced by Doppler shifts, indicating that the expelled material is flowing out of the star at velocities of as much as 200 or 300 kilometers per second. There is no evidence that this material ever returns to the star. If it is completely ejected, these stars must lose a substantial proportion of their mass. L. V. Kuhi of the University of California at Berkeley has calculated that a typical T Tauri star, when in its most active phase of evolution, sheds material at the rate of one solar mass in about 30 million years, and that by the time it matures into a stable star it may have lost as much as a third of its original mass.

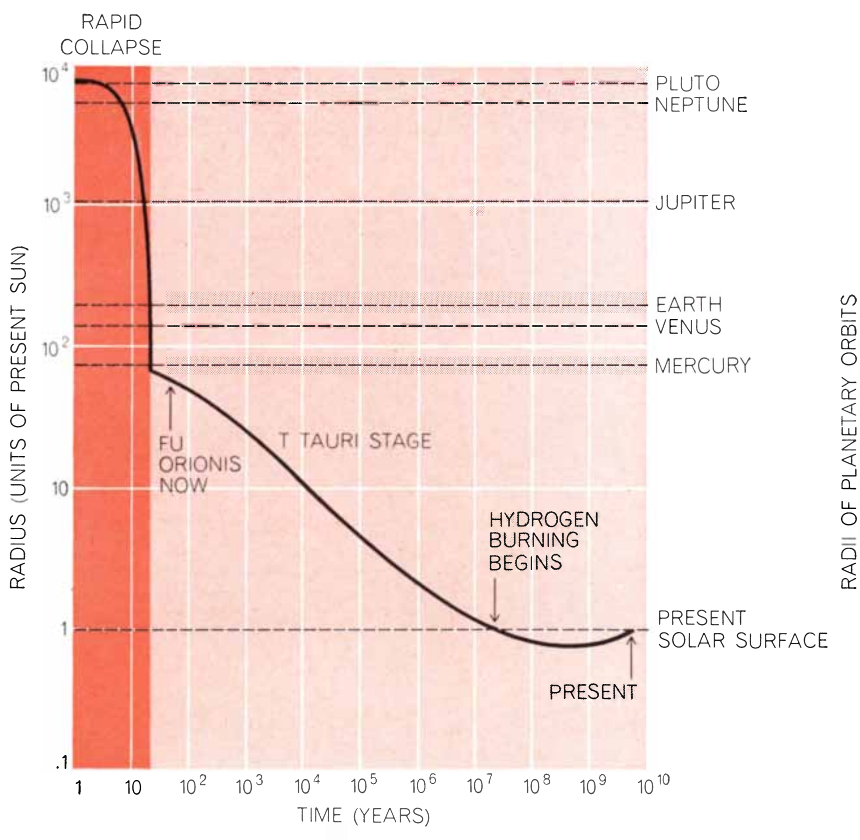
A fourth oddity of the T Tauri variables is that their brightness usually changes in an erratic and unpredictable fashion, showing no regular cycle. Some fluctuate within a few hours; others show no change in brightness for many years. The German astronomer Cuno Hoffmeister has, however, found a tendency to regularity in the fluctuations of several T Tauri stars: they appear to have a cycle length of a few days or a week that is broken occasionally by erratic outbursts but eventually reestablishes itself. Hoffmeister believes, on the basis of close study of these stars, that the cyclical variation in brightness arises from the star's rotation, bringing active areas into view and then hiding them again. If so, the brightness cycle gives an indication of the star's period of rotation, and it is interesting that the rotation rates deduced in this way agree with the rates implied by the hypothesis that rapid rotation is responsible for the Doppler broadening of spectral lines in the T Tauri stars.

A fifth unusual feature of the T Tauri variables is the fact, first discovered spectroscopically by Kurt Hunger of Germany, that they show an extraordinarily high abundance of lithium. The atmospheres of these stars have a lithium content from 80 to 400 times higher than that in the sun's atmosphere, according to estimates by W. K. Bonsack and Jesse L. Greenstein of the Mount Wilson and Palomar Observatories.

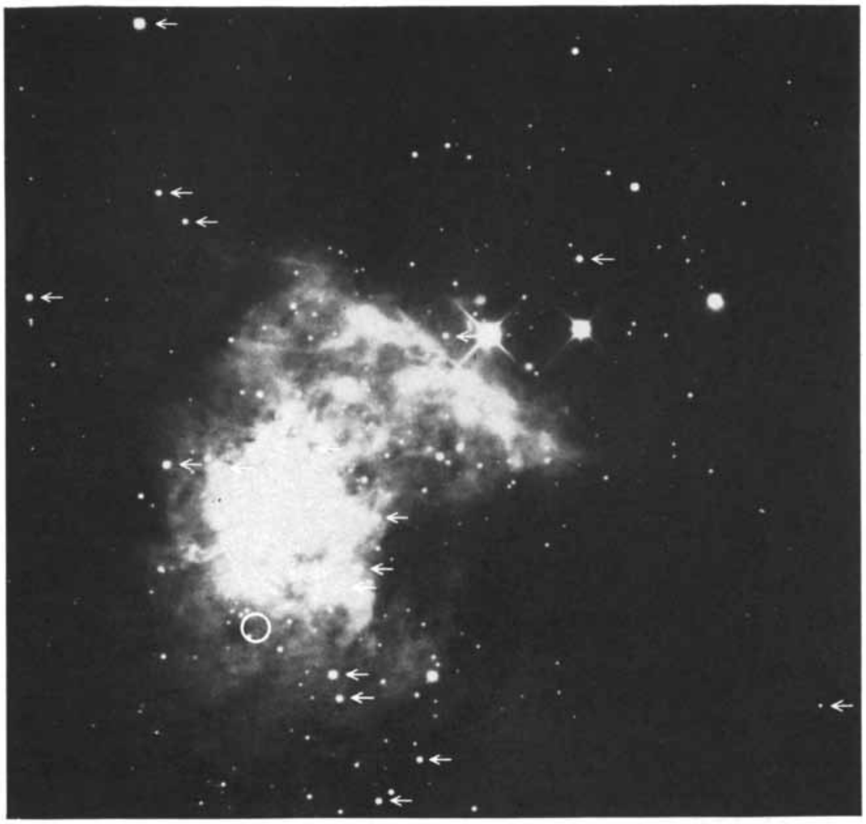
What physical processes or attributes could account for the distinctive features of the T Tauri stars: their extremely active and luminous chromospheres, their massive ejections of surface material, their variability in brightness, their high lithium abundance? None of these phenomena are predicted by the modern theory of the contraction of young stars. Each is still a complete mystery.

The sun is a middle-aged star that presumably passed through the T Tauri phase about five billion years ago (some millions of years after the sun came into luminous existence). Does it still show a few feeble memories of its more active youth? Apparently it does: the activity of the solar surface suggests a faint echo of the enormous surface activity that characterizes the T Tauri stars, and the solar wind may be a trace remnant (reduced more than a millionfold) of the immense ejections of matter in its T Tauri youth.

Seeking to reconstruct the decline of chromospheric activity in such stars, O. C. Wilson of the Mount Wilson and Palomar Observatories arranged normal stars of the sun's type in order of age and found that the younger stars indeed show much stronger chromospheric emission lines than the older ones do. Can the strong emission from the younger stars, and the still stronger emission in the very early T Tauri phase, be con-



RADIUS OF THE SUN is shown at various stages in its history. The initial stage of rapid collapse was completed in about 20 years. The subsequent slow contraction to a stable stage of hydrogen-burning took about 50 million years, the uncertainty arising from the fact that the conclusion of this stage is not well defined. Thereafter the radius has increased slightly as hydrogen has been consumed in the interior. The position shown for the star FU Orionis (30 years after the end of the collapse stage) is only schematic, since its present radius is only about 20 to 25 times the present radius of the sun, not 60 times as the curve indicates. Since the mass of FU Orionis is unknown, it is not possible to say how significant this difference is. The general features of this diagram are probably about the same for other stars not too different from the sun in mass, except that the time scale is compressed for higher masses, as shown in the contraction-time curve on page 32.



T TAURI STARS IN THE ORION NEBULA (*arrows*) **were photographed in near-infrared light with the Lick 120-inch telescope. In this region the nebula is so bright in hydrogen-alpha radiation that the spectroscopic technique usually used for the discovery of T Tauri stars fails. The stars marked here were distinguished by their irregular light fluctuations and are presumed to be T Tauri variables on that basis. The circle marks the location of a large infrared source (called the Becklin-Neugebauer object) that may represent a protostar in the precollapse stage of formation. Theoretically such an object should complete the transition from a dark body to a visible T Tauri star within about 20 years.**

nected to a specific cause? On the surface of the sun, Albrecht Unsöld of Germany has pointed out, the areas marked by particularly intense emission lie where local magnetic fields have more than normal strength, and the decline with age of the chromospheric emission in stars may be due to the decay of such surface fields. One is led immediately to look for strong magnetic fields in the T Tauri stars. Unfortunately an attempt by Horace W. Babcock of the Mount Wilson and Palomar Observatories to detect a field in T Tauri was unsuccessful. This does not necessarily mean that high local fields do not exist; since one must work with the average field over the star's surface, intense local fields could well be missed.

Exploration of the lithium problem has proved to be somewhat more productive. What can have happened to all the lithium the sun must have had when it was a T Tauri star? We have indirect evidence that the sun in the beginning probably did contain substantially more of the element than it does now. This evidence is found in certain kinds of stony meteorites, which can be regarded as samples of the outer material of the primeval sun. In these meteorites the lithium content is at least 35 times higher than it is in the present solar atmosphere. If such amounts of lithium were originally present in the sun, how did so much of it vanish? Current opinion is, briefly, that most of the original solar lithium was ultimately converted into helium by the impact of protons at great depths below the sun's surface, as a consequence of the circulation of surface material through the surface convective region.

Evry Schatzman and Christian Magnan of the Institute of Astrophysics in Paris have studied the question of the earlier development of the prestellar cloud when the lithium itself may have been synthesized by nuclear processes. They recently made the interesting suggestion that a strong flux of energetic protons from the center of the condensing cloud would have ionized a considerable volume of gas around the nucleus. The ionized gas, they pointed out, might be visible as a small, luminous nebula. As it happens, in 1946 a number of small, rather peculiar nebulas that fit the Schatzman-Magnan prescription very satisfactorily had been found in certain interstellar dust and gas clouds. About 40 such peculiar nebulas (called the Herbig-Haro objects) have now been detected, and all of them are located in dust clouds among T Tauri stars. T Tauri itself is embedded in one of the brightest of these nebulas. It seems likely that the Herbig-Haro objects do mark the site of some kind of preliminary activity that may culminate in the appearance of a star.

In 1966 the Mexican astrophysicist Eugenio E. Mendoza V, then working at the University of Arizona, discovered that a large amount of infrared radiation is emitted by the T Tauri stars; this energy is greater than the amount that would be expected to be radiated at infrared wavelengths from stars of that surface temperature. It could be explained by assuming that the extra radiation comes from large clouds of dust, with surface temperatures of about 700 degrees K., lying very near the stars. The observation raises the possibility that the outlying dust, if that is the origin of the infrared excess, may represent material left over from the condensation of the star, perhaps even of the kind from which our planetary system formed.

E. E. Becklin and G. Neugebauer of the California Institute of Technology recently discovered in an infrared survey of the Orion nebula an object that could represent a cool infrared-radiating cloud in the precollapse stage of formation. It is emitting infrared radiation similar to that from the T Tauri stars but much greater in amount. The source of this radiation is invisible in ordinary light and could be explained by a cool, dark body about 1,500 times the size of our sun. That corresponds to the predicted size of a prestellar condensation of the sun's mass just prior to collapse. Hayashi and Nakano have calculated that a cloud of this mass should complete its transition from a large, cool body to a visible star within about 20 years.

Further developments are awaited with the greatest interest. If, within the next decade or two, a new star like FU Orionis suddenly makes its appearance at the site of the Becklin-Neugebauer object, it will be a celebrated triumph for the blend of theory and observation that began with Joy's discovery of the T Tauri stars in 1942.

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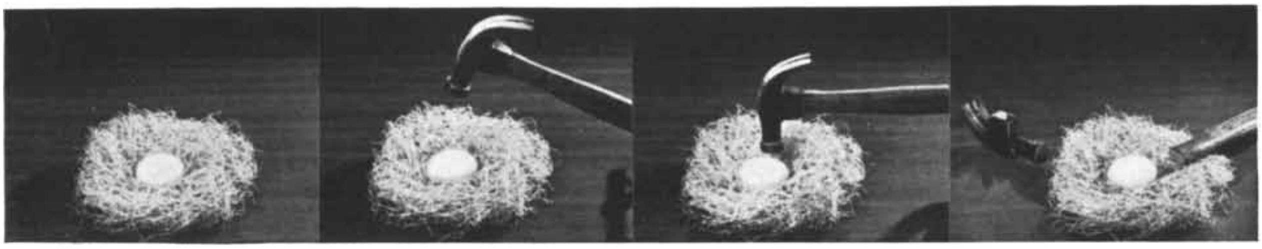

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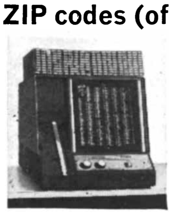

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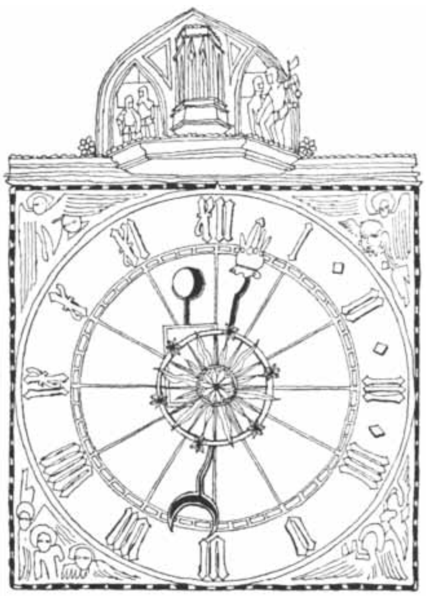

China's Thermonuclear Bomb

Ever since the era of mutual thermonuclear deterrence opened in the 1950's it has been understood that Communist China would eventually develop a hydrogen bomb, and that this would represent a significant turning point and a potential disturbance of the balance of terror. On June 17 China announced that it had "successfully exploded its first hydrogen bomb." The explosion apparently had a yield of at least three megatons.

The speed with which China has developed a thermonuclear device surprised many Western experts. Seven and a half years passed between the first U.S. atomic (fission) explosion in 1945 and the first U.S. thermonuclear (fusion) test. The U.S.S.R. achieved an atomic explosion in 1949 and a thermonuclear explosion five years later. It took less than three years for the Chinese to cover the route. China's first atomic explosion came in October, 1964. Its third test, in May, 1966, was reported to have "contained thermonuclear material." Its fifth test, last December, seemed to be a final step toward a thermonuclear explosion, but U.S. experts assumed that it would be a year before an actual thermonuclear weapon was achieved. The test came in half that time.

China's thermonuclear capability is likely to have an effect on the U.S. debate about defense against long-range missiles and on the prospects for a nuclear nonproliferation treaty. It will increase pressure for the development of a U.S. antimissile system designed to defend against at least the "light" attacks of which China is expected to be capable once it has produced long-range missiles. It may increase the desire of the U.S. and the U.S.S.R. to conclude a treaty to prevent the spread of nuclear weapons, but at the same time it is likely to increase the reluctance of many smaller powers to accede to such an agreement.

Bigger Dishes

Four universities have formed a consortium to design, build and operate a 328-foot (100-meter) radio telescope that would be the largest fully steerable instrument of its kind in the world. The institutions are the California Institute of Technology, Stanford University, the University of California and the University of Michigan. Joining as Associates for Radio Astronomy, they propose to build the telescope at the Owens Valley Radio Observatory in California, which is operated by Cal Tech. The cost of the telescope is estimated at about $19 million, part of which Associates for Radio Astronomy will seek from the Federal Government.

Large radio telescopes are particularly useful for the kind of radio spectroscopy that works with the faint signals from atoms and molecules in interstellar space. Associates for Radio Astronomy has said its telescope would also be used "to study the rotation rates of planets and to pinpoint the locations of radio objects." The largest steerable radio telescope now in operation is the 250-foot antenna at Jodrell Bank in England. (A 600-foot "dish" that the U.S. Navy began building in Sugar Grove, W.Va., several years ago was not finished because the work ran into technical difficulties.) The Associates for Radio Astronomy's 328-foot telescope would conform to a recommendation made two years ago by a panel of the National Academy of Sciences, which in outlining a 10-year program for ground-based astronomy called for "two large parabolic steerable antennas of the 300-foot class." A plan for a 450-foot radio telescope is being developed by a group of New England institutions—Harvard University, the Massachusetts Institute of Technology and the Smithsonian Astrophysical Observatory—operating under the collective name of the Cambridge Radio Observation Committee. This instrument may be built under a huge plastic dome (transparent to radio waves) to protect it from the wind.

Oceanography Applied

A Federal program designed to encourage applied research in oceanography has been set in motion by the National Science Foundation. Based on the National Sea Grant College and Program Act, which was passed by Congress last year, the program will make grants in two distinct categories: to institutions and to projects. The Administration has asked Congress to appropriate $4 million for the grants in the current fiscal year and is expected to seek about $15 million next year. The program is one of several that the Federal Government operates to encourage marine research; its particular distinction, according to an official at the National Science Foundation, is that it is the first aimed at developing the technology for "making money from oceanography."

Institutional grants under the program will be called National Sea Grant College support. The National Science Foundation said they will go to institutions "engaged in a broad-based program...related to development of marine resources." Individual grants will be called Sea Grant Projects and will go to "individual projects designed to support a specific aspect of marine resource development," the foundation said. Most of the projects, according to the agency, "will be supported by a single grant for one or two years," but "continuing support may be available for a reasonable length of time for projects of outstanding merit and promise."

Combustion without Pollution

A system for generating power indirectly from fossil fuels without polluting the atmosphere has been proposed by an engineer at the Lockheed Aircraft Corporation. According to the new concept the fossil fuels (coal or natural gas) would not be burned directly but would serve as raw materials for the synthesis of a "clean" fuel by a procedure that would employ nuclear energy in the processing step. The clean fuel produced would be ammonia (NH_3), whose complete combustion in air yields nitrogen gas (N_2) and pure water (H_2O).

The author of the proposal, Leon Green, Jr., outlines his scheme in a recent issue of *Science*. He writes that "to remove the offending elements (carbon and sulfur) from the fuel prior to combustion is a much more efficient and less expensive procedure than trying to clean up the combustion products." In the large-scale commercial production of ammonia, natural gas is used as the raw material for a "steam re-forming" process, which generates hydrogen for the synthesis reaction. In the course of this process sulfur and carbon dioxide are removed from the gas. The cost of manufacturing ammonia in such a plant is about three-quarters of a cent per pound, and with a scale-up of production the cost would be even lower. Moreover, Green points out, since the largest factor in this cost, aside from the raw material, is the generation of heat for the re-forming process, the cost would be reduced still further by the use of a nuclear reactor as the source of heat.

Among the "obvious practical difficulties" in the use of ammonia as a fuel for private automobiles, Green cites the problems of distribution and handling, incomplete combustion and toxicity in confined spaces. Nonetheless, he does not rule out the eventual development of practical turbine-powered automobiles operating on ammonia. It is in the area of stationary power plants and large turbine-powered vehicles such as buses, trucks and trains, however, that he believes the problems of operation with ammonia should be minimal. For example, he estimates that the fuel cost for a "modest-sized" 100-megawatt electrical plant, consuming some 2,100 pounds of ammonia per minute, would be just under 10 mills per kilowatt hour, not taking into account the sale or use of the products recovered from the ammonia synthesis or the combustion process. For such stationary power plants Green believes that his concept "appears feasible technically and perhaps economically as well, depending upon the economic value of the by-products of sulfur, carbon dioxide, water and possibly nitrogen, and upon the price we are willing to pay for a clean environment."

Radar and Vegetation

Experiments are in progress to determine the usefulness of radar in large-scale surveys of vegetation, both cultivated and natural. The work is described in *BioScience* by Richard K. Moore and David S. Simonett of the University of Kansas, who see a potential for radar "in agricultural surveys, in mapping natural vegetation and in forest surveys." The possibility arises from the fact that different kinds of vegetation return a radar's signals with different degrees of intensity, so that the signals as recorded on film have distinct variations in brightness.

The technique uses side-looking airborne radar and for that reason has been given the acronym SLAR. Used over an agricultural test site in Kansas radar distinguished 85 percent of field boundaries and clearly discriminated sugar beets from other crops. Over natural vegetation in three areas, the authors say, "the ability of a trained radar operator to use the radar imagery to detect vegetation boundaries and, along with other information, to infer gross vegetation types has been demonstrated to some degree."

Tigris to Danube

It begins to look as if a single system of writing, or at least a set of similar signs used in writing or in magic, may have reached all the way from Mesopotamia to Crete and southern Europe as early as the end of the third millennium B.C. Writing in the journal *Antiquity*, the English archaeologist M. S. F. Hood reaches this conclusion on the basis of his study of inscribed clay tablets unearthed in the prehistoric settlement of Tartaria in Romania.

Some 50 years ago archaeologists had noted that signs on pottery found at Vinča in the Danube valley near Belgrade and at Tordos in Romania were similar to signs from Troy, Egypt, the Aegean island of Melos and Knossos in Crete. The idea grew that a single language system might have spread from Egypt northward, but the evidence was scanty. In 1961 N. Vlassa of the Historical Museum at Cluj in Romania, excavating the Tartaria site, discovered three clay tablets among ashes in what appeared to be a ritual pit. He found that their inscriptions were remarkably similar to those on some early Mesopotamian tablets. Now Hood reports that some of the Tartaria signs are even closer to some of the earliest writing found in Crete and that the tablets are also comparable in shape to some from Knossos. On the basis of this archaeological finding and others, Hood proposes that the ideas of writing represented by these signs spread from Mesopotamia to Syria and Cilicia and thence by sea to Crete and also, via Troy, to the Balkans.

Hood recognizes a serious dating problem. Primarily on the basis of archaeological correlations with Troy, the pertinent Vinča-Tordos and Tartaria levels seem to date from about 2200 B.C. Hood believes the features common to Troy and the Balkans almost surely moved from south to north, not the other way. In that case (and indeed even if they moved south) it is hard to square the archaeological dating with radioactive-carbon evidence that the Balkan culture began before about 4000 B.C. "There is something of an impasse here," Hood writes, "which time will no doubt resolve."

Under the Spreading Sea Floor

The hypothesis that the floor of the oceans has been spreading seeks to explain some characteristics of ocean basins and the continents by supposing that material welling up from the interior of the earth forms mid-ocean ridges and then, as new material rises, moves outward, away from the ridges. The hypothesis has been strengthened recently by the discovery that bands of alternating normal and reversed magnetism parallel the mid-ocean ridges, apparently indicating upwellings of molten rock during different magnetic "polarity epochs" (see "Reversals of the Earth's Magnetic Field," by Allan Cox, G. Brent Dalrymple and Richard R. Doell; SCIENTIFIC AMERICAN, February). Now studies of the depth of sediments near mid-ocean ridges by workers at the Lamont Geological Observatory suggest that the spreading process is intermittent, and that the present cycle began some 10 million years ago after a long period during which the sea floor did not move.

Examining a number of profiles of sediment depth obtained by seismic traverses of different mid-ocean ridges, John and Maurice Ewing of Lamont found a similar pattern in each case. There is a very thin layer of sediment or none at all on the crest of the ridge, extending out to between 100 and 400 kilometers. Then there is a discontinuity, an abrupt increase in depth, beyond which the depth remains constant or increases only very slowly with distance. Dividing the distance to the discontinuity in each area by the rate of spreading there (from one centimeter per year in the North Atlantic to four centimeters per year in the Pacific, based on the magnetic-anomaly patterns) gives a date of about 10 million years ago for the occurrence of the discontinuity in all areas. The discontinuity could be explained by a sudden, brief and very large increase in the rate of sedimentation everywhere in the world at that time, but such an increase seems unlikely.

In an article in *Science* the Ewings

PATENT NO. 3,260,902: ISOLATED BURIED LAYER TRANSISTORS FOR INTEGRATED CIRCUITS

suggest that the explanation lies in a change in the rate of spreading rather than in the rate of sedimentation. They suggest that an early cycle or cycles of spreading, accounting for the movement of the continents, ended roughly 100 million years ago and was followed by a period during which the ocean floors were quiescent. Material deposited during this period on the static crust of the earth accounts for most of the currently observed sediments. Then, about 10 million years ago, a new spreading cycle began. The discontinuities mark the locations of the ridges at that time; the thin sediments of the ridge crest have been laid down since then.

Deep Divers at Work

Six divers working out of a submerged chamber have demonstrated that prolonged diving operations can be conducted in water as much as 600 feet deep. The designers of the system hope to extend its effectiveness to 1,000 feet by 1969 and to 1,500 feet within five years. Scuba divers can go to a maximum of about 250 feet and can stay at that depth for only a few minutes; the deepest that divers receiving their air from the surface can work effectively is about 300 feet.

The six divers went to 600 feet in the Gulf of Mexico, using a technique designed by the Westinghouse Electric Corporation and called the Cachalot system (from the French name of the deep-diving sperm whale). Its key components are two chambers—the submersible one, which is nine feet high and five feet in diameter, and a larger surface chamber that stays on the mother vessel and is used to prepare the divers for the pressure they will encounter in the water (300 pounds per square inch at 600 feet) and to decompress them when they return to the surface. At the start of an operation the two chambers are joined. Divers enter the surface chamber, undergo compression and move into the diving chamber, which is then detached from the surface chamber and lowered into the water by a crane. On the bottom the men move out of the diving chamber to the work site, where they can stay for as long as six hours. They wear "wet suits" and draw their air from tanks attached to the diving chamber, which remains near them. At the end of the work period they are lifted to the mother vessel and move into the surface chamber, where they either stay under compression until their next shift below or are decompressed. In a typical assignment four divers remain under compression for a week's work and four other divers replace them the following week. The system has already been used for three work assignments at depths of up to 235 feet: repair of steel trash racks at the bottom of the hydroelectric dam at Smith Mountain Lake in Virginia; salvage of damaged petroleum-production platforms in the Gulf of Mexico, and preparation of pilings for a bridge in Narragansett Bay. The jobs entailed a total of 5,000 hours of work by divers on the bottom.

The Temptation of Dr. Johnson

Was Samuel Johnson an alcoholic? A prodromal alcoholic at least, J. S. Madden maintains in the journal *Medical History*, that is, he was on the edge. There is ample evidence that the 18th-century writer and lexicographer had trouble moderating his consumption of alcohol and that he therefore laid off drink for long periods. "I can't drink a little child, therefore I never touch it," he once said. "Abstinence is as easy to me, as temperance would be difficult." On another occasion a hostess countered his refusal to take a drink by urging, "I am sure, sir, you would not carry it too far"; Johnson replied, "Nay, madam, it carried me." Madden finds that Johnson was a hard drinker until about 1739, abstained for a few years, resumed heavy drinking until about 1765 and abstained again until three years before his death in 1784. He was not often seen drunk, partly because he drank alone—another sign of alcoholism. Still, he was probably not a fully developed alcoholic; drinking did not ruin his marriage or his career, and he was in fact sometimes able to drink just a little.

Why did Johnson drink? First, his was a hard-drinking era; inordinate wine consumption was prevalent among the gentry. (Johnson said: "Claret is the liquor for boys; port for men; but he who aspires to be a hero must drink brandy.") Johnson certainly had emotional problems that could have led him to drink. Madden finds that he had a "permanent depressive outlook of a neurotic nature," tics, compulsions, obsessional doubts, insomnia, fear of insanity and a morbid dread of death. And there were non-psychological stresses: early poverty, grotesque appearance, deafness, gout and emphysema. Madden credits Johnson's strong personality and high intelligence with saving him from destructive alcoholism and concludes that "his lonely, largely successful combat with alcohol . . . affords one more ground for revering this great and lovable man."

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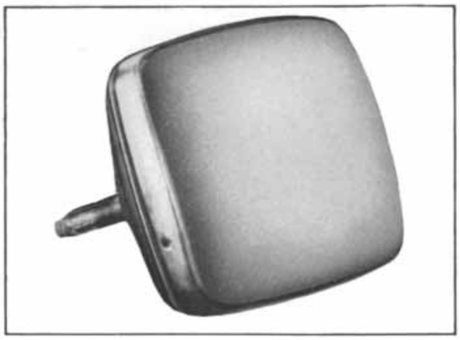

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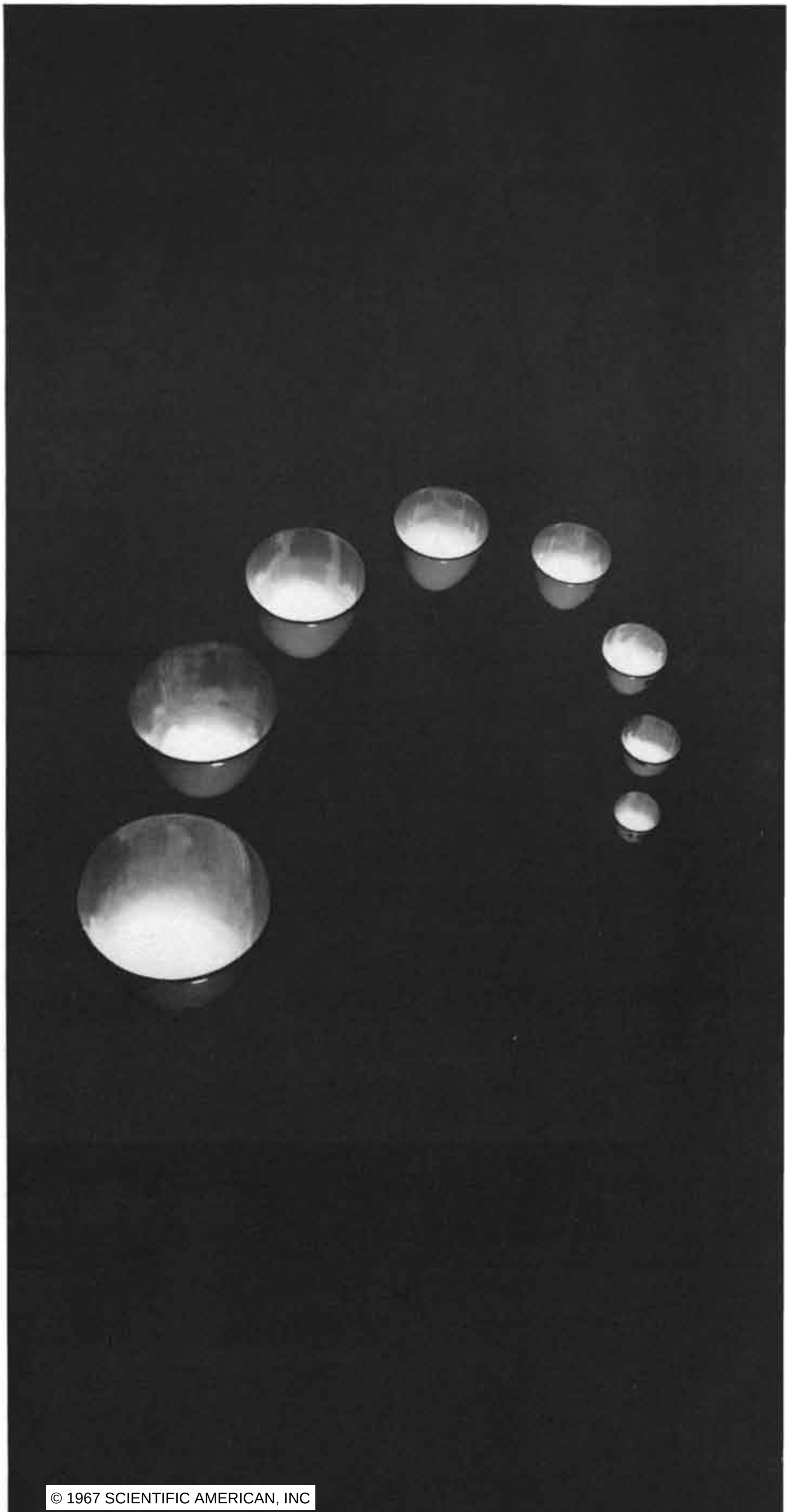

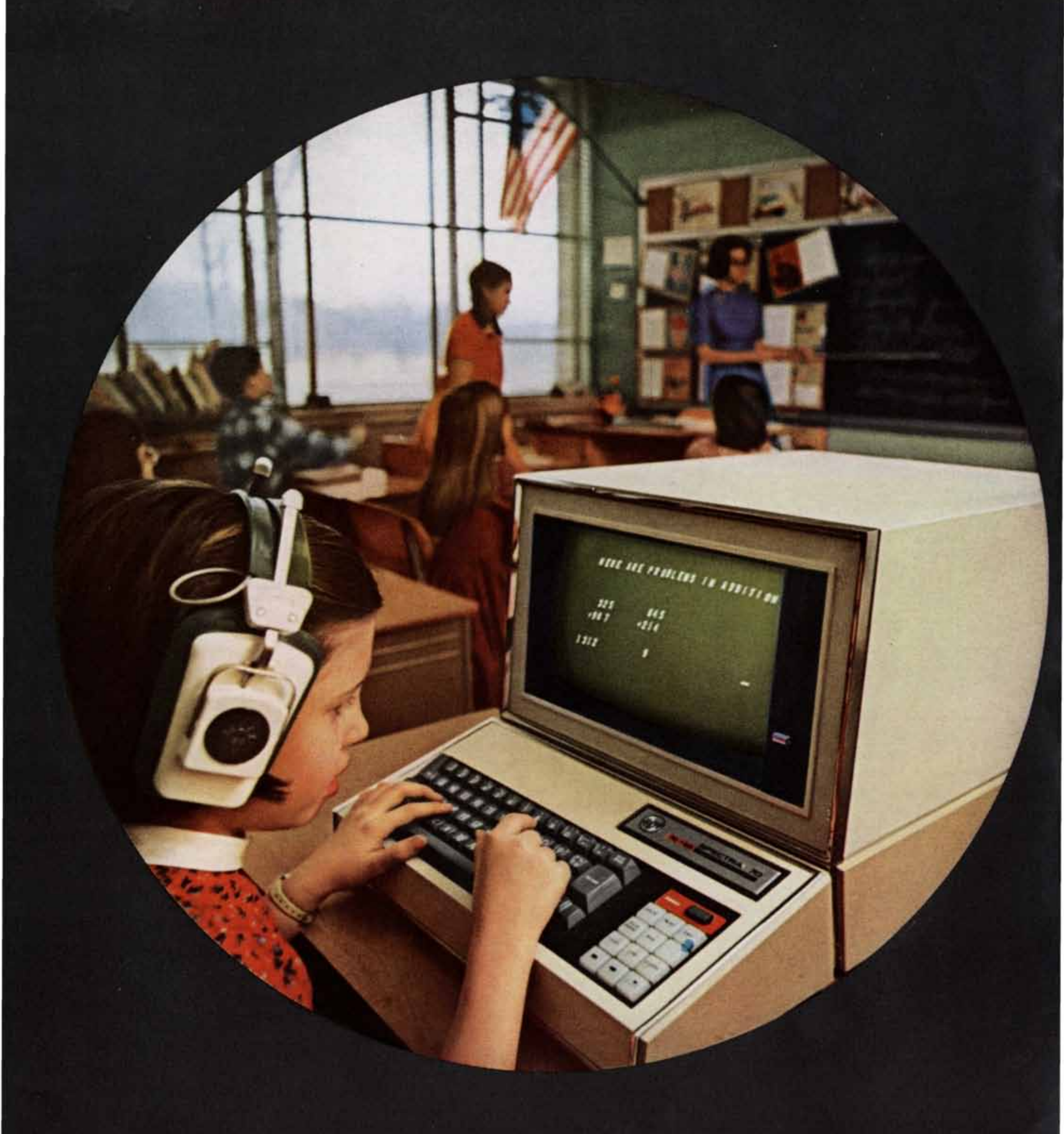

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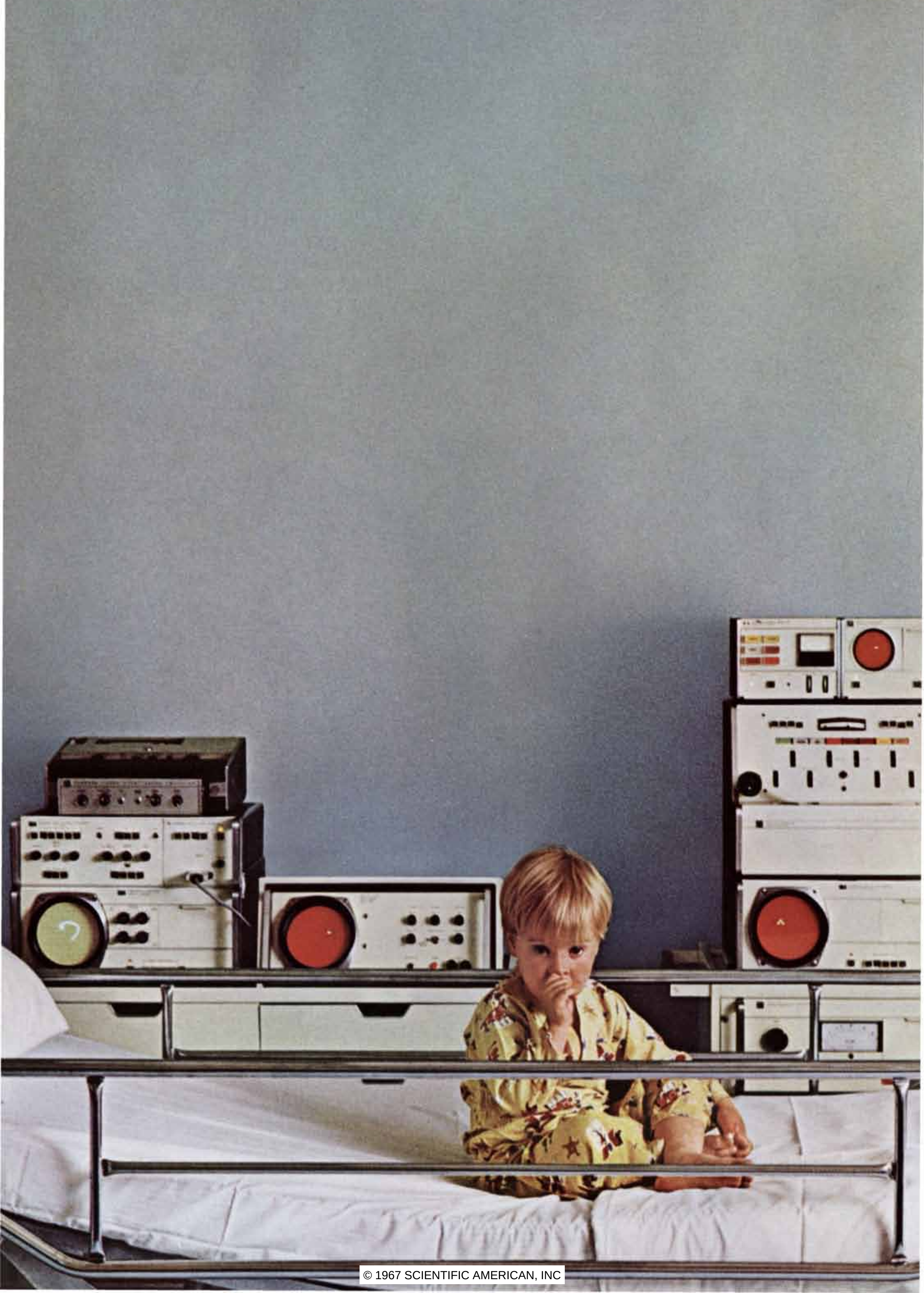

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MECHANICAL HARVESTING

The fact that less than 5 percent of the U.S. labor force is engaged in agriculture reflects a high degree of mechanization. The machines harvest not only grains but also such crops as fruits and vegetables

by Clarence F. Kelly

Last year in the U.S. there was harvested more than 200 million tons of grains, 220 million tons of hay and silage, 21 million tons of vegetables, 20 million tons of fruit, 62.5 million tons of milk, 3.7 million tons of cotton, 106,500 tons of wool and similarly large quantities of other agricultural products. If all this produce had had to be harvested by human hands, probably the entire U.S. population would not have sufficed to perform the job.

The fact that the production of these amounts of food and fiber engages only 5 percent of the U.S. labor force is primarily due to the mechanization of farming. Other technological developments—chemical fertilizers, pesticides, plant breeding and so on—make essential contributions, but mechanization is still the outstanding factor. Among the functions of mechanization none is more important than its role in the harvesting of crops. The picking and winnowing of a crop usually accounts for at least half of the total cost of production. It is also by far the most difficult part of the agricultural process to mechanize. Nevertheless, the mechanization of harvesting in the U.S. has made such strides that, in spite of the costliness of the machines and other technical aids, the cost of food to American families, in terms of its percentage (18 percent) of their income, is the lowest in the world.

We are still a long way from complete mechanization of this sector of agricultural activity. The harvesting of fruit, for example, is still done largely by hand. Research and ingenious new ideas, however, are rapidly adding one important crop after another to the list of those that can be picked by machine. Harvesting is a critical field for research. The economic condition of the nations and peoples of the world is determined in considerable part by the stage they have reached in harvesting efficiency.

The Saving of Labor

Man began his agricultural career by picking wild fruits and nuts and digging roots. When he progressed to planting grain, he harvested it by hand, stalk by stalk, and centuries may have passed before he advanced to using a crude knife to lop the head of grain from the plant. This tool evolved into a curved knife, or sickle (whose use depended on cultivating grain plants that would ripen together instead of at separate times), then a scythe, then a scythe with an attached cradle that would catch the stalks as they were cut and thus collect them in a sheaf for threshing.

This hand tool essentially represented the state of harvesting technology less than two centuries ago. In 1790 agriculture occupied 90 percent of the U.S. population. Half a century later the proportion had dropped to 70 percent, and by 1890 only 43 percent of the people were farmers. The principal events responsible for this trend were John Deere's production of the steel plow, Cyrus McCormick's invention of the reaper (which he began to manufacture in 1847) and John Appleby's development (in 1879) of the grain binder. After 1890 the substitution of machines for manpower on U.S. farms proceeded at a much more rapid rate. The farm population declined to 30 percent in 1915 and to 18 percent by 1940. The gasoline engine was, of course, the prime factor, and it became the motive power of a variety of mass-production harvesting machines: the self-propelled wheat combine, the hay baler, the cotton and corn pickers. Since World War II a proliferation of other harvesting devices, handling perishable as well as nonperishable crops, has been partly responsible for the further reduction of the farm labor force to the present 5 percent.

The harvesting of grains is now totally mechanized, and that of roots and tubers is almost wholly so. For efficiency's sake a harvesting machine should not be a mere accessory for human hands; it should perform the full task. This sometimes calls for a high degree of specialization in the machine. For certain crops there are now machines that are guided by sensing mechanisms and duplicate almost exactly the motions of the human hand. The design of a harvester must often be adapted not only to the crop but also to the vagaries of nature. For example, a sugar beet harvester that works well in the peat soils of the Sacramento River delta is helpless in the heavy, sticky soils of Ireland; a grain combine that is beautifully suited to harvesting wheat in the North Dakota prairie cannot operate in the wet rice fields of Arkansas unless its drive is changed to the crawler type; a berry harvester that picks raspberries efficiently in Oregon cannot be used in the San Joaquin Valley of California because of differences in the climate and in the berry varieties.

Let us look at the principal machines and see what their development entailed. By far the most important, of course, is the grain combine.

Grain

Analyzing the functions to be performed by the reaper (the original element of the combine), McCormick found that he had to solve seven problems: avoiding side draft (the tendency of the machine to pull sideways), holding the stalks for cutting, separating them from the stalks left standing at the end of the

COTTON PICKER harvests two rows of cotton at a time at a maximum speed of 3.3 miles per hour. The machine uses rotating spindles to twist the cotton fibers out of the bolls. The basket at rear can hold up to 3,000 pounds of cotton before it has to be dumped.

cutter bar, pushing the stalks to be cut against the knives, cutting them, catching the fallen stalks and providing a power drive for the necessary operations. The apparatus McCormick developed to deal with these problems consisted of a main ground wheel that furnished the power and carried most of the machine's weight (to minimize side draft), a vibrating horizontal knife, a divider at the end of the cutter bar, fingers to hold the stalks, a reel to push the stalks against the knife and a platform to catch the cut stalks, with the heads of the grain falling toward the rear of the machine.

The reaper was only a first step toward fully mechanical harvesting. The grain stalks still had to be bundled in the field by hand, then be picked up and hauled to the stack or threshing machine. Mechanization advanced a substantial step when the reaper was equipped with a binder that bundled the grain mechanically. The final step to complete mechanization was the invention of the modern combine, which combines reaping and threshing in the same machine. This apparatus eliminates several operations: the grain no longer has to be bundled, stacked and transported to the thresher. The grain combine, to be sure, introduced new problems. Because the grain is threshed as it is collected, it must be fully ripe. The ripened grain in the field therefore runs the risk of being shaken from the stalks by wind before the harvester can cut it. Moreover, if the threshed grain is not uniformly ripe, pockets of moist grain in the storehouse may quickly spoil. These problems have been considerably reduced, however, by countermeasures in plant breeding and cultivation.

Today the combine has become so versatile a machine that it is used to harvest not only grains such as wheat but also grass seed, beans and even corn. The machine carries attachments and a variety of adjustments that enable the operator to adapt its operations to the crop or even the particular condition of a crop. By raising or lowering the cutter bar, by changing the speed of the various machine elements and by other adjustments he can allow for the height of the plants, the slope of the land, the length of the stalks, the moisture content of the kernels, the size of the kernels (from the smallest grass seed to the largest beans), the presence of weeds, even the direction and force of the wind. The machine is designed to garner the full yield of the field without damaging the crop or collecting extraneous material.

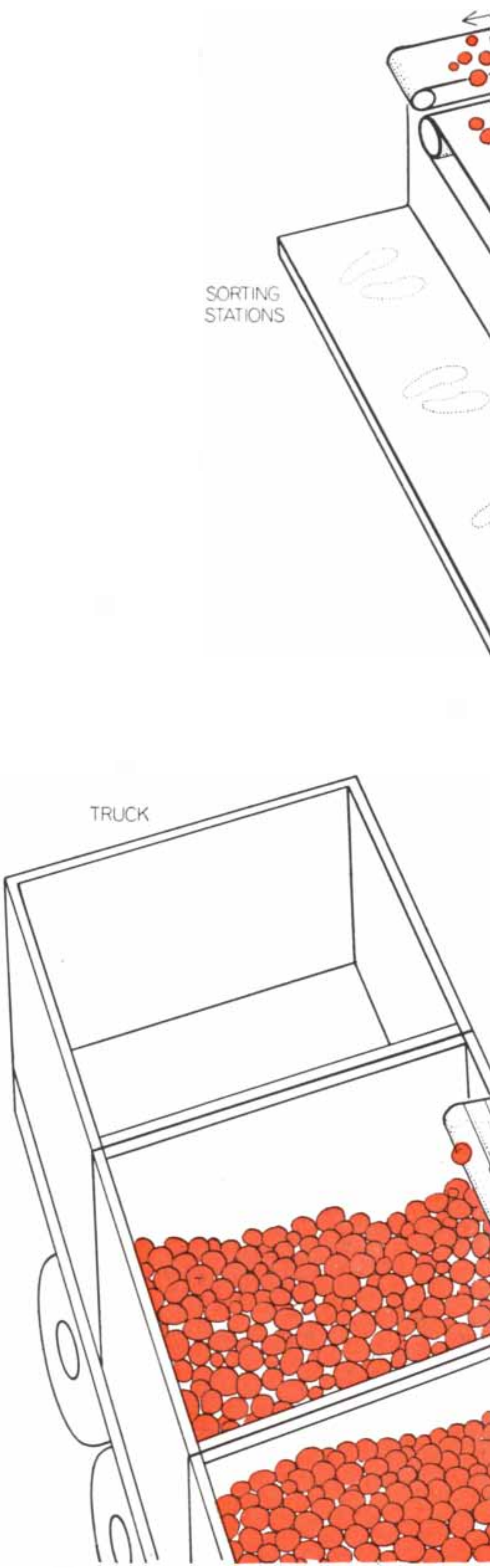
Research is going forward on further refinements of the combine. The aims are to reduce the cost of the machine and its power requirements, to design automatic controls that will keep it operating at its most efficient capacity and thresh out every last kernel of grain and to incorporate a drier that will reduce the moisture content of the grain as it is reaped. Meanwhile plant breeders are working to adapt the plants more closely to the machine, seeking to develop varieties that will have a higher ratio of grain to straw, that will dry more evenly in the field, that will withstand more wind without shattering and that will produce more grain per acre.

The usefulness of the present combine can be measured by the fact that with this machine in California the harvesting of rice (reaping, threshing and hulling) requires less than one man-hour per acre, whereas in Japan, where the work is done largely by hand, the average labor expenditure according to a recent study is 258 man-hours per acre. It is a significant commentary on the world food problem that rice, the largest crop on our planet, is still harvested by hand, stalk by stalk, in most of the countries around the world.

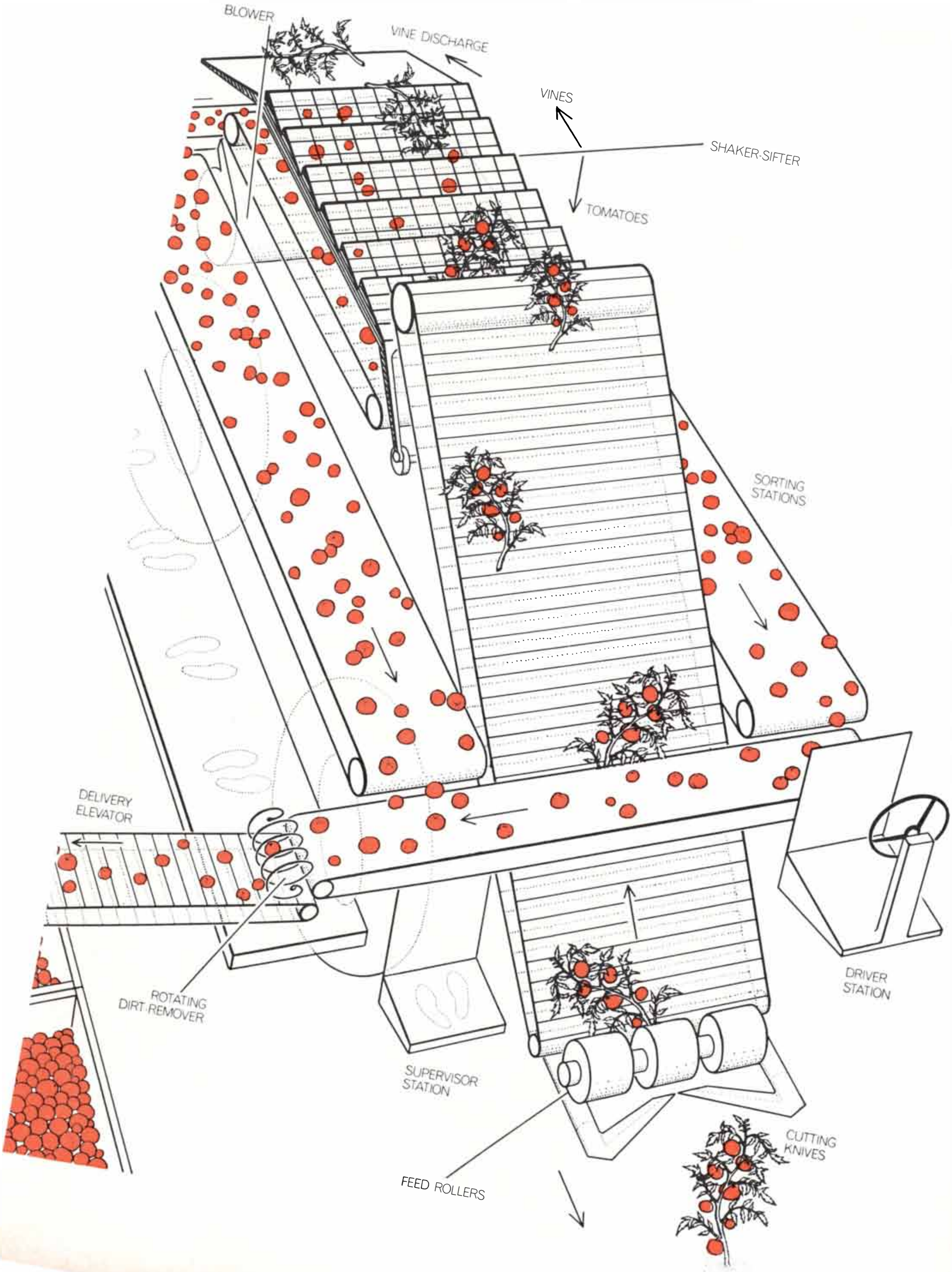
Hay

Just as the combine has revolutionized the harvesting of grain, other machines have transformed the production of hay. In acreage and tonnage (more than 120 million tons a year) hay is the largest U.S. crop, and in dollar value (about $2 billion) it ranks second only to wheat. An important harvester of this crop, principally alfalfa, is of course the cow. Animal husbandry has always depended in large degree, however, on the mowing and storage of dried grass as feed, not only for seasons when pastures are dormant but also for areas where there are no pastures.

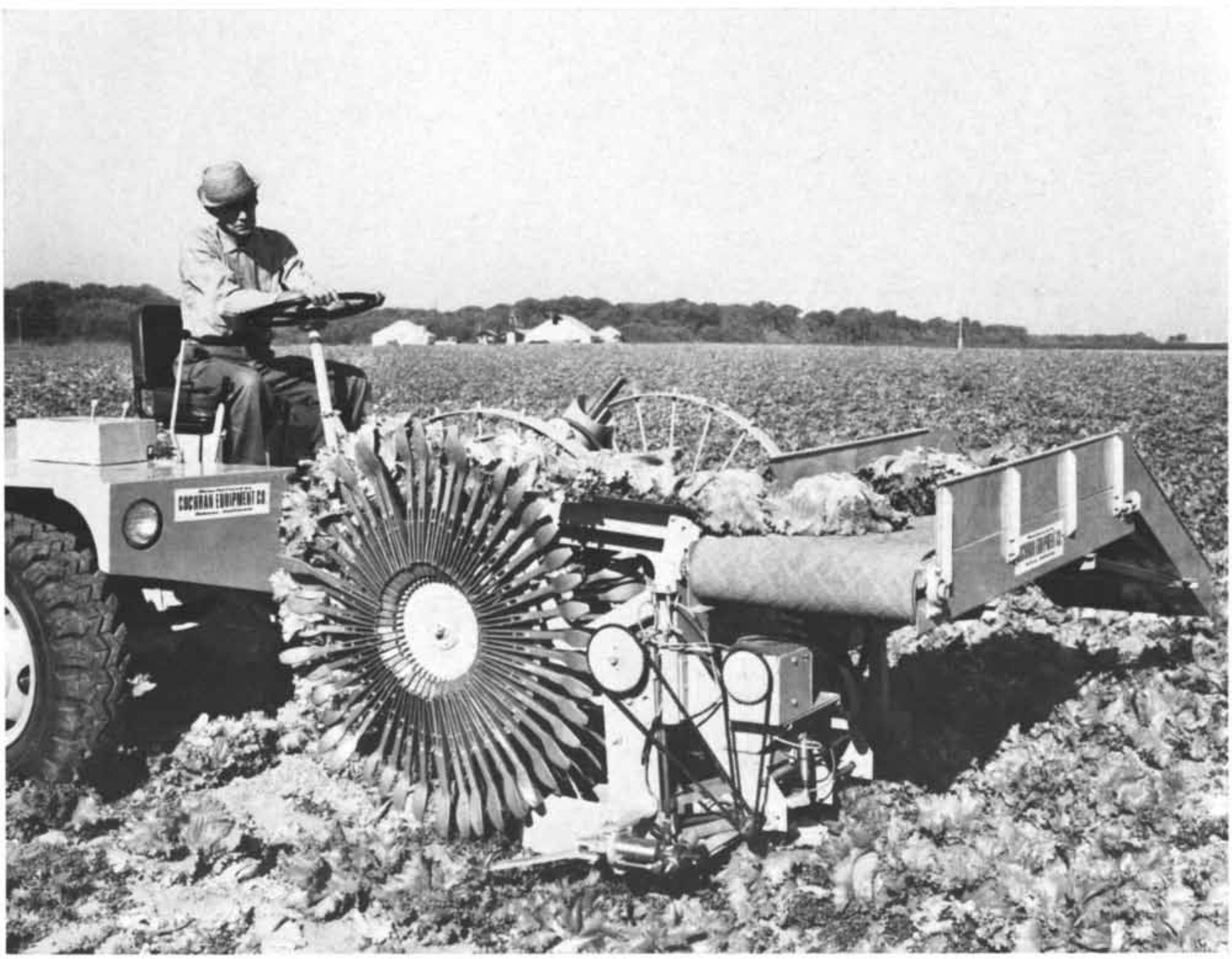
The traditional method of making hay,



TOMATO HARVESTER has transformed tomato growing from total reliance on hand picking to largely mechanized harvesting within the past four years. The machine, known as the U.C.-Blackwelder for the University of California, which developed it, and the Blackwelder Manufacturing Company, which makes it, cuts the whole tomato plant and conveys it to the shaker-sifter, where the tomatoes are shaken loose. The vines are blown back onto the field; human workers stationed along the sorting belts remove tomatoes that are defective.


BLOWER
VINE DISCHARGE
VINES
SHAKER-SIFTER
TOMATOES
SORTING STATIONS
DELIVERY ELEVATOR
ROTATING DIRT-REMOVER
DRIVER STATION
SUPERVISOR STATION
CUTTING KNIVES
FEED ROLLERS

FIELD TECHNIQUE of harvesting tomatoes mechanically uses tractor-drawn bulk bins in connection with the harvesting machine. The three rollers on the front of the machine are directly behind the vine-cutters and are used to feed the cut plants into the machine. Harvesting is done only once per crop; it was therefore necessary to breed tomato plants that all ripened at virtually the same time.



LETTUCE HARVESTER was developed by the University of California at Davis. The machine gauges the heads of lettuce and cuts those that are firm enough and large enough for harvesting. Spokes of the wheellike device, which is a rotary elevator, grip the cut heads and lift them to the conveyor belt on the front of the machine. The belt delivers them to baskets or to a packing machine.

described by Roman writers as early as 2,000 years ago, was to cut the grass, let it dry in the sun for a few days and then store it for protection from rain and snow. Mechanization of the process began with the replacement of the hand scythe by mowing machines (using the reciprocating knife) in the second half of the 19th century. The mown grass was stacked by hand in small haycocks before it had lost its green color and then, after further drying, was piled in large stacks outdoors or stored in a barn. Within the past century machines of various types have been developed to handle and pack the hay in convenient forms.

One simple method depends on the use of the so-called bull rake. The swath of grass felled by the mower is raked into windrows, and after it has dried in the field the bull rake collects it in piles for storage. The system is inefficient in use of storage space: the comparatively loose hay takes up 500 cubic feet of space per ton. Moreover, a strong human handler is still required to wrest the hay from the pile with a pitchfork and deliver it to the mangers. On most American farms the bull-rake collection method has been replaced by machines that bale the hay, and various systems for mechanical delivery of the hay in palatable form to the animals are being tested.

An automatic baler was developed about 1940. The machine, manned only by a driver, picks up the dried hay from the windrows in the field and rams it into bales weighing from 85 to 150 pounds each. In this packing the hay takes up only 200 cubic feet of space per ton—less than half the space of the bull-raked piles. A vehicle to pick up the bales and transport them to the barn follows the baler, and there are now devices for fully mechanical handling or packing of the bales in this task. One device is a bale thrower: it picks up the bale (made conveniently small in this case) and like a mortar tosses the package into the cart. A more orderly version of the device, developed within the past five years, picks up the bales (which can be larger in size) and packs them neatly in the wagon instead of throwing them in. At the storage destination the wagon tilts and deposits the load of bales in a tight stack. Some of these machines can deliver 14 tons of baled hay an hour.

The problem of delivering baled hay as loose hay to the feed manger by machine has not yet been solved satisfactorily, but there are some promising approaches. One is a machine that opens the bales, fluffs up the hay and delivers it by conveyor to the feed troughs, but the bales have to be lifted manually onto the conveyor. Another device is a system that chops the hay into small pieces at the outset. A chopping machine cuts the hay into pieces one to three inches long as it picks up the hay from the windrow in the field; the machine blows the chopped hay into a following vehicle, which transports the hay to the barn and there blows it into bins. The chopped hay is economical in storage space (taking up only about 170 cubic feet per ton), but the system has some unacceptable drawbacks: it produces a great deal of dust; the nutritious leaves of the alfalfa or clover hay are partly lost, and animals do not like the chopped hay as well as they like long hay. The experimenters working on the method hope, however, to solve these problems. One approach under study is a machine that will strip the nutritious leaves from the grass plants in the field, leaving the woody stalks to grow more leaves.

Perhaps the most promising innovation in haymaking is the delivery of the feed in capsule form. Two such devices are already on the market: they produce small, dense cubes of packed hay—two and a half inches or an inch and a quarter in size. A huge machine goes down the windrow in the field picking up the dry hay and squeezing it under tremendous pressure into the little cubes. They have a density equivalent to 25 to 40 pounds per cubic foot, and when packed together they would occupy only 50 to 80 cubic feet of storage space per ton. The cubes flow fairly satisfactorily by gravity, can be shoveled and are quite acceptable to the cattle. The machine in its present form, however, is expensive, and it will work only on hay that has been dried to a low moisture level; consequently so far it seems the method will not be suitable for hay grown in humid regions.

Sugar Beets and Cotton

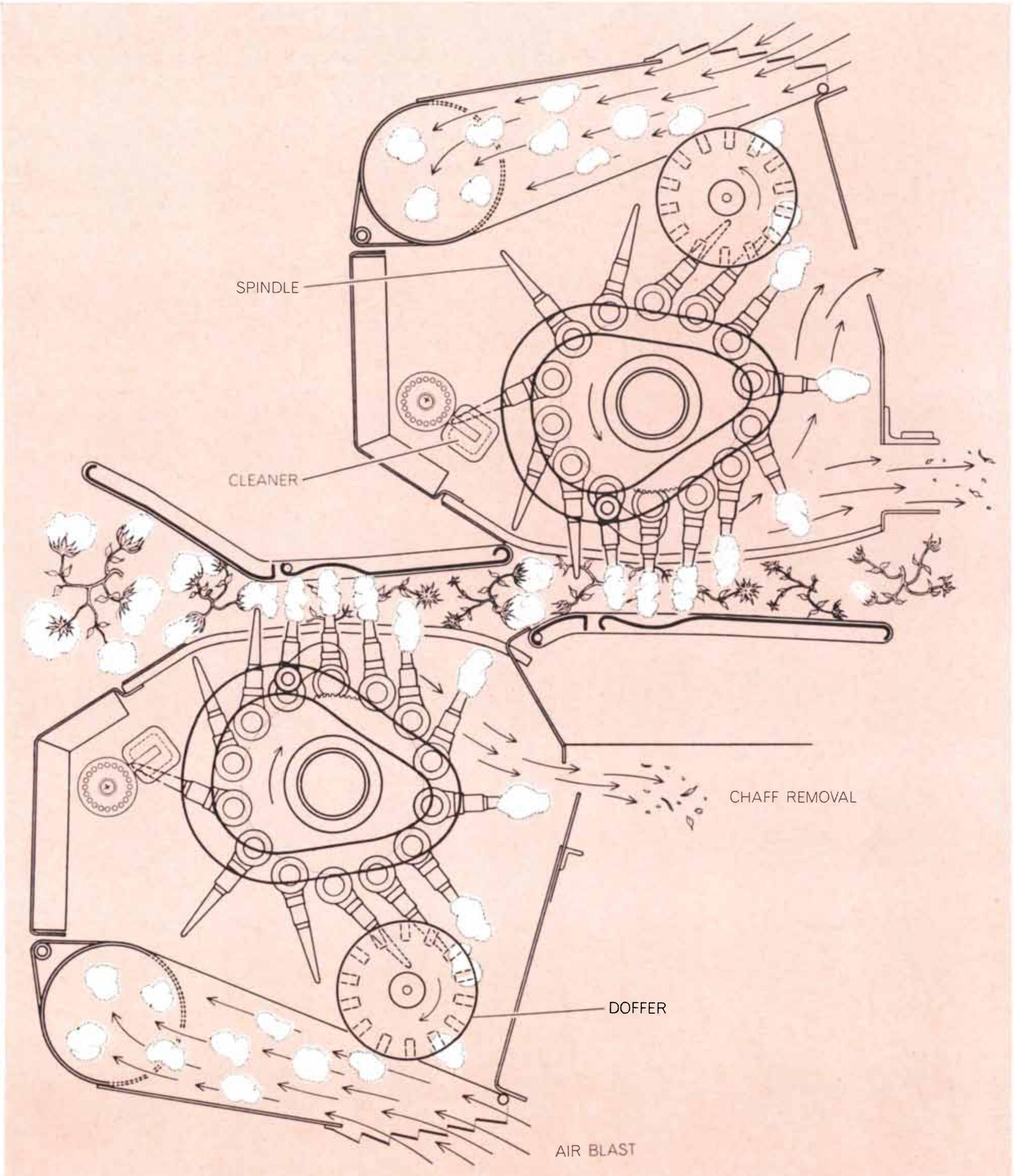
Sugar is another crop whose harvesting has been largely mechanized. The U.S. consumes about 10 million tons of sugar a year (nearly 100 pounds per capita), and more than 60 percent of this is now produced within the U.S. and its Caribbean island possessions. A little more than half of the production comes from sugarcane, the rest from sugar beets. The harvesting of cane still depends heavily on hand labor, as no satisfactory machine has yet been developed to cut the cane efficiently in the field. Sugar beets, however, are harvested entirely by machine, and the 1965 crop in the U.S. was 21 million tons.

The production of sugar from beets was established in 1811 by the development of a refining method by the French investigator Benjamin Delessert and by a decree of Napoleon that subsidized the founding of a sugar beet industry. Sugar beets yield a good sugar content (12 percent or more), but their cultivation and harvesting presented formidable labor problems. For one thing, the beets grow too thickly and in clumps, because each seed has several nuclei from which plants grow. In cultivation by hand one worker would go down the row of young plants with a short-handled hoe thinning down the clumps to one every 10 or 12 inches, and another worker would follow, on hands and knees, to reduce each clump to a single plant.

By the 1920's American beet farmers were using horse-drawn machines that seeded and cultivated the plants and lifted the grown beets out of the soil at harvest time. Human workers still had to pick the beets, top them (cut off the tops with a knife) and load them into wagons. In the 1930's U.S. agricultural agencies and beet growers, seeking to make beet sugar competitive with sugar produced by foreign cane growers, undertook an intensive program to mechanize the entire process of beet harvesting. The most difficult problem proved to be topping: mechanical toppers, failing to distinguish between large and small beets, cut the tops at the wrong levels. Eventually a University of California at Davis agricultural engineer, John B. Powers, solved the problem by devising a gauging mechanism that adjusted the topper to make the cut correctly through the crown of each plant. After World War II two highly effective beet harvesters were developed. One of these machines tops the plant, then lifts the beets out of the ground by means of a pair of rotating disks, cleans the soil from them and conveys them to a trailer cart. The other type of machine employs a different method: a large, spiked wheel turns into the ground along the row of plants, impales each beet on a spike and brings it up to a mechanism that tops the beet, pulls it off the spike and drops it in a carrier.

These machines, and the development of devices (genetic and mechanical) that reduce each sugar beet seed to a single nucleus, have succeeded in fully mechanizing the harvesting of beets. In 1944 only 7 percent of the sugar beet crop in the U.S. was harvested by machine; within 13 years the mechanical takeover was complete and machines harvested the entire crop.

One of the last of the major U.S. crops to yield to mechanization was cotton. The long search for a workable mechanical cotton picker is a familiar story. Over the course of the past century more than 1,800 patents were issued in the U.S. to hopeful inventors who proposed a great variety of schemes for removing the cotton fiber from the boll: sucking or blowing it off pneumatically, threshing it off, pulling it off by static electricity, extracting it chemically, combing it off, brushing it off or picking it off by means of mechanical fingers. In 1885 a Scottish engineer named Angus Campbell found a road that was to lead to success. He devised a machine that swept rotating spindles through the cotton and twisted the loose fibers off the plant. The spindle idea, promptly recognized as sound in principle, attracted many inventors, but it proved to be difficult to translate into an effi-



KEY SECTION of the cotton picker includes spindles, which remove cotton fiber from the bolls, and doffers, which remove the fibers from the spindles. Blowers take the cotton from the doffers to the conveyor system. After a spindle has been doffed it goes through a cleaner that wipes off gum and plant juices with moistened pads. Spinning dries the spindle before it enters the next boll.

cient machine. The International Harvester Company spent 40 years and more than $5 million in the effort to develop a satisfactory spindle cotton picker. Success was finally achieved by the Rust brothers, John and Mack, who in 1927 produced a machine that proved its worth in field tests. The chief problem had been that, although barbed or serrated spindles were effective in pulling off the cotton, there still remained the difficulty of removing the fibers from the spindles. The Rusts solved this problem by using a smooth spindle and moistening it so that the fibers would stick and wind around it. The Rust picker, aided by the breeding of plants specifically designed to facilitate its use, has made cotton picking a machine operation and has radically reduced farm employment in the South (as the Rusts feared it would).

Fruits and Vegetables

The crops I have discussed so far—grain, hay, sugar beets, cotton—can withstand rough handling. Far more difficult is the problem of mechanizing the harvesting of the "perishable" crops—fruits and watery vegetables—that must be handled gently. These products, whose acceptability depends on their coming to the table ripe, fresh and undamaged, are incompatible with machine picking not only because of their structural delicacy but also because they characteristically ripen on the plant not all at one time but over the course of the plant's bearing period.

Let us consider first the progress that has been made in the machine harvesting of deciduous-tree fruits, such as prunes, apples, cherries, peaches and apricots. The basic approach being pursued is to develop machines that will shake the ripe fruit off the trees and catch it without damaging the tree or the fruit. Investigators from various points of view have been working on this problem for many years. They have accumulated a great deal of information about the properties of fruit trees and fruits—probably more than is known about any other class of crop plants. These studies have gone into details such as the amount of energy required to shake a given fruit off the branches under various conditions of fruit maturity and air temperature, the relation between the movements of the shaken branch and the kinetic energy imparted to the fruit itself, the power needed to shake tree limbs of various diameters effectively, the relation of the vibration frequency and amplitude to this power

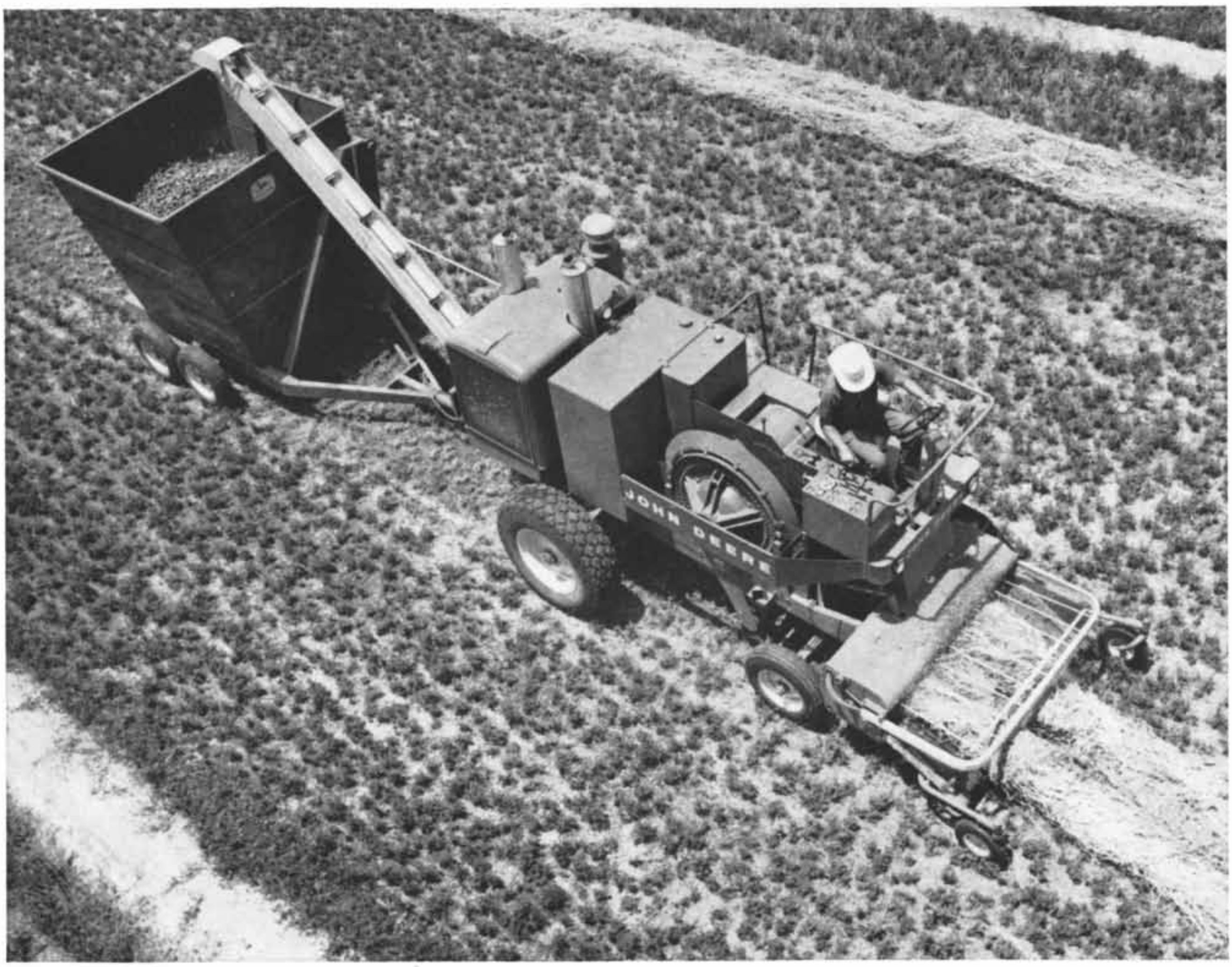
EXPERIMENTAL CITRUS HARVESTER shakes the limbs of the fruit trees with blasts of air. The fruit drops to a catching frame attached to the harvester. The machine, which was developed by the Florida Manufacturing Corporation, clears a tree in two to three minutes.

requirement. The requirement is smallest when the frequency is comparatively low and the amplitude is comparatively large. It has been learned, for example, that only a third as much power is needed to shake most of the prunes off a tree with vibrations at the frequency of 400 cycles per minute and two inches in amplitude as with vibrations of one-inch amplitude at 1,100 cycles per minute.

Leaders in the research on mechanical fruit harvesting have been Robert B. Fridley of the University of California at Davis and P. A. Adrian of the U.S. Department of Agriculture. A workable system has now been developed. Its main elements are an efficient mechanical shaker that is attached to the tree trunk or a limb, a "catching frame" for receiving the falling fruit and a pruning system that minimizes the chances of the fruit hitting branches as it falls and does not appreciably reduce the tree's production. The shaker is of the "inertia" type: it shakes the tree without seriously shaking the tractor on which the shaker is mounted. In effect it works something like unbalanced front wheels on an automobile, which can impart considerable shake to the steering mechanism. Power from a hydraulic motor is applied to an assembly of unbalanced weights that rotates in eccentric fashion and vibrates the tree by way of a housing attached to the tree. The falling fruit drops into a well-padded catcher placed below the lowest branches; the fall is so cushioned that the fruit does not bounce and is not damaged. The entire operation is housed in a self-propelled vehicle that automatically collects the fruit in bins.

Mechanical fruit picking is still only in its infancy. Within the past five years a more dramatic advance in machine harvesting has taken place in another field: the picking of tomatoes. In California, where 125,000 acres were devoted to tomato growing, the growers used to have to recruit 40,000 workers at the season's peak to harvest the crop by hand. Two investigators at the California Agricultural Experiment

CHERRY PICKER, part of which is shown on the cover of this issue, has two arms that grip limbs of the cherry tree and shake them. The cherries fall onto the fabric catcher and roll into the conveyor system. The cherry harvester is now in commercial production.



HAY-CUBER was designed to reduce the bulk of hay. It picks hay up from a windrow and compresses it into cubes of a size that cattle can eat readily. The machine was designed for alfalfa and other legumes rather than for hay crops consisting predominantly of grass; the legumes contain natural adhesives that, with water added in the machine, provide the bond that holds the cube together.

Station in Davis—an agricultural engineer and a plant biologist—undertook to develop a system for mechanizing tomato picking. They attacked the problem on two fronts. Gordie C. Hanna, the biologist, would breed a tomato plant designed for machine handling. The plant would bear tomatoes that were of uniform size and all ripened at the same time, that could easily be detached from the vine but would not drop off prematurely, that had a skin tough enough to withstand mechanical handling, that would store well and that would be pleasing to the consumer in flavor and other qualities. Coby Lorenzen, Jr., the agricultural engineer, meanwhile would work on the design of a machine that would harvest this tomato rapidly, efficiently and at reasonable cost.

After 10 years of study, experiments and development the two men achieved their objective in 1962. The plant was ready and so was the machine: a harvester that cut off the plant at ground level, lifted it, shook off the tomatoes and deposited them in a bin in which they were hauled to the processing plant [*see illustration on pages 52 and 53*]. The tomato "combine" won remarkably rapid acceptance. Within three years this machine was harvesting 24 percent of the California tomato crop; last year 800 of the machines were available and they picked almost 80 percent of the crop; this fall, with at least four major manufacturers now producing machines, a large percentage of the tomatoes grown in the U.S. for processing will be harvested by machine.

Machine Economics

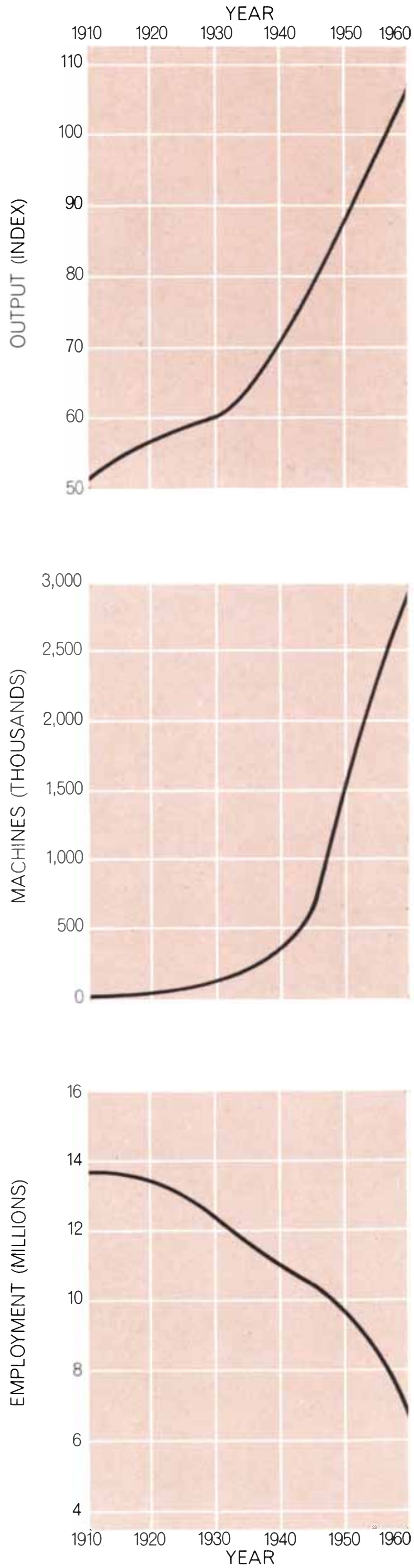
How far can the mechanization of harvesting be carried? Fundamentally that will depend a great deal on the comparative costs of performing the task by hand and by machine. In most cases mechanical harvesting is less expensive, even allowing for the capital cost of the machine. For example, the Tennessee Agricultural Experiment Station recently studied the costs of harvesting snap beans by hand and by machine. Hand harvesting on farms of substantial size (200 acres) with an average yield (two tons of beans per acre) averaged $103 per acre; the cost of machine harvesting under the same conditions averaged only about $25 per acre, taking into account the cost of the machine for a five-year lifetime. The size of the farm, of course, makes a difference: harvesting the beans from a small farm of only 25 acres yielding two tons per acre cost about the same for machine as for hand picking ($95); on the other hand, on a farm of 500 acres the cost by machine was only $11.32 per acre. In any case, aside from the money, there is a tremendous saving of labor; whereas the average bean picker picked 1.06 bushels per hour by hand, a single operator could pick 95.8 bushels per hour by machine. Moreover, there are a number of side benefits from harvesting by machine, including delivery of cleaner fruits and vegetables to the consumer.

It seems likely that, economics permitting, we shall see further extension and increasing sophistication of harvesting by machine in the U.S. Under study and test are various ideas for selective machines that will be able to discriminate between ripe and unripe fruits or vegetables (picking only the red tomatoes, for example, and leaving the green). Among these projects are machines for picking citrus fruits and strawberries. Harvesting machines for lettuce, asparagus and grapes are now entering the stage of development for manufacture by equipment companies.

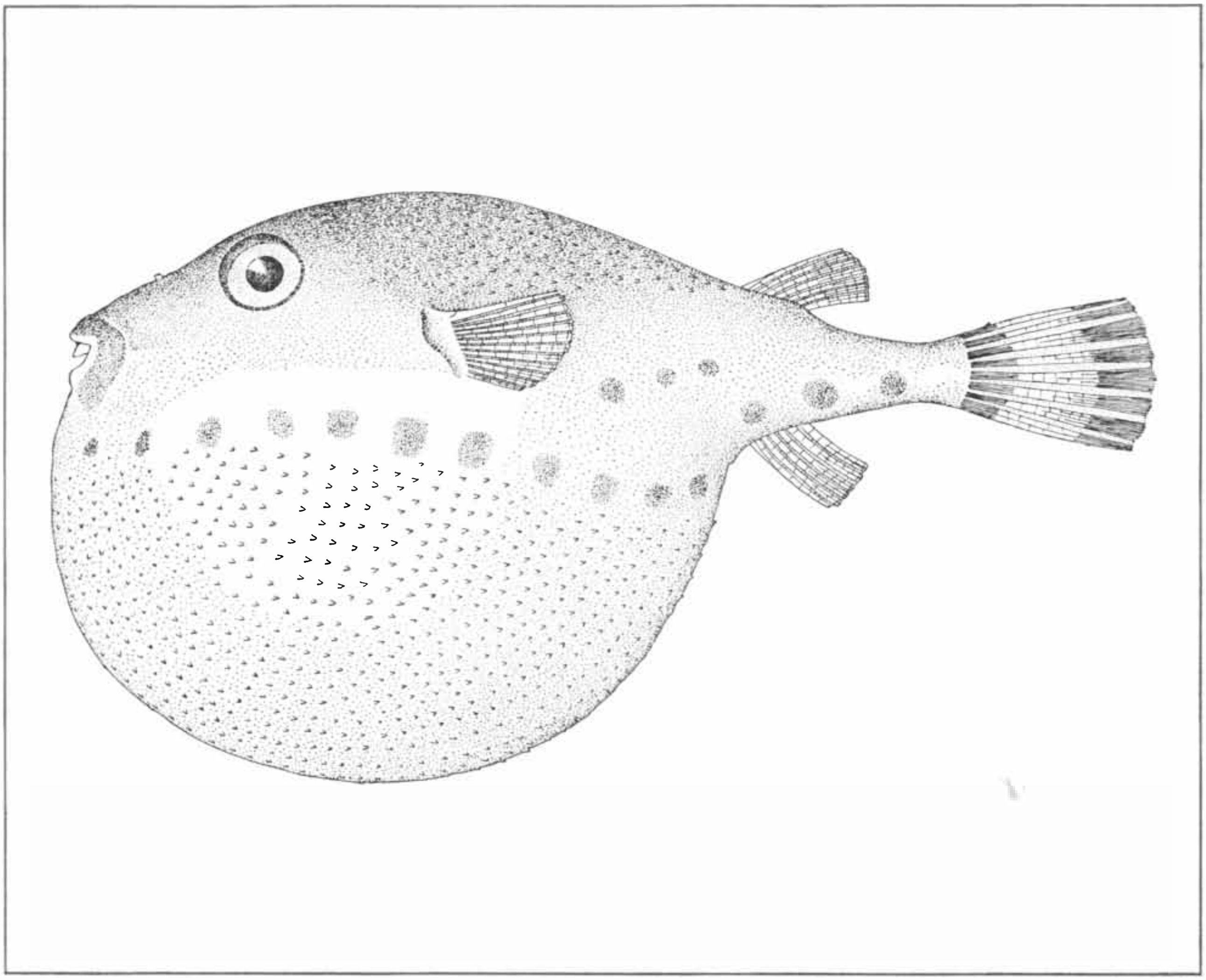
Some Limitations

Yet it is obvious that, in a country with a highly developed state of agriculture, harvesting by machine must eventually reach a level of diminishing returns. There is a limit, after all, to the amount of food a population will consume, and consequently a limit to the economic incentive for employing machines to increase production. For example, a lettuce-harvesting machine has been designed that can pick four rows at a time, and I am told that 600 of these machines could harvest all the lettuce now grown in the world. Obviously it would not pay a large manufacturer to go to the expense of developing and tooling up for a machine with so limited a prospective sale. Usually in such a situation a publicly supported university does the research and a small manufacturing firm carries out the development and production.

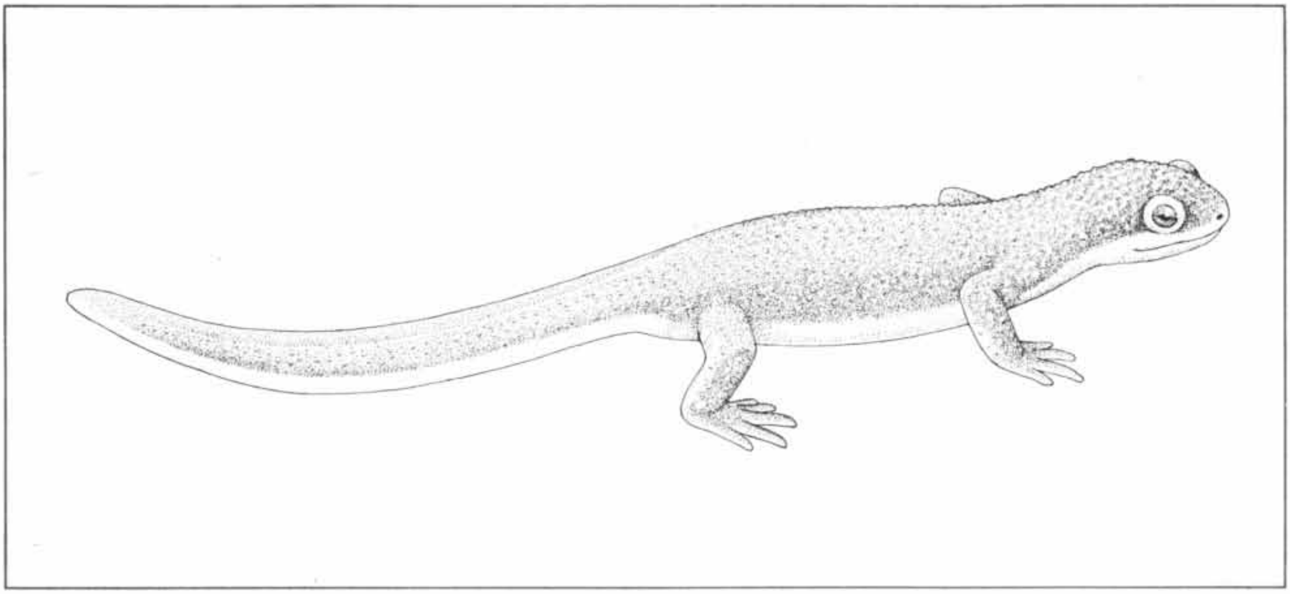
It is in the developing countries of the world, where harvesting is still done mainly by hand, that the development and use of harvesting machines now has its most important future. For those countries, 90 percent of whose population is occupied in farming, harvesting machinery would provide a prime means of releasing people from the soil and thereby making possible the industrialization of the countries.



EFFECT OF MECHANIZATION on U.S. agriculture is indicated by a comparison of farm production, the extent of mechanization and the number of people employed in farm work. The charts cover the 50 years in which harvesting by machine developed to its present major role in agriculture.



PUFFER FISH harbors the potent nerve poison tetrodotoxin. The fish's ovaries and liver are extremely poisonous, the intestines and skin less so. A true puffer, *Spheroides spengleri* of the Tetraodontidae family, is shown. Poisonous puffers are found in warm seas.



CALIFORNIA NEWT, *Taricha torosa*, also contains tetrodotoxin, principally in its skin, muscles and blood. Shown almost twice its actual size, *Taricha torosa* is the most toxic of the 11 species of Salamandridae, or true newts, that are known to contain the toxin.

TETRODOTOXIN

It is a powerful poison that is found in two almost totally unrelated kinds of animal: puffer fish and newts. It has been serving as a tool in nerve physiology and may provide a model for new local anesthetics

by Frederick A. Fuhrman

On September 8, 1774, His Majesty's Sloop *Resolution*, commanded by Captain James Cook, lay at anchor off the South Pacific island of New Caledonia, discovered by Cook a few days earlier. That afternoon the ship's clerk traded with a native for a fish. Captain Cook asked to have the fish prepared for a supper he was to share with the expedition's two naturalists, J. R. Forster and his son Georg. Later Cook recorded in his journal: "The opperation of describeing and drawing took up so much time till it was too late so that only the Liver and Roe was dressed of which the two Mr. Forsters and myself did but just taste. About 3 or 4 o'clock in the Morning we were siezed with an extraordinary weakness in all our limbs attended with a numbness or Sensation like to that caused by exposeing ones hands or feet to a fire after having been pinched by frost, I had almost lost the sence of feeling nor could I distinguish between light and heavy bodies, a quart pot full of Water and a feather was the same in my hand.... In [the morning] one of the Pigs which had eat the entrails was found dead."

Captain Cook's account and the Forsters' description of the fish leave no doubt that the three men had been served a puffer fish. It was fortunate that they had eaten sparingly, because many puffer fishes contain a powerful toxin that can kill a man. Curiously this toxin, which is now called tetrodotoxin, has also been found in an almost totally unrelated animal, the newt. Recently a number of investigators have become interested in tetrodotoxin's chemical nature and mode of action. It has been discovered that the toxin blocks the conduction of nerve signals, and in a highly specific manner. This action has already proved useful in the study of nerve-signal transmission; it also suggests that tetrodotoxin may be able to serve as a model for valuable new local anesthetics.

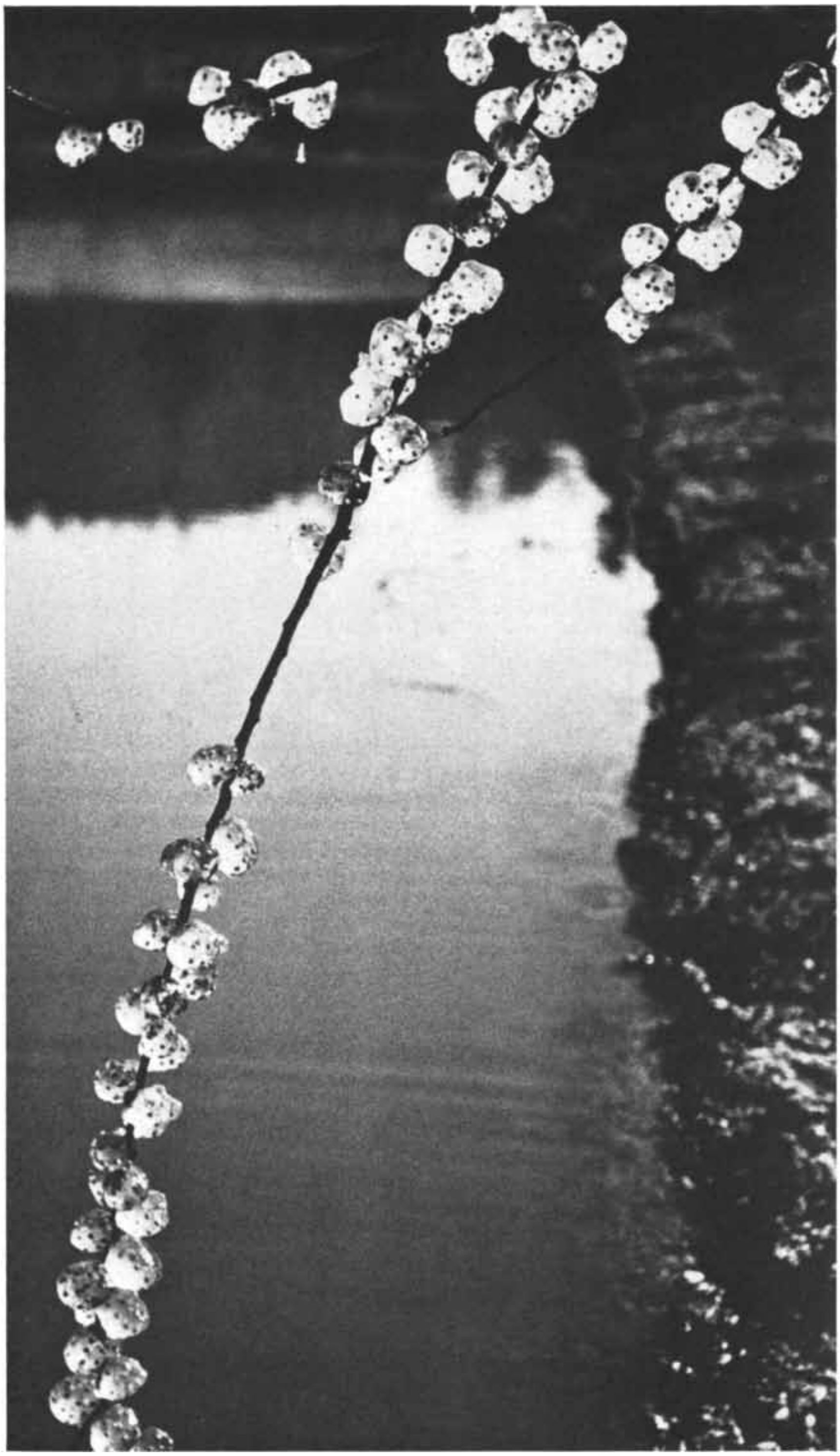
At least 40 species of puffer fish are known to be poisonous. Most of them belong to the family Tetraodontidae, but some species in at least three other closely related families are toxic when they are eaten. In puffer fish tetrodotoxin is most highly concentrated in the ovaries and the liver; smaller amounts are found in the intestines and skin. In some species the muscle tissue is also toxic.

Many different kinds of fish and shellfish are known to be poisonous, and frequently it is difficult to identify the toxin that is responsible. Sometimes poisoning is caused by bacteria in spoiled fish; sometimes it results from one-celled organisms the fish have ingested. In the latter case a species may be safe to eat in one season of the year and poisonous in another. Mussels, for example, can be poisonous in the summer because in those months they sometimes ingest and concentrate one-celled dinoflagellates. Some of these organisms contain saxitoxin, the potent toxin that is responsible for "paralytic shellfish poisoning." By the same token a species of fish may be safe to eat if it is caught in some places and poisonous if it is caught in others. In most waters the red snapper is safe, but around certain islands in the South Pacific it can cause a form of poisoning called ciguatera. Puffer fish, on the other hand, are poisonous throughout the year (although the amount of tetrodotoxin in their viscera may vary with the season) and wherever they are caught.

Poisonous puffer fish are found in all the warm seas of the world. Their common names usually refer to their ability to inflate themselves when they are disturbed: puffer fish, swellfish, blowfish, toado, globefish, porcupine fish, balloonfish. They are called *makimaki* in Hawaiian, *blaser* in Indonesian, *botete* in Spanish and *fugu* in Japanese. In Japan puffer poisoning was common for centuries. In his *History of Japan*, published in 1727, Engelbert Kaempfer wrote of the puffer: "He is rank'd among the poisonous Fish, and if eat whole, is said unavoidably to occasion death.... Many People die of it, for want, as they say, of thoroughly washing and cleaning it.... The Japanese won't deprive themselves of a dish so delicate in their opinion, for all they have so many Instances, of how fatal and dangerous a consequence it is to eat it."

Indeed, puffer poisoning still occurs in Japan. In 1957, 176 cases were recorded; 90 of them were fatal. An effort has been made to reduce such incidents by licensing chefs who are considered competent to prepare puffer fish, but the fascination of eating the fish complicates the problem of control. Poisoning has been known to result from drinking a mixture of hot *sake* and *fugu* testes, which is supposed to contribute to virility. The testes of some species contain tetrodotoxin and the testes of others do not, but under such circumstances the drinker is unlikely to distinguish between testes and the highly poisonous liver and roe. It is also said that a small amount of puffer liver imparts a particularly piquant flavor to certain dishes. Dangerous pleasures are not unknown in human life, but this one calls for an uncommon nicety of judgment. (There is little danger in eating *fugu sashimi*, thin slices of raw *fugu* muscle arranged in flower or bird patterns. The slices are cut from the nontoxic back meat of the fish.)

Since puffer poisoning has been comparatively common in Japan, it has not escaped the attention of Japanese scientists. As early as 1883 an attempt was made to prepare a pure toxin from the fish; Yoshizumi Tahara of Tokyo named the substance tetrodotoxin, but we know today that his best preparations were only about .2 percent pure. Nevertheless, Tahara's tetrodotoxin was widely used as a drug for the relief of pain, and he was granted a U.S. patent on it in 1913. It was not until 1950 that Akira Yokoo of Okayama University and Kyosuke Tsuda of the University of Tokyo independently succeeded in crystallizing the substance in pure form.

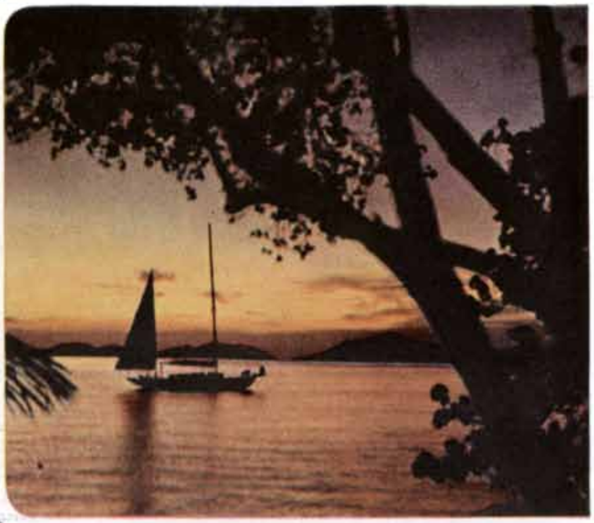


EGGS OF *TARICHA TOROSA*, enclosed in a translucent jelly-like substance, are deposited in spherical clusters along twigs of branches submerged in ponds and streams. Tetrodotoxin from the newt was first isolated at Stanford University from eggs found near the campus.

Meanwhile a line of investigation had been started in the U.S. that had no apparent connection with the Japanese work. In 1932 Victor C. Twitty joined the department of biological sciences at Stanford University. He had come from Yale University, where he had been studying the grafting of tissues in the common eastern American salamander *Ambystoma punctatum*. In order to continue his work he needed a substitute animal, and one was easily found. In the spring the hills above the Stanford campus abound in the California newt, *Taricha torosa*. The newts come out of the forested areas to spawn in ponds and streams; spherical clusters of their eggs are found attached to submerged twigs or lying free on the bottom.

Twitty grafted eye and limb buds from embryos of the California newt into larvae of the striped salamander, *Ambystoma tigrinum*. Much to his surprise he observed that the host salamanders became paralyzed and remained so for many days. In a series of superb experiments he established that the paralysis was caused by a potent toxin found in the eggs and embryos of *Taricha torosa*. A few years later a group of biologists at Stanford succeeded in obtaining a concentrated preparation of the toxin. We now know that it was only about 1 percent pure, but when it was administered to animals, it gave rise to dramatic physiological effects. I joined the Stanford group in 1941, and somewhat later we found that the toxin acted to block the transmission of nerve impulses in frogs. We did not attempt a more intensive study. It was obvious that our best preparations of the toxin were quite impure, and we thought it unlikely that detailed examination of its effects in animals would lead to anything conclusive. Further purification of the toxin was not possible with the methods that were available at the time.

After the war powerful new methods for the separation of organic compounds, such as chromatography and electrophoresis, came into common use. By 1960 it seemed reasonable to apply some of these methods to the purification of the toxin from newt embryos. Harry S. Mosher, a Stanford chemist, agreed to collaborate with me, and with the aid of nets and a kitchen strainer we collected some 200 pounds of newt eggs. A young graduate student, M. S. Brown, undertook the task of attempting to extract pure toxin from the gelatinous mass. By the summer of 1962 he had crystallized



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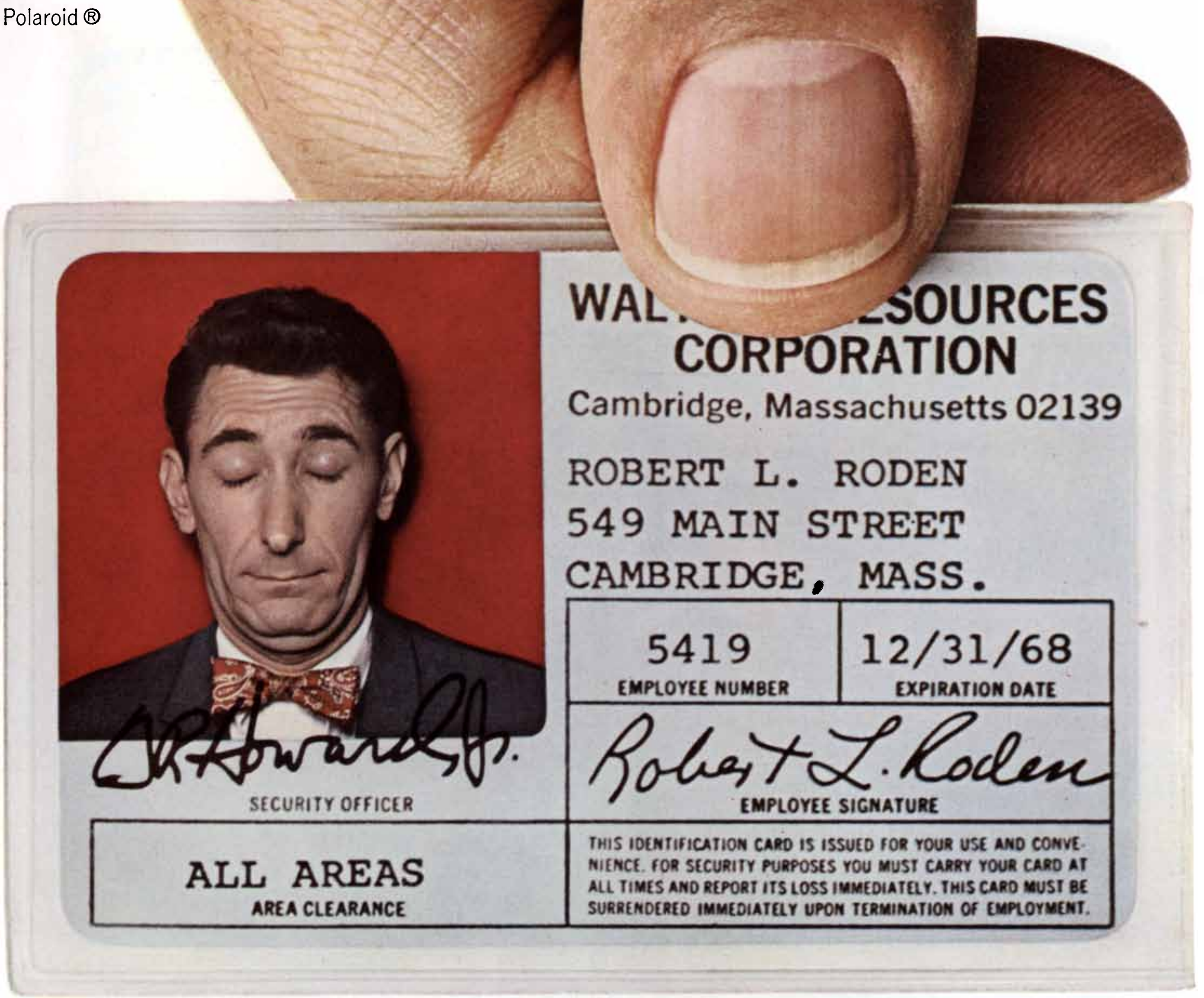

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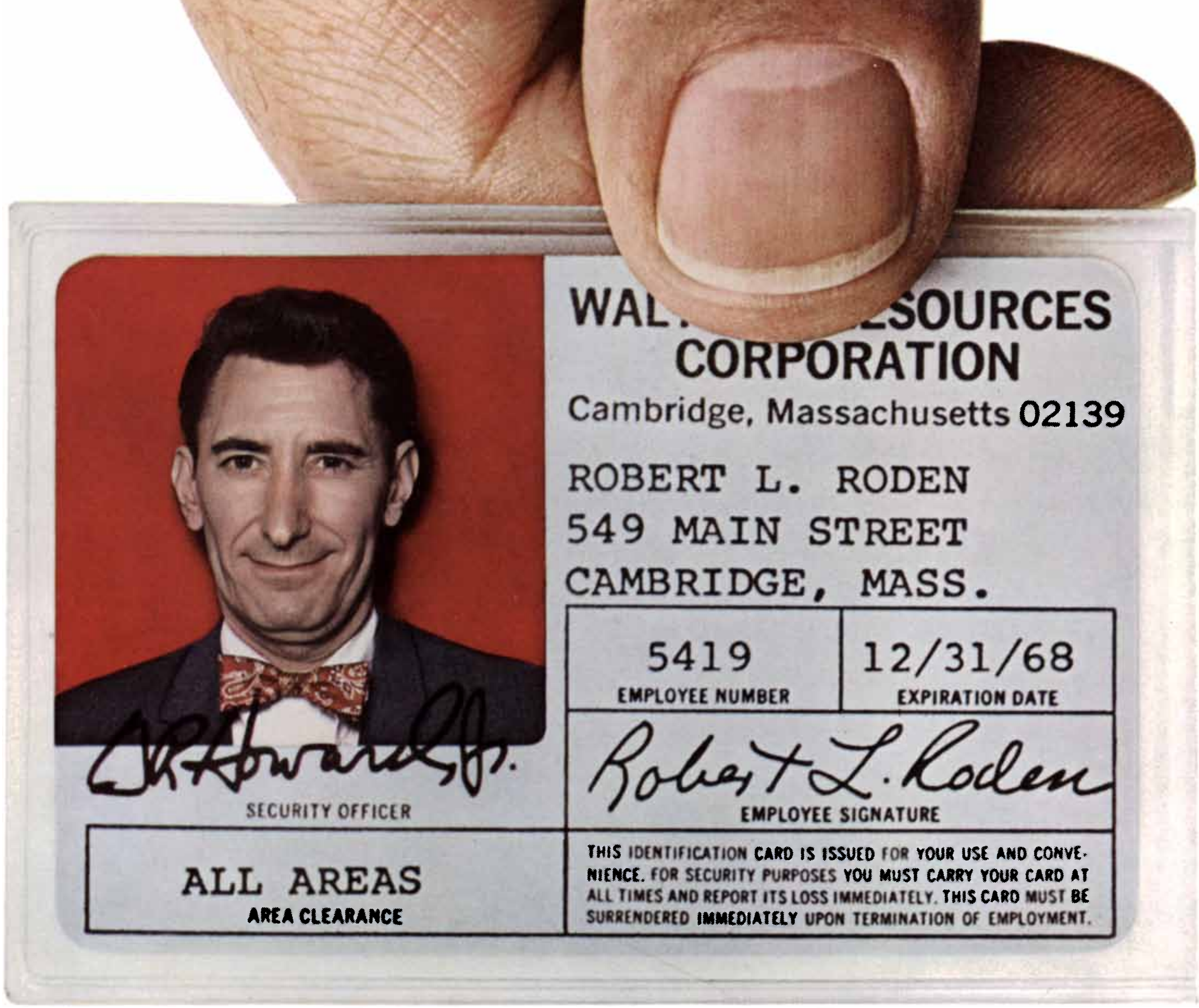

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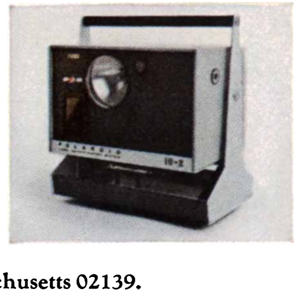

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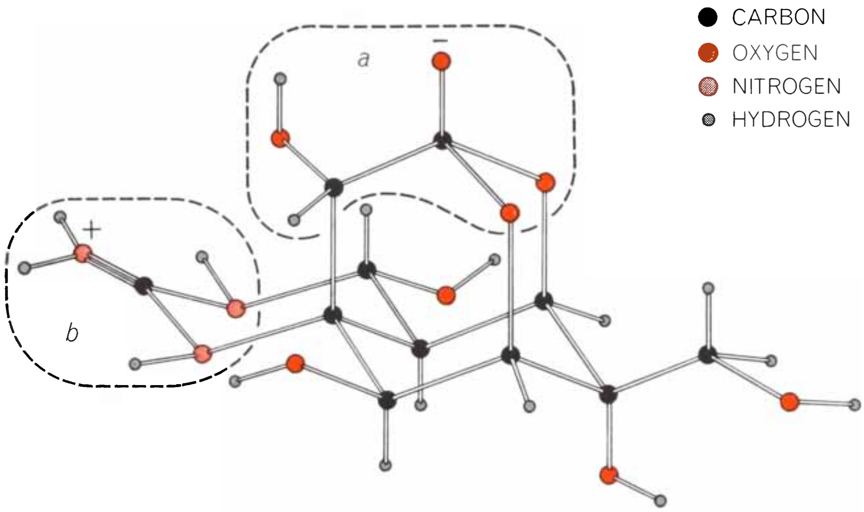
a substance of which each milligram was sufficiently potent to kill about 7,000 mice. We named the substance "taricha-toxin." C. Y. Kao of the Downstate Medical Center of the State University of New York joined us for the summer, and we began to investigate the pharmacological action of this new toxin.

Poisonous substances from animals fall into two broad chemical classes: proteins and nonproteins. Protein toxins are by far the most potent, but they are also extremely difficult to characterize and reconstruct in the laboratory. Nonprotein toxins are more tractable, and they also offer the possibility that they can be modified and used to treat disease. Tarichatoxin was not a protein. As its purification progressed and it became apparent that it was more toxic than any other nonprotein then known, we studied the literature on toxic substances to see if any of them resembled it. We found two: saxitoxin from shellfish and tetrodotoxin from puffer fish. We readily determined that there were certain chemical and pharmacological differences between tarichatoxin and saxitoxin, but to our astonishment every new piece of evidence indicated that tarichatoxin and tetrodotoxin were identical. Working with a sample of tetrodotoxin supplied by Tsuda, we compared the two closely. Their infrared and nuclear-magnetic-resonance spectra were the same, and they behaved identically in various chromatographic systems. Mice, rats, goldfish and other animals were killed by both toxins; newts were affected by neither of them. We concluded that puffer fish and newts, biologically different though they are, contain the same toxin. We suggested that the older name "tetrodotoxin" be retained.

Many different species of newts and salamanders have been tested in our laboratories for the presence of the toxin, but it has been found only in the 11 species of a single family: the Salamandridae, or true newts. (A different toxin, samanderin, has been isolated from specialized glands of the European fire salamander.) The most poisonous newts belong to three closely related species that range along the Pacific Coast from southern California to Alaska; one of them is *Taricha torosa,* whose eggs first yielded the newt toxin. The toxin of these species is concentrated principally in their skin, muscle and blood. It seems likely that the ability to synthesize tetrodotoxin in both newts and puffer fish has evolved as a protection against predators. Why this ability should be found in only one suborder of fishes and one family of amphibians, however, is an evolutionary mystery.



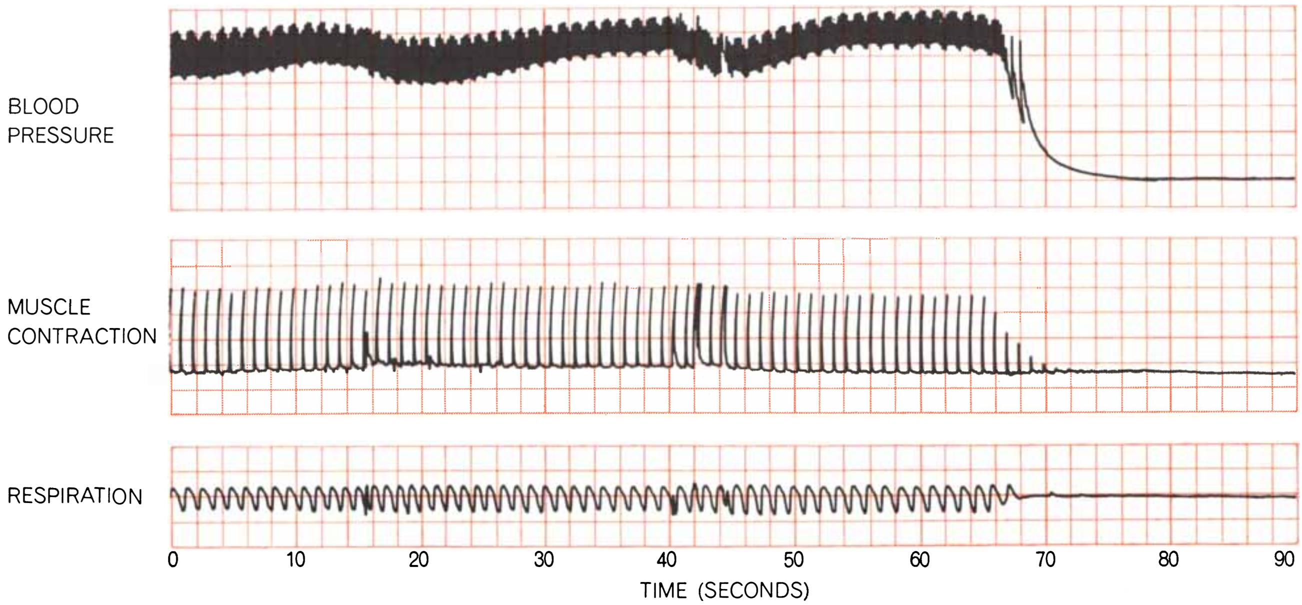
STRUCTURE OF TETRODOTOXIN is unusual. The hemilactal link at *a*, which joins the molecule's two rings, was not previously known to exist. At *b* is a guanidium group, characteristic of the poisonous substance guanidine; this group may contribute to tetrodotoxin's physiological action. In acid solution the molecule carries both a positive and a negative charge. This presentation of the molecule was worked out by Harry S. Mosher of Stanford.

Tetrodotoxin is chemically unusual. The tiny colorless crystals that have been obtained from puffer fish and newt eggs are soluble in water if a trace of acid is added but decompose easily in both strongly acid and strongly alkaline solutions. The empirical formula of the molecule is $C_{11}H_{17}N_3O_8$. The problem of the structure of the molecule occupied chemists in the U.S. and Japan for many years. Finally, at a conference in Kyoto in 1964, the same structure was proposed by four different groups of chemists who had worked on the problem independently of one another. The groups were led by Tsuda, Mosher, Toshio Goto of Nagoya University and Robert B. Woodward of Harvard University. Tetrodotoxin is a weakly basic compound that in an acid solution exists as a molecule carrying both a positive and a negative charge—a "zwitterion." The molecule does not show a close resemblance to any other that is known.

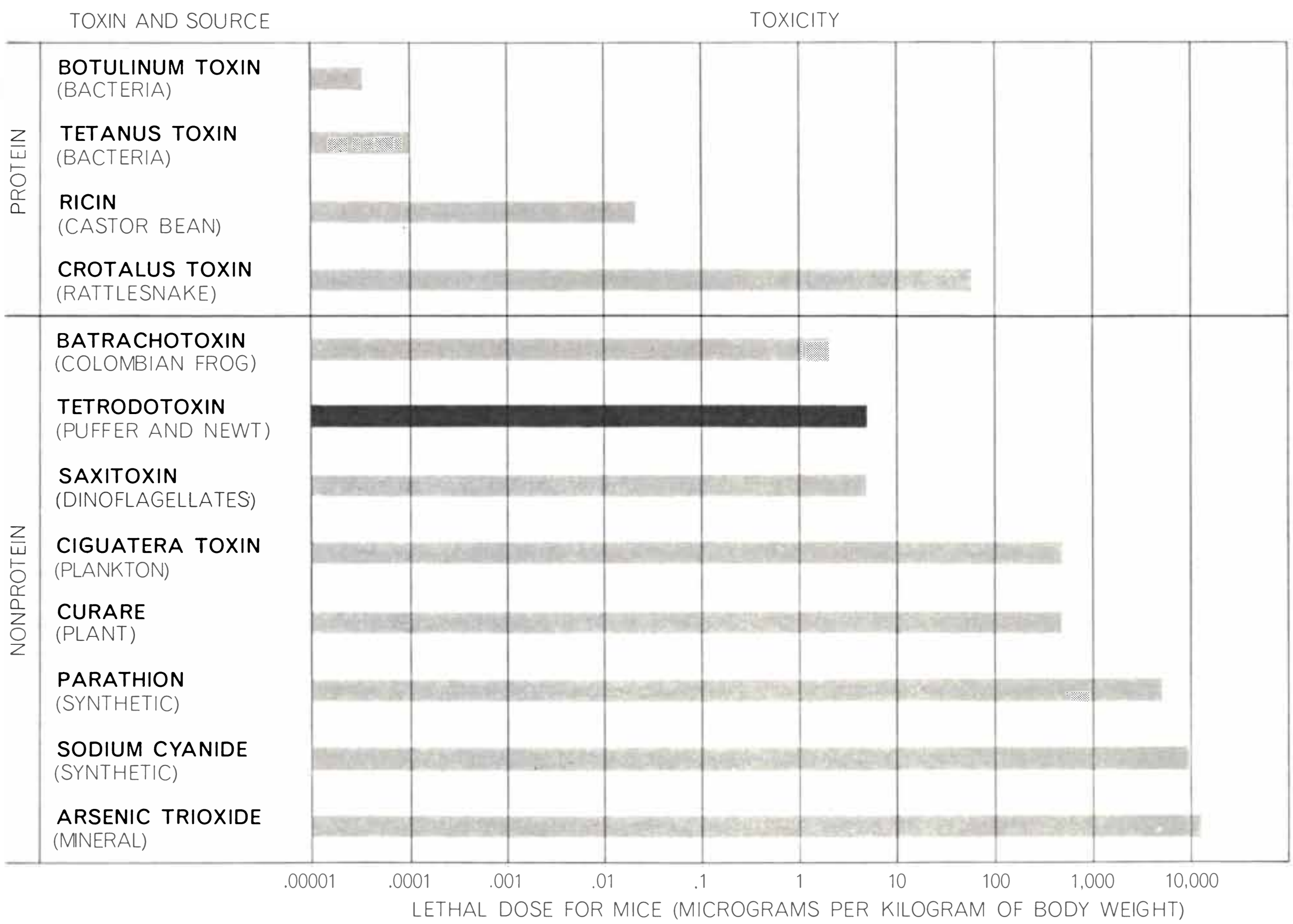
The mode of action of tetrodotoxin has been investigated in several ways. The outward symptoms of tetrodotoxin poisoning are sufficiently distinct for them to be readily differentiated from other types of fish poisoning, with the possible exception of paralytic shellfish poisoning. The first symptoms, which some individuals have felt within 10 minutes of having eaten the fish, are a numbness and tingling of the lips, tongue and inner surfaces of the mouth. Weakness follows and then a paralysis of the limb and chest muscles. Sometimes there is vomiting. The blood pressure is low and the pulse usually faster and weaker than normal. Death can occur within 30 minutes.

The most important physiological effects of the toxin can be observed directly if a small dose is injected into an animal that has been prepared so that recordings can be made of its blood pressure, rate of respiration and the response of a leg muscle when the nerve to it has been stimulated. After about 10 micrograms (millionths of a gram) of tetrodotoxin have been introduced into a vein there is a prompt drop in blood pressure and a decrease in the depth of respiration. The muscle fails to contract in spite of continued stimulation of its nerve. Administered in this way, tetrodotoxin can take effect so rapidly that an animal's respiration changes from a normal pattern to complete stoppage in the course of a single breath.

The effect of the toxin on the functioning of nerve and muscle has been particularly useful in unraveling its mode of action. The technique with which this effect has been explored is essentially a modern version of one used in 1856 by the great French physiologist Claude Bernard to study the Indian arrow poison curare. After a poison is injected into an animal electrical stimulation is applied to a motor-nerve axon, the long fiber that extends from the bodies of nerve cells in the spinal cord to the muscle cells. When an axon associated with the muscle of a limb is stimulated in this



INTERRUPTED RHYTHMS of blood pressure, muscle contraction and respiration occur almost immediately when a small amount of tetrodotoxin is introduced into the vein of an anesthetized animal. About 10 micrograms (millionths of a gram) of the toxin produced the changes shown in these records. Limb-muscle contractions were induced by electrical stimulation of a nerve.



SCALE OF TOXICITY rates selected protein and nonprotein poisons and shows arsenic trioxide to be the least toxic of the group. The crotalus toxin rated here is crotactin. Batrachotoxin, the most toxic of the nonprotein compounds, is present in the skin of a South American frog. The toxin was isolated after tetrodotoxin was found in Salamandridae (true newts). Tetrodotoxin also is present in 40 species of fish, most of them Tetraodontidae (true puffers). Saxitoxin causes "paralytic shellfish poisoning"; it is present in dinoflagellates, one-celled organisms that shellfish ingest. The agent responsible for ciguatera poisoning has not been determined; it is probably caused by a toxin in plankton that are ingested by fish. Curare is an Indian arrow poison; parathion is an insecticide.

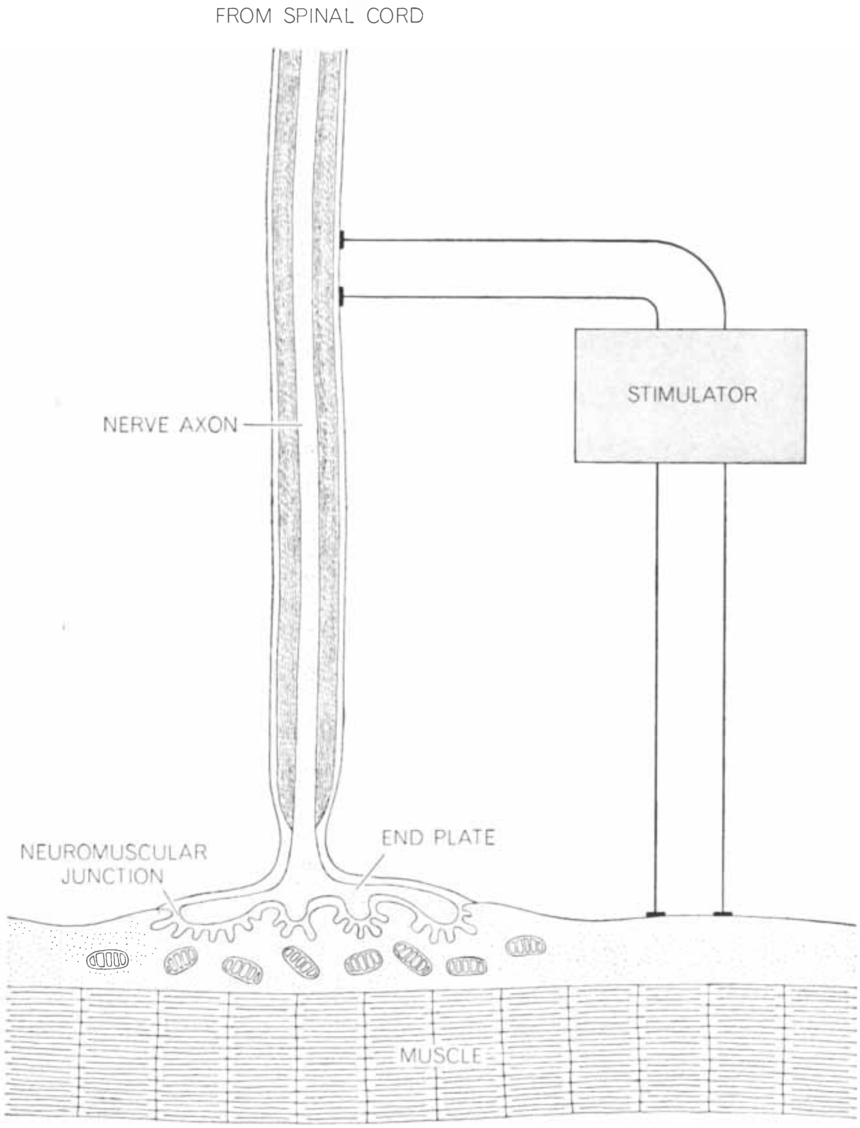
way, it causes the muscle to contract just as a normal nerve impulse does. After the injection of a toxin, however, the muscle may fail to respond to its nerve. In this event electrical stimulation is applied directly to the muscle. If the muscle then contracts, the site of action of the toxin must be either the axon or the junction between nerve and muscle.

Experiments of this kind were performed with tetrodotoxin, some of them by Fusao Ishihara of the University of Tokyo, as early as 1918. They were difficult to interpret. After tetrodotoxin was administered, a muscle stimulated through its nerve failed to contract. When the muscle was stimulated directly, it did contract at first, but after a few minutes the contractions stopped. Today we know that tetrodotoxin first affects the axons of motor nerves and then, somewhat later, it affects the muscle fibers in much the same way. This has been demonstrated in a clear-cut fashion by more elaborate experiments.

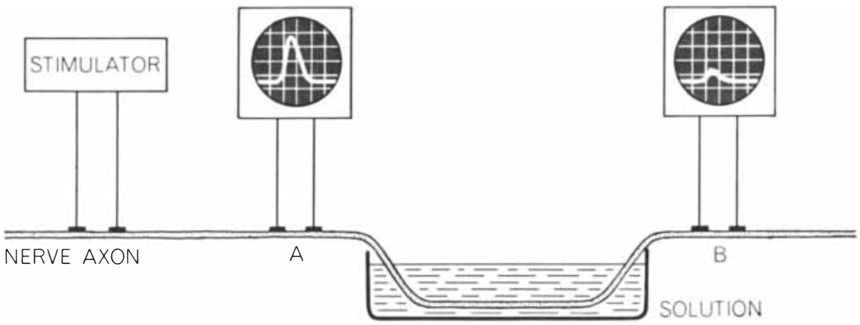
When a nerve relays an impulse from the brain to a muscle, the signal carried by the axon can be measured as a change in electric potential. A similar change occurs in a nerve that has been removed from a frog and stimulated by artificial means. An experiment can be set up so that the electric potential is measured at two points along an isolated axon, and between these points the axon can be immersed in a weak solution of tetrodotoxin. When the nerve is stimulated under these conditions, there is no change in electric potential beyond the place where the toxin is applied; conduction of the impulse has been blocked. Most of the effects suffered by those who have eaten poisonous puffer fish can be explained by this effect of tetrodotoxin.

Experiments similar to this one have been performed with nerves of the California newt. When newt axons are bathed in a solution of tetrodotoxin that is 25,000 times stronger than a solution that prevents conduction in frog nerve, they continue to transmit impulses. It is clear that some characteristic of their nerves renders newts (and also puffer fish) resistant to their own toxin.

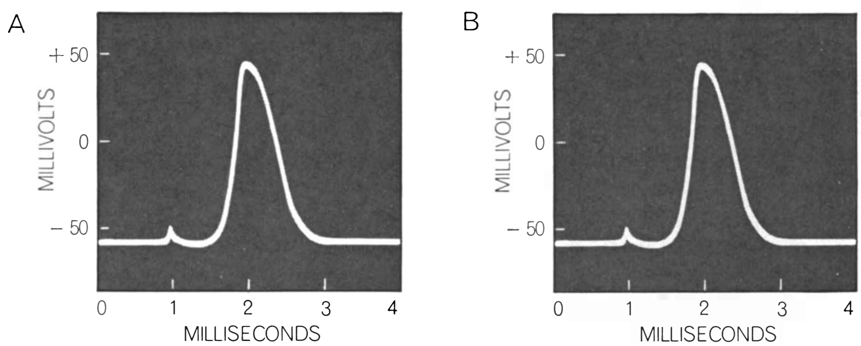
The investigation of the exact way in which tetrodotoxin blocks conduction in nerves has required experiments of much precision and delicacy. Such experiments have been undertaken by John Wilson Moore and Toshio Narahashi of the Duke University School of Medicine and by others. Before taking up this work, it will be helpful to review the way in which a nerve impulse is relayed along the axon from the spinal cord to a neuromuscular junction. The essential conditions for the process are differences in the concentration of sodium and potassium ions inside and outside the axon, and an axon membrane capable of undergoing rapid transient changes in permeability to these ions. In a resting nerve the concentration of potassium ions is higher inside the axon than it is in the external fluid. The concentration of sodium ions is the reverse: it is higher in the external fluid than it is inside the axon. At this time the axon membrane is almost impermeable to sodium ions, but its permeability to potassium ions is sufficient to allow them to diffuse outward. As a result there is an electric potential difference of about 60 millivolts (thousandths of a volt) across the axon membrane and the inside of the axon is negative with respect to the external fluid. When the axon is stimulated, the resting potential at the point where the impulse arises is reduced and the two phases of the action potential begin. In the first phase the axon becomes permeable to sodium ions, allowing them to pass inward through the membrane. The axon remains in this state for one or two milliseconds, during which time the sign of the potential difference across the membrane is reversed: the interior of the axon is about 50 millivolts positive with respect to the external fluid. In the second phase of the action potential the axon becomes comparatively impermeable to sodium ions and highly perme-



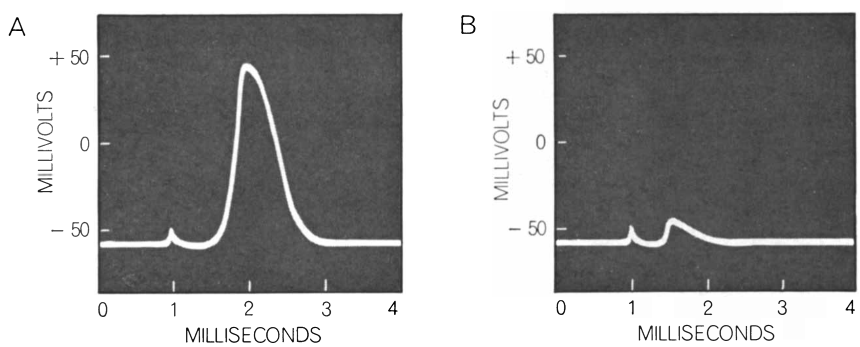
NERVE AXON AND MUSCLE are stimulated to locate the site at which a toxin acts. After a toxin is injected into an animal a nerve axon to a muscle is stimulated. If the muscle fails to contract, the toxin may be acting on axon, muscle or the junction of the two. If direct muscle stimulation does produce contraction, the toxin's site must be the axon or junction.



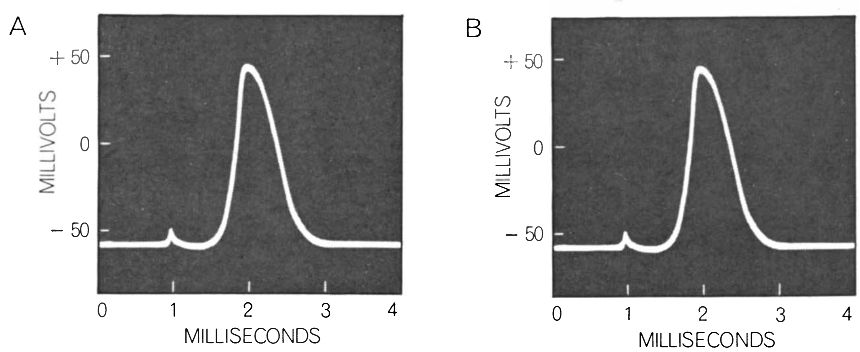
IMMERSED NERVE AXON demonstrates tetrodotoxin's effect on nerve conduction. The axon is stimulated (*left*), producing an impulse marked by a change in the membrane's electric potential that is recorded (*A*). At point *B* the same change is not recorded when tetrodotoxin is present in the solution bathing the nerve between recording points.



NERVE-IMPULSE RECORDS from two points along the axon are exactly the same when the axon is bathed in a solution similar to the blood surrounding it in normal conditions.



WITH TETRODOTOXIN present in the solution bathing the axon, stimulation produces a normal change in electric potential at the first point; a change is not recorded at second.



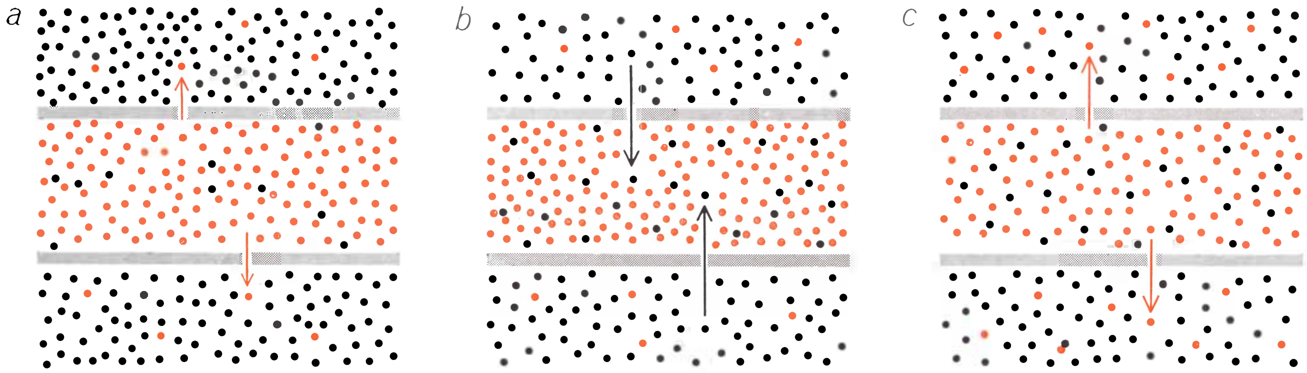
AFTER TETRODOTOXIN immersion the axon is washed and placed in a bloodlike solution. Following stimulation a normal change in potential is recorded at both axon points.

able to potassium ions. Potassium ions pass outward from the inside of the axon and the resting potential difference is restored.

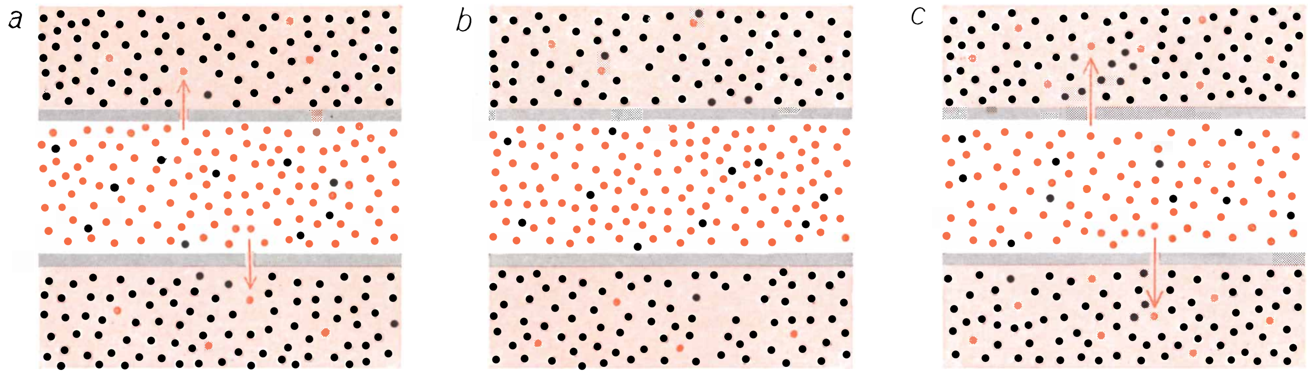
This process, which moves down the axon like a wave, can be blocked by drugs and toxins. Most commonly such agents make the axon membrane equally permeable to both sodium and potassium ions. The potential difference therefore cannot be maintained, which is to say that the membrane is depolarized. Tetrodotoxin, however, has a much more specific effect on the movement of ions. It is for this reason that it provides a new tool for the study of nerve function.

The specific manner in which tetrodotoxin blocks conduction in nerves was learned from experiments with large axons dissected from a squid or a lobster. In these experiments the axon was studied by means of an arrangement called a voltage clamp, with which it is possible to regulate the potential difference across the axon membrane at a desired level. This is done by means of an external voltage source controlled by a feedback amplifier and connected to electrodes on the inside and outside of the membrane. The current that flows through an area of the membrane under the influence of the fixed voltage is measured with a separate amplifier. Such measurements have been made of the electric current flowing through axons that have been immersed in a fluid resembling the one that normally surrounds the axon except that it contains a low concentration of tetrodotoxin. Under these conditions the current carried by sodium ions that move into the axon is first reduced and then completely abolished. The current outward from the axon carried by potassium ions is not affected.

By what kind of mechanism does tetrodotoxin accomplish its selective effect on the flow of ions? It has been demonstrated that when the toxin is present inside an axon rather than outside it, the movement of sodium ions is not suppressed. The toxin must therefore be outside the membrane in order to produce its effect. In other experiments it has been shown that this effect is not restricted to sodium ions. Certain other ions have been substituted for sodium in the fluid surrounding an isolated axon, and it has been found that these ions function as sodium ions do in carrying a current inward across the membrane. When tetrodotoxin is present in such a fluid, however, the current is suppressed just as it is when it is carried by ions of



ION DIFFUSION through the axon membrane establishes an electric potential. Because of concentration gradients, sodium ions (*black dots*) tend to diffuse from outside the axon inward; potassium ions (*colored dots*) tend to diffuse from inside outward. A resting axon is more permeable to potassium than to sodium; this creates a resting potential (*a*). An action potential begins when the nerve is stimulated. First the membrane becomes permeable to sodium ions; they move inward (*b*). After sodium "gates" close, the membrane becomes permeable to potassium ions, which move out (*c*). Action potential ends with reduced potassium permeability.



BLOCKED ION DIFFUSION occurs when a nerve axon is placed in a bloodlike solution containing tetrodotoxin. The diffusion of potassium ions outward from the axon is not affected; a normal resting potential is established (*a*). Following stimulation that normally initiates an action potential, the membrane fails to become highly permeable to sodium ions; their passage is blocked (*b*). The second phase of an action potential is not blocked by tetrodotoxin; potassium ions diffuse normally through the axon membrane (*c*).

sodium. Apparently the toxin's selective effect is not on the ions but on the membrane through which the ions move to carry current inward.

With certain other agents it is possible to block the movement of potassium ions that carries a current outward across the membrane. These agents do not abolish the current flowing inward. To be sure, all these experiments measuring the flow of ions across an axon membrane rely on the voltage clamp and hence study nerve processes under highly artificial circumstances. The British physiologists A. L. Hodgkin and A. F. Huxley have shown, however, that measurements made under these conditions are in accord with knowledge of the normal nerve impulse. It follows from the voltage clamp experiments, then, that there must be two kinds of "gate" in the nerve membrane, one controlling the inward movement of sodium ions during the first phase of the action potential and one the outward movement of potassium ions during the second phase. Exactly how tetrodotoxin produces its effect on the membrane is not known. It may attach itself to the membrane in such a way as to keep the gates from opening or it may block gates that are open.

In addition to the gates through which sodium and potassium ions pass there is in the axon another mechanism for moving sodium and potassium ions: the ion pump. A nerve is capable of transmitting impulses for hours at a time, but ultimately lost potassium ions must be replaced and sodium ions that have penetrated the axon must be expelled. This is accomplished by the ion pump. The routes taken by sodium and potassium ions associated with it are altogether separate from the gates that operate during an action potential.

When conduction in a nerve axon is blocked, signals to and from the central nervous system are not transmitted; if the block occurs in a sensory nerve, local anesthesia results. Most of the common local anesthetics—for example cocaine—block conduction by mechanisms that are less specific than the mechanism of tetrodotoxin. Applied to an isolated frog nerve, tetrodotoxin is about 150,000 times more effective in blocking conduction than cocaine is. Nevertheless, tetrodotoxin is not a good local anesthetic. One reason is that when tetrodotoxin is injected in the vicinity of a nerve, it does not remain in contact with the nerve tissue but diffuses away. Moreover, it is carried by the blood to other tissues; there it can be toxic and may even cause death. Workers in several laboratories have undertaken to learn what group of atoms in the tetrodotoxin molecule is responsible for its specific effect on nerve conduction. If this active group can be identified, it should be possible to synthesize new compounds that have the favorable characteristics of other local anesthetics and that contain the active group of tetrodotoxin.

Fossil Behavior

Some fossils represent the tracks or burrows of ancient animals. Such fossils can seldom be identified with a particular animal, but they do show how the animal behaved and something of how behavior evolved

by Adolf Seilacher

Most of what is known about the evolution of plants and animals has been learned from fossils. One might think that this information is limited to the anatomical changes in living organisms, but such is not the case. There is a class of fossils that provides evidence on animal behavior. These fossils consist not of animal remains but of fossilized animal tracks and burrows.

The great majority of such fossils are markings made in the soft sediments of the ocean floor by ancient invertebrates: ancestral marine worms, starfishes and sea snails, extinct arthropods such as the trilobites, and the like. The tubes and tunnels, trails and feeding marks left by these animals are preserved as raised or depressed forms in layers of sediment that gradually became rock. Paleontologists call such forms "trace fossils"; they classify them in taxonomic arrays and assign them names. Geologists find them useful as indicators of age in formations of sedimentary rock that do not contain the usual kind of fossil. Trace fossils are also clues to the interrelations of organisms and environments—the ecology—in the ancient oceans. This article, however, is concerned with what trace fossils reveal about the behavior of the animals that produced them and how such behavior evolved over periods of millions of years.

The most obvious question to ask about trace fossils is: What animal is responsible for a given track or burrow? This question is one of the most difficult to answer. Except in the case of some trilobites and a few other arthropods, clear-cut "fingerprints" preserved in tracks and burrows are rare or difficult to recognize. As far as the identity of the animal that made them is concerned, many trace fossils may remain a mystery forever. Such fossils can nevertheless be sorted out and classified according to the behavior that gave rise to them. Many of their differences are functional; for example, a hole that was dug as a shelter would differ from a hole made by a sediment-feeding animal in the course of a meal. Similarly, an animal that feeds on the surface of the ocean bottom produces one kind of trail when it is foraging and a distinctly different kind when it is trying to elude a predator.

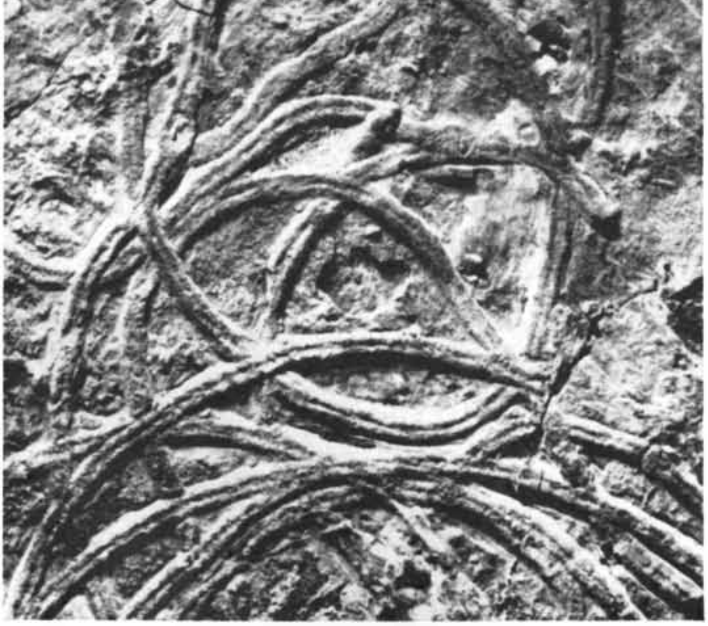
I have found it useful to put trace fossils in groups representing five activities [*see illustrations on pages 74 and 75*]. The first group consists of crawling tracks, indicative of nothing more than simple motion. The second is made up of foraging tracks, marks left by animals that moved along the ocean bottom (or just below it) in the course of feeding. The third consists of feeding burrows, as distinct from foraging tracks, made by animals that tunneled well into the bottom sediments. The fourth contains "resting" tracks made by animals that took temporary refuge by burying themselves in sandy bottoms. The last group is composed of dwelling burrows, the permanent shelters of animals such as marine worms that lived and fed without moving from place to place but gathered their food from outside the burrow.

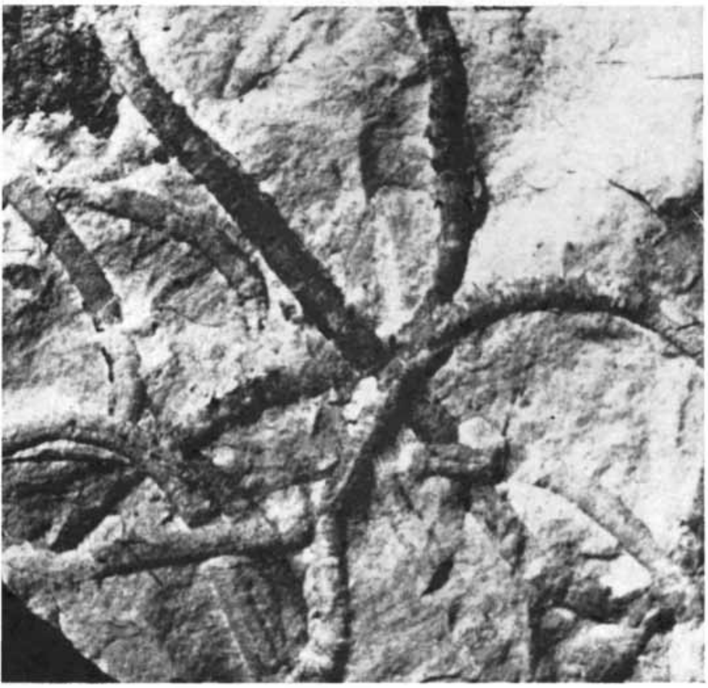
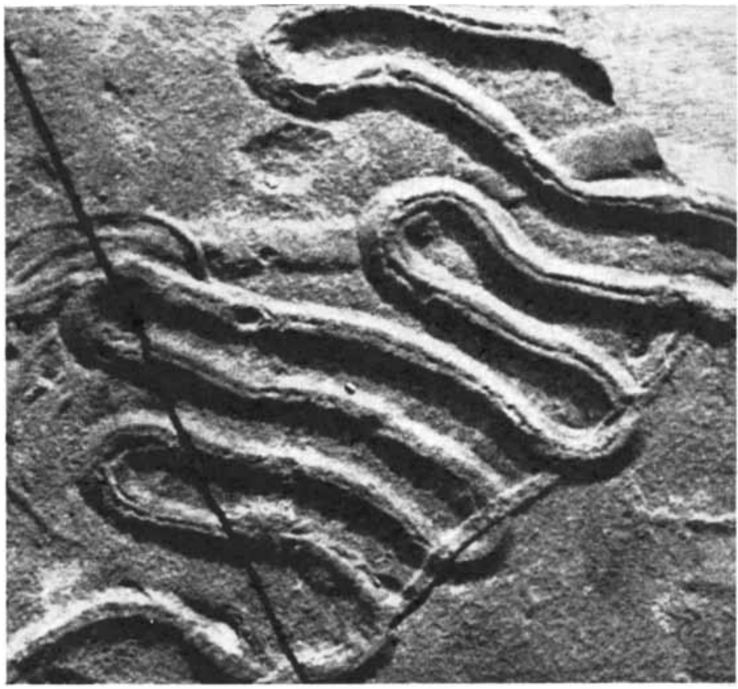
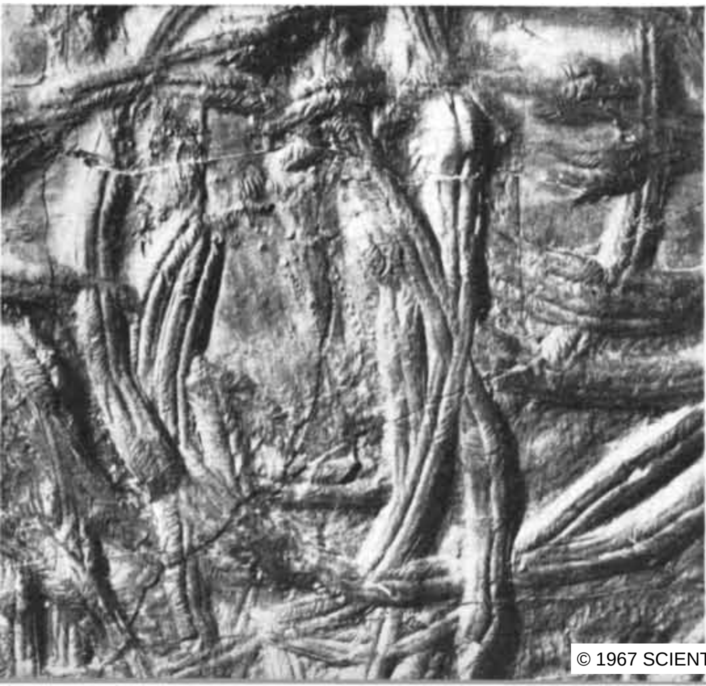
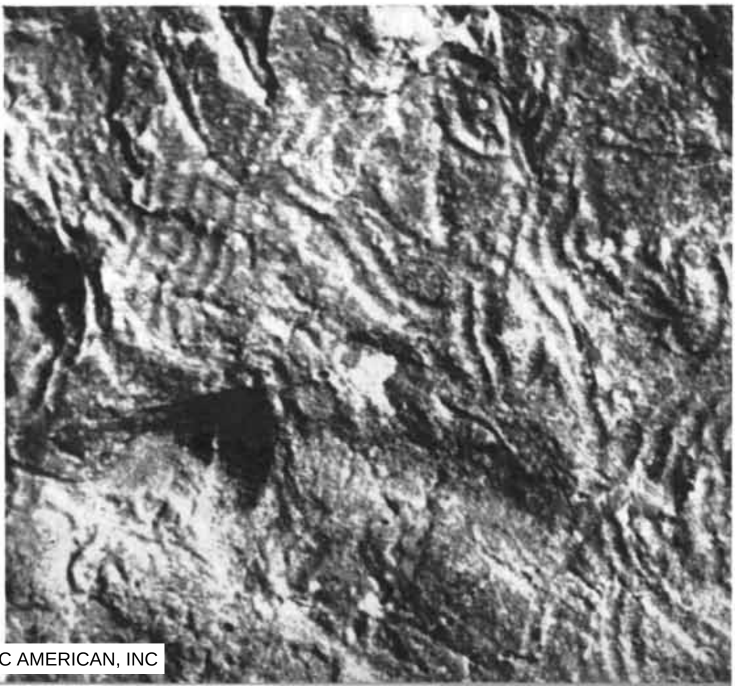
One way of reconstructing fossil behavior is to devise a model of it in the form of a program of commands such as might be written for a computer. The validity of the model can be tested by determining if the sequence of commands will produce actions that are compatible with the fossil evidence. A simple example is provided by "pipe-rocks," seacoast sediments that have now become sandstone and are named for the abundance of vertical dwelling burrows they contain. The pipe-rock burrows, known in trace-fossil taxonomy as *Scolithos,* appear to be the work of animals with a simple pattern of behavior. A model program for the *Scolithos* animals might consist of two commands. The first would be "Dig down vertically for *n* times your length," the second "Avoid crossing other burrows." Behavior responsive to these two commands would suffice to produce pipe-rocks.

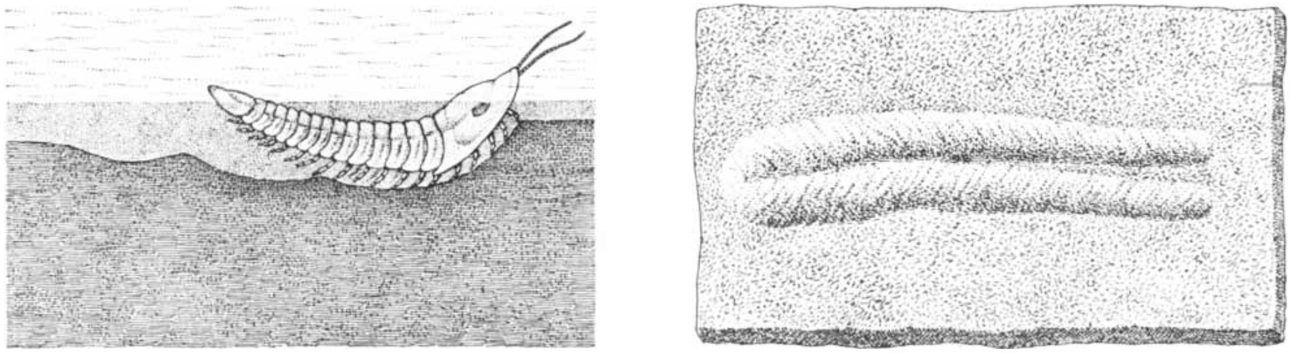
The trace fossils left by animals that feed on sediments, either by grazing on the bottom or by tunneling into it, show particularly prominent behavior patterns. The nutrients in a given area or volume of sediment are best extracted by orderly movements rather than random ones. Efficient sediment-feeders produce regular winding or branching tracks in which repetition of the same kind of turn produces an intricate pattern. Some 40 years ago the German paleontologist Rudolf Richter pointed out the significance of these patterns in a pioneering behavioral analysis of the trace fossil *Helminthoida labyrinthica.*

The animal responsible for the *H. lab-*

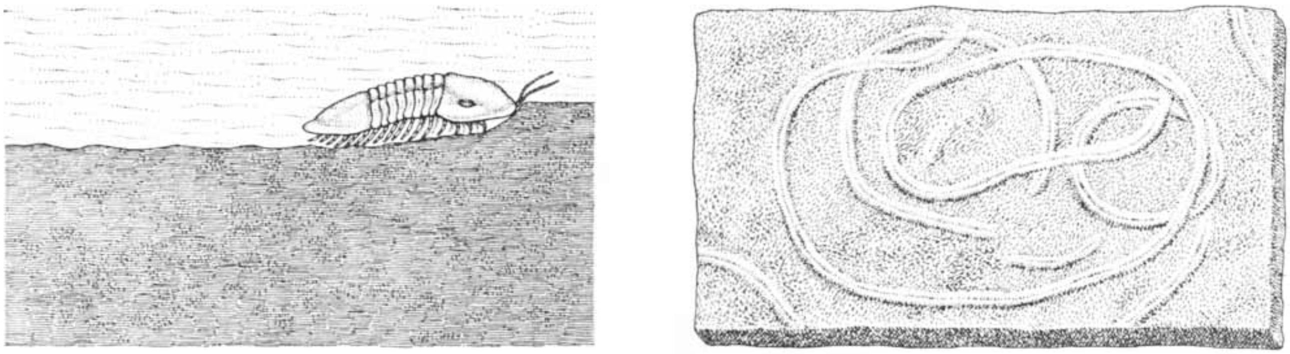
FOSSIL TRACKS on the opposite page suggest how the behavior of animals that lived on the ocean floor evolved in the Paleozoic era (from 600 million to 230 million years ago). At left are primitive "scribbles." The track at top was made during the Ordovician period by a wormlike animal; the track in the middle, during the Cambrian period, possibly by a snaillike animal; the track at bottom, during the Cambrian by a trilobite. At right are more advanced spirals and meanders. The one at top was made during the Mississippian period by a wormlike animal; the one in the middle, during the Pennsylvanian period, possibly by a snaillike animal; the one at bottom, during the Ordovician by a trilobite.



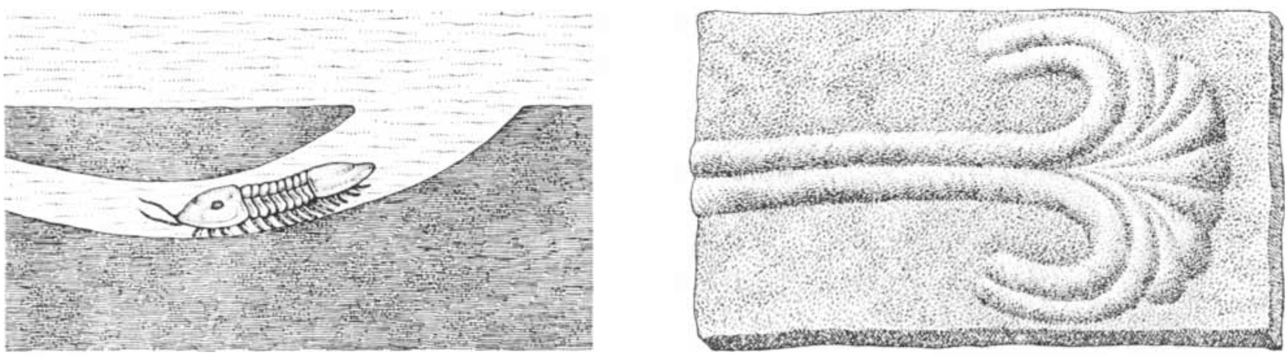







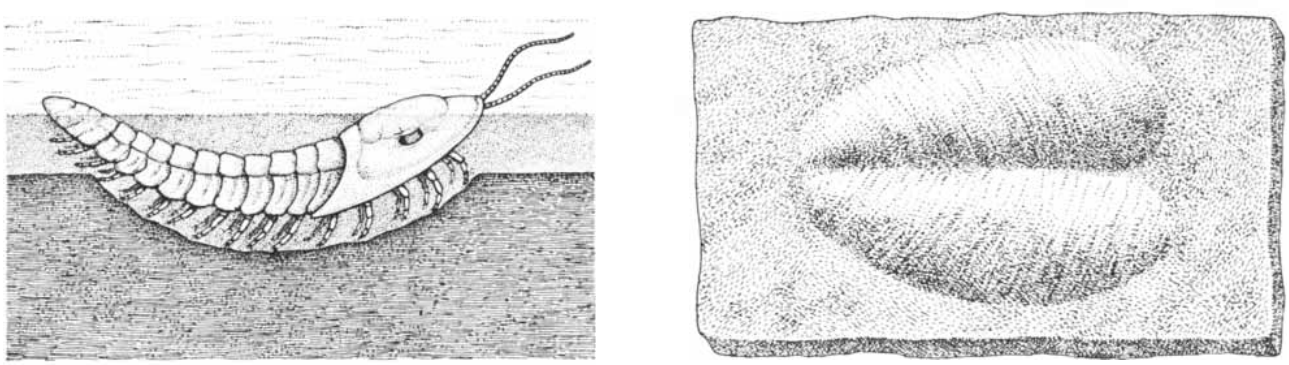
CRAWLING TRACKS comprise one of the five main categories of "trace fossils," which record the various activities but seldom the identity of the animals that made them. The examples illustrated on this page, however, are all attributable to trilobites.



FORAGING TRACKS differ from crawling tracks by recording not simple motion but an animal's search for food over the surface of the ocean floor. The illustrations of fossil tracks and burrows on these pages and on pages 76 and 78 were made by Thomas Prentiss.



FEEDING BURROW is made by an animal that tunnels into the sediments of the ocean floor and eats the bits of organic matter it extracts from the silt. The burrow also provides shelter. The meander and the spiral illustrated on page 76 are feeding burrows.



RESTING TRACKS are the marks produced when an animal, often a scavenger, makes a shallow, temporary hiding place just below the surface of the ocean floor, usually where the bottom is sandy. Their shape generally corresponds to the outline of the animal.

yrinthica markings was a particularly efficient sediment-feeder. Its tunnels are found in fine-grained sedimentary rocks in the Alps and in Alaska, usually silts and marls of Cretaceous and Eocene age (between 135 million and 36 million years ago). To rephrase Richter's analysis as a model program, it appears that the animal obeyed only four commands. The first command was "Move horizontally, keeping within a single stratum of sediment." Obedience to this command is evident in the fact that the animal's tunnels lie within the horizontal laminations of sediment. The second command was "After advancing one unit of length make a U-turn." Here obedience to the command is apparent in the "homostrophy," or uniformity of turning, that is characteristic of the animal's tunnels. The third command was "Always keep in touch with your own or some other tunnel." Biologists call obedience to such a command "thigmotaxis," meaning an involuntary approaching movement in response to the stimulus of contact with some object. That this command was followed is evident in the closeness of the tunnels to one another. The fourth command was "Never come closer to any other tunnel than the given distance *d*." Obedience to this command, which is termed "phobotaxis," would have prevented the animal from digging across another tunnel; crossed tunnels are in fact absent from the fossils.

The model program does not, of course, specify the sensory responses that would have allowed the animal to follow its orders. It is possible, however, to guess what these responses were. For three of the commands nothing more complex seems to be needed than positive or negative chemotaxis: either approach to a chemical stimulus or avoidance of it. Each of the laminations in the sediment would probably have had, so to speak, a characteristic flavor that would have served to guide the animal's horizontal movements and thus would have resulted in obedience to the first command. Obedience to the third and fourth commands is somewhat harder to understand until one considers that as the animal moves it churns up sediment along the sides of its tunnel. Such areas of disturbance are visible in some of the fossils. It seems likely that the animal could chemically distinguish between the disturbed sediment of an adjacent tunnel and the undisturbed sediment in the opposite direction.

The second command, on which the animal's turning maneuver depends, seems to require something other than a chemical stimulus. Its successful execution must be related to the fact that the *H. labyrinthica* animal was shaped like a worm. The length of its body could thus serve as a measuring rod. When it had dug a straight section of tunnel far enough forward, its tail would emerge from the last U-turn and straighten out. Tail-straightening was probably the only cue required for the animal's head to start its next U-turn.

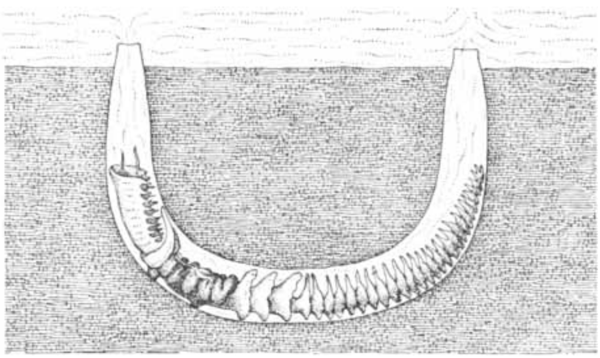
One fact of real life that is neglected in model command programs is that a need to disobey commands must frequently arise. If the tunnel being dug by the *H. labyrinthica* animal had penetrated between two other tunnels, for example, obedience to the third and fourth commands would have left it trapped if it had not violated the first command and moved upward or downward. In such circumstances disobedience is required for survival.

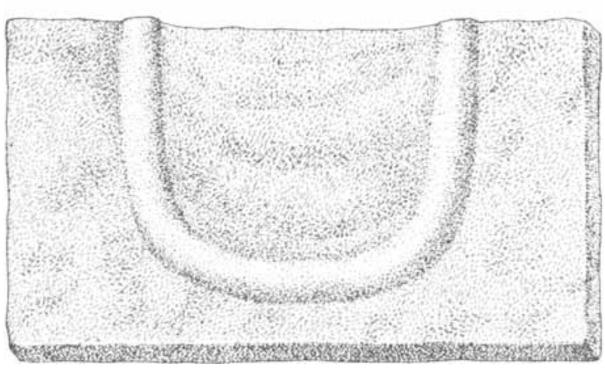
Evidence that the commands were not inflexible is found in the way the animal's meandering turns were executed. The length of the individual "lobes"—the straight passages between the U-turns—are not uniform; some are shorter than average and some are longer. One can assume that a short lobe was formed when contact with some unidentified object triggered obedience to the fourth, or "avoid," command before the entire "forward march" portion of the second command had been executed. A long lobe could have been formed if some accidental bend in the straight part of the tunnel misled the animal into delaying its next U-turn. There is no way to test the hypothesis concerning short lobes. Examination of many fossils shows, however, that in a majority of cases long lobes are associated with secondary bends.

The same behavioral program operating in different animals can be expected to give rise to a wide variety of patterns because of the differences in the animals' modes of locomotion, feeding, responses to commands and even simpler differences such as body length and turning ability. The *H. labyrinthica* animal, for instance, could make very tight turns. The animal that produced the trace fossil *Helminthoida crassa,* however, was apparently less agile: its turns tend to be shaped like a teardrop. Judging by these loose meanders, the *H. crassa* animal was also less sensitive to thigmotactic commands.

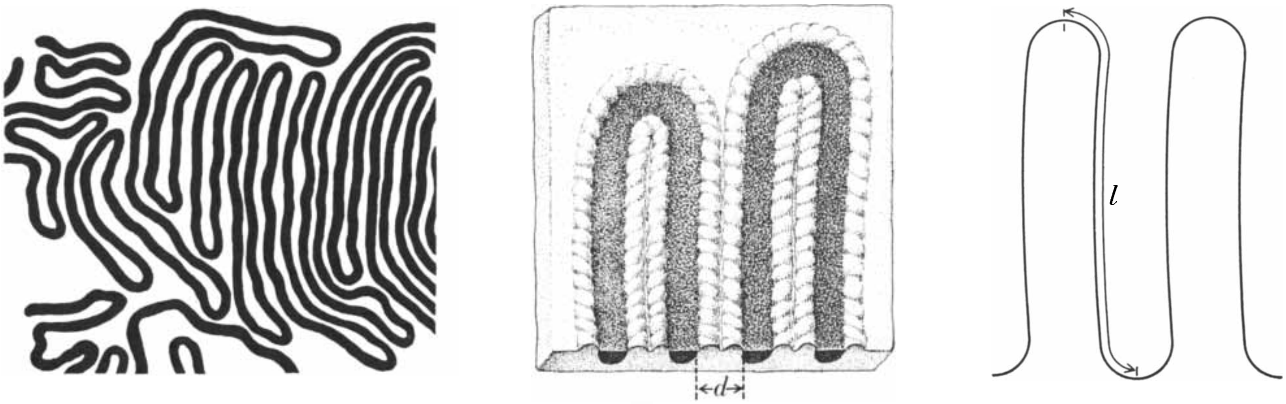
A further example of such meandering trace fossils should be mentioned because it demonstrates how a relatively small change in a command program can result in a highly complicated meander. In the Alps and Alaska and other regions of folded sedimentary rocks there are series of a particular kind of sandstones called graywacke. On the bottom of graywacke layers one finds the complicated trails of *Spirorhaphe.* If one draws a loose inward spiral, makes a U-turn at the center and spirals outward again in the space between the inward whorls, one has traced the path taken by the *Spirorhaphe* animal.

I thought at first that it would be very

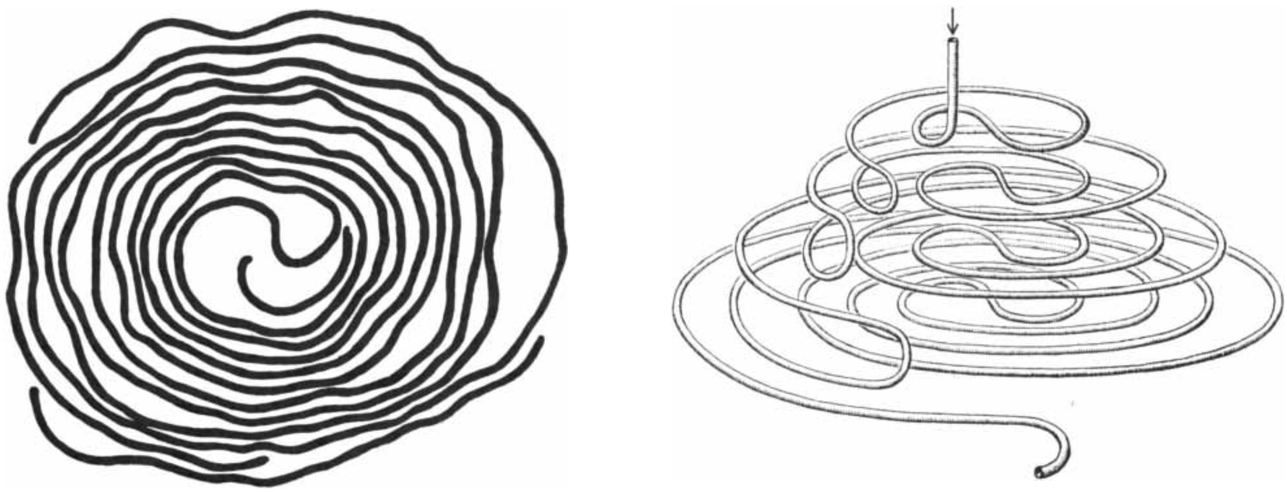




DWELLING BURROW differs from other trace fossils because it represents the shelter of an animal that is stationary and may even be anchored. The animal illustrated here is a polychaete worm that feeds by filtering particles of food from the water around it.



HORIZONTAL TRAIL (*left*), **composed of tight meanders, was made by an unknown wormlike animal. The diagram in perspective shows how the animal's passage through the sediments on which it fed churned up the silt on each side of its tunnel. The width of this churned area established the critical distance ("*d*" *in perspective diagram*) that separates adjacent trails. The average distance between one of the animal's U-turns and the next ("*l*" *in plan view*) seems to have been determined by the length of its body.**



SPIRALING TRAIL (*left*) **is actually a double spiral made by a wormlike animal that circled inward, made a U-turn at the center and circled outward again in the empty area between its entering whorls. "Mistakes" in the position of the U-turns in many specimens and the presence of short alien trails nearby** (*short segment in center at left*) **made author aware that the fossils preserved only a single, horizontal segment of what had been an elaborate, three-dimensional burrow in the silt** (*hypothetical outline at right*).

difficult to devise a command program for the motions required to make such a double spiral. In examining a number of *Spirorhaphe* specimens, however, I noted small "mistakes" in several of them. In each case the animal had been forced to reverse its course too soon because the center of its spiral was already occupied by an alien U-turn (for which no inward and outward spirals could be seen). The meaning of these mistakes soon became apparent: each *Spirorhaphe* fossil preserves only one horizontal layer of what had been a multifloored, three-dimensional tunnel [*see lower illustration above*]. The "alien" U-turns are the earlier work of the same *Spirorhaphe* animal tunneling down from above. The spiral portions of the earlier whorls, which lay at a higher level, have not been preserved.

Although the three-dimensional tunnels are complex in appearance, the program required to produce them is simpler than the one required for the one-story pattern. Only two commands need be added to the four that produce horizontal meanders. The first additional command is "After spiraling inward make a U-turn and keep in contact with the adjacent tunnel on the horizontal." The second is "After spiraling outward turn down and keep in contact with the adjacent tunnel vertically." The wormlike *Spirorhaphe* animal could distinguish between inward and outward spirals by means of a cue similar to the one responsible for homostrophy among the animals that meander horizontally. As the animal spiraled inward its head end would be more tightly curved than its tail end. As it spiraled outward after making its U-turn the tail end would be more tightly curved.

What are the ways in which the evolution of behavior among trace-fossil animals can be detected? One is the comparative study of groups of fossils that appear to be both closely related and closely associated in space and time. One such group is the Graphoglypt family; two of the trace fossils I have mentioned, *Spirorhaphe* and *Helminthoida crassa,* belong to this group. Graphoglypt trace fossils are alike in their time range, being found mainly in formations of Cretaceous to Tertiary age (between 135 million and some two million years ago). They are found in the same kinds of rock and are preserved in the same way. It seems probable, in view of the factors they have in common, that they were made by closely related animals, al-

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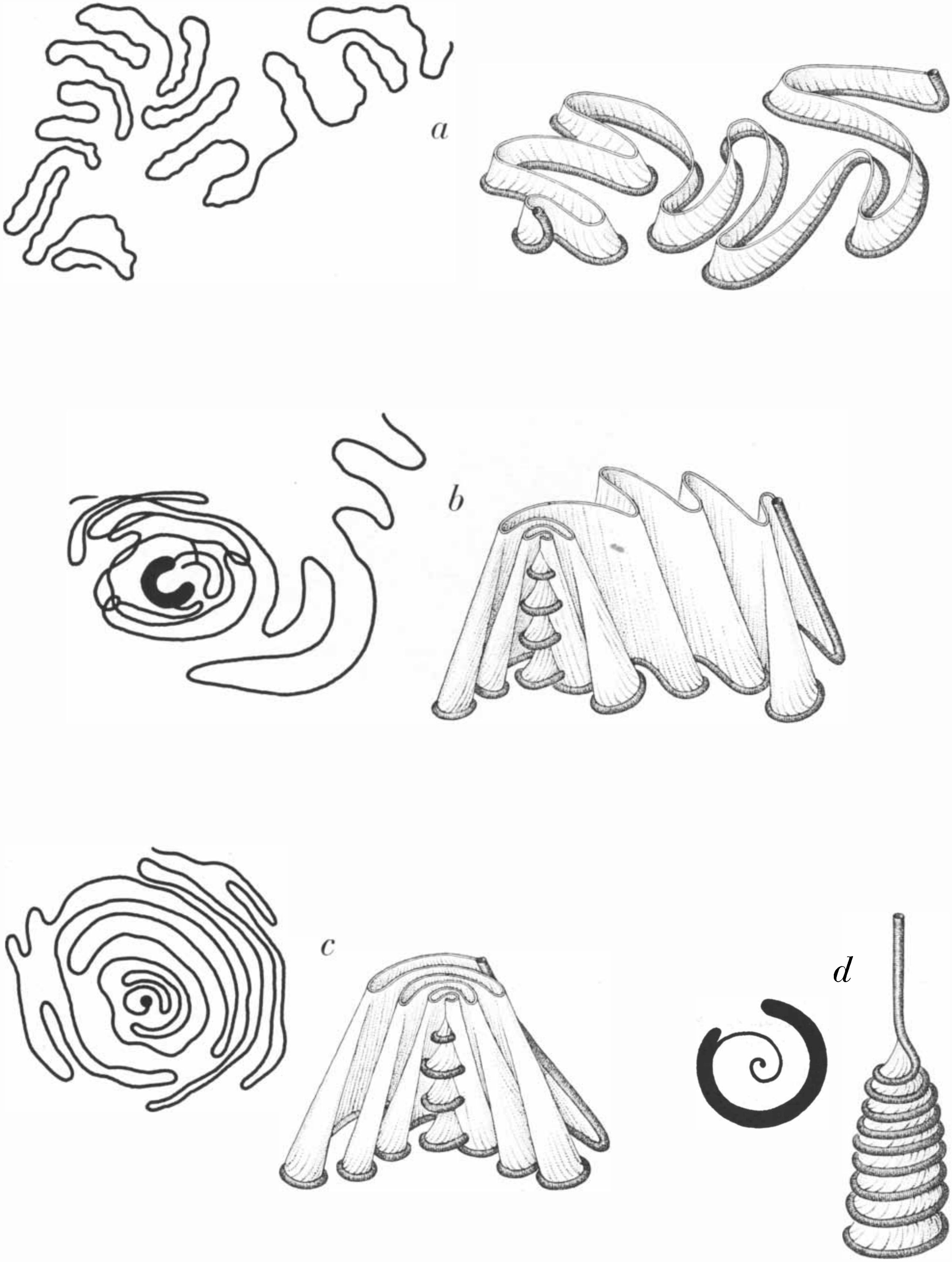

CHANGE IN BEHAVIOR over 150 million years is apparent in the trace fossil *Dictyodora*, the trail left by an animal that seems to have possessed a siphon. In Cambrian times it fed by meandering a few millimeters down in the sediment (*a*). The path produced during its foraging is at left; a three-dimensional reconstruction of the entire fossil track is at right. Later the animal left the upper sediment, eating its way deeper along a corkscrew-like path and then meandering in a restricted manner (*"b" and "c"; path at left, reconstruction at right*). By middle Mississippian times the animal no longer meandered (*d*) but only corkscrewed deep into the silt.

though what animals they were we do not know. When the group is examined as a whole, the various Graphoglypt genera and species can be arranged in a few lines of descent that have one trend in common. In terms of behavioral programming, the lines of descent show a gradual shift from rigid behavior patterns to patterns that gave the animals freedom to adapt to local circumstances. This trend toward flexibility enabled the animals to forage more efficiently, a factor that is of obvious value in terms of natural selection.

A second example of behavioral evolution is apparent when trace fossils are examined in a more general way to see what changes have taken place over millions of years in behavior that deals with one specific biological task. The obvious task to study is foraging, and we quickly find that several methods of foraging other than meandering exist. The simplest of them resembles the scribbling of children: the tracks form a series of circles with slightly offset centers. The program for scribbling has one simple command: "Keep heading to one side but don't stay in the same track." Scribbling, however, covers an area much less efficiently than meandering does.

A somewhat more complex method of foraging, and one more efficient than scribbling, consists of moving in an outward spiral. The tighter the spiral, the more efficient the coverage. The program for tight spiraling has two commands. One is "Keep circling in one direction"; the other is "Keep in touch with the spiral whorl made earlier." This program is simpler than the four-command program for meandering, but spiraling is less efficient than meandering. The strips between the spirals remain unexploited.

Several unrelated groups of trace fossils provide evidence that the progress from simple to complex forms of foraging is an evolutionary one [*see illustration on page* 73]. In Cambrian times, at the beginning of the Paleozoic era some 600 million years ago, none of the animals of the ocean floor had evolved the meandering method. Scribbling was practiced by some trilobites and by what were probably varieties of marine snails. In a heterogeneous group of trails attributed to various unknown worms good scribbles appear just after the end of the Cambrian, in Ordovician times (between 500 million and 425 million years ago). Eventually, however, scribbling disappears altogether and worms, snails and trilobites all begin to forage by meandering, with spiraling as an occasional alternative method. Complex meanders and dense double spirals are features of

ANTLER-SHAPED BURROWS are the work of an anchored animal of the Cretaceous period, roughly 100 million years ago. The lobes in the sediment that the animal excavated for their food content were narrow and much of the adjacent sediment was left unexploited.

SKIRTLIKE BURROW is the work of the same kind of animal some 40 million years later, in the Tertiary period. One large excavation is more efficient than the series of lobes made in Cretaceous times. Early in the life of the animal, however, its burrow is Cretaceous in design (*bottom left*), showing that behavioral ontogeny can recapitulate phylogeny.

comparatively late geologic history, not appearing until Cretaceous times, at the end of the Mesozoic era (between 135 million and 63 million years ago). They indicate further progress in the foraging efficiency of sediment-feeders.

There is a well-documented instance in which an anatomical change in a trace-fossil animal was accompanied by a change in its behavior. The fossil *Dictyodora* is the work of an unknown sediment-feeder that tunneled into the ocean floor and filled its tunnel behind it. The *Dictyodora* animal, however, seems to have been equipped with a long, thin siphon that allowed it to maintain contact with the water above it. As the animal traveled in loose meanders through the sediments, its siphon, dragging behind it, left its own marks in the ooze.

From Cambrian to Devonian times, between 600 million and 350 million years ago, *Dictyodora* animals foraged only a few millimeters down in the sediment. Their siphons were short. This particular ecological niche, however, was within reach of many smaller competitors. By Mississippian times (beginning about 350 million years ago) the *Dictyodora* animals had become adapted to feeding at a deeper and less crowded level. Their siphons had become longer and their meandering trails were well below the reach of their sediment-feeding contemporaries.

As the *Dictyodora* animals evolved anatomically they also altered their behavior. In earlier millenniums they had apparently not begun to feed until they had worked their way down to a specific foraging level. The Mississippian animals, however, ate their way deep into the sediments, leaving a corkscrew trail behind them, before they began their horizontal meanders. Moreover, the first several meanders no longer followed the older loose pattern. Instead they curled concentrically around the initial corkscrew.

As time passed the *Dictyodora* animals' behavior became simpler. Trace fossils from later Mississippian formations in East Germany show a high percentage of *Dictyodora* burrows in which loose meanders are altogether absent, concentric meanders taking their place. Trace fossils from southern Austria, which are possibly even younger, show a further behavioral change. Most of the burrows have no meanders at all; the central corkscrew trail has simply been drilled deeper. It is doubtful that the *Dictyodora* animals remained in the same burrow all their lives. During their reproductive cycle, for example, the animals probably rose to the surface of the sediment. Thereafter they would have had to dig new burrows, if they did not begin a new mode of life altogether.

Trace fossils include the burrows of a few animals that led a sedentary existence. These burrows, of course, are a record of the animals' entire life. One such fossil is *Zoophycos;* it is the burrow of an unknown wormlike animal that foraged through the sediments by constantly shifting the lobes of its U-shaped tunnel. The two openings of the tunnel remained fixed in the sediment, but thin concentric layers inside the lobes record the tunnel's shifts of position. *Zoophycos* burrows have been found in sedimentary rocks as old as the Ordovician. In Cretaceous and Tertiary times (from 120 million to some two million years ago) the *Zoophycos* animal seems to have taken over the *Dictyodora* animal's ecological niche in the deep sediments that have since become Alpine rocks.

When the burrows of a *Zoophycos* animal of Cretaceous and Tertiary times are compared, an evolution in behavior is readily apparent. The earlier burrow consists of narrow lobes that join to form an antler-like pattern [*see top illustration on preceding page*]. The sediment that lay between the lobes remained exploited. In Tertiary *Zoophycos* burrows, however, the individual lobes have fused into a continuous, skirtlike pattern inside which all the sediment has been explored [*see bottom illustration on preceding page*]. A much more effective coverage of a food-bearing area has resulted from what was probably a relatively small change in the *Zoophycos* animal's behavioral program.

Surprisingly this more efficient behavior is found only in the portions of the burrow occupied by the adult Tertiary animal. The behavior of the young animal remained like that of its Cretaceous ancestor, producing the same narrow lobes. Thus we see that the old adage about ontogeny recapitulating phylogeny can be applied to an animal's behavior as well as to its anatomy.

Scanty though the trace-fossil material now available is, it gives one hope that the early evolution of behavior patterns will become as valid an area of study as the evolution of anatomical structures. Further insights into fossil behavior should not only add a new aspect to paleontological research but also help to counter the belief that paleontologists are concerned only with dead bodies and have no real comprehension of ancient life.

OPPORTUNITIES

At the Fort Worth Division of General Dynamics, virtually all technical disciplines are needed to fulfill the variety of Research and Engineering manpower requirements. Disciplines and their application to functions in Research and Engineering are shown below to illustrate the scope of the assignments and career opportunities offered.

AERONAUTICAL ENGINEERS
All Degree Levels

Aero—Aeroanalysis, stability and control, aerodynamic design, performance, flight mechanics, flight test planning and analysis. **Propulsion**—propulsion system analysis, internal aerodynamics, aerothermodynamics. **Structures**—stress analysis, weight control, structural loads/criteria, structural dynamics / flutter / vibration. **Design**—structural, propulsion and fluid systems. **Other**—model test, structures and fluid dynamics test laboratories, liaison.

CIVIL ENGINEERS
All Degree Levels

Stress analysis, structural loads/criteria, structural dynamics/flutter/vibration, structural design, structures laboratory.

ELECTRICAL ENGINEERS
All Degree Levels

Systems—navigation and guidance, weapon delivery, flight control, airborne and ground data acquisition and processing (digital, analog and hybrid), ground support, electrical, radar homing and warning, mission and traffic control, IR detect and evasion, ECM, radar and fire control. **Test Facilities**—radar and antenna ranges, design and development laboratories, electrical and electronic test laboratories. **Other**—antennas, electromagnetic scattering, radar and ECM simulation, flight and ground test instrumentation, microelectronics, nuclear detectors, maintainability/reliability.

MECHANICAL ENGINEERS
All Degree Levels

System Design—Mechanical, structural, arresting and landing gear, crew station, environmental control, escape, pneudraulic, pyrotechnic, fuel and oil. **Installation Design**—propulsion and armament systems. **Other**—advanced design concepts, reliability/maintainability, value/safety engineering, materials/processes, structural/fluid dynamics laboratories, propulsion system analysis, aerothermodynamics, weight control, model design, liaison.

PHYSICS
Predominantly M.S. and Ph. D.

Nuclear—Shielding, radiation effects, detectors/instrumentation system, reactor analysis. **Other**—infrared /visible/ultraviolet sensor systems, photography/camera systems, flight mechanics, engineering test laboratories, applied research (Ph.D. candidates only).

MATHEMATICS
Predominantly M.S. and Ph.D.

Operations research, information processing systems, digital computation and data analysis, reliability, maintainability, flight sciences, system analysis and synthesis, management information systems, math modeling.

INDUSTRIAL ENGINEERING
All Degree Levels

Operations research, value engineering, reliability, maintainability, management planning and control, contract data and specifications, management support.

OTHER DISCIPLINES
All Degree Levels

Engineering—metallurgical, chemical, systems, ceramic, nuclear. **Other**—industrial management, business administration, industrial psychology, behavioral sciences, economics.

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GENERAL DYNAMICS

Fort Worth Division

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But now the police have a new weapon against crime—an IBM computer. Dick McDonell joined IBM to help develop the computer's law-enforcement potential on a nationwide scale.

He and his colleagues have tightened the net around stolen cars by devising an information system that ties together more than 400 different police agencies.

Now, police can file "hot" car reports immediately from hundreds of remote terminals, all wired into a central IBM computer. A patrolman who suspects a car is stolen can radio in its license number—get an answer in seconds—and give chase if the car *is* hot.

This is just the beginning. Similar systems throughout the nation are also providing facts on such items as stolen goods, criminal records and outstanding warrants. And these growing networks are being tied together to make this information on crime available throughout the country.

"Computers can't make arrests," says McDonell. "But in a split second, they can give the policeman his most effective weapon against crime—information."

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Solid Helium

It is a material in which quantum phenomena play an important role in determining gross properties. Nonetheless, in a surprising number of cases its behavior resembles that of an ordinary classical solid

by Bernard Bertman and Robert A. Guyer

As a gas helium is a remarkably uninteresting element. It is colorless, odorless, tasteless, chemically inert and theoretically unexceptional. At temperatures near absolute zero, however, helium takes on a completely different character, and its behavior in such circumstances has intrigued physicists for more than 50 years. One indication of this anomalous behavior is the fact that at atmospheric pressure helium is the only substance that remains liquid all the way down to absolute zero. It is possible, however, to obtain solid helium; this is done by subjecting the liquid to a pressure of about 25 atmospheres, or 375 pounds per square inch.

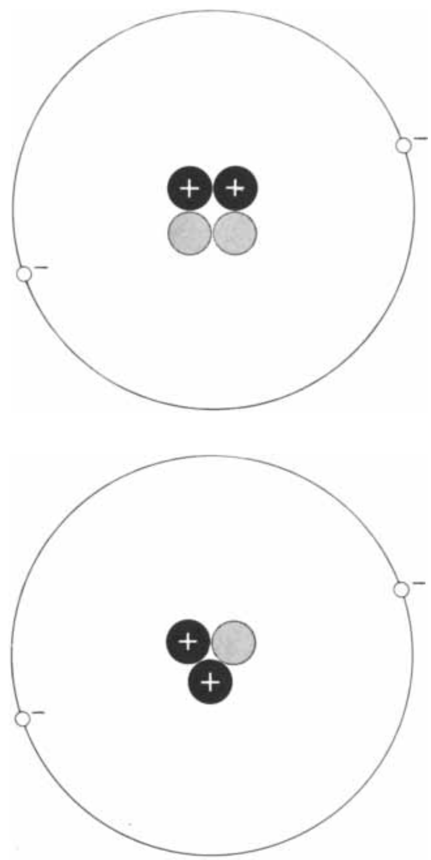
Both as a liquid and as a solid, helium exhibits many unusual properties. Below a certain temperature, for example, liquid helium becomes a "superfluid," in which the liquid flows without friction [see "Superfluidity," by Eugene M. Lifshitz; SCIENTIFIC AMERICAN, June, 1958]. Solid helium, on the other hand, is the softest solid: the application of a moderate pressure results in a large change in the spacing of the atoms in the crystal lattice. Such changes in interatomic spacing in turn alter the solid's thermodynamic properties.

The unique properties of solid helium are attributable to a combination of two factors: the small mass of the helium atoms and the weak force that exists between them. (Only hydrogen atoms are lighter, and they interact much more strongly.) It is this combination of factors that makes the theoretical treatment of liquid and solid helium so complex, since it enables quantum-mechanical effects to play a major role in determining certain properties. Solid helium, in fact, is the only known example of a "quantum solid," just as liquid helium is the only known example of a "quantum liquid." Perhaps the most perplexing characteristic of solid helium, though, and one that has engaged our special interest for several years, is the fact that in spite of its unique quantum nature solid helium behaves in so many respects as a purely classical, or nonquantum, solid.

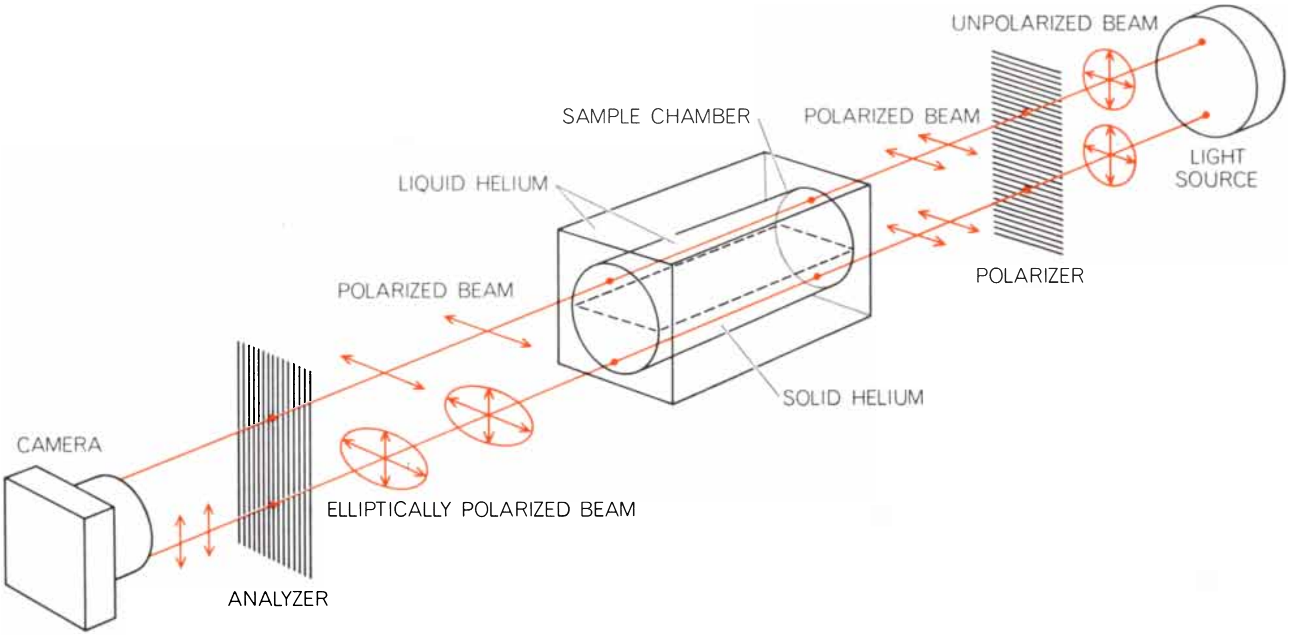
SINGLE CRYSTAL of solid helium (*white semicircle at lower center*) was photographed through the transparent end window of a cylindrical growing chamber maintained at a temperature of about 1.3 degrees Kelvin (degrees centigrade above absolute zero) and a pressure of about 25 atmospheres. The blue semicircle at upper center is liquid helium. The photograph was made during an experiment performed by O. W. Heybey and D. M. Lee at Cornell University. A schematic drawing of the photographic technique appears at the top of the next page.

There are two stable isotopes, or nuclear species, of helium: helium 4, with two protons and two neutrons in its nucleus, and helium 3, with two protons and one neutron [*see illustration at right*]. Each isotope has a closed electron shell containing two electrons. The electrons are tightly bound to the nuclei, so that it is difficult for them to participate in a chemical bond. Hence helium is classified as an inert, or noble, gas. Like the solids formed by the other noble gases, solid helium is a dielectric, or electrically insulating, substance [see "Solid Noble Gases," by Gerald L. Pollack; SCIENTIFIC AMERICAN, October, 1966].

The range of temperature and pressure over which a substance exists in various phases—gas, liquid or solid—is represented by a phase diagram. As the phase diagram for helium 4 shows, at pressures greater than 25 atmospheres this isotope exists as a solid with a "hexagonal-close-packed" atomic structure [*see top illustration on page* 87]. Over a small range of temperatures and pressures (temperatures between 1.3 and 1.8 degrees Kelvin; pressures between 26 and 31 atmospheres) helium 4 exists in a "body-centered-cubic" structure. At very high pressures (higher than 1,100 atmospheres) the solid assumes a "face-centered-cubic" structure. The three basic



TWO STABLE ISOTOPES, or nuclear species, of helium are depicted here. Helium 4 (*top*) has two protons (*black*) and two neutrons (*gray*) in its nucleus, whereas helium 3 (*bottom*) has two protons and one neutron. Each stable helium isotope has a closed electron shell containing two electrons (*white*).

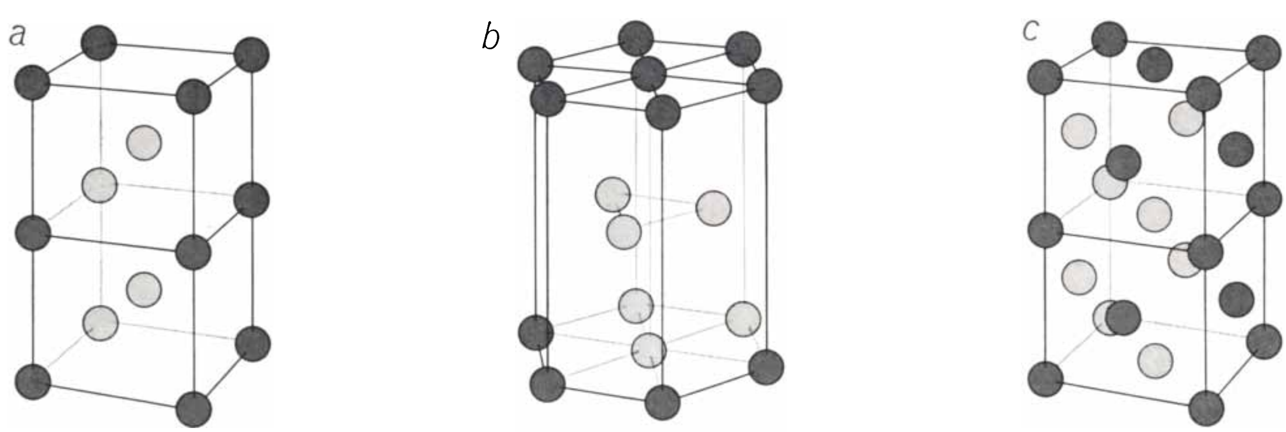


SPECIAL TECHNIQUE was required to make the color photograph on page 84 because both liquid and solid helium are entirely transparent with nearly the same index of refraction; as a result the boundary between the two phases is normally almost impossible to see. The sample chamber was placed in a container of liquid helium between two crossed polarizing filters and illuminated from the back with white light. Since the polarization state of the light is not affected by transmission through a liquid, almost no light reaches the film through the top half of the sample chamber, which contains liquid helium. The solid helium in the bottom half, on the other hand, is birefringent (that is, it refracts light at different speeds in different directions), and consequently the solid changes the polarization of the transmitted light, allowing some of the light to pass through the second polarizing filter and reach the film.

crystal structures are shown in the illustration below.

In the phase diagram for helium 3 the solid region is essentially the same as the solid region for helium 4, with the exception that the temperature-pressure range over which helium 3 exists in the body-centered-cubic phase is considerably greater than the corresponding range for helium 4 [*see bottom illustration on opposite page*]. Solid helium is the only dielectric solid in which these body-centered-cubic phases occur. The preference of a solid for existing in one phase, or one geometric arrangement of atoms, rather than another arises from the fact that all physical systems tend toward the state in which their total energy is at a minimum. For solid helium 3 at pressures lower than 100 atmospheres less energy is required to arrange the atoms in a body-centered-cubic geometry than in a hexagonal-close-packed geometry. Helium 4, on the other hand, prefers the body-centered-cubic geometry at lower pressures (in the vicinity of 25 atmospheres) and only within a fixed temperature range. The preference for a body-centered-cubic geometry over the hexagonal-close-packed geometry is intimately related to the small mass of the helium atoms and the weak attractive force between them.

The most important consequence of the combination of small mass and weak interaction is that the atoms in a solid-helium crystal have a large zero-point motion. This concept is purely quantum-mechanical and has no classical analogue. To grasp its meaning it is necessary to venture for a moment into the mathematical language of quantum mechanics. The quantum-mechanical description of a particle is given in terms of its wave function [*see top illustration on page 88*]. The wave function is large in a region of space where there is a high probability of finding the particle

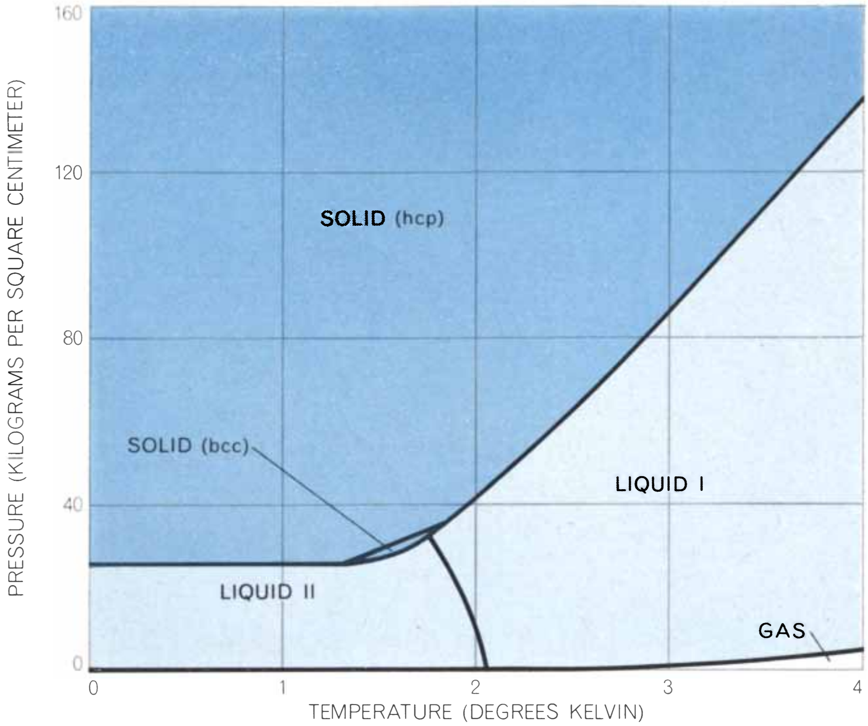


THREE CRYSTAL STRUCTURES formed under different conditions of temperature and pressure by the atoms in solid helium are the "body-centered-cubic" structure (*a*), the "hexagonal-close-packed" structure (*b*) and the "face-centered-cubic" structure (*c*).

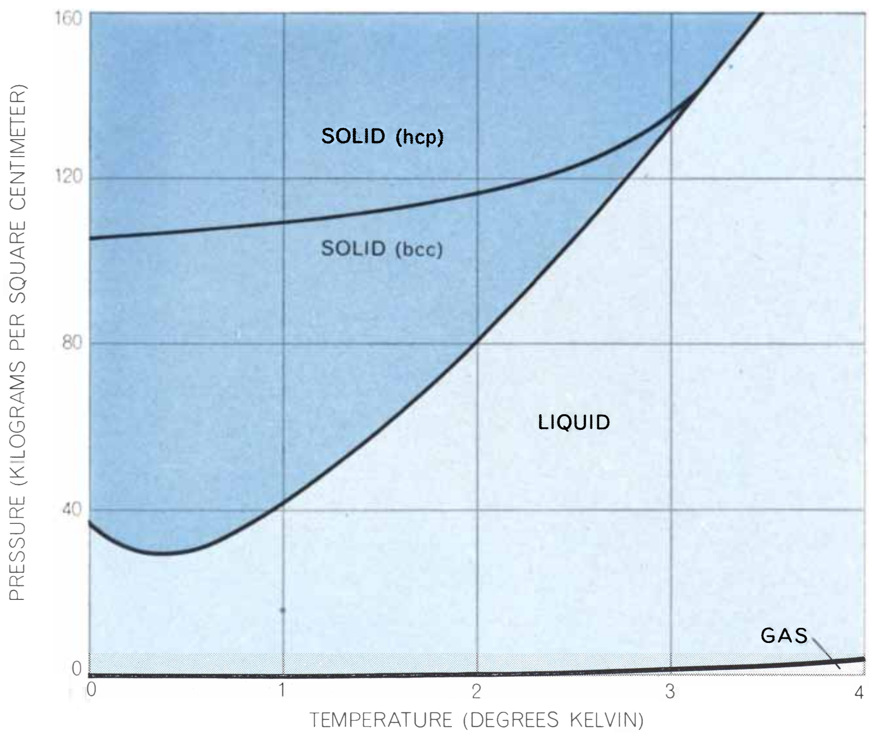
and small in a region where there is a low probability of finding the particle. The dynamic properties of the particle are related to the shape of the curve that represents the wave function. For example, the kinetic energy, or energy of motion, of the particle is inversely proportional to the square of the width of the wave function. Hence a particle whose wave function has a width w has four times as much kinetic energy as a particle whose wave function has a width $2w$. If two particles have wave functions that are the same width, the lighter of the two particles will have the higher kinetic energy, because kinetic energy is also inversely proportional to mass. It is in this way that the small mass of the helium atom exerts an influence on the crystal structure of solid helium. The zero-point energy of a particle is the kinetic energy of the particle when it is in the lowest energy state consistent with the nature of its environment.

The zero-point energy of a particle is strongly influenced by the particle's potential energy, or energy due to its environment. The potential energy of a particle in a crystal lattice is represented by a parabolic potential-energy curve in which the particle is at rest at the bottom of the potential "well" [*see bottom illustration on next page*]. The force with which a particle in such a well is pushed toward the center of the well (the point of lowest potential energy) is proportional to its distance from the center of the well. In the quantum-mechanical picture of a particle in a parabolic potential well the potential energy of the particle is directly proportional to the square of the width of its wave function. Therefore the potential energy of such a particle can be made arbitrarily small by reducing the width of its wave function. This can only be done, however, at the expense of the zero-point kinetic energy, which varies inversely as the square of the width of the wave function. The final width of the particle's wave function is the outcome of a compromise between two competing factors: the reduction in the zero-point kinetic energy that is accomplished by increasing the width of the wave function and the increase in potential energy that accompanies such an increase [*see illustration on page 89*].

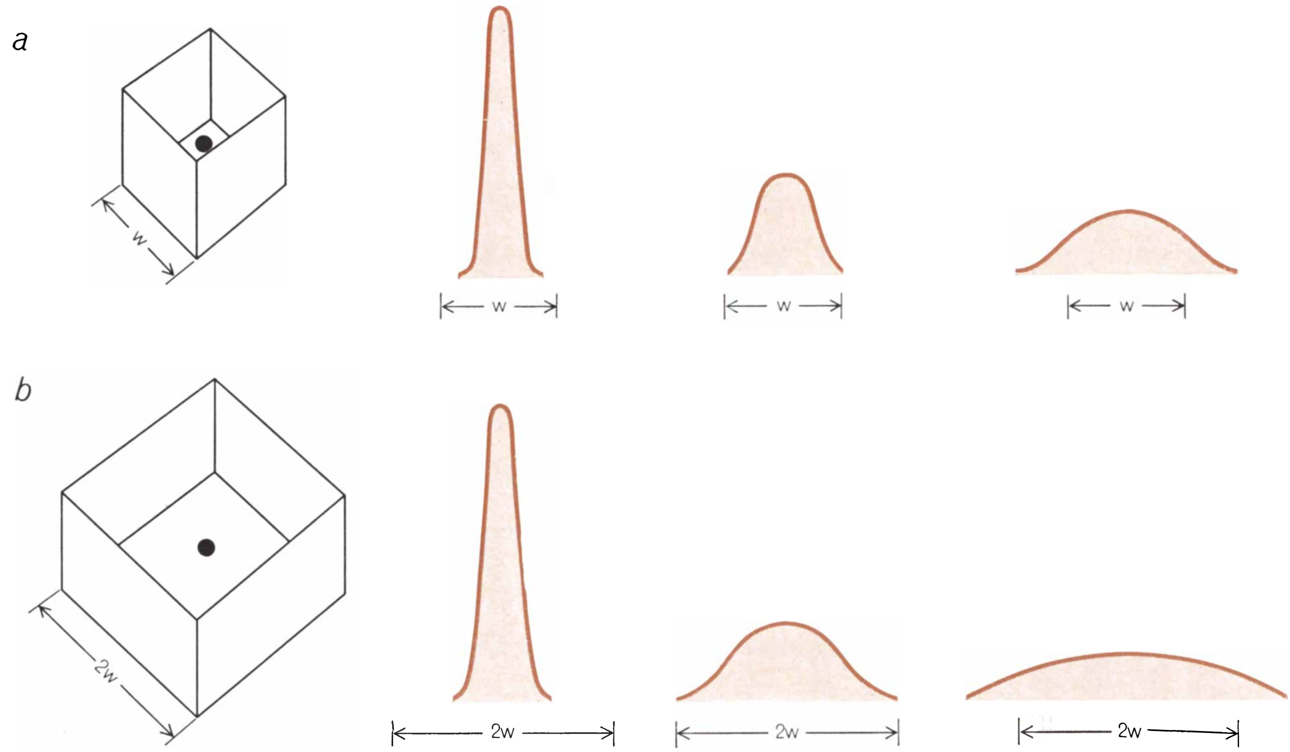
The potential-energy curve for solid helium is similar to the curves of the other solid noble gases, for example the curve for argon [*see top illustration on page 90*]. The important difference here is that the potential well for argon is about 10 times deeper than the one for helium. An argon atom can reduce its



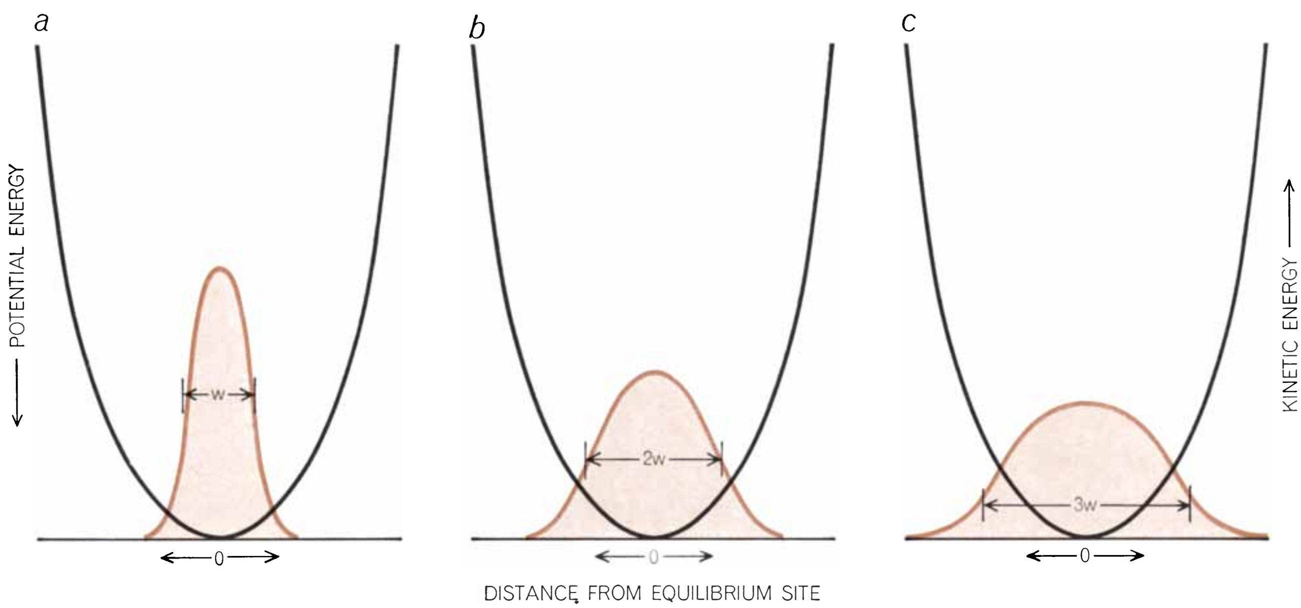
PHASE DIAGRAM FOR HELIUM 4 gives the range of temperature and pressure over which this isotope exists in various phases in the neighborhood of absolute zero. As the diagram indicates, helium 4 exists as a liquid down to the lowest temperatures unless an external pressure is applied. Below a certain temperature the liquid behaves as a superfluid and is designated helium II to distinguish it from the normal liquid, called helium I. At pressures greater than 25 atmospheres the isotope exists as a solid with a hexagonal-close-packed (*hcp*) structure. At temperatures between 1.3 and 1.8 degrees K. and pressures between 26 and 31 atmospheres helium 4 exists in a body-centered-cubic (*bcc*) structure. At very high pressures (*not shown here*) the solid assumes a face-centered-cubic (*fcc*) structure.



PHASE DIAGRAM FOR HELIUM 3 is essentially the same in the solid region as that for helium 4, with the exception that the temperature-pressure range over which helium 3 exists in the body-centered-cubic (*bcc*) phase is considerably greater than the corresponding range for helium 4. Unlike liquid helium 4, liquid helium 3 does not become a superfluid.



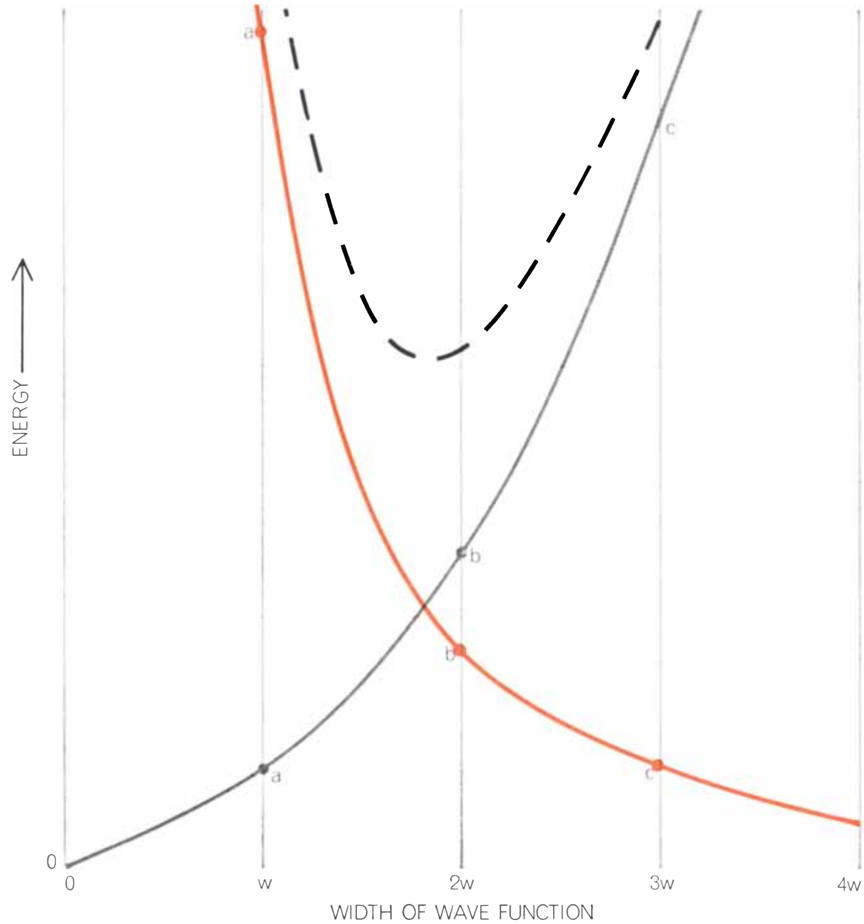
ZERO-POINT MOTION of an atom in a crystal lattice is defined in quantum mechanics in terms of the atom's "wave function": a mathematical expression of the probability of finding the atom at a given point in space. The wave function is large in a region of space where there is a high probability of finding the atom and small where there is a low probability. In the diagrams at left two atoms are shown in imaginary boxes that have a depth corresponding to the atom's potential energy and a width corresponding to the distance by which the atom is separated from its neighbors. For each atom a choice of three wave functions is represented by the curves at right. The kinetic energy, or energy of motion, of the atoms is inversely proportional to the width of the wave-function curves. In both cases the choice of the middle wave function is optimum; that is, it corresponds to the least total energy (potential energy plus kinetic energy) of the atom. The zero-point energy of the atom is the kinetic energy when it is at this lowest total energy.



POTENTIAL ENERGY of a particle in a crystal lattice is represented by a parabolic potential-energy curve (*black*) in which the particle is at rest at the bottom of the potential "well." The potential energy of the particle is directly proportional to the square of its distance from the center of the well (in classical terms) or to the square of the width of its wave function (in quantum-mechanical terms). Therefore the potential energy of the particle can be made arbitrarily small by reducing the width of the wave function. Reducing the width of the wave function, however, has the effect of increasing the zero-point kinetic energy of the particle, which varies inversely as the square of the width of the wave function. For example, the wave-function curve in *a* (*color*) has the smallest potential energy and the largest kinetic energy of the three curves shown, whereas the reverse is true of the wave-function curve in *c*.

total energy by resting at the bottom of the potential well created by its neighboring atoms. Although the wave function of the argon atom is localized and small, the potential well is deep enough to hold the atom. In other words, the kinetic energy the atom has because of its localization does not exceed the depth of the well. In contrast, a helium atom attempting to rest at the bottom of its neighbors' potential well will be too localized (that is, have too much kinetic energy) to remain bound. As a result the helium atom must spread out in space into regions of higher potential energy. Since helium has the smallest attractive potential of any atom, it has the smallest zero-point kinetic energy and the largest zero-point motion.

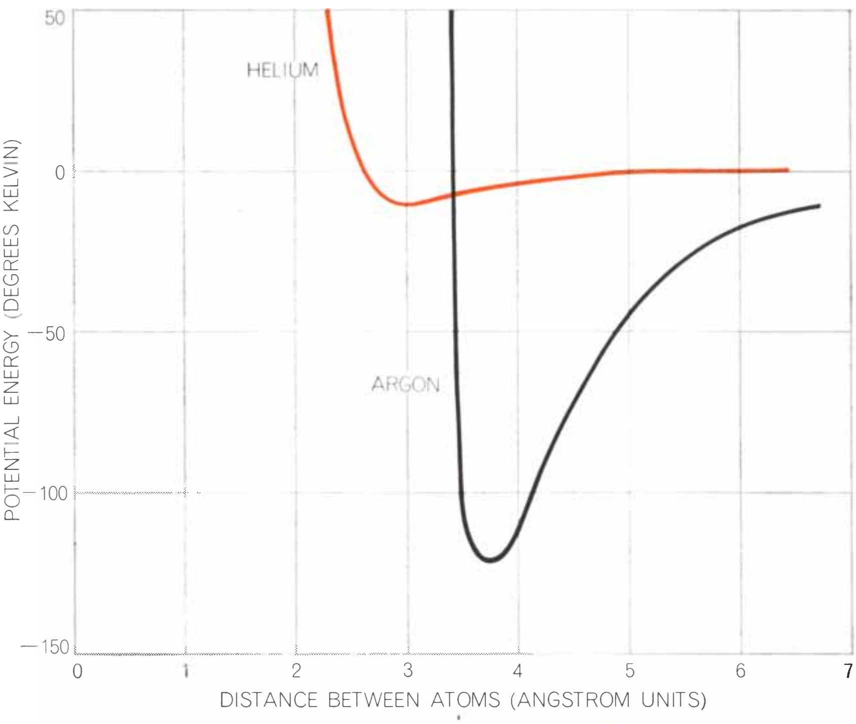
When a theoretical physicist attacks the problem of computing the properties of a solid, the basic ingredients he has to work with are a knowledge of the crystal structure of the sample and of the strength of the force between atoms. Generally the procedure is to fix attention on a single atom and ask how this atom behaves in the presence of the forces exerted on it by its neighbors. As a rule the force a given neighbor exerts can be computed with the neighbor resting at its equilibrium site in the crystal lattice. Solid helium, however, does not follow this rule [*see bottom illustration on next page*]. Because of their large zero-point motion the neighboring helium atoms cannot be regarded as remaining at rest at their equilibrium sites. They are moving in a way that is closely related to the motion of the atom on which attention is fixed. Their positions are highly correlated with the positions of that atom. It is this correlation of the motion of the atoms that complicates the theoretical treatment of solid helium. A great deal of progress in understanding how to handle these problems has been made by L. Nosanow and his colleagues at the University of Minnesota.



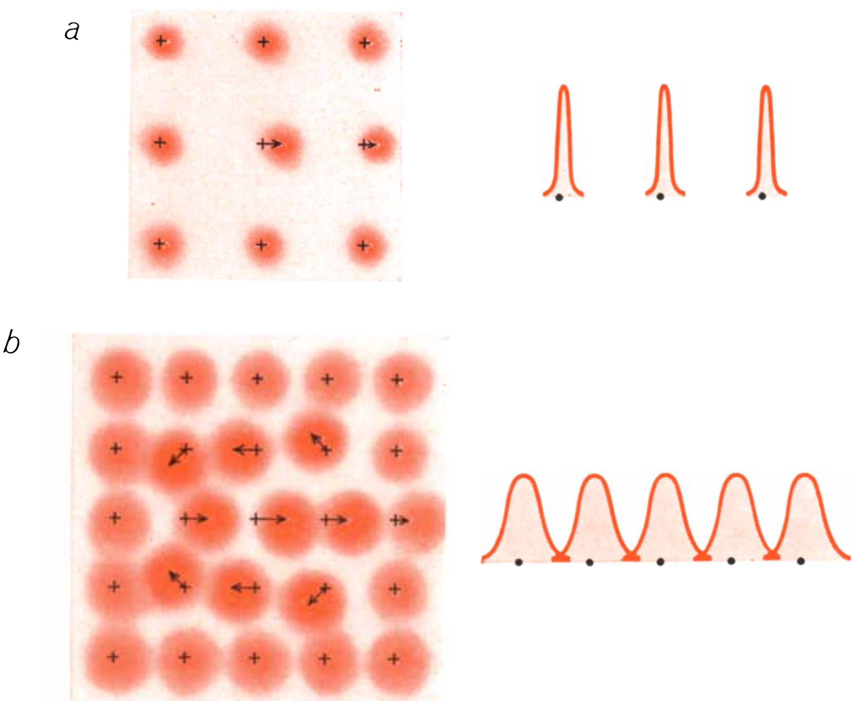
COMPROMISE between two competing factors (*broken black curve*) determines the final width of a particle's wave function. The two factors are the reduction in the particle's zero-point kinetic energy (*colored curve*) that is accomplished by increasing the width of the wave function and the accompanying increase in the particle's potential energy (*gray curve*).

The large zero-point motion of the atoms in solid helium provides an explanation of a number of easily observed properties of the solid. The very fact that pressure is required to form solid helium in the first place is a consequence of its large zero-point motion. Unless the atoms are held together by an externally applied pressure they cannot be brought close enough to be bonded as a solid. In most nonquantum solids the solid will melt when the amplitude of vibration of the atoms is about a tenth of the interatomic spacing. At absolute zero the amplitude of vibration of the atoms in solid helium is already about a third of the interatomic spacing because of the zero-point motion. Finally, in the hexagonal-close-packed phase of low-pressure solid helium the wave functions of the atoms sit quite far out in the attractive part of the potential well, but the wave function is large. The atoms can then wander into the region of their neighbors' "hard cores." Part of the explanation of the existence of a low-pressure body-centered-cubic phase has to do with the effort of the atoms to reduce the number of neighboring hard cores.

Several desirable experimental properties of helium also result from the large zero-point motion. For example, since all other substances are frozen at the boiling point of liquid helium, one can simply freeze out chemical impurities. This fact, coupled with the fact that the two isotopes of helium, helium 4 and helium 3, can be obtained separately, makes it possible to produce chemically and isotopically pure crystals of helium. There are a number of experiments for which such crystals are necessary. As we have mentioned, the small mass and weak interaction of helium atoms lead to a loosely packed solid. The application of moderate pressure can bring about a large change in the thermodynamic properties of the solid. The softness of helium crystals and the large zero-point motion of the atoms make the crystals "self-annealing"; in other words, the large zero-point motions of the atoms lead them to tend to repair mistakes in the structure of the solid. This property makes it possible to grow and maintain large single crystals of solid helium. The recent observation by one of our colleagues, C. C. Ackerman of Duke University, of "temperature waves" in solid helium could not have been accomplished if large chemically and isotopically pure single crystals of helium could not be grown. Since the wave functions of the atoms are large, they may overlap a great deal, making an exchange interaction between neighboring atoms possible. This interaction be-



POTENTIAL-ENERGY CURVE FOR SOLID HELIUM (*color*) is similar to that for argon (*black*), with the difference that the potential well for argon is about 10 times deeper than the one for helium. An argon atom can reduce its total energy by resting at the bottom of the potential well created by the neighboring atoms. In contrast, a helium atom has too much kinetic energy to remain bound at the bottom of the potential well of its neighbors, and as a result it spreads out in space into regions of higher potential energy. The attractive parts of the curves are at the right; the repulsive "hard core" parts are at the left.



CORRELATION of the motion of helium atoms, arising from their large zero-point motion, complicates the theoretical treatment of solid helium. In these diagrams the wave functions of the atoms are represented by intensity of color (*left*) and by the corresponding wave-function curves (*right*). In a normal crystal lattice (*a*) the atoms are well localized at their equilibrium sites; there is little correlation of the motion of neighboring atoms and their wave functions do not overlap. In the solid helium crystal lattice (*b*) the atoms are not well localized; their motion is highly correlated (*arrows*) and their wave functions overlap.

tween the atoms leads to the interesting magnetic properties of solid helium 3.

So far we have been concerned with the ground-state properties of solid helium, or those properties that the solid has even at absolute zero. We have not yet explained, for example, what happens to the solid when energy is added to it. In order to be able to understand the kind of behavior that will result from putting energy into the solid one must have some notion of the kinds of atomic motions that are excited by such an introduction of energy.

For the purpose of considering the modes of motion of the atoms we need a less fine-grained view of the composition of the solid than we have employed so far. The solid can be viewed as a regular array of atoms (each atom with a site at which it is in equilibrium) exerting forces on one another that can best be represented by ordinary springs [*see illustration on opposite page*]. If energy is applied at the end of the crystal, the local atoms experience an increase in vibrational amplitude, which they communicate to the neighboring atoms through the interatomic forces represented by the springs. This is how a traveling wave is excited in the solid. Sound (fluctuations in density) and energy are both transported through the solid by means of these waves.

The energy carried by a wave is proportional to the frequency of the wave. Waves of any wavelength, from very long down to twice the interatomic spacing, can exist in the solid; since the frequency of the waves is inversely proportional to their wavelength, the wave with the shortest wavelength corresponds to the highest frequency. For this wave two neighboring atoms vibrate 180 degrees out of phase. The frequency of this shortest-wavelength vibration is determined by the strength of the spring that represents the force between the two atoms.

The energy and momentum carried by the waves in a solid are quantized; that is to say, they exist only in the discrete energy units called phonons. These quantized waves are analogous to the photons of light. The phonons are the elementary excitations of the dielectric solid. They provide the conceptual link between microscopic and macroscopic properties of the solid.

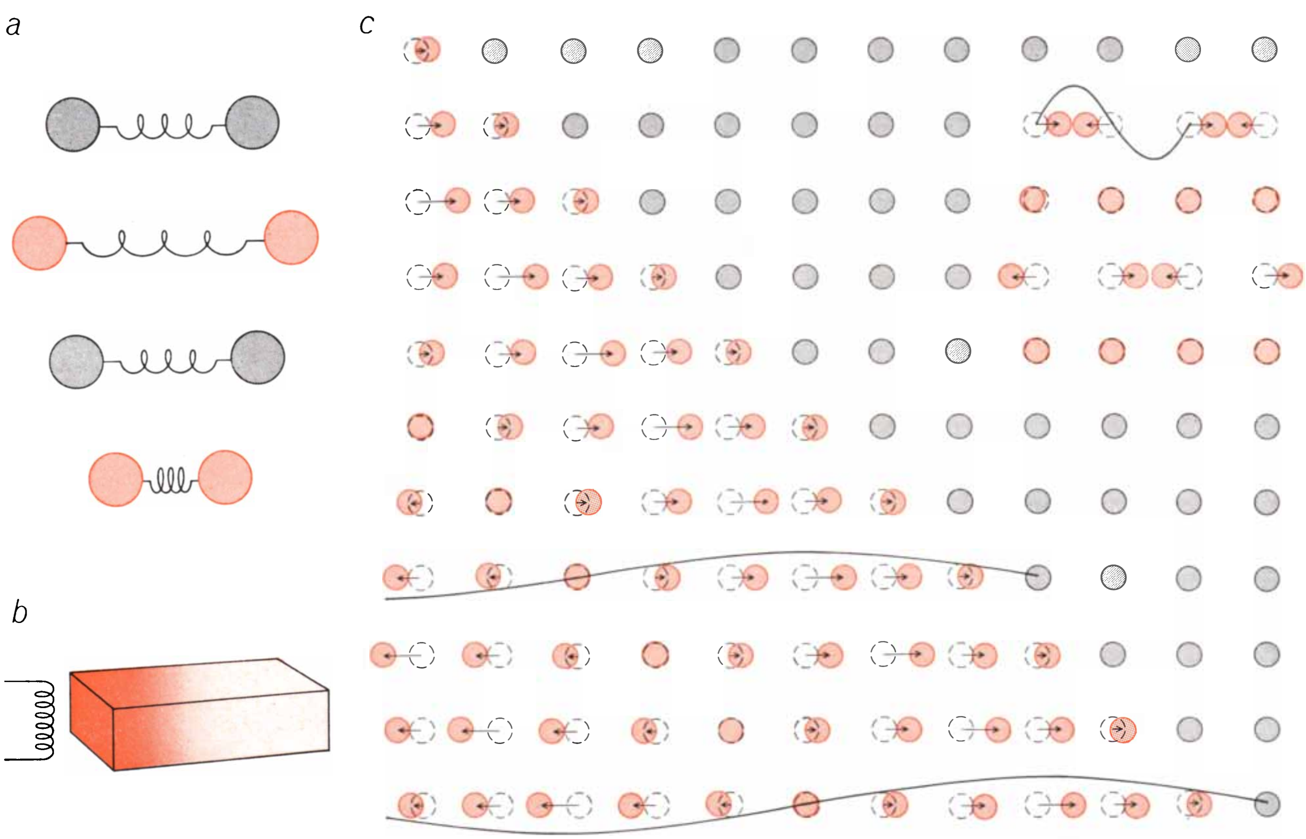
The various experiments done in the study of solid helium are classified according to the properties they measure. Two distinct classes of properties are of interest: (1) thermodynamic properties, which yield information about the characteristics of the elementary excitation spectrum (what phonons can be present in the solid), and (2) transport properties, which yield information about the interactions of the elementary excitations (how the phonons interact with one another).

The most fundamental thermodynamic property that can be measured for a system is its specific heat. The quantity that is measured is the ratio of an amount of energy put into the solid to the attending change in the temperature of the solid. The principle of a specific-heat experiment is shown in the top illustration on the next page. When the solid is at a temperature *T*, the energy of a typical phonon in the solid is proportional to *T* and the number of phonons is proportional to T^3. The total energy in the solid is proportional to T^4. An additional amount of energy is then put into the solid, bringing it to a new temperature $T + T'$. The ratio of the added energy to the attending temperature change T' (in other words, the specific heat of the solid) is expected to be proportional to T^3; the proportionality constant is a measure of the stiffness of the springs, that is, of the strength of interaction between the atoms of the solid. This expectation is a consequence of the conventional phonon view of the elementary excitations in the solid. One of the fundamental assumptions on which this view rests is that the atoms in the solid undergo only small displacements from their equilibrium sites. This assumption is certainly violated in the case of crystals of solid helium.

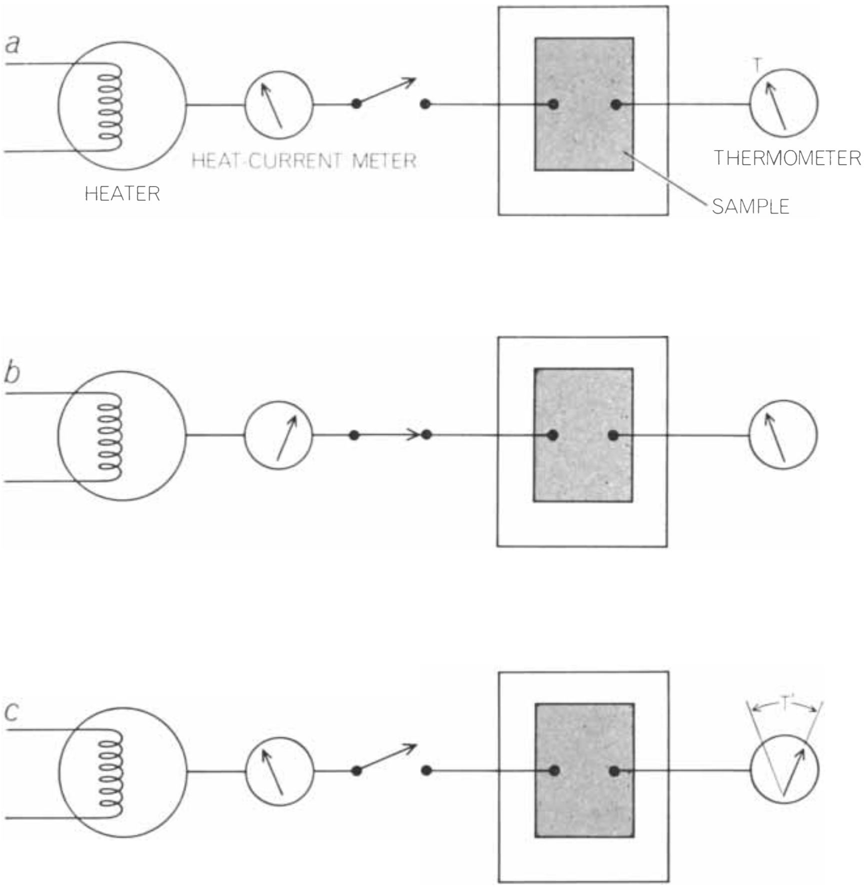
Let us now turn our attention to the measured specific heat of solid helium and see if our expectations are borne out. The specific heat of solid helium has been measured by many workers over almost the entire temperature range of its existence, and at pressures up to 3,000 atmospheres. The graph at the bottom of the next page is a plot of the specific heat v. temperature-cubed for a typical sample of solid helium. The straight line would be observed if strict proportionality to T^3 were correct. At the lowest temperatures the expected temperature-cubed behavior is observed and a deviation starts just above 1 degree K. This is precisely the behavior displayed by the specific heat of most other solids, including the heavier inert gases whose properties are not strongly influenced by zero-point motion.

As we mentioned earlier, solid helium is particularly useful for investigating the effects of changes in interatomic spacing. As the pressure on the solid is increased the springs between the atoms should become stiffer, a given input of energy should result in higher-frequency vibrations (that is, higher temperatures) and the specific heat should become smaller. This expectation is in good agreement with the observed behavior of solid helium. Thus it is possible to understand the dependence of both temperature and pressure on the specific heat of solid helium in terms of classical theory. Even though the zero-point motion might play a role in determining the quantitative details, the general behavior of this property of solid helium is quite like that of other solids.

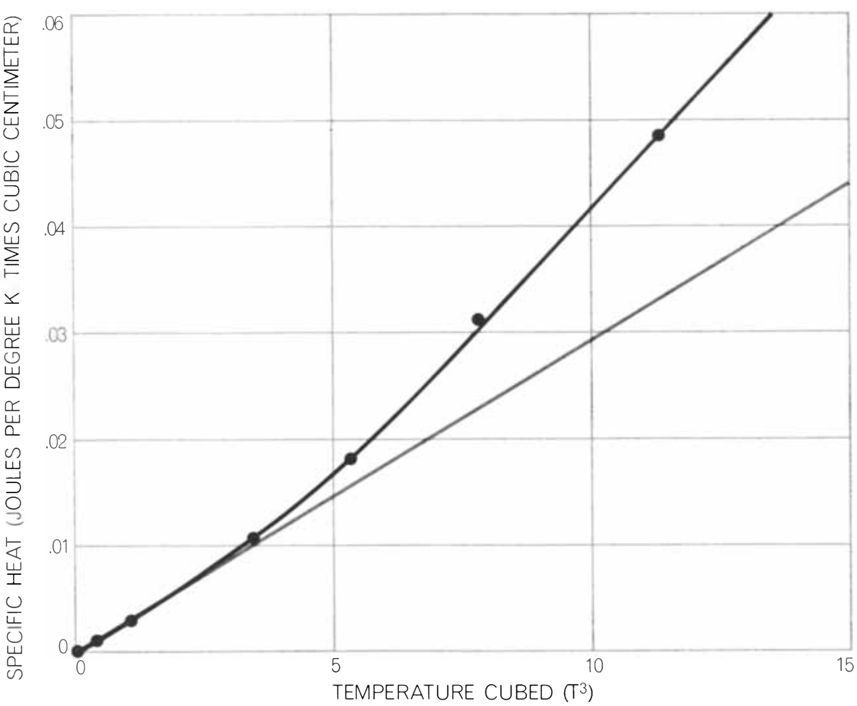
There are other kinds of thermodynamic measurements that can be made on solids to gain information on the spectrum of elementary excitations. Usually the quantities measured are less directly related to the elementary excitation spectrum than the specific heat is;



ATOMS IN A CRYSTAL LATTICE exert forces on one another that can be best represented by ordinary springs (*a*). When energy is introduced at one end of a solid, say by a heater (*b*), the first rank of atoms in the crystal lattice (*c*) experience an increase in vibrational amplitude, which they communicate to the neighboring atoms through the atomic forces represented by the springs. This creates a traveling wave along the rows of atoms. In the example at the lower left the traveling wave that is excited in the solid has a wavelength 12 times the interatomic spacing. The shortest possible wavelength is two times the interatomic spacing (*upper right*).



SPECIFIC HEAT IS MEASURED by means of an experiment of the general type outlined here. The sample is first isolated and brought to equilibrium at a temperature T (a). Then an additional amount of heat is put into the sample (b). The sample is again isolated and allowed to come to equilibrium at a new temperature $T + T'$ (c). The specific heat is defined as the ratio of the additional heat to the attending change in the temperature (T').



SPECIFIC HEAT OF SOLID HELIUM departs from a strict proportionality to the cube of the temperature (T^3) at temperatures above about 1 degree K. This behavior, which has to do with the stiffness of the interatomic "springs," resembles that of most classical solids.

thermodynamic theory, however, provides the necessary link to allow the extraction of such information from the data. Generally one measures the dependence of volume, pressure and temperature on one another. Extensive measurements of many of these properties have been carried out in several laboratories.

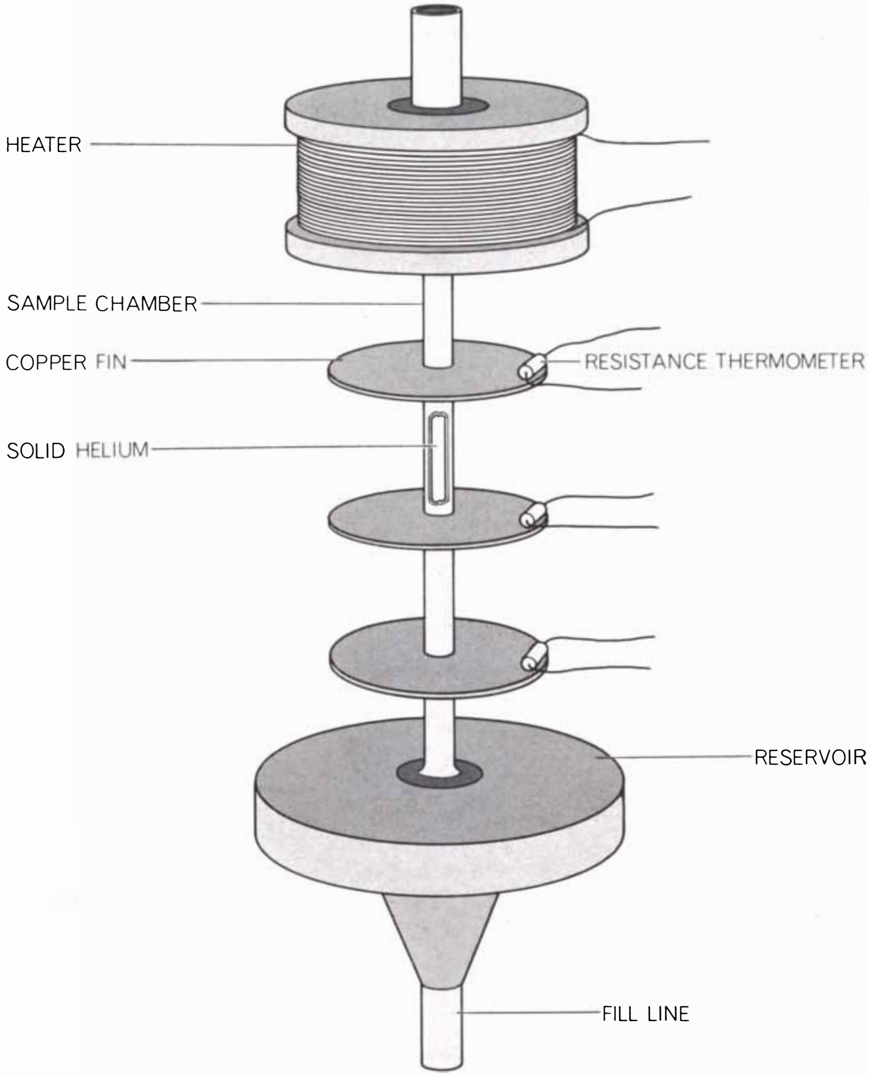
The primary example of a transport property is thermal conductivity. The thermal conductivity of a substance is measured by the experimental arrangement shown in the top illustration on the opposite page. One end of a crystal is in contact with a temperature reservoir at temperature T. A flux of energy is applied at the other end, usually with an electric heater. The temperature at the heater end of the crystal rises to $T + T'$, and the applied energy flows toward the reservoir at T. The thermal conductivity is defined as the ratio of the energy flux to the temperature difference per unit length along the sample. A high value of thermal conductivity means that the crystal conducts heat very well. Phonons are the manifestation of heat energy in the crystal; the conduction of energy from $T + T'$ to T is due to the transport of phonons along the sample. The thermal conductivity is a measure of the phonon flow. If the phonons could flow along the crystal unimpeded, the thermal conductivity would be infinite. No temperature difference would be needed to drive the phonons along the crystal, and the crystal could be considered a thermal superconductor. In practice phonons undergo many collisions, or interactions, among themselves, with impurities and with boundaries as they attempt to transport energy along the crystal. The thermal conductivity is proportional to three factors: the mean free path of the phonons (that is, the average distance traveled by a phonon between collisions), the specific heat (a measure of the number of phonons available to carry the heat energy) and the velocity of sound.

There are many types of phonon collision. In general the mean free path for any type depends on temperature. An understanding of thermal conductivity depends on an understanding of the mean free paths for the various phonon collision mechanisms. A fundamental type of phonon interaction is the three-phonon process. Here two phonons collide to form one phonon, or one breaks into two [*see bottom illustration on opposite page*]. This kind of collision process is present in all solids; its probability is related to the displacement of the atoms from their equilibrium sites. These displacements are particularly large in helium crystals, so that the three-phonon process should be very important in determining the thermal conductivity of solid helium. Energy is conserved in three-phonon processes; the energy of the resulting phonon is equal to the sum of the energies of the two initial ones. The direction of energy flow, however, can be the same as, or the reverse of, the original direction. Three-phonon processes that result in no change in the direction of energy flow and do not directly impede the conduction of energy are called normal processes. Three-phonon processes in which the direction of energy flow is reversed are termed "umklapp" processes (from the German word meaning literally "flopover"). Such processes impede the conduction of energy and thus reduce the thermal conductivity.

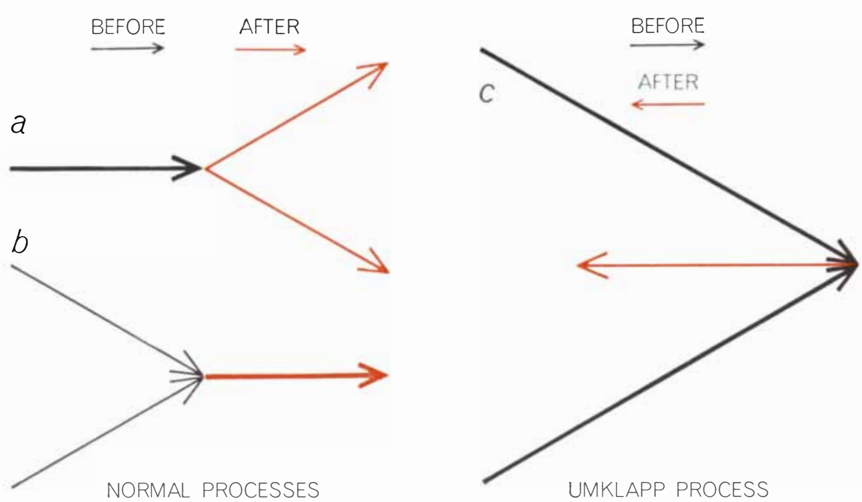
We can now compare the measured temperature dependence of the thermal conductivity in solid helium to the behavior expected from our ideas on phonon interactions. Since helium can be obtained that is very pure both chemically and isotopically, and mechanically good single crystals can be made, the first experiment we consider is the measurement of thermal conductivity in a nearly perfect crystal. In this case umklapp processes are responsible for the reduction of the thermal conductivity. In order for an umklapp process to occur, one of the interacting phonons must have a high energy. The number of high-energy phonons decreases strongly with decreasing temperature. Hence an umklapp process becomes very improbable and the umklapp mean free path becomes very long. So we expect the thermal conductivity, which is proportional to the mean free path, to increase strongly with decreasing temperature.

At a sufficiently low temperature the umklapp mean free path becomes so long that it is comparable to the size of the crystal. In this case resistance to thermal conduction is due to collisions of the phonons with the crystal boundaries; the mean free path is constant at about the diameter of the crystal. We then expect the thermal conductivity to depend on the cube of the temperature, just as the specific heat does.

Starting at the highest temperature, as the temperature decreases we expect the thermal conductivity to increase, reach a peak and then decrease. The upper curve in the illustration on the next page is a plot of the measured thermal conductivity against temperature for a sample of pure helium 4. In the region above 1 degree K. the sharp increase characteristic of umklapp scatter-



THERMAL CONDUCTIVITY of solid helium is measured with the experimental device shown here. Heat energy is introduced at the top of the stainless-steel sample chamber by means of a heater. Measurements of the temperature difference between two of the copper fins and of the heat flow along the sample enable one to calculate the thermal conductivity.



ENERGY AND MOMENTUM ARE CARRIED in a solid by quantized waves called phonons (*large arrows*), which are analogous to the photons of light. In a normal three-phonon process one phonon can break into two (*a*) or two can collide to form one (*b*). Normal three-phonon processes result in no change in the direction of energy flow (*small arrows*). A three-phonon process that reverses the energy flow is called an "umklapp" process (*c*).

ing is observed, and a peak occurs at about .8 degree K., below which the decrease is proportional to the temperature cubed. Hence the observed thermal conductivity of pure solid helium can be understood in terms of the ordinary theory of phonon interactions.

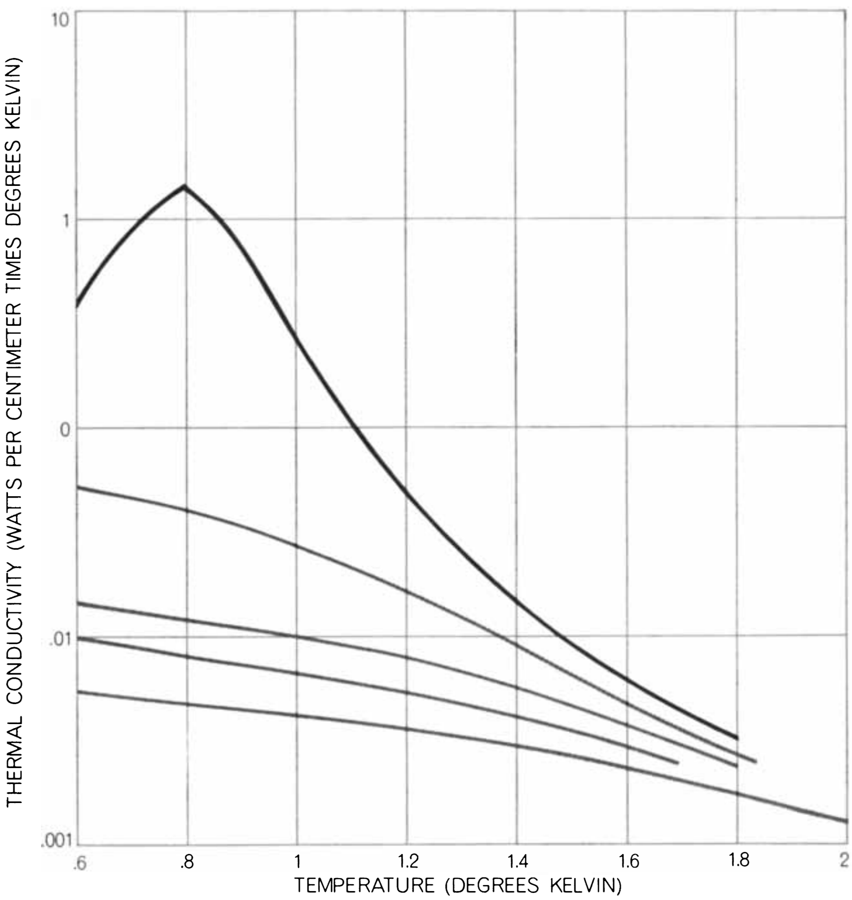
The effect of collisions of phonons with point defects, or local imperfections, in the crystal lattice can be studied by adding known concentrations of helium 3 to helium 4 or vice versa. In the case of a small concentration of helium-3 atoms in a lattice of helium 4, the phonons see a different mass at the site of each helium-3 atom and are scattered by these point defects. The mean free path for such collisions is much shorter than the umklapp mean free path, and the thermal conductivity is drastically reduced. The lower curves in the same illustration are the measured thermal conductivities of four isotopic mixtures. The observed thermal conductivity is much lower than would be expected if mass-difference collisions alone were important. It is believed that the large zero-point motion of a helium-3 atom in a helium-4 lattice causes a distortion of the lattice around the helium-3 site. This lattice distortion increases the probability of a phonon collision at the helium-3 site and further reduces the mean free path. Thus ordinary phonon theory qualitatively explains the effect of isotopic impurities in solid helium, although the zero-point motion is significant in determining the magnitude of the effect.

From the foregoing discussion of some of the properties of solid helium it is evident that much of its behavior is affected quantitatively but not qualitatively by its large zero-point motion, and can therefore be interpreted in terms of the ideas that have been used in treating nonquantum solids. We now consider an area in which the large zero-point motion plays the dominant role: the nuclear magnetic properties of solid helium 3.

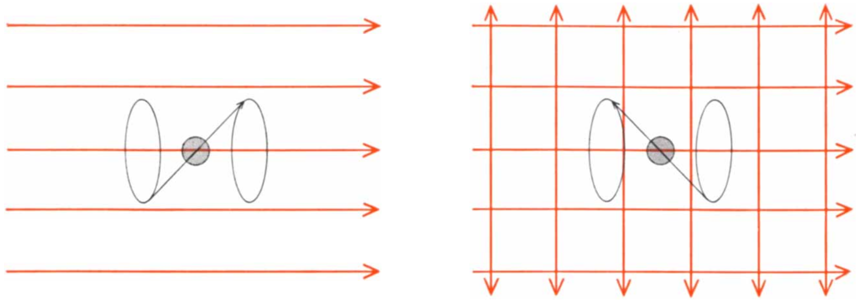
The fundamental particles of which helium atoms are made—electrons, protons and neutrons—all have a magnetic moment; that is, one can think of each particle as containing a tiny bar magnet. In addition, a magnetic moment is generated by an electron circulating in an orbit. In general the magnetic properties of solids arise from the electronic moments, which are some 1,800 times stronger than those of protons or neutrons. In helium, however, there are two electrons forming a closed shell, so that the electronic moments are in opposite directions and cancel each other, resulting in a total absence of electronic magnetism. The helium-4 nucleus is composed of two protons and two neutrons. The magnetic moments in this case cancel in pairs, resulting in no net nuclear magnetic moment, so that the helium-4 atom is completely nonmagnetic. This is not the case in helium 3, where the nucleus has only one neutron and two protons, giving rise to a net magnetic moment. This allows the investigation of nuclear magnetism in a system with large zero-point motion. In this situation certain quantum-mechanical effects play a major role, and they can be observed experimentally.

When a sample of a magnetic solid is put in a magnetic field, the sample will itself acquire a certain magnetization. This magnetization, being itself a magnetic field, is a vector quantity; a direction as well as a magnitude is necessary to specify it. The magnetization of a solid can be in such a direction that it adds to the applied field (paramagnetism), or it can oppose the applied field (diamagnetism). Materials vary in their response to a given field; the same field applied to different materials will in general result in different magnetizations. We can thus define a property of a given substance, magnetic susceptibility, as the magnetization per unit of applied field. A large susceptibility means that a given field causes a large magnetization and vice versa. The susceptibility can be thought of as a measure of the ease or difficulty of magnetizing a substance and, as we shall see, it results from the interactions of the elementary magnetic moments.

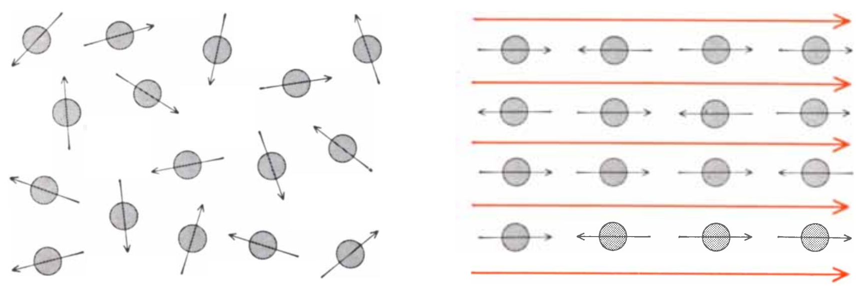
Since nuclear magnetic moments are very weak, measurement is accomplished with the technique of nuclear magnetic resonance. When a magnetic field is applied to the sample, the individual nuclear moments will precess, or wobble, around the field direction at a frequency proportional to the strength of the field.



THERMAL CONDUCTIVITY OF HELIUM 4 is depressed by the addition of small concentrations of helium 3 to the helium 4. The helium-3 atoms act as point defects, or local imperfections, in the helium-4 crystal lattice. Black curve shows the variation of thermal conductivity with temperature for a sample of pure solid helium 4. Gray curves show the measured thermal conductivity for four isotopic mixtures of helium 4 and helium 3; the lower the curve, the higher the concentration of helium 3. Ordinary phonon theory qualitatively explains the effect of isotopic impurities in helium crystals, although the large zero-point motion of the helium atoms is significant in determining the magnitude of the effect.



NUCLEAR MAGNETIC MOMENT PRECESSES, or wobbles, in a steady magnetic field at a frequency that is proportional to the strength of the field (*left*). When an alternating field whose frequency is equal to the precession frequency is applied at right angles to the steady field, it can cause the moment to flip (*right*). This phenomenon is the basis of the technique of nuclear magnetic resonance, which measures the magnetic moments of nuclear particles.



MAGNETIC MOMENTS of particles in a crystal lattice are normally oriented at random (*left*), so that the individual moments cancel on the average and the sample has no net magnetization. When an external magnetic field is applied to the crystal (*right*), the moments tend to align parallel to the field, causing a net magnetization in the direction of the field.

If one applies a small alternating field whose frequency is equal to the precession frequency and whose direction is perpendicular to the original field, it can cause some of these moments to flip over [*see upper illustration on this page*]. The number of moments that flip depends on the number originally aligned with the steady field, so that total energy absorbed is a measure of the sample's magnetization (or, since the applied field is known, the susceptibility).

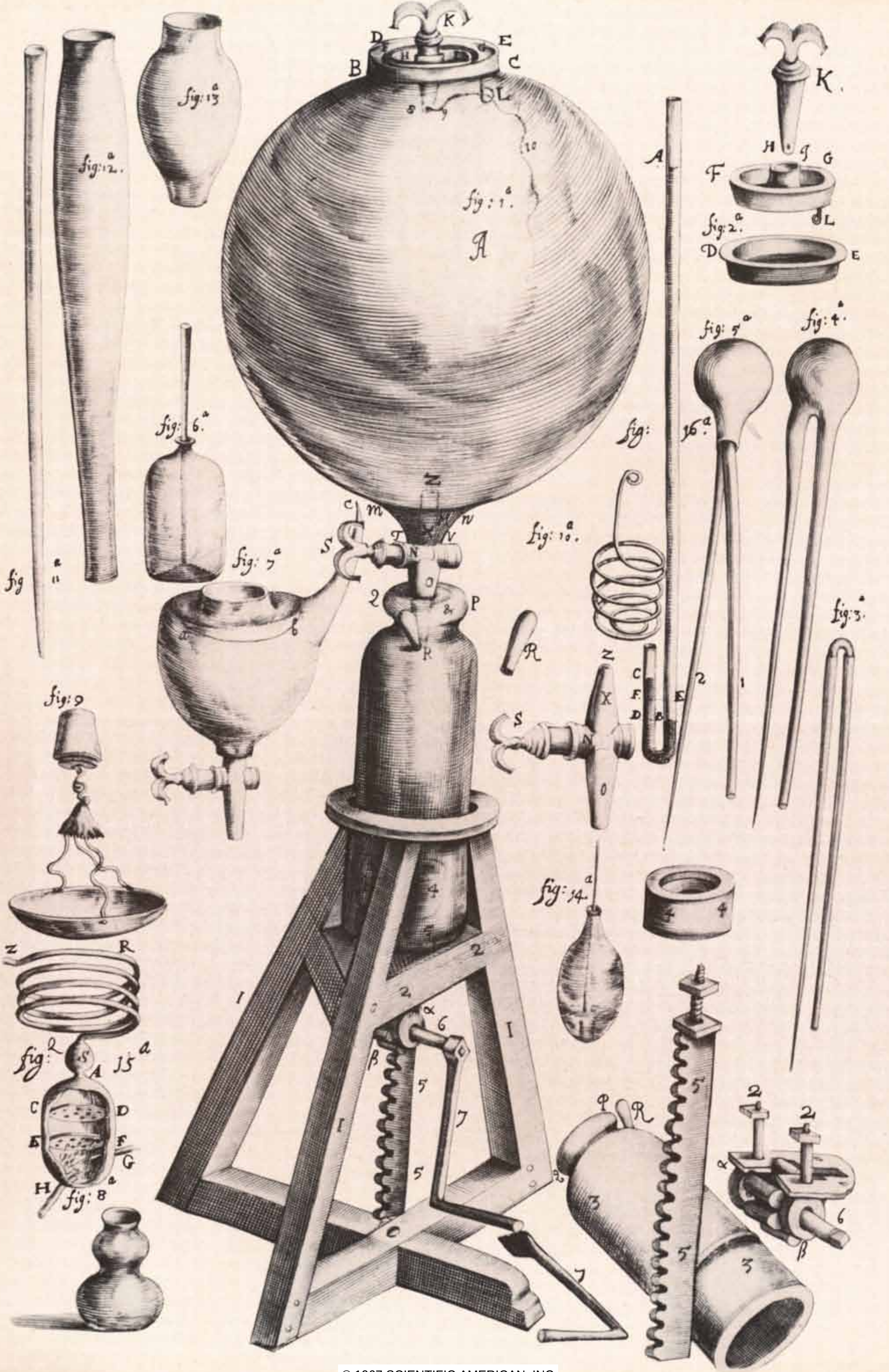
In order to understand the significance of the information derived from these measurements, consider a lattice of elementary magnetic moments [*see lower illustration on this page*]. Thermal motion keeps the magnetic moments randomly oriented, so that the individual moments cancel on the average and the sample has no net magnetic moment. Now suppose an external field is applied There is a tendency for the moments to align parallel to the field, causing a net magnetization in the field direction. Opposing this alignment is the random thermal motion of the atoms. Thus one has a situation in which two opposing forces are acting: the field tending to orient the moments and the thermal motion tending to randomize them. The thermal energy is proportional to temperature, and the magnetic energy to the strength of the field. Calculation of the effects of these two energies leads to a magnetization that is directly proportional to field and inversely proportional to temperature. The susceptibility is obtained by dividing the magnetization by the field magnitude and is inversely proportional to temperature. This result is called Curie's law, after Pierre Curie; it is valid in general for any system of free, noninteracting magnetic moments.

Now suppose the elementary moments in the lattice interact with one another. The particles are not free in this case and Curie's law does not hold. Since the thermal energy depends on temperature, however, and the interaction energy does not, it is possible to conduct the experiment at temperatures high enough for the thermal energy to be large compared with the interaction energy. Curie's law is then observed. This is the basis of our investigation of solid helium 3. Starting in the region governed by Curie's law the susceptibility is measured as temperature is decreased. The temperature at which deviation from Curie's law is observed is a measure of the interaction energy. A particular type of interaction is characterized by a particular energy, so that this information can lead to an understanding of the mechanism of magnetic interaction in the crystal lattice.

The large zero-point motion in solid helium leads us to expect a strong interaction—the exchange interaction—whose origin is quantum-mechanical. As we have mentioned, the probability of finding a particle at a given point in space is given mathematically by the value of its wave function. In the quantum-mechanical description of an ordinary lattice (that is, one having small zero-point motion) no two wave functions are nonzero at the same point in space. The situation is quite different in solid helium, where the wave functions actually overlap, leading to an interaction between moments of quantum-mechanical origin—the exchange interaction. The energy of the exchange interaction is equivalent to a temperature of about a thousandth of a degree.

We have shown that the large zero-point motion in solid helium leads to the prediction of an exchange interaction between the elementary nuclear moments. In order to investigate this experimentally the nuclear susceptibility is measured; the temperature of its deviation from Curie's law gives an idea of the interaction energy, which can then be compared with the theoretically predicted exchange energy. Horst Meyer and his colleagues at Duke University have made such measurements and have found an interaction energy of about a thousandth of a degree. Meyer's group and a group at the University of Oxford have also measured the value of the exchange energy indirectly by nuclear resonance and have obtained comparable results. Since the interaction energy of classically behaving nuclear moments would be about a millionth of a degree, a thousand times smaller than the observed energy, the data confirm the predictions that the large zero-point motion leads to an exchange interaction. Accordingly the quantum nature of the magnetic properties of solid helium 3 has been established.

Although quantum-mechanical effects are large in solid helium, many of its properties are much the same as those of nonquantum solids. This surprising result is one of the mysteries that makes the study of helium fascinating. The effect of the large zero-point motion is not to change the qualitative behavior of the solid but to make the connection between its basic characteristics and the observed properties much more complicated. The complexity of the quantum effects leaves the primary burden on the theorist, who must explain why solid helium is not more perverse in its behavior than it actually is.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ROBERT BOYLE

He stands out among 17th-century natural philosophers as a careful observer and investigator. Particularly in pneumatics and chemistry, his experiments laid a firm groundwork for much that was to follow

by Marie Boas Hall

When Robert Boyle died in 1691, Christian Huygens and Gottfried Wilhelm von Leibniz commiserated that he had wasted his talents trying to prove by experiments what they knew to be true in the light of reason—that he was more interested in observation than in reasoning and had left no unified body of thought. These great contemporaries of Boyle's misunderstood his purpose and aim. With his discoveries in pneumatics, his investigations in optics and physiology and particularly his shaping of the emerging science of chemistry, he had established himself as the leading proponent in England of the new "experimental philosophy." The Cartesian philosophers on the Continent could not perceive that his reputation was the more secure because his discoveries and his theoretical concepts, being grounded in experiment, would provide a firm basis for further work. Unlike the purely rational systems built by so many of his predecessors and contemporaries, his contribution was not to be torn down and superseded.

Robert Boyle was born into an outwardly secure world in 1627, the son of an Elizabethan adventurer who had carved for himself a large property in Ireland. As a boy he was sent to be educated on the Continent, where he traveled for several years learning languages, a European outlook and a little about the new trends and problems of science. When he returned to England in 1644 at the age of 17 his father was dead and his older brothers were fighting rebellion in Ireland. During the English Civil War his family was divided in allegiance, some members remaining loyal to the king and some supporting the Cromwellians—in both cases temperately, so that none of them lost his life or his wealth.

BOYLE'S AIR PUMP consisted of a 30-quart glass "receiver" (*A*) connected by a stopcock (*N*) to a 14-inch-long brass pumping cylinder through which a padded piston (*4*) was drawn by a toothed shaft (*5*). First the stopcock was closed and the piston was cranked down. Then, with the stopcock opened, Boyle wrote, "part of the Air formerly contain'd in the Receiver, will nimbly descend into the Cylinder." Then the stopcock was closed, the brass plug (*R*) removed and the piston raised, expelling air from the cylinder. As the procedure was repeated, the air in the receiver was attenuated. Other objects shown are accessories for experiments.

Boyle's own preference was to avoid public affairs. Guided by his brother Roger and his elder sister Lady Ranelagh, who was a friend of John Milton's and other Puritan intellectuals, he tried his hand at literature and then turned to agriculture and medicine. Medicine led him to take up chemistry; he found it an enthralling pastime and soon became proficient in it. He was not, however, to become the conventional 17th-century chemist. Although he read widely in chemical and medical works, he was saved from the mysticism of contemporary chemists by his earlier acquaintance with contemporary physics. He had read Francis Bacon and Galileo as a boy. Descartes's *Principles of Philosophy*, published in 1644, influenced his later thought far more deeply than the cloudy views of alchemists did. The fascination of chemical experiment did not keep him from studying the novel ideas in physics and astronomy that were then characterized as the "new learning"; he thought as much about the physical nature of air and Evangelista Torricelli's recent experiments with a column of mercury as he did about the best ways of compounding drugs, and he was as familiar with Copernican astronomy as he was with conflicting interpretations of alchemical doctrine.

By 1653 Boyle was beginning to know some of the most original and active English scientists and to find his aims were theirs. Since 1645 there had been weekly meetings in London, often at Gresham College, of physicians, natural philosophers and mathematicians to discuss the "new learning" they attributed to Bacon and Galileo. The subject matter was broad: the circulation of the blood and the Copernican hypothesis; comets, Jupiter's satellites and the valves in the veins; the nature of air, the appearance of the moon, the improvement of telescopes and the possibility of creating a vacuum; sunspots and novas and the fall of heavy bodies.

By 1650 several members of this group had moved to Oxford, where they continued to meet and where Boyle was invited to join them. He did so in 1654, settling in Oxford for 14 years. The London meetings continued, and after the Restoration they were attended by Royalists who returned to the capital and by many of the Oxford group. It was after a Gresham College lecture on November 28, 1660, that 12 men, including Boyle, decided to establish a "college" to promote "Physico-Mathematicall, Experimentall Learning." That was the beginning of the Royal Society, chartered in 1662 as the first scientific society in Britain and one of the first in the world. Boyle was to become one of its most active members and proudest ornaments.

The scientific circle he joined at Oxford in 1654 was a brilliant one from which he had much to learn. It included England's leading mathematician, John Wallis; Cromwell's brother-in-law John Wilkins, an early adherent of Galileo and a great exponent of Copernicanism; Seth Ward, an early follower of Kepler who had his own original theories in astronomy; distinguished physicians, such as Jonathan Goddard, who were in-

PORTRAIT OF BOYLE was engraved from a drawing made by William Faithorne about 1664. Boyle, then living in Oxford, was already noted for his air pump (*right background*).

terested in the new chemical remedies; bright young undergraduates and beginners such as Christopher Wren and Robert Hooke, the latter to become Boyle's assistant and later a distinguished natural philosopher in his own right.

Boyle was accepted as being fully competent to share the group's interests, whether mathematical, physical or biological. Curiously he suffered some ridicule for his devotion to chemistry. Whereas the study of chemistry seemed reasonable enough in relation to medicine, it was less understandable in someone, as Boyle himself put it, "who professed to cure no disease but that of ignorance."

Even in the 1650's Boyle was deeply interested in chemistry as the key to the nature of matter. He had begun to compile material for a dozen or more books on the "mechanical philosophy," which was coming to be the most commonly accepted theory of matter. It rejected the long-established Aristotelian theory of matter in favor of one based on new principles of mechanics established by Galileo and Descartes. Descartes in particular had shown in his *Principles of Philosophy* how, by defining matter and establishing the natural laws of motion, one could construct an entire system of the world without recourse to the vague world of Aristotelian "forms" and "qualities." Heat, for example, instead of being a "form" inherent in substance (and so not further capable of definition) could be regarded as arising from the size, shape and motion of the particles of matter.

The English scientists were much influenced by Descartes's careful formulation of his mechanical philosophy, toward which they were further predisposed by their adherence to similar ideas of Bacon's. Some preferred Pierre Gassendi's modernized and Christianized version of Greek atomic thought as it had been developed by Epicurus; Gassendi's work had appeared several years after Descartes's but it had the appeal of familiarity because the writings of Epicurus were well known (Boyle, for instance, had read them as a boy).

By the middle 1650's Boyle had worked out his own version of the mechanical philosophy—the "corpuscular philosophy," he called it—in which he drew on both the Cartesian and the atomic views but wholly accepted neither. He believed "those two grand and most Catholic principles, matter and motion," sufficed to explain all the properties of matter as we experience it. He rejected the Epicurean idea that the specific shapes of atoms determined the nature of substances and also Descartes's concept of an undetectable "aether"; he thought Descartes was wrong in rejecting the possibility of the vacuum and proved the Epicureans were wrong in insisting that there were "frigorific atoms," special corpuscles that produced freezing. Above all, he believed it should be possible to demonstrate the truth of the mechanical philosophy by experiment—particularly by chemical experiments, in which, he held, the scientist was manifestly dealing with small, discrete particles of matter that could survive chemical transmutations and so must have a real and definite existence.

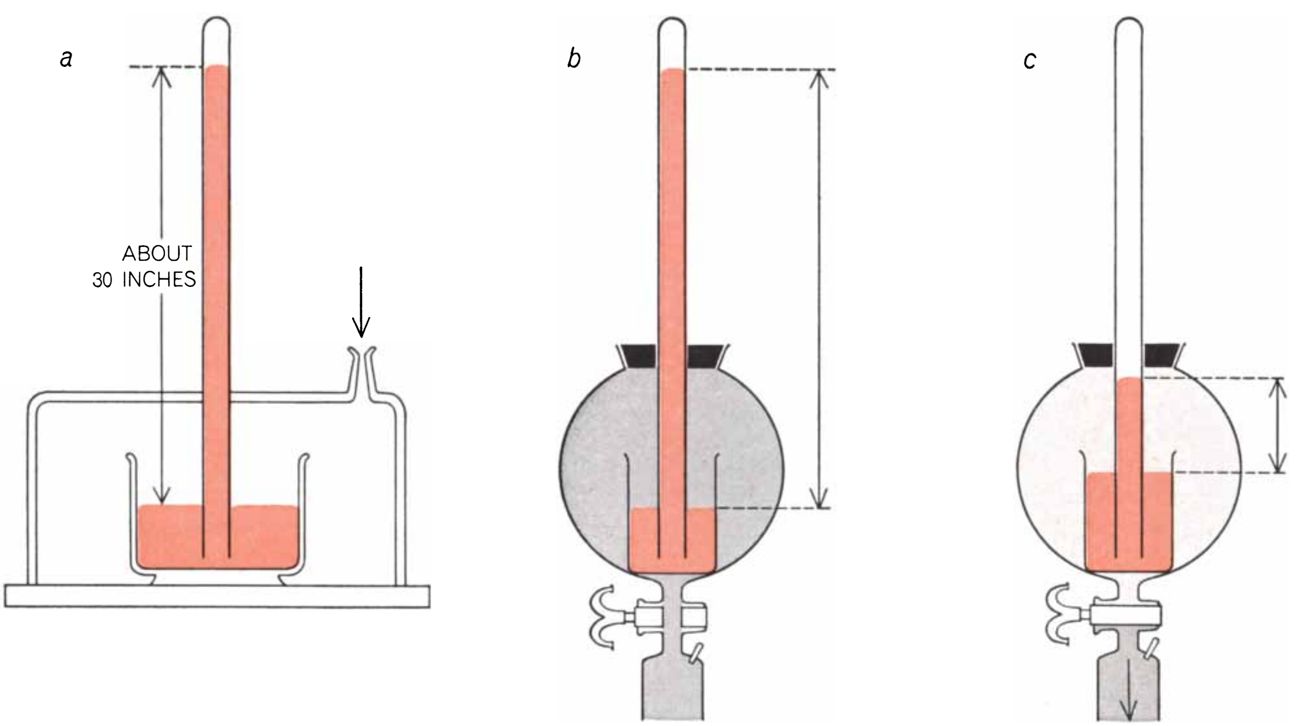
The notion that one could *prove* a scientific theory by experiment was itself novel, and it was by no means universally acceptable to 17th-century thinkers. Boyle (and Isaac Newton after him) saw clearly just how a theory might stand or fall in the light of experimental evidence. Many others—Huygens and Hooke among them—followed Descartes in the belief that the ultimate test of a theory was the appeal to reason. In 1662 and 1663 a notable exchange of views between Boyle and the leading contemporary rationalist philosopher, Benedict Spinoza, was conducted in a correspondence between Spinoza and Henry Oldenburg, secretary of the Royal Society and Boyle's literary assistant. Spinoza could not understand how Boyle could subordinate reason to experiment, or indeed how a chemical combination of particles could behave differently from a physical mixture. The correspondence provides a fascinating example of how one of the most powerful intellects of his day totally failed to grasp concepts that now appear to be obvious. It is also evidence for the originality of Boyle's approach to scientific proof—and to chemistry.

Boyle's major contributions in chemistry were not to be published for some time because in 1657 his attention was turned from chemical to physical experiment by news of an exciting new instrument: the air pump invented by Otto von Guericke, who had utilized an ordinary suction pump to empty first a wine barrel filled with water and then a metal receptacle containing only air. Boyle first learned of the pump from a treatise on hydraulics and mechanics by a German Jesuit, Gaspar Schott, who described von Guericke's work only to refute it; Schott, a good Aristotelian like all 17th-century Jesuits, refused to contemplate the existence of a vacuum.

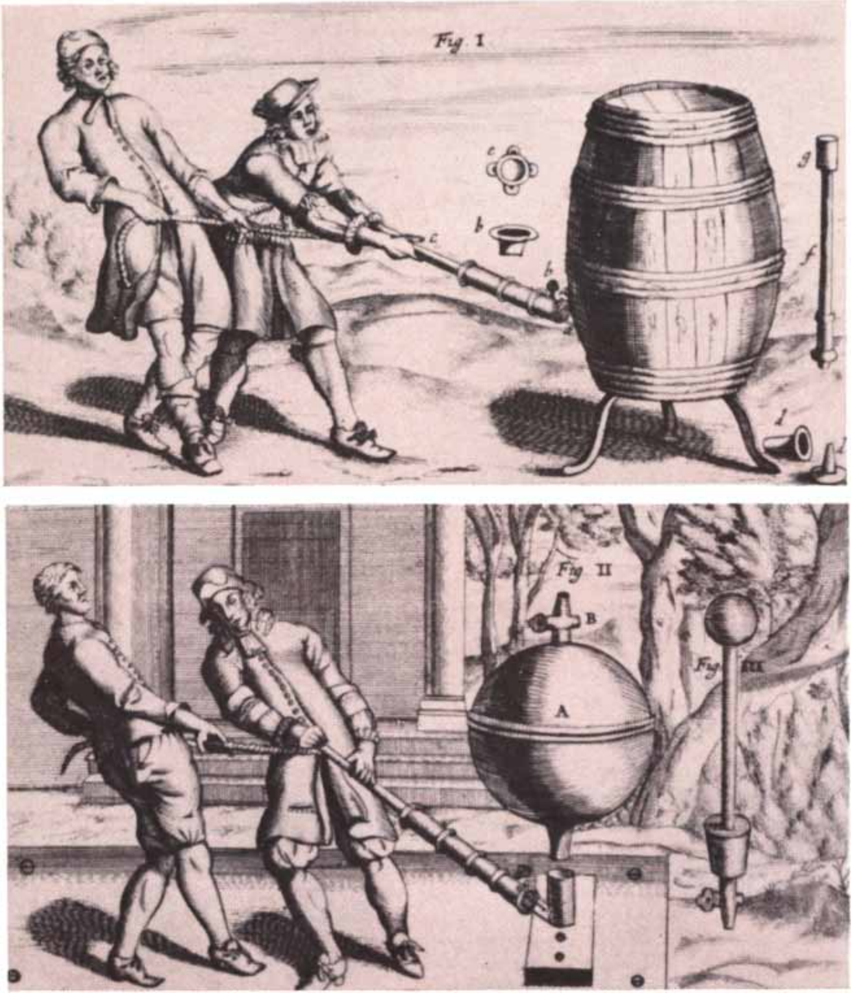
Boyle's imagination was caught by the account; he recognized the potential uses of an air pump more manageable than von Guericke's, the operation of which required the prolonged efforts of two strong men. He set his assistants the problem of constructing a laboratory version, and the result was the first deliberately designed air pump. Designed by Hooke, it was easily worked by one man and was moderately airtight. Its receiver, seven to eight gallons in volume, was made of glass and fitted so that objects could be readily put into it before pumping and then could be manipulated in the vacuum.

For more than 30 years natural philosophers had been trying to understand the working of suction pumps and siphons. For 25 years they had been demonstrating the effects of the pressure of the air. Torricelli had performed his experiment in 1644. In it he had taken a tube closed at the bottom, filled it with mercury and then inverted it in a bowl of mercury; when the mercury moved partway down the tube, it was evident that the closed space above it could contain no air. Nearly 10 years earlier in Rome, however, the same experiment had been done with water. After Torricelli had described his experiment, the effect of atmospheric pressure was investigated often, and Pascal's Puy-de-Dôme experiment comparing mercury readings at different altitudes was repeated under varying conditions. French workers, notably the astronomer Adrien Auzout and the anatomist Jean Pecquet, had tried to use the Torricellian vacuum—the space above the column of mercury—for various experiments, but it was a cumbersome device at best. Boyle's vacuum now made possible a vast number of experiments. He could show how a deflated bladder swelled, how the mercury in the Torricellian tube fell and how the ticking of a watch suspended by a thread grew fainter and stopped as air was removed; how a burning candle went out and how a bird or kitten without air languished and eventually died. It seemed to Boyle that there was no end to the experiments that could be performed with his air pump—experiments, moreover, that confirmed a "mechanical" view of the physical nature of air and supported the mechanical philosophy in general.

Boyle wrote up his experiments on air in 1659, and his report was published in 1660 under the title *New Experiments Physico-Mechanicall Touching the Spring of the Air, and its Effects, (Made for the most part, in a New Pneumatical Engine) Written by way of a Letter to the Right Honorable Charles Lord Viscount Dungarvan, Eldest Son to the Earl of Corke*. It was at one and the same time an admirable example of the kind of work English scientists had been doing in the 1650's and an appeal to the upper classes to take an interest in, and perhaps support, science. Boyle showed that the new experimental science could be gentlemanly, easy to read about and nontechnical, and at the same time precise and, to use the 17th-century expression, "fruitful." His was a new kind of scientific report; no one before Boyle had so carefully described the details of an experiment—the equipment, the specific manipulations and the precautions required to repeat it; no one before Boyle had so honestly confessed difficulties and errors. Although one did not need to be a scientist to repeat what he had done, few scientists could hope to do better; although many scientists rushed to have



TORRICELLI'S EXPERIMENT showed that the pressure of the atmosphere would sustain about 30 inches of mercury (*a*). It was pressure, not the weight of the air, since stopping up the opening (*arrow*) made no difference. One of Boyle's objectives in devising an air pump was to reduce the pressure of air on the exposed mercury surface. With a Torricellian tube placed inside the sealed receiver of his apparatus (*b*), he removed air from the receiver and reported that the level of the mercury column fell (*c*).



FIRST AIR PUMP, invented in 1650 by Otto von Guericke, was a suction pump that emptied a wine barrel filled with water (*top*). It did not really work. The second version removed air from a copper globe (*bottom*). In both cases operation required the sustained effort of two strong men. The engravings are from a book von Guericke published in 1672.

air pumps made for their own use, not even the great Huygens could go much beyond Boyle's original experiments with what came to be universally known as "the Boyleian vacuum."

The most others could do was to follow up Boyle's suggestion that from the effect of the vacuum on the Torricellian tube he might be able "to give a nearer guess at the proportion of force betwixt the pressure of the air and the gravity of the quicksilver," the gravity being "defined" in relation to the volume of air removed at each turn of the pump. Richard Towneley, a gentleman instrument-maker in the north of England who had been studying the Torricellian experiment, worked out the relation between the volume and pressure of rarefied air sometime after 1660. So did both Hooke and Lord Brouncker, the first president of the Royal Society. In the course of further demonstrating that the elasticity of the air could more than support the weight of mercury required in a Torricellian experiment, Boyle performed his famous J-tube experiment, in which—compressing air rather than rarefying it—he established the validity of "the hypothesis, that supposes the pressures and expansions to be in reciprocal proportion" [*see illustration on opposite page*]. The account of his experiments, with "A table of the condensation of the air" and with due acknowledgment to the independent work of others, was published in 1662. What Boyle called his "hypothesis" is of course what we call Boyle's law: with temperature held constant, the volume of a gas is inversely proportional to the pressure on it. The hypothesis seems to have been first called Boyle's law by the Swiss mathematician Jakob Bernoulli in 1683, when the term "law" was just coming into scientific use.

When Boyle published further pneumatic experiments in 1669, he remarked that others seemed unable to construct workable air pumps or perform significant experiments. Having given his original pump to the Royal Society, Boyle had done his new experiments with an improved second version. This model, in which some of the working parts were placed under water, maintained a better vacuum than the first; Boyle also attached various gauges with which to measure the degree of vacuum attained. Eleven years later he published still another work on pneumatics, this one particularly concerned with the effects of compressed air and the production of "factitious airs," such as carbon dioxide obtained from fermentation.

Boyle was a prolific writer in theology as well as science. His first book, *Seraphic Love* (1659), was much admired but is a juvenile and turgid work of devotion. Far more interesting today are his attempts to use science as a support for religion, as in *The Excellency of Theology compar'd with Natural Philosophy* (1674)—to which, characteristically, is annexed *Some Occasional Thoughts about the Excellency and Grounds of the Mechanical Philosophy*. Boyle's numerous other theological works all served science, if less directly, in that they helped to show that science did not lead to atheism and that opposition to Aristotle did not mean opposition to established religion. It is difficult now to see how any group with as many clergymen in it as the Royal Society could have been suspected of irreligion, but the society was nonetheless denounced from the pulpit, and its Fellows came to be touchy about any accusation of godlessness.

Perhaps stimulated by the establishment of the Royal Society and the fame of authorship, Boyle began in 1660 to gather up and publish his notes from earlier investigations. He maintained the informal style of the book on pneumatics and the practice of including a wealth of experimentation, carefully described. In all his works he sought to advance the mechanical philosophy by means of his old passion for chemistry. Indeed, the great aim of his life came to be the proof of the mechanical philosophy through chemical experiment and the creation of a science of chemistry through the introduction into chemical thought of the mechanical philosophy. For example, in *Certain Physiological Essays* (1661) Boyle begins with two essays on experimental science in general, continues with "Some specimens of an attempt to make Chymical Experiments Usefull to Illustrate the Notions of the Corpuscular Philosophy" and concludes with essays on the nature of fluidity and of firmness.

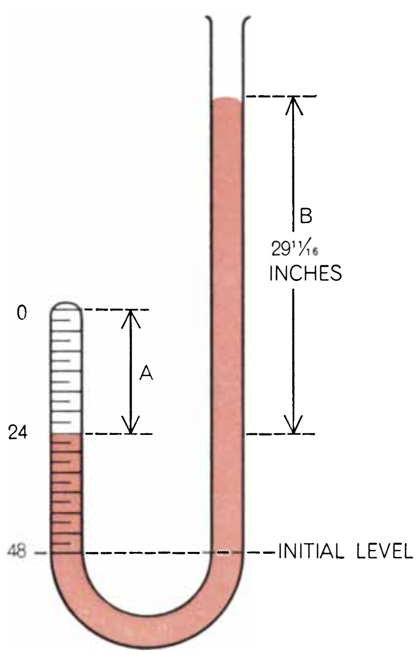
Here he demonstrates the reality of corpuscular entities by decomposing niter, or saltpeter (potassium nitrate, KNO_3), with a glowing coal and later reconstituting it. This was propaganda of a high order for the mechanical philosophy and a useful demonstration to nonchemists of the way chemical experimentation could be used to establish the truths of the mechanical philosophy. Indeed, it was the first time a chemical experiment had been used to prove the validity of a scientific theory!

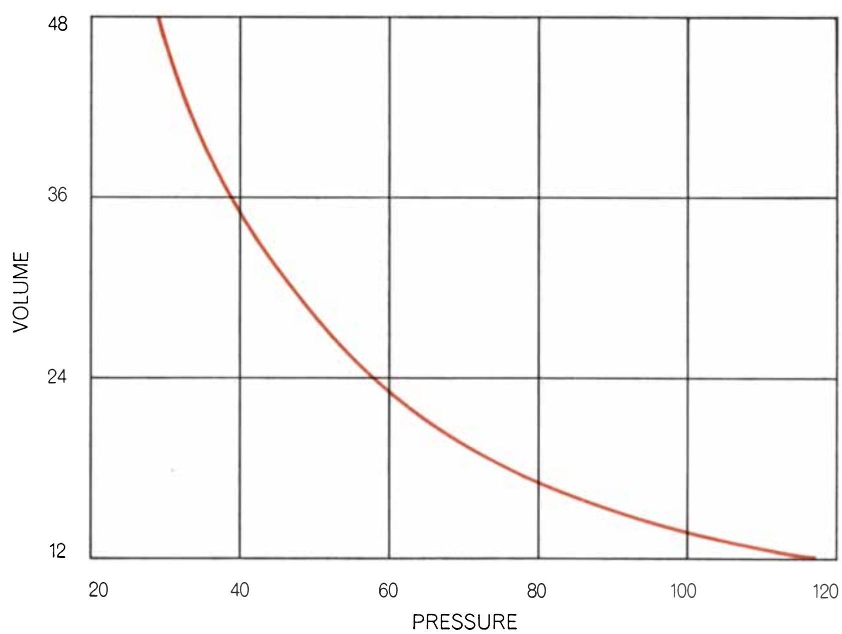
In *The Sceptical Chymist,* also published in 1661, Boyle had two aims: to demonstrate the unreality of contemporary notions of elements (substances supposed to be found in all bodies and into which all bodies could be decomposed) and to introduce chemists to the virtues of the mechanical philosophy. There is nothing modern about Boyle's definition of an element, nor did he mean to define elements in any way that would be incompatible with 17th-century theory. What he wanted to show was that those things taken to be elementary (that is, the universal components of all substances, such as earth, air, fire and water or salt, sulfur and mercury) either were not stable or were not found in all substances, and that the only things that really conformed to the accepted definition of an element were fundamental particles. Chemists were only partly convinced, but they were influenced by Boyle's novel experiments and by his manifest chemical competence.

Chemistry and the mechanical philosophy continued to go hand in hand for Boyle. In 1664 he published one of his most important works, *Experiments and Considerations Touching Colors.* He began with an exploration of the phenomena of whiteness and blackness, which he correctly declared to be the results of total reflection and total absorption of light. He demonstrated temperature effects with what was to become a classic series of experiments, including putting black and white cloths on snow and painting an object half black and half white and observing the temperature difference. When he attempted to discuss color in mechanical terms and show how the particulate structure of matter could be made to explain colors, he went partly astray; it was left to Hooke and later Newton to perceive that it was the light, rather than the colored object, that required analysis. Boyle studied the prismatic colors and tried reflecting and mixing them; it was left to Newton to refract them. So too he noticed the colors on soap bubbles and glass surfaces (Newton's rings), which first Hooke and later Newton undertook to explain in terms of light.

Because he focused his attention on matter rather than on light, Boyle was particularly interested in colored solutions; although he was therefore unable to make any advances in optics, he did make important contributions to chemistry. Studying changes in color achieved by mixing chemicals, he perceived for the first time that it was not some few particular acids that turned the blue of "syrup of violets" red but all acids, and only acids; all alkalis turned syrup of violets green. Then he found that all blue vegetable substances were similarly affected. He quickly saw that this provided not only an experimental basis for discussion of the mechanical interpretation of color but also a useful chemical discovery: if all acids turned blue vegetable solutions red, what did not do so was not an acid. And so Boyle proceeded to establish for the first time in chemistry a classification based on irrefutable chemical evidence: the division of substances into acid, alkali and neutral. (To recognize that some substances were neither acid nor alkali was a subtlety beyond many of Boyle's chemical contemporaries.) He went on to devise tests for distinguishing two of the common alkalis, potassium carbonate and ammonia, by another color change: the production of an orange or a white precipitate with a solution of mercuric chloride.

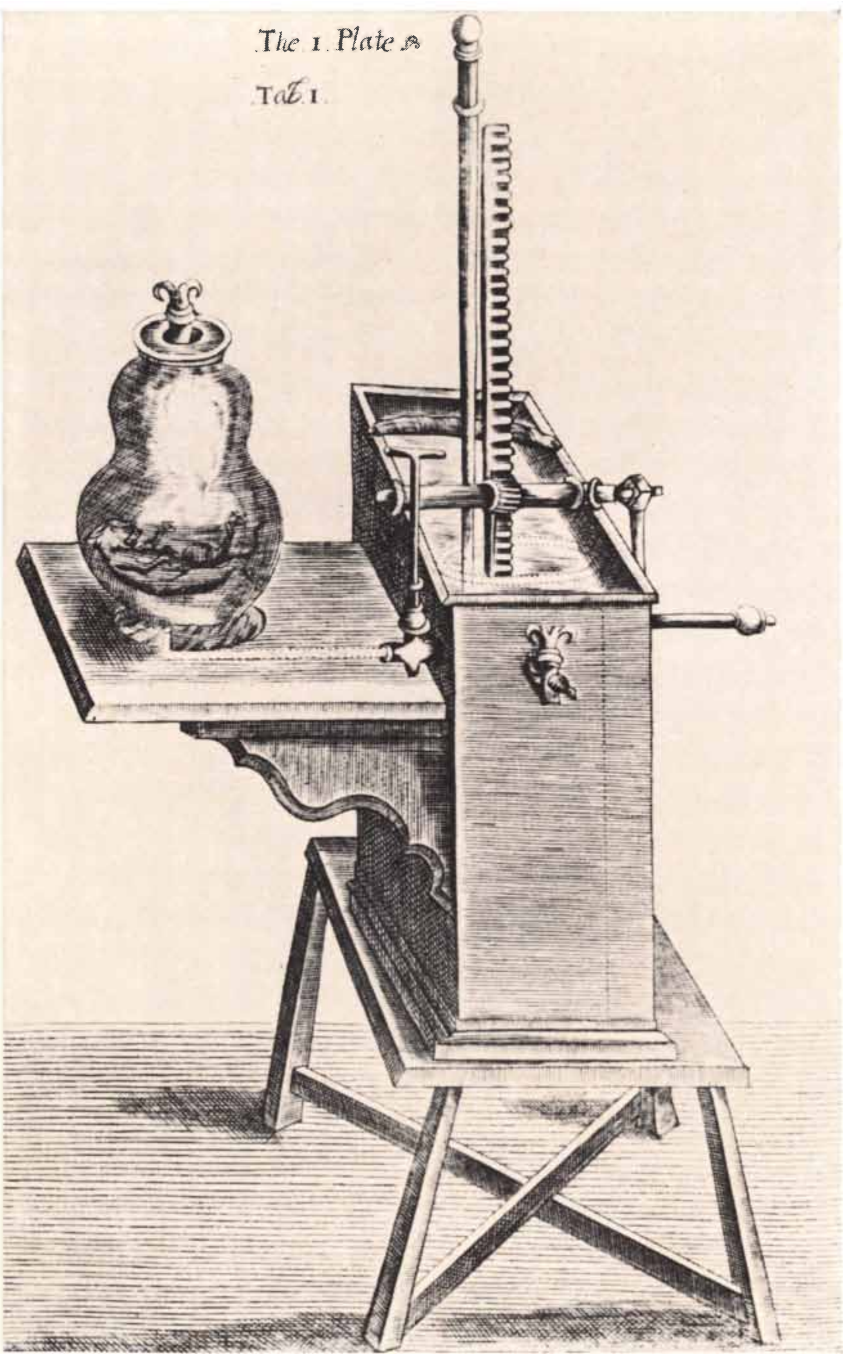
Boyle was soon in a position to work out methods of chemical analysis by combining these chemical tests with others, such as the blue imparted to solutions of copper, the characteristic shapes





J-TUBE EXPERIMENT led to Boyle's law. Boyle noted the initial volume (48 units) of the air above the mercury on the closed side of the glass tube. Adding mercury through the open end, he saw that when the volume of the trapped air was cut in half (*A*), the mercury in the long arm of the tube was 29¹¹⁄₁₆ inches higher (*B*) than in the closed side. From a table of such measurements (which yields the curve shown at the right) Boyle concluded that "the pressures and expansions" are "in reciprocal proportion," or that the volume of a gas is inversely proportional to the pressure on it ($P_1V_1 = P_2V_2$), provided that the temperature is kept constant.

of crystals, the specific gravity of fluids, the formation by silver of its chloride with "spirit of salt" (hydrochloric acid), the blackening of this compound on standing and so on. How such tests could be used he demonstrated in three later treatises: *Short Memoirs for the Natural Experimental History of Mineral Waters* (1685), a collection of tests and questions to be applied to unknown samples from new mineral springs, and *The Aerial Noctiluca* and *The Icy Noctiluca* (1680 and 1681), two wonderful experimental investigations of the element phosphorus. This mysterious substance had been discovered in Germany. Boyle saw some samples; being told that it derived from "somewhat to do with man's body," he deduced that the starting point must be human urine and soon produced a small quantity of the luminescent substance. His investigation was thorough and well organized, and he detected nearly all the properties of phosphorus that were to be known until the late 19th century: its flammability and how to control it by keeping the solid under water; its white and red forms; its solubility in various compounds, including certain aromatic oils; its ability to glow at low dilutions; its formation of phosphoric acid. *The Icy Noctiluca* deserves to be read as a classic example of early chemical analysis.



SECOND AIR PUMP made for Boyle was somewhat more airtight than the first. The glass receiver (in which a kitten is seen in the engraving) stood separate, connected by tubing (*broken lines*) to the pump proper, the working parts of which were placed under water.

Besides such contributions to practical chemistry, which were much appreciated by his younger contemporaries, Boyle made important contributions to theoretical chemistry. His refusal to accept the concept of elements forced him into a totally new pattern of thought in which complex chemical substances were considered to be composed of simpler, known chemical substances rather than of mysterious, unknowable and vaguely defined elements. His analysis of chemicals in terms of "simple substances" such as the common salts, acids, metals and so on transformed chemistry, because analysis in these terms added to knowledge in a way that analysis in terms of hypothetical elements did not. The change in approach is illustrated by the fact that the early chemists of the French Academy of Sciences had spent two decades analyzing plants in the older style, inevitably reaching no conclusion other than that plants were all very similar in composition; Boyle's methods led French chemists in a similar stretch of time to reach the modern definition of a salt—a conclusion they might have reached earlier had they not at first regarded his turn of thought as too rationally physical to be serviceable in chemistry.

In 1668 Boyle had left the comparative quiet of Oxford for the busy life of London. In 1680 he was elected president of the Royal Society, but he turned down the office because of a scruple about taking an oath. Even without the title, however, he fulfilled the role of distinguished representative of English science. Learned foreigners came to visit; young scientists came to work in his laboratory. Through these young men—German, French and Dutch as well as English—his methods spread slowly but surely. His concept of a chemistry based on simple substances of definite composition that could be detected by experiment became universally accepted in the 18th century. He was, as a contemporary put it, "a mighty chemist."

Basic Research at Honeywell
Research Center
Hopkins, Minnesota

The Effects of Pulsed Laser Bombardment of Surfaces

Studies of the interaction of pulsed laser beams and opaque surfaces in vacuum have revealed some unexpected non-thermal effects. New experiments now suggest a theory to explain this phenomenon.

Much work has been done to develop the laser since it was first operated in 1960, yet little is known about what occurs when the beam strikes a surface.

Earlier work in the U.S. and abroad confirmed that when a laser beam strikes a surface, heat is pumped in so fast that the surface temperature rises to the vaporization point of the material. Ions, electrons, neutral atoms and molecules are emitted. Unexpectedly, however, high-energy particles are observed. These ionized atoms are emitted with too high an energy (several hundred electron volts) to have resulted from ordinary thermal effects. Normally such energies would require surface temperatures above 1,000,000°C, yet the vaporization points of the materials examined do not exceed 6000°C.

Honeywell scientists, supported by the Ballistics Research Laboratory, Aberdeen Proving Ground, Maryland, are conducting experiments hopefully to explain this phenomenon.

Their approach is to observe the particle energy emitted in relation to the power in the laser beam as it strikes a surface. They are working with a Q-switched or giant pulsed, laser which generates short, very high-power pulses in contrast to a normal pulsed laser where the energy is higher but the power lower.

In Honeywell's experiments a quadrupole mass spectrometer is used as a mass filter to detect particles emitted as the laser beam strikes the target. Using it scientists can identify and count the number of particles of a given mass regardless of energy. Thus all species desorbed can be identified.

Also, a time-of-flight spectrometer developed by Honeywell is used to detect all charged particles and to separate them by time of arrival. With proper calibration masses and energies can be identified.

Using tungsten as a target, Honeywell scientists have observed desorption of gases and the emission of high-energy neutral molecules and high-energy ions. For example the quadrupole spectrometer has recorded CO, CO_2 and H_2 pulses plus hydrocarbons, water vapor and oxygen emitted through normal desorption processes.

However, some high-energy neutral CO, CO_2 and H_2 molecules were detected with energies of the order of 100 electron volts per molecule, an energy far too high to have been produced by normal vaporization processes. Figure 1 shows a series of these high-energy pulses. The narrow initial pulse is caused by ultraviolet light. The second, higher pulse is due to hydrogen molecules; the third to CO molecules.

On the time-of-flight spectrometer, ions were identified as mainly sodium and potassium but were also lithium, carbon, hydrocarbons and, of course, tungsten. Very few CO and CO_2 ions were observed. Sodium and potassium ions registered energies of 200 electron volts per ion, again far too high to be produced by vaporization.

These measurements produce results that appear to be different. Sodium is observed on the time-of-flight but not on the quadrupole; mostly CO and CO_2 are observed on the quadrupole but only traces appear on the time-of-flight. These apparent contradictions can be explained by considering that the quadrupole spectrometer is better suited to detect neutral molecules, while the time-of-flight is primarily an ion detection instrument.

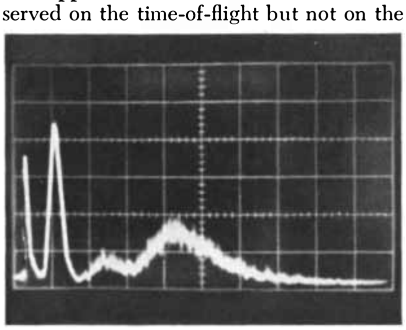


FIGURE 1. Oscilloscope trace of quadrupole spectrometer output. Pulses are of high-energy molecules emitted from a tungsten surface struck by a laser beam with a power of about 50 million watts per sq. cm. Horizontal time scale: 10 microseconds per major division. Vertical: 10 millivolts per major division.

From their observations Honeywell scientists have developed this hypothesis: when the laser pulse strikes the surface a small amount of material vaporizes early in the interaction, forming a thin sheath of electrons and ions. The laser light is absorbed by the charged particles in this thin sheath. The ions become hot because of the absorption of energy. Since the rate of energy exchanged by collision is slow, the neutral molecules remain cool. The sheath goes to several hundred thousand degrees centigrade while the surface itself is at the boiling point of tungsten (5900°C). The relatively small component of neutral high-energy molecules results from energy exchanges in collisions with the hot ions. The sheath and the surface are then out of equilibrium and are thermally decoupled. Thus the heat of the sheath cannot flow to the material.

When the laser pulse ends and the heating stops, the high-temperature gas expands. This adiabatic expansion gives the high velocity to the ions and explains the emission of high-energy ions. Similar effects have been observed using targets of titanium, nickel, platinum and carbon.

Hopefully further work will lead to new laser applications and further understanding of the nature of materials.

If you are engaged in research in laser surface interactions and wish further information on Honeywell's plans for work in this area, write Mr. John F. Ready, Honeywell Research Center, Hopkins, Minn. 55343. If you are interested in a career at Honeywell's Research Center and hold an advanced degree, write Dr. John Dempsey, Vice President Science and Engineering at this same address. Several important new staff positions are unfilled at the present time.

Honeywell

MATHEMATICAL GAMES

In which a computer prints out mammoth polygonal factorials

by Martin Gardner

Mathematical formulas, particularly combinatorial formulas, are sometimes sprinkled with exclamation marks. These are not expressions of surprise. They are operational symbols called factorial signs. A factorial sign follows a whole number or an expression for such a number, and all it means is that the number is to be multiplied by all smaller whole numbers. For example, 4!, read as "factorial four," is the product of $4 \times 3 \times 2 \times 1$. (In older books factorial n is symbolized $\underline{n|}$.)

Why are factorials so important in combinatorial mathematics and in probability theory, which relies so heavily on combinatorial formulas? The answer is simple: Factorial n is the number of different ways that n things can be arranged in a line. Visualize four chairs in a row. In how many different ways can four people seat themselves? There are four ways to fill the first chair. In each of those ways there are three ways the second chair can be occupied, and so there are 4×3, or 12, ways to fill the first two chairs. For each of those ways there are two ways to occupy the third chair, and so there are $4 \times 3 \times 2$, or 24, ways to fill the first three chairs. In each of those 24 instances there is only one person left to take the fourth chair. The total number of distinct ways of occupying the four chairs is therefore 4!, or $4 \times 3 \times 2 \times 1$, or 24.

The same reasoning shows that 52 playing cards can be made into a deck in 52! different ways, a number of 68 digits that begins 806581. . . .

What is the probability that a bridge player will be dealt 13 spades? We first determine the number of different bridge hands. Since the order in which the cards are dealt is irrelevant, we want to know not the permutations but the number of different *combinations* of 13 cards that can be made with the 52 cards of the deck. The formula for a combination of n elements, taken r at a time, is $n!/r!(n-r)!$. With n equaling 52 and r equaling 13, we have $52!/(13! \times 39!)$, which works out to 635,013,559,600. A bridge player can therefore expect to be dealt all spades once in every 635,013,559,600 deals. His expectation of getting 13 cards of the same suit—not necessarily spades—is one-fourth of this, or once in every 158,753,389,900 hands. The chance that such a hand will be dealt to one of the four players at the table is obtained by again dividing by four. This is still such a stupendously low probability, 1/39,688,347,497, that one is led to conclude that reports of such hands, which turn up in some newspaper around the world about once a year, are almost certainly false reports or hoaxes.

It is easy to see from these elementary examples that it is much simpler to express large factorials by using the factorial sign than to write out the entire number. Indeed, factorials increase in size at such a rapid rate [*see illustration at left on this page*] that until the advent of high-speed computers exact values for factorials were known only up to about 300!. Higher factorials were approximated by means of "Stirling's formula," named after James Stirling, a Scottish mathematician of the 18th century:

$$n! \simeq n^n e^{-n} \sqrt{2\pi n}\,.$$

It is a strange formula, involving the two best-known transcendental numbers, pi and e. The formula's "absolute error" (the difference between a factorial's true

0! = 1
1! = 1
2! = 2
3! = 6
4! = 24
5! = 120
6! = 720
7! = 5,040
8! = 40,320
9! = 362,880
10! = 3,628,800
11! = 39,916,800
12! = 479,001,600
13! = 6,227,020,800
14! = 87,178,291,200
15! = 1,307,674,368,000
16! = 20,922,789,888,000
17! = 355,687,428,096,000
18! = 6,402,373,705,728,000
19! = 121,645,100,408,832,000
20! = 2,432,902,008,176,640,000

Factorials from 0 to 20

FACTORIALS	NUMBER OF DIGITS
7	4
12	9
18	16
32	36
59	81
81	121
105	169
132	225
228	441
265	529
284	576
304	625
367	784
389	841
435	961
483	1,089
508	1,156
697	1,681
726	1,764
944	2,401

Tree factorials less than 1,000!

```
            1
           081
          39675
         8240290
        900504101
       30580032964
      9720646107774
     902579144176636
    57322653190990515
   3326984536526808240
  339776398934872029657
 99387290781343681609728
0000000000000000000000000
```

Tree print-out of the 169 digits of 105!

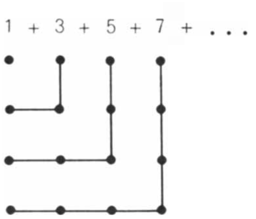
value and its approximation) increases as factorials get larger, but the "percentage of error" (absolute error divided by the true value) steadily decreases.

For practical purposes the formula gives excellent approximations of high factorials, but mathematicians have a compulsion to know things precisely. Just as mountain climbers will scale a peak or space explorers go to the moon "because it is there," so mathematicians with access to computers have irrepressible urges to explore the "outer space" of enormous numbers. It was the computer that made it possible, by monitoring and interpreting signals, to take closeup looks at the surface of the moon and Mars. Those same computers have also made it possible to take closeup looks at large factorials, gigantic numbers that have been known for centuries only in a vague, out-of-focus way.

The remarks in the previous paragraph are taken from a communication received from Robert E. Smith, director of computer applications at the Control Data Institute in Minneapolis. He writes that he was exploring the outer fringes of factorials when it occurred to him that it would be pleasant to print on his Christmas card, in the form of a Christmas tree, one of his mammoth factorial numbers. It would be necessary, of course, to have the computer print one digit for the top of the tree, then a row of three digits, then a row of five and so on. Are there factorials with the proper number of digits so that such a print-out would form a perfect tree? Yes, there is an infinity of them. The table [*see illustration at left on opposite page*] shows, for example, that 12! has nine digits. It can be printed in tree form like this:

4
790
01600

A tree factorial obviously must have a number of digits that is a partial sum of the infinite series $1 + 3 + 5 + 7 + \dots$. A glance at the square array of spots below shows that all such sums are perfect squares:



Smith's task, therefore, was to pro-

5
119
90692
7755879
266003615
25819185379
7984360677298
470133958906714
46011174633964398
5839112233165772956
548496166254935516795
14565079522588677608012
6423489045662147453126349
825790036437158643266482002
88113505694916924243929121639
7995123320680205388149829536720
697546589338105120020005674705145
28641409978978956631664608452253922
2182139322091260889711710217500934598
659546487929459214735007200769105667735
54074289548655659977226200540160335058131
8365384235510714071491098835812736588922795
511456461421254773804907853073384484888784090
75030962875912509521999525292598359880846423952
3931204111818280979213544777644751538435208774603
088477116032223651164439419220002073567325180151958
35354728897604905269289015307797618984464654042934912
7882733479825616955531216107050271401259459875249508169
440013327395316887000833911764483284987619075088343797786
47371945157918046252226969546616811434035461815792968273198
2545625613705049834238544557702694536385292145346080336071424
289160111720849018903249047529128422886467764267877861568498090
42964480000000000000000000000000000000000000000000000000000000000
0000000000000000000000000000000000000000000000000000000000000000000

Tree print-out of the 1,156 digits of 508!

17108972589718074
1439528307936299026
080765545554532458183
43255130543516432376912
4663791911119657860822050
367340495642348613717749611
38104459104482535212494659899
5225079402598873366451131040234
240130493689852679573590918519290
66647636392705738600295487428650940
0535103538524596394743595531728001643
083783948745781956212836911156587085000
40781396853030778257813849856692950471963
5089328018573725755534194119396813233357487
709737509271413007324171020350516977549843435
61187933295519151457453789138048055187827977590
7750007855795139817496078270462761613125177420579
97170554688538703689036095806399241086011592997
020790226888203087101533653915806041722653430
0377642434651424325601245917031000886439794
86942002854170097571338930915447098888372
333024657251637441276280296188483408232
2723195014038951851520634322622612616
12431271509190879459978732133255390
601413833379281814639023615443036
2338368861798856260050552801204
22598617062894203619648023806
809643832836096000000000000
0000000000000000000000000
00000000000000000000000
000000000000000000000
0000000000000000000
00000000000000000

Hexagon print-out of 477! (1,073 digits) gram a computer to search for large factorials that have a square number of digits but instead of printing them in square formation to print them in lines of 1, 3, 5, 7... digits to form a tree. This was done. The computer tested all factorials up to approximately 1,000! (and a few larger ones) and found exactly 20 factorials less than 1,000! that contained a square number of digits [*see upper illustration at right on page 104*].

In the print-out of 105! from Smith's computer, note the bottom row of zeros [*see lower illustration at right on page 104*]. If the reader will study the series $1 \times 2 \times 3 \times 4 \times \ldots$, he will see that every multiplication by a number ending in 5 will add a zero to the running product, and that every multiplier ending in one or more zeros will add additional zeros. Because the zeros cannot be lost by later multiplications they are cumulative, piling up steadily at the ends of factorials as they get larger. Factorial 105 has a "tail" of 25 zeros. Stirling's formula, applied to 105!, gives an approximation of 1,081, followed by 165 zeros. When this blurry result is compared with the exact result shown in the illustration, one sees that, in Smith's words, "using Stirling's formula for arriving at large factorials is analogous to a blind man trying to visualize an elephant by grasping a couple of inches of its trunk in one hand and the tip of its tail in the other."

Octagon print-out of 2,206! (6,421 digits!)

"It may surprise the reader to know," Smith continues, "that computers cannot calculate results this large in one piece. That is, the capacities of individual computer cells are soon exceeded, so normal computer arithmetic cannot be used." The trick, he explains, is to use several internal "bins," each of which is allowed to hold t digits of a result. "After each multiplication is performed, excess digits beyond the t digits, in each bin, are removed and added to the bin on their immediate left. Then the digits in all the bins, which form the factorial result, are printed." A fuller explanation of this program will be found in Smith's recent book, *The Bases of FORTRAN*, published earlier this year by the Control Data Institute. Another large tree factorial computed by this method is shown at the top of the preceding page.

When the number of digits in a factorial is equal to two consecutive squares, such as 35!, which has $4^2 + 5^2 = 41$ digits, it can be printed as a diamond. One simply turns the smaller tree of 4^2 digits upside down and fits it against the base of the 5^2 tree like this:

1
033
31479
6638614
4929 6665
1337523
20000
000
0

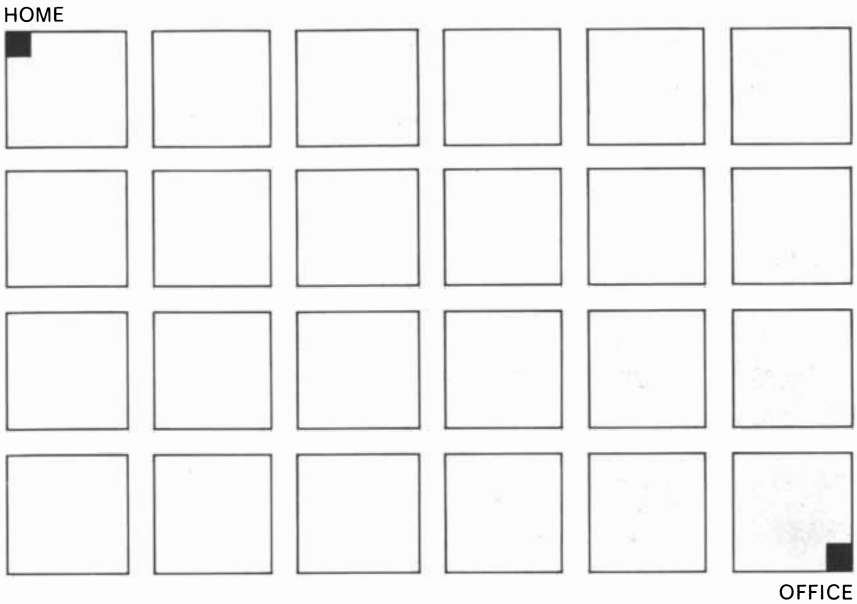
I have blanked out the center digit to give the reader an amusing quickie problem to be answered next month. What is the missing digit? By applying a simple trick that is known to most accountants and that every reader of this column ought to know, the missing digit can be found quickly without a single multiplication.

There are many other basic geometric shapes in which certain factorials can be printed. Smith's computer printed 477! as a hexagon with 17 digits on each side [*see bottom illustration on preceding page*]. Finally, to stagger the reader with one of Smith's computer's supermonstrosities, there is 2,206! in octagonal form [*see illustration on opposite page*]. If someone had predicted 50 years ago that before the century ended this factorial would be written out in full, digit by digit, most mathematicians would have laughed at such a preposterous prophecy.

Factorials, as one would expect, are closely related to primes. The most famous of the many elegant formulas that link the two kinds of number is known as Wilson's theorem, after an 18th-century English judge, Sir John Wilson, who hit on the formula when he was a student at Cambridge. (It later developed that Leibniz knew the formula.) Wilson's theorem says that $(n-1)!+1$ is divisible by n if, and only if, n is a prime. For example, if n equals 13, then $(n-1)!+1$ becomes $12!+1=479{,}001{,}601$. It is easy to see that 12! is not a multiple of 13, because 13 is prime and the factors of 12! do not include 13 or any of its multiples. But astonishingly the mere addition of 1 creates a number that *is* divisible by 13. Wilson's theorem is one of the most beautiful and important theorems in the history of number theory, even though it is not an efficient way to test primality.

There are many simply expressed but difficult problems about factorials that have never been solved. No one knows, for example, if a finite or an infinite number of factorials become primes by the addition of 1, or even how many become squares by the addition of 1. (We are concerned now with the number itself, not its number of digits.) It was conjectured back in 1876 by H. Brocard, a French mathematician, that only three factorials—4!, 5! and 7!—become squares when they are increased by 1. Albert H. Beiler, in *Recreations in the Theory of Numbers*, says that this has been investigated by computers up to factorial 1,020 without finding any other solution, but Brocard's conjecture remains unproved.

It is easy to find factorials that are the products of factorials but hard to find them if the factorials to be multiplied are in arithmetic progression and still harder to find them if they are consecutive. According to Joseph S. Madachy, in *Mathematics on Vacation*, only three consecutive instances are known: $0!\times 1!=1!$, $1!\times 2!=2!$ and $6!\times 7!=10!$. It should be explained, in connection with the first solution, that 0! is defined as 1 in spite of the fact that 1 is also the value of 1!. Strictly speaking, 0! is meaningless, but by making it 1 many important formulas are kept free of anomalies. Suppose, for example, we use the formula given ear-

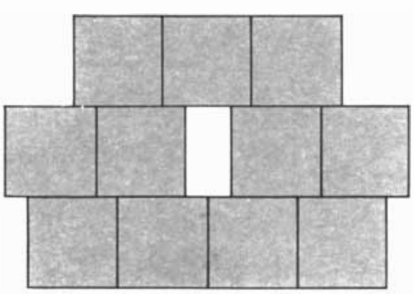


Route problem involving factorials

lier, $n!/r!(n-r)!$, to determine how many combinations there are of two things taken two at a time. The answer, of course, is 1. The formula gives that answer only if 0! equals 1. If 0! equals 0, the formula has a value of 0. The famous binomial theorem, discovered by Isaac Newton, is another classic instance of a basic formula that can be debugged, so to speak, by defining factorial 0 as 1.

The quaint problem of finding whole numbers equal to the sum of the factorials of their digits has recently been solved. There are, says Madachy in his book, only four solutions. Two are trivial: $1 = 1!$ and $2 = 2!$. The largest example was found in 1964 by Leigh Janes of Houston, using a computer: $40{,}585 = 4! + 0! + 5! + 8! + 5!$. Can the reader find the remaining solution? It can be expressed as $A! + B! + C! = ABC$, each letter standing for a different digit. The solution will appear next month.

Of many classic recreational problems for which factorials provide elegant solutions, I select one from graph theory. A man who lives at the top left corner of a rectangular grid of city blocks [*see illustration above*] works in an office building at the bottom right corner. It is clear that the shortest path along which he can walk to work is 10 blocks long. Bored with following the same route each day, he begins to vary it. How many different 10-block routes are there connecting the two spots? And what is a compact formula for the number of different minimum routes joining the diagonally opposite corners of any rectangular area of blocks? The answers will be given next month. (Here is a hint: The number of different arrangements, or permutations, of n objects of which a objects are identical and the remaining b objects are also identical is $n!/a!b!$.)



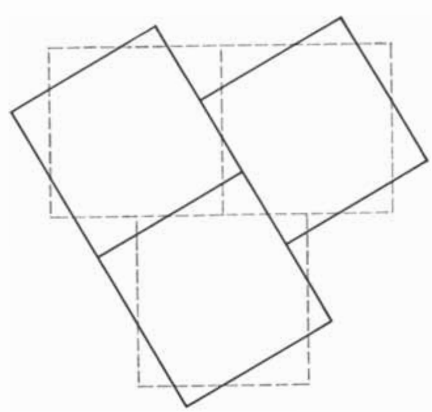
Solution to monomino problem

Readers were asked last month to determine why the game of Brussels sprouts, which appears to be a more sophisticated version of sprouts, is considered a joke by its inventor, John Horton Conway. The answer is that it is impossible to play Brussels sprouts either well or poorly because every game must end in exactly $5n-2$ moves, where n is the number of initial crosses. If played in standard form (the last to play is the winner), the game is always won by the first player if it starts with an odd number of crosses, by the second player if it starts with an even number. (The reverse is true, of course, in misère play.) After introducing someone to sprouts, which is a genuine contest, one can switch to the fake game of Brussels sprouts, make bets and always know in advance who will win every game. I leave to the reader the task of proving that each game must end in $5n-2$ moves.

Another problem last month was to arrange 11 identical squares so that four colors would be needed to prevent two squares of the same color from bordering. The bottom illustration at the left on this page shows a solution. It is easy to prove that the squares cannot be colored with fewer than four colors, but so far no one has proved that 11 squares is the minimum.

The answer to the question about the cubes is that a maximum of six cubes can be placed so that every pair shares a common surface. In the solution [*see illustration below*] the three cubes shown by the solid lines rest on three shown by dotted lines.

In June, when I reported a conjecture that there are 108 pentaboloes, I intended to say hexaboloes. Wade E. Philpott, Charles Trigg and Spencer Brown were the first to point out that there are 30 pentaboloes and 107 hexaboloes. Walter Gilbert of the National Bureau of Standards wrote a computer program to count the polyhexes of orders 6, 7 and 8 and found the count to be 82 hexahexes, 333 heptahexes and 1,448 octahexes. This confirmed an earlier computer program by Louis S. Kassel of Park Ridge, Ill., for orders 6 and 7. David Fried, George R. Pryde and Van E. Wood were the first of many to prove that the solution for the tetrahex tower is indeed unique except for the trivial variation cited in July. Philpott found a second set of four tetraboloes (C, I, K, L) that solved the replication problem. John Harris completely cleared up the problem of the four unsolved tetrabolo rectangles by proving ingeniously that the 3×8 and the 4×6 are impossible, and by supplying solutions for the two h-edge rectangles, 2×6 and 3×4.



Solution to cube problem

THE AMATEUR SCIENTIST

A gauge to measure tiny changes in gas pressure; growth substances in plants

Conducted by C. L. Stong

Several interesting experiments in meteorology, biology and low-speed aerodynamics require the measurement of small variations in the pressure of gases. For example, wind speed is routinely measured by an anemometer that consists of a sensitive pressure gauge connected to a slender tube, the open end of which faces into the airstream. Pressure inside the tube varies with the velocity of the airstream and is displayed in units of velocity on the dial of the gauge.

Practical forms of the instrument, such as the airspeed indicators of airplanes, also include provisions for balancing out the effects of normal atmospheric pressure. At airspeeds of a few miles per hour the change of pressure inside the open-end tube amounts to less than .001 ounce. Accurate gauges capable of measuring pressures of this magnitude have been based on several principles, but most of the devices are priced beyond the amateur's reach and include precisely machined parts that cannot readily be made at home.

An exception is a gauge developed recently in the Department of Engineering Mechanics at the University of Michigan. The device is a micromanometer consisting essentially of a relatively large reservoir of fluid, connected through a flexible hose to a glass tube that is inclined at an angle of about five degrees from the horizontal, and a dial indicator for measuring vertical displacements of the inclined tube. Differences in gas pressure exerted on the surfaces of the fluid in the reservoir and the inclined tube change the level at which fluid stands in the tube. The altered level can be restored to its normal position by raising or lowering the tube. The pressure of the gas is then calculated by taking into account the density of the fluid and the displacement of the inclined tube as measured by the dial indicator. When water is used as the working fluid, the instrument is capable of measuring a change of gas pressure of .00015 pound per square inch to an accuracy of approximately 1 percent. Details of the construction are described by Walter R. Debler, associate professor of engineering mechanics at the University of Michigan, who writes:

"Our micromanometer was designed primarily for use in wind-tunnel tests, although it should work as well in any application that involves gas pressures ranging from about one pound to a small fraction of a pound per square inch. When it is equipped with a dial indicator of two-inch working range, the instrument is capable of measuring pressure heads of two inches in increments of .001 inch. With mercury as the working fluid a two-inch head of pressure is equal to .98 pound per square inch. With water as the working fluid the maximum and minimum measurable pressures are .07 pound and .00004 pound per square inch.

"The instrument [*see illustration on opposite page*] was assembled from inexpensive and commercially available materials. It is rugged and portable. It is six inches wide, six inches deep and nine inches high.

"A 250-milliliter aspirator bottle serves as the reservoir. The hose connection at the bottom of the bottle is coupled by flexible tubing to a four-inch length of eight-millimeter glass tubing that is supported at a grade of 10 to one by a five-inch length of square aluminum tubing. The bottle is fastened to the base with an apparatus clamp.

"The inclined tube is transported vertically by a structure consisting of two lengths of square aluminum tubing equipped with a nylon sleeve bearing that slides on a half-inch steel rod. The assembly is moved up or down by a screw that engages a threaded plug free to slide inside the lower of the two square aluminum tubes. The threaded screw passes through slots in the upper and lower surfaces of the square tube and therefore has the same freedom of movement as the threaded plug. This feature of the design prevents the assembly from binding if the screw and the steel rod are misaligned during assembly.

"The brass screw is of the prethreaded type that can be bought in most hardware stores. It functions merely to raise and lower the inclined tube assembly. The position of the assembly is measured by the dial indicator. Hence there is no need for the costly device known as a lead screw.

"The ends of the screw were machined to a diameter of a quarter-inch to fit nylon bushings in the base and in a bracket at the top of the instrument. This operation was performed on an engine lathe, but it could have been done with a hand drill and a file. A fiducial, or reference, mark was made at the center of the inclined tube by chucking the glass in a lathe and holding a file lightly against the tube. The resulting scratch was filled with India ink. A satisfactory mark could also be made by winding a fine wire around the tube.

"The dial indicator is supported by a vertical column of square aluminum tubing; the column is attached by screws or rivets to the base and to the upper cross members. The plunger of the dial indicator bears against the upper surface of the inclined tube assembly. The dial indicator of our instrument, which is graduated in intervals of .001 inch, can measure a maximum length of two inches. It was bought from a mail-order house for about $28. Similar dial indicators with a maximum capacity of one inch and half an inch are currently priced at $15 and $12 respectively. The maximum range of the dial indicator and the density of the working fluid establish the maximum gas pressure that can be measured with the micromanometer. A column of mercury one inch high exerts a pressure of .49 pound per square inch and a one-inch column of water .036 pound.

"The instrument is fitted with two gas connections, one leading to the reservoir and the other to the inclined tube. The inclined-tube connection is made through a flexible tube that leads to a fixed nipple at the top of the supporting structure. Pressures higher than normal atmospheric pressure are measured by connecting the source to the reservoir and leaving the inclined-tube connection open to the atmosphere. When measuring differences in pressure, the high-pressure source is connected to the reservoir and the low-pressure source to the inclined tube.

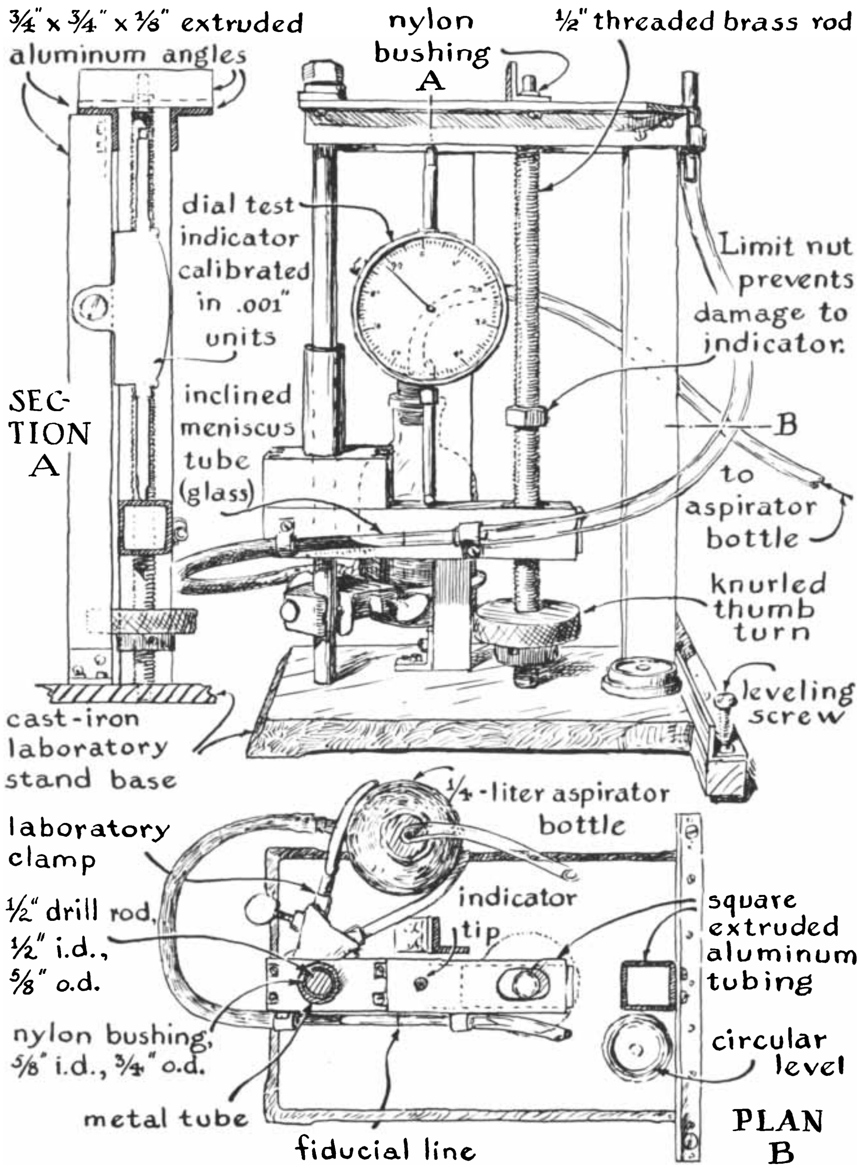
"The apparatus rests on three legs. One is a fixed leg near the edge of the base on the bottom. The other legs are adjusting screws located at the corners of the opposite edge. The screws turn in T-shaped metal fittings that are nailed to a small wooden block. The fittings are available in hardware stores. The base assembly also includes a circular bubble level.

"The instrument is put in operation by leveling the base, moving the inclined tube assembly to the position at which the plunger of the dial indicator is fully extended, filling the reservoir until the meniscus, or upper surface, of the working fluid in the inclined tube is centered on the fiducial mark and rotating the dial of the indicator to zero. Gas pressure admitted to the instrument will shift the position of the meniscus in the inclined tube by a distance 10 times greater than the difference in the level at which fluid stands in the reservoir and the inclined tube. The inclined tube is next raised or lowered to center the shifted meniscus on the fiducial mark. The vertical displacement is then read on the dial of the indicator to the nearest .001 inch. Finally, the unknown gas pressure is calculated, in pounds per square inch, by multiplying the weight of one cubic inch of the working fluid by the dial reading. The density of most common fluids is listed in standard reference texts."

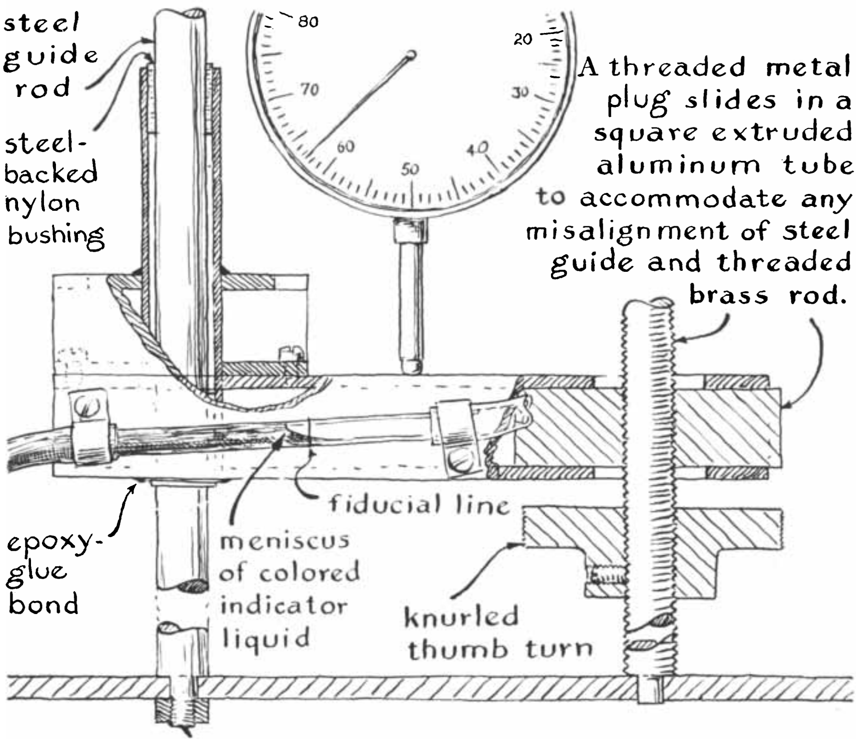
The power of the gibberellins to accelerate the growth of plants continues to fascinate amateur botanists. One of the substances is gibberellic acid, also known as GA-3. It is a natural product of an Asian fungus that destroys rice. Even though it has become commercially available in recent years from dealers in biological supplies, most amateurs now prefer to extract their own supply from such common plants as the cucumber, cantaloupe, corn and beans, all of which are plants that develop growth-promoting substances that are either identical with or closely related to the gibberellins. During the past two years Edward Pinto of Ridgefield, N.J., a student at St. Peter's Preparatory School in Jersey City, has worked out an inexpensive procedure for extracting the substances and observing their influence on the cellular structure and density of plant pigments in beans. Pinto describes the procedure as follows:

"My experiments were undertaken in an effort to find a simpler procedure than the one described previously in 'The Amateur Scientist' [Scientific American; August, 1964] for extracting gibberellin-like substances from common plants; to study the effects of the substances on plant growth at the cellular level, and to equate the substances with the gibberellins. As sources of materials I used the seeds of fresh cantaloupe and fresh wild cucumber and the dry seeds of corn, peas and three species of bean—pencil rod, lupine and pinto.

"The procedures used in my final experiments begin with the extraction of the growth-promoting substances. First, all seeds are dried at room temperature and chopped into particles about three millimeters in diameter. Two hundred grams of each specimen are soaked (at a temperature of 4 degrees centigrade) for seven days in 110 milliliters of a solution that consists of 10 parts (by volume) of acetone, five parts of isopropyl alcohol, two parts of ethyl alcohol and five parts of distilled water. The extract is poured off and the particles that remain are rinsed with 40 milliliters of a solution consisting of equal parts of acetone and isopropyl alcohol. The rinsing solution is added to the extract and heated, in a



Views of the micromanometer developed at the University of Michigan

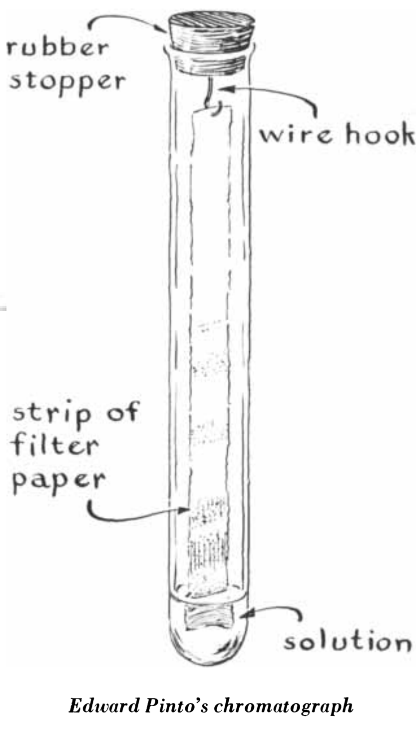


Inclined-tube subassembly of the micromanometer

double boiler on a hot plate, to a temperature of 45 degrees C. (Warning: The solution is highly flammable and must not be exposed to an open flame.) The heating is continued until the residue evaporates to the consistency of thin tar and is almost dry.

"The residue is next mixed with 100 milliliters each of water and ethyl acetate. The *p*H of the solution is brought to 8 (slightly alkaline) by the addition of potassium hydroxide; at this *p*H the gibberellins are soluble in water. After the mixture has been shaken for two minutes the water is drawn off and mixed with another 100 milliliters of ethyl acetate. This procedure is carried out a total of three times.

"Now the water is made acidic (*p*H 3) by the addition of hydrochloric acid; at this *p*H the gibberellins are soluble in ethyl acetate. The solution of acidic water is added to 100 milliliters of ethyl acetate. The water is drawn off and the procedure is repeated twice more, after which the ethyl acetate solution is dried to a paste. Half of the tarlike mass is mixed with four grams of lanolin. The remaining half is dissolved in 30 milliliters of acetone and stored in a stoppered flask. The lanolin mixture is applied to test plants; the solution is stored for subsequent analysis by chromatography.



Edward Pinto's chromatograph

"I learned from the literature that the pinto bean is exceptionally sensitive to the gibberellins and accordingly I chose this plant for the tests. I bought the beans in a grocery and planted them in 20 labeled pans containing sterilized topsoil. They were grown in a corner of our basement under artificial sunlight supplied by a bank of special fluorescent lamps designed for strong emission in the deep red portion of the spectrum.

"According to the literature the gibberellins inhibit root growth in young plants. For this reason the beans were allowed to grow normally for two weeks. During this time each plant developed two mature leaves.

"Ten plants were then selected from each pan for treatment. The upper surface of each mature leaf was coated lightly with the lanolin paste. The paste was applied with a fingertip, care being taken to avoid damaging the plant. The procedure was repeated for each kind of extract, including a specimen of commercial GA-3 gibberellic acid, although not all experiments were made simultaneously.

"Following treatment, a daily record was made of the growth of each plant for three weeks. The measurements included leaf area and stem length. Leaf color was estimated visually and recorded.

"The growth of all treated plants either equaled or exceeded that of the controls. Plants that were treated with extracts of pencil-rod beans grew approximately 50 percent larger than the controls, with lupine-bean extract 55 percent and with wild cucumber extract 75 percent. The growth with wild cucumber extract matched the growth of plants treated with commercial GA-3. In most cases treated plants that were smaller on the average than the controls at the beginning of the experiment exceeded the control plants in size at the end.

"In general the color of the treated plants was lighter than that of the controls, indicating a lower density of pigment. Leaf specimens of both treated and control plants were analyzed for pigment concentration by chromatography, a simple procedure in which substances are separated according to the rate at which they migrate on an adsorbing medium such as filter paper. To make the analysis the pigments must be extracted from the leaves of interest in the form of a suspension in a fluid.

"Two grams of specimen leaves are chopped into pieces approximately four millimeters in diameter and placed in 30 milliliters of a solution consisting of equal parts by volume of acetone, ether and isopropyl alcohol. The mixture is boiled by immersing the container in water heated to approximately 95 degrees C. by an electric hot plate. The solution is highly flammable and must not be exposed to an open flame. Boiling is continued until the leaf particles are fully bleached, which usually takes about 30 minutes. The solvent will then have been reduced to approximately half of the initial volume. It is cooled to room temperature and stored for analysis.

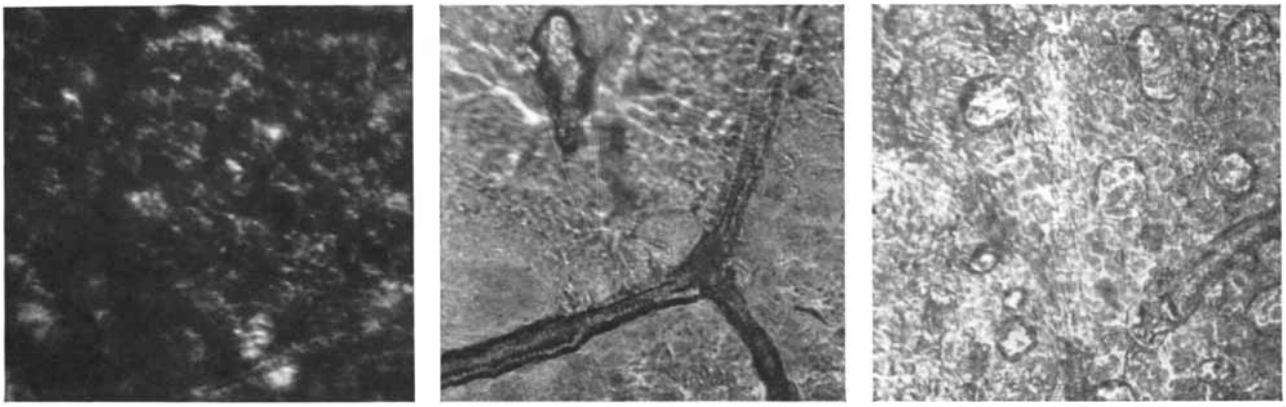
"The chromatographic column consists of a strip of Whatman No. 1 filter paper about two centimeters wide and 24 centimeters long. The paper is suspended in a test tube 38 millimeters in diameter and 300 millimeters long and is supported by a wire hook from the center of a rubber stopper. Leaf extract is applied to a spot in the center of the paper about two centimeters from the bottom edge at a rate of one drop at a time until 20 drops have been applied to the same spot. Each drop is allowed to dry before the next is applied.

"A solvent consisting of 95 parts of petroleum ether (by volume) to five parts of acetone is now placed in the test tube to a depth of 15 millimeters. The paper strip is placed in the test tube so that the lower edge dips into the solvent. Make sure that the spot does not touch the solution. Solvent flows up the strip by capillary attraction and carries the plant pigments along, each kind of pigment moving at a characteristic rate. By the time the migrating front of the fluid reaches the top of the paper the several pigments have separated and appear as an array of colored bands spaced at intervals along the strip. The carotenes and xanthophylls are yellow, beta-chlorophyll is yellow-green and alpha-chlorophyll is blue-green. From top to bottom the order of the pigments on the strip is carotene, xanthophyll, beta-chlorophyll and alpha-chlorophyll.

"The chromatograms of all plants, treated and untreated, displayed traces of all pigments in varying amounts. The concentration of carotenes remained substantially constant. The chlorophylls usually varied inversely with growth rate. This result was not surprising, because leaves that exhibited the most growth appeared lightest in color.

"The explanation of the inverse relation between leaf color and growth rate became more apparent when portions of the leaves were examined under the microscope. Under sufficient magnification clear spaces could be seen between the chloroplasts of treated plants. The chloroplasts are the oval bodies outside the nucleus that contain the chlorophylls. Photomicrographs made at a magnification of 750 diameters show a striking difference in density between treated and control plants [*see top illustration on next page*].

"Even more striking is the effect of the extracts on the shape of cells in the leaf stems and the stalks of the plants. Nor-



***Sections of bean leaves: normal* (left), *treated with GA-3* (center), *treated with bean extract* (right)**

mally these cells are more or less rectangular. Those treated with gibberellic acid and similar substances, however, are elongated as much as 10 or more diameters [*see illustration below*].

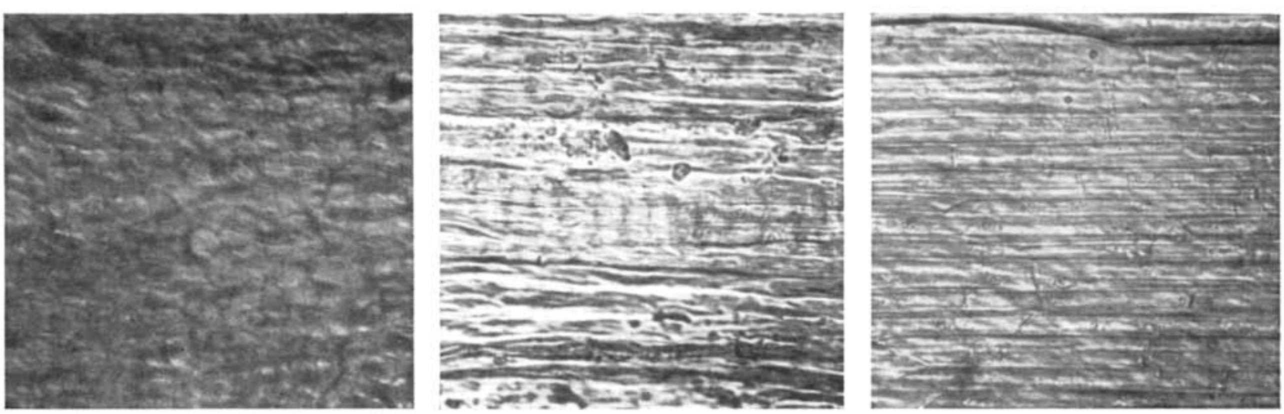
"Temporary microscope slides of plant sections are easy to prepare. In the case of stalks and leaf stems a thin sliver is cut from the growth with a razor blade. The specimen is then removed by similarly cutting a thin slice from the exposed interior. The resulting sliver is placed flat on a clean microscope slide, surrounded with a thin ring of petroleum jelly and enclosed with a cover glass that adheres to the petroleum jelly. Photomicrographs were made by placing the front surface of the 135-millimeter, *f*/4.5 lens of a Speed Graphic camera at the eye position of the microscope and exposing Polaroid film (Type 55 P/N) at full aperture for half a second. The substage of the microscope was lighted by a 12-volt bulb of the type used in automobile spotlights. The total magnification of the reproduced images is approximately 3,500 diameters.

"As the final portion of the experiment I subjected the seed extracts that were prepared in the form of fluids to chromatographic analysis to learn if their behavior on the chromatographic column resembled the known characteristics of the gibberellins. A spot of each extract was placed on the lower portion of the filter paper, as in the analysis of leaf pigments. The bottom edge of the paper was then dipped into a solvent, in the test tube, consisting of 25 parts (by volume) of tert-amyl alcohol, 25 parts of n-butyl alcohol, 25 parts of acetone, 10 parts of concentrated ammonia solution and 15 parts of distilled water. The solvent was allowed to act for three hours.

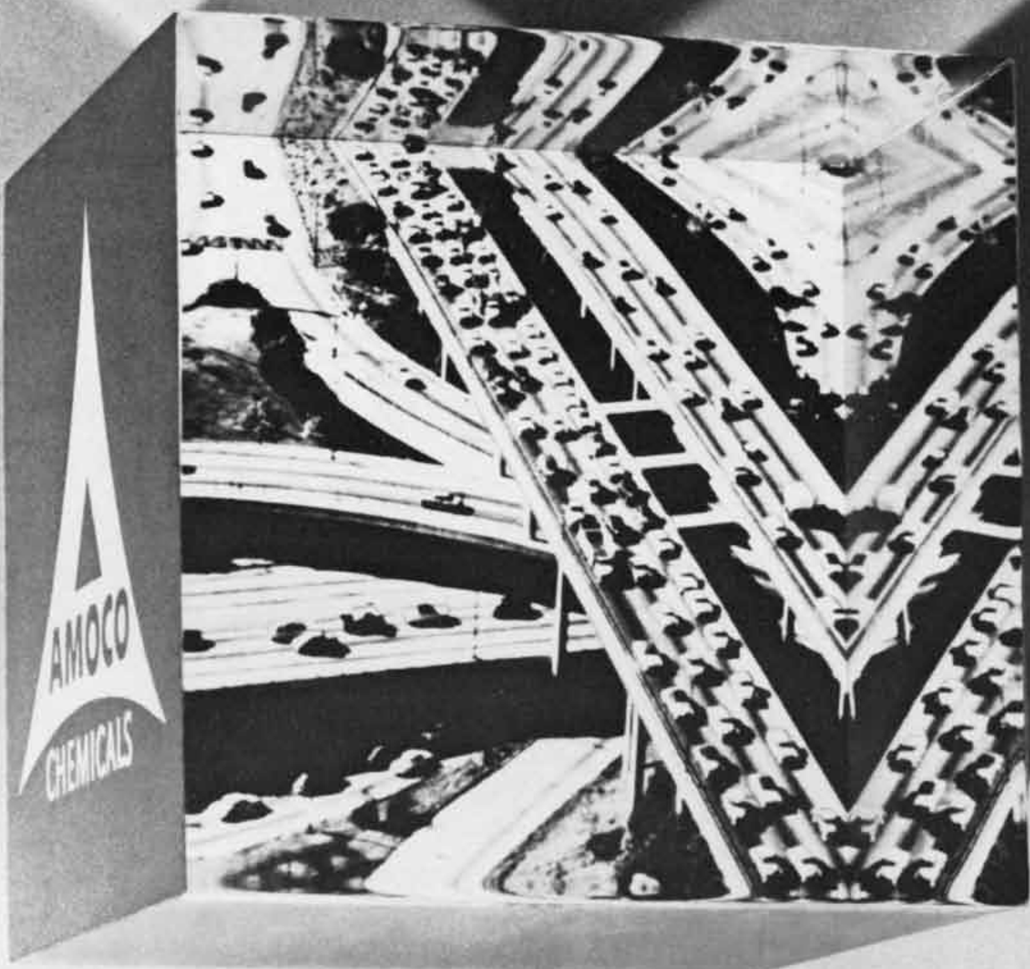
"When the paper is removed from the test tube, it appears uniformly white. The bands of adsorbed growth-accelerating substances must be developed by spraying the paper strip with a solution consisting of one part of potassium permanganate to 200 parts of distilled water (by weight). After reacting chemically with the potassium permanganate the adsorbed substances emerge as an array of brown bands.

"Each substance migrates up the paper at a unique rate that bears a constant ratio to the rate at which the solvent migrates. The ratio is known as the 'R_f' value of the substance and is calculated by dividing the distance between the starting point of the specimen and the final position reached by the solvent by the distance between the initial and the final position of the specimen. The R_f values of a number of gibberellins have been determined. These include GA-2 (.65), GA-3 (.40), GA-4 (.57), GA-5 (.49), GA-6 (.42), GA-7 (.57) and GA-8 (.25). The R_f values that I measured for the extract of pinto beans were .10, .25, .35, .40, .50, .56, .65 and .77. These results suggest that GA-2, GA-3, GA-4, GA-5, GA-7 and GA-8, or closely similar substances, may be present in the extract of pinto beans. The higher and lower values may possibly be explained by the breakdown products of gibberellins or by unknown gibberellins.

"Many of the treated plants eventually wilted, possibly because the rate of food production of the plant did not match the accelerated growth rate. This guess gains some support from the fact that wilting was observed only among plants that grew fastest. Another conclusion that can be drawn from the experiments is that gibberellins can be found in most plants and constitute a widespread group of growth-promoting substances."



***Stem cells of pinto bean: normal* (left), *treated with GA-3* (center), *treated with bean extract* (right)**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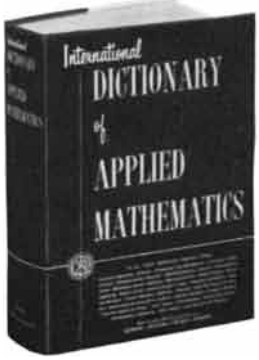

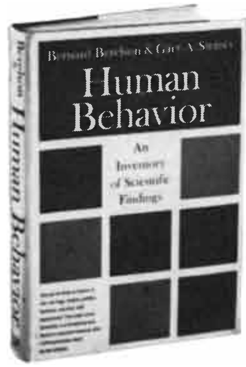

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BOOKS

Culture history v. cultural process: a debate in American archaeology

by Kent V. Flannery

AN INTRODUCTION TO AMERICAN ARCHAEOLOGY, VOLUME I: NORTH AND MIDDLE AMERICA, by Gordon R. Willey. Prentice-Hall, Inc. ($16.95).

A dominant characteristic of American archaeology has been its long history of reaction to American ethnology. When ethnology was little more than the collecting of spears, baskets and headdresses from the Indians, archaelogy was little more than recovery of artifacts. When ethnology increased its attention to community structure, archaeology responded with studies of settlement pattern—an approach in which Gordon Willey was an innovator. Publication of works by Julian H. Steward and others on "cultural ecology" was answered by great archaeological emphasis on "the ecological approach." When the concept of cultural evolution emerged triumphant after years of suppression, archaeology showed great interest in evolutionary sequences and in the classification of "stages" in the human career. The interaction of these two disciplines has been increased by the fact that in the U.S. both are housed in departments of anthropology; as Willey remarked some 10 years ago, "American archaeology is anthropology or it is nothing."

And now, in 1967, Willey—Bowditch Professor of Mexican and Central American Archaeology and Ethnology at Harvard University—has written a monumental synthesis of New World prehistory. There is nothing like it. Recently we have had several edited volumes on the New World with contributions by regional specialists, but this book is written cover to cover by one man. Thus the inevitable lack of firsthand familiarity with certain areas is partially offset by the advantage of having one consistent approach and writing style throughout. Although aimed at the student, the book's costly format almost prices it out of the student range. It is a centerpiece for the coffee table of the archaeological fraternity, at least until an inexpensive paperback edition can be produced.

Willey's archaeological career is reflected in monographs and articles on every major land mass of the New World, from the region of the Woodland culture in the U.S. Northeast to the Maya area, the shell mounds of Panama and the coastal border of the Andean civilization. He is a perennial favorite who for a variety of reasons has never come under attack. One reason is his avoidance of any one polarized theoretical position; the other is his adaptability in the face of continual change. While other members of the establishment have clenched their fists and gritted their teeth when their formerly useful theories dropped from favor, Willey has shown no such hostility; younger archaeologists sense he would rather join them than lick them. And he is always free to join them as long as he maintains no vested interest in any comprehensive theory that needs defending.

This book, well organized from the primary literature and from constant conversations with Willey's colleagues, is no exception. It is unlikely to stir up controversy except where Willey commits himself to one of a series of possible theories proposed by others—for example, siding with Emil W. Haury rather than Charles C. Di Peso on the interpretation of the U.S. Southwest, or with Henry B. Collins rather than Richard S. MacNeish on the American Arctic. It is not Willey's aim to intrude his own theories into the synthesis. Indeed, he tells us that he is "not demonstrating or championing any one process, theory or kind of explanation as a key to a comprehensive understanding of what went on in prehistoric America." Clearly Willey feels that it would be misleading to do more than present the student with the facts as most of his colleagues agree on them in 1967. Hence "the intent of this book is history—an introductory culture history of pre-Columbian America."

This statement by Willey makes it appropriate to consider one of the current theoretical debates in American archaeology: the question of whether archaeology should be the study of culture history or the study of cultural process. In view of this debate it is interesting to note that in practically the same paragraph Willey can brand his book "culture history" and yet argue that he is "not championing any one point of view."

Perhaps 60 percent of all currently ambulatory American archaeologists are concerned primarily with culture history; this includes most of the establishment and not a few of the younger generation. Another 10 percent, both young and old, belong to what might be called the "process school." Between these two extremes lies a substantial group of archaeologists who aim their fire freely at both history and process. And although Willey himself belongs to this group, his *Introduction to American Archaeology* also constitutes a massive restatement of the accomplishments of the culture-history school.

Most culture historians use a theoretical framework that has been described as "normative" (the term was coined by an ethnologist and recently restressed by an archaeologist). That is, they treat culture as a body of shared ideas, values and beliefs—the "norms" of a human group. Members of a given culture are committed to these norms in different degrees—the norm is really at the middle of a bell-shaped curve of opinions on how to behave. Prehistoric artifacts are viewed as products of these shared ideas, and they too have a "range of variation" that takes the form of a bell-shaped curve.

In the normative framework cultures change as the shared ideas, values and beliefs change. Change may be temporal (as the ideas alter with time) or geographic (as one moves away from the center of a particular culture area, commitment to certain norms lessens and commitment to others increases). Hence culture historians have always been concerned with constructing "time-space grids"—great charts whose columns show variation through the centuries. Some have focused an incredible amount of attention on refining and detailing these grids; others have been concerned with discovering "the Indian behind the artifact"—reconstructing the "shared idea" or "mental template" that served as a model for the maker of the tool.

While recognizing the usefulness of this framework for classification, the process school argues that it is unsuitable for explaining culture-change situations. Members of the process school view human behavior as a point of overlap (or "articulation") between a vast number of *systems*, each of which encompasses both cultural and noncultural phenomena—often much more of the latter. An Indian group, for example, may participate in a system in which maize is grown on a river floodplain that is slowly being eroded, causing the zone of the best farmland to move upstream. Simultaneously it may participate in a system involving a wild rabbit population whose density fluctuates in a 10-year cycle because of predators or disease. It may also participate in a system of exchange with an Indian group occupying a different kind of area, from which it receives subsistence products at certain predetermined times of the year; and so on. All these systems compete for the time and energy of the individual Indian; the maintenance of his way of life depends on an equilibrium among systems. Culture change comes about through minor variations in one or more systems, which grow, displace or reinforce others and reach equilibrium on a different plane.

The strategy of the process school is therefore to isolate each system and study it as a separate variable. The ultimate goal, of course, is reconstruction of the entire pattern of articulation, along with all related systems, but such complex analysis has so far proved beyond the powers of the process theorists. Thus far their efforts have not produced grand syntheses such as Willey's but only small-scale descriptions of the detailed workings of a single system. By these methods, however, they hope to explain, rather than merely describe, variations in prehistoric human behavior.

So far the most influential (and controversial) member of the process school has been Lewis R. Binford of the University of California at Los Angeles, and it is interesting to note that Binford's name is confined to a single footnote on the last page of Willey's text. It is Binford's contention that culture historians are at times stopped short of "an explanatory level of analysis" by the normative framework in which they construct their classifications. Efforts to reconstruct the "shared ideas" behind artifact populations cannot go beyond what Binford calls "paleopsychology"—they cannot cope with systemic change. And where Willey says that "archaeology frequently treats more effectively of man in his relationships to his natural environment than of other aspects of culture," Binford would protest that most culture historians have dealt poorly with these very relationships; their model of "norms," which are "inside" culture, and environment, which is "outside," makes it impossible to deal with the countless systems in which man participates, none of which actually reflect a dichotomy between culture and nature. The concept of culture as a "superorganic" phenomenon, helpful for some analytical purposes, is of little utility to the process school.

As a convenient example of the difference in the two approaches, let us examine three different ways in which American archaeologists have treated what they call "diffusion"—the geographic spread of cultural elements. It was once common to interpret the spread of such elements by actual migrations of prehistoric peoples (a view, still common in Near Eastern archaeology, that might be called the "Old Testament effect"). The culture historians attacked this position with arguments that it was not necessary for actual people to travel—just "ideas." In other words, the norms of one culture might be transmitted to another culture over long distances, causing a change in artifact styles, house types and so on. A whole terminology was worked out for this situation by the culture historians: they described cultural "traits" that had a "center of origin" from which they spread outward along "diffusion routes." Along the way they passed through "cultural filters" that screened out certain traits and let others pass through; the mechanics of this process were seen as the "acceptance" or "rejection" of new traits on the part of the group through whose filter they were diffusing. At great distances from the center of origin the traits were present only in attenuated form, having been squeezed through so many filters that they were almost limp.

Since process theorists do not treat a given tool (or "trait") as the end product of a given group's "ideas" about what a tool should look like but rather as one component of a system that also includes many noncultural components, they treat diffusion in different ways. The process theorist is not ultimately concerned with "the Indian behind the artifact" but rather with the system behind both the Indian and the artifact: what other components does the system have, what energy source keeps it going, what mechanisms regulate it and so on? Often the first step is an attempt to discover the role of the trait or implement by determining what it is functionally associated with; some process theorists have run extensive linear-regression analyses or multivariant factor analyses in order to pick up clusters of elements that vary with each other in "nonrandom" ways. When such clusterings occur, the analyst postulates a system—tools *X*, *Y* and *Z* are variables dependent on one another, constituting a functional tool kit that varies nonrandomly with some aspect of the environment, such as fish, wild cereal grains, white-tailed deer and so on. By definition change in one part of a system produces change in other parts; hence the process theorists cannot view artifacts *X*, *Y* and *Z* as products of cultural norms, to be accepted or rejected freely at way stations along diffusion routes. When such elements spread, it is because the systems of which they are a part have spread—often at the expense of other systems.

Thus the archaeologist James Deetz recently presented evidence that the spread of a series of pottery designs on the Great Plains reflected not the "acceptance" of new designs by neighboring groups but a breakdown of the matrilocal residence pattern of a society where the women were potters. Designs subconsciously selected by the women (and passed on to their daughters) ceased to be restricted to a given village when the matrilocal pattern collapsed and married daughters were no longer bound to reside in their mothers' villages. In this case, although each potter obviously did have a "mental template" in her mind when she made the pot, this did not "explain" the change. That spread of design could only be understood in terms of a system in which designs, containers and certain female descent groups were nonrandomly related components. The members of the process school maintain that this is a more useful explanatory framework, but even they realize that it is only a temporary approach. They are becoming increasingly aware that today's human geographers have ways of studying diffusion that are far more sophisticated and quantitative than anything used by contemporary archaeologists.

One other example of the difference in approach between the culture historian and the process theorist is the way each treats the use of "ethnographic analogy" in archaeological interpretation. The culture historian proposes to analyze and describe a prehistoric be-

havior pattern, then search the ethnographic literature for what seems to be analogous behavior in a known ethnic group. If the analogy seems close enough, he may propose that the prehistoric behavior served the same purpose as its analogue and then use ethnographic data to "put flesh on the archaeological skeleton."

The process theorist proposes a different procedure. Using the analogous ethnic group, he constructs a behavioral model to "predict" the pattern of archaeological debris left by such a group. This model is then tested against the actual archaeological traces of the prehistoric culture, with the result that a third body of data emerges, namely the differences between the *observed* and the *expected* archaeological pattern. These differences are in some ways analogous to the "residuals" left when the principal factors in a factor analysis have been run, and they may constitute unexpectedly critical data. When the archaeologist sets himself the task of *explaining* the differences between the observed archaeological pattern and the pattern predicted by the ethnographic model, he may come up with process data not obtained through the use of analogy alone.

Willey is certainly alert to the current debate, and although he summarizes the New World in a predominantly culture-history framework, he concludes Volume I with a discussion of the hopes and promises of the process school. These he leaves for the future: "I shall be less concerned with process or a search for cultural 'laws,' " he says, "than with at times attempting to explain why certain cultural traditions developed, or failed to develop." Certainly the process school would argue that he cannot explain, within a culture-history framework, why such traditions developed or failed to develop; yet, as he explicitly states, explanation is not the purpose of this volume but rather history.

Let us hope, as Willey seems to, that there is a place in American archaeology for both approaches. Certainly we can use both the historical synthesis and the detailed analysis of single processes. By no stretch of the imagination do all process theorists propose to reject history, because it is only in the unfolding of long sequences that some processes become visible.

In fact, what does the difference between the two schools really amount to? In terms of the philosophy of science, I believe the process approach results in moving "decisions" about cultural behavior even farther away from the individual. It is part of a trend toward determinism that the culture historians began.

It was once common to hear human history explained in terms of "turning points," of crucial decisions made by "great men." This view proved unacceptable to the culture historians, with their normative framework of shared ideas, values and beliefs. They argued convincingly that this body of shared norms determined the course of history —not the individual, who was simply a product of his culture. Possibly the most devastating critique of the individual as decision-maker was due to Leslie A. White, who in one brilliant polemic concluded that the course of Egyptian history and monotheism would have been the same "even had Ikhnaton been a bag of sand."

Now the process school would like to move crucial decisions still farther from the individual by arguing that systems, once set in motion, are self-regulating to the point where they do not even necessarily allow rejection or acceptance of new traits by a culture. Once a system has moved in a certain direction, it automatically sets up the limited range of possible moves it can make at the next critical turning point. This view is not original with process-school archaeologists—it is borrowed from Ludwig von Bertalanffy's framework for the developing embryo, where systems trigger behavior at critical junctures and, once they have done so, cannot return to their original pattern. The process school argues that there are systems so basic in nature that they can be seen operating in virtually every field—prehistory not excepted. Culture is about as powerless to divert these systems as the individual is to change his culture.

Obviously individuals *do* make decisions, but evidence of these individual decisions cannot be recovered by archaeologists. Accordingly it is more useful for the archaeologist to study and understand the system, whose behavior is detectable over and over again. Obviously this approach is too deterministic for some purposes, but for others it is of great theoretical value.

But then if both historical and processual approaches are useful, why should there be a debate at all? I believe the debate exists because of two basically different attitudes toward science.

The previous generation of archaeologists, who did mostly culture history but also laid the foundations for the process school, were often deathly afraid of being wrong. Many of them felt (and many still feel) that if we will only wait until all the facts are in they will speak for themselves. They spoke in awe of the incompleteness of the archaeological record and of the irresponsibility of speculating on scanty data. Somehow they seemed to feel that if they could get together a few more potsherds, a few more projectile points or a few more architectural details, their conclusions would be unshakable. There has not been, however, any convincing correlation between the quantities of data they amassed and the accuracy of their conclusions.

The process theorists assume that "truth" is just the best current hypothesis, and that *whatever* they believe now will ultimately be proved wrong, either within their lifetime or afterward. Their "theories" are not like children to them, and they suffer less trauma when the theories prove "wrong." Their concern is with presenting developmental models to be tested in the field, and they have noted no consistent relationship between the usefulness of a given model and the absolute quantity of data on which it is based. To be useful a model need only organize a body of disorganized data in such a way that hypotheses can conveniently be tested, accepted, modified or rejected. Thus the process school will continue to present model after model on the basis of returns from the first few precincts, and at least some of the culture historians will continue to accuse them of being "hasty," "premature" and "irresponsible." And the issue will be settled years from now by another generation that will probably not belong to either school.

Willey's synthesis sums up nearly 100 years of American archaeology, and it comes at the start of one of the most exciting archaeological eras yet begun. My prediction for the next decade is that we shall see general systems theory, game theory and locational analysis all applied successfully to American archaeology in spite of the loudest mutterings of the establishment. I also predict that, in spite of his decision to concentrate his own efforts on producing reliable culture history, we shall hear all these subversive approaches applauded by Gordon Willey.

Short Reviews

THE ENCYCLOPEDIA OF PHILOSOPHY. Paul Edwards, editor in chief. The Macmillan Company and The Free Press ($219.50). When the *philosophes* of France assembled their great *Encyclopèdie,* it undertook to encompass at least the beginnings of all knowledge. Accordingly its marvelous prints display all the engines and artifacts of the age.

The Encyclopedia of Philosophy is the dwindled and specialized work of our modern philosophers, some 500 of them, mainly American, with a company of British allies and a sprinkling from the corners of the earth. Most of the authors are professors of philosophy by vocation, but a few are distinguished amateurs such as Arthur Koestler and Martin Gardner. All the great universities house contributors, IBM claims one on the payroll, but what the expert on Arabic Aristotelianism does to earn his pay from the Pentagon is not made clear.

There are some 1,500 articles in the 4,000 large pages of these eight volumes. About 900 of the articles are listed by the names of individual thinkers from the history of philosophy up to the present day. Indeed, several of the contributors are the subjects of articles. The affinities of the contemporary profession are clear from the apportionment of space. Rousseau and Socrates get about five pages each, Whitehead 10, Hobbes and Hume 15 each. To dispose of the protean thought of Lord Russell takes 20 pages and three authors, who divide their task by topics.

The central nonbiographical burden of the work is the analysis of language. Witty and acute, if somehow picayune, article after article discusses just what one might mean under headings such as "Any and All," "If" and "Why." There are many excellent pieces on the foundations of mathematics and of logic, with a simple and clearheaded article on infinity and a deep one on Zeno and his paradoxes particularly catching the eye. More technical matters are also handled carefully, such as Craig's theorem on the completeness of axioms. Another large theme is theology, treated with a catholic interest in all eras and climes. There is, for example, an entry "Popular Arguments for the Existence of God," and a quite earnest piece on reincarnation.

The word is the beginning and the end of this work; there are amazingly few pictures, maps, diagrams or even formulas. Two articles on logic display a major fraction of the diagrams in the volumes. All this verbal argument lends a curiously vicarious quality to these books. The article on aesthetics is 50 pages mostly about visual impressions of beauty and has zero illustrations. There is evidence that this second-handedness leads to credulousness; extrasensory perception is regarded as a fact as plain as evolution, and even the ruined mind of Wilhelm Reich, the promoter of orgone, is not entirely dismissed. There are good pieces on scientific issues such as action at a distance, although they are only beginnings, and an outstanding one on quantum mechanics. A monument to its author, the late Norwood Russell Hanson, it brings to the reader something of the power that physicists know resides in that subtle and surprising theory.

Students of philosophy, in both the broad and the narrow sense, will find the book valuable, although perhaps not for a long time to come. It will nonetheless remain an honest and able report of a time of fashionable modesty. And it is impossible not to admire one volume whose spine displays these alphabetized contents: *Cabala to Entropy.*

FOUR-LEGGED AUSTRALIANS, by Bernhard Grzimek. Hill and Wang ($7.95). This great conservationist and zooman, defender of Africa's Serengeti Plains, travels to Australia to report on what the two-legged inhabitants have preserved of their unique fauna. Market-hunting for kangaroo meat is a busy trade, but it does not in fact appear to threaten any species of kangaroo with the final solution; rather it is the loss of specialized habitat to road, ranch and suburb that brings extinction to many species. There are three genera of marsupials that have learned to behave like "animated kites": the pygmy glider, the sugar glider and the greater glider. They represent separate inventions, that is, each resembles the others less than it does some nonflying marsupial. The photographs of them shown here are endearing. There is also a shot—perhaps unique—of a wild dingo running free. His marsupial analogue, the Tasmanian wolf, "has reached the point of no return and the best of intentions will not save it now." Here Grzimek becomes angry. If this largest carnivorous marsupial dwelt somewhere nearer the centers of culture, he feels, "there would be a tremendous to-do"; in Tasmania it was attacked by trap and gun as a sheep-killer. Roads and people spoiled its natural cover, just as they did the cover of the wolf in England and Scotland. No Tasmanian wolf has been captured since 1933, although as recently as 1961 a couple of fishermen seem to have encountered one. There are probably a few on the wild western shore of the island, but they are doomed unless a range is set aside for them and stocked with game.

The case of the rabbit (and the less familiar and less dramatic case of the camel) present man-caused increases in the Australian fauna. There is a fine chapter on the rabbit, complete with the story of the artificial rabbit plague and the rabbit flea. It is a striking fact that the rabbit was a native of Spain and Morocco; everywhere else in the Old World he has been introduced. Rabbits came to England in the 12th century. There they paradoxically became a thoroughgoing pest once they were subjected to the steel trap; the trap all too frequently caught their predators, and the fast-breeding rabbits gained thereby. The control of rabbits by myxomatosis was a special case; the Australian rabbits are gaining an immunity even to the new strains of the pathogen. Airplane-sown poison bait and good fencing has controlled Tasmanian rabbits even though they have never been attacked with myxomatosis. Forty million wild rabbits are killed for skin and meat in Australia each year; there are plenty more on the range.

Grzimek comes away enthralled, saddened and hopeful: there are two-legged Australians who begin to care and scientists who are finding out how to make their concern count. If they, and not the heedless developers, succeed, the year 2000 will find Australian animals still visible outside of the postage stamps where today the Tasmanian wolf is memorialized. One hopes Grzimek is right; his book will help.

THE ORIGIN OF GENETICS: A MENDEL SOURCE BOOK, edited by Curt Stern and Eva R. Sherwood. W. H. Freeman and Company ($2.25). HERITAGE FROM MENDEL, edited by R. Alexander Brink with the assistance of E. Derek Styles. The University of Wisconsin Press ($2.95). G. MENDEL MEMORIAL SYMPOSIUM 1865–1965, edited by Milan Sosna and B. Sekla. Verlag Werner Flach (31 West German marks). Gregor Mendel planted peas in the experimental garden of the Augustinian monastery at Brno in eight cycles; his biggest plot was about seven meters by 35. He chose peas because he was studying traits of hybrids under conditions that allowed artificial crossing. He taught experimental physics and natural history at the Brno secondary school, for which he had a good university training in mathematics and in physical experiment. He published his work—for which he had found a remarkable explanation, particulate heredity and chance segregation—in a single paper in the journal of the Brünn Research Society. He published a second paper four years later, the year after he had been elected abbot. Then the choice of materials was not so happy; the flower he used is often parthenogenetic, and shows no response to an apparent cross. His work was repeated unknowingly by two other experimenters in the years before 1900, and in that year Carl Correns of Germany and Hugo de Vries of the Netherlands independently found his results and his paper, and modern genetics began its triumphs. (The paper was by no means unknown in its time but was simply misunderstood and neglected by those who were aware of it.)

The first of these books is a reprint volume, bringing together the papers of Mendel, his letters to a contemporary scientist, the excerpt from the contemporary treatise that misunderstood his work, the papers of the rediscoverers and a wonderful controversy. The argument was raised by the statistician R. A. Fisher, who demonstrated that Mendel's data showed improbably small fluctuations and strongly suggested that he altered them to make his logical point. It would be a great joke on the world if that were true; however, Sewall Wright makes it pretty clear that what happened, and still happens in most genetic papers, was a slight unconscious selection in the course of the many subtle decisions one must make, in classifying and counting living materials, that tends to eliminate extreme cases.

The second work is a first-rate symposium celebrating genetics. The main sections, each including papers at a general level by four or five leading geneticists, center on the gene concept, the molecular basis of heredity, the action of the gene in the cell, the genetics of populations; finally, there is a fascinating set of papers on applied genetics and its history, plus papers by the always original Tracy M. Sonneborn and H. J. Muller. Indeed, the last word is had by the irrepressible and even eccentric Muller, who seeks to define life itself as a material with the potential for evolution by inherited variation and natural selection. This is perhaps Muller's last piece of writing (he died earlier this year), and it is well worth reading.

The third book contains the proceedings, both historical and biological, of a fine symposium held at Brno itself on the Mendel centennial. It is particularly valuable as a record of the present-day Russian estimate of Mendel and his work—an estimate much like the non-Russian one.

RADIO ASTRONOMY, by John D. Kraus, with a chapter by Martti E. Tiuri. McGraw-Hill Book Company ($13.75). Thirty years ago the author, then a young radio engineer working with astronomers at the University of Michigan, tried without success to detect radiation from the sun at a wavelength of one centimeter. For 20 years he worked in the fundamentals of radio, becoming an

outstanding designer of antennas and inventor of the helical microwave antenna. For a dozen years now he has been caught by radio astronomy; he is the designer of the Ohio State University radio telescope, a structure with a fixed vertical parabola looking at a tilting plane mirror across three acres of a ground plane defined by wires, and he directs its work. This is his text, aimed at seniors or early graduate students, and it is patently a useful, up-to-date, clear and pedagogically often brilliant introduction to the subject. It is plainly the book of an understanding engineer; about two-thirds of the text is devoted to fundamentals such as the elements of radiative transfer, polarization by Stokes parameters and matrixes, the theory of propagation in an ionized medium, the response of antennas and the many ingenious designs for squeezing signal and resolution out of noise and small scale. A chapter on receivers is done by a guest expert (Tiuri) at the level of block diagrams and noise-figure analysis. A good general chapter on coordinates and the usages of astronomy begins the work, and a lively overview of the appearance, nature and mechanisms of radio sources from the sun to quasars ends it—almost. The fact that it is a working text is made visible by the appendixes: the famous 3C list of radio sources is here, with three or four other useful tabulations; there is a graph for easy conversion between galactic and equatorial coordinates, and another one for correcting catalogued positions for the precession of the equinoxes. This is a book that will sit on the desk of just about every American student of radio astronomy, and the pirating of that splendid page which presents the outlines of 10 extragalactic radio sources on a common linear scale will be epidemic among popular and technical lecturers alike. Only for the theory of the sources and for a deeper astronomy is the text insufficient; there we still have nothing better than the Russian astronomer I. S. Shklovski's now somewhat dated book and the journals.

THE FOUR-COLOR PROBLEM, by Oystein Ore. Academic Press, Inc. ($12). Francis Guthrie was a mathematics student of Augustus de Morgan at University College London in 1852; he dropped out, but first he showed his brother Frederick that "the greatest necessary number of colors to be used in coloring a map so as to avoid identity of color in lineally contiguous districts is four." Frederick Guthrie asked de Morgan to explain the result, and de Morgan wrote William Rowan Hamilton. Thus did this famous problem enter the ken of proof; there seems to be no sure evidence concerning any earlier practical understanding, or concerning Leonhard Euler's reputed prescience in the matter. This monograph is a full and modern review, with all the methods, in their interrelation, that a century of effort has built for the solution of the still-refractory problem. The problem is part of the theory of plane graphs: figures consisting of a set of points in a plane and reasonable curves joining them. The earliest methods of this theory do go back to Euler; his famous formula using the number of faces, edges, vertexes and so on is a pillar of the work. Duality plays a role, as between cube graph and octahedron, and most of the work revolves around ingenious means of relating graphs one to the other, by various operations such as contracting and decomposing. The results are tantalizing. It is known that five colors will suffice for any graph; there is a certain conjecture which in a particular case would imply the truth of the four-color theorem that has been proved for a wide variety of cases but not for the correct one; finally, the best result to date, due to C. E. Winn 30 years ago, demonstrates that any graph that *cannot* be managed with only four colors must have at least 36 vertexes. The proof appears here, and it requires a rather tedious and inelegant enumeration of cases. Professor Ore clearly hopes his review will set a "younger generation" to work. The problem does not seem to be accessible to amateurs; this text is, however, much more self-contained than most modern monographs of mathematics.

PRINCIPLES OF STATISTICS, by M. G. Bulmer. The M.I.T. Press ($7.50). A good college library is likely to give over 10 or 15 feet of shelving to elementary texts on statistics and applied probability. Here is a fresh text that stands out from that host. It achieves its graceful eminence by the breadth and interest of its prose and by the balance of its intellectual design. It never offends by the cookbook presentation of tables, of results and of complicated procedures that the reader is supposed to learn to use without much understanding, nor does it inhibit by the detailed and rigorous pursuit of mathematical results into depths where a user will rarely expect to dive. For instance, it derives the central-limit theorem via moment-generating functions with a few clear pages of text on the implications of the result. The troubles with those distributions whose moments diverge are mentioned, and the more difficult task of using characteristic functions for the most general result is simply alluded to. This expresses very well the interests of most readers of such a text, who are sure to prize meaning above detailed technique, even above the maximizing of generality. Distributions that are not normal come in for genuine attention (although not enough time is spent on the lognormal case), and tests of significance and regression methods are given brief but quite usable explanations. One helpful chapter, informal and critical, is spent on inference, comparing the classical use of confidence intervals with more dubious methods and the modern decision methods out of games theory. This chapter is a convincing and commonsense account, kept at a surprisingly simple mathematical level (and rather skeptical in its conclusions). Some calculus alone will carry any thoughtful reader through this small, clear book, leaving him with both understanding and skill.

ON A PIECE OF CHALK, by Thomas Henry Huxley. Edited and with an introduction and notes by Loren Eiseley. Drawings by Rudolf Freund. Charles Scribner's Sons ($4.95). Next year will mark the centennial of this luminous lecture delivered to the workingmen of Norwich during a meeting of the British Association for the Advancement of Science. Good-humored and yet grave, deep and yet simple, this noble account of the span of geologic time and of the flux of life remains as eloquent as when it was new. In these pages one sees the Foraminifera of the chalk, flint tools from the boulder clay, the *Great Eastern*, and the sea bottom from Ireland to Newfoundland (not, alas, as level as the "prodigious plain" evoked by Huxley but broken by the massif of the Mid-Atlantic Ridge). These are among the dozens of subjects drawn with devotion to the truth and a quiet beauty by Rudolf Freund, in a way that would have pleased Huxley. Professor Eiseley writes a sensitive and elegiac appraisal of the man that would not have pleased him at all, shadowed as it is with pessimism and underlain by the very hunger Huxley thought he had helped men to assuage. A handsome reissue of a piece of prose that has fully earned the epithet "classic."

PRINCIPALS AND PRACTICES OF HEAVY CONSTRUCTION, by Ronald C. Smith. Prentice-Hall, Inc. ($16). There are many city folk who find themselves watching the performance of builders with the attentiveness and pleasure of the concertgoer. This is a work for them. It will serve equally well the many specialists, in blue collar and white, who play some role in the construction of large buildings. It is a sequential and detailed account of what all the people are doing, and what tools of the trade are used, as a great structure grows. The text is most specific in visible and easily grasped techniques; it contains no hint of the sophisticated means of computing stresses or testing subsoil. It tells about setting the batter boards for running the surveyor's wire lines, about blasting caps, about piling of every kind, about staying off high steel in the rain, about checking concrete, about calking aluminum sash. It has photographs of concrete vibrators, roof framing, metal fire doors, terrazzo machines and bulldozers. Practical and matter-of-fact, it is a once-over-closely story of how men build big. It is based on recent Canadian practice, and the final chapter, illustrated with photographs of coke-burning "salamanders," tells how and why to work through a bitterly cold winter. There is no account of the great whirligig cranes for putting up steel structures; perhaps they do not use them in Canada, as they did not in the U.S. until the past decade. This is an agreeable book; its varied lore, detailed in part although always rather superficial, will not suffice to design and build a building, but it is a genre all its own.

The Language of Nature: An Essay in the Philosophy of Science, by David Hawkins. Doubleday & Company, Inc. ($1.95). A paperback reprint of a handsome book that thoughtfully seeks an ambitious unity of viewpoint (mainly on the basis of information theory) toward number and motion, chance and order, acting and knowing—all the way from the axioms of Giuseppe Peano to the theory of personal and social moral choice.

Capital: A Critique of Political Economy, by Karl Marx. Translated from the third German edition by Samuel Moore and Edward Aveling. Edited by Frederick Engels. International Publishers Co., Inc. ($9.95). A three-volume paperback facsimile of an 1887 translation, with the corrections made by a Moscow publisher in 1965.

On the Origin of Species, by Charles Darwin. A facsimile of the first edition, with an introduction by Ernst Mayr. Atheneum Publishers ($3.45). A paperback reprint of the 1964 hardback. Admirable.

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