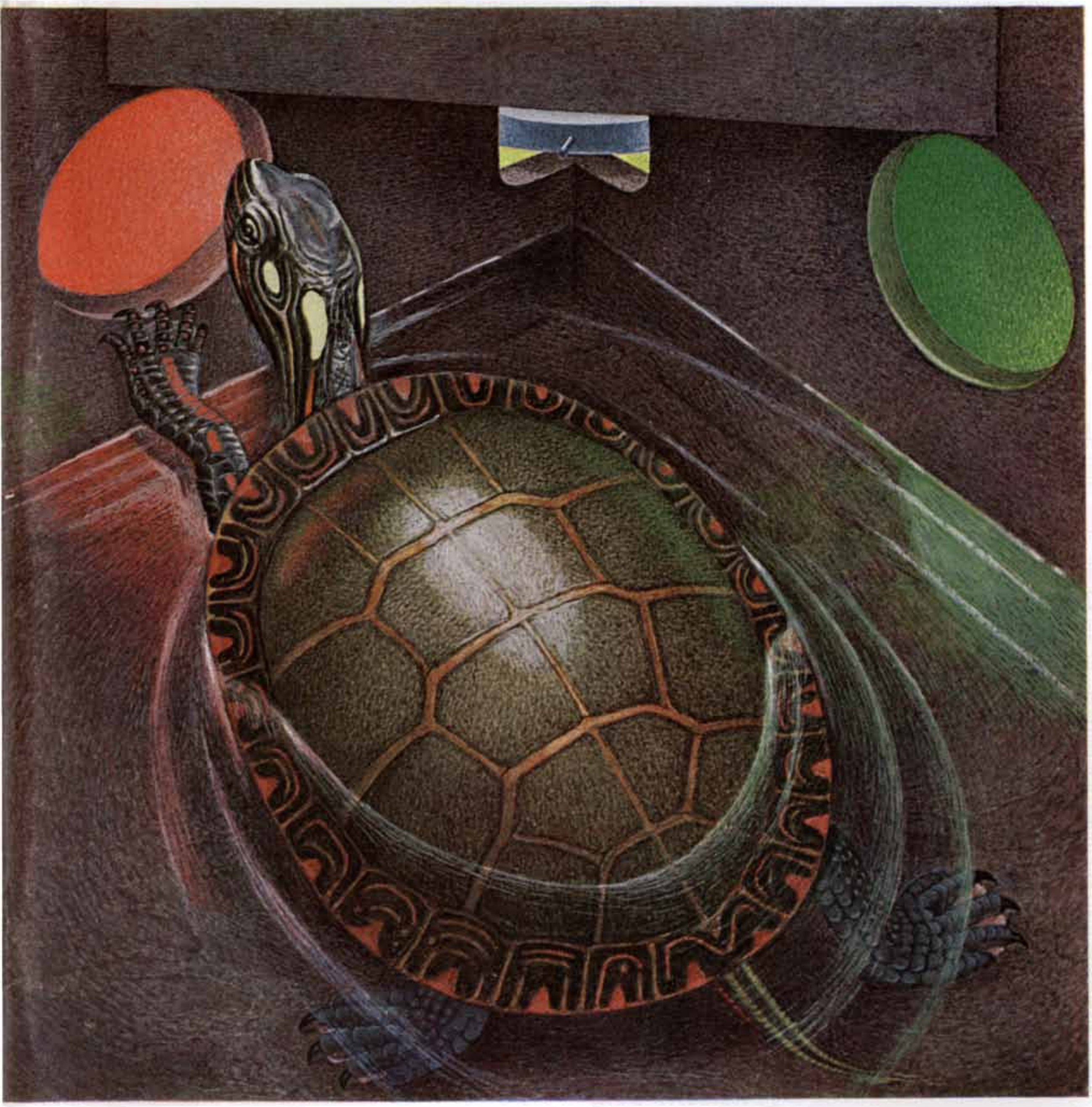
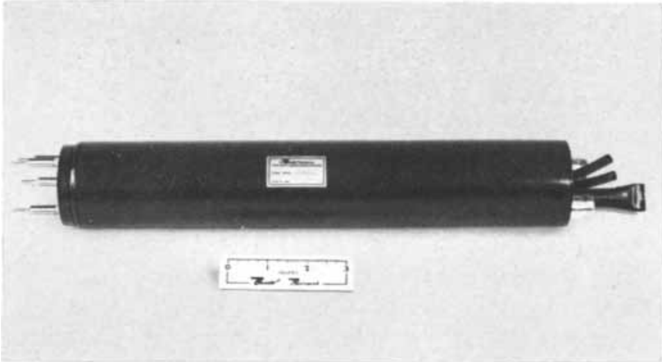
# SCIENTIFIC AMERICAN



EVOLUTION OF INTELLIGENCE

*SIXTY CENTS*

*January 1965*



Liquid cooled Wide Band Power Amplifier Tube and Electromagnet Focusing Structure

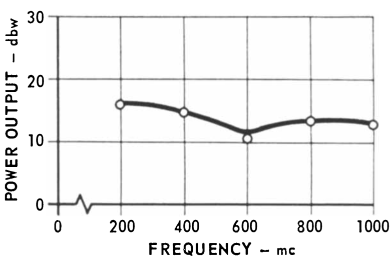
# Wide Band Power Amplifiers with 3:1 Frequency Coverage

Three Traveling-Wave Tube power amplifiers, which have an operating frequency range of better than 3 to 1, have been developed by The Bendix Corporation, Research Laboratories Division. These power amplifiers, a wide band driver tube and two high power tubes, are extremely short and compact, and they are capable of feed-through or "transparent" operation.

The driver tube can produce an output of 10 watts throughout the 200- to 1000-megacycle frequency range at a nominal gain of 15 db. Actual performance data on a recent tube is shown above. The gain, however, can be increased to suit specific needs. A low VSWR is maintained over the entire frequency range from 170 to 1000 megacycles by using coaxial r-f input and output transmission line to helical r-f circuit transitions.

Together, the two high power tubes cover the 100- to 600-megacycle frequency range. Each tube can produce an output in excess of 100 watts at a minimum gain of 13 db.

Because of their rugged constructional features, these tubes offer special advantages where extreme environmental conditions are experienced. Metal-ceramic construction provides great




Saturated power output across the 5:1 frequency range for the TW-143 type driver tube varies only ±3 db

insensitivity to shock, vibration, and temperature conditions. This type of construction also permits high temperature bakeout techniques to be used during vacuum processing which greatly contributes to long life operation.

These tubes utilize a triode electron gun which produces a hollow electron beam and simplifies construction. Either solenoid or permanent magnet focusing structures can be used. The tubes are field changeable, and they can be liquid- or air-cooled.

The broad band operating characteristics of these tubes were achieved by using computer programs to optimize the design. A correlation study has shown that the actual operating characteristics compared very favorably to the computed performance characteristics. These Bendix Computer programs will permit tube designs to be defined for specific performance characteristics throughout a designated frequency range with a minimum of effort.

Bendix Research embraces a wide range of technology including acoustics, nuclear, solid-state physics, quantum electronics, mass spectrometry, photoelectronics, electron beam and tube technology, measurement science, applied mechanics, energy conversion systems, dynamic controls, systems analysis and computation, navigation and guidance, microwaves, digital techniques, data processing and control systems. Motivation: to develop new techniques and hardware for The Bendix Corporation to produce new and better products and complete, integrated, advanced systems for aerospace, defense, industrial, aviation, and automotive applications. Inquiries are invited. We also invite engineers and scientists to discuss career position opportunities with us. An equal opportunity employer. Write Director, Bendix Research Laboratories Division, Southfield, Michigan.

**Research Laboratories Division**

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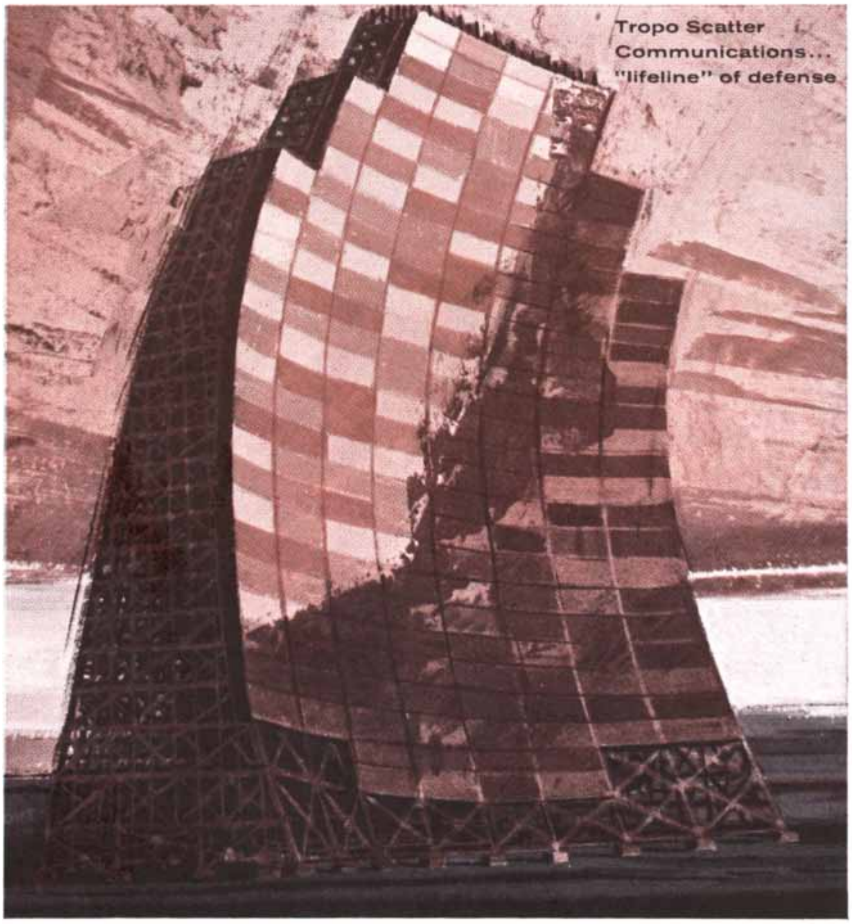


**A-B HOT MOLDED RESISTORS** are available in 1/10, 1/8, 1/4, 1/2, 1, and 2-watt ratings, and in all standard EIA and MIL-R-11 resistance values and tolerances, plus values above and below standard limits.


**TYPE R ADJUSTABLE FIXED RESISTORS,** rated 1/4 watt at 70°C, available in total resistance values from 100 ohms to 2.5 megohms.

## *Radio Engineering Laboratories Tropo Scatter Radio Terminals use Allen-Bradley Hot Molded Resistors because there are none more reliable!*

Radio Engineering Laboratories, Inc. must employ the most reliable components available to achieve the 99.9% reliability required—only 8.7 actual hours outage in a total year round-the-clock operation—in systems under the direction of the Defense Communications Agency. For this reason, extensive use is made of Allen-Bradley fixed and adjustable fixed resistors in REL'S 2600 Series Tropospheric Scatter Radio Terminals.

The Allen-Bradley hot molding process—invented and used exclusively by A-B—produces such precise uniformity from resistor to resistor, from one million to the next, that long term resistor performance can be accurately predicted. Their stable characteristics and conservative ratings have made Allen-Bradley hot molded resistors preferred wherever the ultimate in performance is demanded.

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For more details on the complete line of Allen-Bradley quality electronic components, please write for Publication 6024: Allen-Bradley Co., 1204 S. Third Street, Milwaukee, Wis. 53204. In Canada: Allen-Bradley Canada Ltd., Galt, Ontario.

This illustrates reduction in volume achieved in R E L'S 2600 equipment. Racks at left show exciter and receiver using tube construction. At right are the equivalent solid state gear. Transistorized circuitry and the use of Allen-Bradley Type CB 1/4-watt resistors contributed to this size reduction. A-B pioneered and introduced the 1/4-watt resistor.

A-B QUALITY

**ALLEN-BRADLEY**
QUALITY ELECTRONIC COMPONENTS

Established 1845 **SCIENTIFIC AMERICAN** January 1965 Volume 212 Number 1

ARTICLES



DEPARTMENTS





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## THE COVER

The painting on the cover shows a painted turtle (*Chrysemys picta picta*) making a visual choice in a laboratory at Bryn Mawr College (see "The Evolution of Intelligence," page 92). The turtle is confronted with a pair of translucent Plexiglas panels on which two different colors are projected from behind; it makes a choice by pressing with its head or forefoot against one or the other of the panels. The turtle is rewarded for making a "correct" choice by a bit of fish, which is presented at the window in the far corner impaled on a prong fixed to a rotating wheel. The reward device is triggered automatically, so as to exclude the possibility of the experimenter's presence having any influence on the outcome of the experiment. If the turtle makes an incorrect choice, there is a six-second interval of darkness, after which the correct panel alone is illuminated (a procedure called "guidance") and the turtle is rewarded for responding to it.

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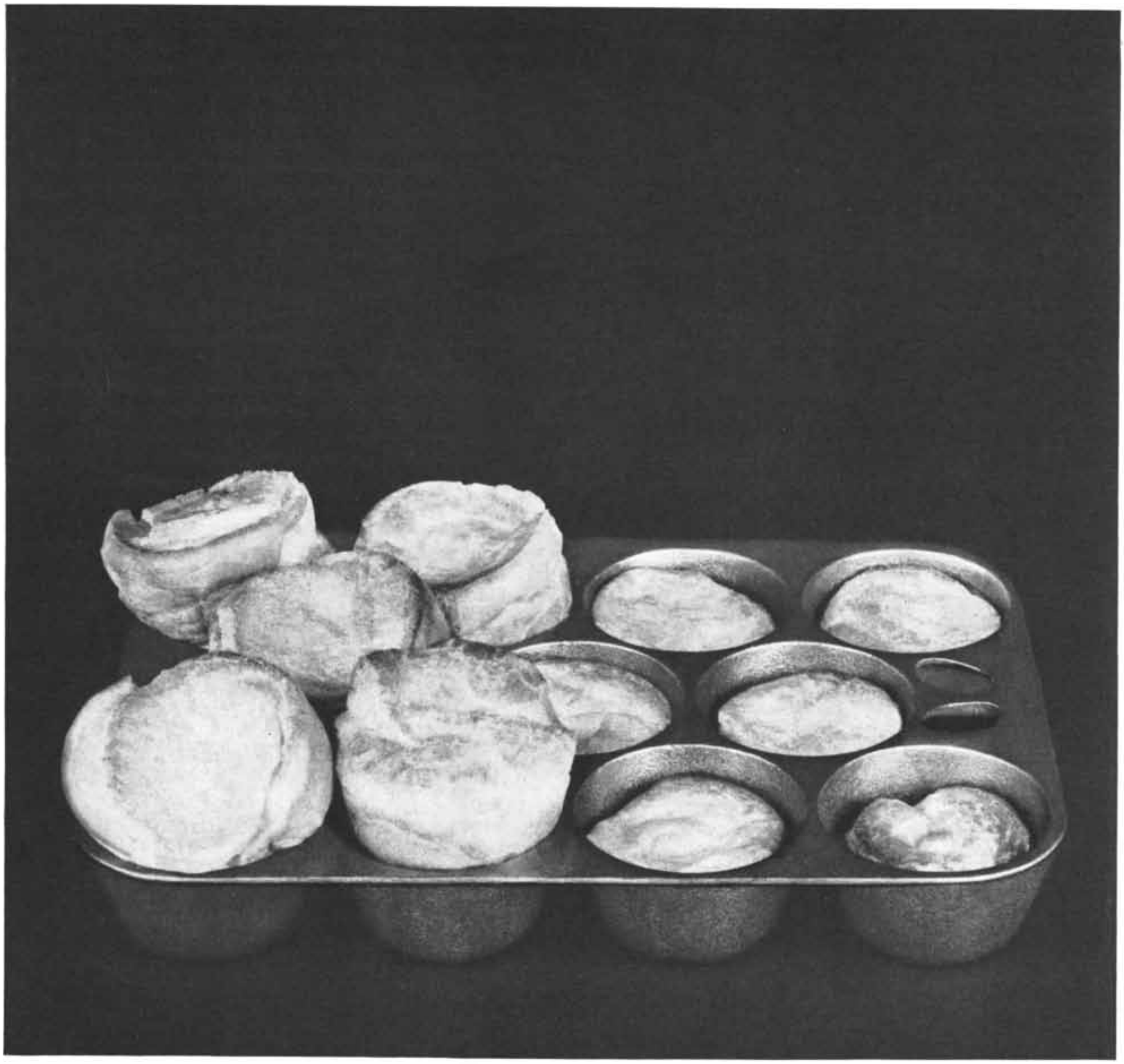

# Explosions in miniature put the pop in popovers

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Or establish the exact amount of heat which will cause a plastic baby's bottle to take its shape—or lose it. Such tabletop explosions, carried out in Perkin-Elmer's Differential Scanning Calorimeter, are opening up a brand new field of low cost analysis. The DSC-1 directly measures and records energy required to effect transitions associated with polymerization, fusion, crystallization and decomposition. Chemists in such varied fields as polymers, insecticides, fats, greases, explosives, organic chemical research and general chemical analysis are finding it invaluable.

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# LETTERS

Sirs:

May I be allowed a reply to A. E. Mirsky's review of my book *Essays of a Humanist* [SCIENTIFIC AMERICAN, October, 1964]? Dr. Mirsky's competence in molecular biology is undisputed and outstanding, but I must question his competence to review books of a general nature. His so-called review of my book is not a review at all but a criticism of a single essay on a single subject. The book includes essays on the history of Darwinism, on a novel approach to the mind-body problem, on the evolution of organization, on education, on conservation, on scientific natural history, on religion, on population, and a key one on humanism. Yet he does not even mention any of these, except to say that much of my Evolutionary Humanism "boils down to eugenics." This statement is untrue and extremely misleading.

Meanwhile he writes that my book is studded with examples of my uncritical attitude, and accuses me of being "uncritical" in general. This seems a somewhat sweeping accusation to bring against a writer of comprehensive scientific works such as *Evolution: the Modern Synthesis* and *Problems of Relative Growth*, of technical reports such as the *Conservation of Wild Life and Natural Habitats in Eastern Africa*, and of a series of papers that established field studies of behavior as being scientifically reputable and helped to lay the foundation of modern ethology.

He may, however, have intended the accusation to apply only to this semipopular work. If so, he has failed to note in the essay on Eugenics my critical discussions of "fitness," of the implications of a high degree of heterozygosity, and of the fact that a small increase in mean mental capacity will bring about a large increase in the numbers of exceptionally well-endowed individuals (by the way, I did not state that "true geniuses" were characterized by exceptionally high I.Q.'s), or of my highly critical strictures of all racist theories and all failures to recognize that genetic and social improvement are inextricably linked.

As regards his strictures on my treatment of the "social problem group" he is ignorant of the facts. The term is a technical one, coined by social workers. Recent studies in our new and more affluent society (for example, by Dr. Dorothy Morgan in *Family Planning*, Autumn, 1964) have shown that improved environmental standards have very little direct effect on the general shiftlessness, the low educational attainments or the size of family of such groups, thus strengthening the presumption of a strong genetic component underlying their behavior; but that domiciliary visiting and patient explanation of family planning can overcome ignorance and can bring about spaced births and small families, and that this will indirectly bring about a better standard of family life and a more hopeful general outlook.

Nor does he mention my critical discussions in other essays of problems such as biological organization, the evolution of improved mental faculties in animals, the effects of explosive but differential population increase, the conservation of varied natural resources, and above all the nature and workings of the evolutionary process and its underlying mechanism, selection.

Indeed, Dr. Mirsky's understanding of this major field of biology seems rather sketchy. If he had digested the outstanding modern works on the subject, such as R. A. Fisher's *Genetical Theory of Natural Selection*, Bernard Rensch's *Evolution above the Species Level*, Ernst Mayr's *Animal Species and Evolution* and George Gaylord Simpson's *Major Features of Evolution*, he would not assert with such assurance that we need much more detailed knowledge of human genetics before attempting any sort of eugenic improvement, even by the modest and voluntary methods suggested by H. J. Muller and myself. No detailed knowledge of genetics was needed by Darwin to deduce that natural selection would produce adaptation and therefore the improvement of each creature in relation to its conditions; and that "this improvement inevitably leads to the gradual advancement of the organization of the greatest number of living beings throughout the world"; or by breeders to produce wonderful improvements in domesticated animals and plants; or by God, nature, the élan vital or any other agency to bring about the astonishing diversification and advance of life during evolution. Nor is it needed by us today to raise the level of desirable genetic potential in man, although of course the process could be materially assisted by deliberate manipulation of the social environment.

Your columns are not the place for detailed discussion of Dr. Mirsky's criticisms. But I must reaffirm that his attack on my views on eugenics is not a review of *Essays of a Humanist*, and I leave it to readers of the book to judge.

JULIAN HUXLEY

London

Sirs:

Sir Julian Huxley says that my "so-called review" of his book of essays is not a review because it is "a criticism of a single essay on a single subject." It is true that the review deals with a single subject: what Huxley calls "evolutionary humanism." It is also true that I said that "much of evolutionary humanism boils down to eugenics." It surely does. Why did I concentrate on evolutionary humanism? I did so because in his preface Huxley says of his book: "But my main aim has been to set forth the facts and principles of evolutionary humanism, and my main hope is that my readers will be convinced of its importance and will take part in the great debate on man's destiny and fundamental beliefs which are now beginning among thinking men and women." Well, in my review I took part in the great debate. I did not refer to essays that, as Huxley said in his preface, were included "to lighten this perhaps rather solid menu."

Huxley says that my review deals with "a single essay" and that it "does

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not even mention" others, among them "a key one on humanism." Actually I quoted from three essays and there were a number of quotations from the "key one on humanism."

Huxley makes a number of specific complaints. I shall mention two that are especially significant for a discussion of eugenics. He says that I failed to note his "highly critical strictures of all racist theories." Actually I wrote that "Huxley has tried to dissect racism from Galton's philosophy." Concerning genetics and intelligence he now says: "I did not state that 'true geniuses' were characterized by exceptionally high I.Q.'s." He makes this statement because in my review there was this quotation from his book: "It is to man's higher level of intelligence that he owes his evolutionary dominance; and yet how low that level still remains! It is now well established that the human I.Q., when properly assayed, is largely a measure of genetic endowment. Consider the difference in brainpower between the hordes of average men and women with I.Q.'s around 100 and the meagre company of Terman's so-called geniuses with I.Q.'s of 160 or over, and the much rarer true geniuses like Newton and Darwin, Tolstoy and Shakespeare, Goya and Michelangelo, Hammurabi and Confucius. . . ." After this quotation my comment was: "There is, in fact, no reason to assume that Tolstoy, Goya or Hammurabi had high I.Q.'s." It would be curious but tedious to deal with all of Huxley's detailed complaints.

His main charge is my ignorance of certain subjects that he considers (and rightly so) to be of importance in discussing eugenics. According to Huxley, I am "ignorant of the facts" concerning the "social problem group." In his book he said (and I quoted this in my review): "By social problem group I mean the people, all too familiar to social workers in large cities, who seem to have ceased to care, and just carry on the business of bare existence in the midst of extreme poverty and squalor. All too frequently they have to be supported out of public funds, and become a burden on the community. Unfortunately they are not deterred by the conditions of existence from carrying on with the business of reproduction; and their mean family size is very high, much higher than the average for the whole country."

When I read this passage, I thought I knew just which people Huxley had in mind. It now appears that my ignorance was a barrier of which I was not aware. "Recent studies," he now says, ". . . have shown that improved environmental standards have very little direct effect on the general shiftlessness, the low educational attainments, or the size of family of such groups, thus strengthening the presumption of a strong genetic component underlying their behavior. . . ." This statement is in line with a passage in his book: ". . . the indications are that they are genetically subnormal in many other qualities, such as initiative, pertinacity, general exploratory urge and interest, energy, emotional intensity, and willpower." After quoting this in my review I said that almost nothing is known about the genetics of these characteristics and about the relative importance of heredity and of environmental conditions in the origin of these human qualities. From what Huxley now says, recent studies of which I have not been aware have provided evidence for his claims, and he cites Dorothy Morgan's study in particular.

It was difficult to find the journal (*Family Planning*) in which Dr. Morgan's article appeared; it was not in some of the main libraries of science and medicine. In the meantime I wondered about the ingenious and resourceful procedures Dr. Morgan might have used to resolve even partly one of the great problems of the social sciences. Finally we found *Family Planning*, and in the October 1964 issue (page 63) there is an article on Dr. Morgan's work by Renée Brittan. This article describes a birth-control service for "problem families" in the slums of Southampton. The problem families of Southampton are much the same as many families in U.S. cities; indeed, they are like many who live around the corner on West 95th Street in New York, 100 yards or so from where I live. When I read about the social problem group in Huxley's book, these were the people I had in mind. Renée Brittan refers to "what is politely termed 'substandard' housing, in shocking tenement blocks with poor sanitary conveniences, to families at the lowest level of subsistence, some mentally subnormal, many ill, and all social misfits, whose children were already known to the health visitors." This description would apply to families on West 95th Street. Millions of Americans know about this problem.

Approaching the problem families of Southampton in a tactful and patient way, Dr. Morgan induced many of them to accept birth-control procedures. The result, as Huxley says, was beneficent. As for Huxley's other statement (fourth paragraph in his letter) about how Dr. Morgan's work has shown the ineffectiveness of improved environmental standards and thus strengthened the presumption of a strong genetic component, there is nothing at all about this in Renée Brittan's article. Huxley's far-reaching claim that improvement in the environment has little direct effect on the shiftlessness of these people is derived either from some other article or by subtle reading between the lines of this one. The existence of families such as those described in the Southampton report has always been one of the props of eugenics; but obtaining the kind of critical data that Huxley claims was obtained in the Southampton study has always been elusive.

Another prop of eugenics is presented by Huxley in the next to the last paragraph of his letter. The paragraph begins, of course, with the comment that my understanding of evolutionary biology "seems rather sketchy." He soon comes to the problem of whether our knowledge of human genetics is adequate for a program of positive eugenics (that is to say, not merely the elimination of well-defined defective genes). He thinks it is. Indeed, he says: "No detailed knowledge of genetics was needed... by breeders to produce wonderful improvements in domesticated animals and plants.... Nor is it needed by us today to raise the level of desirable genetic potential in man...." Since the time of Galton, and even centuries before, the example of the breeder has inspired eugenicists.

To proceed with man as the breeder does with animals is a proposal that is frequently made with great self-assurance, but it simply will not do. The breeder has clearly defined aims (such as an increase in milk production or egg-laying), whereas what is desirable in man is in itself a great problem. Moreover, the breeder keeps his animals under conditions that are substantially the same for all of them, whereas for human beings the environment is so variable that the genetic potentialities of individuals are often obscure.

No, before man can embark on a program of eugenics he will need far more knowledge and wisdom than he possesses today. For one thing, more knowledge of human genetics is required. Huxley has brushed aside the criticisms of many of our leading human geneticists. It was in this connection that I said he is uncritical.

A. E. Mirsky

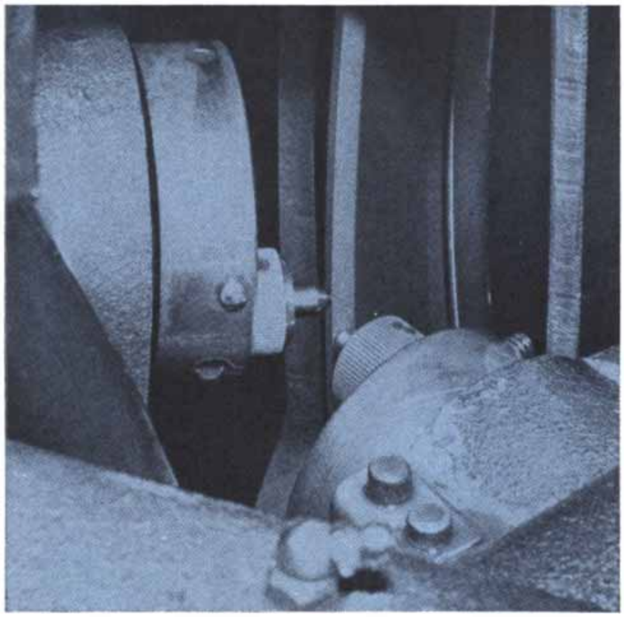
The Rockefeller Institute
New York, N.Y.

## Removing Dinosaur Tracks

from quarry in Dorset, England. Scientists are lifting 24 yards of trackway containing clearly defined dinosaur prints. A natural-diamond saw was used because of its ability to cut quickly and cleanly without causing splintering or cracking. After removal, the slabs will be placed on the lawns of the Natural History Museum in London.

## Dressing Abrasive Wheels

Two diamond tools dress a V-shaped form on the face of a resinoid-bonded wheel used to grind threads in taps at Morse Twist Drill & Machine Co., New Bedford, Mass. As the wheel is driven at 9500 s.f.m., the diamonds dress both sides of the V-form simultaneously. This is a completely automatic operation, operating on a pre-set cycle.

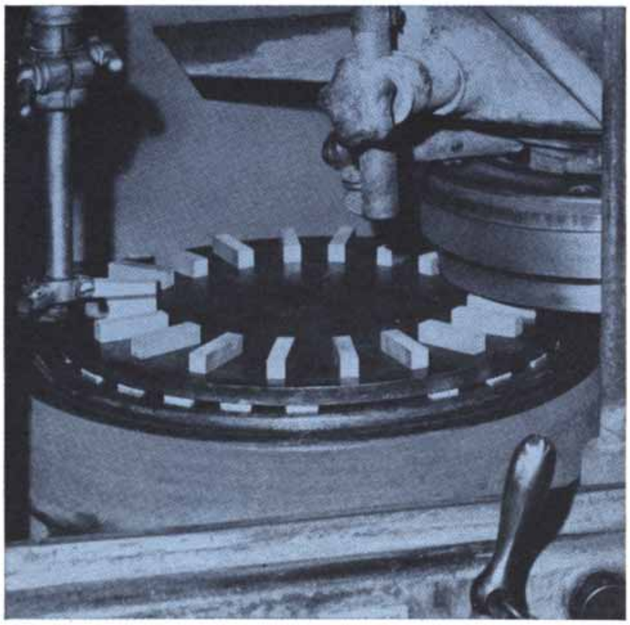


# LOOK WHAT THEY'RE DOING WITH DIAMONDS

### Grinding Ceramics

Piezoelectric ceramics are ground at Honeywell Ceramics Department, Golden Valley, Minnesota.

The wheel on the Blanchard grinder contains 150-mesh diamond grit in a metal bond. Grinding is done at 3000 to 4500 s.f.m. Finish requirement: 32 microinch.



### Drilling Ceramic Floor

Three-inch core drill cut through ceramic tile floor at Los Angeles County General Hospital to make holes for an X-ray drainage sink. This bit, designed by Diamond Tool Research Co., was air-driven at 600 rpm. It took about 15 minutes to drill a hole, six to eight inches deep, through ceramic tile and steel-reinforced concrete.

People with imagination are using industrial diamonds for unusual jobs like removing dinosaur tracks from a quarry, or for more ordinary jobs like cutting holes through ceramic tile floors. Although the jobs shown on these pages span completely different industries and applications, they still share one important detail: with diamonds each job is done more quickly and more economically than before. Some of them couldn't have been done <u>without</u> diamonds.

When you use diamonds, you get the unique combination of excellent cutting or grinding ability linked with fantastic endurance. The results: your diamond tools last longer than any other cutting tools you can use. Your people spend more time in actual production . . . less time changing tools.

The Diamond Research Laboratory in Johannesburg, world's leading authority on diamond technology, is constantly engaged in finding new and better uses for industrial diamonds.

If you cut, sharpen or smooth <u>anything</u> in the course of your business, <u>you</u> can probably use diamonds, too. Why not <u>test</u> diamonds against the method you're now using? You'll learn how efficient—and economical—industrial diamonds can be.

Industrial diamonds cut practically everything . . . especially your operating costs. Your wheel and tool manufacturer can show you how. Write to this magazine for more information.

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# 50 AND 100 YEARS AGO

JANUARY, 1915: "Undoubtedly the most important scientific event of 1914 was the discovery by Prof. Kamerlingh Onnes that when certain metals are cooled to a temperature near absolute zero they cease to have any measurable electrical resistance and a current started in a coil thus cooled will continue indefinitely without any appreciable diminution. Whether this discovery will ever have any commercial application in the field of electrical engineering it is impossible to say. At present it seems unlikely, and yet all our electrical machinery of the present time had its beginning in experiments that seemed just as impracticable, from a commercial point of view, as this one."

"On December 24 and again on Christmas Day, German aviators flew over England and dropped bombs. On the former day a machine appeared high above Dover and dropped a single bomb in an effort to hit Dover Castle. The bomb fell in the garden of the rectory and did no damage. British aeroplanes arose in pursuit, but the German machines had too great a start and they were unable to overtake them. On Christmas Day a second German aviator flew over England and penetrated as far as Rochester, which is but 28 miles east of London and seven miles from Gravesend. The bomb which he dropped fell in a road and did no serious damage."

"Encouraged by the former raids on Düsseldorf, Friedrichshafen and Brussels, the British authorities carried out a similar raid from the sea on Christmas Day. Seven aeroplanes participated in this raid, which was made off Heligoland from two British cruisers and several destroyers and submarines as a base. The water craft managed to thread their way successfully through the mines and to get within a short distance from the coast, where they remained for three hours without being fired upon by the German ports or warships and where they safely re-embarked three of the seven hydro-aeroplanes with their occupants before returning home. Three other aeroplanes returned to their base and were picked up by submarines, which afterwards sank the aeroplanes so that they would not be captured. Flight Commander E. T. Hewlett (a son of Maurice Hewlett, the novelist) was the only one who did not return. His machine was found wrecked some eight miles off Heligoland and its pilot was thought to have been lost."

"Dr. Heber D. Curtis of the Lick Observatory has recently given a preliminary account of the results obtained from comparing a large series of nebular photographs made with the Crossley reflector 12 to 16 years ago with a similar series now being made with the same telescope. In this interval changes due to motions of translation or rotation in the nebulae are quite unappreciable or at most exceedingly small. This applies to several large irregular nebulae, including the Orion nebula, and to several prominent spirals, including the great nebula in Andromeda. It is inferred from these observations that the nebulae in question are enormously remote and therefore, of course, enormous in size."

JANUARY, 1865: "An immense telescope has just been completed for the Chicago University. The object glass is worth $11,187 and required two years for its completion by Mr. Alvan Clark of Cambridge, Mass. The telescope weighs 6,000 pounds, the length of the great tube being 18 feet and the magnifying power ranging from 80 to 1,800."

"Steam carriages on common roads have for many years been a favorite scheme with inventors. The idea is attractive to the mechanical mind, and it is no wonder that men of genius have been charmed with it. To roll over the highway, fast or slow at will, to travel from one end of the country to the other without fear of wearing out horse flesh, to be ready at all hours and seasons for the journey, to have the control of an almost illimitable force under one's hand, would certainly be pleasant in many respects, and every one of the sensations alluded to has already been realized. From the early part of the century up to the present day inventors have brought forth one steam carriage after another, but at this time the number in existence in this country, to say nothing of those in use, may be counted on the fingers. However alluring the scheme may seem, the day is far off when the system will be popular."

"A lucifer match is now in the market that differs from anything hitherto in existence. Upon the side of each box is a chemically prepared piece of friction-paper. When struck upon this, the match instantly ignites; when struck upon anything else whatever, it obstinately refuses to flame. You may lay it upon a red-hot stove, and the wood of the match will calcine before the end of it ignites. Friction upon anything else but this prepared pasteboard has no effect upon it. The invention is an English one, and by a special act of Parliament the use of any other matches but these is not permitted in any public buildings."

"Nothing in the history of this country, if we except the furor that followed the opening of the gold fields of California, has caused so much excitement in business circles as the rapid development of the petroleum oil interests. These are oil stock exchanges, oil stock journals and all the other appliances of regular commercial and financial operations. Oil cities even have sprung into existence, and speculation is running up to fever heat; hundreds of joint stock companies have been organized. Thousands of persons are being allured to invest their money in the stocks of these companies under the stimulus of promises of large dividends. Although there is substantial merit in the oil well productions of the country and it is true that there are many really substantial companies, it behooves those who are infected with the oil fever to be extremely cautious how they invest their money or they will surely suffer loss. It is impossible for any sound-minded man to ignore the fact that thousands, if not millions, of dollars will be abstracted from the people's pockets and wasted upon a set of men who under the guise of respectability are nothing more or less than a set of genteel swindlers."

"The extension of the electric telegraph to Russian America, binding it to this country, is one of those great enterprises which will open up new countries to the influence of civilization and tend to dissipate ignorance, the twin brother of barbarism."

Report from

**BELL LABORATORIES**

## HIGH-SPEED SWITCH FOR MODERN TELEPHONE SYSTEMS

"Ferreed" switches are key elements in the talking paths for telephone conversations in the Bell System's new electronic central office. In setting up connections through the office, the appropriate ferreeds are closed under the direction of the system's central control unit.

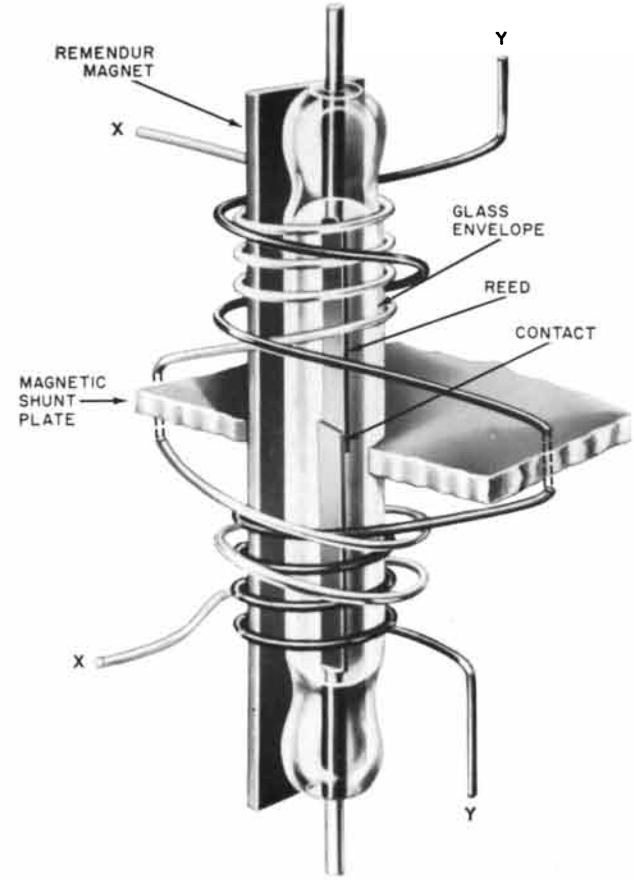
As indicated in the drawing (top right), ferreed switches include glass-enclosed contacts operated by external magnets. Contacts close when central control causes short current pulses to energize the external magnets. A contact remains closed, without expenditure of additional power, until another pulse opens it.

The name for the ferreed switch was coined from "ferrite," the material used in the external magnet when this device was first described by Bell Telephone Laboratories in 1960, and "reed," referring to the magnetic members inside the glass enclosure.

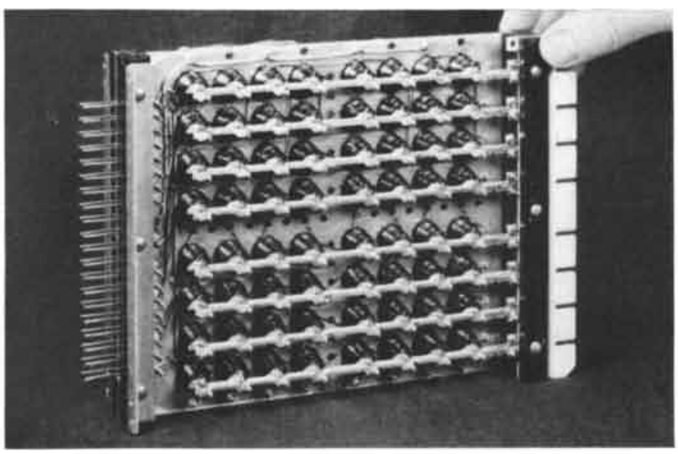
In its most recent form as developed at Bell Laboratories, Remendur is used in place of ferrite. Remendur, also a Bell Laboratories development, is a cobalt-iron-vanadium alloy with square hysteresis loop and values of coercive force intermediate between those of soft magnetic materials and permanent magnets. The device achieves fast contact closure (about a half millisecond) with even faster control pulses—characteristics that are compatible with the high-speed, versatile performance of modern telephone communication systems.

**Bell Telephone Laboratories**

**Research and Development Unit of the Bell System**



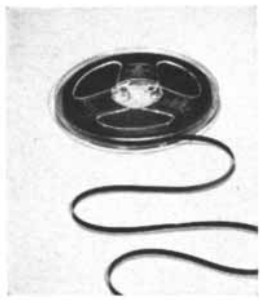

**Concept of the ferreed in simplified form. Windings around magnet and glass-enclosed reeds are arranged in such a way that the contact is opened or closed in response to pulses of current on the x and y leads. For the closed state shown here, simultaneous pulses on both x and y leads effectively cause the Remendur to become one magnet. The two reeds are now magnetically attracted and the contact is closed. To open the contact, a pulse is applied to either the x or the y winding. This pulse effectively divides the Remendur into two magnets at the magnetic shunt plate. The ends of the Remendur then are both north (or both south) poles, and the contact is opened.**



**Typical array of 64 ferreed elements used as network crosspoints in electronic switching systems. Coincident current pulses on x and y leads (see drawing) permit operation of one ferreed crosspoint but not others in the same horizontal row and vertical column. Unit was carefully designed, in cooperation with the Western Electric Co., for efficiency of manufacture and economy.**


KEEP OUT

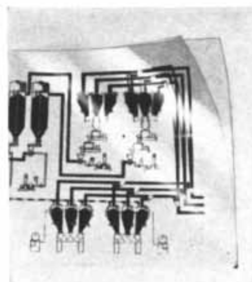
# Which Polyester Film is better?



There are compelling reasons to switch to Celanar polyester film. For magnetic tape. Packaging. Engineering reproduction. Metalizing. Stationery and office supplies. And electrical applications.

Celanar gives you more meaningful service for the price you pay.

Case in point: The purer the polyester film, the better it processes. To make certain new Celanar is the purest you can buy, Celanese produces it in a sealed-off "White Room" clean enough for surgery. It is by far the most modern in the industry.



Another case in point: Many of our customers felt that undetected environmental changes which might occur in shipment of polyester film could adversely affect their processing operation. As a result, we started shipping new Celanar with temperature recording flags. To the best of our knowledge, the other supplier doesn't.

Other unique examples of the meaningful service that makes Celanar your best polyester film buy include the fact that Celanar is *protected against dust contamination* by use of non-fibrous plastic cores. That it is periodically *shipped with Impact Recorders* to protect you against accepting film jolted and damaged in transit. That its splice-free roll lengths are *tailored to its customers' specifications.*

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# Celanar® Polyester Film

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Celanese

# THE AUTHORS

NORA K. PIORE ("Metropolitan Medical Economics") is an economist with the New York City Department of Health and adjunct professor of economics at Hunter College. She was graduated from the University of Wisconsin with A.B. and M.A. degrees in 1934. Among her activities before she joined the city health department in 1958 were service as a staff member of the Committee on Labor and Public Welfare of the U.S. Senate and as a program analyst in a New York State poverty study. Since 1961 she has directed a medical-economics research project established by the city health department and Hunter College.

JOHN STRONG ("Infrared Astronomy by Balloon") is professor of experimental physics at Johns Hopkins University. A Kansan, he was graduated from the University of Kansas in 1926 and received a Ph.D. from the University of Michigan in 1930. He served at the California Institute of Technology and at Harvard University before assuming his present post in 1946. In connection with the subject matter of his article, he wishes to acknowledge his indebtedness to two former Air Force officials, Knox Millsaps and Daniel E. Hooks, "for encouraging our continuation in this field after the first phase of our program was completed."

DAVID TURNBULL ("The Undercooling of Liquids") is Gordon McKay Professor of Applied Physics at Harvard University. Turnbull grew up on a farm in Illinois and was graduated from Monmouth College in 1936, obtaining a Ph.D. from the University of Illinois three years later. From 1939 to 1946 he taught and did research at the Case School of Applied Science, where as a result of studies on the kinetics of the oxidation of metals he became interested in solid-state physics and particularly in crystal imperfections and structure-sensitive phenomena. From 1946 until 1962, when he took his present post, he was with the General Electric Research Laboratory, where from 1950 to 1958 he was manager of the chemical metallurgy section.

SIR JOHN ECCLES ("The Synapse") is professor of physiology at the Australian National University. He shared the Nobel prize for physiology and medicine in 1963; in 1958 he was knighted by Queen Elizabeth. Sir John writes: "My interest in nerve physiology dates from my medical-student days in Melbourne, when I became interested in philosophical problems relating to all the events experienced in consciousness. I became dissatisfied with the explanations given by psychologists and philosophers and decided to try and investigate for myself the basic phenomenon of nerve action. For that purpose I went to Oxford to work under Sir Charles Sherrington, who was then the acknowledged world leader in this field." Eccles was a Victorian Rhodes scholar at Oxford, receiving a D.Phil. there in 1929. He held fellowships at Exeter and Magdalen colleges at Oxford until 1937, when he returned to Australia as director of the Kanematsu Research Institute of Sydney Hospital. In 1944 he became professor of physiology at the University of Otago in New Zealand, taking his present post in 1951. Sir John was the author of the article entitled "The Physiology of Imagination" in the September 1958 issue of SCIENTIFIC AMERICAN.

RUTH SAGER ("Genes outside the Chromosomes") is an investigator in the department of zoology at Columbia University. She was born in Chicago and was graduated from the University of Chicago in 1938, obtaining an M.S. from Rutgers University in 1944 and a Ph.D. from Columbia University in 1948. Thereafter she worked for several years at the Rockefeller Institute before returning to Columbia in 1955. She is coauthor with Francis J. Ryan of *Cell Heredity*. In describing how she became interested in nonchromosomal inheritance, she writes: "I have always been intrigued by the physicist's approach to scientific inquiry, particularly in the fact that the way to find out something really new is to question a basic tenet of existing theory. In this case, the theory that chromosomal genes constitute the total genetic apparatus of cells was already under fire. The serious possibility of a second genetic system, however, was rarely discussed. I found this situation challenging."

GIORGIO DE SANTILLANA ("Alessandro Volta") is professor of the history and philosophy of science at the Massachusetts Institute of Technology. He is a native of Rome and was graduated from the University of Rome in 1925 with a Ph.D. in physics. After teaching at the university for some years and helping to organize the School for History of Science, he went to the Sorbonne as a student and lecturer. He moved to the U.S. in 1936 and became a naturalized citizen in 1945. His affiliation with M.I.T. dates from 1941. Among his books are *Galileo's Dialogue of the Great World Systems, The Crime of Galileo* and *The Age of Adventure.* He wrote the article entitled "Greek Astronomy" in the April 1949 issue of SCIENTIFIC AMERICAN.

M. E. BITTERMAN ("The Evolution of Intelligence") is professor of psychology and chairman of the department of psychology at Bryn Mawr College. He is a native New Yorker who was graduated from New York University in 1941; the following year he obtained an M.A. from Columbia University and in 1945 he received a Ph.D. at Cornell University. During World War II he was involved in the training of specialized military personnel. Subsequently he taught at Cornell and the University of Texas. He spent a year as a visiting professor at the University of California at Berkeley and two years as a member of the Institute for Advanced Study before going to Bryn Mawr in 1957. Since 1955 he has been coeditor of the *American Journal of Psychology*. He conceived the general plan of his present work in the comparative intelligence of animals as an undergraduate but has pursued it intensively only in the past few years.

GEORGE B. BENEDEK ("Magnetic Resonance at High Pressure") is associate professor of physics at the Massachusetts Institute of Technology. Born in New York City, he was graduated from Rensselaer Polytechnic Institute in 1949 and received a Ph.D. from Harvard University in 1953. After two years at the Lincoln Laboratory of M.I.T. he returned to Harvard as a research fellow in solid-state physics, being appointed lecturer in that subject in 1957 and assistant professor of applied physics in 1958. He joined the M.I.T. faculty in 1961. Benedek describes his main interests as "magnetic resonance, high-pressure physics, the Mössbauer effect, physics of shock waves and magnetism in the critical region." He adds: "Right now I am doing what to me are very interesting experiments studying 'Brillouin scattering' of laser beams by sound waves in liquids and solids."

ASA BRIGGS, who in this issue reviews E. P. Thompson's *The Making of the English Working Class,* is professor of history and dean of the School of Social Studies at the University of Sussex.

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# Metropolitan Medical Economics

*An account of a study of how medical care is financed in the nation's largest city: New York. One finding is that nearly a third of the bill for personal medical services in the city is paid out of public funds*

by Nora K. Piore

In New York City early in the 19th century a combined almshouse, public workhouse and house of correction located on the Common near City Hall housed "102 sick, 82 lunatiks, 336 prisoners and 842 crippled and aged persons and disabled veterans of the American Revolution and the War of 1812." The city treasury paid for the care of additional sick and poor New Yorkers by disbursement of funds to a private hospital established in 1658 by the Church of New Amsterdam.

In 1826 the city institution was moved to a site on the outskirts of town known as Belle-Vieu Farm. "Work on the new building," the archives record, "was hastened as the inmates of the old almshouse persistently invaded the City Hall across the road, bombarding the City Fathers with complaints and demands, and could not be driven out without giving rise to charges of cruelty and inhumanity."

Today the Bellevue Hospital complex, which stands on that original site, is one of 28 municipal hospitals with an aggregate capacity of 19,000 beds, operated by the New York City Department of Hospitals at a cost of $242 million in the fiscal year 1964–65. The identity of the New Amsterdam Church Hospital is lost in history. In accordance with the precedent set by its subvention from the city treasury, however, 75 or more voluntary hospitals will this year receive disbursements totaling $67 million from the city on a budget line providing for "Payments to Charitable Institutions for the Care of the Medically Indigent Sick and Infirm." All together the city has earmarked $443 million—one out of every eight dollars in its $3,400 million executive budget for the current fiscal year—to provide or purchase medical, dental and mental health services and institutional care for city residents who need care and are deemed unable to pay for it. The Federal and state governments will spend almost as much in addition to buy health insurance for their employees in the city, to provide medical services to veterans, merchant seamen and members of the armed forces and their dependents who live in the city, and to care for New Yorkers in mental hospitals and rehabilitation centers.

These outlays from tax funds account for nearly a third of the total bill for personal medical services rendered to the 2.8 million families in New York City, and for more than half of the cost of care received by New Yorkers as inpatients in hospitals and related institutions. Only a fraction of these public expenditures goes for the care of the destitute portion of the population that is "on welfare." Almost 80 percent of the patients in New York municipal hospitals are people who manage to cover their ordinary expenses but who lack the margin in income, savings or health insurance to pay the hospital and the doctor when they get sick.

These are the summary findings of a study made by the Urban Medical Economics Research Project, a joint undertaking of the city's Department of Health and the Urban Research Center of Hunter College in the City University of New York. The study was financed in part by the Health Research Council of the City of New York, an agency that spends $8 million of city tax funds each year, or roughly one dollar per capita, in support of medical research in the medical schools, hospitals and universities in the city.

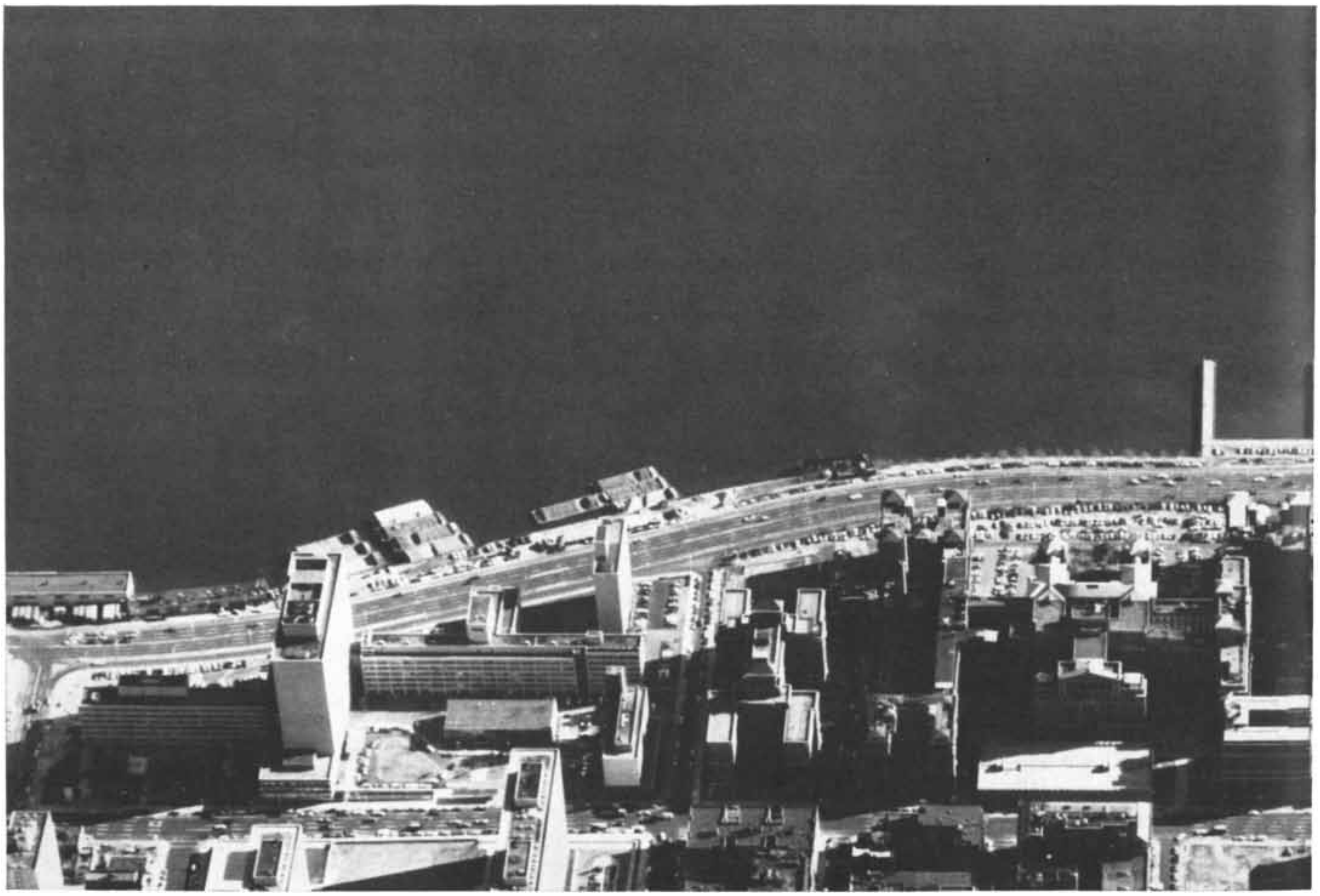
The facts emerging from the study do not quite fit the picture of the *status quo* on which most of its defenders, and many of its critics as well, appear to base their judgments. The steady rise in material wealth has enabled government agencies to extend their medical services far beyond the pesthouse and the poorhouse of earlier times, and the nation is currently performing better than many critics allege in distributing the medical benefits of an affluent society. In common with many urban social services, on the other hand, the administration and distribution of health services constitute a "system" only in the loosest sense. The City Charter authorization for expenditures on medical care still speaks the language of another century: "To promote the public health and to care for the emergent and needy sick." The concepts of who is needy and who is sick and what is required to promote the public health have undergone enormous change. The addition of specific services from year to year and generation to generation came about in response to the urgency of current needs. As a result in New York and other metropolitan communities responsibility for the unification of the "system" is dispersed throughout numerous public agencies and the services are too frequently uncoordinated, unbalanced and uneven in quality.

Comparing death rates in the poorest area of the city with a middle-income neighborhood, George James, the city's Commissioner of Health, attributes 13,000 deaths a year—preventable in the light of present medical knowledge—to the conditions in which the poorest fifth of the city's population lives. Some of these unmet needs could presumably be satisfied by more efficient allocation of resources. Inefficiency in the public sector in turn compounds the formidable economic and organizational problems of the private sector, financed by consumer and insurance payments and by philanthropy, which shares the costs and the single pool of manpower, facilities and resources on which the health of the entire community depends.

Defenders of the *status quo* must concede that the pertinent issue for public concern is not whether government should be involved in financing medical care; it already is. The situation calls for answers to other questions: How present and future medical costs can best be distributed over the economy; how the mushrooming and archaic medical-relief establishment can be converted into a modern social instrumentality that will provide health services in an efficient and socially acceptable manner; how services and institutions financed by public funds can be rationally integrated with those financed by consumers and by philanthropy in order to secure the most efficient use of total resources allocated to health. Ample statutory precedent and economic latitude are available for developing better ways to finance and organize medical services. None of these would make nearly such a radical departure from the *de facto* present as is commonly thought. If the facts were more widely known and fully appreciated, the search for solutions might be freed from many inhibitions that now hinder it.

The health problems of New York and the measures the city has taken to deal with them may not be entirely typical of the nation as a whole. The sheer size of the city, however, and the heterogeneity of its population practically guarantee that problems encountered there will be met in other cities. Given the trends in medical technology and the rise in urban social pressures everywhere in the country, differences are before very long likely to be of degree rather than of kind.

For some time the Social Security Administration has prepared annual estimates of health-care expenditures by source of funds and type of service for the U.S. as a whole. The present study of the medical economics of New York City is believed to be the first effort to develop a systematic analysis of this kind for a single community. We took 1961 as our sample year for establishing our methodology and our chart of accounts. As of 1961 we found that the public treasury provided $530 million, or nearly a third of the total medical-



**MAIN MEDICAL COMPLEX in New York City developed around Bellevue Hospital (*central buildings*). The 11-block complex now includes the New York University Medical Center, the privately operated University Hospital (*buildings near river at left*) and a**

care bill of $1,770 million [*see illustration on next page*]. The estimates of out-of-pocket consumer expenditures and health insurance benefits necessarily present the largest uncertainties; they are derived from data obtained from sample surveys of households and are subject to the usual sampling and reporting errors. The figures for the public sector have a firmer statistical base, but it was no simple task to disaggregate, classify, collate and reconcile data from many different sources. We found that no fewer than 25 separate agencies of government—14 city departments, five state and six Federal departments or agencies—are involved in disbursing money for medical care in New York City [*see illustration on page 23*]. Because our objective was to identify and total public outlays from tax revenues for medical care rendered directly to individuals, the figures exclude health research, sanitation, control of air pollution and other services conventionally identified with the public health function of government. For these services in 1961 New York City appropriated $133 million, bringing the total public expenditures for health in the city to $660 million in that year. Our figures for the public sector also exclude such items as workmen's-compensation payments, mandated but not paid for by government, and insurance contributions for employees of the city's transit system and other quasi-public authorities paid for out of operating revenues rather than taxes.

**federally operated Veterans Administration Hospital (*T-shaped building at right*).**

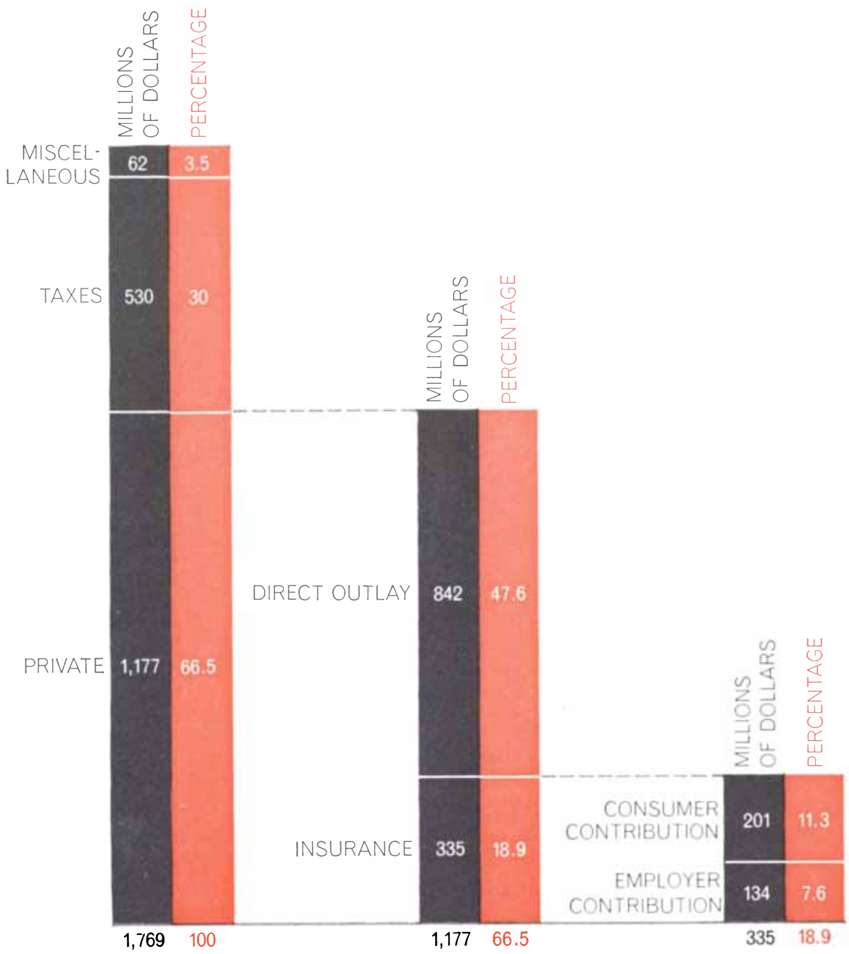
Public expenditures for medical care for New York City residents are rising rapidly in absolute terms; they are up from $530 million in 1961 to $734 million in 1965, an average increase of $51 million per year. During that period state and Federal outlays increased 43 percent while the amount contributed from city tax funds rose 33 percent. At the same time outlays for other public services also advanced, so that over the four-year period the relative importance of medical-care expenditures in the total New York City expense budget remained about the same.

In spite of increased appropriations the city's public medical-care system is short of funds and continues to press urgent claims on the limited municipal revenues that are its principal support. The pressure is turned on the city because the city's obligation is open-ended, whereas the state and Federal governments provide services to special classes of people. The state principally provides long-term care for the mentally ill, whereas the Federal Government principally looks after "its own": its employees and veterans and their dependents. All other tax-financed medical care is administered and largely financed by the city. The city's performance as general practitioner—delivering the infants, immunizing the children and filling their dental cavities, treating communicable and acute diseases, rehabilitating the handicapped and ministering to the aged, the infirm and the chronically ill—is thus determined by the size of municipal appropriations.

In 1961 the city budgeted $302 million for these purposes. Offsetting that figure were some $19.5 million collected from patients and third parties and $67 million received in grants, matching funds and per capita reimbursement under various Federal and state programs. The city receives no reimbursement, however, for in-hospital services rendered to people not on the relief rolls, except for the care of psychiatric and tuberculosis patients and the rehabilitation of children with physical handicaps.

For the 80 percent of the city's patients who are not on relief rolls, eligibility for city care is predicated on means tests and ability to pay. The criteria vary from program to program but usually include money income, size of family, assets, probable duration and cost of the illness and the ability of legally responsible relatives to help pay the bill. Judged only by the money-income criteria of the Department of Hospitals, the members of more than half of the households of the city would have been eligible, as medical indigents, for hospital-care assistance in the census year 1960. This undoubtedly overstates the size of the medically indigent population to the extent that members of these households may have been covered by health insurance, may have had savings or may have had relatives who could contribute to the payment of their hospital bills. Medical indigency can occur at any income level, however, when a costly illness cuts off income, exhausts insurance benefits and wipes out savings.

On the basis of actual admissions it is apparent that not less than a third and perhaps as many as 40 percent of the New Yorkers who enter a hospital can be classified as medical indigents. These figures on the one hand measure the deficit in the ability of individuals to pay for needed medical care out of income, savings or health-insurance benefits; on the other they reflect the extent to which the city currently undertakes to make up that deficit out of public funds. This level of public willingness to provide for the needy sick is a tradition that arises from New York's unique political, social and civic history. A long-standing alliance of the city's "first families" and of the leaders of successive generations of its immigrant minorities—knitted together by such unlikely collaborators as trade union leaders, professional social workers, ward politicians and the faculties of the medical schools of the city (now six in number)—established the tradition and maintains it today. At $227 per capita, expenditures for medical care in New York City exceed the national average of $142 by 60 percent; the difference is due both to public expenditures and to private and philanthropic ones. The chief difference in public expenditures lies in the city's extensive program of hospital care for persons who have low incomes but are not on welfare. The city spends about a fifth of its budgeted health funds on services to infants and children, a third on services to the aged and the remainder—slightly




**MONEY FOR CARE OF NEW YORKERS was spent as shown in this table for fiscal year 1961. The bar at left divides the total expenditure into private outlay, tax money designated for medical purposes, and philanthropic and miscellaneous contributions. The middle bar splits the private expenditures into direct outlay and insurance funds. The bar at right divides insurance into consumer and employer sources. Figures are in millions of dollars.**

less than half—on services to adults under 65.

Turn-of-the-century milk stations and school inspections to isolate children with contagious infections have evolved into a network of agencies supported by the state and the city that, at least on paper, provides newborn infants and children of preschool and school age with extensive preventive, case-finding and treatment services. Forty-four percent of all the babies born in the city each year are delivered in municipal hospitals or the wards of voluntary hospitals. Three out of seven deliveries in the city are paid for by the city. The same proportion of infants is examined and immunized by Department of Health pediatricians when they are six weeks old and periodically thereafter. On first entering public or parochial school half of the children are given medical examinations by school physicians because they do not turn up with certification from private physicians. On an average day 2,800 children are in the hospital; of these a third are in municipal hospitals and perhaps another 15 percent are city-charge patients in voluntary hospitals. All these figures suggest that well over a third of the children in the city do not receive systematic medical supervision from private practitioners, and that the city is the chief provider of the care they receive.

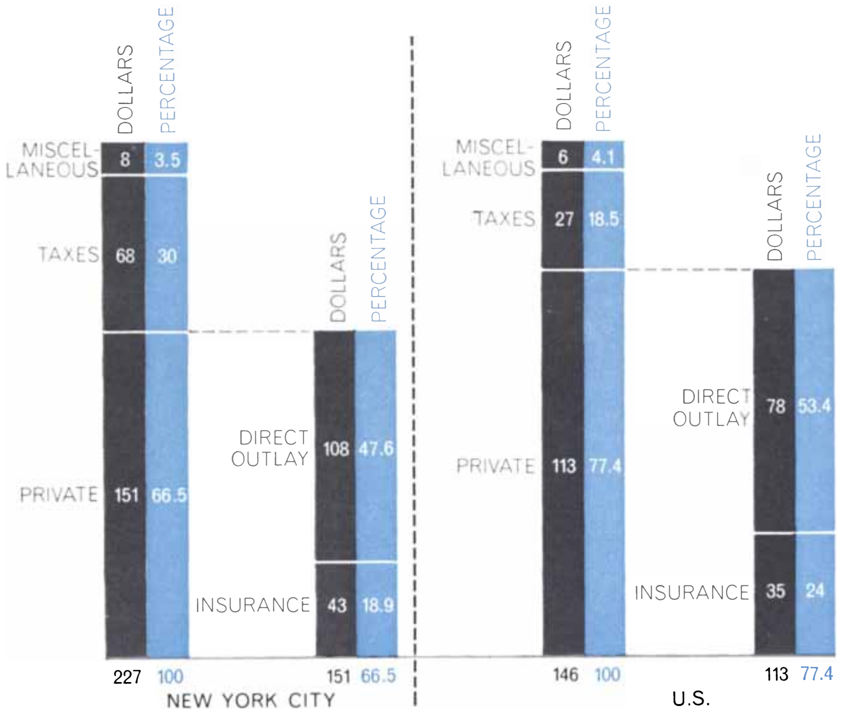
Services provided for the aged range from house calls by physicians, homemaker and visiting-nurse calls, podiatry, drugs, dentures and eyeglasses to nursing-home care for welfare clients and clinic, hospital and home-infirmary care for the medically indigent as well as for welfare recipients. On an average day there are 3,500 aged New Yorkers in the domiciliary or infirmary sections of homes for the aged and another 8,500 are patients in the city's 86 proprietary nursing homes. Tax funds pay some part of the cost for almost all the elderly people in homes for the aged and for two-thirds of those in nursing homes. Of the 8,600 elderly people to be found in short-term hospitals on an average day, 3,600 are city-charge patients. The elderly also receive a substantial portion of the 746,000 days of care provided each year by the municipal hospitals for chronic disease, and they make up a large number of the 2,000 discharged hospital patients for whom municipal hospital staffs provide home care. Similarly, they account for many of the three million visits made each year to the outpatient clinics of the municipal hospitals.

From such figures the exact proportion of the 800,000 aged people in the city who rely on public medical services can only be surmised. The provisions for medical assistance to the aged under the Kerr-Mills Act passed by Congress in 1960 have slightly increased the number eligible for city-administered care. In New York City the principal effect of this legislation has been to shift the fiscal burden so that the Federal and state governments now carry 75 percent of the expenditures formerly borne 100 percent by the city. Thus in a one-day hospital census taken in 1958 it was found that 83 percent of 4,000 aged people were receiving care financed in whole or in part by the city; 62 percent were city-charge patients but not on welfare. At present it is likely that almost all such city-charge elderly patients come under the welfare provisions of the Kerr-Mills Act. One may speculate that if Congress had passed the King-Anderson ("medicare") bill instead, at least some of these indigent elderly sick might have been enabled to move upward to the status of semiprivate patients rather than downward to the status of welfare recipients.

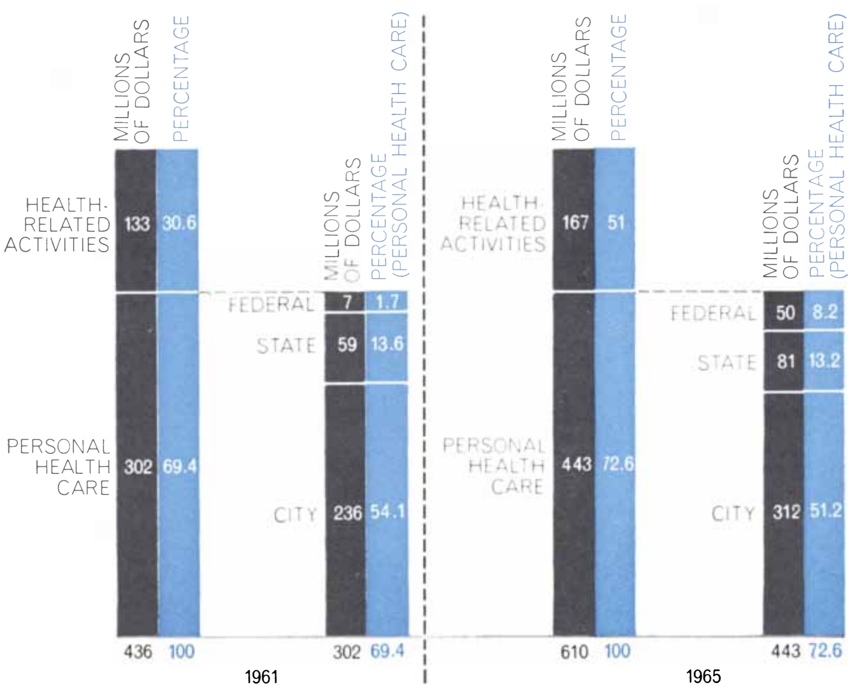
For adults under the age of 65 the city provides a variety of in-hospital, outpatient and emergency-room services, treatment for tuberculosis, venereal and tropical diseases in Department of Health clinics and emergency psychiatric care and some therapy for emotional illness in psychiatric wards and clinics. A one-day hospital census conducted in 1961 by the Associated Hospital Service of New York showed that more than a third of the 21,800 adults hospitalized for short stays were in municipal hospitals or were city-charge patients in voluntary hospitals. In that year the cost of care in such establish-

| | AGENCIES | BUDGET | SOURCE OF TAX FUNDS | | |
|---|---|---|---|---|---|
| | | | CITY | STATE | FEDERAL |
| NEW YORK CITY | Department of Hospitals | $151,443 | $129,246 | $19,997 | $2,200 |
| | Payments to charitable institutions | 41,460 | 34,747 | 6,713 | |
| | Department of Welfare | 30,333 | 14,051 | 11,346 | 4,936 |
| | Community Mental Health Board | 22,331 | 12,953 | 9,378 | |
| | Department of Health | 16,475 | 8,643 | 7,542 | 290 |
| | Employee health services and benefits | 9,402 | 9,328 | 37 | 37 |
| | Department of Education | 7,871 | 3,949 | 3,922 | |
| | New York City Youth Board | 976 | 574 | 402 | |
| | Department of Correction | 714 | 620 | 94 | |
| | Board of Higher Education | 235 | 235 | | |
| | Department of Purchase | 91 | 91 | | |
| | Miscellaneous departments | 45 | 45 | | |
| | New York City Division of Veterans Affairs | 6 | 3 | 3 | |
| | Debt service charges for capital improvements | 21,036 | 21,036 | | |
| | SUBTOTAL | $302,418 | $235,521 | $59,434 | $7,463 |
| NEW YORK STATE | Department of Mental Hygiene | $129,791 | | $129,791 | |
| | Department of Correction | 8,436 | | 8,436 | |
| | Department of Health | 1,000 | | 1,000 | |
| | Employee health benefits | 1,100 | | 1,100 | |
| | Department of Education | 445 | | 445 | |
| | SUBTOTAL | $140,772 | | $140,772 | |
| FEDERAL | Veterans Administration hospitals and clinics | $59,300 | | | $59,300 |
| | Department of Health, Education, and Welfare, and affiliates | 10,400 | | | 10,400 |
| | Department of Defense, Medicare | 9,500 | | | 9,500 |
| | Employee health benefits | 5,581 | | | 5,581 |
| | Department of Justice, Federal Detention Headquarters, Hospital Unit | 100 | | | 100 |
| | Hill-Burton | 1,196 | | | 1,196 |
| | SUBTOTAL | $86,077 | | | $86,077 |
| | TOTAL | $529,267 | $235,521 | $200,206 | $93,540 |

**GOVERNMENT FUNDS spent on personal health care of New Yorkers in 1961 are charted according to the agency administering the money (*column at left*). The "budget" column lists the amount, in thousands of dollars, coming from each agency. The columns at right show the source of the funds: city, state and Federal taxes. The bottom row gives the total spent at each level of government.**




**PER CAPITA SPENDING** on medical care for New Yorkers exceeds that for people from other parts of the U.S. The two bars at left describe the spending of New York City residents. The bar at far left breaks down the per capita total ($227) into private, tax and miscellaneous outlays. Private expenditures are divided in next bar into direct payments and insurance funds. Figures in the colored half of each bar give the outlays in percentage of the total. The two bars at right indicate per capita spending on health care for citizens of the rest of the U.S. by the same convention. All the figures are for fiscal year 1961.




**CITY FUNDS DEVOTED TO HEALTH CARE** have risen considerably from 1961 to the present, according to this comparison of budgets for that year and 1965. The bar at far left gives the city expenditures for personal health care and related activities in millions of dollars (*colored part of bar*) and percentage of total (*gray part*). The next bar shows the Federal, state and city contributions to personal health-care funds. The two bars at right similarly express the budget for 1965. In spite of medical-care legislation passed in the interim, resulting in a higher percentage of Federal expense in 1965, the total city outlay rises.

ments as hospitals, nursing homes and mental institutions absorbed 86 percent of the expenditures by all levels of government on health services for adults; only 14 percent went to ambulatory services or care at home. Of course, the lower unit cost of noninstitutional services accounts for some part of this apparent imbalance; another part is due to the fact that many families that cannot meet a large hospital bill manage to pay for a visit or even several visits to a doctor's office. In a broad sense, aggregate tax expenditures for personal health can be said to furnish low-income families with a counterpart or substitute for the institutional services purchased through voluntary health insurance by the better-off members of the population; today the major portion of insurance benefits relates to hospitalization and surgery.

The analogy is supported by the many similar diseconomies that are fostered under the two systems of financing, all dissipating some of the potential benefits of the private and public money spent for medical care. Both set up a strong fiscal bias against adequate preventive and ambulatory care. Recent studies by Paul M. Densen, the city's Deputy Commissioner of Health, and his associates indicate that a population with ready access to preventive and health-maintenance services experiences fewer days of hospitalization than a comparable population insured for hospital care only. In contrast, the financial and diagnostic criteria that qualify the adult New Yorker for city-aided health services are such that the city tends to get the patient when he is either very sick or wholly unable to afford treatment.

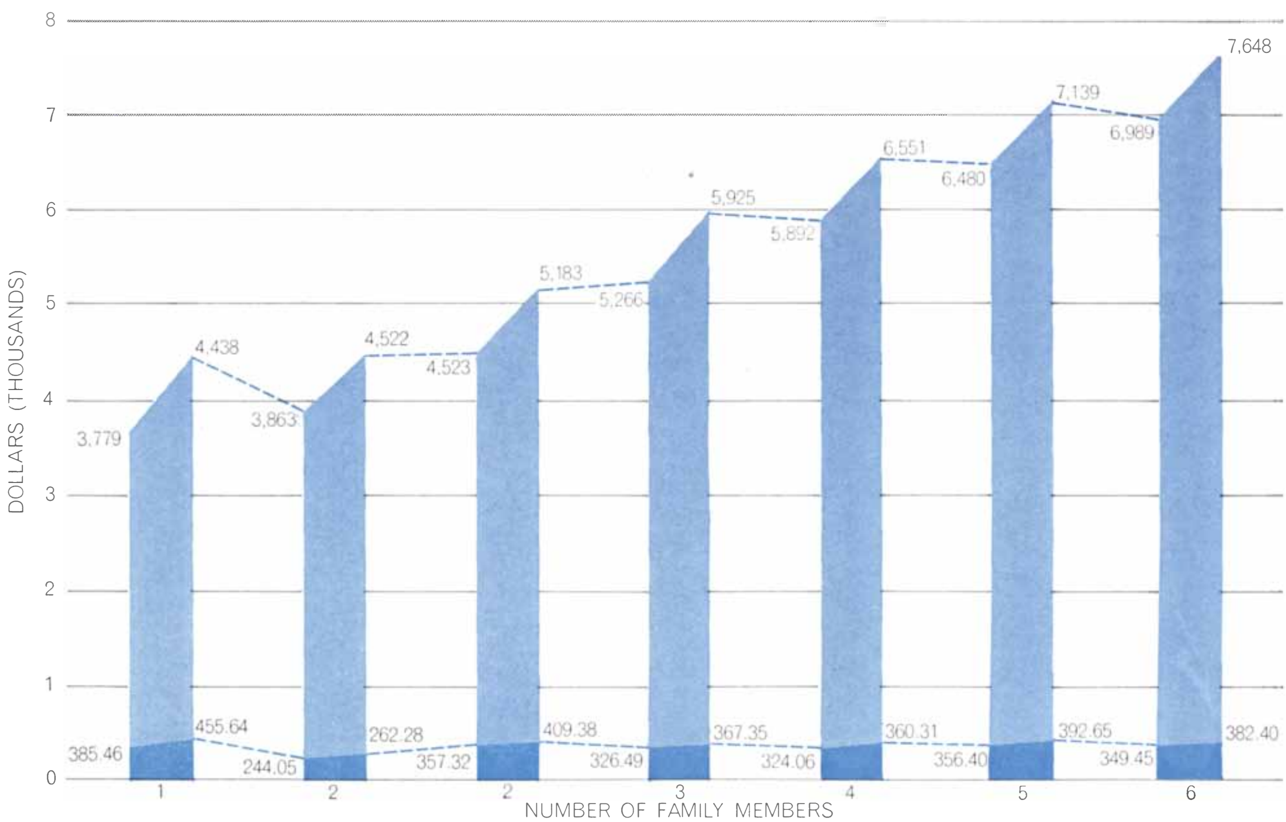
A study prepared for the city in 1962 by Herbert E. Klarman (now at Johns Hopkins University) estimated that $717 million was spent by all public, private and voluntary hospitals and related facilities in New York City and by state mental hospitals outside the city on behalf of city residents; half of these expenditures—$358 million—came from taxes. Such heavy dependence on tax funds is no novelty in the fiscal history of hospitals in New York City. What is surprising is how little the tax burden has been relieved by the growth of voluntary health insurance. A generation ago the city's Commissioner of Hospitals, S. S. Goldwater, expressed the hope that this then novel invention might relieve the city hospitals of "a pressure that has become almost intolerable and that . . . seems destined to grow in intensity from year to year." Currently an estimated 72 percent of the population of the city has some form of hospital insurance; nevertheless, the pressure on the city hospital-care system has in no way diminished.

A significant element in the buildup of pressure is the extent and cost of mental illness. A full third of all the government expenditures accounted for in our study, $188 million, went to treat the emotionally ill and mentally handicapped. These public funds constituted 80 percent of all outlays for mental health care received by New Yorkers. The usual fiscal bias against preventive and ambulatory services shows up strongly in these outlays: only 6 percent of the $188 million was for noninstitutional care.

The state took over the care of the insane from the city in 1890; by 1961 state tax revenues were funding 73 percent of the costs and 70 percent of the care was being rendered in state institutions. Since 1954 some state money has been available for reimbursing the city for its outlays on short-term psychiatric care and community mental health services. Moreover, recent Federal legislation makes funds available for assisting in the construction of community centers for the ambulatory treatment of mental illness. But the contrainducements are there: because the state undertakes 100 percent of the cost of institutional care, officials are reluctant to raise the local revenues required to secure Federal and state matching funds for extrainstitutional care. In sum, the fiscal inducements are all in the direction of continuing to ship the patient off to the state hospital.

The boundary between the public and the private sectors, which is drawn so strongly in ideological discussions of public medical-care policy, would appear to be less clearly defined than is commonly supposed: in New York the city government is and has been a major purchaser of services rendered by private and nonprofit vendors. About a fifth of the total public expenditure for medical care of all kinds and nearly a third of the city-budgeted portion of these outlays go to vendor payments for the services of private practitioners or to voluntary hospitals and clinics. Some payments go through the Department of Welfare to proprietary nursing homes and homes for the aged, to solo-practice physicians for house calls, to voluntary hospitals for outpatient services. More recently, the Department of Welfare has been purchasing "package" care for welfare families from certain voluntary hospitals and from the Health Insurance Plan of New York City, a nonprofit prepayment insurer that provides a comprehensive medical service through group-practice units to 700,000 regular subscribers.

The major flows from the public to the private sector go through the "Charitable Institutions" budget of the city. These funds purchase hospital care from the voluntary hospitals for both welfare and nonwelfare patients. In the one-day hospital census of 1961, 5,239, or 23 percent of the 22,897 patients in the voluntary hospitals, were clients of the city; disbursements from the city to 75 voluntary hospitals that year totaled $36 million and accounted for 13 percent of their operating expenses. In addition to the direct funds paid by the city for the care of city-charge patients in the charitable institutions, the latter received fiscal benefits from the Government in




**ABILITY OF A PATIENT TO PAY is determined partly by money income and size of family, as this chart of sample payment criteria suggests. Figures at bottom left and right of each bar show the respective minimum and maximum the city assumes a family of given size (*below bar*) and income bracket (*top of bar*) can spend annually on medical care. Such criteria are used only as guidelines. In practice, other factors such as duration of illness and solvency of relatives are considered. The size-of-family and money-income criteria serve merely as the starting point for a dialogue between the city collector and the patient who received treatment.**

the form of exemptions from real estate taxes and water charges. Data collected by Klarman in 1957 and 1958 yield an estimate of more than $8 million as the value of these exemptions. A third source of revenue for the voluntary hospitals is philanthropy, in the form of contributions or income from endowments. These funds constitute another 13 percent of the aggregate operating expenses. The remaining 71 percent is made up from direct payments by patients and from payments by third parties such as insurance companies.

The 25 public agencies that administer health services to individuals derive their respective charters from statutes enacted over the years for a variety of public purposes by the city, state and Federal governments. At one time or another agencies at each of these levels of government have found it necessary or desirable to spend tax funds for treatment of individuals sick with tuberculosis in order to protect the public health; for rehabilitation of the handicapped in order to restore earning capacity to people who would otherwise be public charges; for maternal and child health services, without a means test and without charge, in order to produce a healthy adult population; for medical benefits to people such as veterans, to whom there is owed a special public obligation; for care of the needy aged, the infirm and the chronically ill for simple humanitarian reasons.

Each new program has been grafted onto the city's existing institutions and administrative agencies. These have been periodically reorganized—over the past half century six of the city's mayors have appointed commissions on health services and hospital care. Patients, in their ignorance of departmental jurisdictions, have also helped to blur the lines separating them—mothers bring sick babies to well-baby clinics and adults present themselves at emergency rooms to avoid overcrowded clinics. The 25 agencies constitute a patchwork array of services, with different criteria of eligibility, separate medical as well as administrative records and different methods of providing and purchasing care. Nonetheless, they have come to serve a common and central function in the community not contemplated in the diversity of separate purposes for which they were originally created. Taken together, they now constitute the sole or chief year-in, year-out source of medical care for a substantial portion of the population.

In any one unit of the patchwork the best-qualified physicians may serve, and they may employ the most advanced techniques and physical equipment. The arrangements that admit the patient into the system and regulate the flow of services, however, remain largely unchanged from the days when one-shot preventive measures or episodic care was the core of medical practice. Present patterns of morbidity and the present capability of medical technology require continuous supervision of the patient as well as the use of specialized personnel and diagnostic tools. The costly resources of modern medicine cannot be effectively employed in ministering to the "emergent and needy sick"; they must be organized for the systematic care of entire communities or


**SCHOOLGIRL BATHING BABY** typifies the immigrant children who received health and hygiene education from the Department of Health prior to World War I. The Little Mothers' League, an affiliate of the department, stressed infant and child care. The advantage of teaching the schoolgirl was twofold: her mother probably spoke no English, and she herself was a future mother.

population groups, in sickness and in health.

Ever since the late 1950's the city's three chief medical-care agencies—the departments of Health, Hospitals and Welfare—have been committed to reforms aimed at the coordination of their fragmented services and the design of a system for integrating these with the care rendered, often to the same families, by private vendors and voluntary organizations. These departments are also searching for ways to provide, in the control of chronic illness and the care of the aged, the kind of reforms pioneered by the public health profession a half century ago in the control of communicable diseases and the care of children.

Efforts of this kind now seriously lag behind the advance of medical knowledge. In the spring of 1964 Robert F. Wagner, mayor of New York, launched the city's counterpart of the Federal "antipoverty" campaign. He published with his own endorsement the recommendations of his Council on Poverty. Recognizing illness and disability as major causes of urban dependency, the Council had placed improved medical care for low-income families high on the agenda of needed social investment. The ink was scarcely dry on this pronouncement when the nonprofit Associated Hospital Service—the New York Blue Cross—appeared before the State Insurance Commission to ask for permission to increase its rates and to adopt "experience" rather than "community" rating standards in order to compete with the commercial insurance companies that skim the cream off the market by offering attractive rates to low-risk groups. The shift in rating standards was held up pending further study. Such a shift, coupled with rising rates, would further narrow the shelter offered by Blue Cross to the aged and the economically disadvantaged—and would correspondingly enlarge the roll of the city's medical dependents.

The warmth of the medicare issue in national politics suggests that similar crises may arise elsewhere in the medical economy of the U.S. In response to popular dissatisfaction it seems likely that the Federal Government and many states will be allocating more money in the near future—much of it by means of city administrations—for medical care of the indigent and for such special purposes as mental health, medical education and more and better nursing homes.

Measures such as these will surely help to meet urgent needs. They may also, however, compound the inefficiency of current practices, increase the dilemma of middle-income families and, by intensifying the atmosphere of public charity that now prevails, aggravate the very tendency toward social dependency they are intended to reverse. As the present study of medical economics in New York City has shown, the cost of care for one sick individual is now distributed by a variety of mechanisms among many people. Once this is recognized, there is every reason to consider ways of making these distributions according to explicit criteria of equity to the patient and efficiency in the use of some of the most precious resources in the possession of society.

**PATIENTS AT EXPERIMENTAL CLINIC** are residents of a public housing project in which the clinic is located. Before the clinic was opened, residents of this project in Long Island City were an hour by bus from the nearest public clinic. The proximity to such public medical services enables elderly residents to remain in their own homes rather than move to homes for the aged.

# Infrared Astronomy by Balloon

*Telescopes carried to high altitudes can capture radiation that never reaches the ground. One such observation of infrared rays indicates that the clouds of Venus are composed of ice crystals*

by John Strong

The earth's atmosphere isolates the earth's surface from the depths of space in two contrasting ways. The first is a blessing to the organisms that have evolved on the earth. The nitrogen, oxygen and ozone of the atmosphere absorb ultraviolet radiation and X rays and thus form a shield against these harsh constituents of the shortwave end of the sun's electromagnetic spectrum. By the same token water vapor and carbon dioxide absorb the longer-wave infrared radiation emitted by the earth, thereby keeping the temperature of the earth's surface at a mild level. The contrasting kind of isolation lies in the fact that these same substances black out much of the radiation that informs men about the cosmos. Earthbound telescopes are confined to detecting the wavelengths that pass through "windows" in the atmosphere: one in the visible region of the spectrum, one in part of the radio region and a series of narrow apertures in the infrared region [*see top illustration on page 32*].

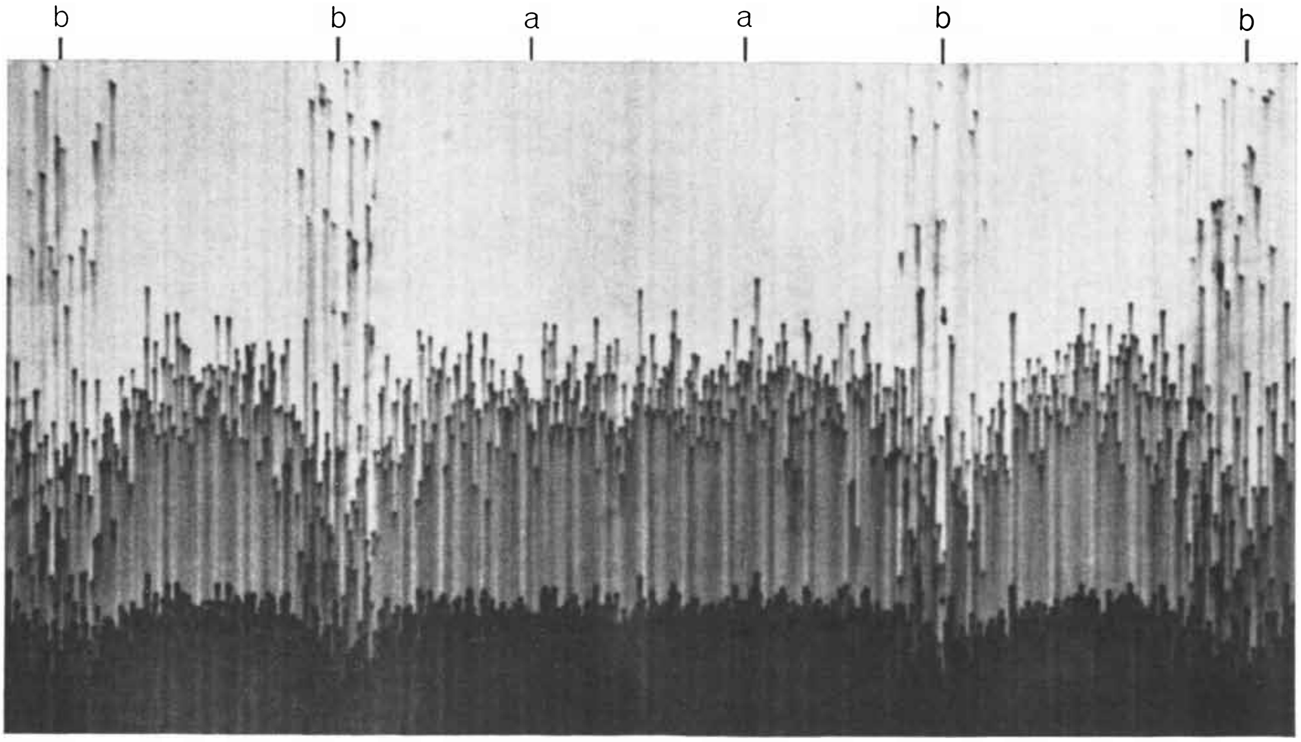
In recent years astronomers have taken steps toward surmounting atmospheric isolation. Using aircraft and balloons, they have lifted astronomical instruments above most of the atmosphere; using rockets and other space vehicles, they have sent such instruments beyond it. Already the new observations that these techniques have made possible have considerably expanded astronomical knowledge. Rockets have proved particularly useful for the study of radiation at wavelengths in the region of ultraviolet radiation and X rays; the equipment required to detect these radiations is light in weight and quick in response. Balloons are well suited to infrared investigations; their mode of flight permits extended periods of relatively motionless observation, a prerequisite for the detection of radiation at these less energetic wavelengths. Balloons are also able to lift comparatively large telescopes and spectrometers to altitudes above as much as 99.9 percent of the obscuring water vapor in the earth's atmosphere.

There is no shortage of astronomical problems that infrared studies can illuminate. Even within the limits of the solar system there are intriguing questions to be answered. To choose only one example, what is the atmospheric pressure on Mars? Infrared observations should provide an answer to this question. In the realm of the galaxy infrared astronomy also offers much promise. Can one detect the infrared radiation of "proto-stars," condensing clouds of matter that have not yet become hot enough to emit light? Which of the stars that emit light have cool, invisible companions or shells? Since infrared radiation penetrates clouds of particulate matter more readily than light does, what will infrared observations reveal about the cloud-obscured nucleus of the galaxy? In my estimation infrared studies, in answering these questions and many others, are capable of substantially increasing the sum of astrophysical information.

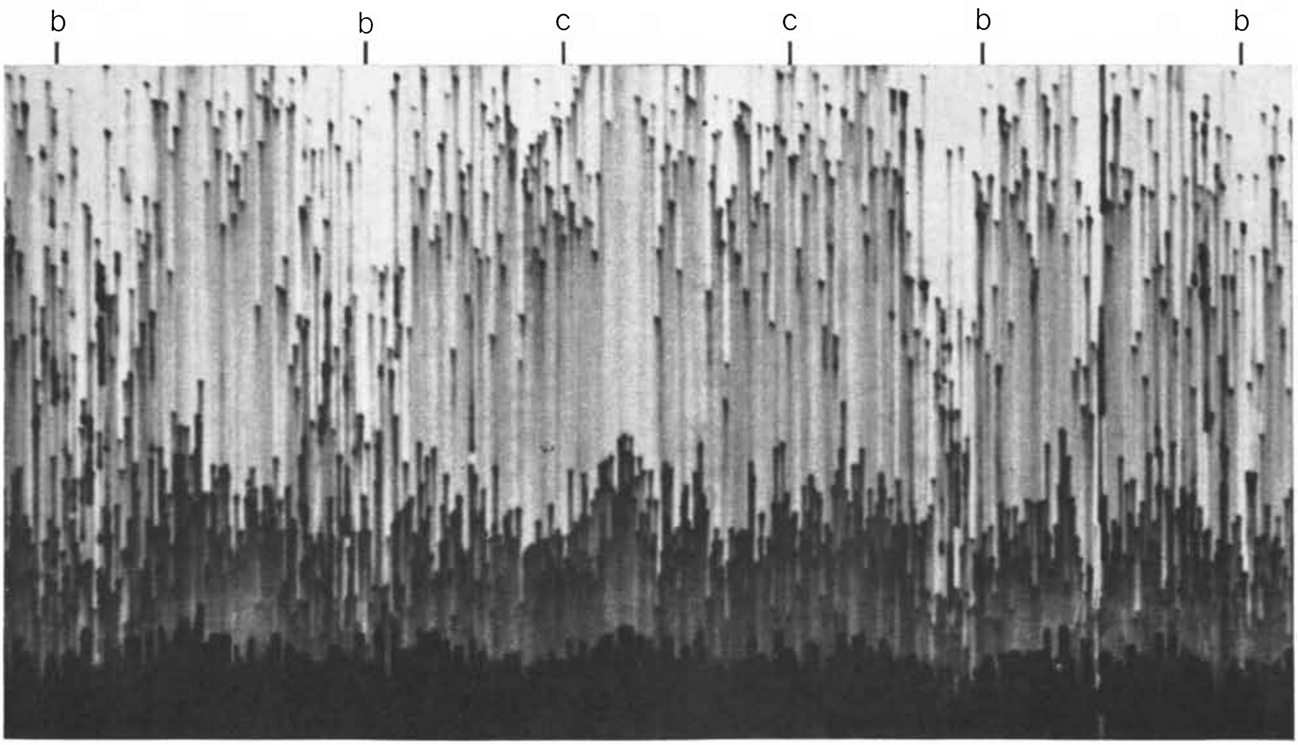
Our group at Johns Hopkins University was initiated into balloon-borne infrared investigations in 1956, when Shirleigh Silverman, Frank B. Isakson and Malcolm Ross of the Office of Naval Research inquired if we had in mind any astronomical projects for which the Navy's two-man high-altitude balloon might provide a suitable platform. Our first thought was to examine the solar infrared radiation reflected by Mars and Venus. The Navy balloon's spherical, pressurized gondola was not the best imaginable base on which to mount a telescope equipped with a spectrometer, but the prospect of these observations was so attractive that we soon managed to devise a means of making them.

Our solution was a specially modified telescope of the Schmidt type: a reflector with a transparent corrector plate. The primary mirror was 16 inches in diameter, although only 12 inches of it were in use at any one time. Both the mirror and the corrector plate, which was 12 inches in diameter, were made of fused quartz to resist expansion and contraction due to temperature change; the combination provided an instrument with the high-speed focal ratio of $f/1.5$. The surplus mirror area was purposely provided: in pointing the entire apparatus borne aloft by the balloon we wished to avoid the need for precise motions by its bulkiest part, the telescope. Instead the requirement for precision was shifted to a relatively light interior component of the telescope: the "optical relay." To achieve this end the telescope was mounted in such a way that all its motions with respect to its accompanying spectrometer pivoted around a point in the center of the correcting plate. The optical relay, mounted on gimbals to swing around the same point, was kept lined up on the target planet by means of a servo-controlled light-sensitive apparatus called a star-tracker.

With this system in action, once the heavy telescope was manually aimed at the planet it could drift as much as three degrees of arc off target, letting the image of the planet wander at random across the primary mirror's focal surface [*see illustration at bottom of pages 32 and 33*]. As this happened, however, the servo-controlled optical relay, locked on target, would unfailingly track the wandering primary image and




**ABSORPTION** of infrared radiation by water vapor high in the earth's atmosphere caused minuscule dips (*below the letter "a"*) in a trace by the Johns Hopkins University spectrometer. Dark, medium and light markings on the sensitized paper tape show the same signals at three increasing degrees of sensitivity. The infrared wavelengths recorded here came directly from the sun. An image-chopper at the spectrometer's entrance slit rotates 26 times per second, so that radiation emerging from the instrument's array of exit slits reaches a photomultiplier as a series of individual pulses, each of which, when amplified, swings the trio of mirror galvanometers that produces the trace. The deep dips (*below the letter "b"*) locate the narrow spectral line from a calibrating light source.

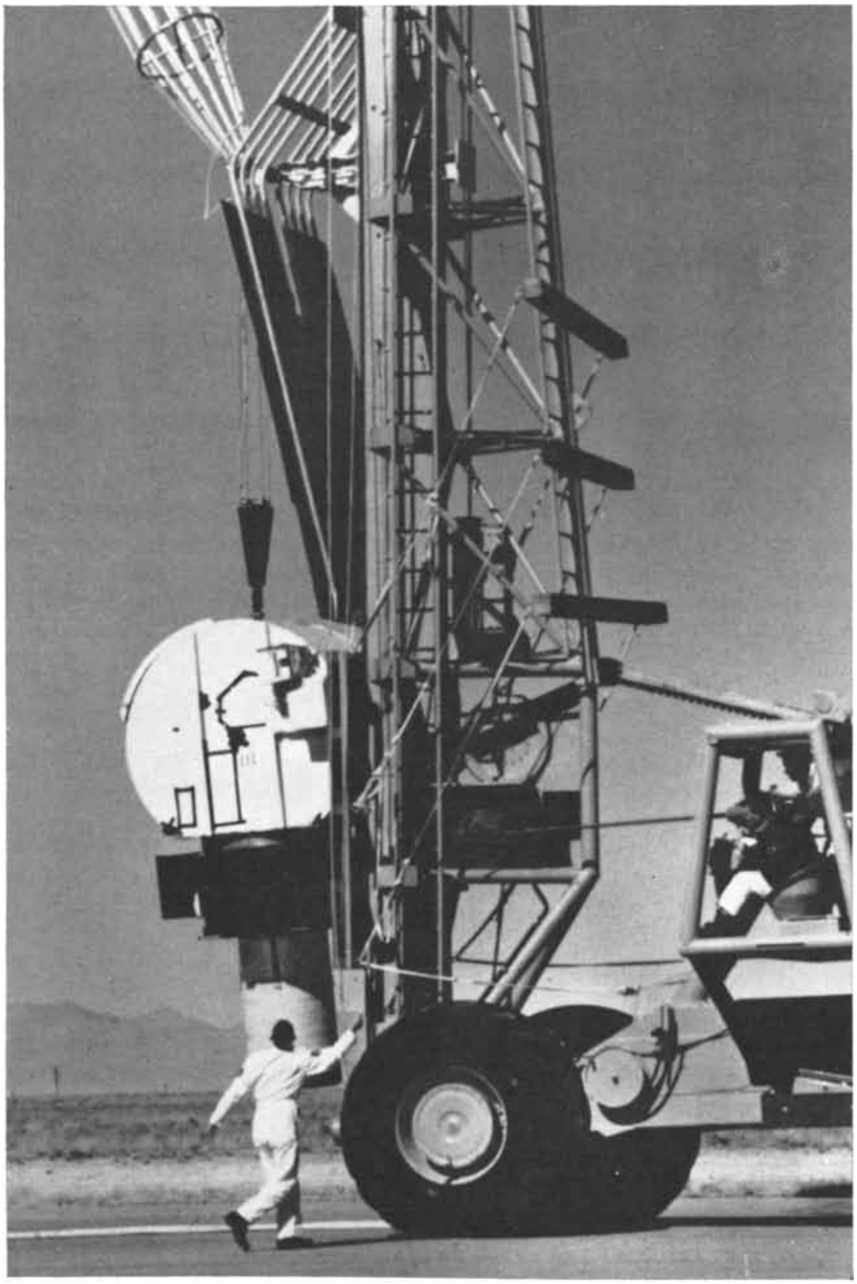



**WATER VAPOR** in the atmosphere of Venus extending above the planet's obscuring cloud layer caused the infrared absorption dips (*below the letter "c"*) in this spectrometer trace. Like the reading for high-altitude terrestrial water vapor (*top illustration*) this one was made during a balloon flight on February 21, 1964, and the deep dips (*below the letter "b"*) are similar calibrations. Both sections of tape reproduced on this page show some 30 seconds of scanning by the instrument's motor-driven array of 21 exit slits. During the February flight more than 500 such scans of the radiation reflected from Venus were recorded; the conclusion that the planet's atmospheric water vapor caused a 10.5 percent dip in the infrared signal received derives from an averaging of all scans.

form a second image, stationary to within three seconds of arc, at the pivot point of the gimbals. This second image, reflected by a small mirror controlled by a linkage, was focused on the spectrometer entrance slit.

Infrared studies of Mars had the first priority in our program for the Navy's manned balloon. The planet was in opposition close to the earth in 1958, but a failure—the first of several—of the balloon's polyethylene bag prevented a flight at this favorable time. It was not until late in November, 1959, that the balloon, with Ross as pilot and C. B. Moore as observer, successfully lifted our instruments to an altitude of 80,000 feet. The opposition of Mars having long since passed, the infrared target was Venus. With no more than an estimated .1 percent of the earth's atmospheric water vapor between them and their target, the balloonists aimed the telescope and recorded absorption lines in the spectrum of solar infrared radiation reflected from Venus.

The lines appeared at the intervals in the spectrum known to indicate absorption by water vapor. The absorption, however, was no greater than 5 percent of the background of reflected solar radiation, and the range of error in the observations was plus or minus 4 percent. The uncertainty arose from the motions of the gondola. These were partly caused by the necessary activities of the gondola's occupants but were mainly the result of abnormal movements by the balloon—movements so severe that the mission almost aborted. That Ross and Moore were able to obtain any results at all was remarkable, and we were very pleased that our apparatus worked as well as it did. Nonetheless, the variation in the radiation recorded was so large that it made the findings equivocal.



**INSTRUMENT PACKAGE, inside metal housing, consists of the telescope and spectrometer mounted side by side (*see top illustration on opposite page*). The cylindrical unit below is the command package; it receives the signals that control the balloon's flight.**

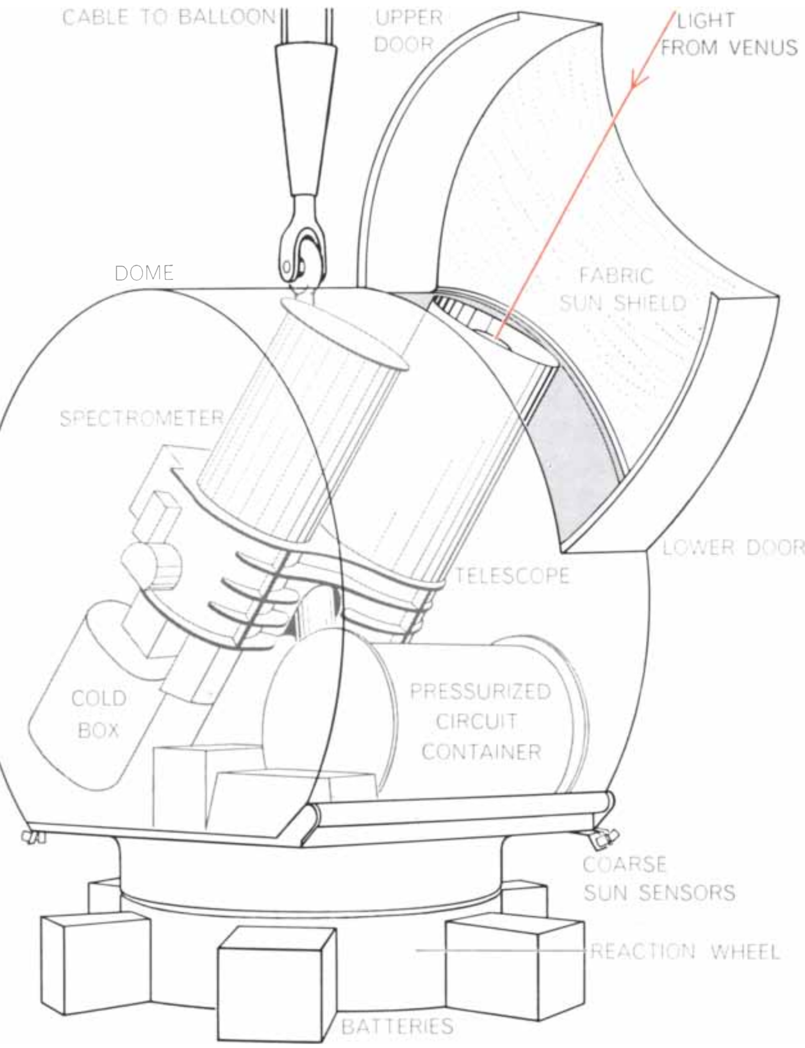
In 1961 the sponsorship of our infrared-astronomy project shifted from the Navy to the Air Force, and we made plans to fly our equipment in unmanned balloons. In 1962 we had the discouraging experience of losing three polyethylene balloons during test flights. The next year we were not much luckier. A flight on May 4, 1963, used a stronger bag made of polyester resin, but now there was a failure in our equipment. This was hastily repaired and another flight was made on May 10 with a polyethylene bag. The balloon burst.

Fortunately for the progress of astronomy, while we were having difficulties Martin Schwarzschild of Princeton University, a pioneer in the field of balloon-borne telescopes, was enjoying success. In 1963 the infrared apparatus in his unmanned *Stratoscope II* detected water vapor in the atmosphere of Mars. When our instrument package was once more ready for flight in February, 1964, our target again was Venus.

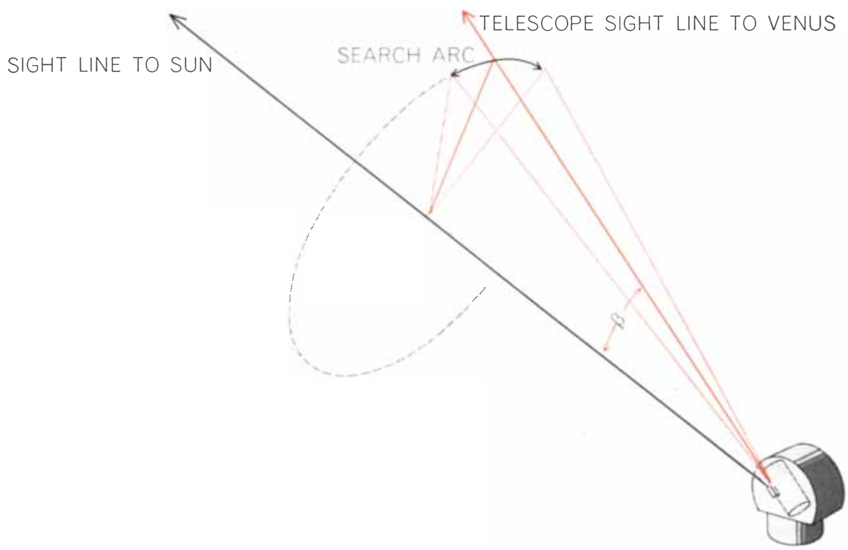
More than four years had passed since the first Venus flight; in the interval a number of changes had been made in our equipment. In 1959 the rough pointing of the telescope had been the observer's job; now that the flights were unmanned this would have to be done automatically. Whereas the 1959 flight was made at night, the unmanned flights were now scheduled for the daylight hours; for such flights we developed a system that would achieve the rough pointing of the telescope with servo controls that sensed the position of the sun. When these locked on the sun, the instrument package could be turned to the elevation and azimuth of Venus. Changes in elevation would work against the restraint of gravity; changes in azimuth, against a counterrotating wheel, its mass consisting largely of the storage batteries that powered the apparatus. The pointing configuration is much like the one developed by Schwarzschild for *Stratoscope I* [see "Balloon Astronomy," by Martin and Barbara Schwarzschild; Scientific American, May, 1959]. This we copied in many respects, except that we located the counterrotating wheel below our telescope rather than above it. Placing this heavy component at the bottom seemed logical on two counts: first, our package would have to make a relatively hard parachute landing, and second, we wanted to make ground tests of the equipment without an obstruction overhead.

The sun-sensing servo system we designed for the daylight flights would be able to do no more than line up the telescope approximately with Venus. A second and more refined sun-sensing system would be required to bring the instrument into position for observations. This device, developed by Murk Bottema, could be programmed in advance of flight time and could also overcome any errors in programming. Called the Beta-rho system, the sensor consisted basically of a light-sensitive cell pointed toward the sun at an angle (with respect to the long axis of the telescope) that was equal to the angle between the sun and Venus in a great circle on the celestial sphere. The change in this great-circle angle resulting from any movement of the observation site on earth is negligible. At our proposed flight date, when Venus would be in an ideal position for observation, the angle also changes very little with the time of day. Thus in effect one element in our sensor system was a constant [*see bottom illustration on this page*].

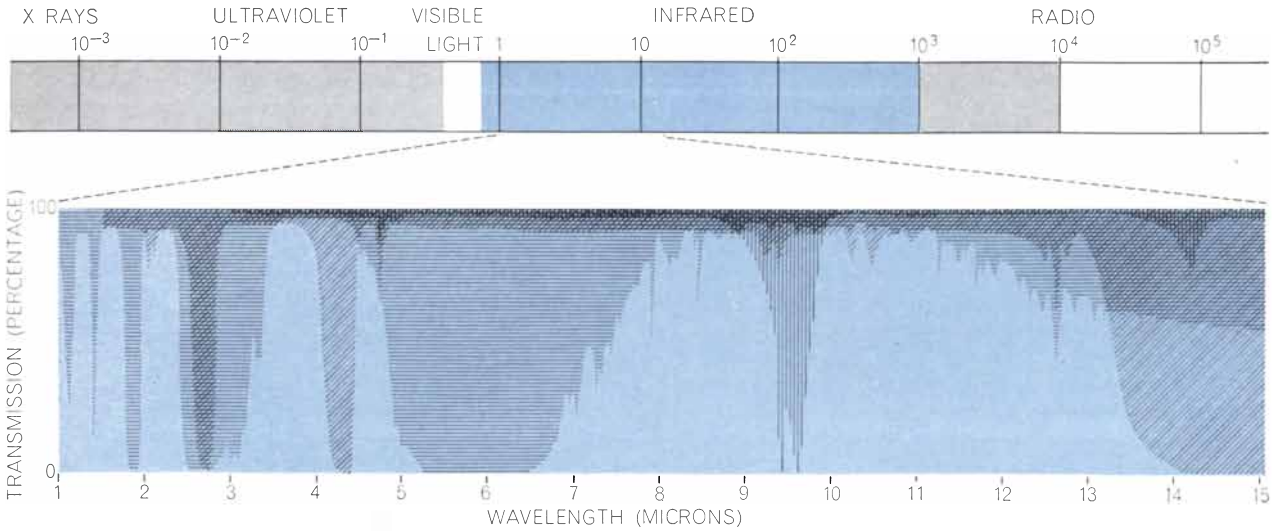
Although the sun sensor would be aimed at the sun, a prism in front of it was positioned so that it could rotate on an axis parallel to the long axis of the telescope. Unlike the great-circle angle, this roll angle would vary both with the geographical position of the balloon and with time: it would be preset for a place and time predicted on the basis of known upper-atmosphere wind direction and wind speed. Should the prediction be in error, a rotation of the prism around its axis would produce a correcting scan. The device takes advantage of the celestial geometry that requires Venus to lie on the arc of a circle with a radius defined by the great-circle angle between the sun and Venus, regardless of the instrument's geographical position. If the balloon was not at the predicted geographical coordinates when it had reached observing altitude, the rotating prism would




**TELESCOPE IN ACTION,** after remote command has opened protective doors, is preset to point generally toward Venus when the coarse sun sensors mounted on the housing above the reaction wheel point toward the sun. Servomotors adjust the telescope's elevation against the restraint of gravity; the wheel provides mass required for azimuth adjustment.



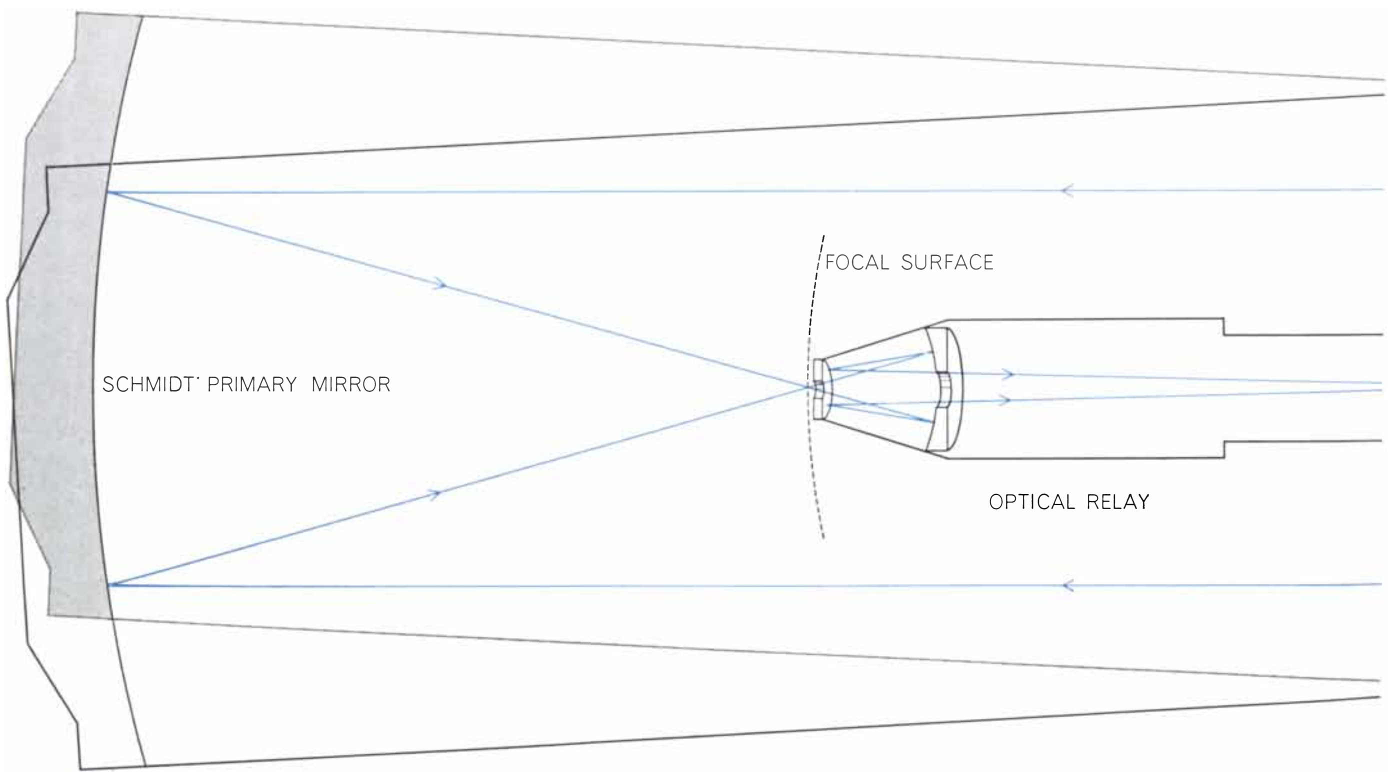

**TARGET ACQUISITION,** the fine pointing required after the balloon-borne telescope is generally oriented, is the task of a refined sun sensor that is programmed to swing the telescope through a search arc. The angle $\beta$ defines the radius of a circle, centered on the sun. The planet Venus will be located within narrow limits along one arc of this circle.




**RADIATION "WINDOWS"** in the normally opaque atmosphere are the clear areas shown in the partial electromagnetic spectrum (*top*). Two major windows admit visible light (*left*) and radio waves of one-centimeter length and longer (*right*) respectively. Infrared absorption from one to 15 microns (*bottom*) ranges from slight (for example, between eight and 13 microns) to total. The absorptive elements are identified by the hatching, horizontal for water vapor, diagonal for carbon dioxide and vertical for ozone.

be set in motion. This action would cause the servomechanisms guiding the telescope to follow a circular search path until Venus was brought before the star-tracker that controlled the telescope's optical relay.

The star-tracker was another key component that had to be redesigned for daylight observations. Although to human observers the sky at 80,000 feet seems almost black, it is still bright enough to make selective detection of the light from Venus an impossible task for an unassisted phototube. After a considerable amount of work Morris Birnbaum of the Impro Corporation devised a daytime star-tracker that would use image-dissecting techniques and photomultipliers to single out the visible light of Venus from the glow of the sky and so control the pointing servomechanisms. We tested Birnbaum's star-tracker on the ground at sunset, when the task of differentiating between sky




**DESIGN OF TELESCOPE** for high-altitude observations puts the burden of accurate pointing on a lightweight interior component rather than on the entire heavy instrument. The oversized primary mirror of the Schmidt telescope can be as much as three degrees out of alignment, yet the optical relay, linked to a star-tracker, will deliver a secondary image to the instrument's monocentric point glow and the light from Venus is about an order of magnitude more difficult than it would be at high noon 17 miles up. The apparatus locked on Venus perfectly.

Preparations for our flight began shortly before dawn on February 21 at Holloman Air Force Base near Alamogordo, N.M. At first the inflation of the balloon was delayed by wind, but helium was piped into the polyester bag beginning at 9:30 A.M. and the launching was successfully completed at 10:46 A.M. The balloon required more than two hours to reach observing altitude; it was 1:15 P.M. when rough orientation of the instrument package was achieved. The doors at the end of the telescope opened on command shortly after 1:30 P.M. The lower door shielded the optics of the telescope from the bright earth below; the upper door shielded them from the bright balloon above. Stretched between the doors at one side was a piece of fabric that shaded the instrument from the direct rays of the sun.

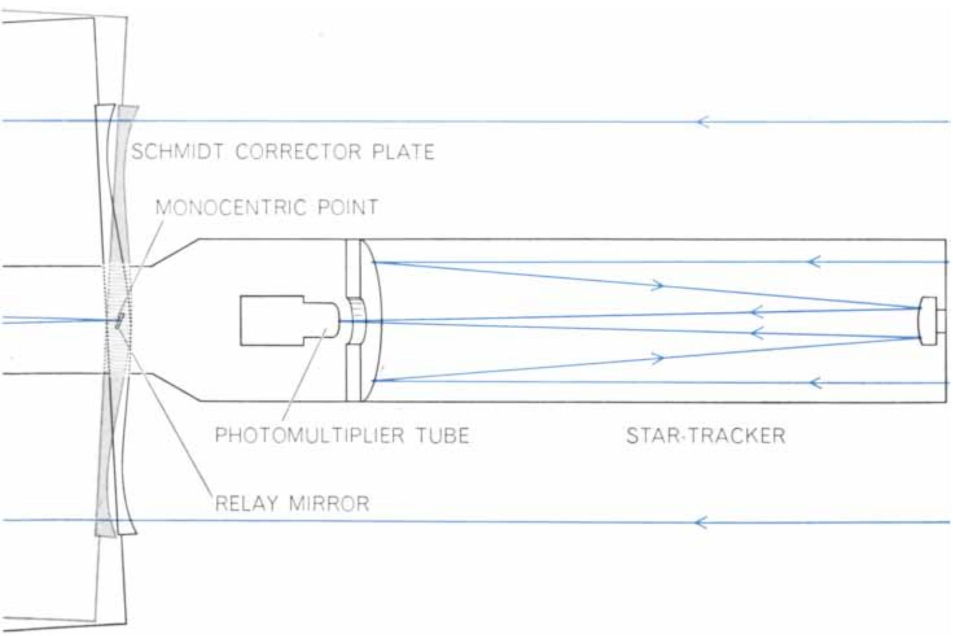
The course plotted for the balloon on the basis of wind predictions was not realized; by the time the apparatus went into fine-scale operation to locate Venus the balloon was 100 miles from the anticipated position and it was necessary to run the sun sensor's prism through its scanning cycle. The first leg of the scan happened to be in the wrong direction, but it was only nine minutes before the prism brought the star-tracker into line with Venus. Once locked on, the planet was solidly tracked for the next two hours.

The entire flight had a single objective: to measure the amount of solar infrared radiation reflected by Venus at the wavelengths absorbed by water vapor. In making such a measurement we would normally have been faced with two spectroscopic alternatives. The spectrometer would spread the wavelengths received into a spectrum by means of a diffraction grating; either the grating could be moved back and forth and various infrared wavelengths passed through a fixed slit to the detection apparatus, or the grating could be kept fixed and the slit moved back and forth instead.

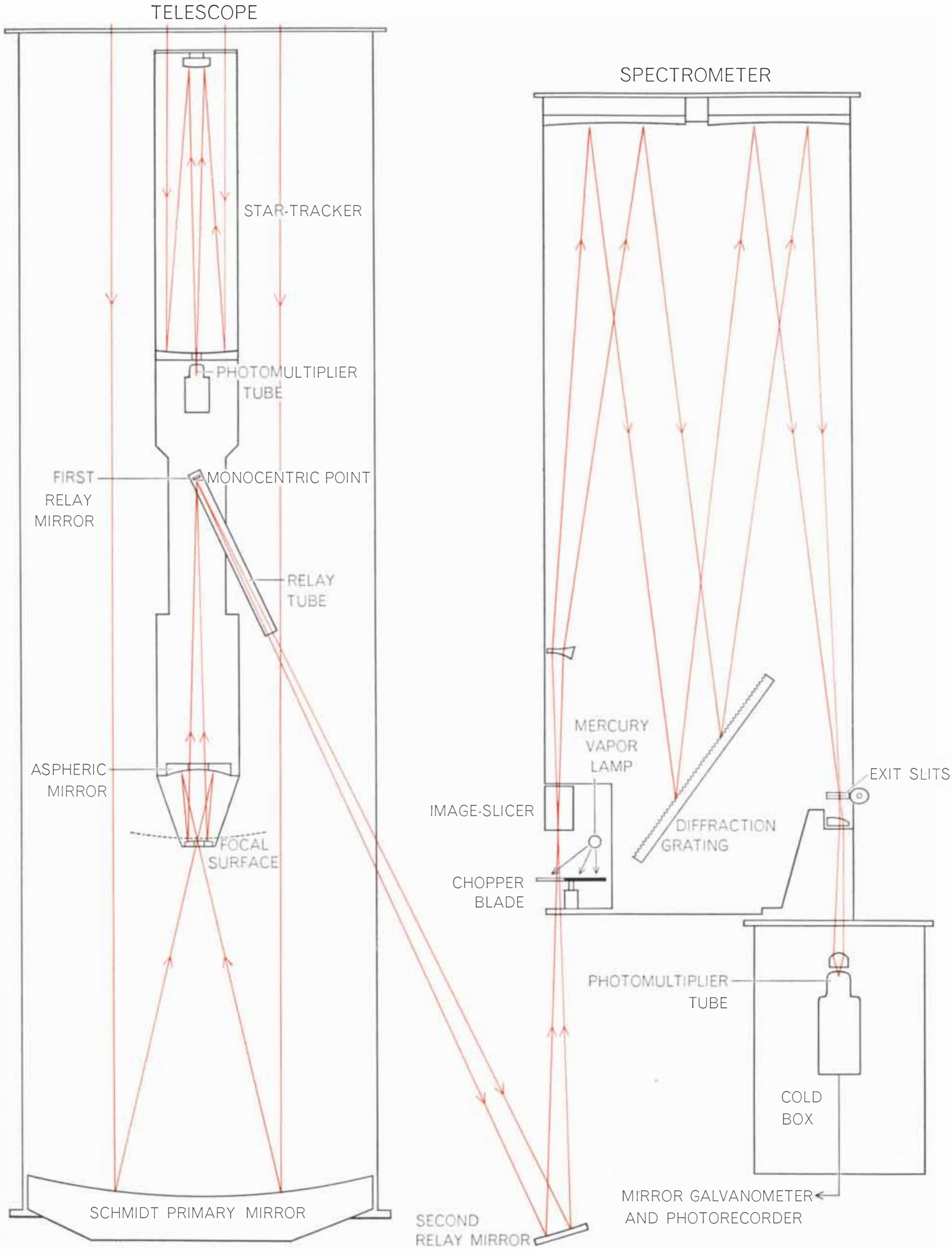
We chose a third alternative. Thanks to the ingenious suggestion of W. S. Benedict, our spectrometer had not one slit but 23 slits, each located at the position of a water-vapor absorption line in the band at the infrared wavelength of 1.13 microns [*see bottom illustration on page 36*]. This stratagem greatly increased the amount of energy that could pass through the spectrometer; a single slit wide enough to pass as much energy as 23 slits would not have been able to distinguish the absorption lines. In order to compare the amount of absorption at the positions of the 23 lines with the absorption in adjacent regions, the slits were moved back and forth across the spectrum once every 10 seconds. The resulting readings were not the usual kind of absorption spectrum, with the absorption lines spread out in orderly array; the readings would simply indicate whether or not the absorption at all the water-vapor lines was greater than that in the adjacent regions. In effect we sacrificed a spectrum in order to obtain a quantitative chemical finding.

The February 1964 flight provided both qualitative confirmation and quantitative refinement of the pioneering observations made in 1959. Analysis of the spectrometric scans indicated that the amount of infrared radiation absorbed by the atmosphere of Venus at the water-vapor lines, compared with the amount absorbed in adjacent spectral regions, was 10.5 percent plus or minus .5 percent. If one takes into account differences in calibration, this reading overlaps one extreme of the earlier one, namely 5 percent plus 4 percent. In addition, once the Venus readings were completed, a reflecting screen was moved into place to feed radiation from the sun directly into the spectrometer. The absorption, caused by the water vapor in the earth's atmosphere above the balloon, was only about a twentieth of that for Venus, or roughly .5 percent.

If further proof is needed that the reading from Venus is genuine and not the result of terrestrial water vapor, and that all but a twentieth of the water vapor detected in the February flight was present in the atmosphere of Venus, another observation can be cited. This involves a comparison of the predicted and observed Doppler shift in the absorption lines as a result of Venus' motion with respect to the earth. William Plummer noted that in the course of one scan three of our spectrometer slits would cross a wavelength that happened to be the wavelength of one of the emission lines of the element mercury (11,287 angstrom units, or 1.1287 microns). This allowed him to use a mercury-vapor lamp for purposes of comparison. He closed two of the three slits; near the end of each scan the third slit allowed the light of the mercury line




**with an error no greater than three seconds of arc. The primary mirror is 16 inches in diameter and the corrector plate is 12 inches in diameter. The overall configuration illustrated produces a highly efficient instrument of reasonable weight with an *f*/1.5 focal ratio.**




**TWIN INSTRUMENTS** are connected in the manner illustrated on this page. A relay mirror at the monocentric point (*left*) reflects the image formed by the servo-controlled optical relay to a second mirror, outside the telescope, that passes the collected infrared rays to the entrance slit of the spectrometer (*right*). Before that the circular beam is not only chopped into pulses but also sliced into a linear shape suitable for spectroscopy. The diffraction grating then spreads the narrow band of radiation into a nearly inch-wide display that impinges on 21 exit slits, each of them matching a point at which water vapor absorbs infrared waves. In this illustration the Schmidt corrector plate is absent; one aspherical unit in the all-mirror system corrects the primary mirror's aberration.

to reach the recorder and provide a fixed standard against which the shift of the water-vapor lines could be measured. The Doppler shift was predicted to be .495 angstrom; the measurement on our February flight was .49 angstrom plus or minus 10 percent. With such a high degree of independent agreement there is little reason to doubt the accuracy of the other spectrometer observations.

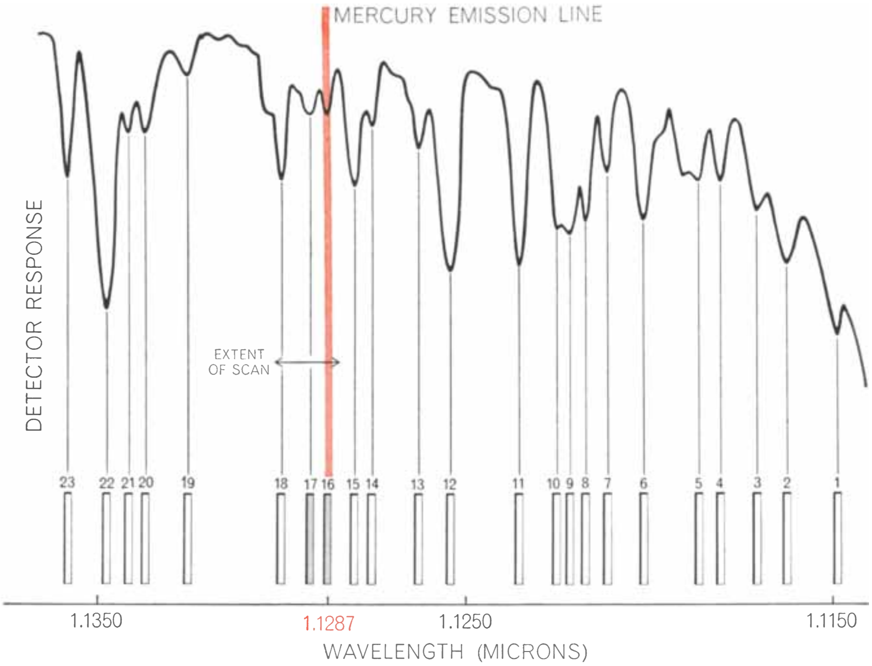
After our February flight had been successfully concluded we decided that the next task for our infrared instrument package was to determine the nature of the clouds in the atmosphere of Venus. Was this obscuring layer of particulate matter composed of water? Or was it perhaps ice, dust or even complex polymerized organic molecules? We were encouraged to look into the matter by a major improvement in our telescope.

In order to analyze the clouds of Venus we needed to extend the range of infrared detection to the wavelengths around 3.5 microns. The quartz corrector plate of our telescope, however, was opaque to wavelengths well below 3 microns. A new correcting system using nothing but mirrors and hence effective at any infrared wavelength was developed from a design by E. W. Silvertooth of Custom Instruments, Inc., to replace the Schmidt plate.

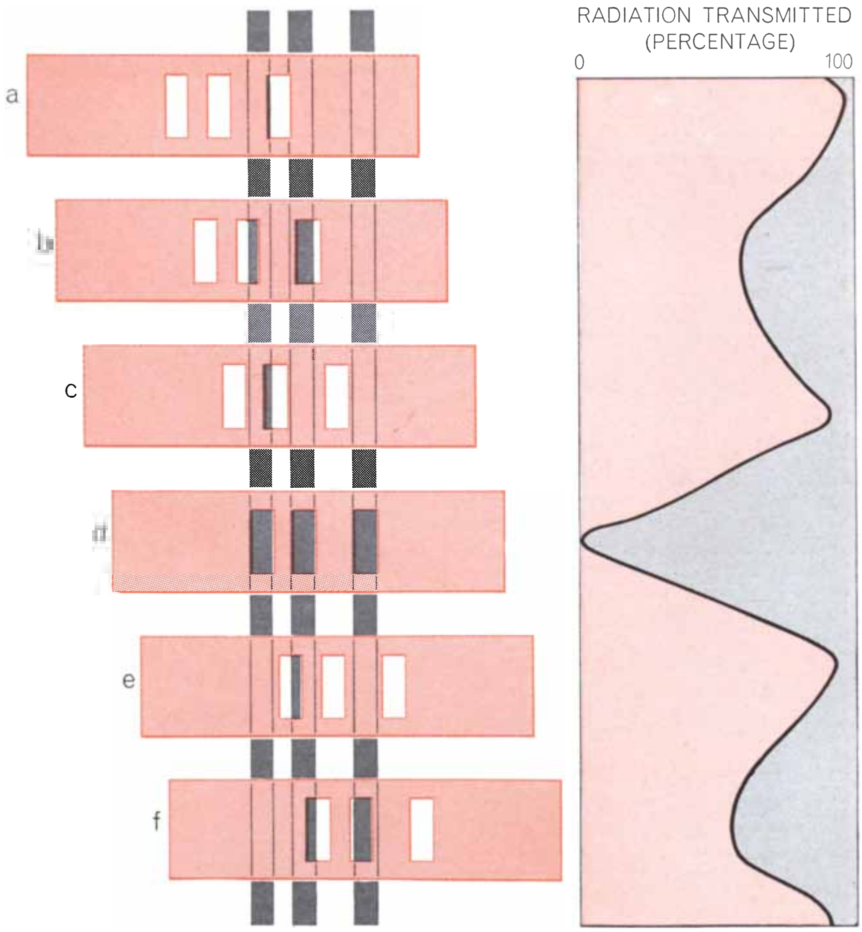
We launched our improved instrument on October 28 of last year. In addition to modifying the telescope, we had altered the spectrometer to suit the task of obtaining an infrared spectrum of Venus' cloud layer. A coarser diffraction grating was substituted for the finer grating used during the February flight; this gave us a broader spectrum of wavelengths. Three detector tubes, mounted side by side, acted as three independent slits; the diffraction grating was rocked back and forth to make the spectral lines pass across the detectors. In this way we could record infrared wavelengths from 1.7 microns to 3.4 microns simultaneously with all three detector tubes.

At an altitude of 86,000 feet the telescope's automatic pointing and tracking devices brought it into line with Venus and held the planet's image steady for more than three hours. The spectrometer not only scanned the infrared spectrum of Venus but also made comparison scans of the solar spectrum; this enabled us subsequently to determine the spectrum characteristic of the planet.

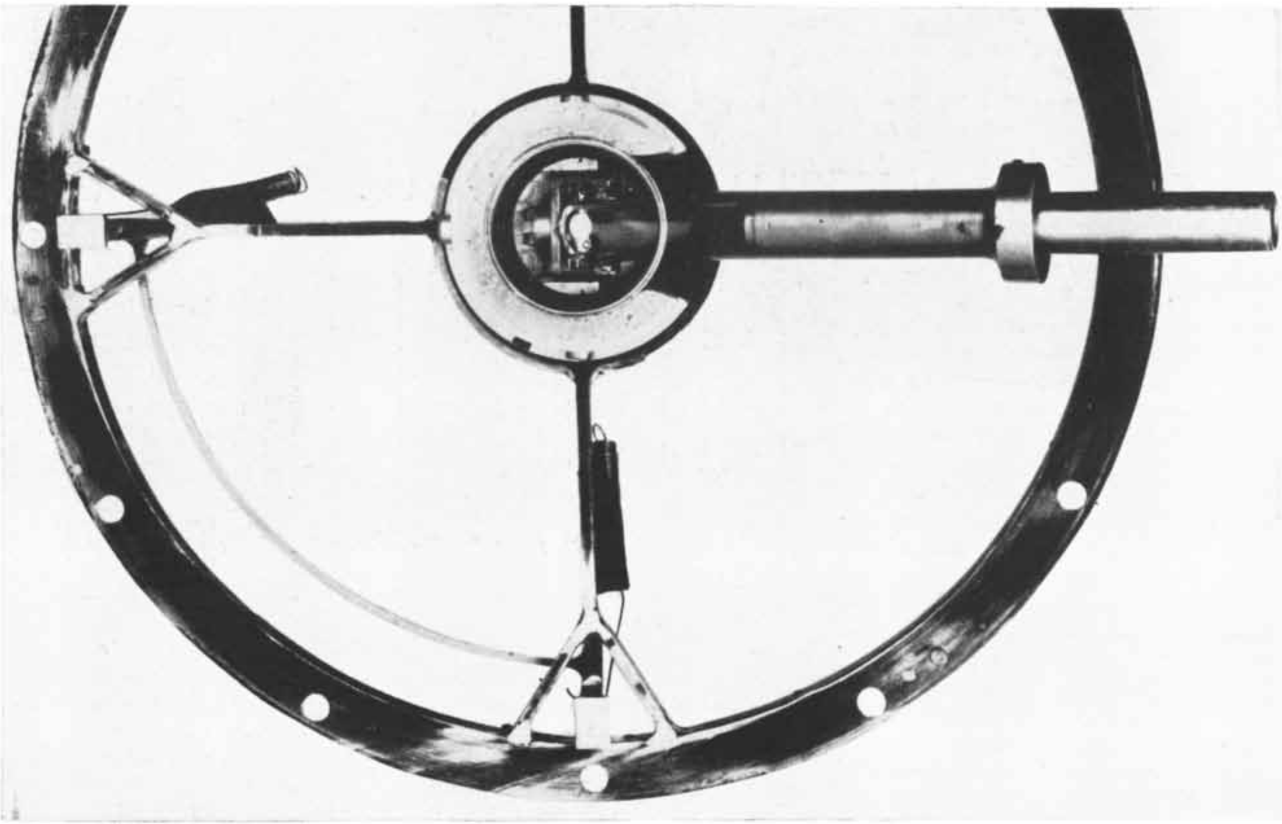
This spectrum of wavelengths is presented in the form of a curve, the peaks of which correspond to the wavelengths



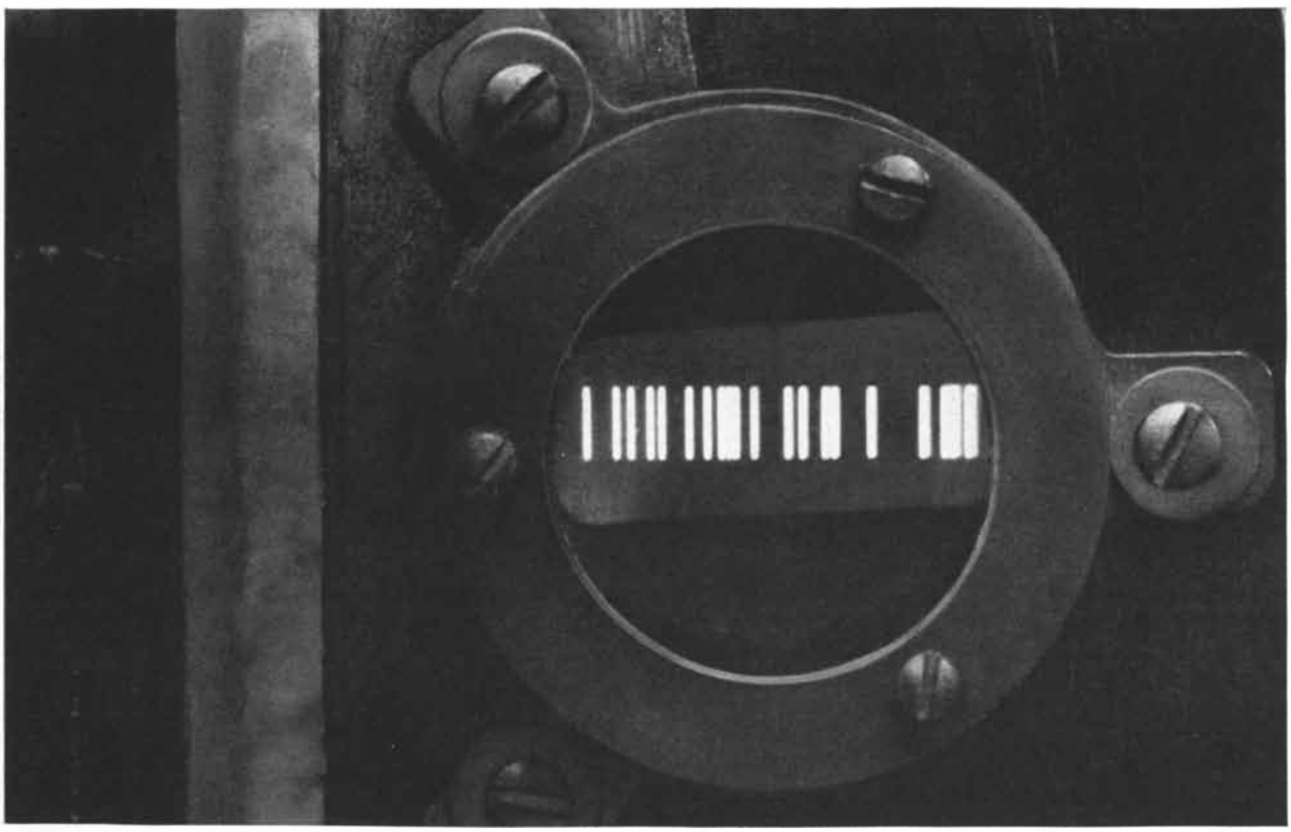

**LABORATORY FINGERPRINT** of water-vapor absorption points in the infrared wavelengths between 1.135 and 1.15 microns is matched with the positions of the spectrometer exit slits. This abundant array of slits literally multiplies the amount of energy that reaches the detector. The double-headed arrow indicates the extent of each left-to-right scan.




**EXIT-SLIT ARRAY** is moved from side to side continuously during observations; the purpose of this oscillation is shown in the schematic illustration. When no slits coincide with the absorption lines ("*a*," *top*), maximum radiation is recorded (*graph at right*). As slits and lines coincide ("*d*," *fourth from top*) maximum absorption minimizes signal strength.



RELAY MIRROR, supported by struts at the monocentric point of the telescope, is suspended on gimbals so that it moves in unison with the optical relay. It reflects the secondary image through the brass tube (*right*) that leads to the adjacent spectrometer.



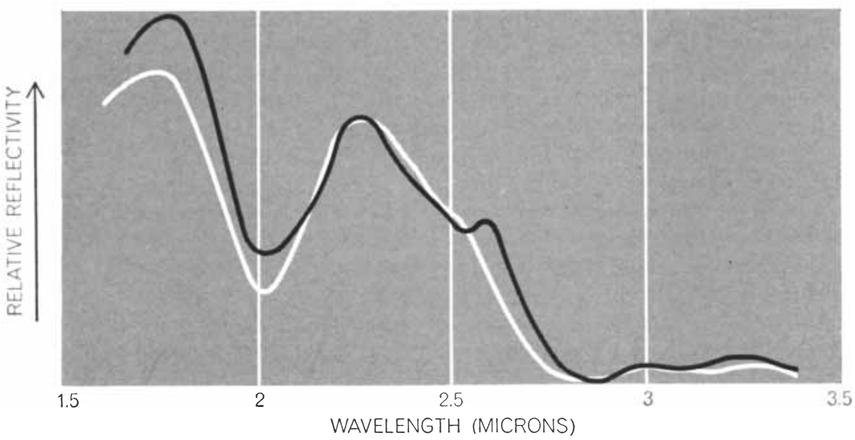
EXIT SLITS of the spectrometer are etched through a thin plate of stainless steel. They total 23 in number, but two of them were closed during the February flight, allowing a third adjacent slit to pass a calibrating spectral line of mercury once in each scan.

most intensely reflected by Venus. As we have reported in *The Astrophysical Journal* for November 15, 1964, we compared the curve with similar reflection spectra obtained in our laboratory by Rodolphe Zander. These included the spectra of silica sand, liquid formaldehyde, a petroleum oil, liquid water, ice, solid carbon dioxide, a cloud of frozen carbon dioxide particles, ordinary frost, a cloud of water vapor and a cloud of ice crystals. As the bottom illustration on this page indicates, the Venus curve closely corresponds to the reflection spectrum of a cloud of ice crystals. The other reflection spectra do not fit the Venus curve. The conclusion appears inescapable: the reflecting cloud layer on Venus is composed of ice crystals. This fits in very well with our earlier detection of absorption lines due to water vapor above the clouds.

At present we are designing an infrared telescope with a 48-inch aperture, a fourfold scale-up of our present instrument. We expect to use this larger telescope with a balloon, but there is no reason why infrared astronomy should be confined to the upper atmosphere. Mountaintop observatories have made their contributions in the past; for example, my colleague William M. Sinton and I were able to make precise observations of the infrared emissions of the moon, Mars and Venus in 1952 with the 200-inch telescope on Palomar Mountain. The principal drawback is that existing mountain observatories are not high enough and cold enough. The infrared study of a cold object—such as a dark part of the moon, or the planets Venus, Mars, Jupiter and Saturn—is quite difficult when the observer must look out through the earth's own hotter atmosphere. It seems feasible that for specific, short-term observing projects a relatively modest instrument, such as our planned 48-incher, could be temporarily placed at very high mountain sites, where the problems arising from air temperature would be reduced to a minimum.

A number of infrared projects are peculiarly suited to the artificial satellite. Foremost among them are studies that would require extended observation times, for instance a systematic infrared survey of the entire celestial sphere. As long as the satellite instrument's pointing history was known and the scanning eventually covered all of the sky, the exact programming of scans would be unimportant. It is certain that many celestial objects that are presently invisible would be discovered.

**PREDAWN INFLATION** of the polyester balloon proceeds under floodlights at Holloman Air Force Base, N.M., in preparation for the October 1964 observations of the cloud layer of Venus. Polyester proved a far more reliable balloon material than polyethylene.




**ICE-CRYSTAL COMPOSITION** of the clouds of Venus was discovered during the flight of October 28, 1964. Spectrometer readings covered a range from 1.7 through 3.4 microns (*white curve*). When this curve was compared with the laboratory reflection spectra of various substances, the closest match was with an ice-crystal cloud (*black*). If the Venus curve is adjusted to minimize the low hump at 1.8 microns (due to water vapor absorption) and the deep dip at 2 microns (due to carbon dioxide), the correspondence of the two curves is further increased. These data first appeared in *The Astrophysical Journal*.

# The Undercooling of Liquids

*A substance is said to be "undercooled" when it remains liquid below its "equilibrium crystallization temperature," or nominal freezing point. The study of such liquids has shed light on the freezing process*

by David Turnbull

The first systematic study of how water freezes into ice was undertaken more than 200 years ago by the German physicist Gabriel Fahrenheit. Earlier investigators had reported that the freezing temperature of water appeared to vary considerably from experiment to experiment, but Fahrenheit hoped nonetheless to use this temperature as a calibration point in a new system of thermometry. He soon found that although the temperature at which freezing *began* in different water samples was indeed widely variable, this temperature was never *higher* than a characteristic value (0 degrees centigrade, or 32 degrees on the scale Fahrenheit eventually devised). Moreover, he observed that just after freezing began the temperature of the mixture of water and ice rose rapidly, leveling off at the maximum freezing temperature. He also noted that freezing invariably began with the formation of one or more tiny crystallization centers (a process that has since come to be known as "nucleation") and then continued by the rapid propagation of crystallization fronts outward from these centers. It became evident to Fahrenheit that the freezing of water takes place entirely at the interfaces between the ice and the water and that the rise in temperature observed just after nucleation is caused by the liberation of the heat of crystallization at the rapidly moving freezing front.

From these findings Fahrenheit concluded that a mixture of water and ice should reach a constant and reproducible temperature that would be suitable as a calibration point in thermometry. This temperature, which is properly termed the "equilibrium crystallization temperature," is defined as the temperature at which the interfaces separating the water from the ice (or the liquid phase of any substance from its crystalline phase) are stationary. Above or below the equilibrium temperature the interfaces move, in directions corresponding to melting at higher temperatures and freezing at lower ones. In spite of Fahrenheit's findings, the misleading terms "melting point" and "freezing point" are still commonly used to denote the equilibrium crystallization temperature.

When a substance is cooled below its equilibrium temperature, it is said to be "supercooled" or "undercooled." Thus water is undercooled when it remains liquid at temperatures below 0 degrees C. Under special conditions very pure water can be undercooled to as low as −40 degrees C. without crystallizing into ice, and many liquids can be undercooled to an even greater degree. Our understanding of the undercooling of water and other liquids is still in a comparatively primitive state. Since World War II, however, research in this area has been carried out at the General Electric Research Laboratory and elsewhere; as a result a much clearer picture of the behavior of undercooled liquids is now emerging.

The amount of undercooling that different samples of water will sustain before the onset of freezing frequently varies by as much as 15 degrees C.; early experimenters reported similarly large variations for other liquids. In contrast, it was observed that the amount of undercooling sustained by a particular sample of a liquid before initial freezing rarely fluctuates more than 1 degree C. through a succession of melting and freezing cycles. This "critical point" of undercooling can be substantially raised by vibrating the sample; it can be lowered by increasing the rate of cooling, but this shift is very small for very large changes in the cooling rate. It follows that undisturbed liquids can remain in the undercooled state for extremely long periods of time. Samples of the metal gallium, for instance, which has an equilibrium crystallization temperature of 29.75 degrees C. (slightly higher than normal room temperature), have been kept molten at temperatures from 5 to 15 degrees C. below their equilibrium temperature for more than a year. This behavior is particularly remarkable in view of the fact that the freezing of such liquids is usually completed within a few seconds or minutes (depending on the size of the sample) after nucleation occurs.

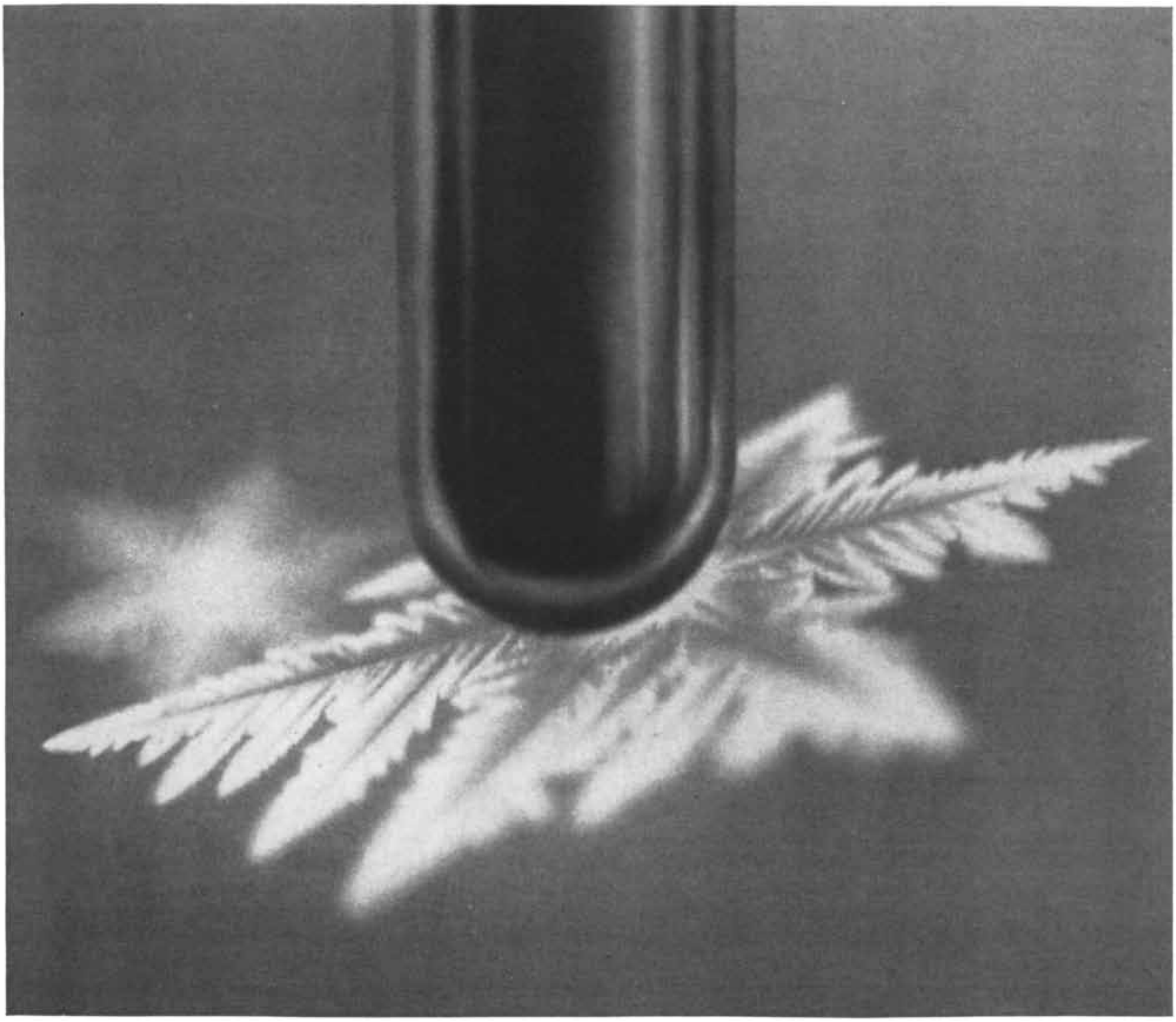
It is the apparent capriciousness of the onset of freezing that makes any attempt to describe the freezing process in detail so difficult. Since the crystallization fronts, once they are formed, usually propagate quite rapidly, this capriciousness must reflect some basic characteristic of crystal nucleation. The problem of understanding the freezing process therefore depends in large measure on finding an effective technique for investigating the phenomenon of nucleation.

It seems clear that the critical point of undercooling associated with a particular sample of liquid actually reflects the amount of undercooling required for the formation of the first crystal nucleus in the sample. The near constancy of this critical value for a particular sample compared with its wide variation among samples of the same liquid suggests that it cannot be an intrinsic property of the liquid. Such behavior is consistent with the interpretation that the initial freezing nuclei are formed on irregularities extrinsic to the liquid; at a given cooling rate each of these irregularities would require a specific critical undercooling to serve as an active crystal nucleus. The proposed irregularities could be either impurity particles (commonly called "motes") suspended in the liquid or certain regions of the container walls. In either case nucleation initiated at such irregularities is called heterogeneous nucleation, as opposed to homogeneous nucleation, which occurs spontaneously within the liquid without the aid of extrinsic irregularities.

Since at a given cooling rate each mote requires a specific critical undercooling to become effective as a crystallization center, freezing will always begin at the single mote for which the critical undercooling is least; thus the observed critical undercooling of a particular liquid sample would be nearly constant through repeated meltings and freezings. As a rule motes are present more or less inadvertently in a liquid; consequently the mote that initiates freezing in one sample might be quite different from the mote that initiates freezing in another. This would explain the observed wide variation of initial freezing temperatures among samples of the same liquid.

The single mote that initiates crystallization in a body of liquid might constitute no more than one part in $10^{15}$ of the body's volume. It is difficult enough to control the impurity content of any substance to within one part in a million ($10^6$); to do so within one part in $10^{15}$ in a relatively large body of liquid is practically impossible. This means that crystallization in any liquid body larger than, say, a thousandth of a cubic centimeter will almost always be initiated by the mote effect. Since the temperature of a liquid rises immediately after the formation of the first crystal nucleus, the action of a single unspecified mote can thwart experiments aimed at observing homogeneous nucleation or even heterogeneous nucleation on specified test surfaces.

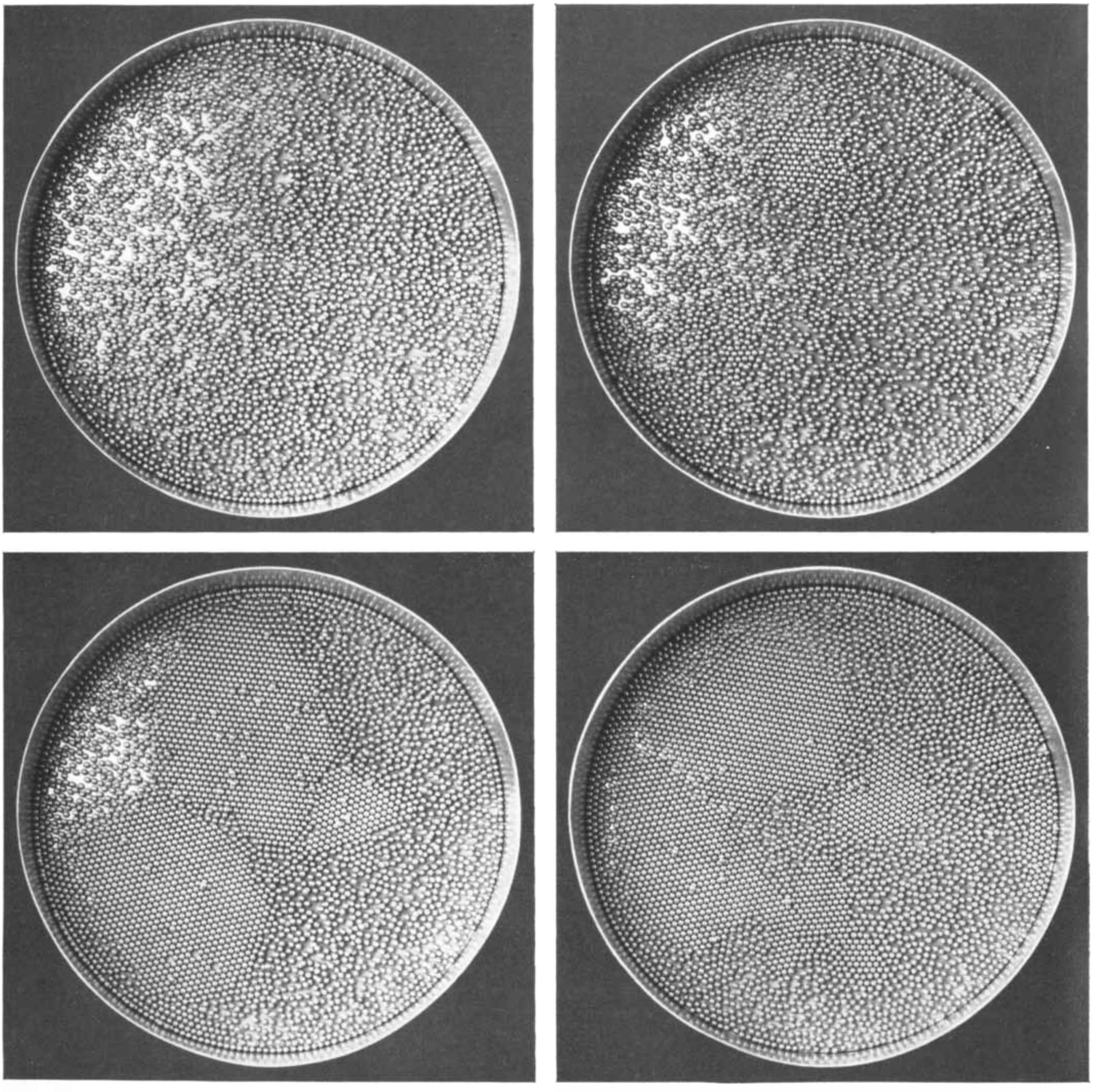
An apparently effective technique for eliminating the mote effect in a large liquid body has only recently been recognized and exploited. The liquid is first subdivided in one of several ways into a large number of small droplets, each of which is isolated from the others by simple physical separation or by a



**ICE CRYSTAL is shown growing at the base of a thermometer that has been immersed in a body of undercooled water. The "dendritic," or treelike, growth of the crystal, which resembles that of a snowflake, is characteristic of many substances that crystallize in their undercooled liquid phase. The photograph was made by R. B. Williamson and Bruce Chalmers of Harvard University.**

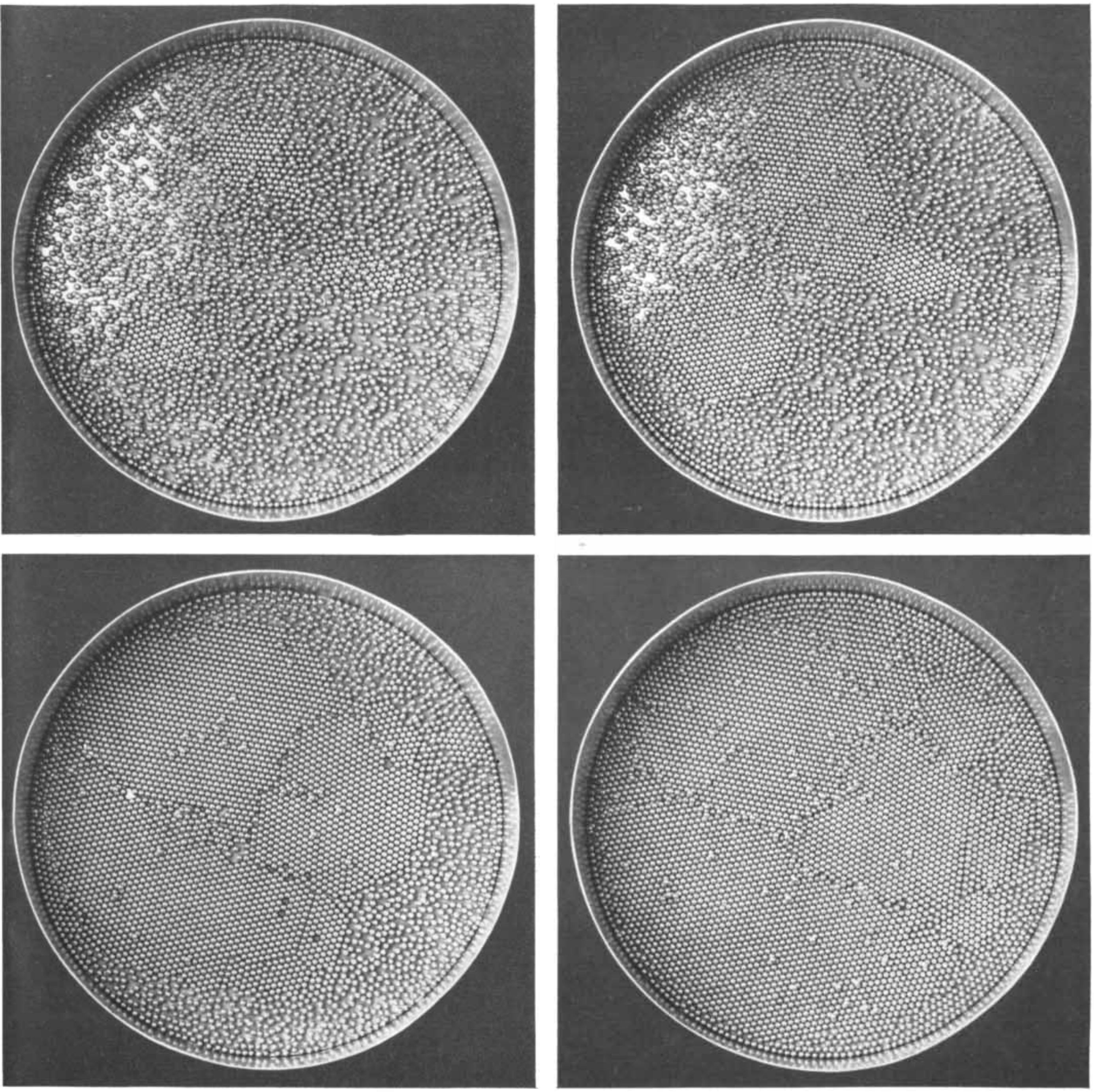
thin coating of some second substance on the surface of the droplets [*see illustration on page 42*]. The liquid can be subdivided by agitating it in an ordinary kitchen blender with a suitable film-forming substance. In such an aggregation of droplets an independent nucleation event would be required for the crystallization of each droplet. Consider, for example, a liquid body in which there are roughly a million motes per cubic centimeter. If the liquid were to be dispersed into tiny droplets, each with a volume of about a hundred-millionth of a cubic centimeter, an average of no more than one droplet in every 100 would contain one of the original motes. The vast majority of the droplets would be uninfluenced by the mote effect and could presumably be undercooled to a temperature at which homogeneous nucleation would occur.

The probability of a crystal nucleating spontaneously in a homogeneous liquid is of course proportional to the volume of the sample. Hence even after the motes are effectively isolated, a further reduction in the size of the droplets should result in a corresponding increase in the critical undercooling required to crystallize the liquid. Since the nucleation frequency rises sharply with increased undercooling, however, this ef-

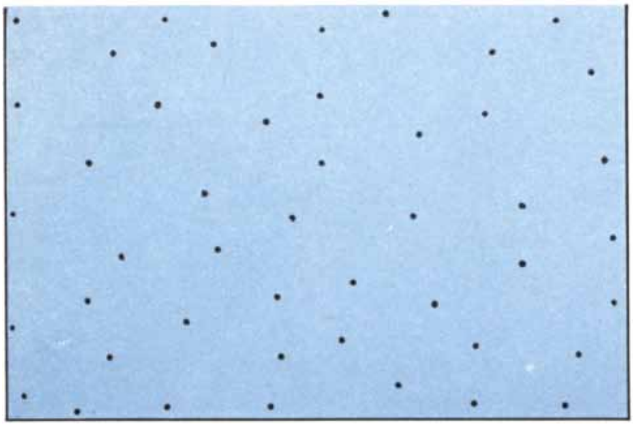


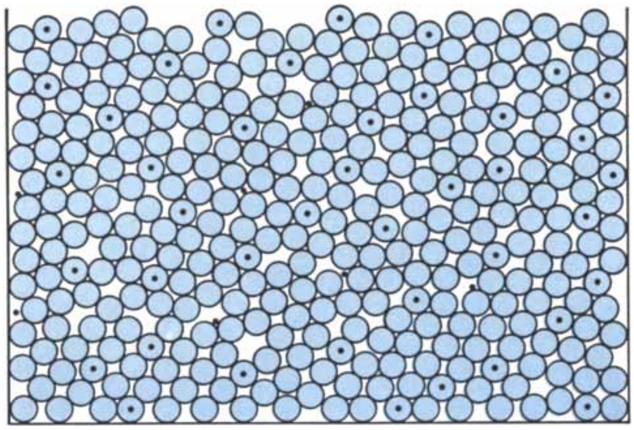
**TWO-DIMENSIONAL MODEL** of crystal nucleation and growth in an undercooled liquid was photographed at the General Electric Research Laboratory. The tiny aluminum balls, which represent atoms, are kept in continual motion by means of an electric vibrator mounted under the container; for these photographs the action has been stopped at several stages by a stroboscopic flash lamp in order to show the growth of the crystals. Crystallization was induced in the model by coating the balls in the crystalline fect is generally small compared with the effect of isolating the motes.

The fact that some substances can sustain much greater undercooling in droplet form than in bulk has been known for nearly a century. Assayers have long been familiar with the momentary brightening that often occurs just as molten gold globules begin to crystallize. This sudden brightening (sometimes called the "blick" effect, from the German word meaning "flash") is caused by the heating of the undercooled, but still red-hot, liquid gold by the heat of crystallization released at the rapidly moving crystallization front. Molten gold globules can frequently sustain critical undercoolings of several tens of degrees C. below their equilibrium crystallization temperature (1,063 degrees C.), whereas the critical undercooling sustained by gold in bulk form is barely measurable. By observing the blick effect in various other metals C. E. Mendenhall and L. R. Ingersoll of the University of Wisconsin reported in 1908 that quite large critical undercoolings were sustained when the metals were in the form of liquid droplets only .05 millimeter in diameter; they were able to undercool molten platinum in this manner by as much as 370 degrees C. below its equilibrium crystallization



**areas with an alcohol film, which simulates the cohesive forces between the atoms. In this respect the model is imperfect, since in an actual substance the interatomic forces are the same in the liquid as in the solid. Nonetheless, the model is thought to give a good representation of the changes in atomic arrangement that occur as a crystal grows in its liquid phase. The first crystal appears in the second photograph from the left in the top row. The last photograph in the series shows a hypothetical crystalline solid.**





**DROPLET TECHNIQUE is useful for controlling the effect on nucleation of "motes," or impurity particles (*black dots*), which are usually present more or less inadvertently in any large liquid body (*left*). By vibrating the liquid vigorously in an ordinary kitchen blender with another substance, the test liquid can be dispersed into a large number of small droplets (*right*), each of which is isolated from the others by a thin coating of the second substance. The vast majority of the droplets would be uninfluenced by the mote effect and could presumably be undercooled to a temperature at which homogeneous nucleation would occur.**

temperature (1,773.5 degrees C.) before the first crystal nucleus appeared.

The potential usefulness of the droplet technique as a research tool did not begin to be fully appreciated until the late 1940's, when Bernard Vonnegut and Vincent Schaefer initiated their extensive investigation of the freezing process at the General Electric Research Laboratory. Stimulated by their experiments, my colleagues and I developed the droplet technique still further, applying it to the study of crystal nucleation in a large number of metallic liquids and even in a few organic liquids. We were able to observe the crystallization of individual droplets visually with the aid of a microscope. In addition we undertook to measure the changes in bulk properties—such as volume and energy content—that usually accompany crystallization. Perhaps the most definitive findings were obtained from the comparatively few volumetric studies of large aggregations of droplets. I shall describe these experiments first because they provide a valuable groundwork for an interpretation of the more complex findings.

In the volumetric experiments large numbers of droplets are isolated from one another by thin coatings of some inert substance and then placed in a device called a dilatometer [*see illustration on opposite page*]. The change in volume accompanying crystallization is calculated from the change in the level of the inert indicator fluid in the thin capillary tube at the top of the dilatometer. If the time required for the crystallization front to move across the droplet is short compared with the time required for spontaneous nucleation to occur, the change in volume directly reflects the occurrence of nucleation. This condition was satisfied in our experiments by adjusting the undercooling to produce nucleation periods measured in hours as compared to crystal-growth times measured in milliseconds. Since we knew the distribution of the droplets by size in the sample, we were able to calculate the mean nucleation frequency from the crystallization rate of the entire sample. At the same time we were also able to determine whether the probability of nucleation in individual droplets was proportional to droplet area (as it would be if the surface film encouraged nucleation) or to droplet volume.

The most detailed volumetric studies were made of aggregations of mercury droplets coated with various films. The equilibrium temperature of mercury is about −39 degrees C.; we observed that the critical undercooling sustained by our samples ranged from a low of about 2 degrees C. to a high of nearly 80 degrees, depending on the nature of the coating on the droplets. In contrast, the maximum undercooling sustained by a large body of mercury before the onset of crystallization is seldom greater than a degree or two below its equilibrium temperature.

Among other things, the results of the volumetric experiments showed that there is a wide variability in the effectiveness of different kinds of impurity coating in promoting crystal nucleation. Aged mercury stearate, for example, promotes copious nucleation in mercury at small departures from the equilibrium temperature. Presumably impurities of this kind initiate freezing in large bodies of mercury that have been only slightly undercooled. Droplets coated with mercury laurate or mercury benzoate, on the other hand, sustained undercooling by as much as 77 degrees C. below the equilibrium temperature. It is only in these droplets that nucleation may have occurred homogeneously, that is, without the aid of motes or impurity films. In order to determine if nucleation was indeed homogeneous in these droplets we measured the crystallization rate of aggregations of the laurate-coated mercury droplets at several temperatures. Analysis of these data revealed that the nucleation frequency per unit of volume was uniform for all the droplets in a sample in a given temperature and that the probability of nucleation occurring in a given droplet was proportional to the volume rather than to the area of the droplet. These results constitute strong, but not absolute, evidence that it was homogeneous and not heterogeneous nucleation that was observed in the experiments. The variation of nucleation frequency with undercooling in the vicinity of the critical undercooling temperature for mercury is represented graphically by the broken colored curve in the top illustration on page 44. The experimental results are indicated by the solid part of the curve, which extends only over the limited frequency range accessible to observation.

According to the simplest theory of nucleation the rise in nucleation frequency with increased undercooling near the critical undercooling temperature should be steeper for substances capable of sustaining only very small undercooling. This prediction was strikingly confirmed by the dilatometer measurements on certain hydrocarbons with long-chain molecules, in which homogeneous crystal nucleation appears to begin at temperatures only a few degrees below the equilibrium temperature. As the top illustration on the next page shows, the nucleation frequency in the hydrocarbon n-octadecane ($C_{18}H_{38}$) increases by a factor of about 8,000 per degree C. of undercooling in the critical region. The magnitude of this response to extremely small changes in temperature, one of the most pronounced ever recorded for any physical process, is close to the value predicted by the simple nucleation theory.

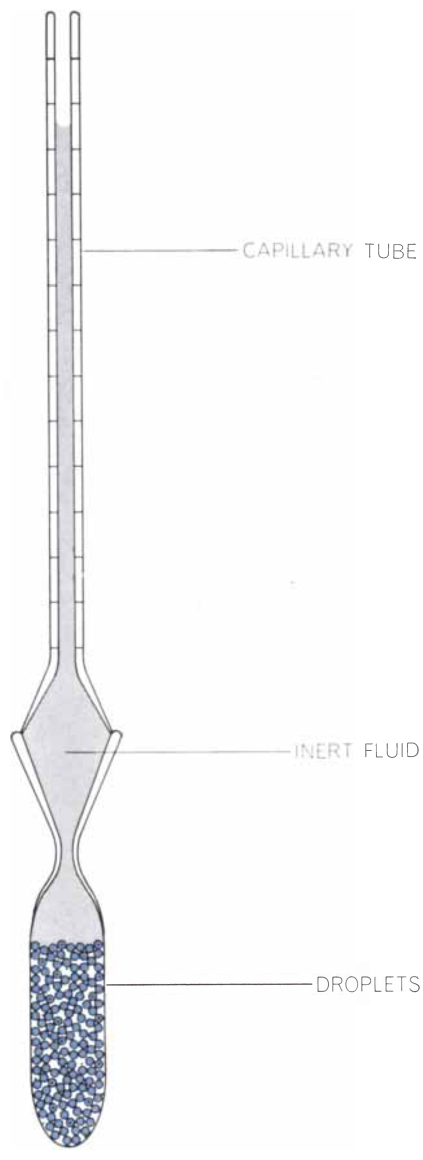
In view of the generally steep rise in nucleation frequency with increased undercooling near the critical temperature, it is not surprising that some observers have been led to conclude that the initiation of freezing requires a specific critical undercooling. Theoretically, however, there should be a finite probability of nucleation for any amount of undercooling greater than zero. Because this probability becomes so small so rapidly, nucleation is practically unobservable at temperatures only slightly higher than the critical value. A cubic centimeter of clean mercury, for example, should sustain undercooling by 65 degrees C. for about 100 days; if the same sample were undercooled by 60 degrees C., it would remain liquid for 10 million years, provided that it was neither seeded nor vibrated.

A prediction that arose out of the volumetric experiments involved the effect of droplet size on undercooling. In general our findings indicated that the amount of undercooling required to produce measurable nucleation in a mote-free sample should increase very slightly with large reductions in the diameter of the sample. In the case of mercury we calculated that the increase in critical undercooling should be only from 67 to 77 degrees C. as the diameter of the sample is reduced from one centimeter to 10 microns [*see bottom illustration on next page*]. Although this prediction is as yet untested for mercury, it has been confirmed experimentally for iron, nickel and cobalt by P. Bardenhaur and his colleagues in Germany and J. L. Walker of the General Electric Research Laboratory. In liquid droplet form all the latter metals can be undercooled by as much as 300 degrees C. or more without crystallizing, whereas in bulk form they can rarely be undercooled more than a few degrees. When coated with a silicate liquid and kept in an atmosphere of inert gas, however, half-pound samples of these metals can often be undercooled by 200 to 275 degrees C. without crystal nucleation. Apparently this unusual resistance to nucleation in bulk form is associated with the high equilibrium temperatures of these metals (all higher than 1,450 degrees C.); at such high temperatures most of the motes would either melt or dissolve.

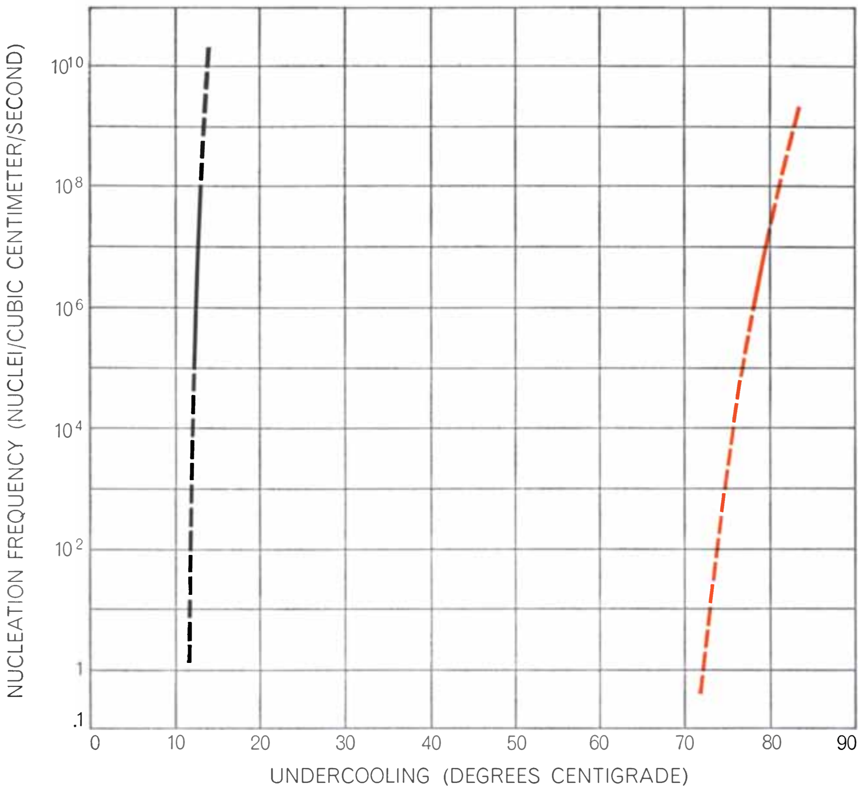
The crystallization of undercooled liquid droplets can be observed optically by means of the blick effect, by roughening of the surface of the droplet or by scintillations produced when light is scattered from the crystal surfaces. These optical techniques are rapid and convenient and have been applied by investigators at the General Electric Research Laboratory and elsewhere to a large variety of liquids. In every respect the results of these tests are qualitatively similar to those for mercury. Some examples of the largest undercoolings recorded optically are: water, 40 degrees C.; n-octadecane, 15 degrees C.; gallium, 70 degrees C.; sodium chloride, 165 degrees C., and copper, 240 degrees C. Presumably the critical undercoolings required for detectable homogeneous nucleation are all equal to or larger than these figures. In addition several simple organic liquids, such as toluene, have been identified in which nucleation rarely occurs in the absence of motes regardless of the amount of undercooling. Unless they are suitably seeded, such liquids will harden into glasses without crystallization on being undercooled sufficiently.

One of the most perplexing questions posed by the results of the droplet experiments is: Why, in view of the high velocities of the crystallization fronts once they are formed, is the resistance of simple liquids to the initiation of crystallization so extraordinarily high? The difficulty involved here is better appreciated when one considers, for example, the results of one of Walker's experiments, which indicated that the crystallization fronts in nickel reach velocities as high as 5,000 centimeters per second at temperatures near 175 degrees C. below the equilibrium temperature for nickel (1,452 degrees C.). This means that at the crystallization fronts the average time required for a nickel atom to go from the liquid to the crystalline state is less than $10^{-11}$ second. Unless they are seeded or vibrated, however, even large masses of nickel may remain liquid indefinitely when undercooled by 175 degrees C.

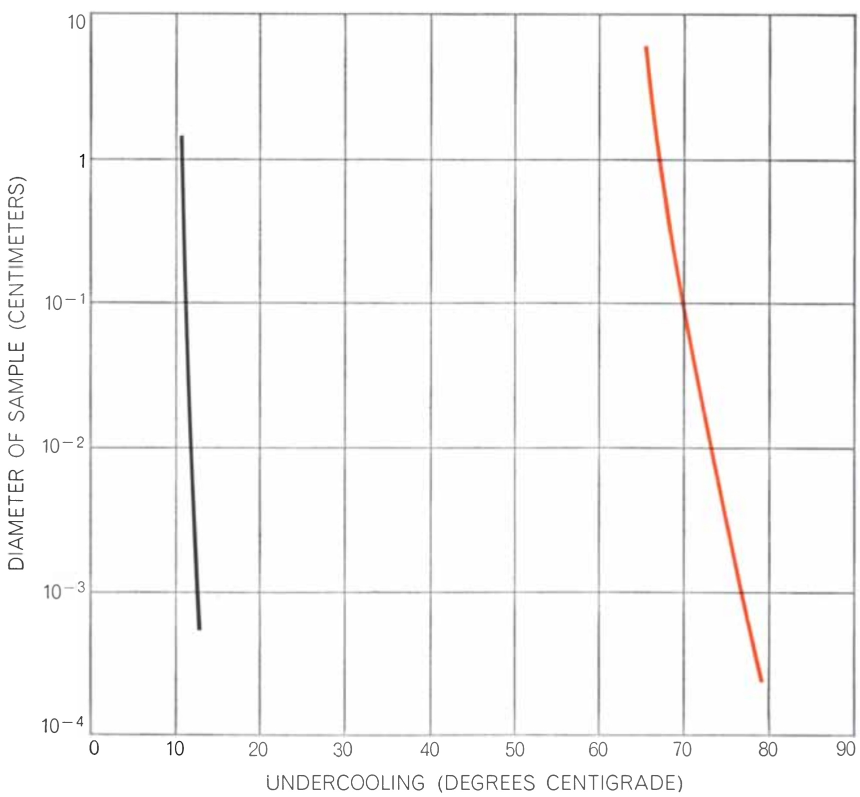
The problem presented by these apparently contradictory findings can be solved in a formal way by supposing that a large amount of work must be done to form the interface between the crystal and the liquid. In other words, the energy density of a crystal immersed in its melt must increase considerably as the size of the crystal decreases; conversely, the stability of the crystal as well as its equilibrium crystallization temperature must decrease with de-




**DILATOMETER measures the effect of crystallization on volume in an aggregation of undercooled liquid droplets. The change in the volume of the test sample is calculated from the change in the level of the inert indicator fluid (*gray area*) in the thin capillary tube at the top of the dilatometer.**




**NUCLEATION FREQUENCY** increases rapidly with increased undercooling. The experimentally obtained variations in the vicinity of the critical undercooling temperature for two substances, n-octadecane (*solid black curve*) and mercury (*solid colored curve*), are depicted in this graph. The extrapolated variations are indicated by the broken parts of the curves.




**UNDERCOOLING REQUIRED** to produce one crystal nucleus per day in a liquid sample increases very slightly with large reductions in diameter of sample. The calculated variations for two substances, n-octadecane (*black curve*) and mercury (*colored curve*), are shown.

creasing crystal size [*see illustration on page 46*].

Regardless of the amount of undercooling, any crystal will be stable enough to grow in its own liquid only if its diameter exceeds some critical value; most of the crystals with smaller diameters will melt back into the ambient liquid. On the atomic scale the process of nucleation consists of a sequence of atomic movements within the liquid, leading to the rearrangement of a critical number of atoms to form a nucleus with a crystalline configuration. It is easy to see why such a sequence of events must be highly improbable, in spite of the fact that the atoms in the liquid move about rapidly. At 10 degrees C. below its equilibrium temperature a crystal nucleus of mercury contains some 100,000 atoms. Obviously the probability of such a large number of atoms rearranging themselves into a stable crystalline configuration must be very low. Thus clean mercury, without seeding or vibration, should sustain undercooling by 10 degrees C. for an indefinite period of time.

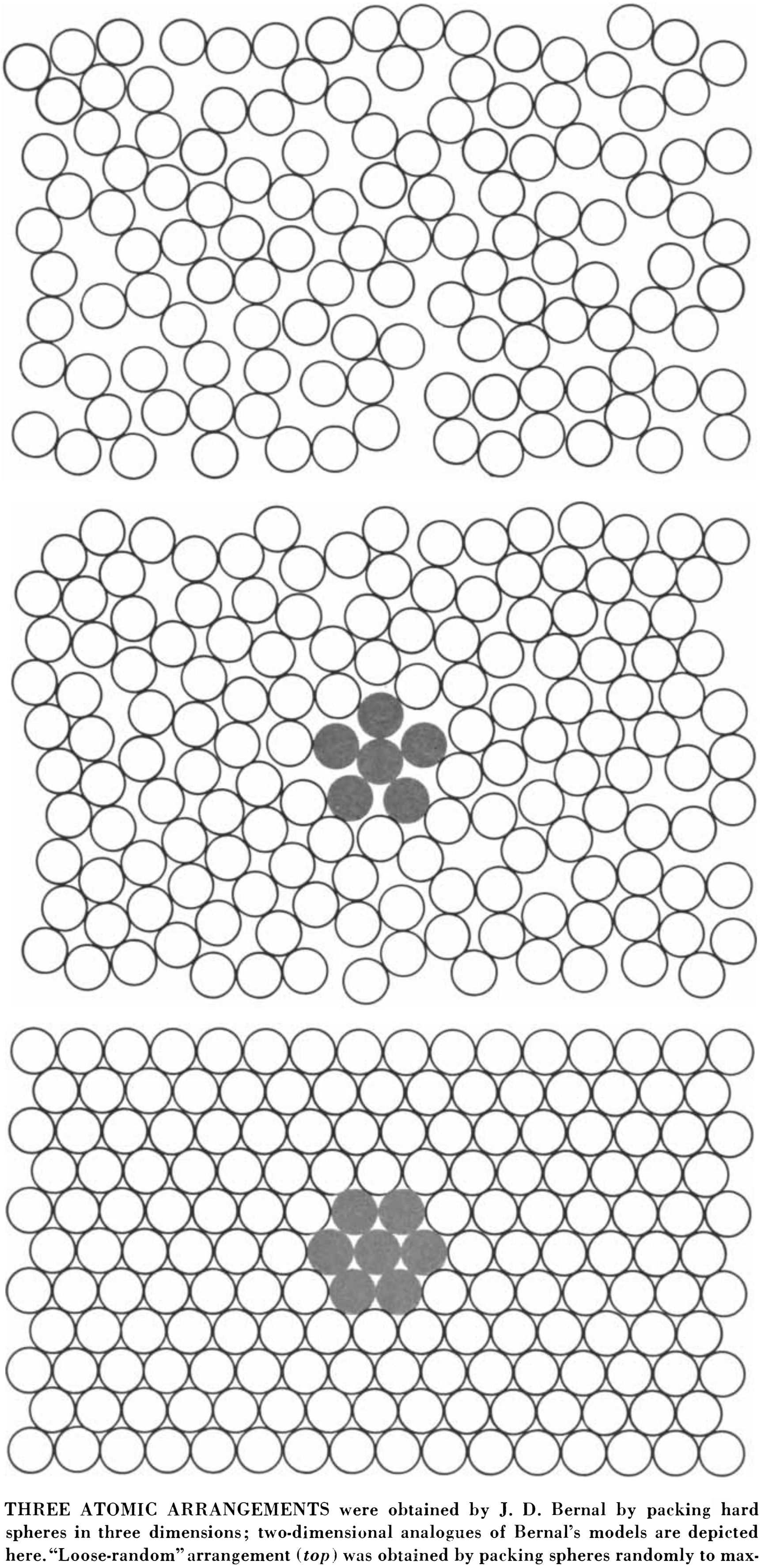
It is not entirely clear why motes facilitate nucleation; one possibility is that they provide a template, or atomic pattern, on which the crystal nuclei are able to form. As Vonnegut has put it, motes “fool” the liquid into crystallizing. It seems likely that the effectiveness of a mote in promoting nucleation is greater the closer its atomic configuration matches that of the forming crystal. In fact, all nucleating motes identified so far have been crystalline. The actual closeness of atomic fit, however, seems to be not the only factor involved in determining the nucleating effectiveness of a mote, although it is of obvious importance.

Why vibrating a liquid often promotes crystal nucleation is even less well understood. Vonnegut has suggested that nucleation may be caused by the collapse of small cavities in the liquid that presumably have been formed by the vibration. At the instant of collapse an extremely high pressure would develop in the tiny region around the collapse focus. This disturbance would be relieved by a wave of alternating positive and negative pressure, or compression and expansion, propagating from the collapse focus. The equilibrium crystallization temperature in the wave, since it depends on pressure, would thus be alternately above and below its normal value. As a result small regions in the vicinity of the pressure wave would be undercooled by a larger amount than would be evident from the temperature of the entire sample. The larger undercooling in these regions would of course increase the probability of nucleation. Although this explanation is plausible, it involves theoretical and experimental difficulties that have so far prevented any quantitative evaluation of its merit.

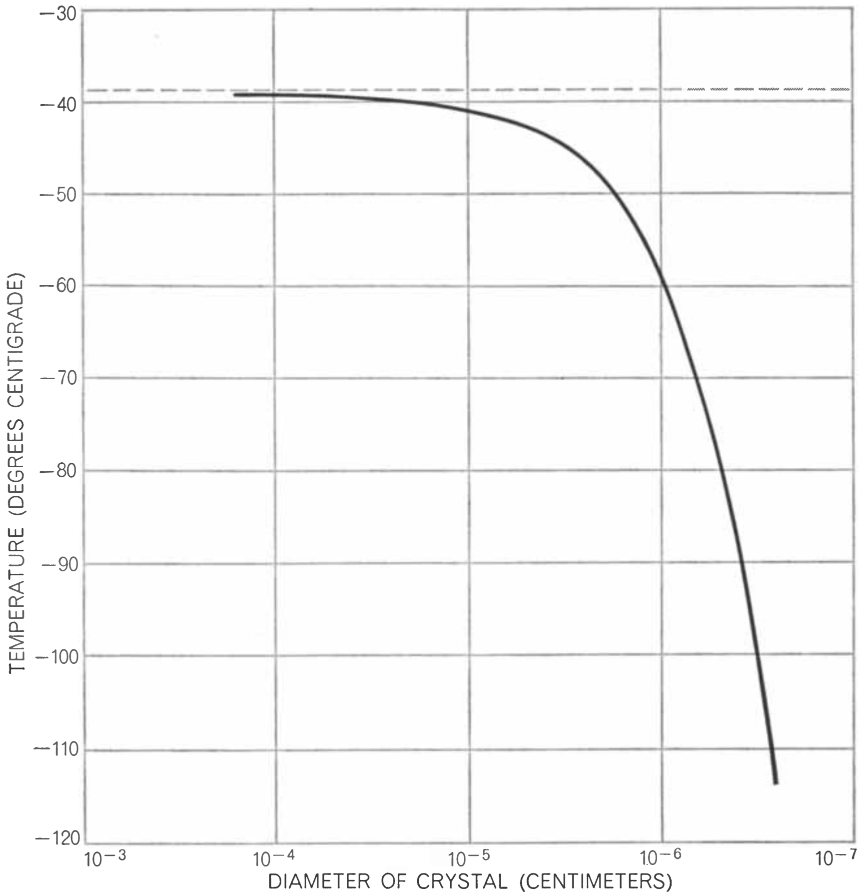
We have seen that the behavior of undercooled liquids can be explained by supposing that the work required to form the interfaces between the liquid and the crystal is both positive and large. The question of why this work is so large remains. For metals and other simple liquids the work required is equivalent to the energy expended in melting approximately half of a crystal layer one atom thick at the interface. Such a large amount of work is difficult to justify on the basis of some of the prevailing views regarding the structure of liquids.

Of the two principal hypotheses of liquid structure, one holds that the instantaneous atomic arrangement in a liquid is such that the immediate environment of any atom is quite like that in the crystalline phase of the same substance. Opposed to this view is the model that has been strongly supported by the work of Joel H. Hildebrand of the University of California at Berkeley, J. D. Bernal of the University of London and their respective colleagues, in which the atomic arrangement in a liquid is considered to be more like that in a very dense gas; that is, it is as random as it can be within the constraints imposed by the impenetrability of the atoms [see "The Structure of Liquids," by J. D. Bernal; SCIENTIFIC AMERICAN, August, 1960].

In order to appreciate the reasoning behind the idea that the local atomic arrangement in a liquid is crystal-like, consider the changes in properties that normally accompany the melting of a crystalline metal. The most conspicuous changes are of course the decrease in viscosity—typically by some 40 orders of magnitude—and the disappearance of the long-range atomic order that characterizes crystalline structure. The changes in some of the other properties, on the other hand, seem quite trivial. In particular the increases in volume and energy content that accompany the melting of a metal usually range from 2 to 5 percent each. Moreover, X-ray diffraction studies indicate that locally the atoms in a liquid appear to be arranged in roughly the same patterns as in the



**THREE ATOMIC ARRANGEMENTS** were obtained by J. D. Bernal by packing hard spheres in three dimensions; two-dimensional analogues of Bernal's models are depicted here. "Loose-random" arrangement (*top*) was obtained by packing spheres randomly to maximum density. When compressed, this arrangement collapsed to "dense-random" arrangement (*middle*), which was less dense than closely packed crystalline arrangement (*bottom*).




**EQUILIBRIUM CRYSTALLIZATION TEMPERATURE for crystals of mercury immersed in their own melt decreases sharply with decreasing crystal size below a certain diameter. Normal equilibrium temperature for mercury is —39 degrees C. (*broken line*).**

corresponding crystal. All this evidence naturally led to the concept that, at least in monatomic liquids (that is, liquids whose molecules consist of one atom each), the atomic movements required for the transition from liquid to crystal would be quite small, with the result that the liquid would simply collapse into the crystalline configuration on being undercooled even slightly. This view of liquid structure is clearly contradicted by the behavior of undercooled liquids observed in the experiments I have just described.

Now consider the interpretation of undercooling in terms of the gaslike, random model of liquid structure. The special meaning of randomness as applied to the local arrangement of atoms in a liquid was brilliantly illuminated by the recent model experiments performed by Bernal and his colleagues. Bernal began by assembling uniform hard spheres randomly in a rectangular container to the highest possible density. When the resulting "loose-random" structure was compressed, it collapsed not to a crystalline structure but to an amorphous "dense-random" structure with a density roughly .86 times that of crystalline structure [*see illustration on preceding page*]. The same structure was assumed by uniform hard spheres randomly packed to maximum density in a spherical container.

Bernal examined the various local arrangements of atoms in both structures and found that the resulting configurations could, with some idealization, be described by means of a few geometric figures, assembled together in various ways to form the whole structure. It is significant that only two of these figures (the tetrahedron and the octahedron) occur in the description of the simplest crystal structures. The other figures, which actually predominate in the dense-random structures, are characterized by pentagonal symmetry. Geometrically such figures are said to be noncrystallographic; that is, they cannot be packed with other, identical figures to form a crystalline configuration. Bernal concluded that the structure of monatomic liquids, judging from the X-ray diffraction data, is intermediate between the loose-random and the dense-random models.

For the purposes of our study perhaps the most important conclusion to arise out of Bernal's experiments is that a substantial reconstruction of the local atomic arrangement is necessary for the transition from the liquid to the crystalline state. Even in the case of monatomic substances the liquid could not, because of the predominance of the noncrystallographic atomic configurations, collapse into the crystal; the transitions must be brought about by crystal nucleation and growth. This view is reinforced by the failure of the actual dense-random models to collapse into the more stable crystalline configurations when they are further compressed. Any liquid whose atomic structure is characterized by the dense-random model would thus exhibit a high resistance to crystal nucleation.

A more quantitative explanation of the behavior of undercooled liquids must wait until the formulation of a theory that will be able to account for the large amount of work required to form the interface between the crystal and the liquid. The outlines of such a theory are suggested by Bernal's dense-random model. Consider a liquid in which the atoms are randomly distributed and that is in contact with some fixed solid surface. Because of the large repulsive forces between the atoms, all the atoms in the liquid can be considered to have a definite diameter. It follows that the centers of the atoms must be excluded from that part of the liquid that lies within a distance from the fixed surface equal to the atomic radius. Unless some drastic readjustment occurs in the arrangement or shapes of the atoms, the density of the liquid at the interface will therefore be only half that in the rest of the liquid. If the fixed surface were the crystal itself, the work expended in forming the interface would be equivalent to that required to remove the half layer of atoms from the interfacial surface of the liquid. Since this work would be done against the interatomic forces, it would be quite large. In actuality it would not be nearly so large as this simple model suggests, since neither the crystal surface nor the atoms in the liquid are rigid; it is therefore conceivable that a readjustment in the arrangement of the atoms at the interface could occur, which would reduce substantially the work required for the transition from liquid to crystal. Nonetheless, it seems clear that the magnitude of the work required for the transition must be related in some way to the exclusion of the centers of the atoms from the interface between the crystal and the liquid.

## Kodak reports on:

**photographic data-recording made easier . . . printing today . . . knots to polyolefin**

### A technology goes into hiding

We hoot and shout that the new KODAK RAR Films for data-recording can be processed as hot as 130°F. You counter that you can find your thrills elsewhere than on a developing-tank thermometer. Maybe you're not getting the full impact. *Consider the logic—*

The hotter the process, the shorter the time to the results. Basic principle.

The shorter the time, the less film is undergoing treatment inside the machine at a given instant.

The less film the machine has to contain, the smaller the machine.

Big machines that fill big rooms need tending by crews with a sense of responsibility. Little machines over in a corner of the department may draw no more attention than the pencil sharpener.

A hundred feet of film can capture and give back a staggering quantity of information. It works very directly. You can *see* the data. That's the good part. It was the bad part—the need to get involved in a fussy, unfamiliar technology—that limited the number of high-volume data-recording applications. Now comes the day when the technology at last hides inside a little black box.

We can't kid you. We don't know where to send you for an all-purpose data-recording system, like a pencil sharpener that takes all calibers. Every engineer who has been thinking of photorecording without doing much of it has a special problem. (If it weren't a special problem, an engineer wouldn't be needed.) Where to take the special problem hasn't been clear. Now that neat solutions are a lot more likely, we hereby make an offer:

*Any engineer who is considering a photographic approach to a data-recording problem he faces and who will sit down and describe the conditions in a letter to Eastman Kodak Company, Special Sensitized Products Division, Rochester, N. Y. 14650, will receive a prompt and thoughtful answer. If we think he is over-optimistic, we'll say why. If we think some other company can help him better, we will direct him there. If we think that somewhere in the large corps of photo-technologists we employ on a diversity of tasks there are a couple of fellow engineers who can tell him what to do to advance his cause without dipping him any deeper into photo-technology than he wants to be dipped, we will arrange to get him and those fellows together in a room with a blackboard.*

### Shallow relief

Another technology which a civilized party can consider worthy of his mild interest is printing. Here, with a new entity designated the KODAK Relief Plate, we have further complicated the complex interconnections among cost, speed, quality, and utility of printing.

We do not recommend our new product either for tomorrow morning's New York *Times* or for a $250-per-copy edition of Rembrandt reproductions. Handbills announcing a postponement of the volunteer firemen's ball can deliver their message cheaper and just as effectively without it. For a journal of plant pathology which needs photomicrographs, gross photographs, and drawings to illustrate nearly every paper, "shallow relief" could mean the difference between life and death as a feasible publishing operation.

Huge rotary presses printed these words from cylindrical shells of great thickness compared to the depth of the areas where ink-covered metal was not to touch paper, as inside this "o." A rotary press is obviously more efficient than a flat-bed press, such as Gutenberg's. Even publications in far less demand than the one you are now reading need rotary presses for economy, but not huge ones. The modest-size rotaries need to print from thin, easily made plates bent to the requisite radius. To retain the strength to keep the outer surface reasonably cylindrical instead of collapsing to an irregularly polygonal contour, and to minimize the need for certain mysterious and time-consuming adjustments called "make-ready" that the printer's customers do not understand but appreciate as the difference between pleasing appearance and mere legibility, the no-ink portions must remain shallow.

One type of such low relief plate uses as its printing surface a polymer which an ingenious chemical reaction sensitizes to light for a few minutes before exposure under the photographic negative that determines where the plastic will remain and where not. It is further demanded that when this miraculous polymer whirls on the press it retain its bond to its sheet metal substrate and never crumble off a comma or a few hundred minimum-size halftone dots.

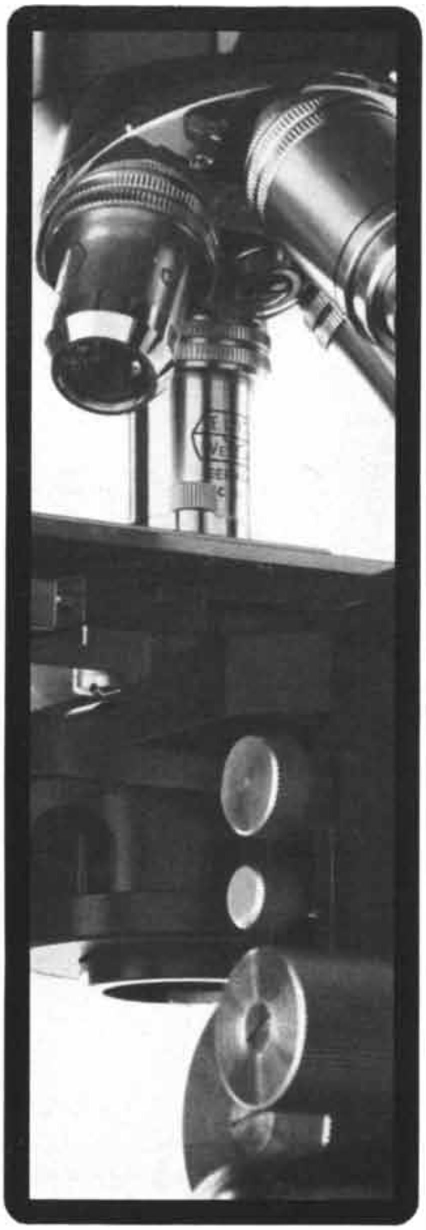
For our own offering in low relief we have been more conservative. By not asking so much of one substance, we ease the problems of bonding, fragility, conformity to the cylinder, exposure convenience, and many other factors that might bore you but not your printer. If your printer happens to print by offset instead of the venerable letterpress principle, his counterpart to the "make-ready" bit is striking a water: ink balance. A rotary press may have to reach full speed before a trial ratio can be judged. On a short run, involving large solidly inked areas, particularly with certain inks, for a fussy customer who has been taught strong feelings about printing esthetics, 20% of the paper consumed may leave the plant via the baler. It has to be paid for somehow. If the job runs on KODAK Relief Plates, no ratio judgement is required. The water is simply turned off.

*In the unlikely event that your printer doesn't know all about this, tell him to get in touch with Eastman Kodak Company, Graphic Arts Sales Division, Rochester, N. Y. 14650.*

### Greetings to all haymakers!

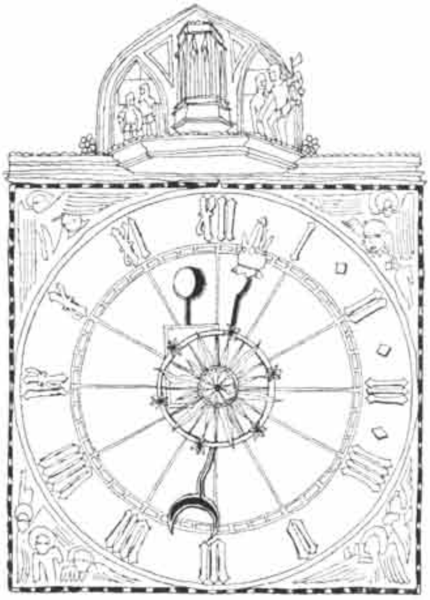
Can you use any baler twine? We make it out of gas issuing from a hole in the ground. We aren't the only people who make polyolefin, but we *are* the only ones who found a way to lick its slipperiness. Now it takes knots of 125-lb. average strength. Comes in 2-ball bales of 9000 feet minimum. Has about the same bulk but less weight than the natural fiber that insects and rodents prefer. Absorbs virtually no moisture. Works satisfactorily on all prominent makes of hay baler but one. Eastman Chemical Products, Inc., Plastics Division, Kingsport, Tenn. (Subsidiary of Eastman Kodak Company) can supply free sample and name of nearest distributor. Lay in enough now for all year. Forget about rot. Don't forget that the kingpin of all technologies is agriculture.

**This is another advertisement where Eastman Kodak Company probes at random for mutual interests and occasionally a little revenue from those whose work has something to do with science**





# SCIENCE AND



## *Arctic Disarmament*

A U.S. biophysicist and a Soviet geochemist have proposed the establishment of an inspection zone in the Arctic regions of the U.S. and the U.S.S.R. as an initial step toward a comprehensive disarmament treaty. The proposal, said to be the first made jointly by a U.S. and a Soviet scientist, was published in the *Bulletin of the Atomic Scientists* by Alexander Rich of the Massachusetts Institute of Technology and Aleksandr P. Vinogradov of the Vernadsky Institute of Geochemistry and Analytical Chemistry in Moscow.

Rich and Vinogradov point out that the Arctic is the only part of the world where the territories of the two great powers adjoin each other, and that although it contains significant military installations, they are not of primary importance to either country. It would therefore be an ideal area in which to work out inspection procedures. Rich and Vinogradov suggest beginning with all or part of Alaska and a corresponding region in eastern Siberia, taking care to select areas of clearly equivalent military value to the two countries. The agreement would bar from these areas nuclear weapons and long-range bombers and missiles. Airfields and bases might be allowed to remain; defensive installations such as radar would continue to be operational. If the earthquake region of the Kamchatka Peninsula were included, it would provide a "fringe benefit": inspection there might help to solve the problem of distinguishing earthquakes and underground explosions, which has stood in the way of extending the nuclear-test-ban treaty to include underground tests.

An Alaska-Siberia zone would have the advantage of requiring agreement between only the U.S. and the U.S.S.R. The plan might be extended into Greenland, where the U.S. has bases, with an equivalent expansion into eastern Siberia. A considerably larger territory would then be disarmed with the addition to the contracting parties of only Denmark (which, according to Rich and Vinogradov, has indicated its willingness to be included in a disarmament plan). A still broader plan, including Canada, Norway and Sweden, could extend disarmament to the entire Arctic zone. This would separate the offensive forces of East and West over the entire northern part of the world and still leave the bulk of both military establishments intact. The development of a reliable inspection system would provide "a considerable measure of reassurance and could contribute materially to fostering the kind of mutual trust that is a necessary prologue to additional disarmament measures."

## *A Generation of Telescopes*

With a warning that "American astronomy will surely stagnate in this century" unless new facilities for ground-based astronomical observation are built, a panel of the National Academy of Sciences has recommended a 10-year plan for constructing both optical and radio telescopes. The cost would be $224 million and would be borne mostly by the Federal Government, with some participation by universities. Such an investment, the panel said, would represent "one-half of 1 percent of that going into the space effort." The panel had eight members, all active in optical or radio astronomy.

By way of background, the panel discussed the present limitations in both fields. In optical astronomy, it said, "only two existing telescopes are adequate for pushing current frontier problems to the observational limit." They are the 120-inch reflector at the Lick Observatory and the 200-inch on Palomar Mountain. In radio astronomy the major limiting factor is "the lack of instruments of the proper design to meet problems now recognized." To remedy "the present serious situation" the panel recommended construction of "three major optical telescopes of the 150- to 200-inch size, four intermediate-size telescopes (60 to 84 inches), a number of smaller instruments capable of important bright-star research and training, two major array-type radio telescopes capable of high resolution, two large parabolic steerable antennas of the 300-foot class and a number of special-purpose radio instruments for the unique problems of great importance."

### *Protein from Petroleum*

The feasibility of using microorganisms to manufacture proteins from crude petroleum is being explored by a group of researchers at the Lavera Refinery of the Société Française des Pétroles B.P., a French partner of the British Petroleum Company. Alfred Champagnat, research director of the Société, reports the latest findings of his group in a recent issue of *Impact*, a publication of the United Nations Educational, Scientific and Cultural Organization (UNESCO).

It has been known for some years that a variety of microorganisms can live and actively reproduce at the expense of petroleum hydrocarbons in storage tanks, refinery slop tanks, oil-soaked soil and even under the bituminous covering of roads. In 1957 a microbiological research program was set up at the Lavera Refinery to evaluate the possible uses of such microorganisms. To date Champagnat and his associates have reached two main conclusions: (1) The petroleum-based microorganisms are rich in proteins that contain all the amino acids required to sustain animal life. Moreover, the amino acids are present in roughly the same proportions found in proteins obtained from animal sources. (2) The microorganisms are capable of consuming certain paraffinic hydrocarbons found in petroleum. Since the only other known method to deparaffin petroleum for use in diesel engines and domestic heating systems produces enormous quantities of unsalable paraffin, deparaffination by means of microorganisms might provide a more efficient stage in the normal refining process as well as a more economic by-product.

Champagnat points out that "proteins from petroleum fermentation are just as natural as those of vegetables and animals, since they are the essential elements of the cells of microorganisms that have adapted themselves to extract their carbon from hydrocarbons rather than sugars or carbohydrates." When they are dried, the cells of such microorganisms burst open and thus become digestible. They can be preserved for extended periods by taking the same precautions as are already taken in the storage of flour. The dry microorganisms, when purified, appear in the form of a powder or whitish flakes, without any noticeable taste or smell of petroleum. They are particularly suited for consumption in those underdeveloped regions of the world in which cereals form the major part of the protein diet, since their amino acid deficiencies are made up by those found in most cereals. The proportion of microorganisms added to such foods could be as high as 20 percent.

The advantages of the petroleum industry as a basis for the cultivation of edible microorganisms are numerous. As Champagnat remarks: "The culture of microorganisms does not require soil, is independent of climate, rainfall and abundant labor. It is carried out in industrial equipment operated by small teams of specialists. It is, then, a kind of soilless culture with very high productivity, comparable to intensive breeding, say of chickens." He adds: "Crude oil, although perhaps not the most abundant carbonaceous material, is certainly the one most easily distributed in the world, thanks to the gigantic tanker fleets. Its price is fairly stable in comparison with those of the other main raw materials and is relatively low because it is almost universally utilized as a source of energy.... The more than 700 petroleum refineries established in all the countries of the world constitute an infrastructure perfectly adapted to the addition of fermentation units—which could at the same time function as refining units." Champagnat has calculated that at the present rate of production of crude oil the potential production of protein-rich microorganisms could reach as much as 20 million tons a year.

## *Textbooks Victorious*

A fundamentalist campaign has failed to prevent adoption in Texas of three high school biology textbooks on the ground that they discuss the theory of evolution (see "Science and the Citizen," October, 1964). In October of last year the 15-member state Textbook Committee voted overwhelmingly to adopt all three books. The group opposing adoption appealed this decision to the 21-member State Board of Education, an elective body. On November 9, after a day of deliberation, the Board of Education voted by a margin of two to one to support the Textbook Committee's decision. In consequence the textbooks—products of a five-year, $5 million project, supported by the National Science Foundation, to raise the level of high school biology teaching—may now be legally used in Texas schools. The three books, known familiarly as the "blue," "yellow" and "green" texts, are respectively *Biological Science: Molecules to Man* (Houghton Mifflin), *Biological Science: An Inquiry into Life* (Harcourt, Brace and World) and *High School Biology* (Rand McNally).

## *Analysis in the Retina*

In recent years it has become clear that not all analysis of visual information occurs in the brain; a great deal of it is accomplished by nerve cells in the retina of the eye. Now it appears that this retinal analysis goes even further than had been supposed. H. B. Barlow, R. M. Hill and W. R. Levick report in the *Journal of Physiology* that nerve cells in the rabbit's retina are able to abstract from the visual scene not only localized dimming and brightening but also direction of motion and speed of motion. In the vertebrate eye signals from a number of photoreceptors converge on the ganglion cells: retinal nerve cells that send impulses along the optic nerve to the brain. Since 1938 it has been known that different ganglion cells respond to different features of the retinal image. Experiments with such animals as frogs, fishes, pigeons, cats and monkeys have shown that in one animal or another these features include the onset or termination of illumination ("on" or "off" responses), the color of the light and the movement of objects or edges across the receptive field of a cell.

At the University of California at Berkeley and at the University of Cambridge, Barlow, Hill and Levick, recording with microelectrodes from ganglion cells in the retinas of anesthetized rabbits, discovered several new types of cell. The most interesting were units that showed directional selectivity: they discharged a much larger burst of impulses when an exploring spot was moved all the way across their receptive field in one direction than when it was moved in the opposite direction—and did so whatever the nature of the moving stimulus. The preferred direction was different for different cells. These units were strongly inhibited by any stimulation of the retinal region just outside their own receptive fields; such inhibition apparently accounts for the behavior of "bug-detector" cells in the frog's retina that are more sensitive to the movement of small, dark objects than to larger shapes, which presumably extend outside the cell's receptive field.

Barlow, Hill and Levick found other units in the rabbit retina that gave vigorous discharges only in response to very rapid movement of an object—even a barely perceptible shadow of a thin thread or wire—across the field. Still other units, packed densely into the central area of the retina, had small fields and responded only to slow movements. The results with rabbits, according to the authors, confirms the fact that complex analysis of sensory information occurs in the retina and that each class of ganglion cell has its own "trigger feature" to which it is most sensitive. "Evidently in the rabbit the speed and direction of motion of the image are particularly important in determining which ganglion cells are stimulated in a particular part of the retina."

## *Superstrong Magnets*

The highest continuous magnetic fields ever generated by man have been achieved in a direct-current, water-cooled magnet at the National Magnet Laboratory, which is operated by the Massachusetts Institute of Technology under the sponsorship of the Air Force Office of Scientific Research. In an altogether different area of magnetry, involving fields that last for fleeting instants, an implosive technique developed by C. M. Fowler at the Los Alamos Scientific Laboratory holds promise that the attainment of usable million-gauss fields may be near. High-intensity magnetic fields of both the continuous and the brief types are valuable in research on the properties of matter.

Up to now the highest continuous field, meaning a field that can be sustained indefinitely or for at least a minute, has been about 152,000 gauss, attained in a magnet at the Naval Research Laboratory in Washington. The achievement at M.I.T. is a magnet that creates a continuous field of 255,000 gauss. The magnet is a three-coil solenoid that draws its power from generators able to supply direct current up to 40,000 amperes at 250 volts. It obtains 205,000 gauss from the coils alone; it can achieve 255,000 gauss for about a minute when two iron poles are inserted in the core, although the poles reduce the space available for experiments. The laboratory hopes to be able soon to push the peak continuous field to 300,000 gauss and to produce multisecond pulsed fields exceeding 400,000 gauss.

Efforts to produce still higher fields encounter the problem of containing the large forces involved. The M.I.T. magnet, for example, creates pressures of 60,000 pounds per square inch, exceeding those at the deepest points in the oceans. Fowler's technique is designed to surmount that problem. He surrounds a metal ring with an explosive, produces a magnetic field of up to 100,000 gauss in the ring and then detonates the explosive in such a way that the ring is compressed symmetrically. The magnetic field increases as the area of the ring decreases. In this manner Fowler has produced fields as high as 15 million gauss. What remains to be developed is a technique for making observations during the microsecond duration of the field and under the destructive circumstances of the implosion.

## *Contraception for Sacred Cows*

The plastic intrauterine devices that have proved successful in limiting human pregnancies may provide one answer to India's problem of unwanted reproduction on the part of the 40 million ownerless, hungry, scavenging, substandard, unproductive but nonetheless revered cattle that roam the country. Faced with the difficult task of improving the milk production and draft capacity of the nation's 120 million standard-breed cattle and 45 million water buffalo, the Indian Ministry of Food and Agriculture has made little progress in rounding up and disposing of strays and wild stock. In the past five years 38 "old cow's homes" have been set up as a means of segregating worthless cattle without offending Hindu beliefs, but the culls have totaled less than 100,000.

Because castration is almost as unacceptable to Hindus as slaughter, Frank W. Parker, a U.S. Government rural redevelopment specialist long stationed in India, hit on the alternative idea of contraception. At his suggestion animal-husbandry experts at the Beltsville, Md., research center of the Department of Agriculture recently equipped 18 cows with plastic spirals and then attempted artificial insemination. A control group of 21 cows averaged 1.8 services per conception; all the protected 18 remained infertile during a series of 42 services. News of the results has been optimistically received by the Indian Ministry of Food and Agriculture, and plans for a pilot project are under way.

## *Exercised Brains*

It has long been assumed that an animal's experience must produce chemical or physical changes in its brain. What, however, is the nature of the changes? A lengthy series of experiments has produced substantial evidence that at least some of the changes are reflected in the weight of the animal's brain and in its enzymatic activity. The experiments are reported in *Science* by Edward L. Bennett, Marian C. Diamond, David Krech and Mark R. Rosenzweig of the University of California at Berkeley.

Rats of the same parentage and sex (male) were separated after weaning at about 20 days of age and placed in one of three kinds of environment. In one environment, which served as a control, the rats were housed three to a cage and were given no special treatment. In a second environment, designed to provide enriched experience, the rats were housed in groups of 10 to 12 in large cages provided with an assortment of "playground" equipment: ladders, a treadmill, a trapeze and so on. In a third environment, which provided reduced experience, rats were caged singly in a dimly lighted and quiet room. After spending 80 days in one of these environments the rats were killed and their brains, identified only by a code number, were turned over to chemists and anatomists who conducted their examinations without knowing how the rats lived.

At the end of seven successive experiments it was found that in 62 cases out of 77 the rats with enriched experience had developed a heavier cerebral cortex than had littermates that had been raised in isolation. The average increase in cortical weight was 4.6 percent. Examination of the cortex by regions showed that the greatest weight increase—6.2 percent—took place in the visual region, which was also 6.2 percent thicker.

The Berkeley workers point out that the absolute weight of the cerebral cortex cannot be correlated with experience or "intelligence." For example, one genetic strain of rat that is poor at maze-solving has a cortex about 10 percent heavier than a strain that is much better at it. Within each strain, however, animals exposed to an enriched environment develop a heavier cortex than do littermates raised in isolation.

The second set of findings has to do with the amount of acetylcholinesterase in the brains of the experimental animals. Acetylcholinesterase is an enzyme that inactivates acetylcholine, a substance that facilitates the transmission of impulses from one nerve cell to another (see "The Synapse," by Sir John Eccles, page 56). Greater acetylcholinesterase activity was found in the cerebral cortex of rats exposed to an enriched environment than in the cortex of rats raised in isolation. And again the greatest increase—3.6 percent—was found in the visual region.

Another series of experiments showed that the same general increases in cortical weight and enzyme activity could be produced in adult rats that had shared a common environment for 105 days after birth and were then transferred to the differing environments for a period of 80 days. If anything, somewhat greater changes were produced in the adult rats than in the young ones.

The Berkeley investigators caution that the changes they have found in rat brains cannot be related in any obvious way with memory storage. "The demonstration of such changes," they conclude, "merely helps to establish the fact that the brain is responsive to environmental pressure—a fact demanded by physiological theories of learning and memory."

## *Faked Life from Other Worlds*

A fragment of a meteorite that fell in southwest France more than a century ago has proved on recent inspection to be ingeniously doctored with terrestrial organic material. The doctoring was almost certainly done by a 19th-century practical joker who may have hoped to sway scientific opinion against Louis Pasteur's contemporaneous denunciation of the notion that living organisms could spontaneously arise from nonliving matter. The attempted hoax fell flat, Edward Anders of the University of Chicago and his associates report in *Science;* instead of attracting attention, the doctored meteorite remained unexamined for 98 years.

Following a meteor display on the night of May 14, 1864, 20 meteorite fragments were recovered in the vicinity of Orgueil, a village near Toulouse. All the fragments were brought to the natural history museum at Montauban in the course of the next few weeks. The Orgueil meteorite was of the peculiar variety known as a carbonaceous chondrite; such meteorites are soft, dark in color and dissolve readily in water. It was the fifth such meteorite to become known to scholars; in the 100 years since its fall only 15 other carbonaceous chondrites are known to have reached the earth and been recovered.

The officials of the Montauban museum sent specimens of the Orgueil meteorite to other museums around the world and retained one 80-gram and one 34-gram piece for their own display; museum records show that the smaller piece had been presented by the mayor of a nearby village between May 27 and June 12, 1864. The jar in which both specimens were placed remained sealed from that year until 1962, when the 80-gram piece was sent to Bartholomew S. Nagy of Fordham University for examination in a general study of carbonaceous chondrites.

After nearly a century the smaller specimen at Montauban had crumbled; later in 1962 museum officials shipped most of it to Anders in Chicago and some to Jean Deunff, a French micropaleontologist. Deunff was astonished to find that mixed in with his sample were fragments of plant matter, subsequently identified as the European reed *Juncus*. Anders and his associates found seed capsules of the same plant deeply embedded in their samples and also bits of gravel, fragments of coal and a substantial trace of protein material resembling glue. The hoaxer had apparently moistened the meteorite fragment until it was soft, inserted the various foreign bodies and then, using glue as a binder, faked a surface crust of the kind produced by atmospheric heating to replace the one destroyed by his manipulations.

While the investigators were speculating on possible motivations for so elaborate a hoax, Walter Sullivan of *The New York Times* pointed out to them that the Orgueil fall had occurred only five weeks after Pasteur had delivered his stormy and widely reported defense of divine creation as the only possible initiator of life. With this coincidence in mind, Anders and his associates envision the hoaxer as an opponent of Pasteur's stand who knew that the chemical composition of legitimate carbonaceous chondrites had already suggested the possibility of extraterrestrial life to some of Pasteur's contemporaries and hoped to provide the evidence needed to transform this possibility into a certainty.

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*Joseph A. Grazier*

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# THE SYNAPSE

## How does one nerve cell transmit the nerve impulse to another cell? Electron microscopy and other methods show that it does so by means of special extensions that deliver a squirt of transmitter substance

by Sir John Eccles

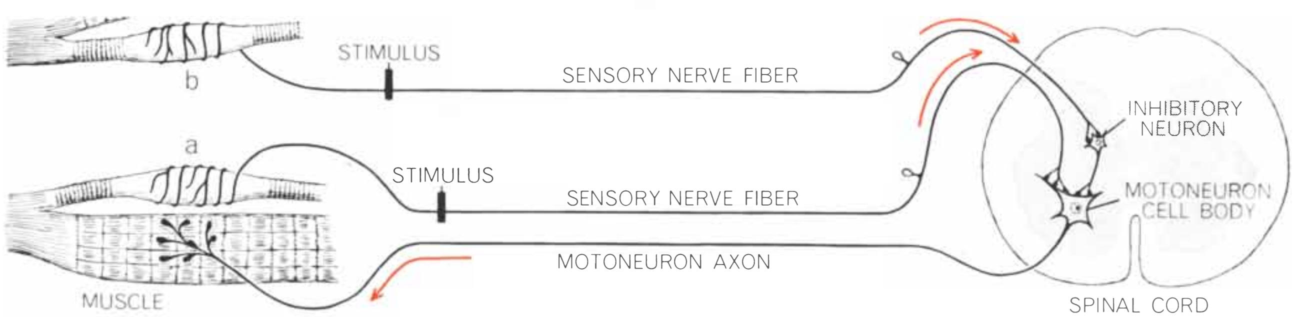
The human brain is the most highly organized form of matter known, and in complexity the brains of the other higher animals are not greatly inferior. For certain purposes it is expedient to regard the brain as being analogous to a machine. Even if it is so regarded, however, it is a machine of a totally different kind from those made by man. In trying to understand the workings of his own brain man meets his highest challenge. Nothing is given; there are no operating diagrams, no maker's instructions.

The first step in trying to understand the brain is to examine its structure in order to discover the components from which it is built and how they are related to one another. After that one can attempt to understand the mode of operation of the simplest components. These two modes of investigation—the morphological and the physiological—have now become complementary. In studying the nervous system with today's sensitive electrical devices, however, it is all too easy to find physiological events that cannot be correlated with any known anatomical structure. Conversely, the electron microscope reveals many structural details whose physiological significance is obscure or unknown.

At the close of the past century the Spanish anatomist Santiago Ramón y Cajal showed how all parts of the nervous system are built up of individual nerve cells of many different shapes and sizes. Like other cells, each nerve cell has a nucleus and a surrounding cytoplasm. Its outer surface consists of numerous fine branches—the dendrites—that receive nerve impulses from other nerve cells, and one relatively long branch—the axon—that transmits nerve impulses. Near its end the axon divides into branches that terminate at the dendrites or bodies of other nerve cells. The axon can be as short as a fraction of a millimeter or as long as a meter, depending on its place and function. It has many of the properties of an electric cable and is uniquely specialized to conduct the brief electrical waves called nerve impulses [see "How Cells Communicate," by Bernhard Katz; SCIENTIFIC AMERICAN, September, 1961]. In very thin axons these impulses travel at less than one meter per second; in others, for example in the large axons of the nerve cells that activate muscles, they travel as fast as 100 meters per second.

The electrical impulse that travels along the axon ceases abruptly when it comes to the point where the axon's terminal fibers make contact with another nerve cell. These junction points were given the name "synapses" by Sir Charles Sherrington, who laid the foundations of what is sometimes called synaptology. If the nerve impulse is to continue beyond the synapse, it must be regenerated afresh on the other side. As recently as 15 years ago some physiologists held that transmission at the synapse was predominantly, if not exclusively, an electrical phenomenon. Now, however, there is abundant evidence that transmission is effectuated by the release of specific chemical substances that trigger a regeneration of the impulse. In fact, the first strong evidence showing that a transmitter substance acts across the synapse was provided more than 40 years ago by Sir Henry Dale and Otto Loewi.

It has been estimated that the hu-




**REFLEX ARCS provide simple pathways for studying the transmission of nerve impulses from one nerve cell to another. This transmission is effectuated at the junction points called synapses. In the illustration the sensory fiber from one muscle stretch receptor (*a*) makes direct synaptic contact with a motoneuron in the spinal cord. Nerve impulses generated by the motoneuron activate the muscle to which the stretch receptor is attached. Stretch receptor *b* responds to the tension in a neighboring antagonistic muscle and sends impulses to a nerve cell that can inhibit the firing of the motoneuron. By electrically stimulating the appropriate stretch-receptor fibers one can study the effect of excitatory and inhibitory impulses on motoneurons.**

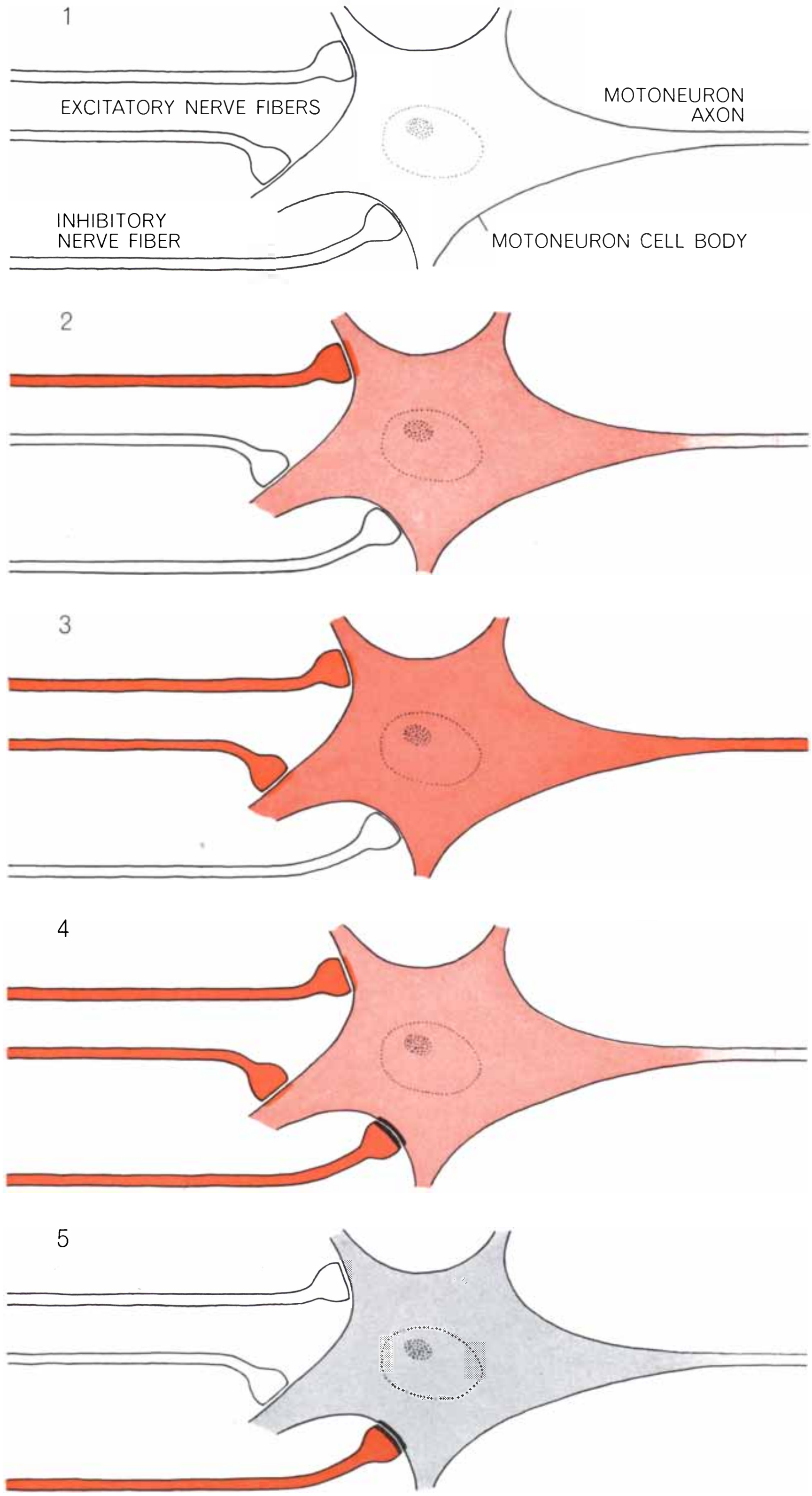
man central nervous system, which of course includes the spinal cord as well as the brain itself, consists of about 10 billion ($10^{10}$) nerve cells. With rare exceptions each nerve cell receives information directly in the form of impulses from many other nerve cells—often hundreds—and transmits information to a like number. Depending on its threshold of response, a given nerve cell may fire an impulse when stimulated by only a few incoming fibers or it may not fire until stimulated by many incoming fibers. It has long been known that this threshold can be raised or lowered by various factors. Moreover, it was conjectured some 60 years ago that some of the incoming fibers must inhibit the firing of the receiving cell rather than excite it [*see illustration at right*]. The conjecture was subsequently confirmed, and the mechanism of the inhibitory effect has now been clarified. This mechanism and its equally fundamental counterpart—nerve-cell excitation—are the subject of this article.

## Probing the Nerve Cell

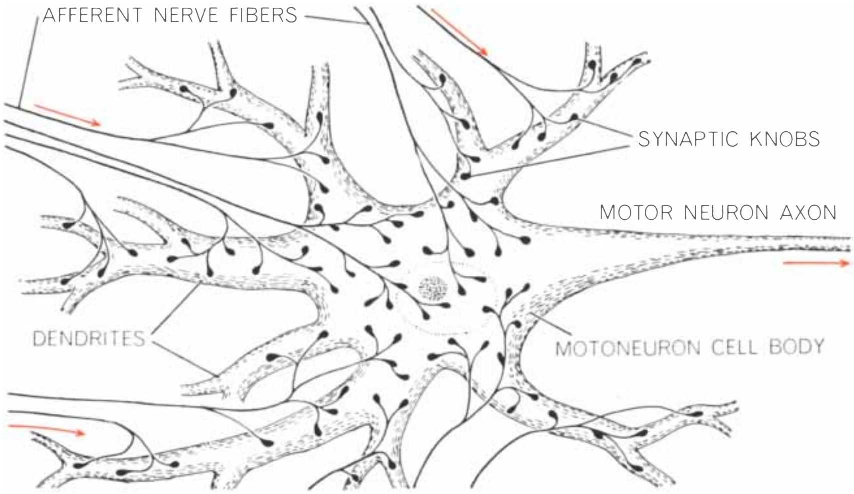
At the level of anatomy there are some clues to indicate how the fine axon terminals impinging on a nerve cell can make the cell regenerate a nerve impulse of its own. The top illustration on the next page shows how a nerve cell and its dendrites are covered by fine branches of nerve fibers that terminate in knoblike structures. These structures are the synapses.

The electron microscope has revealed structural details of synapses that fit in nicely with the view that a chemical transmitter is involved in nerve transmission [*see lower two illustrations on next page*]. Enclosed in the synaptic knob are many vesicles, or tiny sacs, which appear to contain the transmitter substances that induce synaptic transmission. Between the synaptic knob and the synaptic membrane of the adjoining nerve cell is a remarkably uniform space of about 20 millimicrons that is termed the synaptic cleft. Many of the synaptic vesicles are concentrated adjacent to this cleft; it seems plausible that the transmitter substance is discharged from the nearest vesicles into the cleft, where it can act on the adjacent cell membrane. This hypothesis is supported by the discovery that the transmitter is released in packets of a few thousand molecules.

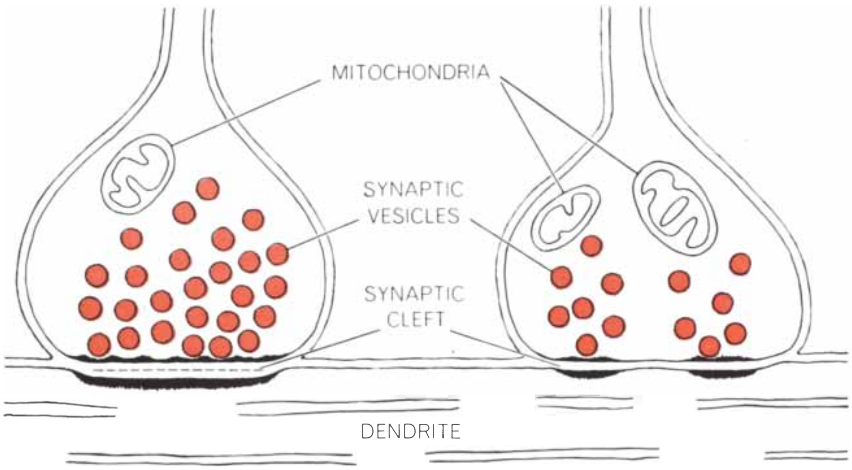
The study of synaptic transmission was revolutionized in 1951 by the introduction of delicate techniques for recording electrically from the interior



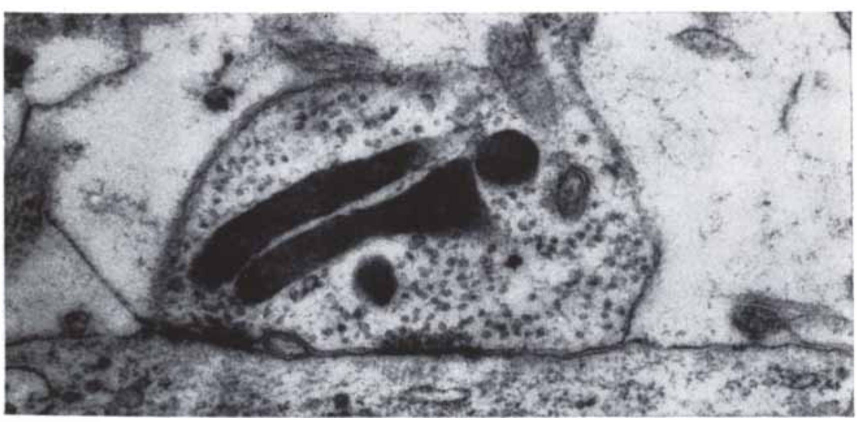

**EXCITATION AND INHIBITION** of a nerve cell are accomplished by the nerve fibers that form synapses on its surface. Diagram *1* shows a motoneuron in the resting state. In *2* impulses received from one excitatory fiber are inadequate to cause the motoneuron to fire. In *3* impulses from a second excitatory fiber raise the motoneuron to firing threshold. In *4* impulses carried by an inhibitory fiber restore the subthreshold condition. In *5* the inhibitory fiber alone is carrying impulses. There is no difference in the electrical impulses carried by excitatory and inhibitory nerve fibers. They achieve opposite effects because they release different chemical transmitter substances at their synaptic endings.




**MOTONEURON CELL BODY** and branches called dendrites are covered with synaptic knobs, which represent the terminals of axons, or impulse-carrying fibers, from other nerve cells. The axon of each motoneuron, in turn, terminates at a muscle fiber.




**SYNAPTIC KNOBS** are designed to deliver short bursts of a chemical transmitter substance into the synaptic cleft, where it can act on the surface of the nerve-cell membrane below. Before release, molecules of the chemical transmitter are stored in numerous vesicles, or sacs. Mitochondria are specialized structures that help to supply the cell with energy.



**ASSUMED INHIBITORY SYNAPSE** on a nerve cell is magnified 28,000 diameters in this electron micrograph by the late L. H. Hamlyn of University College London. Synaptic vesicles, believed to contain the transmitter substance, are bunched in two regions along the synaptic cleft. The darkening of the cleft in these regions is so far unexplained.

of single nerve cells. This is done by inserting into the nerve cell an extremely fine glass pipette with a diameter of .5 micron—about a fifty-thousandth of an inch. The pipette is filled with an electrically conducting salt solution such as concentrated potassium chloride. If the pipette is carefully inserted and held rigidly in place, the cell membrane appears to seal quickly around the glass, thus preventing the flow of a short-circuiting current through the puncture in the cell membrane. Impaled in this fashion, nerve cells can function normally for hours. Although there is no way of observing the cells during the insertion of the pipette, the insertion can be guided by using as clues the electric signals that the pipette picks up when close to active nerve cells.

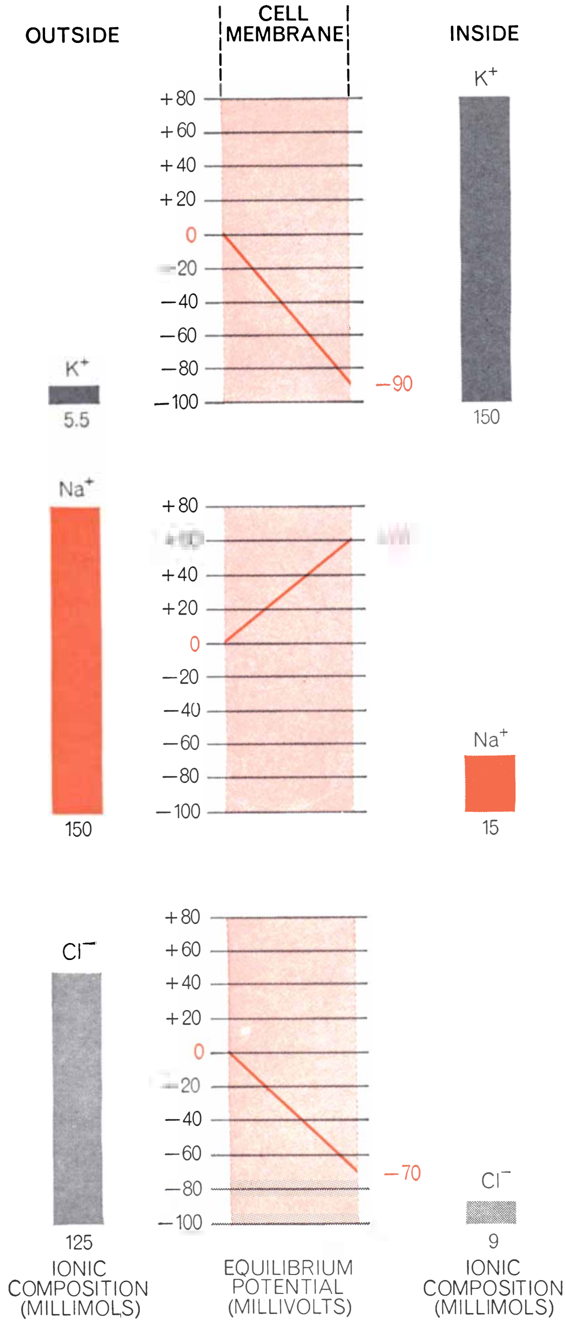
When my colleagues and I in New Zealand and later at the John Curtin School of Medical Research in Canberra first employed this technique, we chose to study the large nerve cells called motoneurons, which lie in the spinal cord and whose function is to activate muscles. This was a fortunate choice: intracellular investigations with motoneurons have proved to be easier and more rewarding than those with any other kind of mammalian nerve cell.

We soon found that when the nerve cell responds to the chemical synaptic transmitter, the response depends in part on characteristic features of ionic composition that are also concerned with the transmission of impulses in the cell and along its axon. When the nerve cell is at rest, its physiological makeup resembles that of most other cells in that the water solution inside the cell is quite different in composition from the solution in which the cell is bathed. The nerve cell is able to exploit this difference between external and internal composition and use it in quite different ways for generating an electrical impulse and for synaptic transmission.

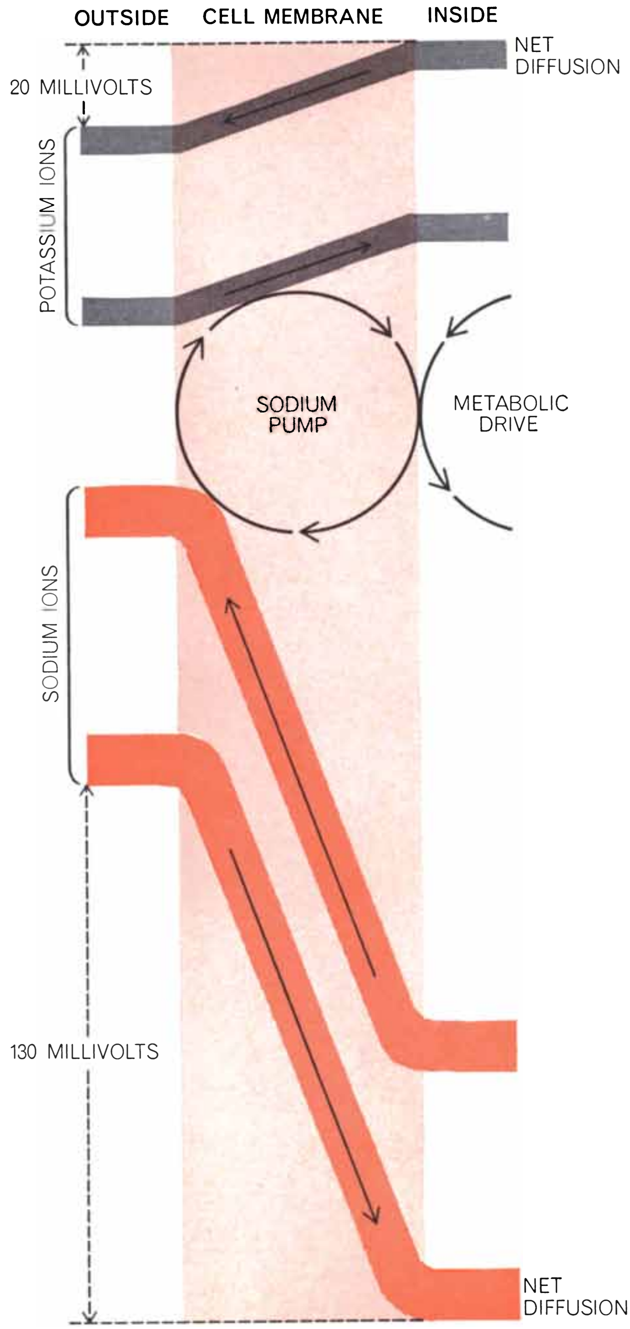
The composition of the external solution is well established because the solution is essentially the same as blood from which cells and proteins have been removed. The composition of the internal solution is known only approximately. Indirect evidence indicates that the concentrations of sodium and chloride ions outside the cell are respectively some 10 and 14 times higher than the concentrations inside the cell. In contrast, the concentration of potassium ions inside the cell is about 30 times higher than the concentration outside.

How can one account for this remarkable state of affairs? Part of the explanation is that the inside of the cell is negatively charged with respect to the outside of the cell by about 70 millivolts. Since like charges repel each other, this internal negative charge tends to drive chloride ions ($Cl^-$) outward through the cell membrane and, at the same time, to impede their inward movement. In fact, a potential difference of 70 millivolts is just sufficient to maintain the observed disparity in the concentration of chloride ions inside the cell and outside it; chloride ions diffuse inward and outward at equal rates. A drop of 70 millivolts across the membrane therefore defines the "equilibrium potential" for chloride ions.

To obtain a concentration of potassium ions ($K^+$) that is 30 times higher inside the cell than outside would require that the interior of the cell membrane be about 90 millivolts negative with respect to the exterior. Since the




IONIC COMPOSITION outside and inside the nerve cell is markedly different. The "equilibrium potential" is the voltage drop that would have to exist across the membrane of the nerve cell to produce the observed difference in concentration for each type of ion. The actual voltage drop is about 70 millivolts, with the inside being negative. Given this drop, chloride ions diffuse inward and outward at equal rates, but the concentration of sodium and potassium must be maintained by some auxiliary mechanism (*right*).




METABOLIC PUMP must be postulated to account for the observed concentrations of potassium and sodium ions on opposite sides of the nerve-cell membrane. The negative potential inside is 20 millivolts short of the equilibrium potential for potassium ions. Thus there is a net outward diffusion of potassium ions that must be balanced by the pump. For sodium ions the potential across the membrane is 130 millivolts in the wrong direction, so very energetic pumping is needed. Chloride ions are in equilibrium.

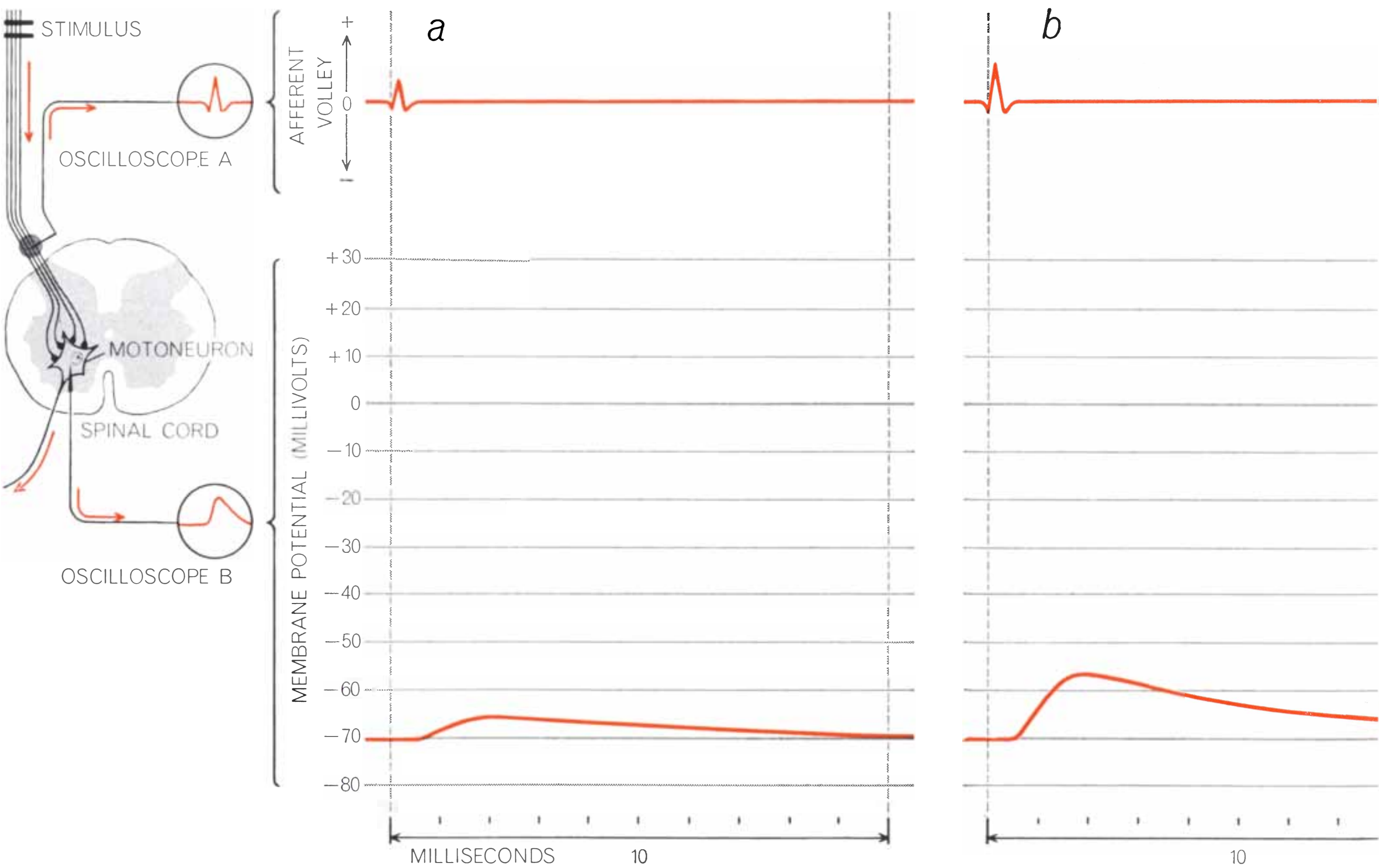
actual interior is only 70 millivolts negative, it falls short of the equilibrium potential for potassium ions by 20 millivolts. Evidently the thirtyfold concentration can be achieved and maintained only if there is some auxiliary mechanism for "pumping" potassium ions into the cell at a rate equal to their spontaneous net outward diffusion.

The pumping mechanism has the still more difficult task of pumping sodium ions ($Na^+$) out of the cell against a potential gradient of 130 millivolts. This figure is obtained by adding the 70 millivolts of internal negative charge to the equilibrium potential for sodium ions, which is 60 millivolts of internal *positive* charge [*see illustrations on preceding page*]. If it were not for this postulated pump, the concentration of sodium ions inside and outside the cell would be almost the reverse of what is observed.

In their classic studies of nerve-impulse transmission in the giant axon of the squid, A. L. Hodgkin, A. F. Huxley and Bernhard Katz of Britain demonstrated that the propagation of the impulse coincides with abrupt changes in the permeability of the axon membrane. When a nerve impulse has been triggered in some way, what can be described as a gate opens and lets sodium ions pour into the axon during the advance of the impulse, making the interior of the axon locally positive. The process is self-reinforcing in that the flow of some sodium ions through the membrane opens the gate further and makes it easier for others to follow. The sharp reversal of the internal polarity of the membrane constitutes the nerve impulse, which moves like a wave until it has traveled the length of the axon. In the wake of the impulse the sodium gate closes and a potassium gate opens, thereby restoring the normal polarity of the membrane within a millisecond or less.

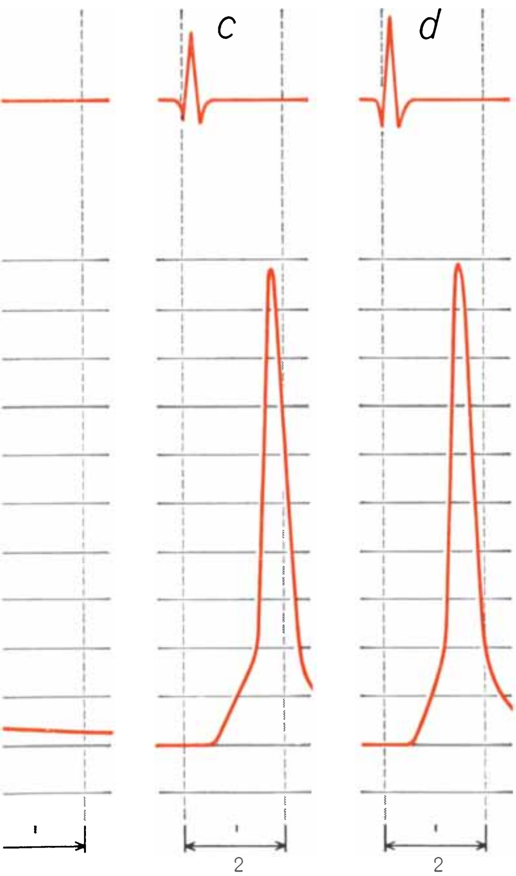
With this understanding of the nerve impulse in hand, one is ready to follow the electrical events at the excitatory synapse. One might guess that if the nerve impulse results from an abrupt inflow of sodium ions and a rapid change in the electrical polarity of the axon's interior, something similar must happen at the body and dendrites of the nerve cell in order to generate the impulse in the first place. Indeed, the function of the excitatory synaptic terminals on the cell body and its dendrites is to depolarize the interior of the cell membrane essentially by permitting an inflow of sodium ions. When the depolarization reaches a threshold value, a nerve impulse is triggered.

As a simple instance of this phenomenon we have recorded the depolarization that occurs in a single motoneuron activated directly by the large nerve fibers that enter the spinal cord from special stretch-receptors known as annulospiral endings. These receptors in turn are located in the same muscle that




**EXCITATION OF A MOTONEURON** is studied by stimulating the sensory fibers that send impulses to it. The size of the "afferent volleys" reaching the motoneuron is displayed on oscilloscope *A*. A microelectrode implanted in the motoneuron measures the changes in the cell's internal electric potential. These changes, called excitatory postsynaptic potentials (EPSP's), appear on oscilloscope *B*. The size of the afferent volley is proportional to the number of fibers stimulated to fire. It is assumed here that one to four fibers can be activated. When only one fiber is activated (*a*), the potential inside the motoneuron shifts only slightly. When two fibers are activated (*b*), the shift is somewhat greater. When three fibers are activated (*c*), the potential reaches the threshold is activated by the motoneuron under study. Thus the whole system forms a typical reflex arc, such as the arc responsible for the patellar reflex, or "knee jerk" [*see illustration on page 56*].

To conduct the experiment we anesthetize an animal (most often a cat) and free by dissection a muscle nerve that contains these large nerve fibers. By applying a mild electric shock to the exposed nerve one can produce a single impulse in each of the fibers; since the impulses travel to the spinal cord almost synchronously they are referred to collectively as a volley. The number of impulses contained in the volley can be reduced by reducing the stimulation applied to the nerve. The volley strength is measured at a point just outside the spinal cord and is displayed on an oscilloscope. About half a millisecond after detection of a volley there is a wavelike change in the voltage inside the motoneuron that has received the volley. The change is detected by a microelectrode



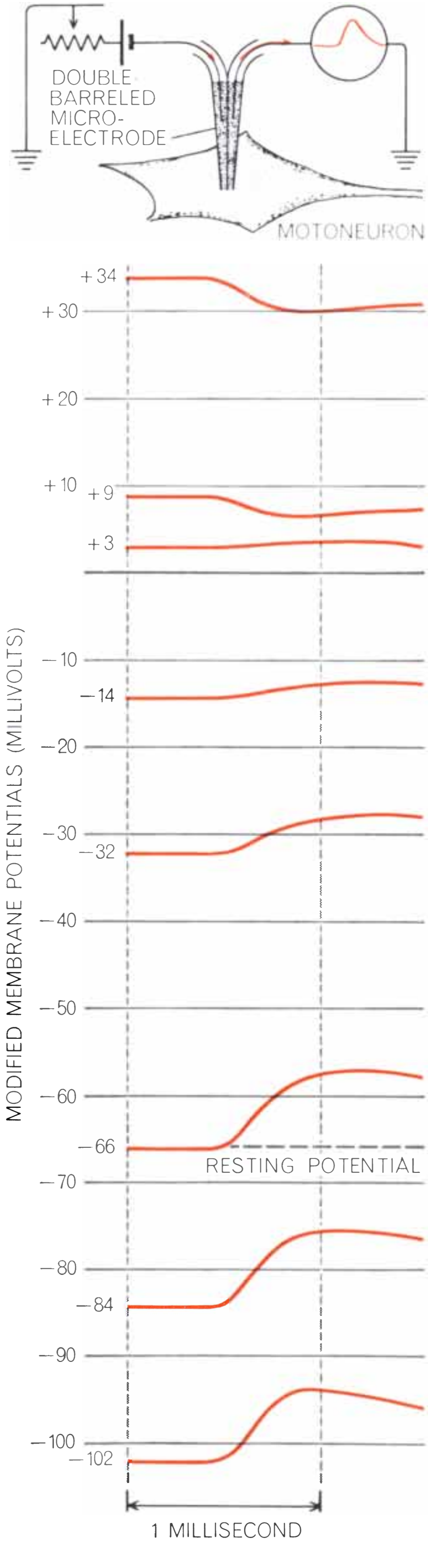

**at which depolarization proceeds swiftly and a spike appears on oscilloscope *B*. The spike signifies that the motoneuron has generated a nerve impulse of its own. When four or more fibers are activated (*d*), the motoneuron reaches the threshold more quickly.**

inserted in the motoneuron and is displayed on another oscilloscope.

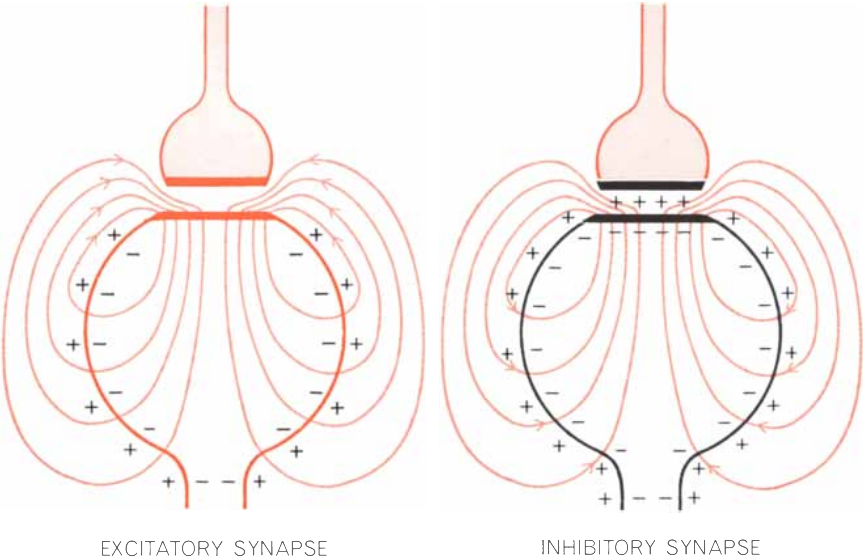
What we find is that the negative voltage inside the cell becomes progressively less negative as more of the fibers impinging on the cell are stimulated to fire. This observed depolarization is in fact a simple summation of the depolarizations produced by each individual synapse. When the depolarization of the interior of the motoneuron reaches a critical point, a "spike" suddenly appears on the second oscilloscope, showing that a nerve impulse has been generated. During the spike the voltage inside the cell changes from about 70 millivolts negative to as much as 30 millivolts positive. The spike regularly appears when the depolarization, or reduction of membrane potential, reaches a critical level, which is usually between 10 and 18 millivolts. The only effect of a further strengthening of the synaptic stimulus is to shorten the time needed for the motoneuron to reach the firing threshold [*see illustration at left*]. The depolarizing potentials produced in the cell membrane by excitatory synapses are called excitatory postsynaptic potentials, or EPSP's.

Through one barrel of a double-barreled microelectrode one can apply a background current to change the resting potential of the interior of the cell membrane, either increasing it or decreasing it. When the potential is made more negative, the EPSP rises more steeply to an earlier peak. When the potential is made less negative, the EPSP rises more slowly to a lower peak. Finally, when the charge inside the cell is reversed so as to be positive with respect to the exterior, the excitatory synapses give rise to an EPSP that is actually the reverse of the normal one [*see illustration at right*].

These observations support the hypothesis that excitatory synapses produce what amounts virtually to a short circuit in the synaptic membrane potential. When this occurs, the membrane no longer acts as a barrier to the passage of ions but lets them flow through in response to the differing electric potential on the two sides of the membrane. In other words, the ions are momentarily allowed to travel freely down their electrochemical gradients, which means that sodium ions flow into the cell and, to a lesser degree, potassium ions flow out. It is this net flow of positive ions that creates the excitatory postsynaptic potential. The flow of negative ions, such as the chloride ion, is apparently not involved. By artificially




**MANIPULATION of the resting potential of a motoneuron clarifies the nature of the EPSP. A steady background current applied through the left barrel of a microelectrode (*top*) shifts the membrane potential away from its normal resting level (minus 66 millivolts in this particular cell). The other barrel records the EPSP. The equilibrium potential, the potential at which the EPSP reverses direction, is about zero millivolts.**




**CURRENT FLOWS induced by excitatory and inhibitory synapses are respectively shown at left and right. When the nerve cell is at rest, the interior of the cell membrane is uniformly negative with respect to the exterior. The excitatory synapse releases a chemical substance that depolarizes the cell membrane below the synaptic cleft, thus letting current flow into the cell at that point. At an inhibitory synapse the current flow is reversed.**

altering the potential inside the cell one can establish that there is no flow of ions, and therefore no EPSP, when the voltage drop across the membrane is zero.

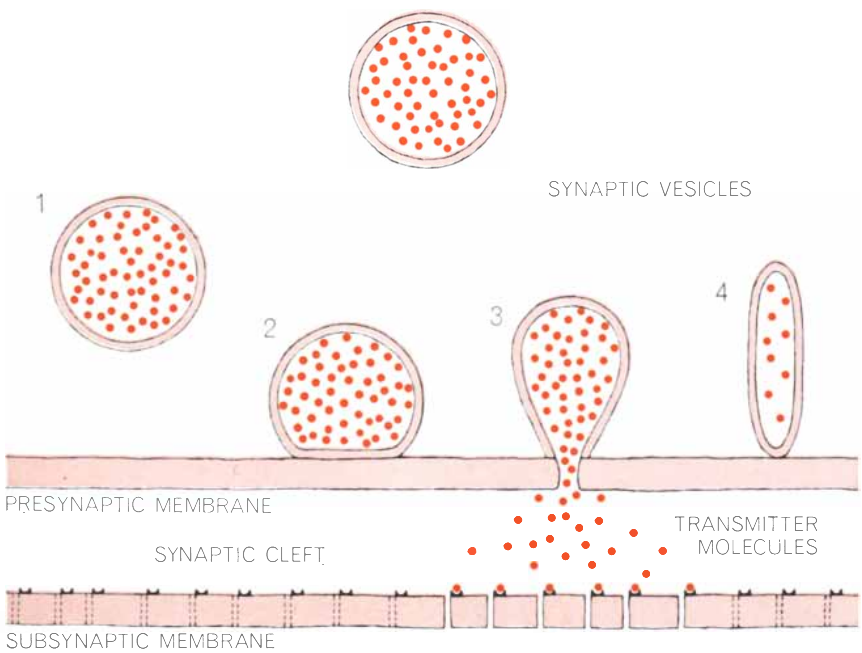
How is the synaptic membrane converted from a strong ionic barrier into an ion-permeable state? It is currently accepted that the agency of conversion is the chemical transmitter substance contained in the vesicles inside the synaptic knob. When a nerve impulse reaches the synaptic knob, some of the vesicles are caused to eject the transmitter substance into the synaptic cleft [*see illustration below*]. The molecules of the substance would take only a few microseconds to diffuse across the cleft and become attached to specific receptor sites on the surface membrane of the adjacent nerve cell.

Presumably the receptor sites are associated with fine channels in the membrane that are opened in some way by the attachment of the transmitter-substance molecules to the receptor sites. With the channels thus opened, sodium and potassium ions flow through the membrane thousands of times more readily than they normally do, thereby producing the intense ionic flux that depolarizes the cell membrane and produces the EPSP. In many synapses the current flows strongly for only about a millisecond before the transmitter substance is eliminated from the synaptic cleft, either by diffusion into the surrounding regions or as a result of being destroyed by enzymes. The latter process is known to occur when the transmitter substance is acetylcholine, which is destroyed by the enzyme acetylcholinesterase.

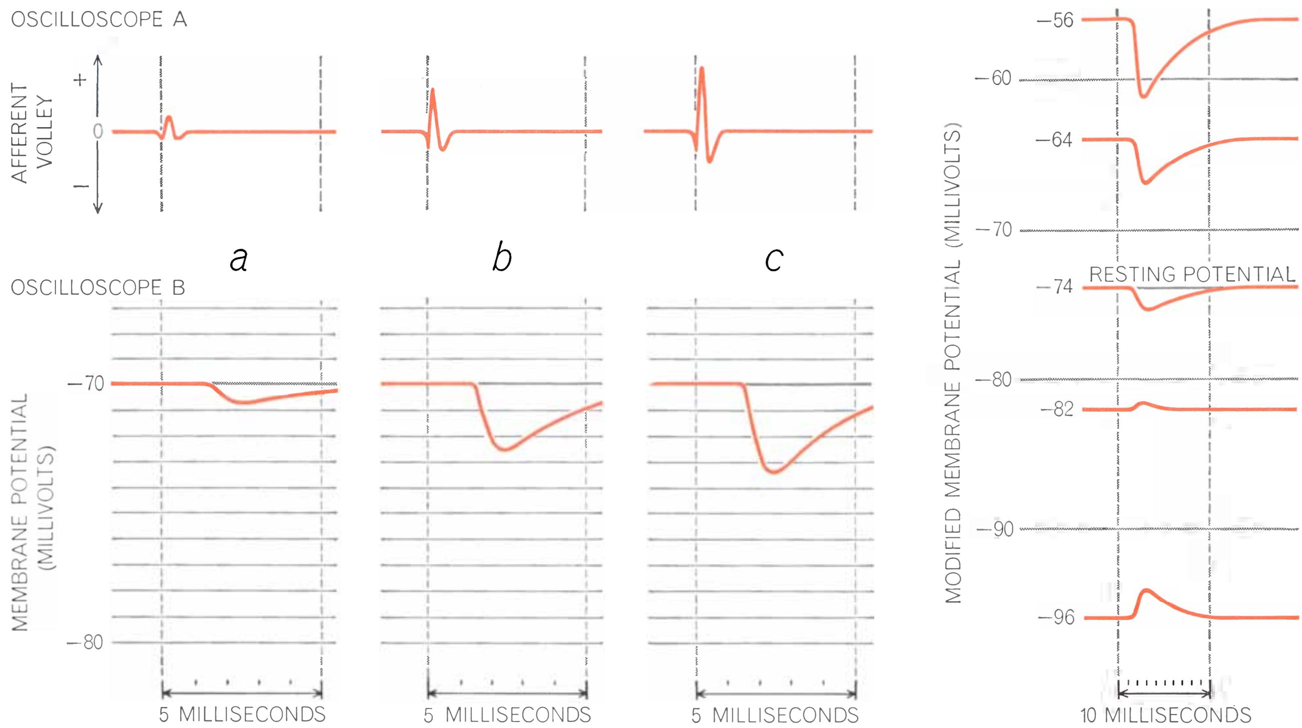
The substantiation of this general picture of synaptic transmission requires the solution of many fundamental problems. Since we do not know the specific transmitter substance for the vast majority of synapses in the nervous system we do not know if there are many different substances or only a few. The only one identified with reasonable certainty in the mammalian central nervous system is acetylcholine. We know practically nothing about the mechanism by which a presynaptic nerve impulse causes the transmitter substance to be injected into the synaptic cleft. Nor do we know how the synaptic vesicles not immediately adjacent to the synaptic cleft are moved up to the firing line to replace the emptied vesicles. It is conjectured that the vesicles contain the enzyme systems needed to recharge themselves. The entire process must be swift and efficient: the total amount of transmitter substance in synaptic terminals is enough for only a few minutes of synaptic activity at normal operating rates. There are also knotty problems to be solved on the other side of the synaptic cleft. What, for example, is the nature of the receptor sites? How are the ionic channels in the membrane opened up?

## The Inhibitory Synapse

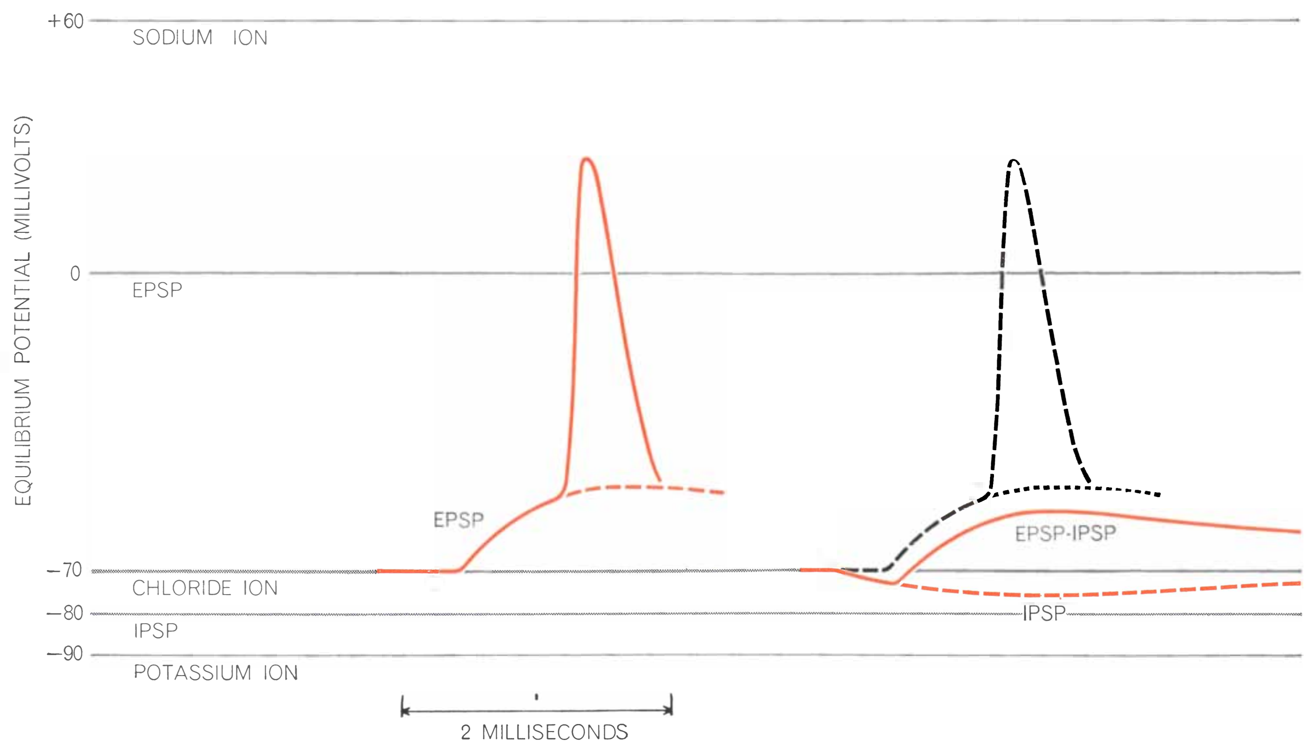
Let us turn now to the second type of synapse that has been identified in the nervous system. These are the synapses that can inhibit the firing of a nerve cell even though it may be receiving a volley of excitatory impulses. When inhibitory synapses are examined in the electron microscope, they look very much like excitatory synapses.



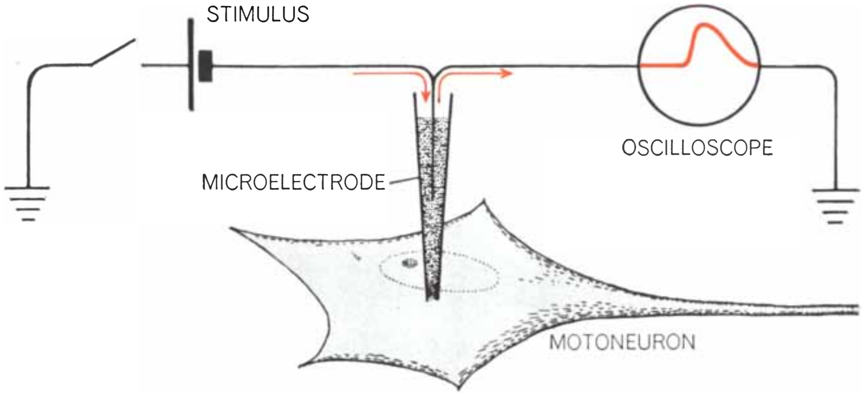

**SYNAPTIC VESICLES containing a chemical transmitter are distributed throughout the synaptic knob. They are arranged here in a probable sequence, showing how they move up to the synaptic cleft, discharge their contents and return to the interior for recharging.**

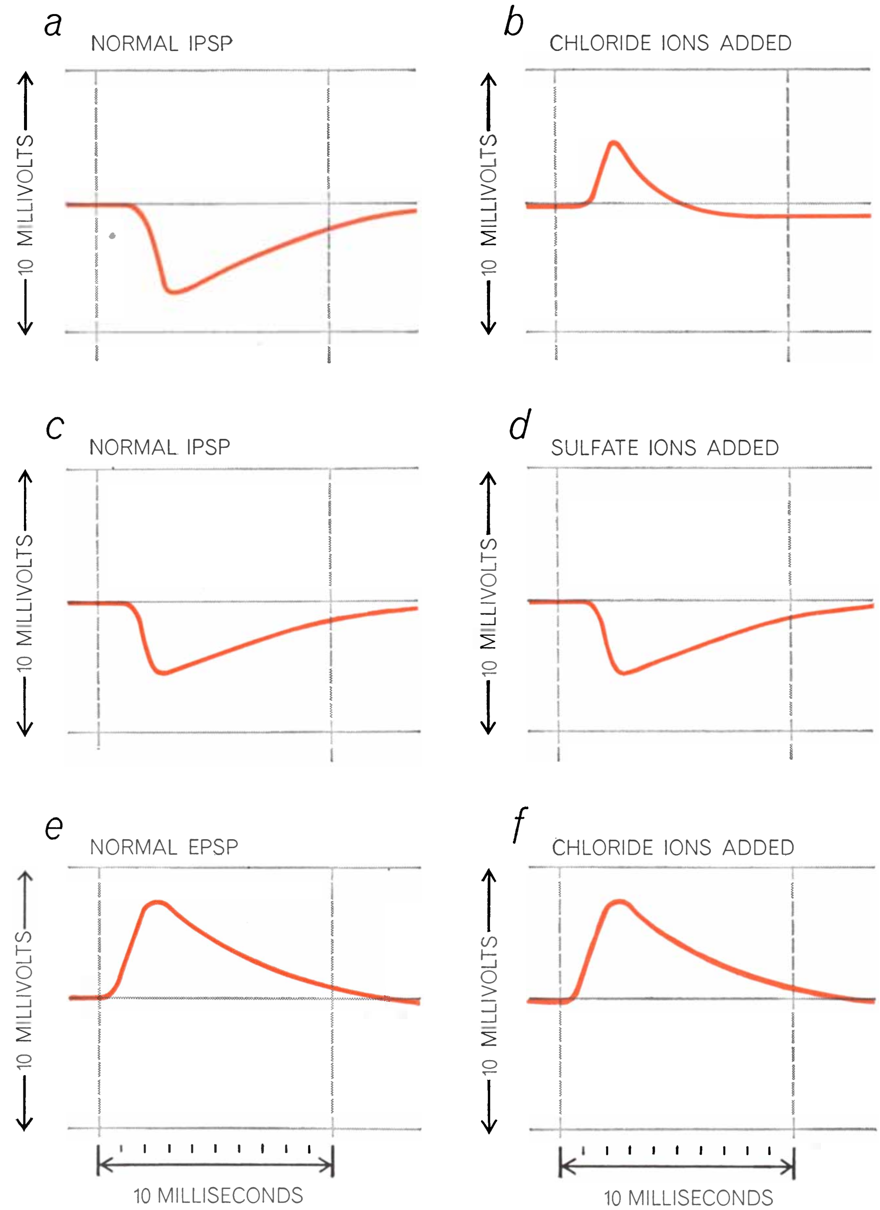



**INHIBITION OF A MOTONEURON** is investigated by methods like those used for studying the EPSP. The inhibitory counterpart of the EPSP is the IPSP: the inhibitory postsynaptic potential. Oscilloscope *A* records an afferent volley that travels to a number of inhibitory nerve cells whose axons form synapses on a nearby motoneuron (*see illustration on page 56*). A microelectrode in the motoneuron is connected to oscilloscope *B*. The sequence *a*, *b* and *c* shows how successively larger afferent volleys produce successively deeper IPSP's. Curves at right show how the IPSP is modified when a background current is used to change the motoneuron's resting potential. The equilibrium potential where the IPSP reverses direction is about minus 80 millivolts.




**INHIBITION OF A SPIKE DISCHARGE** is an electrical subtraction process. When a normal EPSP reaches a threshold (*left*), it will ordinarily produce a spike. An IPSP widens the gap between the cell's internal potential and the firing threshold. Thus if a cell is simultaneouly subjected to both excitatory and inhibitory stimulation, the IPSP is subtracted from the EPSP (*right*) and no spike occurs. The five horizontal lines show equilibrium potentials for the three principal ions as well as for the EPSP and IPSP.




**MODIFICATION OF ION CONCENTRATION within the nerve cell gives information about the permeability of the cell membrane. The internal ionic composition is altered by injecting selected ions through a microelectrode a minute or so before applying an afferent volley and recording the EPSP or IPSP. In the first experiment a normal IPSP (*a*) is changed to a pseudo-EPSP (*b*) by an injection of chloride ions. When sulfate ions are similarly injected, the IPSP is practically unchanged (*b, c*). The third experiment shows that an injection of chloride ions has no significant effect on the EPSP (*e, f*).**

(There are probably some subtle differences, but they need not concern us here.) Microelectrode recordings of the activity of single motoneurons and other nerve cells have now shown that the inhibitory postsynaptic potential (IPSP) is virtually a mirror image of the EPSP [*see top illustration on preceding page*]. Moreover, individual inhibitory synapses, like excitatory synapses, have a cumulative effect. The chief difference is simply that the IPSP makes the cell's internal voltage more negative than it is normally, which is in a direction opposite to that needed for generating a spike discharge.

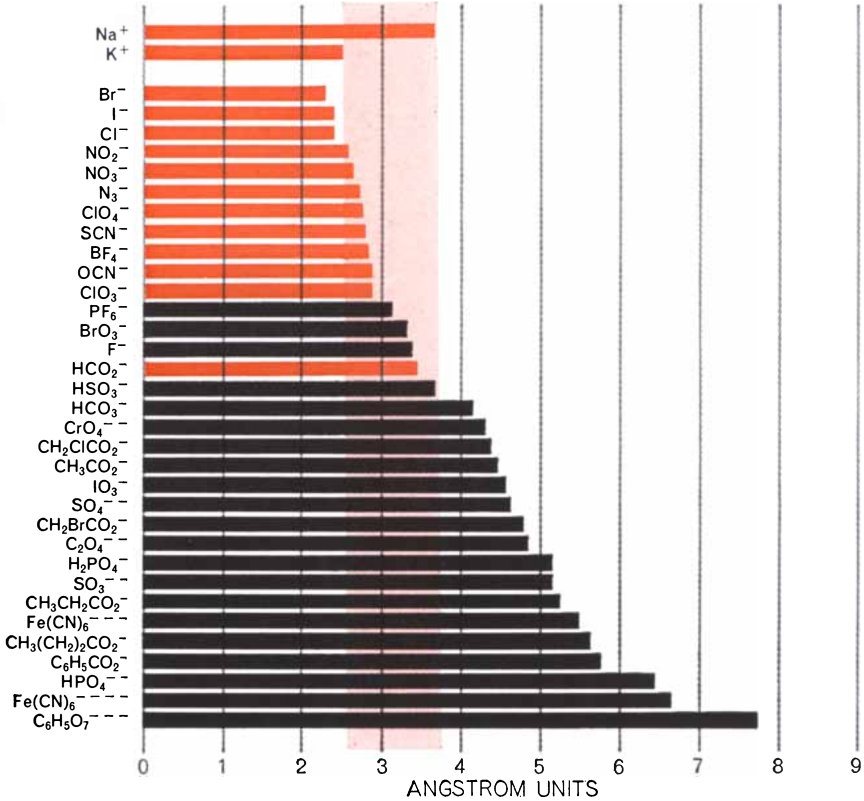
By driving the internal voltage of a nerve cell in the negative direction inhibitory synapses oppose the action of excitatory synapses, which of course drive it in the positive direction. Hence if the potential inside a resting cell is 70 millivolts negative, a strong volley of inhibitory impulses can drive the potential to 75 or 80 millivolts negative. One can easily see that if the potential is made more negative in this way the excitatory synapses find it more difficult to raise the internal voltage to the threshold point for the generation of a spike. Thus the nerve cell responds to the algebraic sum of the internal voltage changes produced by excitatory and inhibitory synapses [*see bottom illustration on preceding page*].

If, as in the experiment described earlier, the internal membrane potential is altered by the flow of an electric current through one barrel of a double-barreled microelectrode, one can observe the effect of such changes on the inhibitory postsynaptic potential. When the internal potential is made less negative, the inhibitory postsynaptic potential is deepened. Conversely, when the potential is made more negative, the IPSP diminishes; it finally reverses when the internal potential is driven below minus 80 millivolts.

One can therefore conclude that inhibitory synapses share with excitatory synapses the ability to change the ionic permeability of the synaptic membrane. The difference is that inhibitory synapses enable ions to flow freely down an electrochemical gradient that has an equilibrium point at minus 80 millivolts rather than at zero, as is the case for excitatory synapses. This effect could be achieved by the outward flow of positively charged ions such as potassium or the inward flow of negatively charged ions such as chloride, or by a combination of negative and positive ionic flows such that the interior reaches equilibrium at minus 80 millivolts.

In an effort to discover the permeability changes associated with the inhibitory potential my colleagues and I have altered the concentration of ions normally found in motoneurons and have introduced a variety of other ions that are not normally present. This can be done by impaling nerve cells with micropipettes that are filled with a salt solution containing the ion to be injected. The actual injection is achieved by passing a brief current through the micropipette.

If the concentration of chloride ions within the cell is in this way increased as much as three times, the inhibitory postsynaptic potential reverses and acts as a depolarizing current; that is, it resembles an excitatory potential. On the other hand, if the cell is heavily injected with sulfate ions, which are also negatively charged, there is no such reversal [*see illustration on opposite page*]. This simple test shows that under the influence of the inhibitory transmitter substance, which is still unidentified, the subsynaptic membrane becomes permeable momentarily to chloride ions but not to sulfate ions. During the generation of the IPSP the outflow of chloride ions is so rapid that it more than outweighs the flow of other ions that generate the normal inhibitory potential.




**MEMBRANE PERMEABILITY during synaptic inhibition has been determined for 33 kinds of negative ion. The size of sodium ions ($Na^+$) and potassium ions ($K^+$) when hydrated (bound to water molecules) is shown at the top of the chart. All the ions that pass through the membrane are smaller than the hydrated sodium ion and with one exception ($HCO_2^-$, the formate ion) all are smaller than 2.9 angstrom units in diameter.**

My colleagues have now tested the effect of injecting motoneurons with more than 30 kinds of negatively charged ion. With one exception the hydrated ions (ions bound to water) to which the cell membrane is permeable under the influence of the inhibitory transmitter substance are smaller than the hydrated ions to which the membrane is impermeable. The exception is the formate ion ($HCO_2^-$), which may have an ellipsoidal shape and so be able to pass through membrane pores that block smaller spherical ions.

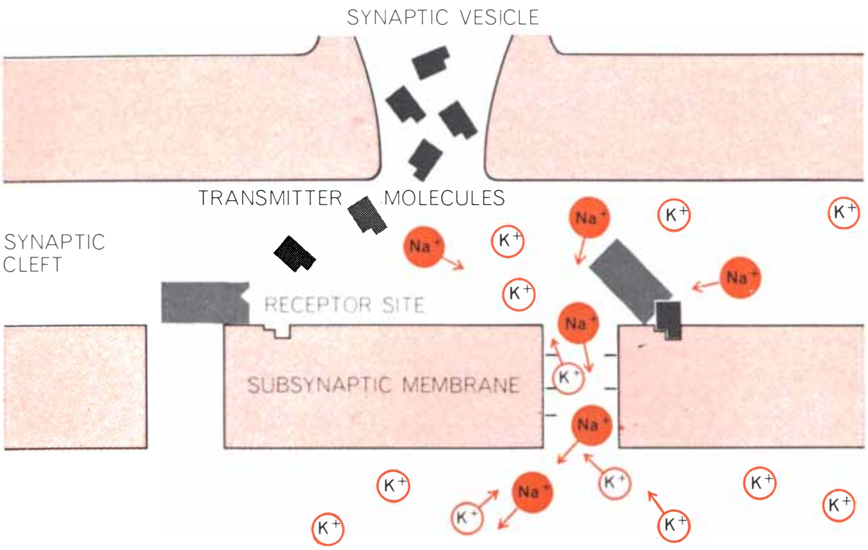
Apart from the formate ion all the ions to which the membrane is permeable have a diameter not greater than 1.14 times the diameter of the potassium ion; that is, they are less than 2.9 angstrom units in diameter. Comparable investigations in other laboratories have found the same permeability effects, including the exceptional behavior of the formate ion, in fishes, toads and snails. It may well be that the ionic mechanism responsible for synaptic inhibition is the same throughout the animal kingdom.

The significance of these and other studies is that they strongly indicate that the inhibitory transmitter substance opens the membrane to the flow of potassium ions but not to sodium ions. It is known that the sodium ion is somewhat larger than any of the negatively charged ions, including the formate ion, that are able to pass through the membrane during synaptic inhibition. It is not possible, however, to test the effectiveness of potassium ions by injecting excess amounts into the cell because the excess is immediately diluted by an osmotic flow of water into the cell.

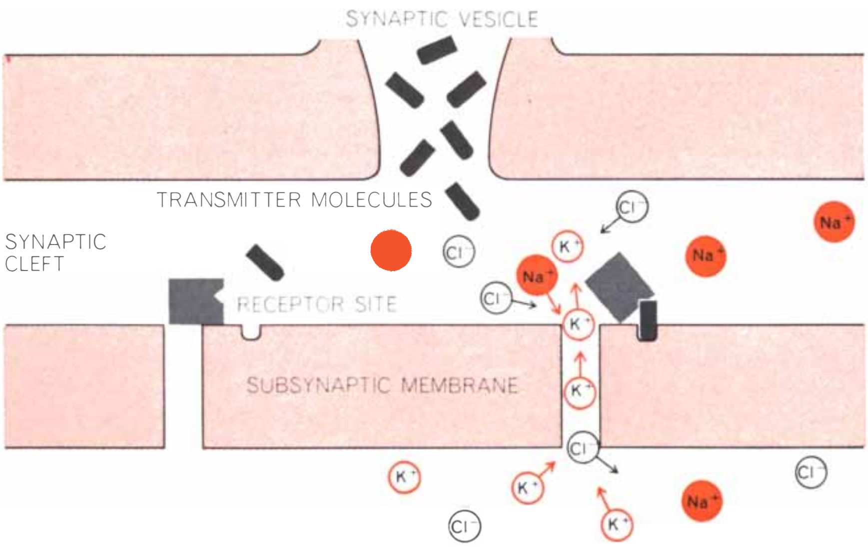
As I have indicated, the concentration of potassium ions inside the nerve cell is about 30 times greater than the concentration outside, and to maintain this large difference in concentration without the help of a metabolic pump the inside of the membrane would have to be charged 90 millivolts negative with respect to the exterior. This implies that if the membrane were suddenly made porous to potassium ions, the resulting outflow of ions would make the inside potential of the membrane even more negative than it is in the resting state, and that is just what happens during synaptic inhibition. The membrane must not simultaneously become porous to sodium ions, because they exist in much higher concentration outside the cell than inside and their rapid inflow would more than compensate for the potassium outflow. In fact, the fundamental difference between synaptic excitation and synaptic inhibition is that the membrane freely passes sodium ions in response to the former and largely excludes the passage of sodium ions in response to the latter.

## Channels in the Membrane

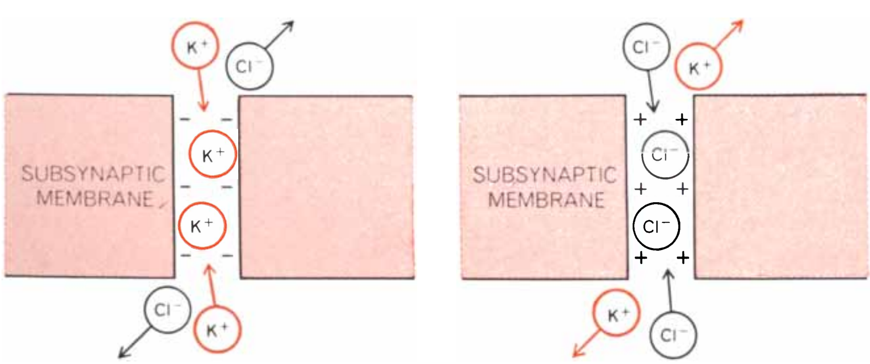
This fine discrimination between ions that are not very different in size must be explained by any hypothesis of synaptic action. It is most unlikely that the channels through the membrane are created afresh and accurately maintained for a thousandth of a second every time a burst of transmitter substance is released into the synaptic cleft. It is more likely that channels of at least two different sizes are built directly into the membrane structure. In some way the excitatory transmitter substance would selectively unplug the larger channels and permit the free inflow of sodium ions. Potassium ions would simultaneously flow out and thus would tend to counteract the large potential change that would be produced by the massive




**EXCITATORY SYNAPSE may employ transmitter molecules that open large channels in the nerve-cell membrane. This would permit sodium ions, which are plentiful outside the cell, to pour through the membrane freely. The outward flow of potassium ions, driven by a smaller potential gradient, would be at a much slower rate. Chloride ions (*not shown*) may be prevented from flowing by negative charges on the channel walls.**




**INHIBITORY SYNAPSE may employ another type of transmitter molecule that opens channels too small to pass sodium ions. The net outflow of potassium ions and inflow of chloride ions would account for the hyperpolarization that is observed as an IPSP.**




**MODIFICATIONS OF INHIBITORY SYNAPSE may involve channels that carry either negative or positive charges on their walls. Negative charges (*left*) would permit only potassium ions to pass. Positive charges (*right*) would permit only chloride ions to pass.**

sodium inflow. The inhibitory transmitter substance would selectively unplug the smaller channels that are large enough to pass potassium and chloride ions but not sodium ions [*see upper two illustrations at left*].

To explain certain types of inhibition other features must be added to this hypothesis of synaptic transmission. In the simple hypothesis chloride and potassium ions can flow freely through pores of all inhibitory synapses. It has been shown, however, that the inhibition of the contraction of heart muscle by the vagus nerve is due almost exclusively to potassium-ion flow. On the other hand, in the muscles of crustaceans and in nerve cells in the snail's brain synaptic inhibition is due largely to the flow of chloride ions. This selective permeability could be explained if there were fixed charges along the walls of the channels. If such charges were negative, they would repel negatively charged ions and prevent their passage; if they were positive, they would similarly prevent the passage of positively charged ions. One can now suggest that the channels opened by the excitatory transmitter are negatively charged and so do not permit the passage of the negatively charged chloride ion, even though it is small enough to move through the channel freely.

One might wonder if a given nerve cell can have excitatory synaptic action at some of its axon terminals and inhibitory action at others. The answer is no. Two different kinds of nerve cell are needed, one for each type of transmission and synaptic transmitter substance. This can readily be demonstrated by the effect of strychnine and tetanus toxin in the spinal cord; they specifically prevent inhibitory synaptic action and leave excitatory action unaltered. As a result the synaptic excitation of nerve cells is uncontrolled and convulsions result. The special types of cell responsible for inhibitory synaptic action are now being recognized in many parts of the central nervous system.

This account of communication between nerve cells is necessarily oversimplified, yet it shows that some significant advances are being made at the level of individual components of the nervous system. By selecting the most favorable situations we have been able to throw light on some details of nerve-cell behavior. We can be encouraged by these limited successes. But the task of understanding in a comprehensive way how the human brain operates staggers its own imagination.

# Our 110B: the non-automated Polaroid camera for non-automated people.

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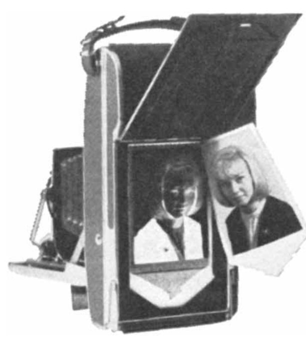

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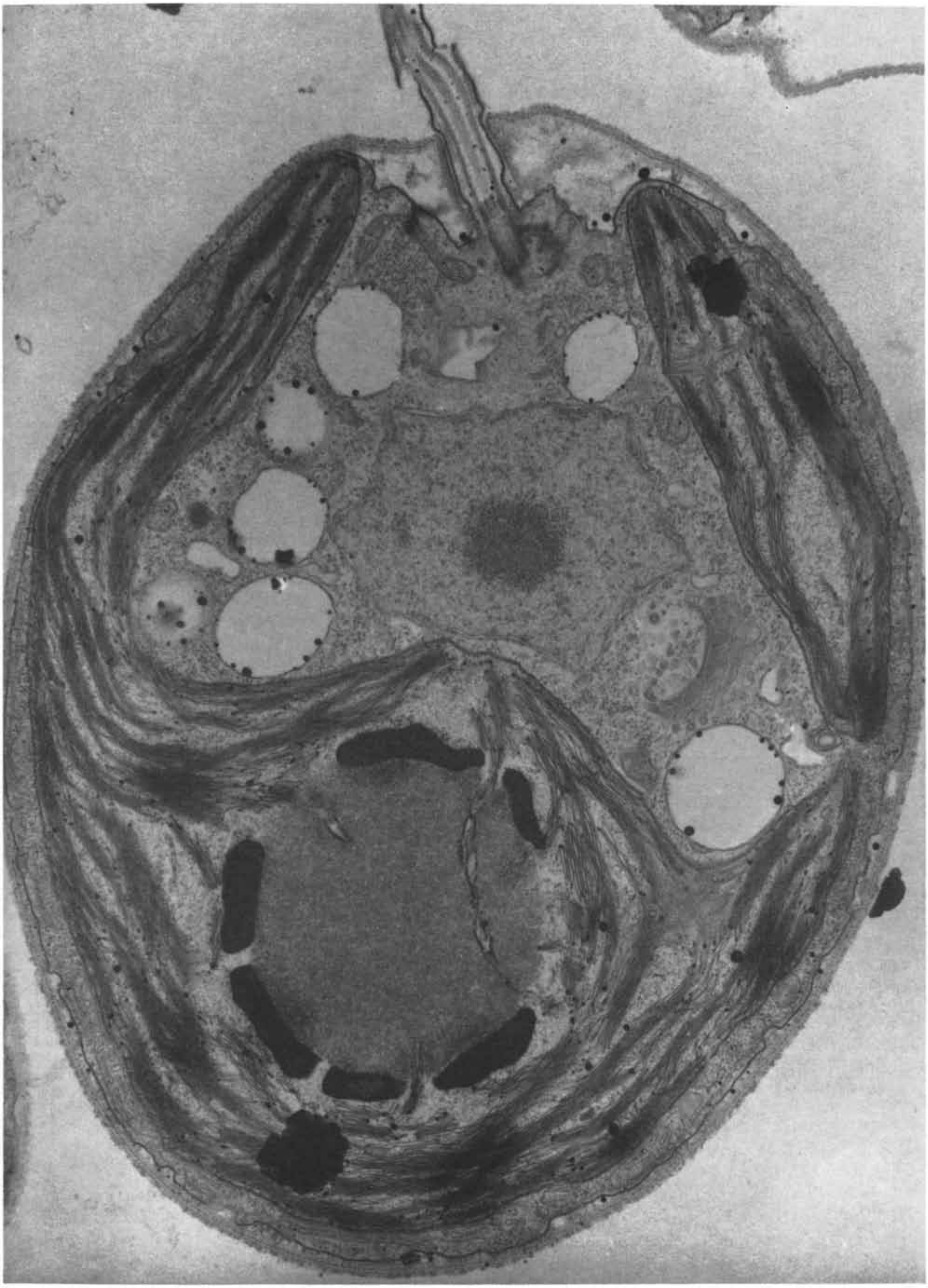


CHLAMYDOMONAS, the one-celled alga that is the subject of the author's genetic experiments, is enlarged about 26,000 diameters in an electron micrograph made by George Palade of the Rockefeller Institute. Major organelles are identified on opposite page.

# GENES OUTSIDE THE CHROMOSOMES

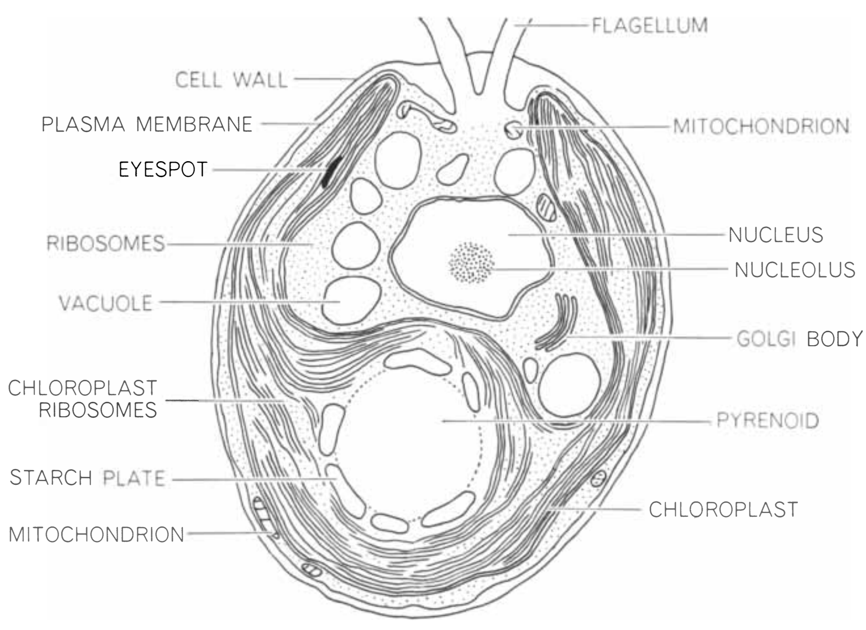
## There has been evidence for many years that hereditary information exists apart from the chromosomal genes. Now nonchromosomal genes have been identified by the traditional methods of genetic analysis

by Ruth Sager

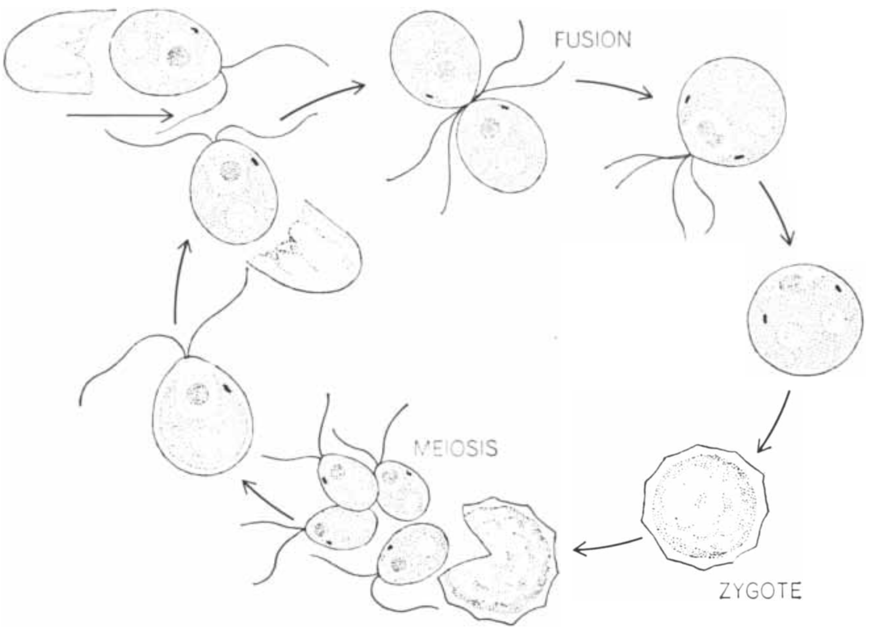
The science of genetics encompasses any and all systems involved in the heredity of living organisms, but in practice it has been largely restricted to the study of chromosomal genes: the molecular carriers of heredity that are arranged in linear order in the threadlike chromosomes in the nucleus of the living cell. The fact is, however, that more information is involved in the heredity of organisms than is contained in the chromosomes. Evidence of additional genetic systems has been accumulating slowly for some time. Within the past few years studies in my laboratory at Columbia University have demonstrated the existence of a second genetic system that is quite amenable to analysis. This system consists of a class of genetic determinants located elsewhere than in the chromosomes. These determinants are particulate elements, stably replicated and transmitted from one generation to the next; they can exist in either normal or mutant form and they influence a wide variety of cell traits. They are probably composed, like the chromosomal genes, of nucleic acid. No one has yet isolated one of these determinants, and they have still to be located in a specific region of the cell, but on the basis of genetic analysis they fully deserve to be called nonchromosomal genes.

The first example of a nonchromosomal gene was described in 1908 by the German botanist Carl Correns, one of the three biologists who had rediscovered the laws of inheritance enunciated in 1865 by Gregor Mendel. Correns thought it likely that all organisms had more than one genetic system, and he and his students investigated the problem in a number of plant species. Interest in nonchromosomal inheritance was soon overshadowed, however, by the explosive development of chromosomal genetics. The convincing experiments that correlated the distribution of Mendel's "factors" of heredity with the observable details of chromosome behavior focused attention on the chromosomal genes to the exclusion of other hereditary phenomena. Biologists tended to forget that nothing in the orderly methodology of chromosomal genetics precluded the existence of additional genetic systems. Although a few hundred well-authenticated examples of nonchromosomal traits have been described in organisms as diverse as insects, flowering plants, algae, yeasts and fungi, this evidence has largely been ignored in textbooks and in the mainstream of genetic theory. This is not to say, of course, that it was not best to concentrate on the chromosomal system first. It is the most accessible system, and it must not only be understood but also brought under control in order to conduct experiments dealing with nonchromosomal inheritance.

In principle the difference in the patterns of inheritance of the two systems is elementary. The salient fact of Mendelian, or chromosomal, heredity is that genes from the male and female parents contribute equally to the genetic constitution of the progeny. This is true in diploid organisms, whose body cells have two sets of chromosomes, and in the more primitive haploid organisms, which have only one set [*see top illustration on page 73*]. A nonchromosomal gene is identified by its failure to follow the Mendelian pattern. Instead the nonchromosomal genes from the female par-




**ORGANELLES of *Chlamydomonas* cell include the chloroplast for photosynthesis, ribosomes for protein synthesis and mitochondria for energy conversion. The chromosomes are in the nucleus. The flagella provide motility and the eyespot is a rudimentary eye.**




**LIFE CYCLE of *Chlamydomonas* includes a sexual phase. Two cells of opposite sex shed their cell walls and form a zygote that undergoes meiosis and produces four progeny cells. These may then divide asexually (*not shown here*) to form clones, or colonies of cells.**

ent are usually transmitted, during the sexual cell-division process called meiosis, to all the progeny and those from the male to none [*see bottom illustration on opposite page*]. The most obvious explanation for this maternal inheritance pattern in most organisms is that the nonchromosomal genes are outside the nucleus in the cytoplasm of the cell; in higher plants and animals it is the female germ cell, not the male pollen or sperm, that supplies most of the cytoplasm for the fertilized egg. This explanation does not account for maternal inheritance in some haploid organisms, however; there are microorganisms in which the male and female contribute equal amounts of cytoplasm in fusing to form new cells. The mechanism of maternal inheritance must therefore involve more than the mere presence or absence of certain cell constituents.

Whatever the mechanism, maternal inheritance is the primary criterion for the recognition of nonchromosomal genes. To classify an inheritable trait as being chromosomal or nonchromosomal one needs only to study its pattern of transmission in test crosses. Two major difficulties account for the fact that in spite of this apparent ease of analysis the investigation of nonchromosomal genes has made little progress until recently.

The first difficulty has been in finding mutant genes. Genetics is based on comparisons between "wild type," or normal, genes and mutant, or altered, forms. An unusual trait—say white eyes in a normally red-eyed fruit fly—signals the presence of a mutant gene that can be followed through generations and studied in detail. Spontaneous mutations of nonchromosomal genes in natural populations are rare in comparison with those of the chromosomal genes. Moreover, the radiations and chemical compounds with which geneticists induce chromosomal mutations have not been effective in producing nonchromosomal mutations. The result is that material with which to study the nonchromosomal system—and even to decide just how important it is in any organism's total heredity—has been hard to come by.

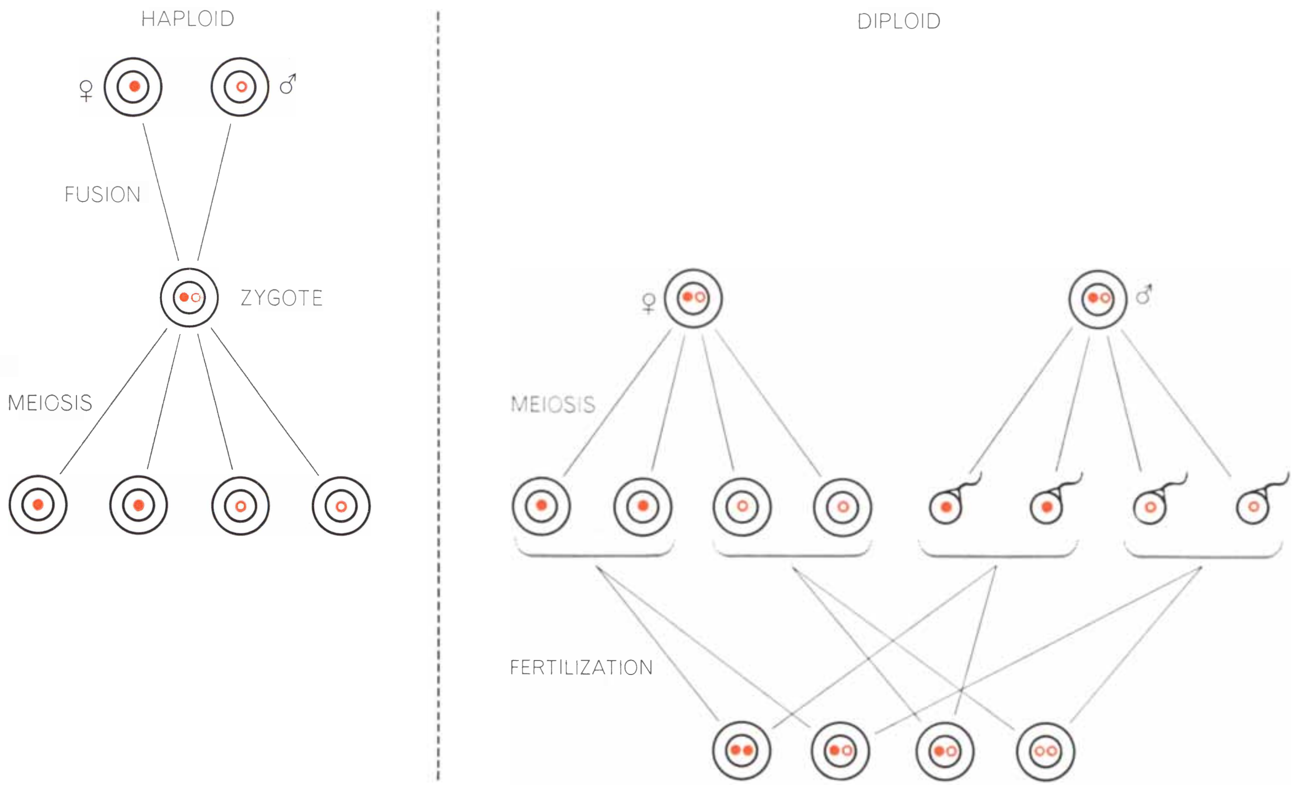
The second difficulty in the study of nonchromosomal heredity lies in the maternal pattern of inheritance that most clearly distinguishes it. As long as all of the female parent's nonchromosomal genes, and none of the male parent's, are transmitted to all the progeny, one cannot bring to bear the standard techniques of genetic analysis, which depend on the manner in which the two parents' genes are distributed and expressed among the progeny.

We have been able to overcome both of these technical difficulties in my laboratory by working with a very cooperative microorganism, the green alga *Chlamydomonas*. This organism belongs to the class of the phytoflagellates, haploid organisms on the main line of evolution to both the higher plants and the animals. Although it can be handled in the laboratory as easily as a simple bacterium, *Chlamydomonas* contains within its single cell the primitive counterparts of such specialized tissues as eye, kidney and muscle. The organelles, or subcellular organs, of *Chlamydomonas* are similar to those of the cells of higher forms. There are mitochondria for generating energy, ribosomes for synthesizing proteins, a large chloroplast for photosynthesis and other such typical structures. In addition the organism reproduces sexually, having two "mating types," or sexes, determined by a single chromosomal gene. Other genes have been located in all of its eight chromosomes.

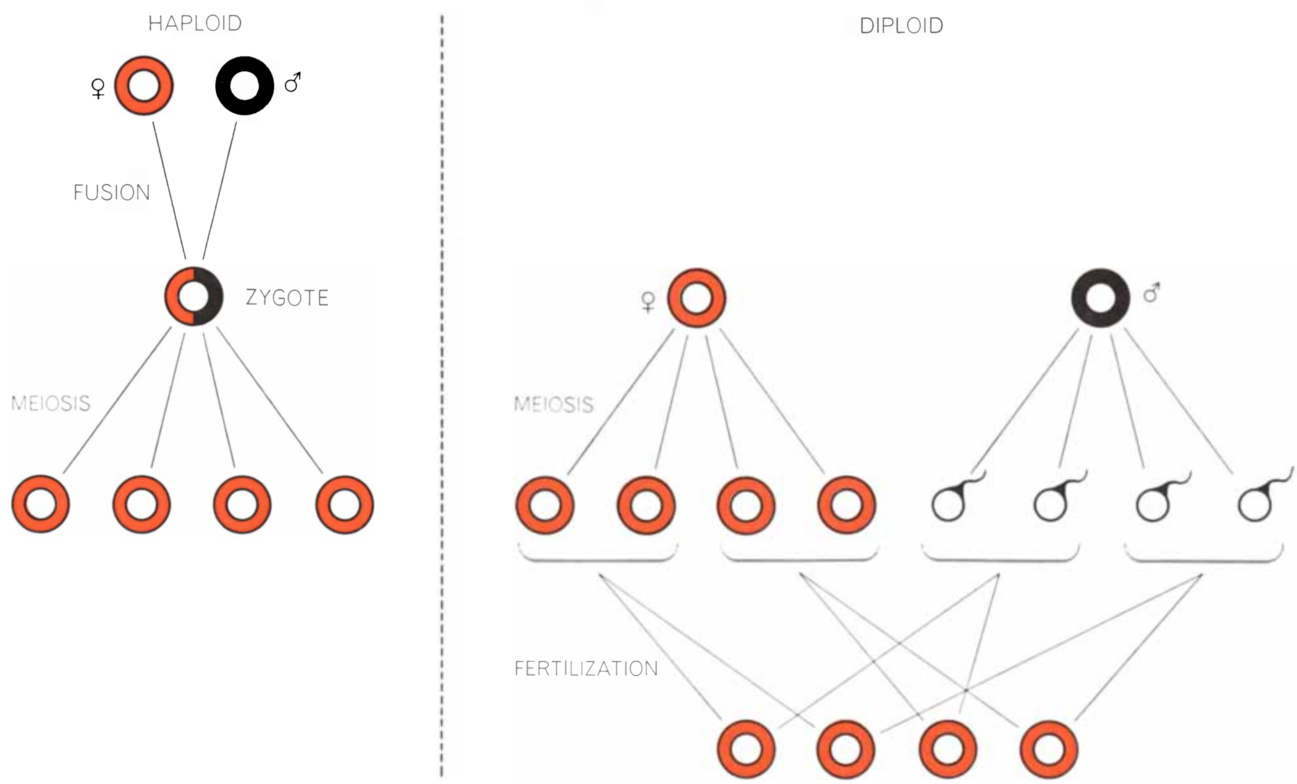
Some years ago we began to search for a compound that would induce nonchromosomal genes of *Chlamydomonas* to mutate. We found the first such mutant by accident. We had placed cells susceptible to the antibiotic streptomycin on a culture medium that contained the antibiotic, and about one in a million cells survived and multiplied to form streptomycin-resistant colonies. Each colony resulted from a mutation—from streptomycin-sensitive (*ss*) to streptomycin-resistant (*sr*). Crossing tests showed that most of these mutations were chromosomal but that about 10 percent of them had a maternal-inheritance pattern: they were nonchromosomal. We found that the chromosomal mutations were spontaneous, arising at random before the cells were placed on a streptomycin-containing medium. The nonchromosomal-gene mutations, however, arose only after sensitive cells had been grown in the presence of toxic but sublethal concentrations of streptomycin. They were induced mutations.

The question then arose whether the drug was inducing specific mutations to drug-resistance or acting as a general mutagen. Subsequent studies made it clear that the streptomycin was acting nonspecifically, and that it could induce mutations in many different nonchromosomal genes. Under special inducing conditions each cell treated with streptomycin multiplies to form a colony, and we find mutant cells of various kinds in almost every colony. This remarkably high level of mutagenic efficiency is quite different from that of chromosomal-gene mutagens, which affect only a small fraction of a population.

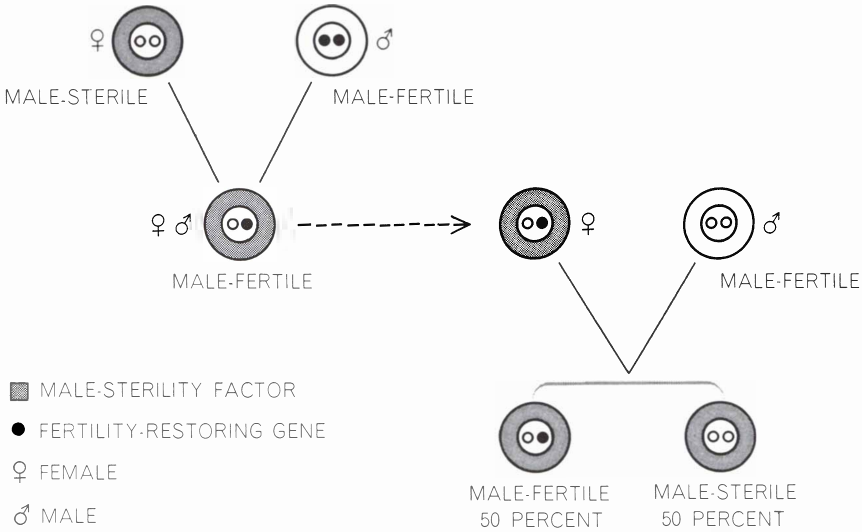
Do nonchromosomal genes control a class of cell traits different from the




**CHROMOSOMAL GENES** are transmitted as shown here. In haploid organisms such as *Chlamydomonas* (*left*) two cells fuse; the resulting zygote undergoes meiosis to produce four progeny. Higher plants and animals are diploid (*right*). Precursors of female and male germ cells undergo meiosis to form germ cells (eggs and sperm or pollen) and fertilization follows. In haploid and diploid organisms chromosomal genes (*colored dots and circles*) from male and female parent contribute equally to progeny.

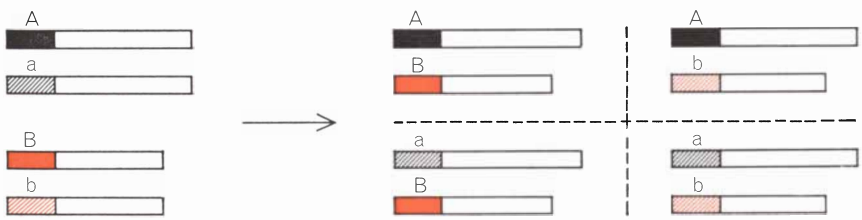



**NONCHROMOSOMAL GENES**, however, exhibit maternal inheritance. In haploid (*left*) and diploid (*right*) organisms all progeny ordinarily get all their nonchromosomal genes (*colored rings*) from the female parent; genes from the male (*black*) are lost.

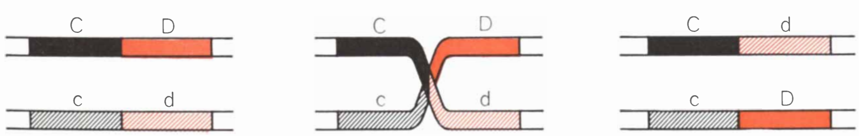



**INTERACTION of chromosomal and nonchromosomal genes in corn is shown. The chromosomal fertility-restoring gene (*black dot*) counteracts the nonchromosomal male-sterility factor (*gray ring*); progeny (*middle left*) are fertile. When female plants of this line are crossed with plants lacking the fertility-restorer, however, half of the progeny are male-sterile, because the fertility-restoring gene is distributed to only half of the progeny.**

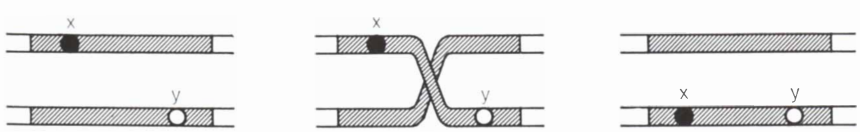
traits determined by chromosomal genes? Probably not. We have so far studied about 30 different nonchromosomal-gene mutations, and most of them resemble chromosomal mutations in their effects; in many cases both kinds of gene affect the identical traits. Even when they influence different traits, the two genetic systems are apparently closely integrated in their action. For example, in *Chlamydomonas* there is a chromosomal gene *A* (for "amplifier"); this gene has no apparent effect in streptomycin-sensitive cells, but in the resistant mutants it greatly amplifies the cell's resistance to the antibiotic. It is equally effective in amplifying resistance brought about by either chromosomal or nonchromosomal genes. This interaction is purely physiological; gene *A* has no influence on the inheritance of either kind of resistant mutation.




**UNLINKED RECOMBINATION is diagrammed for two genes on different chromosomes (*left*). In the course of meiosis the four alleles may be assorted in four different ways.**




**LINKED RECOMBINATION can result from "crossing over." Alleles that were together on a chromosome (*left*) are separated and appear in new combinations in progeny (*right*).**




**INTRAGENIC RECOMBINATION involves a crossing-over of two portions of one gene. By this process two alleles with mutations (*x*, *y*) might give rise to a normal gene (*right*).**

Similar interactions of chromosomal and nonchromosomal genes have been described in yeast and corn; in the latter the system is of considerable economic importance. Corn plants are hermaphrodites with both male and female flower parts, respectively the pollen-bearing tassel and the ear. Several different nonchromosomal-gene mutations have been found that interfere with normal pollen formation, causing a corn plant to be "male-sterile" although it still produces normal ears. Pollen sterility is valuable in hybrid-seed production because the sterile plants cannot fertilize themselves and cross-fertilization is ensured. Sometimes, however, this male-sterility is suppressed by one of several chromosomal genes called "fertility-restorers." In the presence of the proper restorer gene the nonchromosomal trait of sterility cannot express itself and fertile pollen is produced. Then, in plants of the next generation that have lost the restorer gene, the nonchromosomal gene is again operative [*see top illustration on this page*]. Pollen formation is thus controlled by sets of interacting chromosomal and nonchromosomal genes in a manner analogous to the streptomycin-resistance system in *Chlamydomonas.*

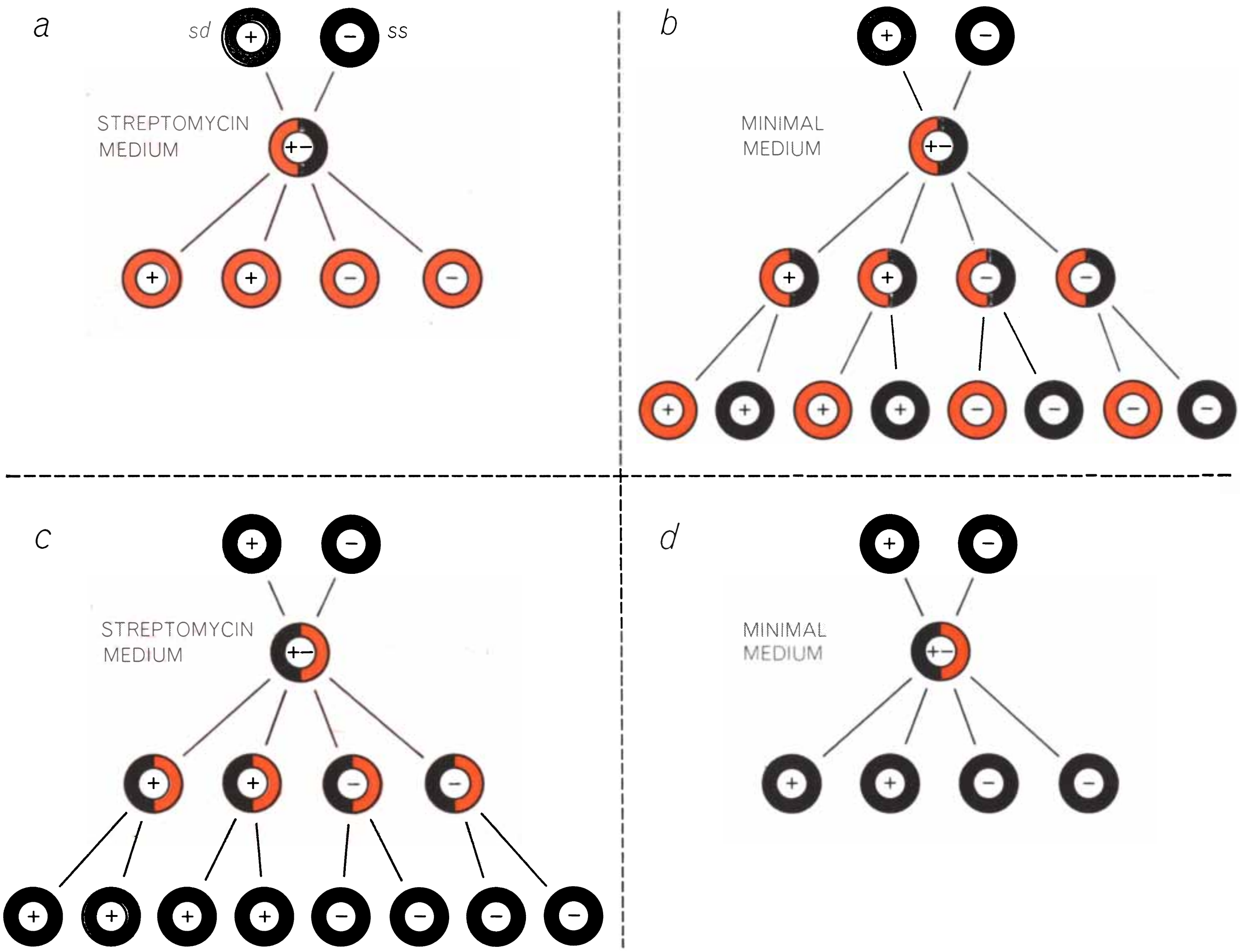
Equipped with a variety of mutant nonchromosomal genes produced by streptomycin treatment, we were ready to face the hard questions: What are these genes and how do they function? We began to approach these questions by applying the classical methods of genetic analysis. The genetic studies I shall report here are interesting not only for the information they revealed but also as a demonstration of the power of genetic analysis.

The two basic processes observed in such analysis are "segregation" and "recombination." Segregation refers to the manner in which a pair of alleles, or alternative forms of the same gene, that come from the two parents are distributed to the progeny. As can be seen in the top illustration on the preceding page, chromosomal genes from both parents are distributed in a strict one-to-one ratio because half of the products of meiosis get the maternal chromosome and half get the paternal one. In the case of nonchromosomal genes [*bottom of preceding page*] there is no segregation: all the progeny get the maternal genes.

Recombination is the process by which genetic material from the two parents is mixed and redistributed among the progeny. If, for example, two genes are on different chromosomes, or "unlinked," they segregate independently in meiosis. A progeny cell may therefore get either allele of each gene [*see third illustration from bottom on opposite page*]. If two genes are on the same chromosome, or "linked," their chances of passing together to the same progeny cell depend on their distance apart along the chromosome. Recombination between linked genes results from an exchange process that occurs more or less at random along the chromosome during meiosis [*see second illustration from bottom on opposite page*]. Recent genetic studies have shown that recombination can occur not only between genes but also within a gene: a progeny cell can receive a gene that is a recombinant of portions of two alleles [*see bottom illustration on opposite page*].

When we set out to look for segregation and recombination of nonchromosomal genes, we of course came up against the problem to which I have alluded: the difficulty of working with a maternal-inheritance system. Nonchromosomal genes in *Chlamydomonas* follow the typical one-parent transmission pattern. The two mating types are designated $mt^-$ and $mt^+$; only the $mt^+$ cell regularly transmits its nonchromosomal genes to the progeny. In such a system segregation and recombination would seem to be precluded. We knew, however, that exceptions to the maternal-inheritance rule had been reported in several organisms. As we worked with the streptomycin-resistance and streptomycin-sensitivity genes we observed that once in a great while the nonchromosomal genes from both parents, rather than from just one, came through to the progeny. We still do not know precisely how this happens, but we have learned to capitalize on the event. We plate the zygotes, the cells that result from fusion, on culture media that favor the survival of the exceptional zygotes in which both parents' nonchromosomal genes survive [*see illustration below*].

By means of this technique Zenta Ramanis and I select exceptional zygotes resulting from multifactor crosses and trace the subsequent segregation and recombination of the nonchromosomal genes. To illustrate the kind of results we have obtained I shall describe a cross involving two pairs of mutant nonchromosomal genes and three unlinked pairs of chromosomal genes that served as markers. In this cross the female parent was designated, on the basis of the genes it carried, $ac_1$ *sd act-r ms-r* $mt^+$; the male parent was $ac_2$ *sr act-s ms-s* $mt^-$. The nonchromosomal genes




**SELECTION of exceptional zygotes that transmit the male parent's nonchromosomal genes is explained. Ordinarily a cross (*a*) between streptomycin-dependent ("*sd*," *colored ring*) female (+) and streptomycin-sensitive ("*ss*," *black ring*) male (—) cells yields only *sd* cells like the female parent, and the zygotes must be plated on streptomycin medium if live progeny are to be recovered. Similarly, in the reciprocal cross (*d*) most zygotes have only *ss* progeny like the female parent. To select exceptional zygotes that transmit nonchromosomal genes from the male parent, one plates the upper cross on minimal medium (*b*) and the lower cross on streptomycin medium (*c*). In both cases exceptional zygotes are selected that transmit both *sd* and *ss* genes to all progeny, which grow equally well with or without streptomycin. Each progeny cell divides asexually to form a clone containing pure *sd* and pure *ss* cells.**

are $ac_1$ and $ac_2$ and also *sd* and *sr; $ac_1$* and $ac_2$ are mutants that block photosynthesis and thereby create a requirement for acetate in the medium; *sd* stands for streptomycin-dependence and *sr* for streptomycin-resistance. Among the chromosomal genes *act-r* and *act-s* determine resistance to the antibiotic actidione, *ms-r* and *ms-s* affect resistance to the substance methionine sulfoximine, and $mt^+$ and $mt^-$ of course determine the mating type.

More than 99 percent of the zygotes resulting from this cross give rise to progeny that segregate normally for the three pairs of chromosomal genes but that like the female parent are all $ac_1$ *sd*. The 1 percent of exceptional zygotes, which we selected out by plating the cells on a medium containing no streptomycin, give quite different results: all the nonchromosomal genes are preserved and transmitted to the progeny, each of which therefore contains one set of chromosomal genes and two sets of nonchromosomal genes. The three chromosomal pairs, segregating normally, serve to identify the four products of meiosis [*see illustration at left*].

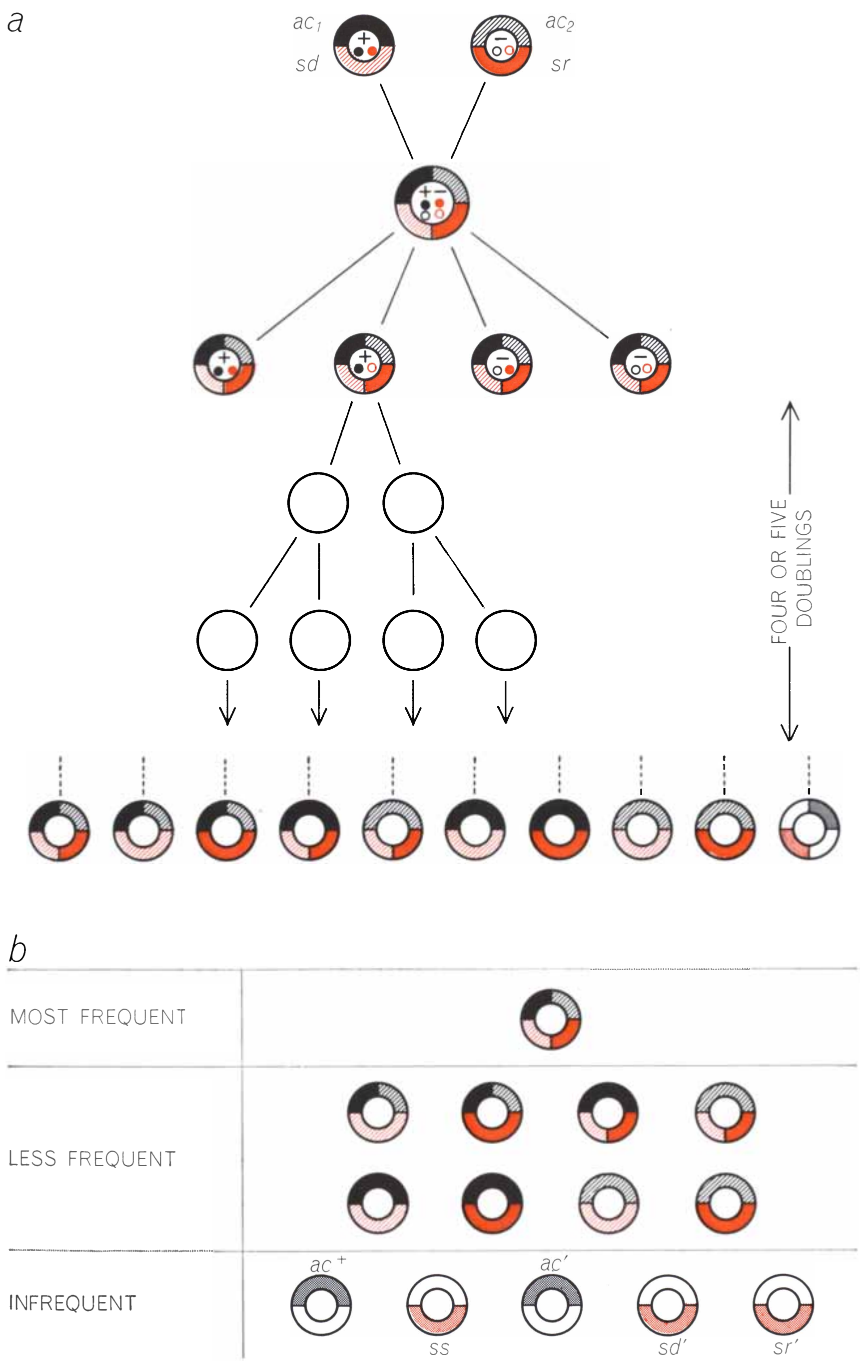
Each of the four kinds of progeny cell begins to divide in the asexual process called mitosis to form clones of cells that remain genetically identical with respect to their set of chromosomal genes. Now, however, the nonchromosomal genes begin to segregate! At each division some daughter cells arise that have only one of each pair of nonchromosomal genes—cells that are pure *sd* or *sr*, pure $ac_1$ or $ac_2$. By classifying samples of progeny after different numbers of cell divisions and determining the frequencies with which each type appears, we have begun to learn how nonchromosomal genes segregate and recombine. These are our preliminary findings:

1. Segregation begins during the first few doublings after meiosis. After four or five cell divisions between 50 and 60 percent of the progeny are still mixed in nonchromosomal-gene constitution, but the rest are pure for one or both of the genes. The occurrence of segregation just after meiosis tells us that at this stage each gene must be present in one copy or a very few copies.

2. The $ac_1/ac_2$ pair segregates in an average ratio of one to one, indicating that equal numbers of copies of the two versions are present in the mixed cells. Individual clones vary greatly, however, in their $ac_1/ac_2$ ratio, suggesting that segregation may not be equational; that is, a mixed cell may give rise to one daughter that is pure $ac_1$ or pure $ac_2$ while the other daughter remains mixed. Whether or not the *sd/sr* pair also segregates one to one is not yet established, and several other aspects of the nonchromosomal segregation process are still under investigation.

3. The $ac_1/ac_2$ and the *sd/sr* pairs segregate independently of each other in both time and space. After several doublings we recover, on the average, equal numbers of the parental combinations $ac_1$ *sd* and $ac_2$ *sr* and of the recombinants $ac_1$ *sr* and $ac_2$ *sd*. In other words, the two pairs of nonchromosomal genes behave as if they are carried on separate particles.

4. Each daughter cell always receives one or another form of each nonchromo-




**EXPERIMENTAL CROSS is diagrammed (*a*) involving cells with three pairs of chromosomal genes and two pairs of nonchromosomal genes (*see text*). The exceptional zygotes transmit to the progeny both sets of nonchromosomal genes. Progeny cells divide and the nonchromosomal genes begin to segregate. The bottom row of the diagram shows, schematically, the kinds of cell that result. Some are still mixed, others pure for one or both of the genes; still others (*far right*) have new mutant or wild-type recombinants. The lower diagram (*b*) indicates the approximate proportions in which various combinations appear. Cells carrying a new recombinant may carry any form (*white segments*) of the other gene.**

somal gene. In this sense $ac_1$ and $ac_2$ are alleles, as are *sd* and *sr*.

5. In addition to the parental nonchromosomal alleles, several alleles not present in the parents arise from this cross. There are clones that are *ss* like the wild-type strain from which *sd* and *sr* arose by mutation, or $ac^+$ like the wild-type strain of which $ac_1$ and $ac_2$ are mutants, and also new mutant *sr′*, *sd′* and *ac′* clones with levels of resistance, dependence or acetate requirement different from those of either parent. Genetic analysis of these strains by backcrossing experiments shows that the new types are genetically stable; they can be considered true intragenic recombinants analogous to the intragenic chromosomal recombinants shown in the bottom illustration on page 74. We think that *ss* arose from an exchange between *sr* and *sd* in which both mutated areas of the gene were replaced by normal sections, and that $ac^+$ arose by a similar exchange between $ac_1$ and $ac_2$.

Data of this kind make it possible to state that in *Chlamydomonas* the nonchromosomal genes we have been studying are particles carrying primary genetic information. Each gene is present in one copy or a few copies per cell, but the number may be different for different genes. The nonchromosomal genetic system, which constitutes a sizable fraction of the organism's heredity, usually shows the characteristic maternal inheritance in crosses. Occasionally, however, nonchromosomal genes from both parents are passed to the progeny, where they segregate in subsequent cell divisions.

The results show, moreover, that the nonchromosomal genes are not distributed at random during cell division but by means of a highly oriented mechanism. This is demonstrated first of all by the fact that each nonchromosomal gene, although present in only one copy or a few copies, is transmitted regularly at each cell division. Second, the mutated and normal forms of nonchromosomal genes behave like alleles when they are present together in the same cells, segregating so that each daughter cell gets at least one of them. Finally, intragenic recombination between nonchromosomal alleles requires some kind of consistent close pairing of two structures in order for a precise exchange of precise portions of a gene to take place.

The occurrence of close pairing also says something about the chemical composition of nonchromosomal genes. At the present time the nucleic acids are the only large molecules known to have the physical and chemical potentialities for the close pairing and intramolecular exchange implied by the recombination process. We therefore consider our genetic data the first evidence for the nucleic acid composition of nonchromosomal genes. There is supporting evidence from biochemical studies.

In most organisms the primary genetic material is deoxyribonucleic acid (DNA). Most of it is in the cell nucleus, but apparently not quite all of it. In the *Chlamydomonas* chloroplast we have found a DNA fraction with distinctive chemical and physical properties. We can distinguish it from nuclear DNA because various DNA's differ in what is called their base composition. All DNA contains the same four bases in two linked pairs: the base adenine paired with thymine and guanine paired with cytosine. The two pairs occur in different proportions, however, in DNA from different organisms or even from different parts of the same cell. The base composition is reflected in the density of a DNA sample, so it is possible to separate two DNA's by spinning them in a high-speed centrifuge. When DNA is centrifuged and its fractions are tested for ultraviolet absorption at 260 millimicrons (the wavelength most readily absorbed by DNA), the optical density of each fraction shows its DNA content. When we centrifuge an extract of DNA from whole *Chlamydomonas* cells, we find two DNA's, one of them a light fraction that accounts for only about 5 percent of the total. When we test an extract of DNA from intact chloroplasts, this light fraction accounts for about 40 percent of the total [*see illustration below*]. It is clearly a special chloroplast DNA.

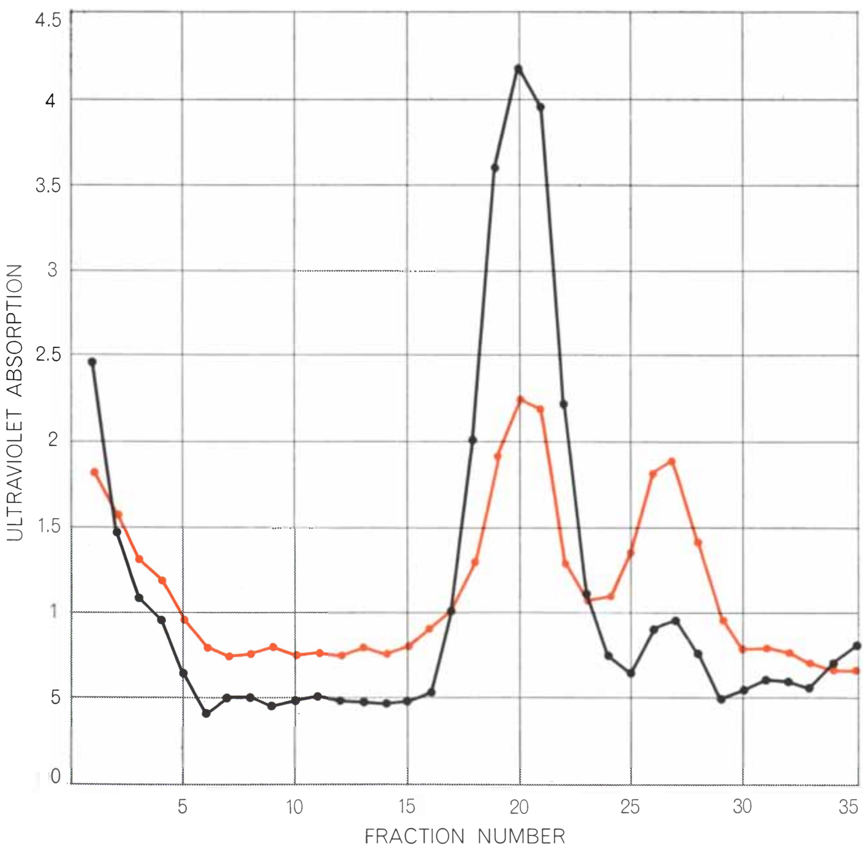
Chloroplast DNA has now been found in a number of other algae and in higher plants. Similar "satellite bands" of nonnuclear DNA are observed in extracts from some animal cells. Recently David J. L. Luck and Edward Reich of the Rockefeller Institute have for the first time been able to demonstrate directly that DNA is localized in mitochondria, which are common to all plant and animal cells.

The existence of both extrachromosomal genes and extranuclear DNA calls for a new and sharply revised picture of cell heredity. Primary genetic information is apparently distributed at many sites within cells in addition to sites on the nuclear chromosomes. How many sites there are and how many genetic systems and what their chemical identity is remain open questions. Because a substantial fraction of the nonchromosomal genes studied so far influence the functioning of chloroplasts and mitochondria, it seems a fair possibility that organelle DNA may be the carrier of some nonchromosomal genes. No experimental evidence has yet demonstrated, however, that organelle DNA has a genetic role. Nonchromosomal genes carried elsewhere in the cell could influence the activity of an organelle, so the mere fact that a nonchromosomal gene affects the development of a chloroplast, for example, proves nothing about the location of that gene. Moreover, a number of chromosomal genes are known to influence the function of chloroplasts and mitochondria.

Some investigators have reasoned from the available data that the chloroplasts and mitochondria must be self-replicating bodies, but I do not think the significant question is whether or not organelles are autonomous. The smallest self-replicating unit is the cell; subcellular organs are only components of a highly integrated system. The problem is rather: How do the chromosomal and nonchromosomal genetic systems interact and what is the particular role of each gene in each system?

It seems likely that nonchromosomal genes affect other subcellular systems as well as the chloroplasts and mitochondria. The *sd* and *sr* genes in *Chlamydomonas*, for example, appear not to affect either of these organelles. We have been trying to find out if it is the ribosomes that are affected by the mutations. Our evidence indicates that ribosomes of the *sd* and *sr* strains do not differ from those of the wild-type *ss* strain in their protein-synthesizing ability in the presence of streptomycin, so apparently these genes are not acting at the level of ribosome formation. It seems likely, therefore, that there are sites of nonchromosomal gene action other than the chloroplast, the mitochondrion and the ribosome. I should also point out that none of our results precludes the possibility that cells contain still other genetic systems not based on nucleic acids and that hereditary mechanisms may involve principles as yet unknown.

The further analysis of nonchromosomal heredity will require the application of methods similar to those developed for the chromosomal system. The




**CHLOROPLAST DNA, or deoxyribonucleic acid, is demonstrated by measuring the optical density in the ultraviolet of fractions of two DNA's: an extract from whole cells (*black curve*) and one from chloroplasts (*color*). A light fraction, about 5 percent of the whole-cell DNA, accounts for much of the chloroplast DNA: it must be localized in that organelle.**

inherent power and high resolution of genetic analysis ensure its continuing usefulness; provided that genes can be made to mutate, they can be identified even if they exist at concentrations far below the resolving power of physical or chemical methods. At the same time the identification of extranuclear DNA has opened the way to a correlated line of investigation, and future studies of nonchromosomal genes will require the most intimate interlocking of evidence from these complementary disciplines.

What is the significance, finally, of the existence of more than one genetic system in cells, systems operating with some local autonomy? Are the cytoplasmic systems vestigial remnants of primitive forms of life or even, as has also been suggested, the remains of what was initially some kind of parasitic invasion of cells? I think the nonchromosomal systems must, on the contrary, be playing an essential role in the life of modern cells, or they would long since have disappeared. I think, moreover, that they played a role from the very beginning. Life began, I would speculate, with the emergence of a stabilized tripartite system: nucleic acids for replication, a photosynthetic or chemosynthetic system for energy conversion, and protein enzymes to catalyze the two processes. Such a tripartite system could have been the ancestor of chloroplasts and mitochondria and perhaps of the cell itself. In the course of evolution these primitive systems might have coalesced into the larger framework of the cell, in which most of the primary genetic information was shifted to specialized structures: the chromosomes.

Why was all the genetic information not shifted to the chromosomes? I think the answer may lie in the cell's requirements for flexibility in growth. Chromosome replication is closely geared to the cellular division cycle, but organelles seem not to be governed entirely by that cycle; to some extent chloroplasts and mitochondria grow independently of cell division in direct response to environmental stresses. The same may be true of other cytoplasmic systems. Under these circumstances it may be desirable that some of the genetic information involved in organelle development be free to replicate at times different from those at which the chromosomal DNA does. The nonchromosomal genetic systems, then, may be of continuing importance in providing flexibility for organelle growth in response to a changing environment.

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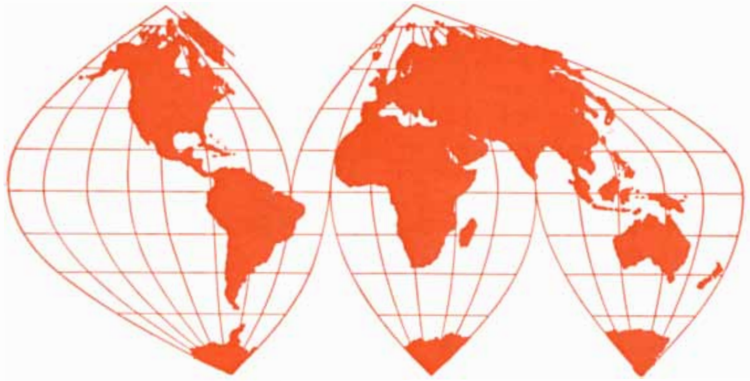

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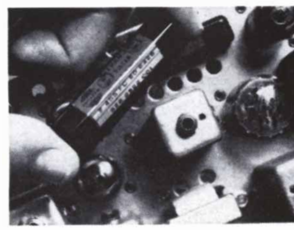

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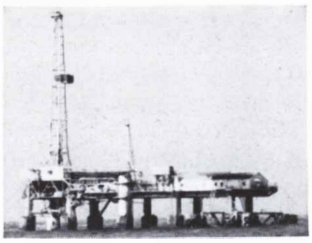

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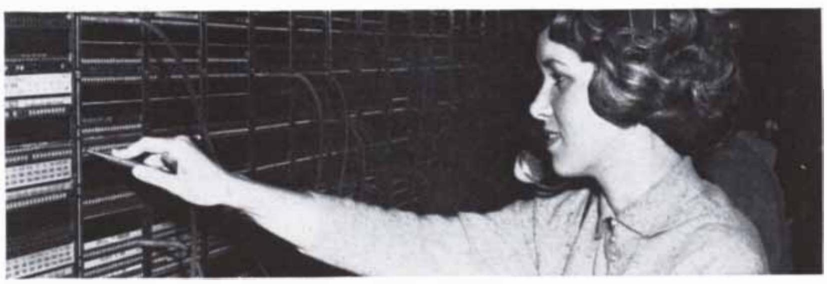

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# ALESSANDRO VOLTA

## His "pile," or battery, opened the age of electric power and settled his celebrated argument with Luigi Galvani. Curiously he then played no part in the epochal developments that his invention made possible

by Giorgio de Santillana

It is difficult to appreciate now, when electricity is so commonplace, what a marvel and mystery it was only 200 years ago. By the same token it is not easy to evoke a clear picture of the situation that confronted the early investigators of electrical phenomena. Electricity manifested itself in ways that were sometimes dramatic but that were also somewhat peculiar, for example in lightning or in amber rubbed with wool. Ascertaining the laws that governed its behavior was a process of groping by trial and error aided occasionally by a flash of intuition.

One can recapture some of the mystification of those times and some of the excitement of dispelling it by retracing the intellectual steps of an early investigator of electricity. Such a man was the Italian physicist Alessandro Volta, whose involvement in a major controversy with his countryman Luigi Galvani led him to construct his famous "pile"—the first electric battery and the first source of continuous electricity. With this device he opened the age of electricity, although he himself took little part in the flood of developments that followed quickly on his invention.

Volta was born on February 18, 1745, in the ancient Lombard town of Como. He had four brothers, three of whom became priests, and four sisters, two of whom became nuns. His father, Filippo, had himself been in the Jesuit order for 11 years before deciding at the age of 41 to marry a girl of 19. Filippo Volta was of noble birth, but he was a poor manager of the family funds. Years later Alessandro wrote: "My father owned nothing except a small dwelling worth about 14,000 lire; and he left behind him 17,000 lire of debt."

As a child Alessandro was a source of concern to his family. He seemed dull-witted and was so slow in learning to talk (he said nothing until he was four) that his parents decided he was a mute. At four his mind ignited; by seven he was regarded as being exceptionally bright in school. After Alessandro became famous his father said: "We had a jewel in the house but did not know it."

Because of his family's straitened circumstances Volta received his education through the endeavors of relatives in the church. From them and from his immediate family he was under pressure to enter the priesthood. He was intensely curious about natural phenomena, however, and by the time he was 14 he knew that he wanted to be a physicist. He also expressed the opinion that there were so many priests, nuns, monsignors and archdeacons in the family that it was unnecessary for him to aid in sanctifying it.

Volta's early interest in nature did not detract from literary inclinations that he maintained throughout his life. His excellent classical education had made him well versed in Latin poetry. He once composed a long Latin poem on nature and science; it dwelt particularly on the researches of Joseph Priestley and also praised Benjamin Franklin and James Watt. The poem has considerable literary merit; the ear attuned to Latin verse will detect in Volta's lines an echo of Lucretius.

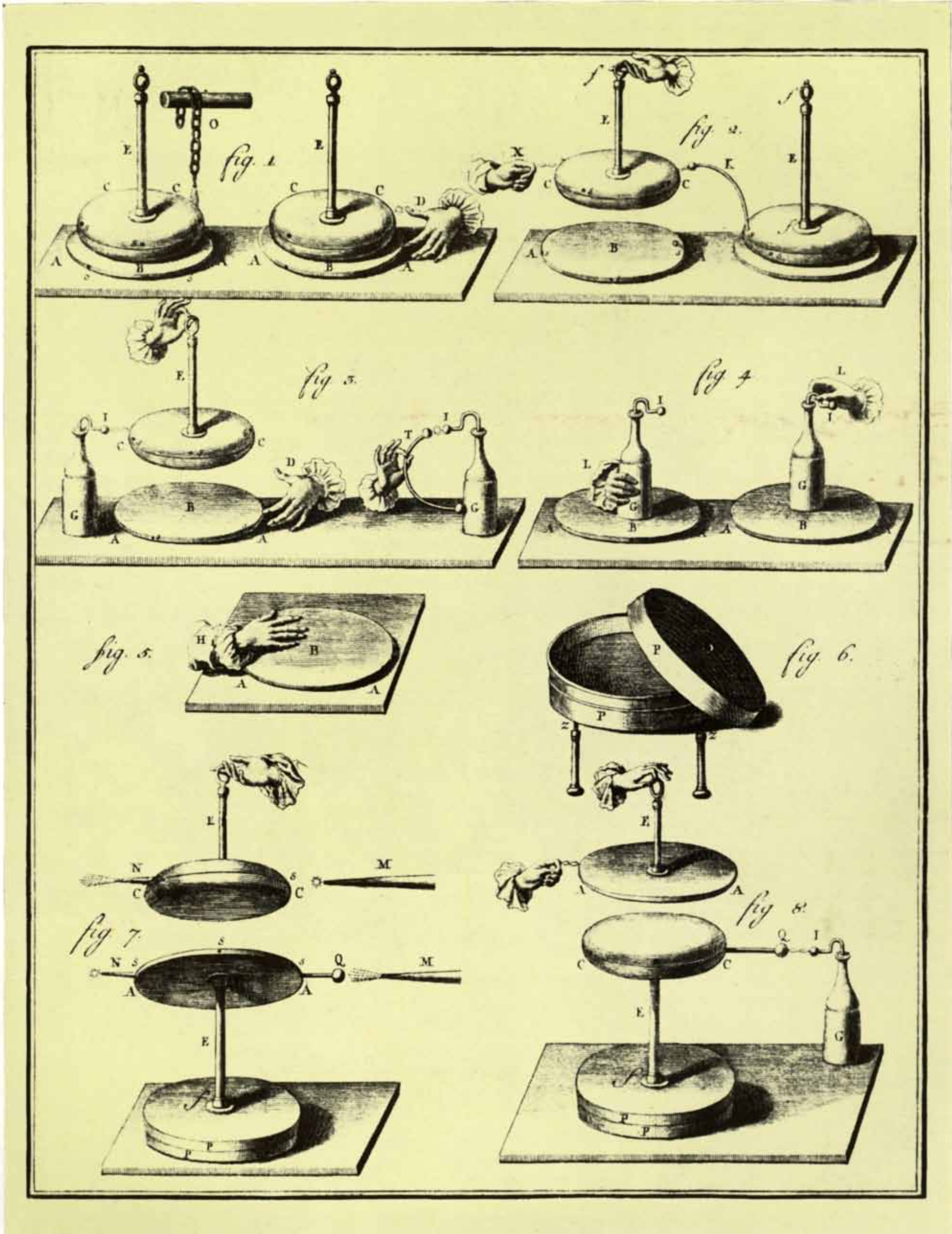
When he was 16, Volta was much under the influence of an older friend, the Canon Gattoni, who provided him with his first experimental equipment. Volta did not, however, share Gattoni's orthodox views. He went so far as to write his friend a long dialogue designed to prove the heresy that not only men but also animals have souls.

The same adventurous turn of mind is apparent in Volta's first published scientific paper, *De vi attractiva ignis electrici* (*Concerning the attractive force of electric fire*). Written when Volta was 24, the paper was addressed to Giovanni Battista Beccaria, professor of physics at the University of Turin and a prominent experimenter in electrostatics. In it Volta undertook to show that all electrical phenomena can be attributed to one fluid or force that is akin to, if not identical with, universal gravitation.

That was the last of Volta's flights of speculation. His basic drive was for experimentation, and it soon led him to master the technique of basing conclusions on experimental findings. Even by the time his first paper was published he had been experimenting with electricity for several years. He was 18 when he began a correspondence with a respected French physicist and investigator of electrical phenomena, the Abbé Antoine Nollet.

Electrical science was of course in its infancy when Volta took it up. Several investigators had discovered during the preceding century that a number of substances behaved like amber when they were rubbed. In 1660 Otto von Guericke had built the first machine for generating an electric charge; it was a sulfur ball turned by a crank and charged by friction. In 1733 this invention, together with the discovery four years earlier that electricity would flow from one place to another through a suitable conductor, led Nollet and Charles François de Cisternay Du Fay to perform an engaging experiment that involved their own bodies. Du Fay was suspended horizontally by silk cords in a dark room and charged up with an electric machine of the von Guericke type; when Nollet touched him, he emitted large sparks. The experiment caught the fancy of the French court, where

**LAST PORTRAIT OF VOLTA** shows him in old age with representations of two of his inventions: the pile and the electrophorus. The date of the portrait and the name of the artist are unknown, although the time was about 1820. Volta died in 1827, aged 82.




**VOLTA'S ELECTROPHORUS** was his first important invention. He announced it in a letter of 1775 to Joseph Priestley, accompanying the letter with illustrations similar to these, which appeared in a publication of the time. The apparatus consisted of a lower and an upper metal plate (*A and C*) separated by a disk (*B*) of nonconducting material such as resin. The resin was given a negative charge by friction, and a positive charge was induced in the upper plate when it was placed on the resin and then grounded and lifted off. Thereafter the upper plate could be charged repeatedly by placing it on the resin and lifting it. The electrophorus was thus a replenishable source of static electricity, which earlier investigators had obtained only by direct friction.

conducting shocks from one person to another became something of a fad.

Du Fay found in another experiment that objects charged from a glass tube repelled each other but attracted objects charged from a rod of resin. The finding led him to two fundamental conclusions: that there are two kinds of electricity (in this he was not quite correct) and that like charges repel but unlike charges attract. Not long afterward the invention of the Leyden jar showed that electricity could be stored. Many investigators, accidentally or deliberately discharging Leyden jars by touching them, received numbing shocks. Shocks were routine occupational hazards for the early investigators of electricity; one wonders how they had the courage to persevere.

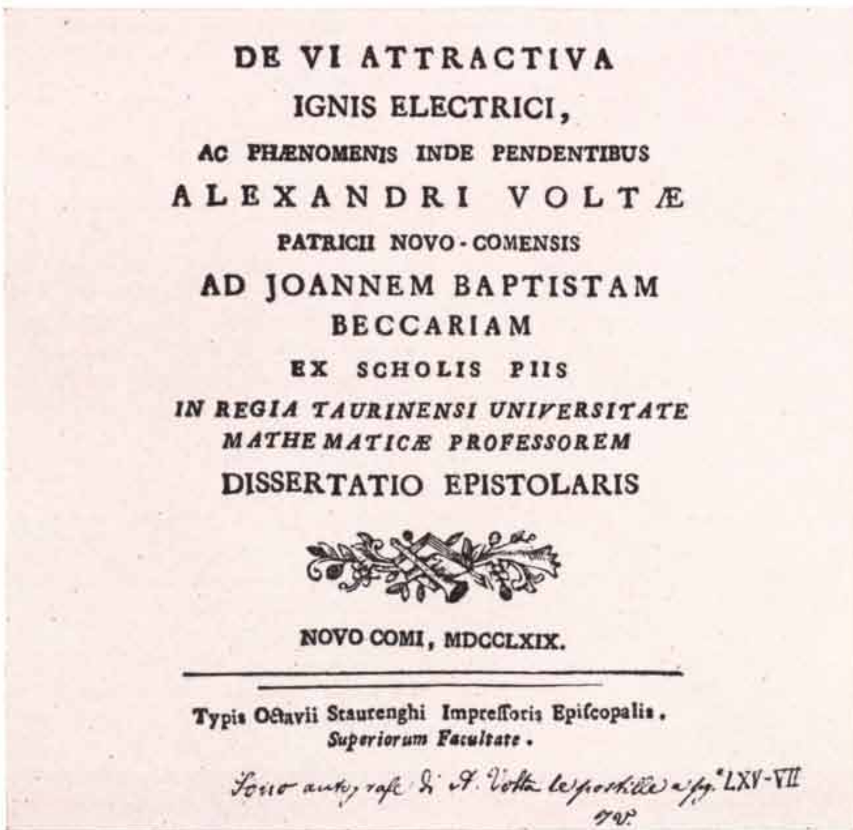
By this time Franklin had begun his electrical investigations, and he soon became the most important figure among the early theoreticians of electricity. From his experiments with the Leyden jar and other apparatus he concluded that the two kinds of electricity were two aspects of a single force: one was an excess of the force and the other a deficiency. It was he who bestowed the name "positive" on the former and "negative" on the latter; he could not know, of course, that the condition he called positive actually represented a deficiency of electrons and the condition he called negative represented an excess of them. Franklin's famous kite experiment, which led to the development of lightning rods, was conducted in 1752. From then on electrical theory had an almost magical import. Man had controlled lightning.

It was against this background that the youthful Volta began his investigations in 1763. For the next decade, however, he devoted himself mainly to his studies. In 1774 he became superintendent of the Royal School in Como. Soon he was called to the chair of physics at the University of Pavia, thanks to the protection of the Austrian emperor, who was then ruler of northern Italy. By that time he had taken on the personal characteristics often used to describe him. He was a handsome, strapping six-footer; affable and outgoing; fond of the outdoors; not disdainful of ordinary pastimes with ordinary people but very much a notable of his own city, personally acquainted with the foremost men of Europe; carefully conservative in his views and soberly objective in his interests.

In 1774 Volta made his first important contribution to electrical science with his invention of the "electrophorus," which provided experimenters for the first time with a steady, replenishable supply of static electricity. The device worked on the novel principle of electrostatic induction instead of on the direct application of friction required to produce a charge in earlier apparatus. It consisted of a cake of resin or some other nonconducting substance with one plate of metal below it and another plate above it; the upper plate had an insulated handle and could be removed [*see illustration on opposite page*]. One removed the upper plate, charged the resin by friction and put the plate back. If one then grounded the upper plate by touching it, the negative charge of the resin would repel the electrons in the plate; they would travel to the ground and the plate would be left with an induced positive charge. Thereafter, by lifting the plate one created an electric potential. If one then touched the plate, it gave off a spark. When the plate was discharged, it could be recharged in the same manner; the process could be repeated indefinitely. Describing the electrophorus in a letter of 1775 to Priestley, Volta wrote: "I present to you a body which, electrified only once for a short time and not strongly, will never more lose its electricity, and will obstinately maintain the live force of its indications, even if it is touched again and again."

It is indicative of Volta's wide-ranging curiosity that his next invention grew out of a fishing trip on Lake Maggiore. He observed the bubbles that rose to the surface of the water in marshy areas, gathered some of this marsh gas (now known to be methane) and found it to be inflammable. He began a series of experiments with the gas and developed his "eudiometer," which was a tube closed at one end. Gases were bubbled into the tube from the water and exploded with an electric spark. The device was to prove valuable in determining the volume of gases that combined. As Volta said later, if he had been able to collect the gases over mercury he would have described the composition of water. He simply did not have enough mercury.

Another of Volta's inventions, announced in 1782, was the condensing electroscope, which was a much more sensitive indicator of the presence of electricity than earlier devices. This invention grew out of work he had done to improve electrometers, the best of which had used the separation of gold leaves to detect electricity. The condensing electroscope resembled the electrophorus except that instead of a


DE VI ATTRACTIVA
IGNIS ELECTRICI,
AC PHÆNOMENIS INDE PENDENTIBUS
ALEXANDRI VOLTÆ
PATRICII NOVO-COMENSIS
AD JOANNEM BAPTISTAM
BECCARIAM
EX SCHOLIS PIIS
*IN REGIA TAURINENSI UNIVERSITATE*
*MATHEMATICÆ PROFESSOREM*
DISSERTATIO EPISTOLARIS

NOVO COMI, MDCCLXIX.

Typis Oćtavii Staurenghi Impreſſoris Epiſcopalis.
*Superiorum Facultate.*

*Sono autografe di A. Volta le postille a pag.e LXV-VII*

FIRST PUBLISHED WORK by Volta was this paper of 1769. The note at bottom, by Volta's son Zanino, verifies that corrections on two pages of the paper were made by Volta.

LUIGI GALVANI is portrayed as he appeared in middle age. He and Volta were long in dispute over what Galvani called "animal electricity" and Volta called "metallic electricity."

slab of resin it used a thinner slab of weakly conductive material; as a result the upper plate could pick up a very small electric charge. This device was a kind of microscope of electricity, and it was what first brought Volta's name to the attention of other men of science. Later Volta used the condensing electroscope to perfect an early form of the kind of electrometer that determined an electric potential difference by balancing the electric force against the force of gravity.

By this time Volta was making frequent and extended trips abroad, a custom that brought him in contact with many prominent investigators and other figures. On one trip in 1771 he paid his respects to Voltaire, little imagining that not too long afterward Napoleon would stop in front of a wreath inscribed "To the Great Voltaire" and strike out the last letters to make it "To the Great Volta." While on a trip in 1789 Volta wrote home: "I have at last met the great Franklin." The acquaintances Volta made on these tours undoubtedly helped to fortify his reputation against the strains of the long controversy with Galvani.

Galvani, who was a physician and professor of anatomy at the University of Bologna, had begun experimenting with electricity in 1780. His interests included the effects of electric discharges on the nervous system of the frog. He discovered quite by accident in 1786 that the amputated hind legs of a frog would kick convulsively if they were made part of an electrical circuit. (On the occasion of the discovery the circuit was closed by one of Galvani's assistants, who happened to touch the crural nerve of the frog with a scalpel at a moment when an electric machine nearby gave off a spark.)

In many experiments with this phenomenon, extending over several years, Galvani found that it occurred when there was a nearby flash of lightning, and that the frogs' legs would twitch even in good weather if they were hung on an iron fence on a hook made of brass or copper. Pondering the phenomenon, Galvani recognized that it could be explained in one of two ways. Either electricity was a special animal property, in which case the outside influences merely caused the property to manifest itself, or the contact of two metals brought on the kicking, in which case the legs merely acted as an indicator of an outside charge. Galvani decided that it was the former: the animal tissue contained a vital force, which he called "animal electricity."

He published his results in 1791, and they created a sensation. Apparently he had found a key to the mystery of life. Among those impressed by the results was Galvani's friend Volta, to whom Galvani had sent one of the dozen copies of the paper, which he had had printed as a pamphlet.

At first Volta thought, with Galvani, that there was some kind of electrical disproportion between nerve and muscle, and that when metal connected the nerve and muscle of a frog's leg, the balance was restored. As Volta experimented with the phenomenon, however, he began to have doubts. He observed that muscles would contract even when they were not part of an electrical circuit; they did so when electric contacts were merely applied to the nerve. Gradually he came to the view that more attention should be paid to the role of the metals.

The story has been told many times as a beautiful example of nature taking good sense by the hand and guiding it to discovery. This concept, however, does less than justice to the fearfully intricate situation with which the experimenters were confronted. True, there were lucky breaks, such as the discovery that the contact of copper hooks and iron railings gave the best contraction of the frogs' legs. That looks decisive now but it did not then. The situation remained a jungle of possibilities.

Such was the setting for one of the great decisions of history, a decision that involved nothing less than the turning point of the scientific revolution. Was the new force of the electric current a special aspect of life or did it arrive to found the technological era? To whom was to belong the only form of immortality permitted by fast-rolling oblivion, namely posterity's choice of his name to designate the fundamental electric unit?

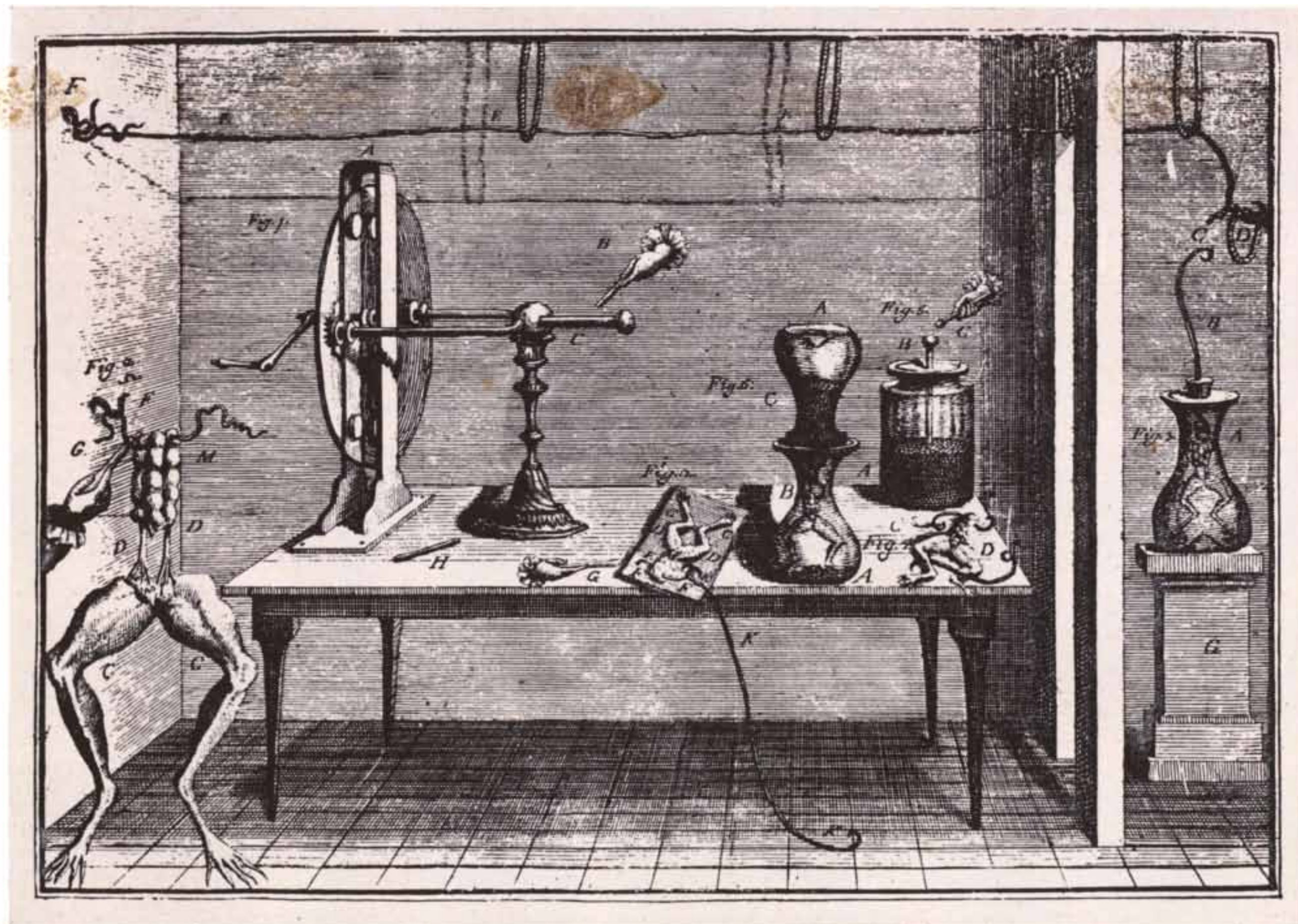
It was only historic justice that the decision fell to Volta. Whereas Galvani stood for the old kind of biological sciences, Volta was the new kind of physicist: quantitatively minded, even if he had no mathematics, and a master of the new experimental approach. He also had the modern ruggedness. Whereas the sensitive, tormented Galvani was a born victim of circumstances, Volta had the gift of impassive adjustment. On the advent of French power in Italy in 1796 Galvani refused the oath of allegiance on conscientious grounds and retired to a silent poverty, from which he was rescued too late by the generosity of the conqueror. Volta, although he resented the invasion, moved into the French orbit as soon as it was prudent to do so. That remained his pattern. Throughout his life storm upon storm of success was to break on that brow and leave it unlined. He went through life unmoved.

In his years of trial after 1792, however, he stood alone and found few to understand him. His opponent had taken 10 years to prepare his case, and the case was strong. Nature had even loaded the dice in Galvani's favor, inasmuch as there was in fact more than one electricity involved. There was an artificial electricity that could provide spectacular discharges, comparable to those of the electric eel, whereas it was still difficult to elicit even the smallest shocks and sparks from metallic electricity. There was also the electricity of the tissues that is now recorded by electrocardiographs. It was, to be sure, a feeble kind of electricity, but on the other hand nature had provided the most sensitive electroscopes in the form of frogs' legs; natural, or metallic, electricity did not get the right responses in the usual electrostatic instruments. Finally, the picture was blurred by actions at a distance and induction not yet rightly understood. As soon as Volta thought he had found a secure starting point in the presence of a bimetallic arc, Galvani was able to cut the ground from under him by showing that muscular contractions could be elicited without any metal in the circuit.

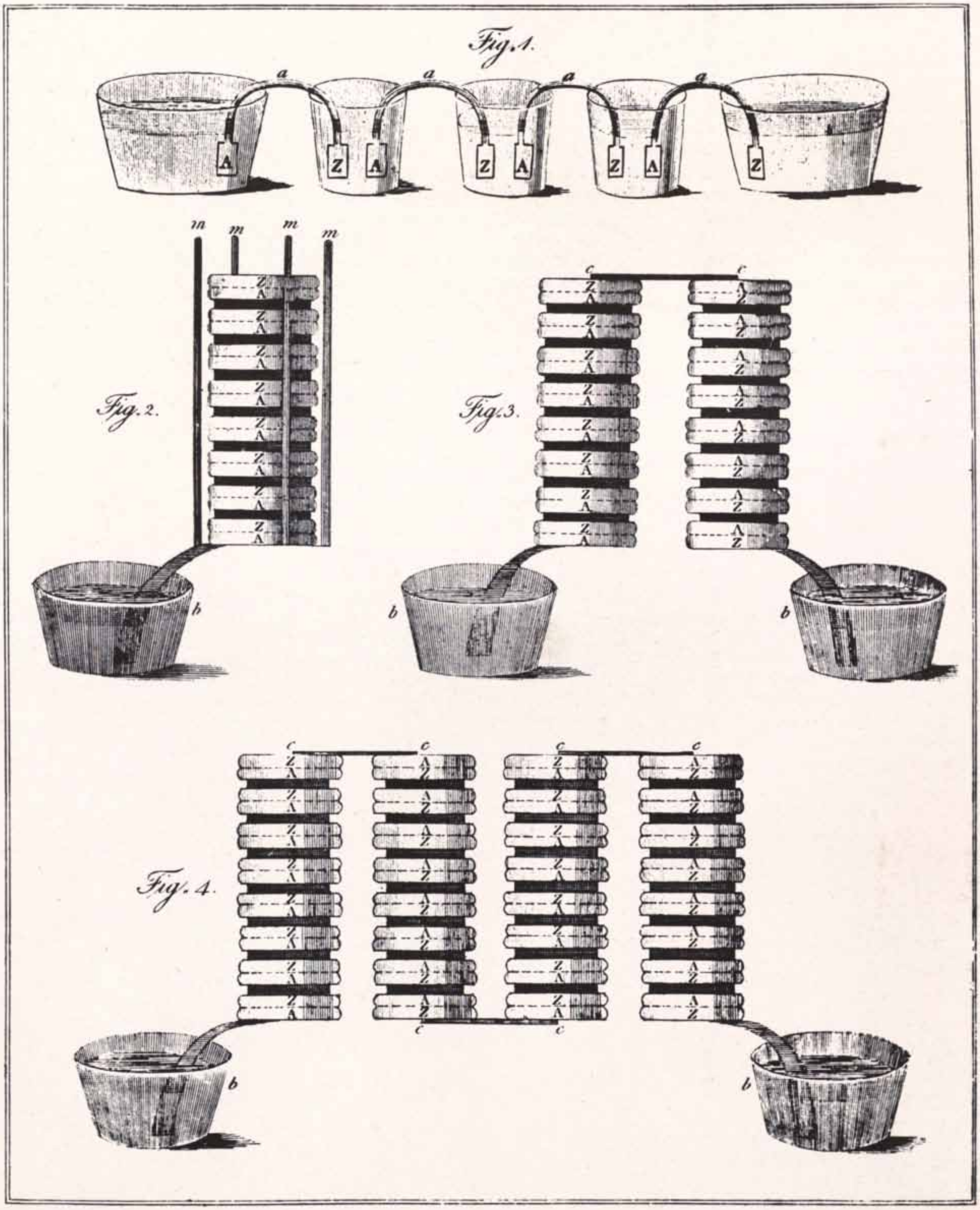
It took great firmness and clarity of mind to withstand the onslaught. "I know those gentlemen want me dead," Volta wrote to a friend in those days, "but I'll be damned if I'll oblige them." He had to work out his own electroscopes to detect metallic electricity, namely the sensitive tissues of his tongue and his eye; he had to show that it was the nerves and not the muscles that were stimulated. He concluded his second memoir firmly: "It is the difference of metals that does it."

Pursuing this approach made it necessary for Volta to rebuild the whole structure of electrical concepts. It was here, in particular, that he had to define tension and distinguish it from quantity. "The quantity of electricity," he wrote, "which is put in motion by the contact of metals is not small, it is in fact considerable, but the current has so little tension that it gives no signs on the electrometer, and can be easily stopped by poor conductors. It is an ample flow, but a gentle one." This statement was descriptively perfect.

To Galvani's master argument that removed metals altogether, Volta re-



**EXPERIMENTS BY GALVANI** with frogs' legs are depicted in this engraving, which was one of four accompanying his publication of the results in 1791. He had found that dissected frogs' legs would twitch when touched by metal in the presence of electricity. Among his sources of electricity were an electrostatic machine, shown at left on the table, and a Leyden jar, at right on the table.




**FIRST ELECTRIC BATTERY,** which Volta called "pile," is shown in the illustration that accompanied his announcement of the invention. The announcement was in the form of a letter to Sir Joseph Banks, president of the Royal Society of London; the letter was published in the *Philosophical Transactions of the Royal Society* in 1800. At top the battery appears in a form that Volta called a "chain of cups." It consisted of dissimilar metals, such as silver (*designated "A" for "argentum"*) and zinc (*Z*), immersed in salt water; "they are made all to communicate by forming them into a sort of chain, by means of so many metallic arcs." The lower figures show aspects of the "columnar" form of the battery, in which silver-zinc couples were separated by disks of moistened pasteboard.

plied by extending the concept of conductor and motor contact to bodies of the second class, as he called them, namely liquids. This concept led him to the problem of adding electromotive forces in chains of conductors. By moving now from electrokinetics to electrostatics, by ingeniously juggling his inadequate instruments and his puny charges, he managed at last to build up his pure contact forces to a tension detectable in the electrometer. Again it was a measuring procedure that led him to his goal. After having searched for two years for a means of multiplying the contact tension of metals by adding up contacts, he hit on his idea of the battery, which he communicated to Sir Joseph Banks, president of the Royal Society, in a letter dated March 20, 1800. The battery was originally a pile of disks: copper-zinc couples, each separated from the next by a disk of cardboard moistened with acidulated water. The 10-year battle had been won: electric power had come into the world.

Volta and Galvani both had their greatness and their limitations. Just as the unfortunate Galvani had invented galvanic electricity and believed its nature to be animal—a misconception that led his followers into the sands of vitalistic quackery and almost killed his discovery—so Volta in saving that discovery had actually discovered electrochemistry without knowing it. He stopped in sight of the promised land. He left it to Anthony Carlisle and William Nicholson of England to achieve within a few weeks the electrolysis of water. They had only to read the letter to Banks and build a battery. Volta never cared to venture into the immense new field. It is strange to think that he was still in his robust fifties when the tide of electrochemical discoveries broke on the world, when Humphry Davy electrolyzed the alkalies and produced the voltaic arc.

It is noteworthy that in a famous demonstration of the pile in Paris in 1801, with Napoleon present, Volta gave an absurd amount of attention to the ways of making his battery assume the appearance of an electric eel by dressing it in a skin. This was not silly gadgeteering, nor was it the cultural lag that made the creators of the railway try to imitate the coach-and-four. It was the final settling of the old quarrel with Galvani about animal electricity. Galvani's articulate nephew Giovanni Aldini had still been defending the cause of galvanism after Galvani's death, so Volta had reason for his attitude. He had knocked down the idea, but he still had to drag it out.

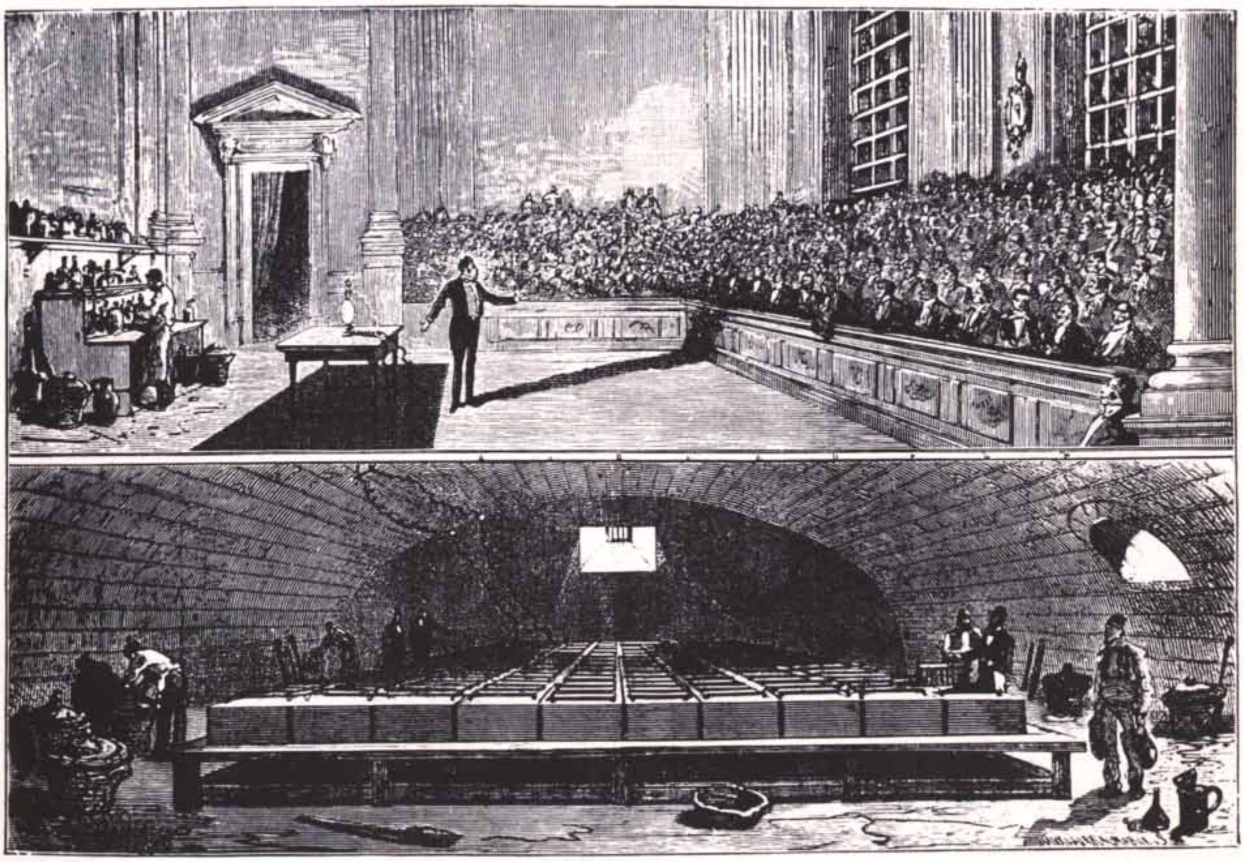
It would be easy to conclude that this last act of the episode is an example of a mind remaining polarized on its moment of creation. Yet nothing in history is as simple as that. One should see that Volta had to go on playing his part in the drama. He had conquered a strongpoint; it then became his business to hold it in the troubled times ahead. That was why his thought continued to have a powerful influence on the next generation. He stood for good method against adventure.

Davy, as early as November, 1800, had concluded that the power of action of the battery is "in great measure proportional to the power of the conducting fluid substance between the double plates to oxydate the zinc." Volta, however, had already taken the position that the mere contact of two metals was enough. In fact, he supplied what seemed to be a convincing proof. Taking a disk of copper and one of zinc, he held each by an insulating handle and applied them to each other for an instant. After the disks had been separated, the electroscope showed that they were electrified with opposite signs. Volta fully understood the phenomenon and explained the impossibility of constructing a pile from disks of metal without making use of moist separators. He showed in 1801 that, in a pile of disks of different metals in direct contact, the disks at the top and the bottom of the pile will be in the same state as disks in direct contact. For him the moist layers only played the part of conductors.

How, then, did he conceive the operation of the pile? Consider first a disk of zinc laid on a disk of copper resting on an insulated support. One may assume, from the single-fluid theory that Volta had learned from Franklin, that electricity will be driven from the copper to the zinc. One may then represent the tension of the copper as $-1/2$ and that of the zinc as $+1/2$, the difference being arbitrarily taken as 1, and the sum, because of the insulation, as 0. In this line of reasoning Volta was using "tension" for an amalgam of two ideas that today are distinct, namely charge and potential.

Now, if one places another disk of copper on top of the zinc, with moist pasteboard between, there will be contact only through the moist conductor, and the copper will receive a charge from the zinc. The states will be represented by $-2/3$ for the bottom copper disk, $+1/3$ for the zinc and $+1/3$ for the top copper disk, giving a sum of 0 as before. If one places another disk of zinc atop the pile, the states will be represented by $-1$ for the bottom copper disk, 0 for the intermediate pair of disks and $+1$ for the top zinc disk. This is how the tension will increase as the pile is built up.

It is clear that the image behind the theory is that of the Leyden jar, an early form of the condenser. The experimenter will receive the shock of a discharge when he touches the top and bottom disks. The great difficulty that remained (and Volta was aware of it) was that the pile does not discharge itself as a condenser does; it maintains its state. "This endless circulation or per-



**ELECTRIC ARC LIGHT, which became known as the voltaic arc, is shown as demonstrated by Humphry Davy at the Royal Institution in London. His source of power was a 2,000-cell voltaic battery built in the basement with money from public subscription. He used charcoal electrodes to produce the arc; a contemporary account said the light was "so intense as to resemble that of the sun."**

petual motion of the electric fluid," Volta wrote, "may seem paradoxical, and may prove inexplicable, but it is nonetheless real." His neat mind recoiled from the obscurities of chemical action. This was true even when he came to deal with it in explaining that bane of early experimenters: the polarization of batteries. Yet he went on avoiding the thought of any link between chemical change and electric force.

Volta's rigor may appear obdurate and sterile to the modern mind armed with hindsight. No doubt it did bar him from the exciting realm of new discoveries that blossomed during his lifetime. One should consider, however, how bewildering the situation in electricity was then, and what misleading speculations it could prompt in lieu of sound theories. Volta stood for clarity and prudence. The same virtues that had guided him surely through the jungle of galvanism now acted as a brake.

Typically enough, at 50, when he had completed his work, Volta made up his mind to marry. Even more typically, he went to two of his clerical friends for advice. For 25 years he had had a liaison with a well-known singer known as Mademoiselle Paris; should he regularize the situation? One of the clerics thought he should. Volta, however, had habitually followed the advice of a brother who was an archdeacon. Taking the brother's advice, he married a woman of good family; she has been described as ill-favored but implacable in the administration of the family estate. Volta now settled down to a new life as a father, a senator and eventually a count of the Napoleonic empire.

One should probably not call him to task for the accommodations of his old age. With the fall of Napoleon, Volta did what everyone else in his region did: he quietly returned to being a good Austrian subject. The Austrians were old protectors, after all, and they were ready to forgive. Volta was not of the stuff that martyrs are made of, and the new time of nations in their birth pangs—with its heroic stands, its exiles, its romantic dreams and its braving of police persecution—was not for him. He was a man of the old order and wanted to live out his time on his country estate in Camnago. It was, perhaps, a not unattractive end. There is poetic symmetry in the fact that the protagonist in the most far-reaching of all revolutions—compared to which the whole Napoleonic adventure was mere sound and fury—should have been a quiet conservative.

# THE EVOLUTION OF INTELLIGENCE

## It has been assumed that the intelligence of animals on various rungs of the evolutionary ladder differs only in degree. New experiments on animals from fish to monkey show that the differences are qualitative

by M. E. Bitterman

Suppose an animal is given a choice between two alternative courses of action, one of which is rewarded consistently and the other never. If the alternatives are readily discriminable, the animal will, after a number of trials, develop the habit of choosing the rewarded one. By plotting trials against errors, the experimental psychologist constructs a curve called a learning function that summarizes the course of the animal's mastery of the problem.

It has been known for some time that learning functions based on such simple problems do not differ significantly among diverse animals; the curves for a monkey and a fish, for example, have a similar shape. This fact, implying some intellectual continuity throughout the evolutionary hierarchy of animals, tended to corroborate a theory of animal intelligence that prevailed during the first half of the 20th century.

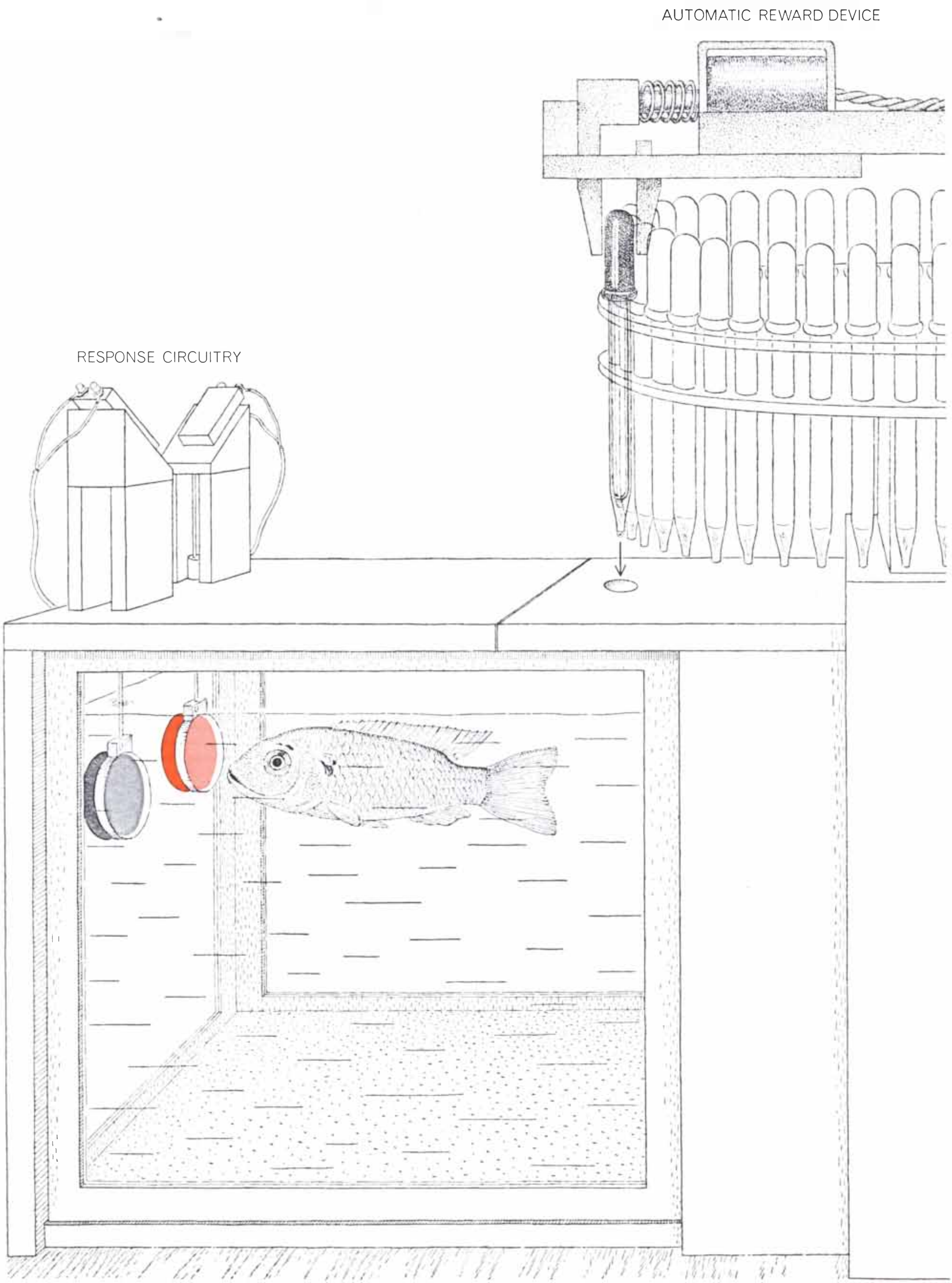
According to this theory, an animal is born with tendencies to react in certain ways to certain stimuli—tendencies based on inherited neural connections between sensory and motor systems. The animal's ability to learn is simply its ability to modify these connections (to break some and to form others) as needs and circumstances dictate. Differences in intelligence from species to species are differences only of degree. The higher animals can form more connections than the lower animals because of better sensory and motor development and because their nervous systems afford more elements for this purpose. Hence the evolution of intelligence merely entails refining old processes and replicating old neural equipment.

Since learning was thought to involve qualitatively similar processes throughout the evolutionary hierarchy it seemed that there was nothing to be gained from studying many different species, and that there was much to be lost in terms of experimental efficiency. Attention became concentrated on a few mammals—primarily the rat—selected for reasons of laboratory custom or convenience and treated as being representative of animals in general. The number of animals under study narrowed, and so did the likelihood of discovering any differences that might in fact exist.

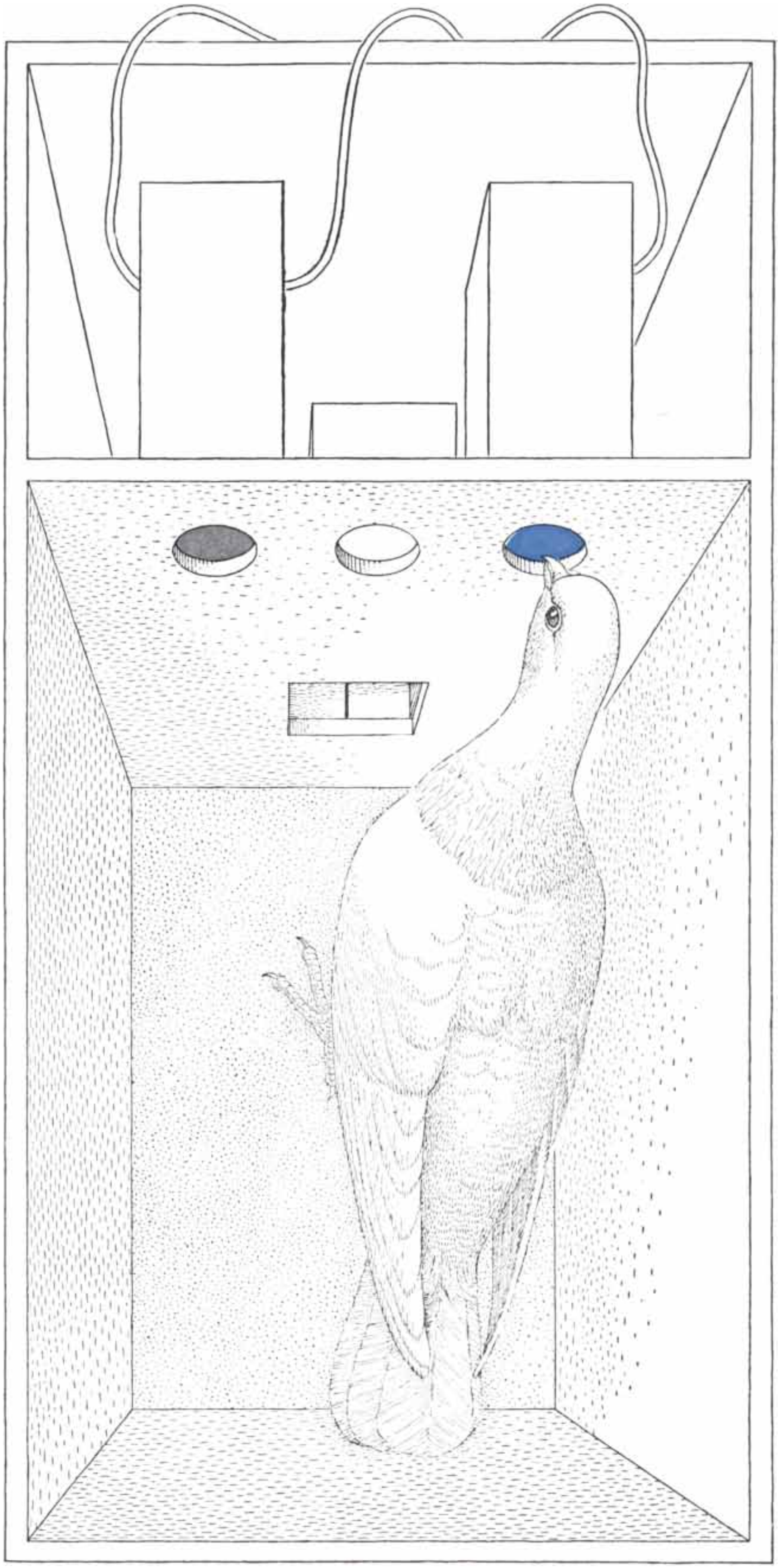
The investigations I have been conducting for several years with my associates at Bryn Mawr College were inspired by the conviction that the traditional theory called for more critical scrutiny than it had received. We began with the knowledge that the simplest problems would not serve to reveal distinct modes of intelligence and different neural mechanisms at work in various animals. Hoping that experiments based on more complex problems would point to such differences, we complicated matters for our test animals by introducing certain inconsistencies in reward. Thus we developed several kinds of experiment on which our diverse subjects (monkey, rat, pigeon, turtle and fish) gave diverse performances. The two I shall describe in this article are habit-reversal and probability-learning experiments.

In habit-reversal experiments animals are rewarded for choosing alternative *A* rather than *B* until a preference for *A* has been established, then *B* rather than *A* is rewarded. When a preference for *B* has been established, *A* is again rewarded, and so forth. Trained in this way, a rat or monkey shows a steady improvement in performance. It may make many errors in mastering early reversals, persisting in the choice of previously rewarded alternatives, but as training continues it shifts its preference more and more readily. A fish, in contrast, shows no improvement at all; later reversals are accomplished no more readily than earlier ones.

Although the various sensory, motor and motivational characteristics of the five species we have been studying call for different experimental environments, we have been able to keep certain elements of the test apparatus analogous. In each case the animal is confronted with a pair of translucent Plexiglas panels on which various colors and patterns are projected from behind, and it makes a choice by pressing against one or the other of the panels in its own way: the fish strikes or bites, the pigeon pecks, the monkey pushes with its hand, the turtle or the rat presses with its head or forefoot or both. A correct choice is rewarded with food (a *Tubifex* worm for the fish, a bit of fish for the turtle, some grain for the pigeon, a pellet of sucrose for the rat, a peanut for the monkey), after which there is a brief interval of darkness and then the next choice is offered. If the animal makes an incorrect choice, there is a six-second interval of darkness (called a "time-out"), after which the correct panel alone is illuminated (a procedure called "guidance") and the animal is rewarded for responding to it. Guidance after error guarantees that the animal will not stop responding altogether in the course of a reversal before it has had a chance to learn that the previously unrewarded alternative now is rewarded. The time-out between error and guidance delays access to the reward and thus penalizes precipitous, undiscriminating choice. Without the time-out it would not matter much to the animal whether its choices were correct or not.




**FISH IN A DISCRIMINATION TANK** is presented with a visual problem in which the lights projected on two stimulus disks are differently colored. By pressing its head against the proper disk the fish triggers an automatic reward device: the pincers above the eyedropper (*top right*) close, squirting a *Tubifex* worm into the tank. The experimental apparatus was designed by the author.



**PIGEON MAKING A CHOICE is offered two visually distinct stimuli. (The center light is used in another type of test.) If the correct choice is made, some grain is presented in the rectangular opening. The experimental sequence is programmed by relay circuitry.**

The entire experimental sequence is programmed by some simple relay circuitry and the responses are graphically recorded. With this introduction of automatic control and the removal of the experimenter there is a gain in objectivity: the animals can no longer be influenced by features of the experimenter's behavior. The task of data collection also becomes less arduous and can be entrusted to a co-worker of limited training, who can take data from several subjects concurrently.

In our experiments we employ both spatial and visual problems. A spatial problem is one in which the alternatives are identical to the eye (that is, the stimuli projected on the two Plexiglas panels are the same) and reward is correlated with the position of the panel. A visual problem is one in which the alternatives look different—blue light and green light, for example, or a triangle and a circle—and reward is correlated with appearance, regardless of position. The results of experiments based on spatial and visual problems can be plotted in comparable fashion, as the two graphs on page 97 indicate.

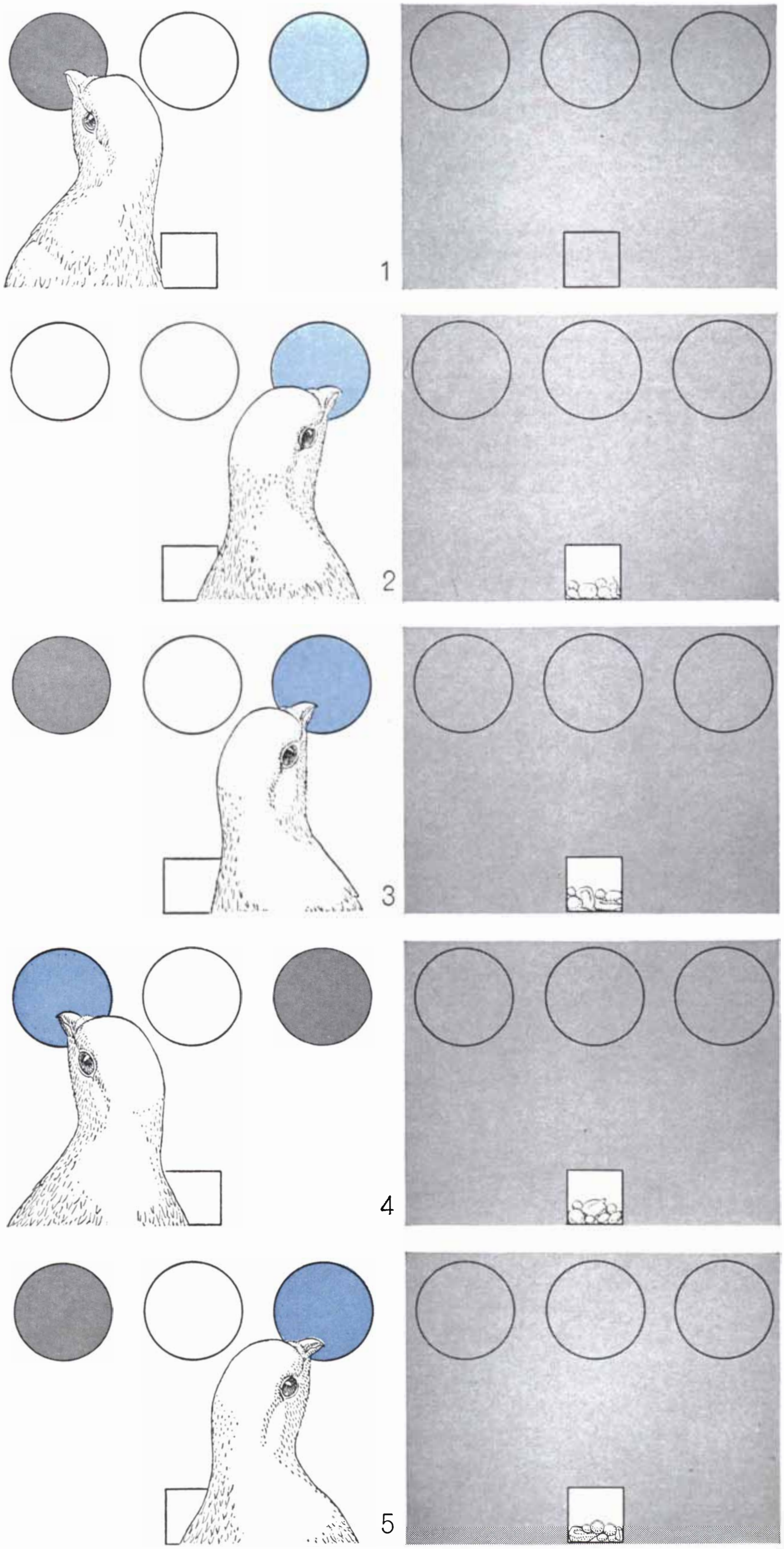
The experiment that provided the data plotted in the top graph was conducted with rats. Each animal was given 40 trials per day and was reversed whenever it made no more than six errors on any given day. The curve traces the average number of errors made in accomplishing each reversal by the group of rats tested. It reveals that the original problem (Reversal 0) was mastered with few errors, that the first reversal was mastered with difficulty and that adjustment to succeeding reversals was progressively less difficult. The bottom graph shows a similar progressive improvement in habit reversal made by pigeons as they were confronted with a visual problem. The plot of average errors per reversal points to a stage of increasing difficulty followed by a stage of steady improvement. Both for the pigeon and for the rat the first reversal is usually the point of maximum difficulty in spatial problems; the point of maximum difficulty tends to occur later in visual problems.

The fish follows a markedly different pattern. Neither of the two types of fish used in our experiments has shown progressive improvement in habit reversal. In two representative experiments fish were tested on spatial and visual problems, and each animal was reversed whenever it made six or fewer errors on a given 40-trial day. When we plot the results in terms of average errors per reversal, both curves rise from the original problem to the first reversal but then fail to decline with continued training [*see upper illustrations on page 98*].

Before we can conclude that the fish is incapable of improvement in habit reversal, two other possibilities must be considered. The first is that the fish is in fact capable of progressive improvement, but only after more reversals than higher animals require. This possibility seems unlikely; in experiments with fish as many as 150 reversals have failed to yield evidence of improvement. Another possibility is that the conditions under which the fish has been tested are to blame for its poor showing, that the difference in performance is to be traced not to a difference in capability but to an inequality in some contextual variable such as sensory demand, motor demand, degree of hunger or attractiveness of reward.

Although the environments we construct for the various animals are roughly analogous, there is no way of equating them exactly with respect to such variables. Do a fish and a pigeon distinguish between a pair of red and green lamps with equal ease? Probably not. Does a *Tubifex* worm have the same reward value for a fish that a sucrose pellet has for a rat? Probably not. We do not know how to select stimuli that will be equally discriminable or rewards that will be equally attractive. Can we ever, then, rule out the possibility that a difference in performance of two different animals in such an experiment stems from a difference in some confounded contextual variable?

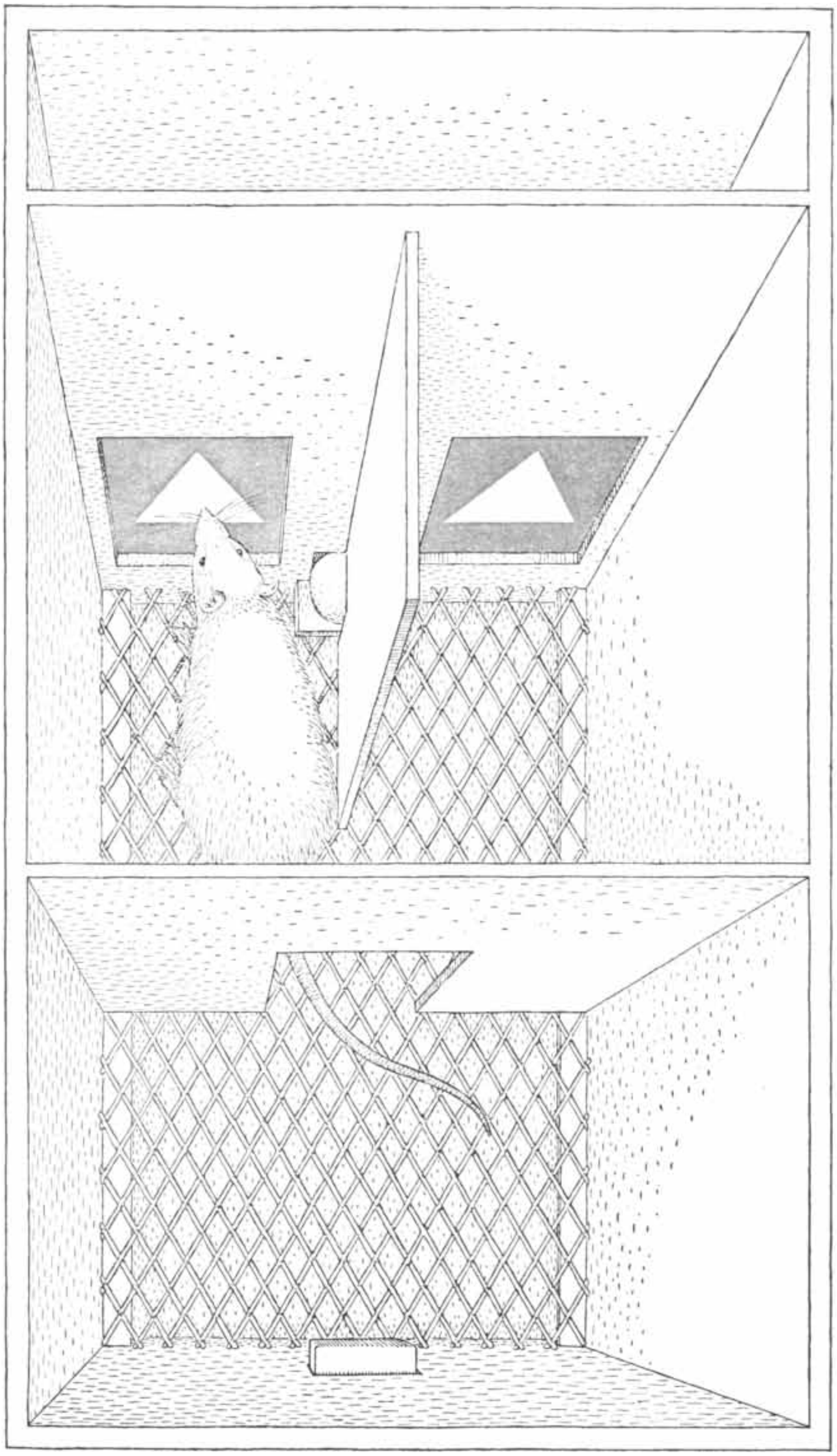
Fortunately, yes, thanks to a technique known as systematic variation. Consider, for example, the hypothesis that a fish fails to show progressive improvement in a given experiment because it is far less hungry (or far more hungry) than a rat that does show improvement. This hypothesis implies that at some level of hunger the fish will show progressive improvement. Thus we can test it—although we cannot produce in the fish the precise degree of hunger in a given rat—by repeating the experiment with subjects of widely different degrees of hunger. Hypotheses about other contextual variables have been tested by similar systematic variation. Progressive improvement in habit reversal has been sought without success in the fish under a wide variety of conditions, whereas the rat




**GUIDANCE is offered an animal after it makes an incorrect choice, as the pigeon has done on its first trial (*top left*). No reward is given and the lights go out in the box for six seconds (*top right*). Then the correct panel alone is lighted and the pigeon is rewarded for pecking at it (*2*). Thereafter the pigeon is shown selecting the proper panel even when faced with the wrong alternative (*3*) or a change in the position of the correct panel (*4 and 5*).**

and the pigeon do progress under an equally wide range of conditions. Indeed, it is difficult to find a set of conditions under which the pigeon and the rat fail to show improvement.

The results of experiments on habit reversal in the painted turtle are in a sense intermediate between those for the fish on the one hand and those for the pigeon and the rat on the other. In spatial problems the turtle shows progressive improvement; in visual problems it does not. The data from two recent experiments with turtles, one group trained in a spatial problem and the other trained in a visual problem, are plotted at the bottom of page 98. Both curves give the average number of errors made per reversal. The curves rise from the initial presentation of the problem to the first reversal; then the spatial curve declines but the visual curve does not. We conclude simply that experiments on habit reversal tap an intellectual capability of higher animals that is not at all developed in the fish and is manifested by the turtle only in a restricted class of problems.



**RAT SOLVING A SPATIAL PROBLEM is confronted by two visually identical stimuli. The rewarded alternative is determined by its position. When the animal chooses correctly, a sucrose pellet drops into the cup between the panels. The rat then initiates the next trial by going into the other section of the box and pulling a lever on the wall (*bottom*).**

Other intellectual differences between our test animals appear when the rewarded alternative is changed within a given trial session (not from session to session). Experiments involving this technique are called probability-learning experiments. In a typical probability-learning experiment alternative *A* would be rewarded on, say, a random 70 percent of the trials and *B* would be rewarded the other 30 percent. As in experiments on habit reversal, we confront the animal with either a visual task or a spatial one. We can employ either the guidance method (in which an incorrect choice is followed by a time-out, presentation of only the correct alternative and finally a reward) or the noncorrection method (in which the trial ends whether the rewarded or the unrewarded alternative is chosen). Trained without guidance, subjects of all species tend to "maximize," choosing the 70 percent alternative on all the trials. (An occasional subject comes to the situation with a preference for the 30 percent alternative and persists in choosing it.) If guidance is used, however, striking differences appear among the various species.

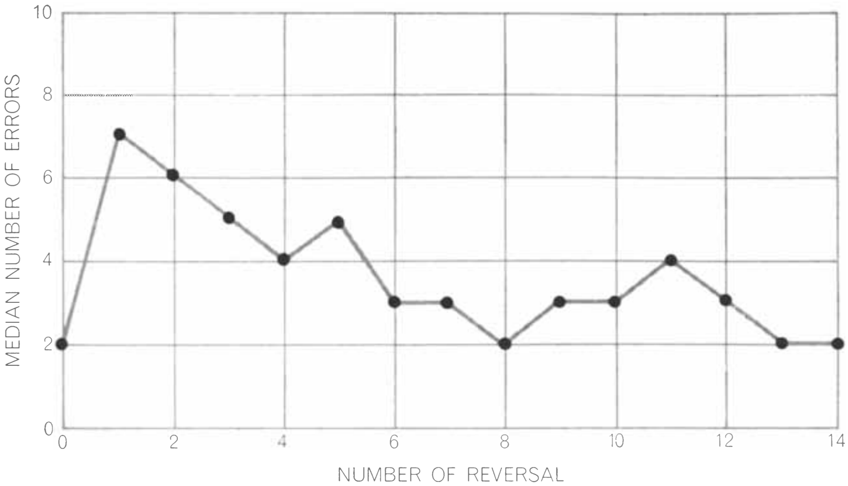
Some representative results for the rat and the fish are presented in the two graphs on page 99. During the first stage of the experiments reflected in the graphs the animals were trained on a visual problem—horizontal *v.* vertical stripes—by the guidance method. The choice of horizontal stripes was rewarded in 70 percent of the trials for the first 30 days and in 100 percent of the trials for the next 10 days. The rat tended to maximize under these conditions: after several days it began to choose the 70 percent alternative much more than 70 percent of the time; with the shift in the reward ratio to 100 percent the trend toward absolute preference continued as it might have even without the shift. In contrast, the fish showed a choice pattern we characterize as "matching." It began to choose the

70 percent alternative about 70 percent of the time after a few days of training, and when the reward ratio was shifted to 100 percent, it rapidly began choosing the rewarded alternative in every instance. In other words, the fish produced a choice ratio that tended to match the reward ratio. We found that in spatial problems too the rat maximizes and the fish matches as long as guidance is used (although without guidance both species tend to maximize).

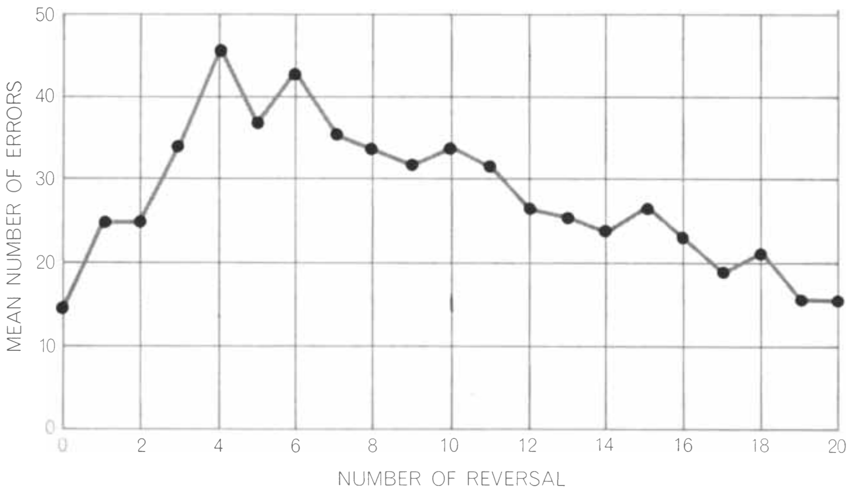
Whereas the rat and the monkey usually maximize in experiments on probability learning even when guidance is used, they sometimes show a correspondence between choice ratio and reward ratio of a rather different kind from that revealed by the fish. The mammals produce a pattern of systematic matching. Occasionally, for example, a group of rats will choose the rewarded alternative of the preceding trial. This tendency toward reward-following produces a 70 percent choice of the 70 percent alternative when the reward ratio is 70 to 30, and a 50 percent choice of each alternative in a problem in which the ratio is 50 to 50. An opposite strategy—to avoid the rewarded alternative of the preceding trial—sometimes has been used by the monkey. No such systematic tendencies are reflected in the matching of the fish, which can be characterized as random.

A pattern of random matching is also produced by the pigeon when it is tested on a visual problem. Since the rat either maximizes in such cases or begins reward-following, the experiments on probability learning have provided us with a clear functional difference between the rat and the pigeon.

Experiments on probability learning yield results for the turtle that are reminiscent of the result of experiments on habit reversal. The efforts of the turtle can be described as random matching in visual problems but maximizing or reward-following in spatial problems. In both kinds of experiment, then, its behavior is ratlike in spatial problems but fishlike in visual problems.




**PROGRESSIVE IMPROVEMENT** of a group of rats tested on spatial problems that required habit changes is plotted. In solving the original problem (*Reversal 0*) the group made a median number of two errors. When the rewarded alternative was switched (*Reversal 1*), many errors were made before the rats mastered the problem and the rewarded alternative could be switched again. The rats then made fewer errors in achieving reversals.




**HABIT-REVERSAL EXPERIMENT** involving a group of pigeons trained on visual problems yielded the results summarized in this graph. The birds were given 40 trials per day. They made a mean number of 15 errors in mastering the original problem. Difficulty in coping with reversals continued past the first one, reaching a maximum on the fourth reversal, when most animals had to be trained for six days before achieving reversal.

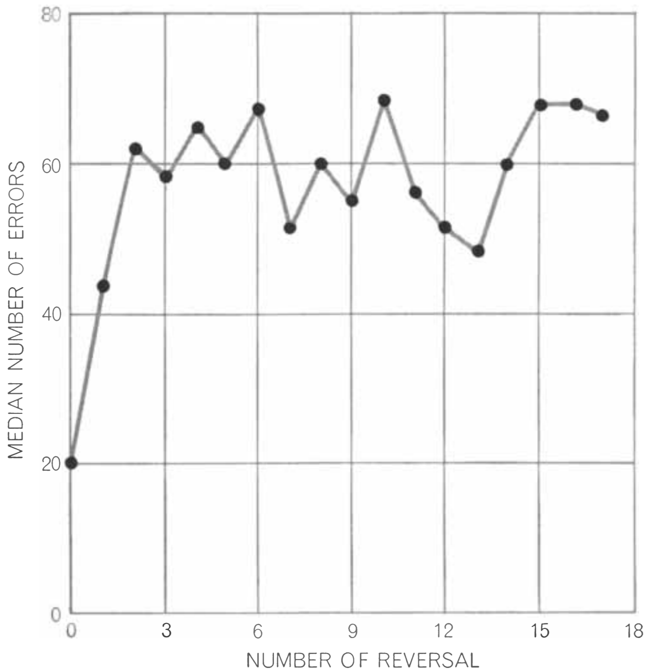
We can use such categories of intellectual behavior (ratlike or fishlike) to tabulate the results of our experiments on habit reversal and probability learning. Such a table [*second illustration from top on page 100*] suggests the following generalizations: As we ascend the evolutionary scale we do not find a pattern of intellectual continuity but one of discontinuity. Moreover, the modes of adjustment evolved by the higher animals appear earlier in spatial than in visual contexts.

The monkey and the rat are not differentiated by the criteria used to construct our table. The two mammals do, however, show differences in their styles of probability learning, with the reward-following of the rat giving way in the monkey to the opposite strategy (avoiding the rewarded alternative of the preceding trial). It is notable that this strategy of the monkey has been observed so far only in spatial problems, providing support for the generalization that as we go up the evolutionary scale new modes of adjustment appear earlier in spatial than in visual settings.

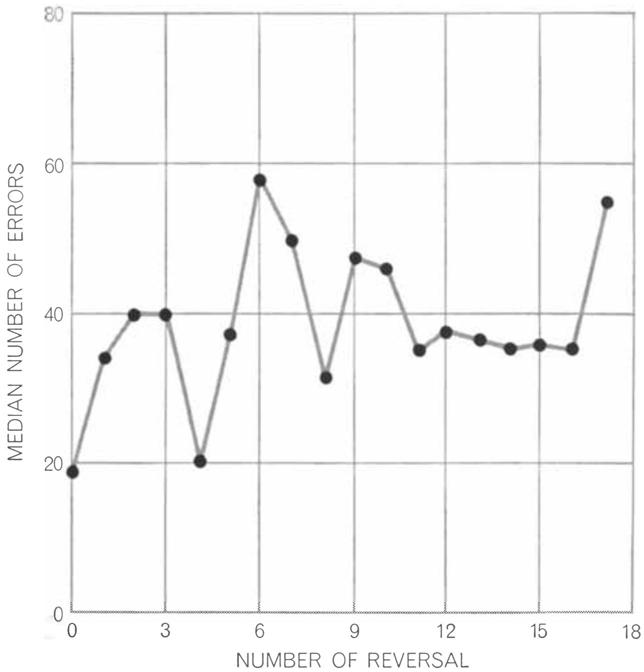
The idea of advance has long been implicit in the idea of evolution. We are thus led to ask if the ratlike modes of adjustment are really effective in the sense that they help the animal to cope with its environment. Do they actually represent a higher intelligence? In general the answer is yes. Progressive improvement in habit reversal represents a flexibility that cannot help but be of value in an animal's adaptation to changing circumstances. As for probability learning, the ability to maximize produces a higher percentage of correct choices than does matching. In a problem where the reward ratio is 70 to 30, for example, the probability of correct choice is 70 percent if the subject is maximizing but only 58 percent—(.70 ×

.70) + (.30 × .30)—for an animal that is matching. Systematic matching is no more successful than random matching by this criterion, and yet we know that human subjects employ systematic matching in trying to find a principle that will enable them to make the correct choice 100 percent of the time. If the use of systematic matching by lower animals is based on some crude, strategic capability, it represents a considerable functional advance over random matching.

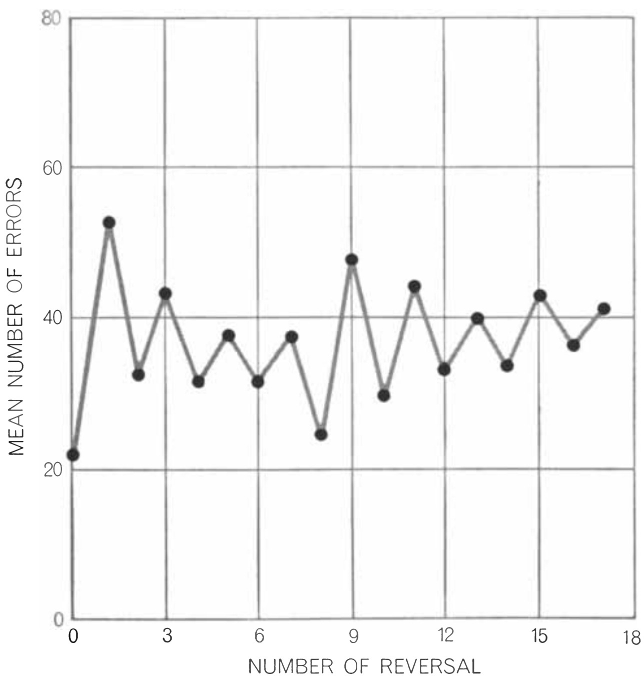
Having found behavioral differences among the various types of animal, we are now trying to trace them to physiological differences. My colleague R. C. Gonzales has lately been conducting experiments on habit reversal and probability learning with adult rats lacking extensive portions of the cerebral cortex, a prominent feature of the mammalian brain that is absent from the brain of the fish and first appears



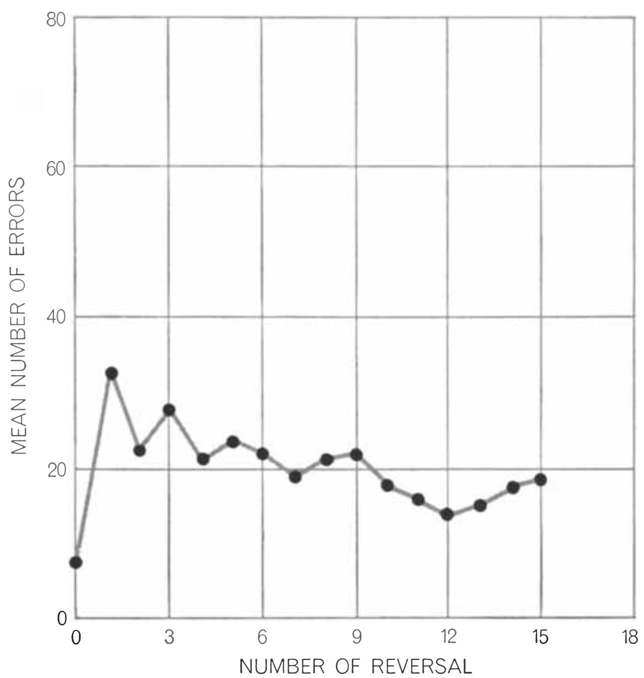

**FISH TESTED ON SPATIAL PROBLEM** yielded data for this graph, which reveals no progressive improvement in habit reversal. The curve remains approximately level after the first reversal.



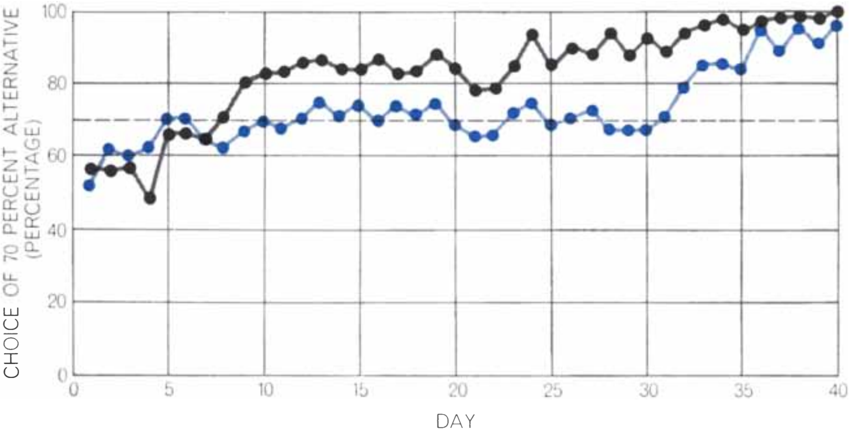

**FISH TESTED ON A VISUAL PROBLEM** show no progressive improvement in habit-reversal experiments. Even graphs of experiments involving 150 reversals do not reveal any downward slope.



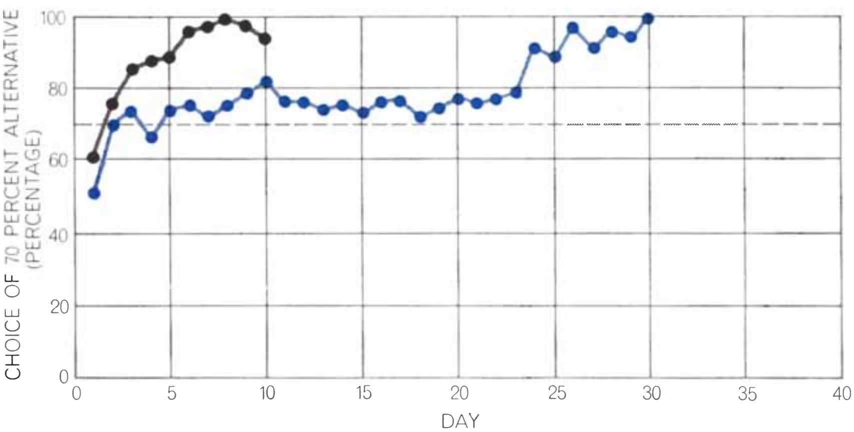

**TURTLES TESTED VISUALLY** failed to show any progressive improvement in habit reversal. Occasional drops between reversals have no significant statistical effect on the slope of the curve.




**TURTLES SOLVING SPATIAL PROBLEMS** do show progressive improvement when the results of a habit-reversal experiment are plotted. In this graph improvement follows the initial reversal.




**PROBABILITY LEARNING is the subject of experiments such as the one summarized in this graph. The graph compares results for a group of rats (*black curve*) and fish (*colored curve*) tested on visual problems. One alternative was rewarded on 70 percent of the trials for 30 days and on 100 percent of the trials thereafter. Almost from the outset the rat "maximized," tending to make the advantageous choice on 100 percent of the trials. The fish "matched," keeping its choice ratio equal to the reward ratio throughout the experiment.**

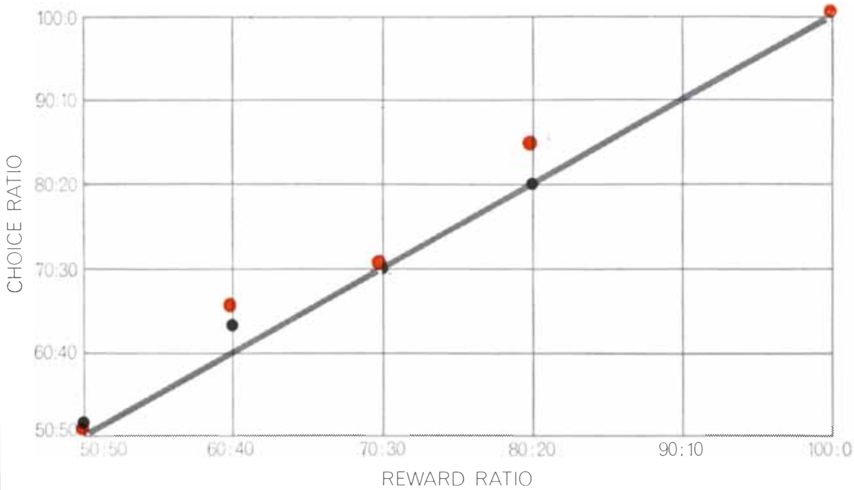



**MAXIMIZING is illustrated for rats (*black curve*) and fish (*colored curve*). The animals were trained on spatial problems in which one alternative was rewarded on 70 percent of the trials. The rat, after 10 days, chose the advantageous alternative almost invariably. The fish matched its choice ratio with the reward ratio for 20 days, at which time guidance was discontinued and it tended to choose the advantageous alternative on almost every trial.**

in the reptilian brain. The decorticated rats showed progressive improvement in habit reversal on spatial but not on visual problems. In experiments on probability learning they maximized on spatial problems but took to random matching on visual problems. The intellectual behavior of these decorticated rats was exactly like that of the turtle, an animal with little cortex.

Summarizing the meaning of these experiments calls for sketching the origins of the study of animal intelligence. A century ago, as Charles Darwin developed his theory of evolution, he denied not only the physical uniqueness of man but also the intellectual uniqueness. In doing so he used the only evidence available to him: episodes described by naturalists, hunters, pet-owners and zookeepers. It was not until the start of the 20th century that the study of animal intelligence was brought from the realm of the anecdote into the laboratory by Edward L. Thorndike, who was then working at Harvard University. Thorndike's experiments led him to deny the existence of intellectual uniqueness anywhere in the evolutionary hierarchy of animals. It was he who set forth the theory that differences from species to species are only differences of degree, and that the evolution of intelligence involves only the improvement of old processes and the development of more neural elements.

Our studies of habit reversal and probability learning in the lower ani-

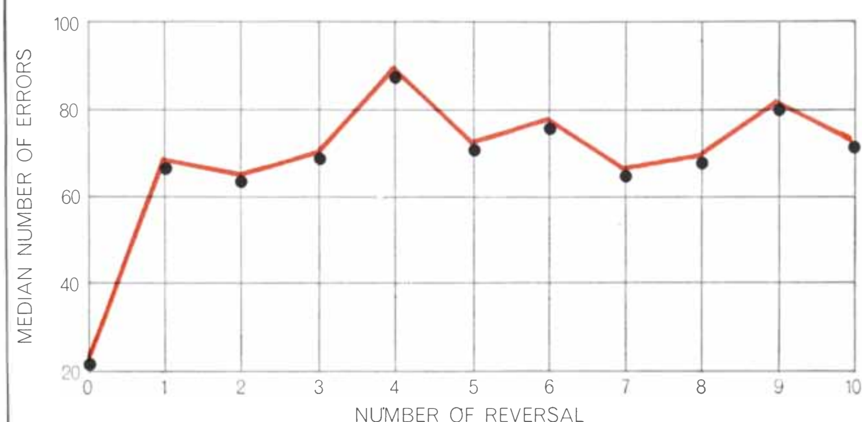



MATCHING of choice ratio (*vertical axis*) with reward ratio (*horizontal axis*) approaches a linear relation for both the pigeon (*colored dots*) and the fish (*black dots*). The graph is based on studies of fish given visual and spatial problems and pigeons given visual ones.

| TEST ANIMAL | SPATIAL PROBLEMS | | VISUAL PROBLEMS | |
|---|---|---|---|---|
| | REVERSAL | PROBABILITY | REVERSAL | PROBABILITY |
| MONKEY | RAT | RAT | RAT | RAT |
| RAT | RAT | RAT | RAT | RAT |
| PIGEON | RAT | RAT | RAT | FISH |
| TURTLE | RAT | RAT | FISH | FISH |
| FISH | FISH | FISH | FISH | FISH |

DIFFERENCE IN INTELLIGENCE of the five animals studied by the author (*column at left*) are tabulated according to the subject's response to spatial and visual problems in experiments on habit reversal and probability learning. The behavior of each animal in each test situation is characterized as ratlike (progressive improvement in habit reversal and maximizing or nonrandom matching on probability-learning tests) or fishlike (no such improvement in habit reversal and random matching on probability-learning tests).

mals suggest that brain structures evolved by higher animals do not serve merely to replicate old functions and modes of intellectual adjustment but to mediate new ones (a contradiction of the Thorndike hypothesis). Work with decorticated rats points to the same conclusion. Yet it should be observed that these recent studies represent a new turn in the investigative path founded by Thorndike himself. Clearly bringing the study into the laboratory was the real first step toward replacing guesses with facts about the evolution of intelligence and its relation to the evolution of the brain.




DECORTICATED RAT tested on visual problems showed no progressive improvement in habit reversal on the test summarized in this graph. The curve rises, then remains level.

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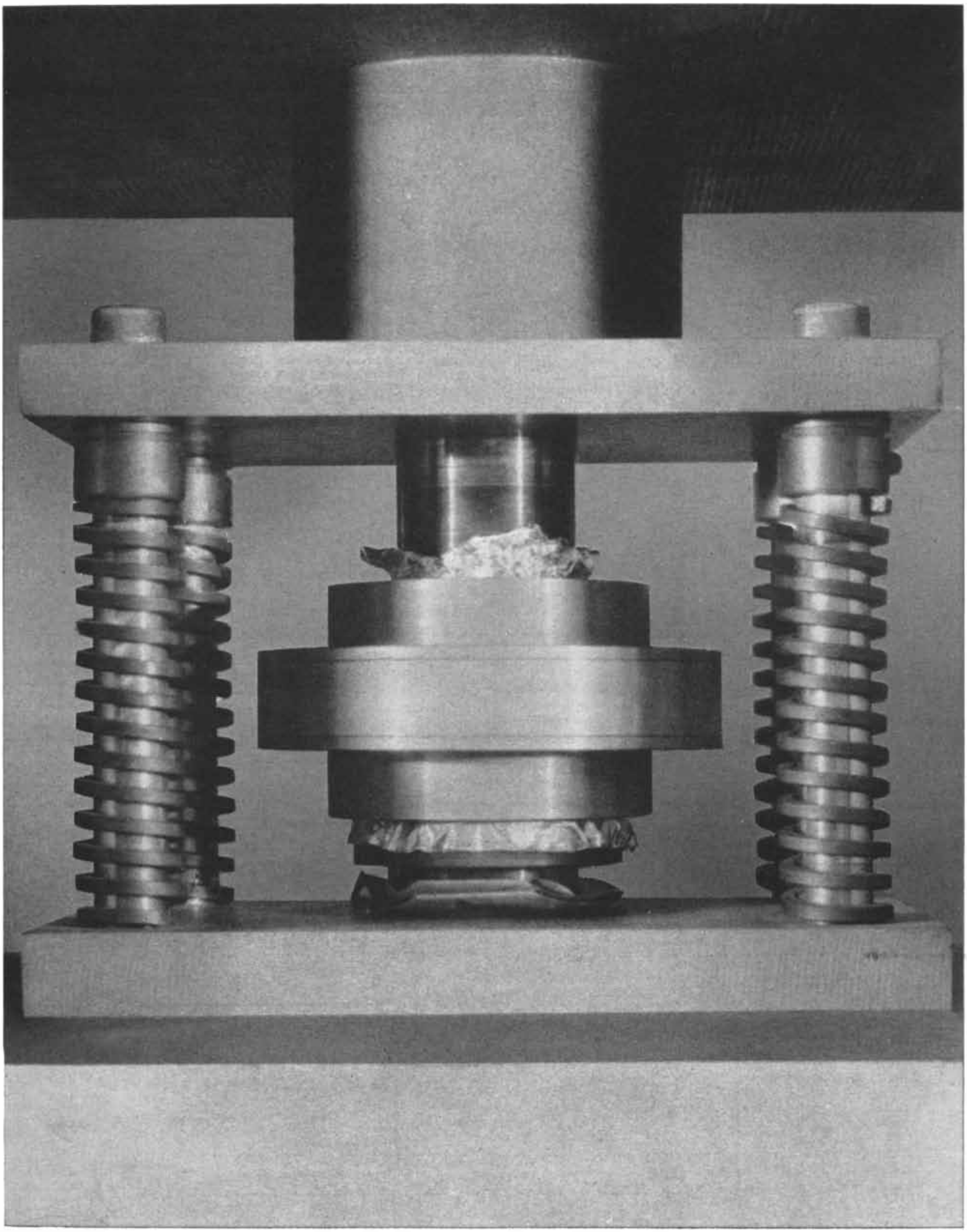


**HIGH-PRESSURE PRESS** at the Lincoln Laboratory of the Massachusetts Institute of Technology was employed by the author and his colleagues in their experiments aimed at measuring the effect of high pressure on nuclear magnetic resonance in iron. The press is capable of generating and containing pressures as high as 65,000 atmospheres. The iron sample is encased in a cylindrical carbide die (*squat disk at center*). Pressure is applied from above the sample by a movable carbide piston; an identical piston below the sample is stationary. The four coil springs, which serve to steady the upper piston, are each about eight inches long. Metallic foil is sometimes used for electrical shielding. Most of the pressure-generating machinery of the press is above upper piston.

# Magnetic Resonance at High Pressure

*New tools for probing the electric and magnetic fields of individual atoms in a solid can detect changes in the electronic structure that take place when the atoms are forced closer together by high pressure*

by George B. Benedek

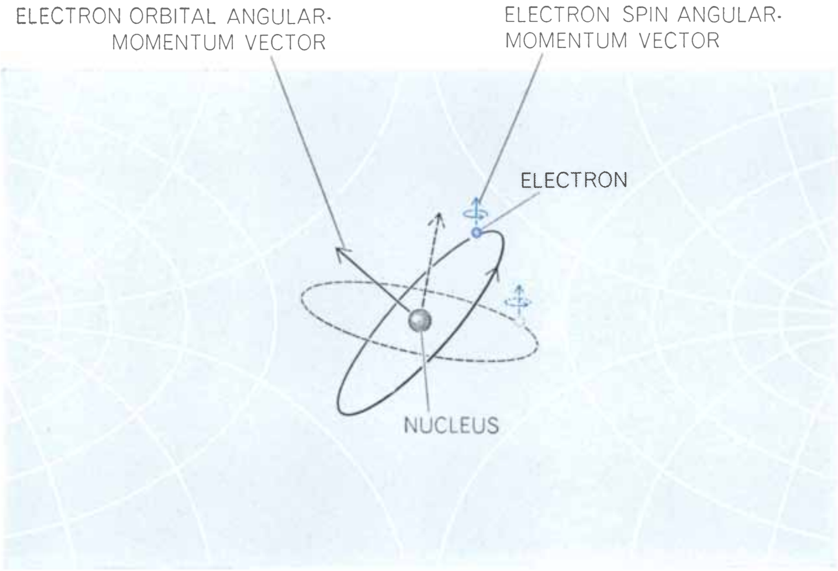
In 1905 Percy W. Bridgman, then a 23-year-old student in the physics department of Harvard University, had a fortunate accident. He had constructed a high-pressure chamber with a glass window for an optical experiment and was increasing the pressure of the liquid in the chamber when the thick glass of the window suddenly burst, scattering fragments in all directions. The explosion destroyed an essential part of his apparatus and Bridgman had to order a new part from Europe. While waiting for it he worked on improving the apparatus. One of its weaknesses, characteristic of all high-pressure equipment at the time, was the leakiness of its joints. Bridgman designed a new packing of soft material that could easily be assembled or taken apart. On testing this design he found to his surprise that the joint never leaked, no matter how high he raised the pressure. He had hit on an arrangement that kept the pressure within the packing material always greater than the pressure in the enclosed fluid.

Bridgman saw that the new principle, making possible leakproof joints and fittings, "opened an entirely new pressure field, limited only by the strength of the containing vessels and not by leak." For the rest of his long and productive life he followed this vision, systematically studying the properties of matter at high pressure. The experimental methods at his disposal were quite successful for exploratory work, but they often failed to provide the detailed information needed today by the theoretical physicist interested in the nature of solids. The chief drawback of the older measurements was that they reflected the combined effect of several of the microscopic properties of a given solid. For example, one could measure the electrical resistance of a solid at high pressure. This resistance, however, results from the combined action of the motion of the conducting electrons of the metal, the vibrations of its atoms and the scattering of the conducting electrons by the vibrating atoms, and it is quite difficult to distinguish the relative importance of each of these properties. What was needed were new experimental techniques that would reveal the microscopic properties of the solid separately and distinctly.

We are now coming into possession of such experimental tools. In the past 20 years the solid-state physicist has developed new means for probing the microscopic world inside solids with a precision and delicacy hitherto undreamed of. He has been able to secure details of the electric and magnetic fields within and around individual atoms, and he has been able to perceive clearly the coupled oscillation of the atoms around their equilibrium positions. With this knowledge he can calculate and predict many of the microscopic and macroscopic properties of solids. It is the purpose of this article to show how some of these modern techniques provide a dramatic visualization of the alterations in atomic structure that are produced when the atoms in the solid are forced closer together by high pressure.

The first experiment I shall describe is a measurement of the effect of pressure on the magnetic field at the very center of an atom in solid iron. Each of the atoms in the iron sets up around and within itself a magnetic field that originates in the following way. Magnetic fields are of course produced by the motion of electric charges; in the atom the moving charges are the electrons. The innermost electrons move around the atomic nucleus with speeds approaching the speed of light, and even the less tightly bound outer electrons have a speed of about 100 million centimeters per second. In addition to the magnetic field developed by its rapid orbital motion, the electron spins on its own axis and thereby sets up an additional magnetic field. Therefore one expects the magnetism of an atom to have two sources: (1) the orbital angular momentum of the electrons and (2) the spin angular momentum. Indeed, both effects are active in producing the magnetism of isolated atoms. An iron atom in a solid, however, is surrounded by neighboring atoms that act to "quench" the free orbital motion of the electrons. This quenching is due to the electric fields of the atoms, which exert torques on an electron's plane of orbital motion and cause the plane to twist rapidly among different random orientations in space. As a result the orbital angular momentum in any particular direction is on the average reduced to zero and the orbital motion makes no contribution to the magnetism of the atom. Only the spin-produced magnetism remains; the spin does not "feel" the electric field of neighboring atoms.

The spins of the electrons in iron atoms interact to produce one of nature's marvels: the phenomenon of ferromagnetism. If two iron atoms are so close together that the spinning electrons of one atom overlap those of the other, a torque is exerted on the electron spins in each of the atoms. In iron this torque twists the spins of electrons in neighboring atoms parallel to each other. The transmission of alignment from atom to atom gives rise to parallel spin alignment in as many as 10,000 billion billion ($10^{22}$) atoms!



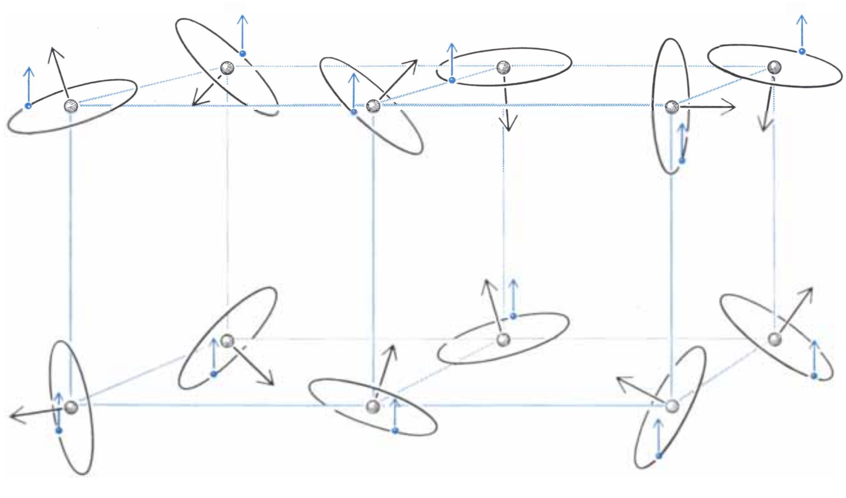

**MAGNETISM OF AN INDIVIDUAL ATOM** is generated by two kinds of electron motion: (1) the rapid orbital movement of the electrons around the nucleus and (2) the spin of each electron around its own axis. In either case the arrangement of the resulting magnetic-field lines (not shown here) is determined by the directions of the angular-momentum vectors (*straight arrows*) produced by the two kinds of motion. In solid iron the close proximity of the atoms to one another has the effect of "quenching" the electrons' orbital movement; the orbits of the electrons circling the nuclei are tilted into various planes at random by the electric fields of the neighboring atoms (*radial lines at left and right*). As a result of this randomness the electrons' orbital motions produce no net magnetism in the substance. In contrast, the electron spin angular-momentum vectors in neighboring atoms remain aligned parallel to one another because of the strong torques exerted between the electron spins.

Let us now imagine that we have entered the submicroscopic world of magnetically aligned iron atoms carrying with us a probe capable of measuring the magnetic field as we move from point to point. The probe would indicate extraordinarily rapid fluctuations. In the region between atoms, where the density of electronic charge is low, the magnetic field will be relatively weak. As one moves close to the center of an iron atom, however, the field will be extremely strong. The average strength of the magnetic field inside a magnetized bar of iron is 20,000 gauss; at the center of the iron atom it is as high as 330,000 gauss!

We know that this is the case because an excellent probe has been placed in the center of an atom by nature; it is the atomic nucleus. The nucleus itself can be magnetic because it too consists of moving and spinning charged particles: the nucleons. The magnetic field produced by a nucleus is typically 2,000 times weaker than that produced by an atom. When the spinning iron nucleus feels the 330,000-gauss magnetic field around it, however, it responds by precessing like a wobbling top around the direction of the field with a frequency of about 45 million cycles per second. If there is any change in the strength of the magnetic field at the nucleus, it will produce a corresponding change in the frequency of precession; the nuclear precessional frequency is in fact directly proportional to the strength of the magnetic field at the nucleus. This frequency is therefore an ideal indicator of the magnetic field at the center of the atom.

We can now measure the precessional frequency very accurately. The key is to be found in the experimental techniques of nuclear magnetic resonance. The principal ideas of this method of experimentation are not difficult to understand. One surrounds the sample under study with a coil of wire and sends through the coil an electric current that oscillates back and forth millions of times per second. The oscillating current produces within the coil and within the sample a rapidly oscillating magnetic field. When the frequency of this "driving" magnetic field is tuned so that it is equal to the precessional frequency of the nucleus, the two oscillating systems come into resonance with each other and the nuclear magnets absorb energy from the driving field. The nuclei absorb energy by tilting their axes of spin down from the direction of the steady magnetic field of the iron as a whole; this absorption is large enough to produce a detectable change in the effective resistance of the high-frequency coil.

By measuring the frequency of oscillation in the coil when nuclear absorption occurs, one can at once determine the nuclear precessional frequency and hence the strength of the magnetic field at the center of the atom. In fact, it is possible to determine the frequency of nuclear precession in iron nuclei with an accuracy of about 2,000 cycles per second in 45 million cycles per second. Therefore if the magnetic field at the nucleus changes by a fraction as small as five parts in 100,000, it can still be detected. To put it another way, if the



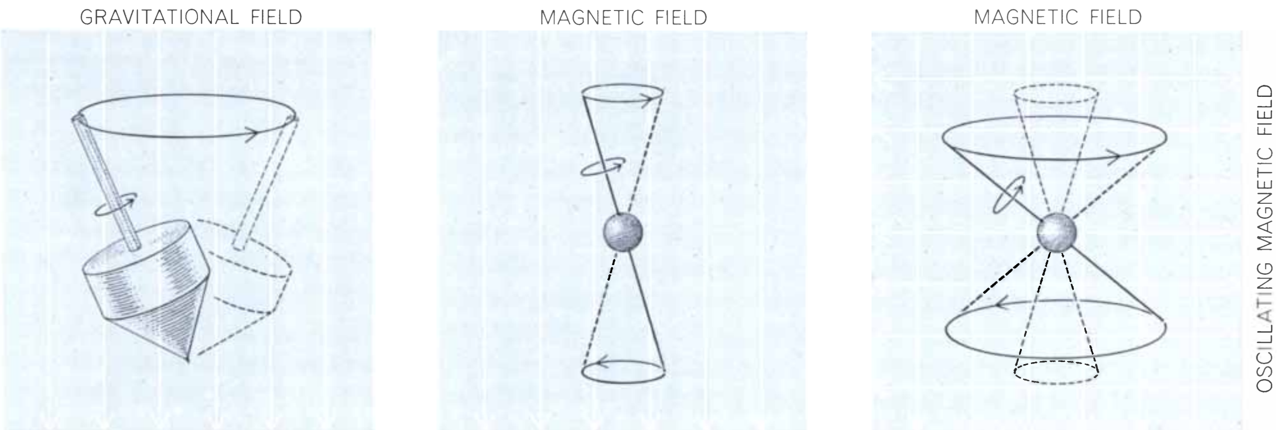
**IN FERROMAGNETIC IRON** the electrons in neighboring atoms act on one another to line up their spin angular-momentum vectors in the same direction (*colored arrows*); orbital angular-momentum vectors are oriented randomly (*black arrows*). In iron as many as 10,000 billion billion ($10^{22}$) atoms may line up their electron spin vectors in parallel.

330,000-gauss magnetic field at the center of the atom changes by as little as 15 gauss, we can detect the change.

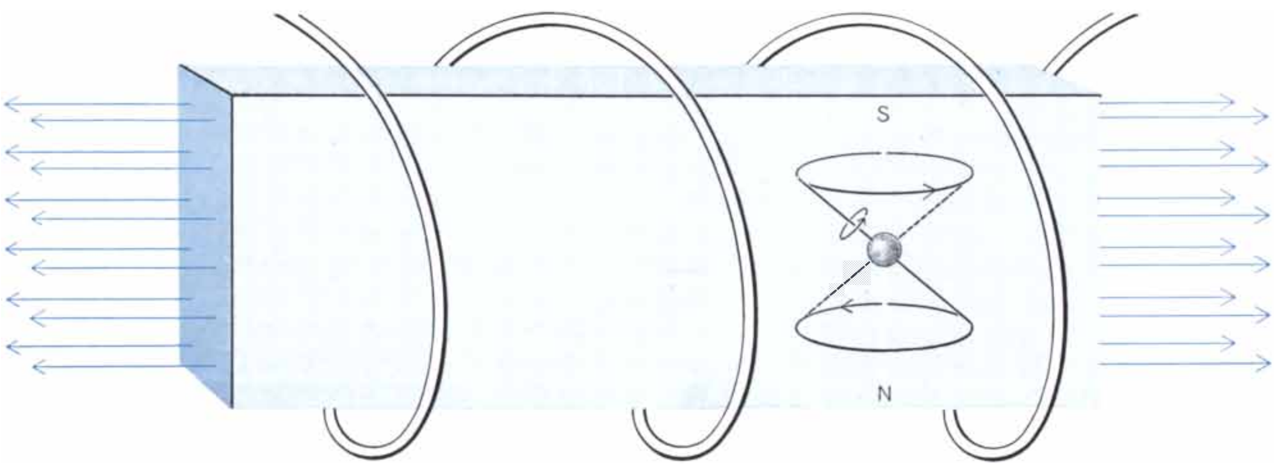
Now the stage is set for the high-pressure experiment. We place the sample of iron inside the high-frequency coil, using iron that is in the form of a powder so that the oscillating magnetic field can penetrate each of the iron particles. The sample and coil are then immersed in a fluid that can be squeezed to very high pressure. The pressure in the liquid transmits itself to the iron, thereby squeezing it and forcing the iron atoms closer together. The first magnetic-resonance studies of iron under high pressure were conducted at pressures as high as 10,000 atmospheres. At this pressure the distance between the atoms of the iron decreases by about .2 percent. The change in interatomic distance produced a change in the magnetic field at the iron nucleus of about 550 gauss.

The press employed to squeeze the liquid was the same one Bridgman himself had built in 1926. Recently an entirely different kind of high-pressure chamber has become commercially available. With this new system one can generate pressures in the sample that can be as high as 100,000 atmospheres. If magnetic-resonance experiments could be conducted in such a press, it would be possible to observe the effect of bringing the atoms almost 10 times closer than was possible in the experiments at 10,000 atmospheres.

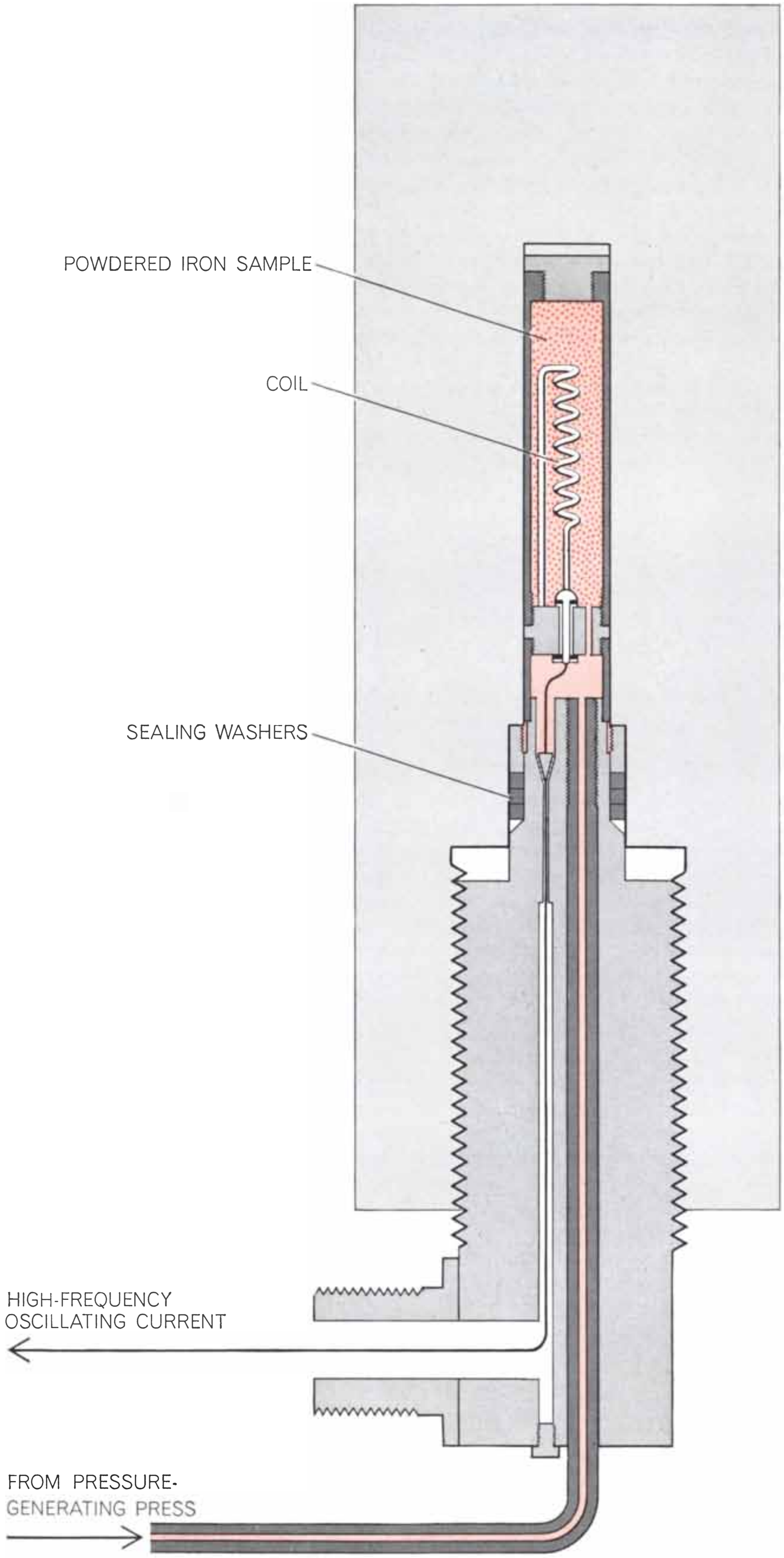
In the new presses the pressure is transmitted from carbide pistons to the sample through a soft solid material: silver chloride. Other experimenters had used the presses to measure the effect of pressure on the electrical resistance of metals; no one had succeeded, however, in devising electrical leads capable of transmitting in and out of the tiny high-pressure region the high-frequency current needed to achieve nuclear magnetic resonance. After some experimentation J. D. Litster, one of my graduate students at the Massachusetts Institute of Technology, was able to adapt the




**PRECESSION of a spinning iron nucleus under the influence of a steady magnetic field (*center*) resembles the gyroscopic action of a spinning top (*left*). The frequency of this precession is directly proportional to the strength of the magnetic field that acts on the nucleus. By applying an additional horizontal magnetic field (*right*), which oscillates back and forth at exactly the same rate as the axis of the precessing nucleus, the two oscillating magnetic systems can be brought into resonance with each other; the nuclei will then absorb energy from the horizontal field by tilting their axes of spin down from the direction of the steady magnetic field.**




**PRECESSIONAL FREQUENCY of a spinning iron nucleus, and hence the strength of the magnetic field at the center of the atom, can be determined by means of the technique of nuclear magnetic resonance. The iron sample is inserted in a coil of wire, through which an electric current oscillates back and forth millions of times per second. The oscillating current produces within the coil and within the sample a rapidly oscillating magnetic field. When the frequency of this field and the frequency of the precessing nuclei are in resonance, the absorption of energy by the nuclei is large enough to produce a detectable change in the effective resistance of the high-frequency coil. In this way it is possible to determine the precessional frequency in the iron nuclei with great accuracy.**




**HIGH-PRESSURE CHAMBER used in the first magnetic-resonance studies of iron under high pressure was capable of sustaining pressures as high as 10,000 atmospheres. Pressure-transmitting liquid (*light colored area*) enters chamber through a steel tube (*bottom*) connected to pressure-generating press. Colored dots in chamber represent iron particles.**

nuclear-resonance coil-and-sample assembly to the new kind of press [*see illustration on opposite page*]. With this arrangement it was possible to compress iron particles to pressures as high as 65,000 atmospheres while simultaneously observing their nuclear resonance. The upper limit of 65,000 atmospheres was set solely by the strength of the press, which was generously made available to us by Harry C. Gatos of the Lincoln Laboratory of M.I.T. At this pressure the distance between the iron atoms was reduced by about 1.3 percent, and the magnetic field at the nucleus was changed by about 3,600 gauss.

One striking result of the measurements up to 65,000 atmospheres was that at pressures up to at least 60,000 atmospheres the increase in nuclear-resonance frequency is directly proportional to pressure. This at once suggested that the nuclear-resonance frequency could serve as a continuous gauge for the pressure inside the press. Such a gauge is much needed in these presses; otherwise a rather elaborate procedure is required for measuring the pressure even at a few fixed points as the pressure goes up. The first ultrahigh-pressure nuclear-resonance experiment had thus provided us with an unexpected technological dividend.

What, however, do these measurements tell us about more central questions? What, for example, is the arrangement of the electrons in ferromagnetic iron? We are looking for a quantitative description of the electronic structure in terms of quantum mechanics. Essentially this boils down to determining the probability of finding an electron inside any given tiny volume within the atom. The experiments yield some information on this point.

The magnetic field at the center of the iron atom is a measure of the electron "spin density" there. If the spin density simply indicated the density of electrons, one could readily calculate the probability of finding an electron within a certain volume at the nucleus. The actual situation, however, is more complicated. Some electrons are spinning in the direction of the atom's ferromagnetism; others are spinning in the opposite direction. The electrons' magnetic field at the nucleus is produced by what we might call the net spin density, or the "spin up" density minus the "spin down" density. From the measured strength of the field (330,000 gauss) we can calculate this net density, and from the effect of pressure on the field at the nucleus we can calculate the change in the net density at the nucleus.

Our aim, however, is to obtain more specific information. We would like to know how the distribution of each category of electrons—those spinning up and those spinning down—is affected by changes in the available volume. There is no way to probe for these quantities separately, but they could be calculated if we knew the *sum* of the spin densities as well as the difference—that is, "spin up" density *plus* "spin down" density as well as "spin up" minus "spin down." Fortunately there was a means of measuring the effect of high pressure in changing the sum of the densities. The measuring technique that solved the problem is based on the Mössbauer effect [see "The Mössbauer Effect," by Sergio De Benedetti; SCIENTIFIC AMERICAN, April, 1960].

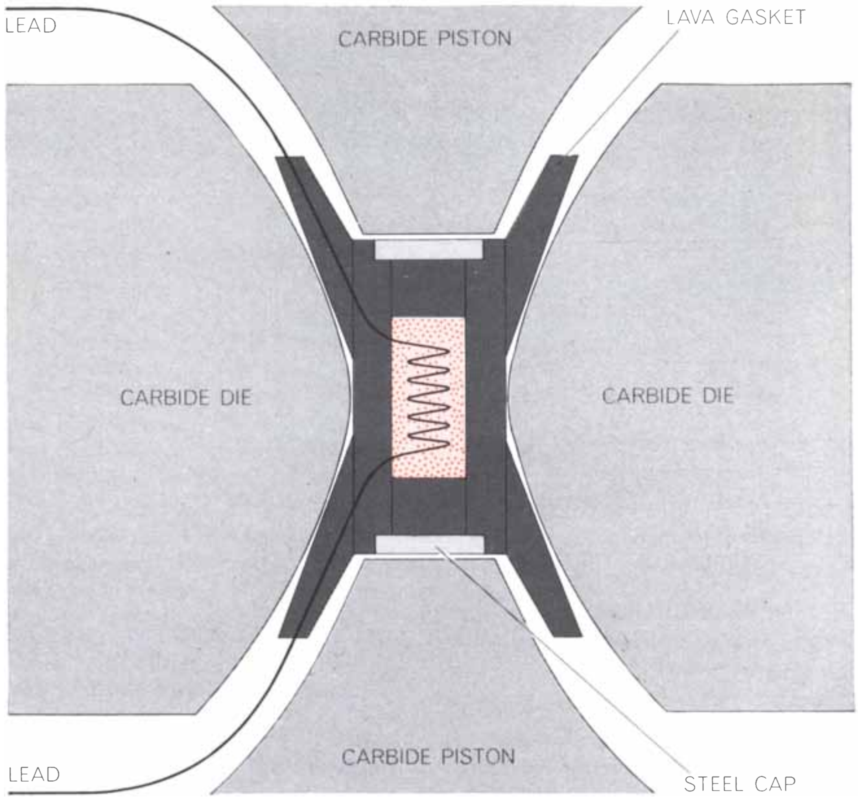
The experiment consists in determining the effect of pressure on the energy of gamma rays emitted by the "excited" nuclei of the isotope iron 57 in ferromagnetic iron. The excited nuclei are deliberately introduced into the iron in the following way. A film of the radioactive isotope cobalt 57 is deposited on the surface of the sample of iron and allowed to diffuse into it. The nucleus of cobalt 57 decays by first swallowing up one of its surrounding electrons. In so doing the nucleus gains a unit of negative charge and becomes an iron-57 nucleus. This nucleus has too much energy to be stable; that is, it is excited. The excess energy is dissipated by the emission of a succession of gamma rays, one of which has an energy of 14.4 kilovolts. It is from this gamma ray that we can obtain information about the electronic charge density at the nucleus.

"But surely," you will say, "the energy of the gamma ray will reflect only changes in *nuclear* structure and will have no connection with electronic structure." It is said in textbooks that nuclear radioactivity cannot be affected by changes that take place among the electrons outside the nucleus. This is almost, but not quite, correct. Although virtually all of the gamma rays' energy is derived from the nuclear process, the electrons make a small contribution to the total energy through an interaction with the nucleus. The radius of the nucleus in the excited state is slightly different from the radius of the nucleus in the "ground" state; as a result the transition from one state to the other causes a change in the energy of the electrostatic interaction of the electrons and the nucleus, and this difference is reflected in the energy of the emitted gamma ray. The energy added by the change is called the "isomer shift." Depending in part on the total spin density of electrons at the nucleus, the isomer shift is proportional to that density. The shift therefore provides an indication of the total electron density (spin up plus spin down) at the nucleus.

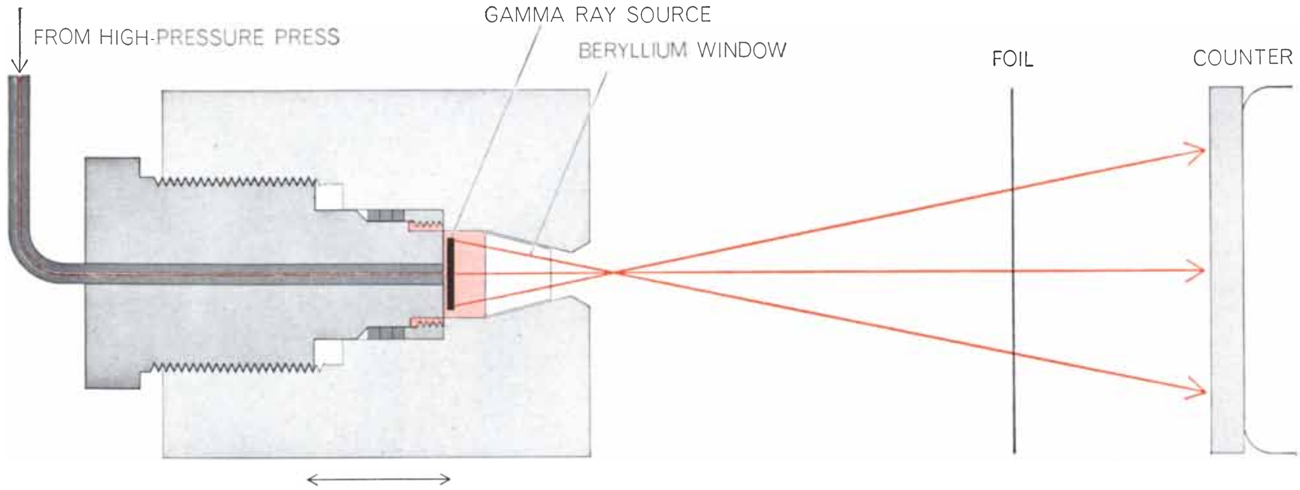
To be sure, the effect we must measure is extremely small. Calculations show that the isomer shift produced by all the atomic electrons is 100 million times smaller than the purely nuclear effect. Furthermore, we must remember that of all the electrons the outermost electrons will be most affected by pressure. The frequency of a 14.4-kilovolt gamma ray is $3 \times 10^{18}$ cycles per second; to this frequency the outermost electrons contribute only $1.5 \times 10^{7}$ cycles per second. We must therefore be prepared to detect a change in the energy of the gamma ray that is considerably smaller than a million-millionth of the total energy.

Two related but separate phenomena make such detection possible. The first is that in terms of energy the excited state of iron 57 is extremely well defined; indeed, the 14.4-kilovolt gamma radiation emitted by the isotope is among the most monochromatic electromagnetic radiations known. The second phenomenon was observed by Rudolf Mössbauer in his discovery of the Mössbauer effect. He found that some of the iron-57 nuclei in a solid did not recoil when they emitted gamma rays; the momentum of recoil was taken up by the solid as a whole. Since recoil would broaden the narrow spectral line of gamma ray wavelengths through the Doppler effect, its absence meant a line of unprecedented sharpness.

Any change in the frequency of the monochromatic iron-57 gamma ray can be detected by a simple and elegant trick. If the radiation falls on an absorbing foil consisting of iron-57 atoms whose nuclei are in the ground state, and if the radiation from the source has precisely the right frequency to cause the nuclei in the absorber to make the transition to the excited state, then the incident radiation will be strongly absorbed and few gamma rays will pass through the foil. If, on the other hand, the gamma ray frequency is not exactly the same as the difference between the excited and the ground states, many




**COIL-AND-SAMPLE ASSEMBLY, devised by J. D. Litster, a graduate student at M.I.T., made it possible to compress iron particles (*colored dots*) to pressures as high as 65,000 atmospheres while simultaneously observing their nuclear magnetic resonance. Solid silver chloride (*light colored area*) is used for transmitting pressure to the powdered iron sample.**




**APPARATUS** illustrated here was employed by R. V. Pound, R. Drever and the author to study the effects of high pressure on the gamma ray emissions from an isotope of iron called iron 57. A sample of ordinary iron was first impregnated with the radioactive isotope cobalt 57, which decays to excited iron 57. The sample was then placed in a high-pressure chamber (*left*) with a window of beryllium, through which gamma rays emitted by the excited atoms could emerge and strike a sheet of foil (*center*) consisting of iron 57 in the ground state. Ordinarily the foil would absorb most of the gamma rays, but at high pressure their frequency would no longer be in resonance with the energy difference between the excited and the ground states, and they would therefore pass through the foil instead of being absorbed. To reestablish resonance in this changed situation the foil and the gamma-ray-emitting iron source were set in motion with respect to each other. At a certain velocity of the relative motion the rays will have the resonance frequency and will be absorbed by the foil. The velocity required to shift the gamma radiation to this resonance frequency thus gives a very precise measure of the amount of change in the energy of the emitted gamma rays that was produced by the high pressure in the chamber.

gamma rays will pass through the foil.

We thus have "resonant" absorption of the gamma ray. One can sweep across the resonance-absorption line simply by moving the source with respect to the absorber; the velocity of motion changes the apparent frequency of the source through the Doppler effect. By sweeping the velocity of the source with respect to the absorber one can sweep out the profile of the gamma ray spectral line by measuring the absorption of the gamma rays as it is related to the relative velocity of the source and the absorber. The maximum absorption will occur when the relative velocity of source and absorber is zero, but if the gamma ray energy of the source is somehow made different from that of the absorber, the resonant absorption will occur only when the relative velocity is different from zero. In fact, by measuring this velocity shift from zero one can measure with great precision any change in the energy of the gamma ray.

R. V. Pound of Harvard University has built equipment that is capable of measuring shifts in gamma ray energy as small as $10^{16}$ times less than the energy of the gamma ray. His equipment made possible the detection of any shift in the frequency of the gamma ray spectral line, even if that shift was only a thousandth as much as the width of the line. With this extraordinary apparatus Pound, R. Drever and I undertook to study the effect of pressure on the energy of the gamma rays from iron 57 in ferromagnetic iron. The change in energy of the gamma rays was measured as the pressure on the sample was increased to 3,000 atmospheres. We found that at this pressure the gamma ray frequency changed by about 23,000 cycles per second in the total frequency of $3 \times 10^{18}$ cycles per second. That is a change of a little less than a hundredth of a trillionth from the energy of gamma radiation emitted by excited iron atoms not under pressure.

From this shift and the other known data, including Bridgman's determinations of the compressibility of iron, it was possible to compute how much the total electron spin density (spin up *plus* spin down) at the nucleus of the iron atom changes with a given change in relative volume. Knowing this quantity, and the dependence of net spin density (spin up *minus* spin down) on volume, we were then able to calculate the dependence on volume of each spin density—spin up and spin down respectively. The experiments showed that on applying the pressure the spin-up and the spin-down density both increased by nearly the same amount. The fractional increase in the spin density was almost the same as the fractional decrease in the volume of the solid as a whole.

One can present the following theoretical picture as to how pressure might affect the electronic structure of iron atoms. When the volume available to the atom is reduced, the outer electrons are forced inward; this uniformly increases their charge density in the interior. In other words, there is an increase in the "probability density" of the electrons. The increase in density is inversely proportional to the decrease in the available volume. On this basis one would expect that the fractional change in spin density at the nucleus would be equal to the fractional decrease in volume produced by the pressure. This is just what we found experimentally. The experiment and theory together provide us with a simple picture of the changes in electronic structure accompanying the application of high pressure.

To sum up our studies of ferromagnetic iron at high pressure, we have used the atomic nucleus as a precise and sensitive probe of the magnetic and electric fields at the center of the atom. By observing the changes in these fields produced by pressure we can determine the concomitant changes in electronic structure. In this way we are aided in forming a clearer image of the distribution of electric charge and magnetic spin that are the sources of all the properties of the solid.

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# MATHEMATICAL GAMES

## *Some comments by Dr. Matrix on symmetries and reversals*

by Martin Gardner

Readers who saw the article in this space last January may recall that Dr. Matrix and his shapely daughter Iva Toshiyori—Iva uses the maiden name of her Japanese mother—had been appearing nightly at the Purple Hat Club in Chicago. So sensational were the numerologist's "readings" that he obtained a six-week booking at one of the plushier Las Vegas hotels. According to several well-authenticated reports the old swindler not only got a top price for his act but also managed to win $70,000 at the blackjack tables by playing his own modification of the system explained by the mathematician Edward O. Thorp in his eye-opening book *Beat the Dealer*. It is said that Miss Toshiyori helped her father by making surreptitious calculations on a small transistorized computer concealed in her handbag.

After closing in Las Vegas the pair flew to western Canada for several weeks of skiing. Then in early October I received from Dr. Matrix the cryptic number 13212 that, as I explained last month, predicted the name of the man who would be elected President. The letter was postmarked Zero, Mont. A postcard from Iva arrived the following week from Unityville, S.D. Her father had bought a Jaguar, she disclosed, and they were driving leisurely about the country on their way to Miami.

When Iva's second card came from Two Rivers, Wis., I guessed that the pair were on a winding route that would probably take them through towns with numerical names in ascending serial order. Cards followed from Triplet, Va., Four Oaks, N.C., Five Forks, W.Va., Six, W.Va., Seven Mile, Ohio, Eight Mile, Ala., Ninety Six, S.C., and Ten Mile, Tenn. The next communication, a telegram from Dr. Matrix decoding his prediction, arrived on the day after the election. They were staying at the Moral Rot Hotel in Miami Beach; would I care to pop down for a visit?

I would indeed. After consulting an atlas I was tempted to drive down, sending them postcards from Odd, Va., and Evensville, Tenn., but on second thought it did not seem worth the trouble. I took a plane.

At three that afternoon I found Dr. Matrix and Iva sitting in the hotel's Marquis de Sade Cocktail Room. Money had agreed with both of them. Dr. Matrix had put on weight. His bony cheeks had filled out, so that he now resembled less a green-eyed hawk than a green-eyed owl. In the lounge's dim light Iva looked younger and more tantalizing than ever.

"That prediction of yours," I said to Dr. Matrix, "leaves much to be desired. It's true that it yields 'L. B. J.' when applied to the Pledge of Allegiance, but one of my readers has just pointed out that if you partition the numerals 13-2-12 and then consult the 13th chapter of the King James Bible—that is, the 13th chapter of Genesis—verse two, you'll find that the 12th word is 'gold,' a clear allusion to Barry Goldwater."

Dr. Matrix blinked his eyes solemnly and Iva smiled faintly. "An astonishing coincidence," he said, "but I'm really not surprised. Improbabilities, you know, are extremely probable, and they always mean something. Did I ever mention before that in the Dewey decimal system for libraries the classification number for numerology is 133.335?"

I took pencil and paper from my jacket as I shook my head.

"If you add that number to its reversal, 533.331," Dr. Matrix continued, "the result is 666.666—a double form of the mark of the beast."

"That interests me," I said. "I've just had a book published called *The Ambidextrous Universe*. It's all about mirror reversals and left-right symmetries."

"I'm halfway through it. An interesting book, but I wish you had consulted me before you wrote that section on words, letters and numbers. I could have given you much better material."

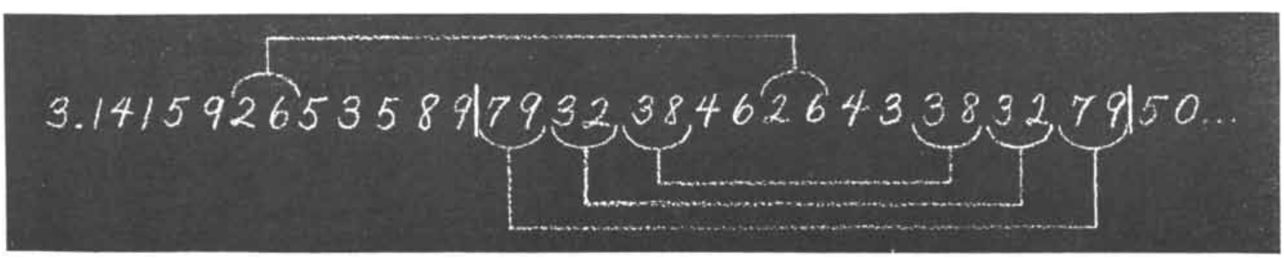
"Examples, please," I said, pencil poised.

Iva glanced at her wristwatch. "You boys must excuse me. I want to get in a swim before the sun's too low." She nodded in my direction. "See you at dinner, Nitram."

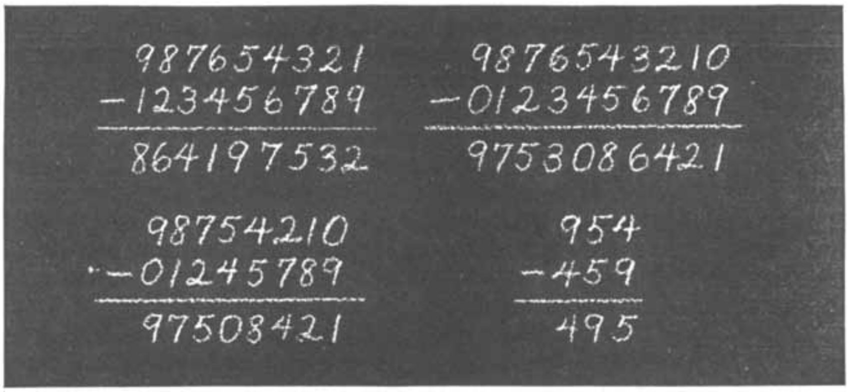
"Consider," Dr. Matrix went on after we had reseated ourselves, "the present international scene. Surely the great left-right split between the United States and the Soviet Union is mirrored by the fact that the initials of the two giants are left-right reflections: U.S. and S.U. And have you noticed that the famous B and K team of Bulge and Khrush that followed Stalin is echoed by the new B and K team of Brezh and Kos? Only now the order is reversed—the B, not the K, is on top."

Dr. Matrix borrowed my pencil and rapidly jotted down the value of pi to 32 decimals [*see illustration below*]. "Mathematicians consider the decimal expansion of pi a random series, but to a modern numerologist it's rich with remarkable patterns."

He bracketed the two appearances of 26. "Twenty-six, you observe, is the first two-digit number to repeat. Note how the second 26 marks the center of a bilaterally symmetrical series." Dr. Ma-




***A curious reflection pattern in pi***




*Four sets of distinct digits that are self-replicating*

trix inserted vertical bars to enclose 18 numerals, then bracketed six other number pairs shown in the illustration. The number pairs 79, 32 and 38 on the left are balanced by the same three pairs in reverse order on the right! He also called attention to the sets of five digits on each side of the first 26. The first set sums to 20, the number of decimals preceding the second 26. The second set sums to 30, the number of decimals preceding the second bar. Together they sum to 50, the two-digit number following the last bar. The sequence between bars starts with the 13th decimal and 13 is half of 26. The three pairs–79, 32 and 38–have six digits that sum to 32, the pair in the middle as well as the total number of decimals shown. The 46 and 43 on each side of the second 26 sum to 89, the number pair preceding the first bar.

"I could talk for hours about that crucial number 32," Dr. Matrix continued. "It's one of the ubiquitous constants of nature. An object falling to the earth accelerates 32 feet per second per second. Water freezes at 32 degrees Fahrenheit. There are 32 crystal classes. Man has 32 teeth. There are 32 electrons in the filled fourth energy level of atoms. There are 32 fundamental, long-lived particles. And so on. Of course 32 is 2 raised to the power obtained by adding 3 and 2."

"And there are 32 counties in Ireland," I put in. "That's one reason why James Joyce, in *Finnegans Wake,* uses 32 as a symbol for the fall of Finnegan."

"Someday," said Dr. Matrix, "I intend to write a commentary on Joyce's number symbolism. But back to pi. I wish I had time to go into the subtler properties and the historical significance of those first 32 decimals. Let me say, though, that 62–64 in the center of that series between the bars indicates the three eventful years that have just passed as the world moves from 33 on the right, the year Hitler became chancellor, to George Orwell's 84 on the left. Correctly interpreted, you know, pi conveys the history of the human race."

"Do you have," I asked, shifting the topic purposely, "any left-right reversal items that might provide puzzles?"

"Thousands," sighed Dr. Matrix. "Consider for a moment the digits from 1 to 9. Arrange them in descending order, reverse and subtract [*see illustration above*]. The result is quite unexpected. The same nine digits reappear in the answer."

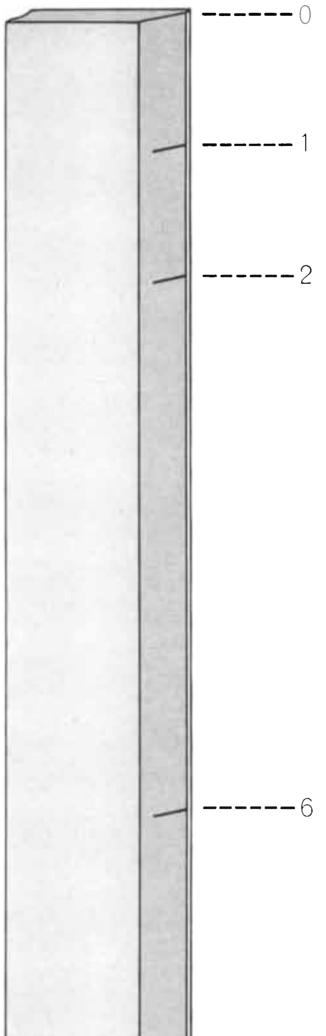
"I've seen that before," I said, "in medieval books on numerology."

"Of course," replied Dr. Matrix. "I bring it up because few recreational mathematicians know the other examples of what I call 'self-replicating sets' of distinct digits."

The problem, as Dr. Matrix explained in more detail than I can give here, is that of finding a set of $n$ digits, no two alike, such that when they are arranged in descending order and reversed and the new number is taken from the old, the same $n$ digits reappear in the result. No examples exist, Dr. Matrix assured me, for sets of one, two, five, six or seven digits. For three digits the unique example is 954. For eight digits 9875-4210 is unique. The example of nine digits given above is also unique, and for 10 digits 9876543210 is obviously the only case. For four digits there is again only one example. Can the reader discover it?

"By the way," Dr. Matrix added, "your readers might enjoy *dividing* 987-654321 by 123456789. It's hard to believe, but the answer is 8.00000007+, seven decimal zeros followed by 7. A pity the quotient isn't exactly 8, but that's the way things are sometimes, in numerology as well as physics. Let's go up to my suite. It's so dark here I can hardly see what I'm writing."

Dr. Matrix paid the tab with a gener-




*Dr. Matrix' 13-inch ruler*

ous tip, and we took an elevator to his rooms on the top floor. "What's *your* room number?" he asked as we made ourselves comfortable. After I told him he closed his eyes for a minute, then opened them suddenly and said: "A most unusual number. It's the only three-digit number with the following property: multiply it by a certain digit and the result is the reverse of what you get if you add that same digit to it instead."

"I'm writing all this up for January," I said. "I'll include that and give the answer in February. Any puzzles involving the number of the new year?"

"I assumed you'd ask me that," Dr. Matrix answered with one of his rare, crooked smiles. "You recall that in your department for October, 1962, you asked readers to insert plus or minus signs wherever they pleased inside the series 123456789 and in the reverse series 987654321 so that in each case the series totaled 100?"

I nodded. "And in the 'Letters' department for January, 1963, we printed computer results giving 11 different solutions for the ascending series, 15 for the descending."

"To complete the record," Dr. Matrix said, "if a minus sign is allowed in front of the first digit, there are three more answers for the descending series and one unique answer for the ascending. [For the interested reader these are: $-9+8+76+5-4+3+21$; $-9+8+7+65-4+32+1$; $-9-8+76-5+43+2+1$; $-1+2-3+4+5+6+78+9$.] You might ask your readers to see if they can insert five signs within the ascending series to make a total of 65, the new year. Five is the smallest number of signs that will do it, and there's only one answer, even if a minus sign is permitted in front of the 1."

"How about the descending series?"

"If no minus sign is allowed in front, the minimum number of signs required to make 65 is six, with five different solutions. They're of no special interest. But if a minus sign in front of the 9 *is* permitted, there's another unique five-sign solution. Your readers might enjoy looking for that also."

"I'm sure many will," I said. "But let's be realistic. All the problems you've mentioned so far are relatively trivial. Can you give me something with a bit more substance?"

Dr. Matrix stood up, walked over to a desk and returned with what appeared to be a silver ruler. When I examined it, I saw that it bore only four marks [*see illustration at left*]. "An old friend in Tokyo sent me this," said Dr. Matrix. "It's 13 inches long and the marks are placed so that one can measure exactly any integral length of inches from 1 through 13."

I studied the ruler. "I see what you mean. From 0 to 1 measures one inch. From 0 to 2 measures two. Three can be measured by the 10 and 13 marks, four by the 6 and 10 marks, and so on."

Three marks, Dr. Matrix told me, are sufficient for a nine-inch rod if one wishes to measure, in one step, any integral length from one to nine inches. But on a 12-inch ruler four marks are required for measuring lengths from one to 12. Can the reader find a suitable way of placing the four marks on a 12-inch rod, and prove that three are not enough? And what is the maximum number of different integral lengths that three marks, suitably placed on a 12-inch rod, *will* measure?

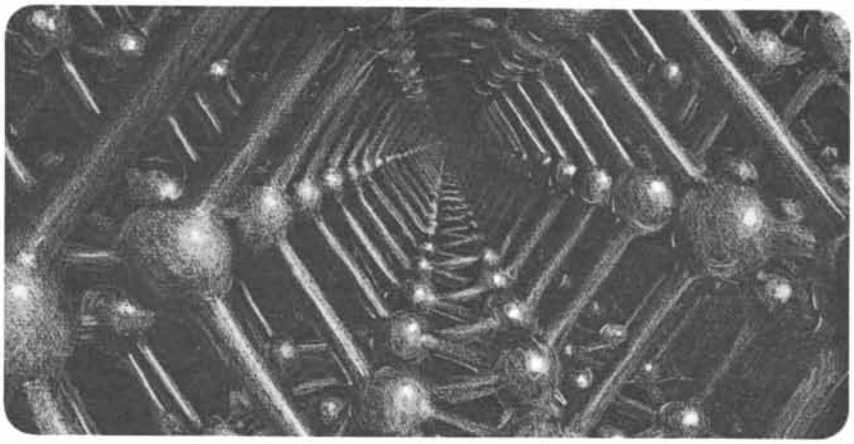
A more difficult question: On a 33-inch stick, what is the smallest number of marks that permit measuring all integral lengths from one through 33? Show how the marks should be placed.

These questions will be answered in this department next month, but the general problem of finding a formula or procedure for obtaining the minimal markings of rods of any length, as far as Dr. Matrix knows, is still unsolved. Many simple questions can be asked that are difficult to answer. For instance, although nine marks are necessary on a yardstick for measuring from one to 36, nine marks can be placed on a longer rod so that more than 36 lengths are measurable. If nine marks are placed on a stick of any size, what is the maximum number of consecutive integral lengths, starting with one, that the stick will measure? What is the maximum number of different lengths, not necessarily consecutive, and the shortest stick that allows measuring that number? I do not know the answers to these and similar questions.

Dr. Matrix and I were discussing various combinatorial approaches to the ruler problem when Iva came into the room wearing a bright orange bikini. I stopped thinking about combinatorial arithmetic to concentrate my attention on the dynamics of oscillating solids.

[Thanks to John Bales, University of Texas, for the Dewey decimal item; Donald C. Rehkopf, English teacher in an Oak Park, Ill., high school, for the pi pattern; Martin Cohen, Beverly Hills, Calif., for the division of 987654321 by its reversal, and Don T. Hastings, a Detroit engineer, for problems involving the insertion of signs in the nine-digit

FREEMAN BOOKS IN SCIENCE . . .



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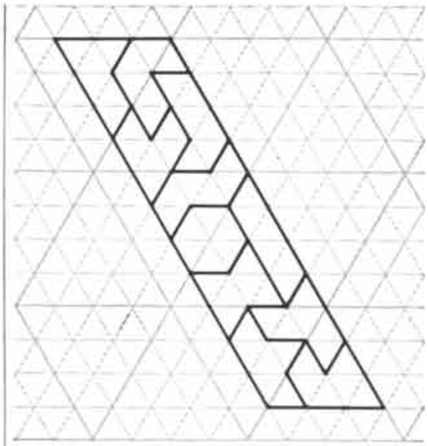

*Forming the 3-by-11 rhomboid*

series. Using only a desk adding machine and the help of his secretary, Hastings recently finished the incredible task of tabulating the sums of all possible ways of adding plus and minus signs to both the ascending and the descending series.]

Last month's hexiamond problems are answered above and below. The 3-by-11 rhomboid can be formed in more than one way, but no pattern has yet been found that does not exclude the piece called the bat. The star's solution is believed to be unique except for rotations and reflections.

In this department for November it was erroneously stated that the harmonic series does not sum to 100 until it has more than $2^{199}$ terms. This should have been $2^{100}$, which was the figure used by William Ransom in the cited calculation. Daniel Asimov, a student at the Massachusetts Institute of Technology, has pointed out the error and has also calculated the exact point in the power series at which the sum of the harmonic series passes 100. At $2^{143}$ terms the sum is below 100; at $2^{144}$ it exceeds 100.

*Only known solution for the star*

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# THE AMATEUR SCIENTIST

## *On a paradoxical pump, reversing cubes, vortex rings and various other matters*

Conducted by C. L. Stong

On the theory that even a gourmet welcomes an occasional potluck meal, this department again presents a potpourri of easy experiments. Some are new and some are not. They cost little and make no great demand on craftsmanship. Aside from their entertainment value, their chief virtue may be that they provide an excellent excuse for putting off odd jobs that need to be done around the house.

The first experiment comes from George Stibitz, a consultant in applied mathematics who lives in Potter Place, N.H. "It is well known," writes Stibitz, "that the principles governing the transmission of vibrations through elastic tubes also apply in the case of the transmission of signals on telephone lines. Among these principles is the familiar one that only those frequencies present in the driving function can be found in the propagated signal. No direct current will appear in the line, for example, unless direct current is developed by the generator.

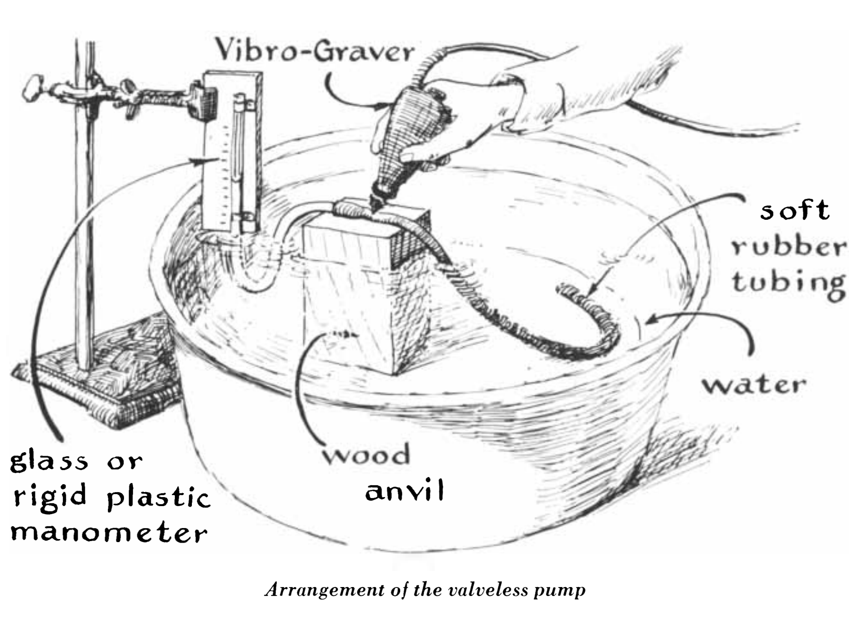
"The validity of this principle can be investigated by an easily assembled hydraulic analogy. First join a length of rubber tubing approximately a quarter of an inch in diameter to a length of clear tubing of glass or rigid plastic. The soft tubing can be a foot long. Heat the hard tubing at a point a foot from the joint and bend it into a right angle. Next place a heavy block of wood in the center of a dishpan so that the top of the block extends about an inch above the edge of the pan. Place a vertical support, such as an apparatus stand, near the edge of the pan and attach the free end of the hard tubing to the support as shown in the accompanying illustration [*below*].

"The support assembly can be calibrated to serve as a manometer for indicating water pressure. Fill both the tubing and the pan with water and rest the joint of the tubing on the block as shown. Now provide some means for vibrating the soft tubing near the point at which it joins the hard tubing. I use a Vibro-Graver as the power source. The block serves as an anvil. When the vibrator goes into action, it is obvious that the water in both tubes must oscillate around fixed positions because the system contains no valves. It is also obvious that the average pressure in the tubes must be zero and that no steady flow could occur.

"To test this assumption turn on the vibrator and observe the manometer. A word of caution: Unless you have made the manometer tube several feet tall be sure to stand back or you will get wet! A steady stream of water will shoot from the orifice at an average pressure of several pounds per square inch.

"A startling fact is that this system acts like a pump with valves of some 200 percent efficiency, whereas if there were valves, none of them could be expected to perform with even 100 percent efficiency. In the case of my own apparatus I have measured the 'stroke' of the vibrating element; that is, I worked the vibrator by hand and measured the resulting displacement of water, which is about .1 cubic centimeter per stroke. The vibrator runs at 120 cycles per second. With perfect valves the output should amount to 12 cubic centimeters per second, or 720 cubic centimeters per minute. An actual run produces some 1,500 cubic centimeters per minute. If the vibrator is moved along the soft tube, a point can be found at which the direction of the pumping reverses. Still further displacement of the vibrator in the same direction restores the flow to its original direction but at a lower volume.

"An explanation of the pump's paradoxical behavior is found in the phase relations of the vibrations in the two sections of tubing. The rigidity of the system is not symmetrical around the point of vibration. For this reason water in the hard tube does not necessarily vibrate in step with that in the soft tube, and a net longitudinal vibration appears at the point where the impulses are applied. Synchronized with this longitudinal vibration is a variation in resistance to flow that is set up by the deformation of the soft tubing. When pressure is ap-




*Arrangement of the valveless pump*

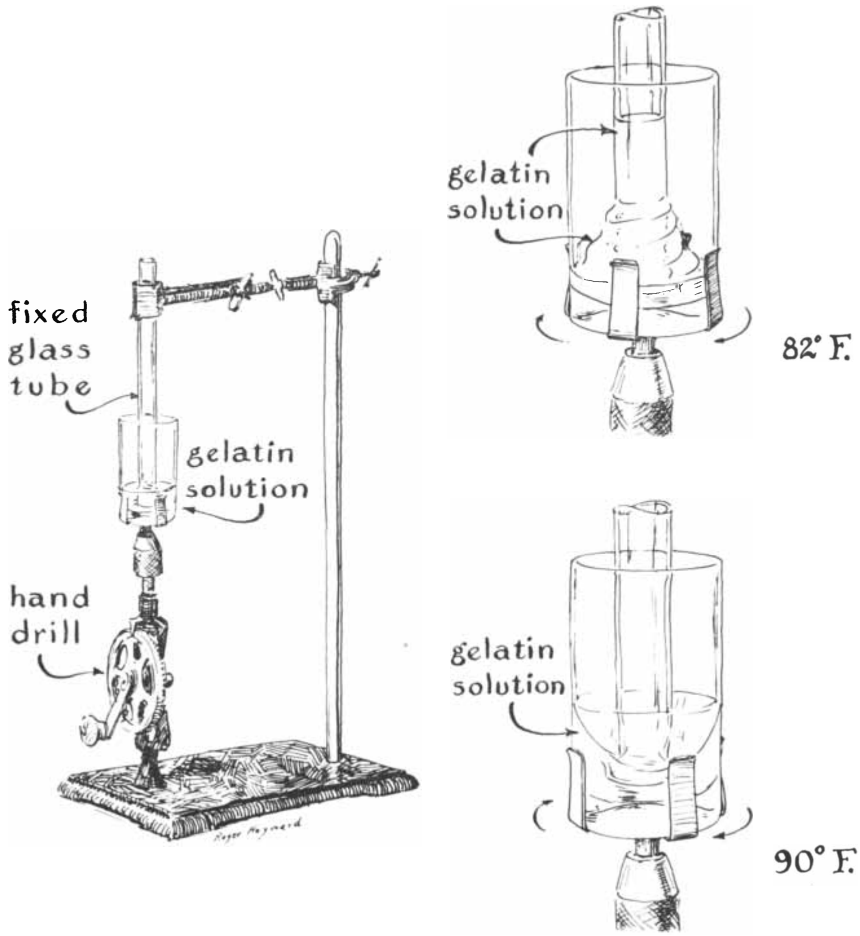
plied at the proper point, the soft tube is squeezed during that portion of the cycle when the water is flowing from the hard tube into the soft one, and the soft tube is expanded when the water is flowing in the other direction. The combined actions generate a unidirectional flow.

"The effect caught my eye quite by chance when I was working on a pneumatic pump I invented for producing a flow of blood in superficial arteries. I spent two weeks finding out why the effect occurs. Mathematically there appears to be no limit to its efficiency. By 'tuning' the vibrator a little—varying both the pressure and the point at which vibrations are applied to the soft tube—one can doubtless make the system perform at higher efficiencies than I have observed."

James H. Wiegand of Sacramento, Calif., calls attention to the equally strange behavior of high-polymer fluids subjected to a shearing force. A second force develops at right angles to the shearing force; this phenomenon is known as the Weissenberg effect. According to Wiegand, the second force arises from the tendency of the giant molecules constituting the fluid to return to their normal orientation when they are deranged by the force of shear. The effect can be demonstrated by an apparatus Wiegand described in the *Journal of Chemical Education* for September, 1963.

Wiegand clamps a vessel such as a drinking glass in a metal fixture that is in turn held in the chuck of a hand drill supported vertically by an apparatus stand. When the drill is turned, the carefully centered glass spins in the normal, upright position around its vertical axis. A stationary glass tube about two centimeters in diameter is also clamped to the stand so that it extends coaxially down into the glass to within a few millimeters of the bottom [*see illustration on this page*].

For the high-polymer solution Wiegand dissolves seven grams of unflavored Knox gelatin in 35 milliliters of water heated to about 130 degrees but not to more than 140 degrees Fahrenheit. When the gelatin has dissolved and the solution has cooled to about 90 degrees, the mixture is transferred to the drinking glass. If the glass is now rotated, the fluid will behave in the expected manner: it will climb the wall of the glass as the surface assumes a paraboloidal shape. When the fluid has cooled to about 86 degrees, a remarkable effect begins to appear. A portion of the fluid climbs the outer surface of the stationary tube and simultaneously rises inside the tube at the expense of fluid adhering to the wall of the rotating glass. The effect becomes more pronounced as the temperature continues to drop. Finally the column inside the tube grows by the upward movement of the liquid from the bottom of the glass. If rotation is continued until the contents have chilled to the gel point (82 degrees F.), the center tube becomes solidly plugged. The plug can be removed easily by warming the glass. Other fluids can also be used to demonstrate the phenomenon, including polyisobutylene and the thick fraction of egg white. Wiegand prefers gelatin, primarily because of its availability and ease of preparation.

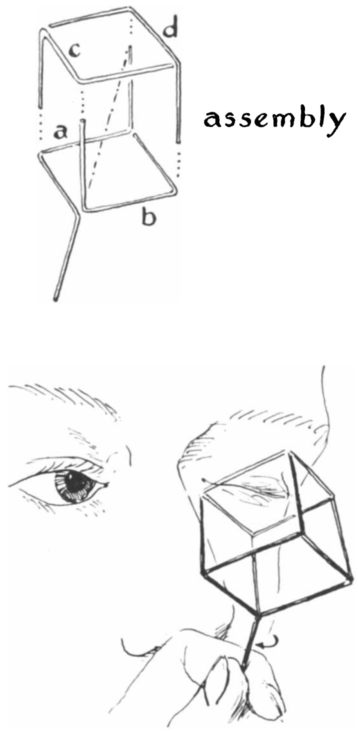



*Apparatus for demonstrating the Weissenberg effect*

Not all paradoxical effects such as those represented by the valveless pump and high-polymer fluids that seem to defy gravity are "real" in the sense that physical forces account for the observed behavior. Some must be ascribed to faulty perception. An example is submitted by Arthur Schlang of Mineola, N.Y. It involves an interesting three-dimensional variation of the familiar reversing-cube illusion.

In its commonest form the illusion consists of a perspective drawing of a cube made by connecting the adjacent corners of two overlapping squares by four straight lines. As one looks at either one or the other of the two points within the figure where three lines meet, the perspective of the cube seems to change. Usually the eyes must come to rest for a few seconds on one or the other of the points before the illusion of reversal occurs. Schlang constructs a real cube of wire, with a short length attached at one corner to serve as a handle; it is aligned with the diagonal between the near and the far corner of the cube [*see top illustration on next page*]. The wires are soldered together at the corners.

To see the illusion hold the handle between the forefinger and the thumb of one hand with the cube at the normal reading distance and, with one eye closed, look at the far corner of the




*Details of the wire cube*

cube. Within a matter of seconds the orientation of the cube will appear to reverse, as in the case of the perspective drawing. When the reversal occurs, roll the handle slowly between finger and thumb. The cube will appear to turn backward! Open the closed eye. The cube will instantly snap back to its true orientation.

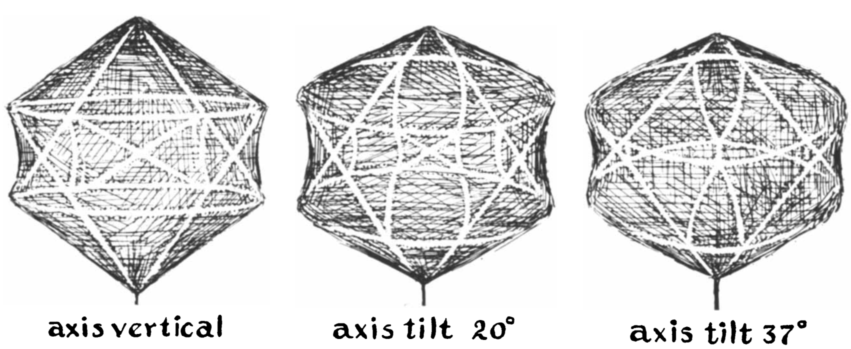
Now hold the handle vertically with the cube on top. Again close one eye and fix attention on the far corner. When the illusion of reversal occurs, incline the cube away from you until the handle is horizontal. During this movement the wire handle will appear to bend at the point at which it is attached to the cube and the cube will swing upward until it seems to perch on one corner at the tip of the handle. Roll the handle. Now the cube appears not only to turn in the wrong direction but also to rotate on its vertical axis as if driven by the handle through a pair of crown gears, which change the angle of drive by 90 degrees. One can also equip the cube with a "synchronous satellite." Place a small object such as a cork on a wire and attach the wire to the handle of the cube so that the cork is about an inch above the equator of the cube (assuming the south pole to be the corner to which the handle is attached). When the illusion of reversal occurs and the cube is rotated, the cube and its satellite will appear to move in opposite directions. A little experimentation will disclose a number of other unexpected effects, including a set of interlocking curves that assume various forms when the unit is turned rapidly and viewed from different angles. The curves show up best if the cube is made of polished wire.

The experiment strikingly discloses a role of binocular vision in the perception of shape, position and distance. Under most circumstances the geometry of a scene can be judged adequately by one eye, which reports the relative sizes of objects, their characteristic patterns of light and shade and their relative apparent motions as well as variations in the intensity of lighting between near and far objects. For this reason a one-eyed person is not impossibly handicapped when viewing most objects and can place a considerable amount of confidence in his visual perceptions. A three-dimensional object that is depicted by a perspective drawing in two dimensions is really a monocular representation and appears practically the same whether viewed by one eye or two. That monocular vision does not always convey enough information about the real world for accurate perception, however, is indicated by the fact that the illusion of reversal ordinarily does not appear when the wire cube is viewed by both eyes. (A few people find, incidentally, that they cannot achieve the reversal even with one eye.)

Many seeming paradoxes would vanish if we were endowed with sharper, more reliable eyes. As matters stand the experimenter must often base his conclusions on indirect evidence. Students of the flight of birds long wondered, for example, how large birds such as turkey buzzards manage to fly effortlessly above unbroken plains on sunny days of flat calm. Ultimately, by using the visible motion of the birds as indirect clues to air movement, some observers concluded that the birds were carried aloft by vortex rings. These rings, which can attain quite large dimensions, form when local masses of heated air in contact with the surface detach and float upward, much as bubbles originate in a pan of gently boiling water. To stay aloft soaring birds merely circle in the local updraft that constitutes the core of the vortex ring [see "The Soaring Flight of Birds," by Clarence D. Cone, Jr.; SCIENTIFIC AMERICAN, April, 1962]. Although such rings can ascend at a rate of many feet per minute, their movement has little effect on the neighboring air. In many respects they behave like solid objects. Even when projected at high velocity through still air, however, they do not create a breeze.

These properties can be investigated with a simple vortex-ring generator that has been improvised from a coffee can by Tom Clements of Hightstown, N.J. He uses a can of the type that comes with a polyethylene top. A centered hole about an inch in diameter is cut in the bottom of the can [*see top illustration on opposite page*]. Vortex rings are projected from the hole by tapping the plastic top. The rings can be made visible by filling the can with smoke. (To make a generator for producing smoke, heat until soft one end of a glass tube into which a cigarette will slide easily and draw the glass to a gentle taper. Light the cigarette and drop it into the tube so that the unlighted part seats against the taper. Blow into the large end of the tube.)

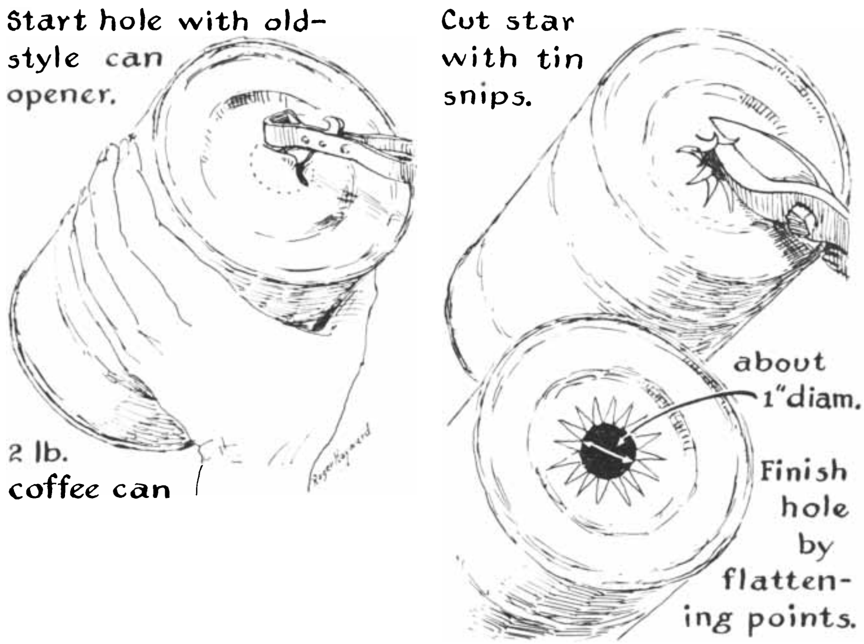
To demonstrate how vortex rings propagate through still air, make a detector by constructing a grid of fine silk threads suspended from a horizontal bar. A grid a foot square with threads at half-inch intervals works well. Shoot vortex rings at the grid at distances ranging from one foot to 10 feet. Observe how all threads remain undis-




*Curves generated by the wire cube*

turbed except those directly in the path of the ring. Observe also how the ring expands comparatively little during its flight. Note how the rolling motion of the vortex ring picks up air in front of it, pushes it aside and finally deposits it relatively undisturbed in its wake as if the ring were a fully streamlined body.

The energy of a vortex ring can be demonstrated by shooting a ring at a lighted candle. Even a comparatively small ring will blow out the flame at a distance of a few feet; large ones will break soap bubbles 15 feet away. To prove that the observed properties of the rings do not arise in the smoke, repeat the experiments without smoke. How do vortex rings behave when they strike obstructions such as hard walls? What happens when two collide head on? The answers to such questions are left to the ingenuity of the experimenter [see "Quantized Vortex Rings in Superfluid Helium," by F. Reif; SCIENTIFIC AMERICAN, December, 1964].

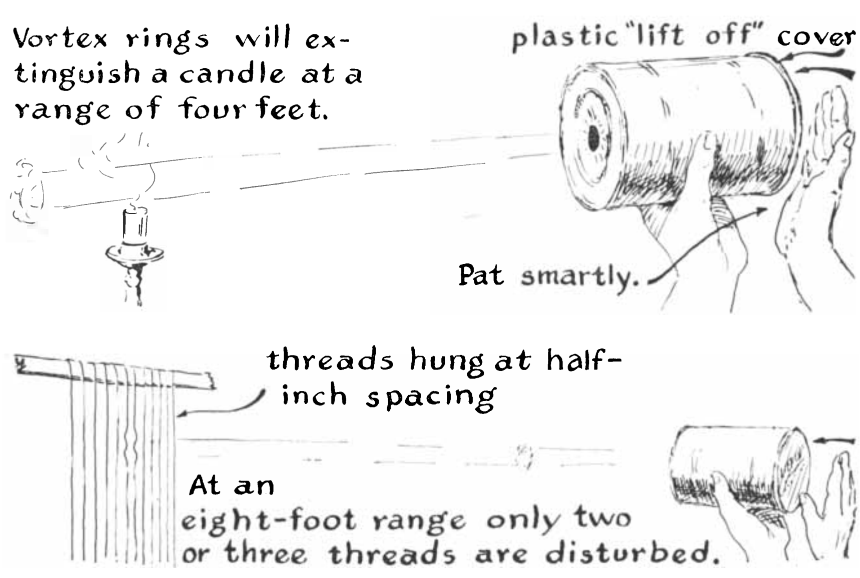


*Preparation of can to produce vortex rings*

Jets of air also exhibit properties on which interesting experiments have been based. Shortly after the introduction of gas lighting in the 19th century, for example, musicians observed that sustained notes of certain pitch, such as those in the upper register of the cello, would cause the lights to dim! Eventually, after much investigation by such British physicists as John Tyndall and Lord Rayleigh, the cause was traced to the instability induced by sound waves in the laminar flow of gas constituting the jet of the burner. By carefully regulating the gas pressure they found it possible to produce jets that would resume laminar flow at the end of a disturbance. With such jets they made an apparatus for investigating the shape of sound waves. It was a primitive but surprisingly effective counterpart of the modern oscilloscope.

Burners for generating these "sensitive" flames can take many forms. I make one version by heating to softness one end of a glass tube roughly eight millimeters in diameter and pulling it into a hairlike capillary. A nozzle is then formed by breaking the capillary at a point about .005 inch in diameter. I use gas from the tank of a propane torch and adjust the pressure for a flame about two centimeters high that floats some 10 centimeters above the nozzle. As part of the adjustment procedure it is usually necessary to increase the diameter of the nozzle by the cut-and-try method of snipping off additional bits of glass. A properly adjusted flame is blue at the bottom and tipped with yellow. Sustained sounds of 400 cycles and higher, such as the *A* note above middle *C* on the piano, cause the flame to drop about two centimeters toward the nozzle. A sharp sound, such as a click, will usually blow out the flame. Even a snap of the fingers will do so at distances of up to 30 feet.

The instantaneous response of sensitive flames can be demonstrated by observing the flame as reflected by a moving mirror. The mirror provides the equivalent of the horizontal sweep of an oscilloscope. In one arrangement a mirror of the size of those used in automobiles is suspended from its upper corners by a pair of threads and set into wobbling vibration around its vertical axis with a push on one end. The flame is seen in the mirror as a ribbon of reflected light. When a sustained sound is projected against the base of the flame, the sound waves appear as undulations along the upper edge of the ribbon. A more convenient horizontal sweep can be made by mounting a set of eight pocket mirrors around the periphery of a wooden disk that can be continuously rotated on a centered ver-



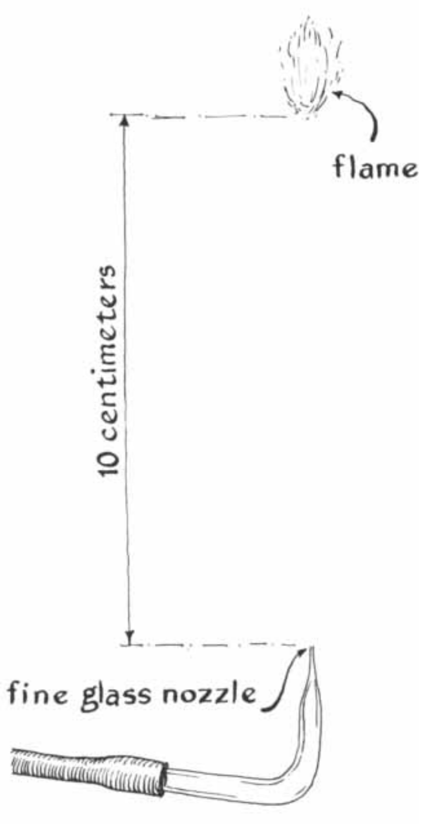
*Effects of vortex rings*

tical shaft. The mirrors successively sweep the image of the flame across the field of view in the direction of the rotation.

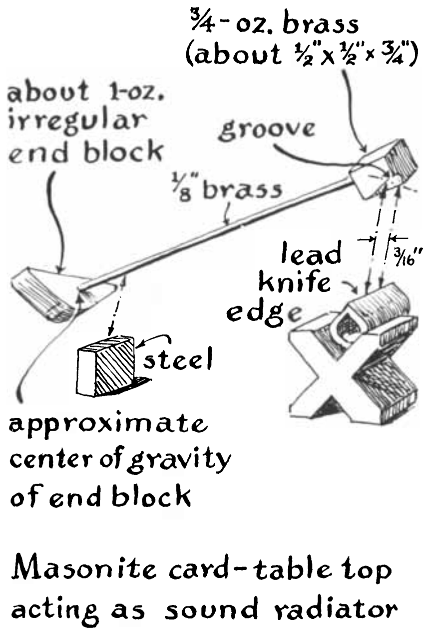
Sensitive flames can also be made to function as oscillators for generating sound waves. For these experiments I use a nozzle about a millimeter in diameter and adjust the jet so that it is on the verge of instability—the point at which the flame changes from the smooth shape of a candle flame to one that flares and roars. If a pipe about two inches in diameter and two feet or more long is now lowered over the flame, a point will be found at which the apparatus sings. The pitch of the tone depends on the distance between the flame and the end of the pipe. The sound is initiated by some random event that triggers instability. The resulting compression wave then traverses the pipe and is reflected back to the jet from the end, initiating the next cycle.

Another interesting oscillator, also powered by heat, is known as Trevelyan's rocker. It consists of a specially shaped block of hot metal in contact with a thin strip of lead. The combination will sing a minute or longer if the block has been heated close to the melting point of lead.

A version of the apparatus made by




*Nozzle-and-flame arrangement*




*Details of Trevelyan's rocker*

Roger Hayward, who illustrates this department, consists of a rectangular block of brass about half an inch square and three-quarters of an inch long. A groove is filed in one of the corners to form a pair of knife edges spaced three-sixteenths of an inch apart, as shown in the accompanying illustration [*above*]. The other end of the block is drilled for a push fit with a brass rod six inches long and an eighth of an inch in diameter. A second block of any metal is similarly drilled for a push fit with the other end of the rod. The lead block must have a single knife edge.

When the knife edges of the heated brass block are placed at right angles across the knife edge of the lead block, the brass-block assembly promptly goes into torsional vibration and emits continuous sound at about 100 cycles per second. The vibrations are generated primarily by the lead. It is almost impossible to place the brass on the lead so that each of the two brass edges bears equal weight. Therefore initially one brass edge transmits more heat to the lead than the other. The lead expands with enough violence to rotate the brass so that somewhat greater pressure is then exerted between the other brass edge and the lead. This volume of lead now absorbs heat at an accelerated rate and expands, rocking the brass in the other direction and initiating the next cycle. The rate of vibration is established by the natural resonance in torsion of the masses that are coupled through the elastic rod.

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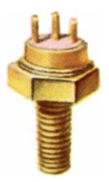

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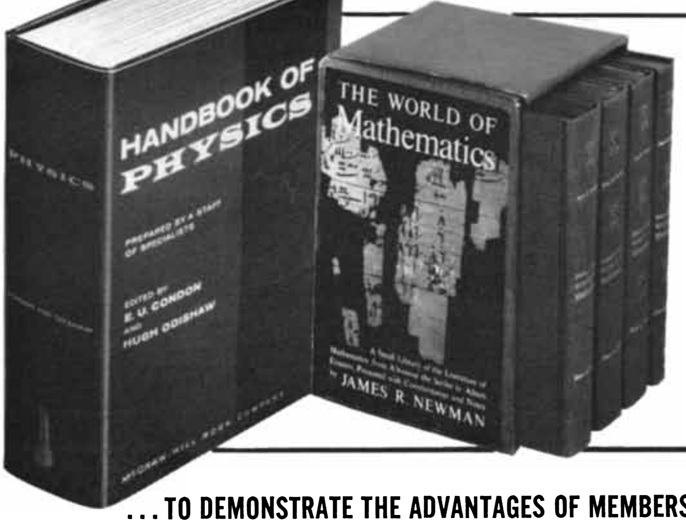


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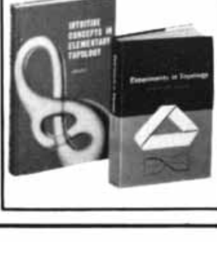

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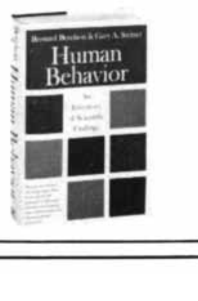

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THE MAKING OF THE ENGLISH WORKING CLASS, by E. P. Thompson. Pantheon Books; a Division of Random House ($15).

When one wishes to examine the responses of human societies to industrialization, two kinds of studies are valuable: detailed empirical investigations of existing communities and broad historical surveys of crucial periods of social transformation. The relation between techniques and values, which assumes importance in any debate about economic development, is deservedly attracting the attention not only of sociologists and anthropologists but also of a new school of social historians. E. P. Thompson's massive volume is a landmark in historiography. Based on highly varied source materials and informed by a lively and powerful historical imagination, it deals with what is perhaps the most crucial period in the history of industrialization: the first few decades of the first industrial revolution—the industrial revolution that took place in Britain in the late 18th and early 19th centuries.

It was during the 1780's that the British economy made its great leap ahead. New techniques, new forms of production and new social relations were all closely associated with one another. People were conscious of a deep transformation. "The general diffusion of manufactures throughout a country," wrote Robert Owen, the advocate of controlled social change, "generates a new character in its inhabitants... an essential change in the general character of the mass of the people." W. Cooke Taylor, who welcomed spontaneous change, said much the same: "The manufacturing population is not new in its formation alone: it is new in its habits of thought and action, which have been formed by the circumstances of its condition, with little instruction, and less guidance, from external sources."

As part of the transformation a new social vocabulary emerged—the vocabulary of class. Before then men had talked of "rank," "order" and "degree," of "middling folk" and "common people." The term "working class," which had been preceded by "upper class" and "middle class," was coined only in the early 19th century. The new terms had an intense emotional charge. They also implied experiments in social organization. Before long it was difficult to avoid an analysis of society in class terms. "They revolve in their eternal circle of landlords, capitalists and labourers," wrote John Stuart Mill in 1834, "until they seem to think of the distinction of society into those three classes as if it were one of God's ordinances not man's, and as little under human control as the division of day or night."

Thompson seeks to show how a working class emerged in Britain: a class with a distinct sense of group identity and distinct modes of behavior. He also seeks to show what the general social implications of the emergence of this class were. He stops short, however, on the eve of Chartism, the first national movement of working-class protest, and, unlike many earlier writers, he goes back in time long before the invention of the steam engine and the leap in the volume of industrial production. He chooses to begin his biography of the working class with an account of the preindustrial inheritance that influenced its formation and early attitudes. Although he is interested in social semantics, he is more interested in the social experience that lay behind the coining of words.

There is a contrast here between recent writing and the picture presented by Friedrich Engels in his famous *Condition of the Working-Class in England in 1844*. Engels wrote about preindustrial society in lyrical rather than in analytical vein, dwelling on the "idyllic simplicity and innocence" of social relations, "the leisure for healthful work in garden or field" and even "the piety and the probity" ordinary people displayed. The only penalty—and to Karl Marx and Engels it was the most serious of all penalties—was "vegetation." "Intellectually," wrote Engels, "they [the people of the preindustrial villages] were dead; lived only for their petty private interest, for their looms and gardens, and knew nothing of the mighty movement which, beyond their horizon, was sweeping through mankind." Thompson is unhappy about this kind of generalization, although he considers it less dangerous than many other generalizations. He wants to understand the preindustrial world, not to exploit it for purposes of polemical contrast. Fascinated as he is by class and by the industrial pressures of the 19th century, when the Marxist theory of class was formulated, he is equally fascinated by older views of society and by the 18th century, when there was far more to life, he suggests, than vegetation. He wants to test Owen and Cooke Taylor as well as Marx, to explore questions of continuity as well as of change, to see whether or not politics or religion before the advent of the steam engine influenced the way Englishmen, at least, thought and felt after it had been introduced into the factories.

For this reason the first part of his book is not really about economics at all. It is called "The Liberty Tree," and it deals with traditions and ideas that were still in the air when the first smoke rose from the chimneys of the new factory towns. He starts, in fact, not with Manchester but with preindustrial London, where there was a radical tradition before anyone talked of socialism. Radicalism expressed itself not only in formal statements of political theory but also in the actions of the mob and the popular disturbances that were generically called "riots." In this age—the age of William Hogarth and *Tom Jones*—society was stratified, but there were recognized areas of free expression, an implicit and sometimes an explicit challenge to "authority" and a proud sense of the Englishman's "birthright." As Thompson observes, "behind every form of popular direct action some legitimizing notion of right is to be found."

Thompson does not exhaust the themes he touches on in this section of his book. Rather he communicates his enthusiasm for getting to know more about them. He wants to fit Tyburn and its gallows into his picture as comprehensively as Manchester and its mills. He has an eye for "Brechtian values": the fatalism of the submerged sections of London society (a fatalism mitigated by belief in luck), the irony of the poor in face of the homilies of the Establishment (homilies that became more systematic under the influence of the Evangelicals and the Utilitarians) and the self-preservative tenacity of the weakest members of the social order. He wants to follow the example of French social historians such as Louis Chevalier by pursuing the phenomena of violence and crime.

Yet Thompson is quick to show that Britain was *sui generis*. There was a tradition of religious dissent in the provinces that was bound up with the pressure for liberty. Methodism, he suggests, was socially an ambivalent movement, a model for the makers of new organizations, particularly working-class organizations, but also torn from within by tensions between authoritarian and democratic tendencies. It could provide the "inner compulsion" that would enable the industrial discipline of the factory to work "naturally"; at the same time it could fire ordinary people to work together with a strong sense of community. Thompson may perhaps lean too heavily on evidence relating to the least attractive side of Methodism, but he recognizes that as the industrial system developed, religious stimuli and reactions were as much a part of the story as the facts of wages and profit. During the transitional years of social transformation, religious and secular egalitarianism could overlap as well as compete. So too could millennial visions, and Thompson is particularly vivid and illuminating in his brief account of this element in Protestant sectarianism.

The section ends, as it begins, with London in the early years of the French Revolution, with the dissemination of Jacobin ideas from London to the provinces and with Thomas Paine and the repressions under the younger Pitt. Thompson argues that in spite of all the differences in social framework (more might have been said about these differences) the English artisans who studied Paine with such intense interest were not unlike the Parisian sansculottes of 1793–1794. Just as there were revolutionary leaders in France who became involved in social revolution, so also were there English Jacobins, particularly John Thelwall, who even in a preindustrial context were on the threshold of passing from radicalism to socialism.

Taken as a whole this section has fascinating general bearings. It shows how the stamp of history affects social orientations in a period of industrial revolution. The official politics of the time had little to say about industry, unlike the official politics of developing countries today, yet even in these "new countries" it is necessary to examine the entire fabric of a society in order to understand reactions to technological change. Industrial changes always start from a social base line, or a series of such base lines. In Britain before the rise of factories there was always a vociferous demand for freedom, and enough zones of toleration in society for the demand to have a chance of expression. Other societies may start from quite a different point of departure. As a matter of fact the revolutionary agitation of the 1790's in Britain was suppressed just at the time when industrial progress was speeding up. Thompson writes: "England differed from other European nations in that the flood tide of counterrevolutionary feeling and discipline coincided with the flood tide of the Industrial Revolution; as new techniques and forms of industrial organisation advanced, so political and social rights receded."

Unfortunately at this point in Thompson's analysis there are omissions, even a falling back on clichés about "bourgeoisie" and "proletariat." The repression would not have succeeded if there had not been a popular element in Britain opposed to both France and the French Revolution. Before there was a clearly defined sense of class there had been a strongly felt sense of nation. Moreover, in the contemporary world there are many complexities associated with the very idea of "popular" pressure. Thompson never carefully examines the work of the political economists, who at that time were prophets of change, and he takes the attacks made on them at face value. The insight and warmth that are never missing in his account of working folk are seldom present when he looks at other groups in society. He has little appreciation of the values associated with "enterprise." In this sense there is running through his book a polemical element, which I am sure he would be the last person to deny. He implies, indeed, that if history is to be felt as well as studied, human feelings cannot be overlooked.

In the second section of his book Thompson turns to "The Curse of Adam," to the rich variety of the world of work and the bleak discipline that was brought into existence to direct and control it. The words "industry" and "work" could no longer be separated from each other. Again he looks for continuity: "Too much emphasis upon the newness of the cotton mills can lead to an underestimation of the continuity of political and cultural traditions in the making of working-class communities." In this quest for continuity he falls back on the idea of the industrial revolution "not as a settled social context but a phase of transition between two ways of life." In such a phase of transition there were bound to be disputes relating not only to the cost of living—these were to be basic to the logic of trade-union development—but also to issues centering on custom, status, justice, fairness, independence, rights and so on.

After surveying the fortunes and changing ways of life and work of different sections of what was to become the working class, Thompson seeks to place in proper perspective the prolonged historical controversy about whether workers became worse off or better off during the early years of the industrial revolution. He is less concerned with economics than with psychology and sociology, although he does have some pertinent points to make about economics; he notes, for example, that John H. Clapham, in his massive economic history of Britain, never mentions the fact that there was unemployment during the early industrial revolution.

In his interpretation of the psychology and sociology of the transitional phase, Thompson presents a significantly different analysis from that advanced by Neil Smelser in his difficult but rewarding study in applied sociological theory: *Social Change in the Industrial Revolution*. What Smelser, who was exclusively concerned with the cotton-textiles industry, described as "regressive" phenomena of disturbance figure in Thompson's analysis as necessary educative ingredients in the making of the unprecedented sense of class. Smelser examines strains and stresses within the value system of the prefactory family; Thompson looks at the growing pains of a new industrial system. The one seeks for evidence of dis-

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integration and functional readjustment, the other for evidence of latent power and for historical movement through unresolved conflict. These differences in analysis represent something more than the difference between "sociological imagination" and the application of a sociological model with sequential phases. They provide quite different answers to basic questions. If the two approaches were related not to past experience but to current diagnosis, they could lead to radically different interpretations of what is happening now.

There is, indeed, a remarkable, if oblique, topical relevance and aptness in Thompson's preoccupations. Although this topicality may raise difficult questions of historiography, it is an unqualified gain for the student of society. The debate on standards of living in the 18th and early 19th centuries tells us more about people now than about people then. It forces the intelligent reader to think not about 18th- and early 19th-century Britain but about 20th-century India. The fact that many of the questions posed in welfare controversies seem unanswerable does not rob them of their appeal.

In the third section of his book, where Thompson turns to the actual making of the sense of a working class, he remains provocative and challenging. In much the most sensitive account yet written of the Luddites, who wrecked the factory machinery they regarded as their enemy, he makes the reader think twice about the current use of the word "Luddite" as a term of unqualified contempt. Since it has become a cliché in the debate about economic progress, any critical reexamination of how the term first came into existence is of refreshing relevance.

To Thompson the Luddite movement was transitional. "We must see through the machine-breaking to the motives of the men who wielded the great hammers. As 'a movement of the *people's own*,' one is struck not so much by its backwardness as by its growing maturity. Far from being 'primitive' it exhibited, in Nottingham and Yorkshire, discipline and self-restraint of a high order. One can see Luddism as a manifestation of a working-class culture of greater independence and complexity than any known to the 18th century." The historical evidence Thompson collects and interprets, however, is sufficiently complex for readers to make up their own minds whether he goes too far in his reversal of accepted interpretations.

The same is true of an even bigger reversal of "orthodox" historical scholarship that Thompson proposes. He is prepared to believe that between 1810 and 1820, when the British sought to repress popular movements on the grounds that they were conspiratorial and subversive of all established order, there really was a serious revolutionary threat. J. L. and Barbara Hammond, vivid and eloquent social historians, believed that the government invented the threat as a pretext, that it treated individuals and groups unfairly because it relied on spies, *agents provocateurs* and manipulated evidence. Thompson disagrees: "What is most noticeable in the Hammonds' handling of the sources is a marked disposition to *commence* their research with the assumption that any bona fide insurrectionary schemes on the part of workingmen were either highly improbable or, alternatively, wrong, and undeserving of sympathy, and therefore to be attributed to a lunatic, irresponsible fringe. But it is difficult to see why, in 1812, this should be assumed. With a year's intermission, war had continued for almost twenty years. The people had few civil and no trade union liberties. They were not gifted with historical clairvoyance, so that they might be comforted by the knowledge that in twenty years (when many of them would be dead) the middle class would secure the vote."

Thompson's compassion is associated with one acute historical observation. He notes that insofar as the historian does not quite know how to interpret the historical evidence relating to "conspiracy" during this period, the reason lies partly in the ambiguous evidence relating to the attitudes of people who made up the new working class. The main reason the sources are clouded is "that the working people *intended* them to be so." In this connection Thompson introduces the useful idea of an "opaque society," that is, a society in which a major group does not disclose its true thoughts and aspirations to other groups, in which "secret traditions" are more important than open arguments. The idea can be applied to many societies, and not only to the working class. In regard to Britain, Thompson expresses it poetically: "From Despard to Thistlewood [two alleged working-class conspirators] and beyond there is a tract of secret history, buried like the Great Plain of Gwaelod beneath the sea. We must reconstruct what we can."

From the 1820's on we can reconstruct much. This was, after all, the period leading up to the open expression not only of class consciousness but also of class conflict. By 1832 the working class was no longer in the making; it had been made. "To step over the threshold, from 1832 to 1833, is to step into a world in which the working-class presence can be felt in every county of England, and most fields of life." Although Thompson is enlightening about William Cobbett and Robert Owen and provides a tidy chronology of successive working-class movements, he tends in his last few pages devoted to "class consciousness" to rely on a number of set categories. A fuller analysis of "middle class" attitudes seems necessary at this stage of the argument; during the critical debates on the Reform Bill of 1832, which gave the vote to a large section of the middle class without enfranchising the working class, there was less unanimity in the middle class than he suggests. There were also many nuances in class relations, and large numbers of people who did not think in class terms at all.

Nonetheless, as Thompson points out, the discussion as to who should have the vote was of pivotal importance in defining attitudes and tactics not only in the debates leading up to the Reform Bill of 1832 but also in the Chartist period that followed. Once the middle class had been given the vote the militant members of the working class felt deprived until they had won it—deprived as citizens and lacking any effective means of exercising control over the developing industrial economy. "Collective self-consciousness," Thompson concludes, "was indeed the great spiritual gain of the Industrial Revolution, against which the disruption of an older and in many ways more humanly comprehensible way of life must be set." It took many years for that collective self-consciousness to express itself fully, although Thelwall had recognized at the very beginning of the process that "every large workshop and manufactory is a sort of political society, which no act of parliament can silence and no magistrate disperse." By demonstrating chains of connection Thompson has rearranged history, and it will be impossible for any historian in the future to ignore what he says about links and continuities.

The main work that remains to be done in this field concerns parallels. Was the formation of the English working class of 1832 "a unique formation"? What about the making of the English middle class? How did the particular forms of industrialization in the period Thompson has examined influence social relations? What happens when the tech-

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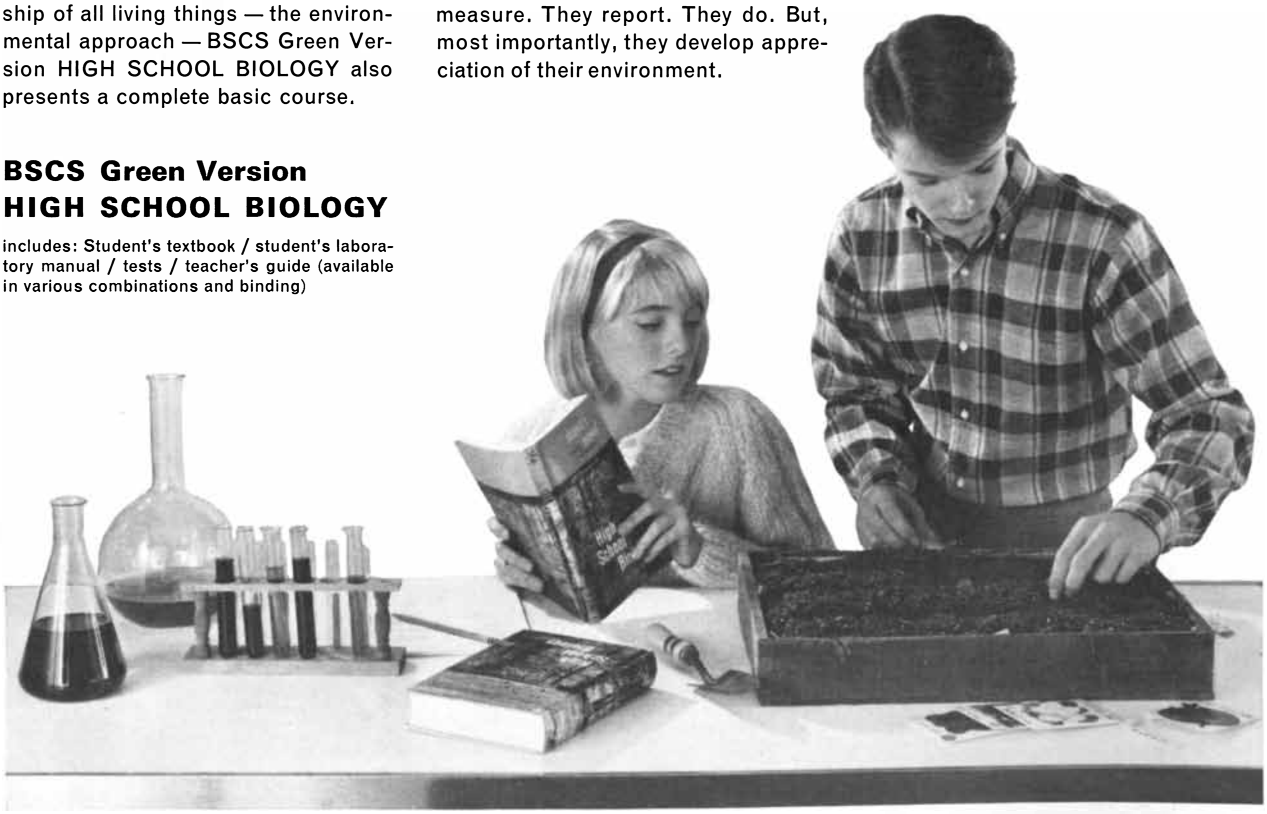

nical base is changed? Only scholars who are as knowledgeable about their own societies as Thompson is about his can lead us toward the comparative social history we most need.

## Short Reviews

Probability and Certainty, by Émile Borel. Walker and Company ($3.50). Pathways to Probability, by Amy C. King and Cecil B. Read. Holt, Rinehart and Winston, Inc. ($2.50). These two relatively slight works demonstrate once again that the theory of probability is of inexhaustible interest. Borel's book, which appears in the attractive "Sun Book" series, is a translation from the French of this eminent student's theories and views on different aspects of probability, from the law of large numbers and the probabilities of causes to entropy, the Petersburg paradox and statistical deviation. Addressed to a nonspecialist audience, the essay affords Borel the opportunity to promulgate one of his pet ideas, namely that extremely unlikely events are not merely highly improbable but in fact impossible. To this category he assigns Sir James Jeans's "miracle" that the water in a pan heated over a hot flame might turn to ice and also the conjecture that a group of monkeys let loose on a few typewriters might by their random efforts turn out the text of *King Lear*. The King and Read volume is little more than a syllabus of the history of probability from Cardan, Pascal and Fermat to R. A. Fisher's mathematics of the tea-tasting lady. Clearly presented, with amusing items here and there and a few of the major concepts explained in some detail, this is an affable primer.

The Great Psychologists: From Aristotle to Freud, by Robert I. Watson. J. B. Lippincott Co. ($7.50). A sound, lucid and readable history of psychology that, in addition to treating the modern experimental period and its immediately preceding philosophical phase, goes beyond Descartes to the Greeks. The book runs from Thales to C. G. Jung, but the greatest of the author's great are Aristotle (34 pages) and Freud (32 pages). William James gets 22 pages, Descartes 19, Wilhelm Wundt, who founded experimental psychology, 17, and J. B. Watson, who founded behaviorism, 16, but mostly you can be great if you get six to 10 pages. A fault in the treatment is its concern with choosing the leading figure of a school: among peers it elevates one into greatness and depresses another into near greatness or even oblivion. Still, in 20 chapters the book tells one quite a bit about the contributions of threescore psychologists, even if it does place J. M. Charcot, T. A. Ribot and Pierre Janet in the shadow of Alfred Binet as the leader of the French school.

The Common Law, by Oliver Wendell Holmes, edited by Mark DeWolfe Howe. Harvard University Press ($5). A reissue, with a notice and a graceful introduction by Howe, of Holmes's famous treatise on the law, first published in 1881. *The Common Law* is that rare thing, a classic that still speaks to us. It lives not so much as legal history but as a source of Holmes's searching philosophical ideas on the nature of our society. Aristocratic in outlook, not indifferent to the blandishments of metaphysics, Holmes was nevertheless of an essentially pragmatic, realistic and skeptical temper. His thought evolved, as Howe points out, in a direction away from the established notions of moral culpability as a basis for law and toward the overriding importance of the common good. Both as a majority spokesman for the Supreme Court and as so often a dissenter, he left a memorable imprint on our society. One of his aphorisms appears in the fourth sentence of his book: "The life of the law has not been logic: it has been experience." Another, less well known but no less pertinent to the circumstances of our time, is that "the first requirement of a sound body of law is that it should correspond with the actual feelings and demands of the community, whether right or wrong."

Computers and Thought, edited by Edward A. Feigenbaum and Julian Feldman. McGraw-Hill Book Company ($7.95). A collection of 20 articles representing advances in the use of computers in many different spheres. Included are classic papers such as A. M. Turing's "Can a Machine Think?" and research reports on chess- and checker-playing machines, machines that prove mathematical theorems, automatic answerers of baseball questions, a pattern-recognizer, decision-making programs, a computer model of elementary behavior and so on.

Annual Report of the Board of Regents of the Smithsonian Institution, 1962. U.S. Government Printing Office ($4.75). The current report contains, in addition to the usual secretary's account of the branches of the Institution, a considerable number of interesting articles on such topics as the early history of radio, the opening of the Arctic Ocean, the increasingly heavy drain on the world's water resources, the place of genetics in modern biology, mangrove trees, the history and relations of the world's cottons, modern glass, the number of people who have ever lived on earth, the evolution of archery in America and scientific methods in the examination and conservation of antiquities. An interesting book and a bargain.

ALGEBRAIC THEORY OF MEASURE AND INTEGRATION, by C. Carathéodory. Chelsea Publishing Company ($7.50). The noted German mathematician Constantin Carathéodory first put forward his general theory of measurement and integration in 1938 and subsequently developed it in a series of papers. The present work, a translation, was assembled by the author and is made up of selections from his papers. Although the manuscript was completed before his death, a considerable amount of editing had to be done to eliminate inconsistencies and other rough spots that were the inevitable by-products of making whole such a mass of heterogeneous material.

CLERK MAXWELL AND MODERN SCIENCE, edited by C. Domb. University of London—The Athlone Press (25 shillings). A collection of six commemorative lectures on different aspects of the life and work of James Clerk Maxwell by Sir Edward Appleton, E. G. Bowen, C. A. Coulson, R. E. Peierls, Sir John Randall and R. A. Smith. Three of the lectures deal with Maxwell's ideas on kinetic theory, thermodynamics and electromagnetic theory, and with developments in these topics since his time. (Particularly interesting is Peierls' discussion of the problems that arise in formulating a quantum theory of the electromagnetic field.) The other three lectures are devoted to recent branches of research that grew out of Maxwell's work on electromagnetic radiation, for example radio astronomy, masers and lasers.

THE MONTESSORI METHOD, by Maria Montessori. Robert Bentley, Inc. ($6.50). A reprint of a translation from the Italian of this celebrated work, originally published more than 50 years ago. Miss Montessori, who was the first woman to be granted a medical degree by an Italian university, had a genius for teaching defective children, particularly in her ability to raise them to nor-



mal educational levels. Her methods were much admired for years, then fell into disfavor, and in recent years, with so-called new methods of teaching, they have regained recognition. A remarkable book by a remarkable human being.

Beyond Time, by Michel Siffre. McGraw-Hill Book Company ($5.95). The story of a young Frenchman who, for some inexplicable reason, spent 63 days alone in an icy cavern 375 feet underground below the French Maritime Alps. The adventure appears to have produced nothing of scientific value other than the fact that Siffre could do what he did without perishing or losing his mind. An eerie and faintly unpleasant tale.

The Victoria and the Triton, by Bern Dibner. Blaisdell Publishing Company ($1.95). A relaxed and unpretentious account of two circumnavigations of the globe, by Magellan's *Victoria* in a voyage that lasted three years and by the nuclear submarine *Triton,* which, submerged, retraced the *Victoria's* route in three months. Illustrations. Paperbacked.

## Notes

A Source Book in Animal Biology, by Thomas S. Hall. Hafner Publishing Company ($15). A reprint of a book first published in 1951 that consists of selections from the writings of leading contributors to animal biology, beginning with Leonardo da Vinci and Andreas Vesalius.

These Fragile Outposts, by Barbara Blau Chamberlain. The Natural History Press ($6.95). A knowledgeable, well-written, effectively illustrated survey of the geology of Cape Cod, Martha's Vineyard and Nantucket. Essential for those who love these sandy outposts and instructive for anyone interested in geology.

Flora of Missouri, by Julian A. Steyermark. The Iowa State University Press ($18.50). This 1,700-page volume presents a comprehensive treatment of the more than 2,400 species of ferns and flowering plants growing in the plains area of Missouri. Hundreds of maps and line drawings.

The Concise Oxford Dictionary of Quotations. Oxford University Press ($1.95). A most satisfactory abridgment of the parent volume, which is the best dictionary of quotations available, this paperback is a pleasure to use and a bargain.

Insight and Responsibility, by Erik H. Erikson. W. W. Norton & Company, Inc. ($5). A collection of six lectures, delivered over the past seven years, on the social and ethical implications of psychoanalysis.

Piezoelectricity, by Walter Guyton Cady. Dover Publications, Inc. ($5). A revised two-volume edition of a standard introduction to the theory and applications of electromechanical phenomena in crystals, an effect discovered in 1880 by Pierre Curie and his brother and intensively studied within the past 20 years. Paperbacked.

The Sea, edited by M. N. Hill. Interscience Publishers ($28). The third and final volume of this superb survey, earlier installments of which have been reviewed in these columns, deals with the earth under the sea and the sea's history. Magnificently illustrated with many charts and plates.

Evolution of the Atherosclerotic Plaque, edited by Richard J. Jones. The University of Chicago Press ($6.75). The papers in this volume, presented at a symposium held in Chicago in 1963, consider the relations between the serum lipids and the arteriosclerotic lesion and review the process of atherogenesis from its earliest inception through ulceration and thrombosis. Illustrations.

Young Geographers, by Lucy Sprague Mitchell. Basic Books, Inc., Publishers ($3). A reprint of Mrs. Mitchell's stimulating little book, first published in 1934, designed to guide teachers in rousing the interest of children in geography by wedding the subject to their own experiences. Many illustrations.

William Herschel and the Construction of the Heavens, by Michael A. Hoskin. W. W. Norton & Company, Inc. ($6). This book presents a number of William Herschel's great papers on stars and star systems, based on his own prodigious observations of the heavens during the period 1783–1802. Hoskin adds his commentary and interpretation.

The Reader's Adviser, edited by Hester R. Hoffman. R. R. Bowker Company ($20). The 10th edition of this annotated guide to leading books in 34 major subjects, periods and fields of knowledge from fiction and poetry through science and philosophy. An indispensable reference companion.

ACOUSTIC BEHAVIOUR OF ANIMALS, edited by R.-G. Busnel. American Elsevier Publishing Company ($45). This elegantly produced 950-page book collects a mass of information, much of it hitherto unpublished, on animal acoustics: the sounds animals hear and make. The noisemakers covered are birds, mammals, fishes, amphibians and arthropods.

SELECTED WORKS IN ORGANIC CHEMISTRY, by A. N. Nesmeyanov. Pergamon Press ($30). This is the first of four promised volumes containing selected papers in organic chemistry and organometallic research by a distinguished Soviet scientist who was for 10 years president of the Academy of Sciences of the U.S.S.R. The papers are translated from the Russian by Avraham Birron and Z. S. Cole.

MATHEMATICAL GAMES AND PASTIMES, by A. P. Domoryad. Pergamon Press ($5). A translation from the Russian of a collection of mathematical recreations. Most of those described are standard, but a few are original or introduce a novel turn to an established pastime.

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