Engineering & Science

SCIENCE/SCOPE

Major developments toward an optical filter that can be tuned electronically to specific wavelengths of light have been reported by Hughes scientists. The device is tuned by a microprocessor that varies the electric field distribution onto an electro-optic crystal. One filter with a lithium-tantalate crystal has been operated across the visible light spectrum from deep blue to deep red. Another has been tuned into the infrared portion of the spectrum. The device promises to find important uses in pollution monitoring, multispectral imaging, and monitoring color consistency in a wide range of commercial products.

Using digital techniques to perform image processing tasks like scan conversion and information storage, a new microprocessor-controlled display system is finding a wealth of applications from medical diagnoses to non-destructive testing. The system, called the Hughes Anaram 80" digital signal processor, is designed to create images with the natural look of analog displays while providing the data-handling benefits of digital techniques. The system can display 60 images per second, freeze one picture for an hour, enhance obscured detail, and display four pictures simultaneously for comparative analysis. Uses include medical ultrasonography, X-rays, radar, graphics terminals, and image transmission.

An advanced goggle that allows soldiers to see at night has been developed by Hughes for the U.S. Army's Night Vision Laboratories. The device, called a holographic one-tube goggle, employs thin-film diffraction optics and advanced electronics. It amplifies dim visual light and near-infrared radiation, then superimposes the enhanced image over the wearer's view. Aided by studies on how the brain overlaps the field of view of each eye, human engineering specialists designed the goggle so that the image intensifier tube, which extends from above the bridge of the nose, would not block any portion of a person's view.

An Exotic chip that would alert a pilot when he has been detected by enemy radar promises to open a new arena in modern electronic warfare. The unique wafer, called an integrated optic spectrum analyzer (IOSA), would allow a pilot to prepare for a dogfight, turn on jamming equipment, or take any other appropriate action. The device works by having a surface acoustic wave device convert processed radar signals into sound waves. These sound waves interact with light from a tiny solid-state laser and cause the beam to bend toward a detector array of charge-coupled devices. The amount of deflection indicates the frequency of the radar signal. The IOSA is being developed by Hughes for the U.S. Air Force.

Highly complex microcircuitry soon may be mass produced with a technique being pioneered at Hughes. The approach, called ion beam lithography, has been used to make very large-scale integrated circuits (VLSI's) having circuit lines as narrow as 0.1 micrometer, about 4 millionths of an inch. These minute dimensions have been possible only by tedious, painstaking methods that use an electron beam to draw circuitry on a wafer. Ion beam lithography, however, is faster and less costly because it uses a collimated beam of protons to "photograph" circuit patterns from a mask onto a whole chip.

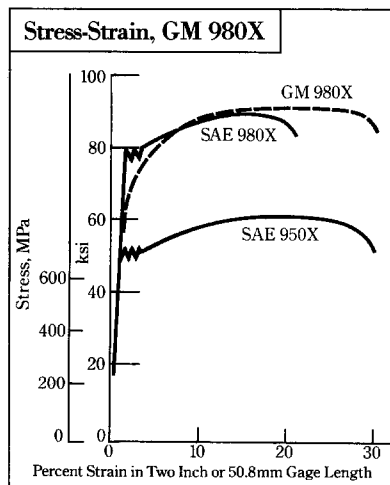Hughes is currently seeking new graduates in electrical, mechanical engineering and computer science or other closely aligned disciplines to meet the demanding challenge of our high technology company. To obtain further information, please write: Manager, College Relations, Hughes Aircraft Company, P.O. Box 90515, SS/100/445, Los Angeles, CA 90009.

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HUGHES
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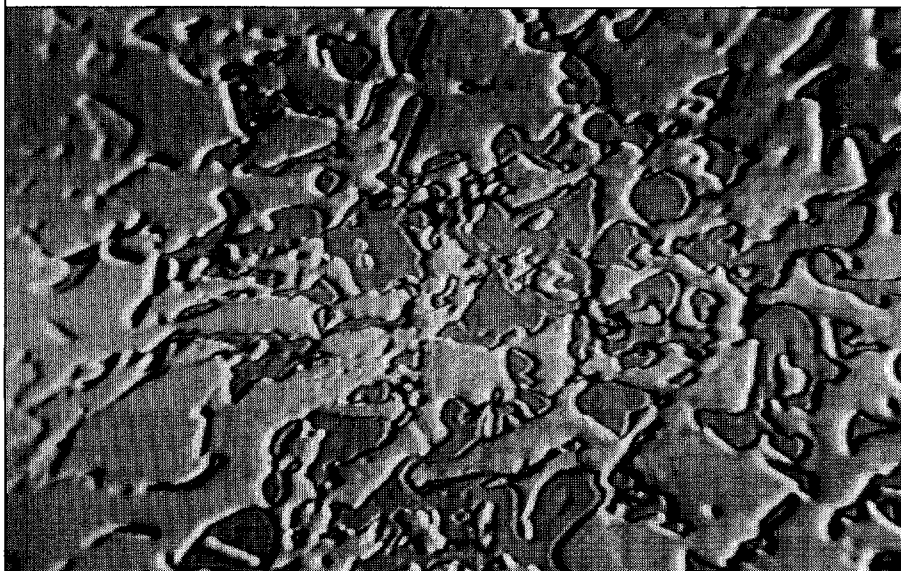*Creating a new world with electronics*

The Ductility Factor

The use of high strength, low alloy steel has been severely limited, due to its low ductility. Now, a simple heat treating and controlled cooling process, developed at the General Motors Research Laboratories, has successfully enhanced formability properties without sacrificing strength.

Stress-Strain, GM 980X

Percent Strain in Two Inch or 50.8mm Gage Length

A comparison of the stress-strain behavior of GM 980X, SAE 980X, and SAE 950X steels. GM 980X offers greater ductility at the same strength as SAE 980X, and greater strength at the same ductility as SAE 950X.

Scanning electron microscope micrograph of dual phase steel at a magnification of 2,000. The matrix (background) is ferrite; the second phase is martensite.

OR SOME TIME, automotive engineers and designers have been faced with the challenge of building cars light enough to get good gas mileage, but still roomy enough to comfortably transport four or five passengers. One technique which has proved fruitful is materials substitution.

Lighter materials, such as aluminum alloys and plastics and high strength, low alloy steels (HSLA), are being phased into new vehicle designs to replace certain plain carbon steel components. Each, though, has displayed inherent problems which limit its utilization.

Unlike plastics and aluminum, however, HSLA steels have the same density as plain carbon steel. Weight reduction is achieved because thinner sections (less volume) can be used to carry the same load. Since the formability (ductility) of most high strength steels is poor, though, it has only been possible to form simple shapes from it. This has severely limited the widespread use of HSLA steels (such as SAE 980X) for auto components. New hope for the increased utilization of HSLA steel has arisen, however, with the development of a new dual-phase steel, GM 980X, at the General Motors Research Laboratories.

General Motors is not in the steel business, and GM 980X is not a brand of steel. GM 980X is the designation for a type of steel displaying mechanical properties similar to those of the samples first formulated at the General Motors Research Laboratories. "GM" in the designation indicates that the steel is a variation of the conventional SAE 980X grade. In the standard SAE system for material identification, "9" designates that the steel is HSLA. "80" is the nominal yield strength of the metal in thousands of pounds per square inch. The "X" denotes a micro-alloyed steel—one containing on the order of 0.1% of other metals such as vanadium, columbium, titanium, or zirconium as a strengthening agent.

GM 980X displays the same strength, after strain hardening, as SAE 980X steel, but has far more ductility. This characteristic allows it to be formed into various complex shapes which were previously thought to be impossible with HSLA steels. The superior formability of GM 980X has substantially increased the utilization of HSLA steel in the manufacturing of automotive components such as wheel discs and rims, bumper face bars and reinforcements, control arms, and steering coupling reinforcements.

Dr. M.S. Rashid, discoverer of

the technique to make GM 980X steel, comments, "I was working on another project using HSLA steel, when I noticed that if SAE 980X steel is heated above its eutectoid temperature (the temperature at which the crystalline structure of metal is transformed) for a few minutes, and cooled under controlled conditions, the steel developed significantly higher ductility and strain-hardening characteristics, with no reduction in tensile strength."

FURTHER experiments proved that the key variables to make GM 980X are steel chemistry, heating time and temperature, and the rate at which the steel is cooled. Specimens of SAE 980X were heated in a neutral salt bath, then cooled to room temperature with cooling rates ranging from 5° to 14°C/sec. (9° to 26°F/sec.). Dr. Rashid notes, "We found that the maximum total elongation resulted when the cooling rate was 9°C/sec. (16°F), and the lowest total elongation resulted from the highest cooling rate (14°C or 26°F/sec.)."

GM 980X steel has a high strain-hardening coefficient or n value, accompanied by a large total elongation. The n value gives a measure of the ability of the metal to distribute strain. The higher the n value, the more uniform the strain distribution and the greater the resistance of the metal to necking (localized hour-glass-shaped thinning that stretched metals display just prior to breaking). Tests have proved that GM 980X distributes strain more uniformly than SAE 980X, has a greater resistance to necking, and

thus has far superior formability.

"The superior formability of GM 980X compared to SAE 980X steel appears to depend on the nature of two microstructural constituents, a ferrite matrix (the principal microstructural component) with a very high strain-hardening coefficient, and a deformable martensite (the other crystalline structure) phase. In the SAE 980X, failure occurs after the ferrite becomes highly strained, but when the GM 980X ferrite is highly strained, strain is apparently transferred to the martensite phase, and it also deforms.

"Therefore, voids leading to failure do not form until after more extensive deformation has occurred and the martensite phase is also highly strained. Obviously, the exact nature of these constituents must be important, and any variations in the nature of these constituents could influence formability. This is the subject of ongoing research."

Dr. Rashid's discovery represents a significant breakthrough in the area of steel development. His findings have opened the door to a new class of materials and have completely disproved the commonly held belief that high strength steel is not a practical material for extensive automotive application. "At GM, we've done what was previously thought to be impossible," says Dr. Rashid, "and now we're hard at work to find an even stronger and more ductile steel to meet the needs of the future."

THE MAN BEHIND THE WORK

M.S. Rashid is a Senior Research Engineer in the Metallurgy Department at the General Motors Research Laboratories. He was born in the city of Vellore in Tamil Nadu (Madras), India, and attended the College of Engineering at the University of Madras—Guindy. He came to the United States in 1963 and was awarded a Ph.D. in Metallurgical Engineering from the University of Illinois at Urbana-Champaign in 1969. After a three year Post-Doctoral Fellowship at Iowa State University, he joined the staff of the General Motors Research Laboratories.

Dr. Rashid is continuing his investigations into the development of even more ductile high strength, low alloy steels. When not in the lab, he enjoys relaxing by playing tennis and racketball with his wife, Kulsum.

In This Issue

Historic Group

On the cover — a 1949 photo of some of the members of the Phage Group lunching at Caltech — from left to right, Jean Weigle, Ole Aaloe, Elie Wolman, Gunther Stent, Max Delbrück, and G. Soli. The Phage Group was a circle of molecular biologists under the informal guidance of Delbrück. It was formed one summer at the Carnegie Institution's genetics research laboratory at Cold Spring Harbor, New York, where for a number of years Delbrück taught a summer course in phage. Delbrück's pioneering studies of viral genetics — the way strains of the virus bacteriophage infect the bacterium *E. coli* and multiply there — led to his receiving the Nobel Prize in 1969 (sharing it with his colleagues Salvador Luria and Alfred Hershey)

His work on phage was largely done between 1937 and 1952, and Delbrück then turned to studies of the nervous system, using the simple *Phycomyces* fungus — whose growth is affected by light — as a way of understanding sensory processes. A *Phycomyces* Group formed too, and Delbrück also taught a summer course on that topic at Cold Spring Harbor.

Delbrück's influence has, of course, been broader than molecular biology. Now Board of Trustees Professor of Biology, Emeritus, he is still — as he always has been — a warmly humanitarian member of the community of scholars. "Max Delbrück — How It Was" on page 21 is the second of two installments adapted by *E&S* from the Oral History of his life compiled by the Caltech Archives.
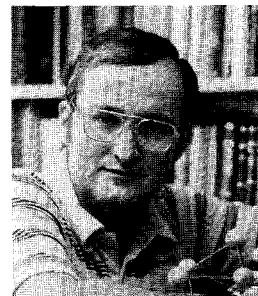

Victor Wouk

Hybrid Helper

Alumnus Victor Wouk (MS '40, PhD '42) is a man with a mission to convert as many vehicles as possible to electric or hybrid automotive systems. He is also a man with a realistic, long-term outlook on the slim likelihood of short-term success — a likelihood that could be improved by the impact of the petroleum shortage. For the last 20 years or so, Wouk has put his time, energy, and money where his convictions are, investing his considerable engineering expertise in several corporations that he developed into successful enterprises. The most recent of these was PetroElectric Motors, Ltd., which developed a low-emission, good fuel economy, hybrid vehicle that is still waiting for a far-sighted manufacturer to put it on the market.

Wouk also teaches, consults, and lectures, and he has about 75 publications — 30 in the area of electric and hybrid vehicles. Since 1970 he has been a member of the International Electrotechnical Commission and U.S. representative to its "Electric Road Vehicles" committee meetings in Stockholm, Stuttgart, Brussels, Ljubljana, Düsseldorf, and Florence. He was chairman of the working group on "Standards of Terminology and Testing."

Not long ago, Wouk volunteered to write a combination retrospective and look-ahead article on his area of expertise for *E&S*, and we were happy to accept. "From Horsepower to Shanks' Mare Power: Is the Automobile Doomed, or Is It Just Us?" on page 6 is the result.

Another result is the montage of photographs we assembled to accompany the article — a mostly tongue-in-cheek look at some of the ways Caltech people have dealt with transportation problems. Wouk may well have an even better idea.

William Johnson

Metallic Glass

Glassy metals are not a contradiction in terms but the result of altering atomic structure — research pioneered 20 years ago by Caltech's Pol Duwez, now professor emeritus. Duwez's work in producing amorphous metal alloys, with atoms arranged not as in a crystal but more like glass, opened up a whole new field of materials research, now being continued by Caltech's William L. Johnson, assistant professor of materials science.

After receiving his BA from Hamilton College in 1970, Johnson earned his PhD at the Institute in 1974 and then did some postdoctoral work. After a couple of years at IBM's Thomas Watson Research Center, he returned to Caltech in 1977, and in a relatively short time has made significant impact in research concentrated on the superconducting properties of metallic glasses and on the relationship of their atomic-scale structure to their electronic and magnetic properties.

In an article on page 13, "New Materials: Atomic-Scale Architecture of Metallic Solids," adapted from his recent Watson Lecture, Johnson explains how the atoms of metal crystals are realigned to create materials with unique properties and discusses some of the current industrial innovations in manufacturing these materials.

Engineering & Science

From Horsepower
to Shanks' Mare Power

Is the Automobile Doomed, or Is It Just Us?

by VICTOR WOUK

Twenty-five years ago Peter Kyropoulous, an associate professor of mechanical engineering at Caltech, wrote a perceptive article on cars "From Horses to Horsepower" for *Engineering and Science*. (I am taking an editorial guess that since the article *appeared* in February 1956, it was written in 1955, and thus I am celebrating a 25th anniversary. Nobody does a retrospective article after 24 years.) I thought it would be interesting to compare some of the descriptions of what Professor Kyropoulous considered important to discuss about cars in 1955 with what the situations are in 1980. He made some guarded predictions about cars in the immediate future as well as 10 to 20 years off, and I thought it would be fun to check his admitted "look into the crystal ball." The editors of *E&S* agreed with me that comparing some worldwide car statistics relevant then and now should be intriguing. Hence this article.

To me, the most outstanding fact of Peter's article is that he made no reference to the problem of air pollution from cars. A corollary is that he made no mention of government regulations of the auto industry. Since Peter knew whereof he wrote (after all, he left Caltech in 1957 to become executive in charge of technical development of styling for General Motors), I believe his omissions prove one of my favorite theses, to wit: The American driving public really doesn't give a hoot about automobile air pollution.

Air pollution became a politician's and an environmentalist's dream topic in the mid-1960s. It was better than motherhood and apple pie. With screams of "We'll all be asphyxiated if we don't clean up cars' exhaust," the state of California led the way — as it does in so many aspects of the American way of life — and the federal government followed in 1968 and 1970 with tough emission standards. In fact, politicians vied with each other in proposing tougher emission standards for cars or in condemning the Detroit monster as a fume belcher — or both. (A few years later the Detroit monster was being flagellated as a gas guzzler, but I'll discuss that later.) Suffice it to say that an important difference between then and now is that 25 years ago the automobile manufacturing industry was virtually free of government regulation. Now, car manufacturers declare, the design of automobiles is determined essentially in Washington, not Detroit.

It is only fair to inject here that I am particularly sensitive on the subject of air pollution from automobiles. I have been working on electric vehicles and heat-engine/battery-electric hybrids since 1962. I have lived through a decade of the federal government having spent $N x 10^6, where $30 \leq N \leq 200$, on developing "clean" cars. Not one gram of HC, CO, or NO_x has been removed from the

air due to commercially produced products or systems developed with government funds. (This statement may be hyperbolic. When I called Washington for quantitative data, no call was returned, the man always being in conference . . . the well-known "call to Washington syndrome.") Some private millions of dollars, plus Detroit's hundreds of millions of dollars, have done the job of reducing auto emissions more than 80 percent since 1968 in order to meet government regulations. So far, improvements in fuel economy have followed the same pattern, but that's the subject of another article.

Here are some comparisons of vital statistics from Peter's 1955 writing:

SUBJECT	THEN	NOW
USA new cars/year	6 million	11 million
World new cars/year	10 million	30 million
Cars in USA	60 million	110 million
Cars in world	80 million	300 million
USA vehicular traffic death rate	7.1 per 100 million vehicular miles	No major difference
Average new car cost	$2,720	$6,000
Imports of foreign cars, % of total bought per year	0.55% (not a mistake)	25%
Volkswagen sales/yr in USA	0.15%	2.5%
Main reason for purchase	Economy (low price, good trade-in, low maintenance)	Low gas consumption
Average mileage/yr	9,000	10,000 +

There are many more goodies in Peter's article. A point-by-point comparison would fill an issue of *E&S*, a goal deemed unnecessary by me and undoubtedly by the editors, if not the readers. However, before I get to the crystal ball, a few of the major changes since 1955 are worth noting. This will be done in the form of quotations from Peter (in italics) followed by an observation or comment by me.

Efforts to sell small European cars as cheap transportation are not particularly successful as long as people can get a full-sized car for less money.

Comments: First, it should be noted that Japanese-built cars were virtually non-existent in 1955. Second, we now

willingly pay more for the smaller VW than the full-sized American car because of our fear of gasoline shortages.

As far as the individual family car is concerned, fuel economy is a grossly overrated item. By changing from 15 mpg to 20 mpg we only change the cost per mile from 8.3¢ per mile to 7.8¢ per mile (based on 10,000 miles per year), a difference which is hardly worth all the bragging that it brings about.

Comments: This is as true today as it was then. Peter took into account, when calculating costs per mile, not only the variable costs of fuel, oil, maintenance, and tires, but — quite properly — the fixed costs of insurance, license fees, and — the greatest cost of all — depreciation (for new cars). Triple all costs for 1980, and the difference between 15 and 20 mpg is 24.9¢ to 23.4¢. Going from 20 mpg to 40 mpg drops the 23.4¢ to 22¢ per mile.

Today it is the fear of lack of availability of gasoline that is driving (double entendre deliberate) motorists to smaller cars, not the cost of the fuel. The first year's depreciation of a $6,000 car, $2,400, equals 1,600 gallons of gasoline at $1.50 per gallon. At 20 mpg this is 32,000 miles, or three years of driving. Depreciation is still a major consideration even at $1.50 or $2.00 per gallon. It is interesting that the AAA (Average American Autoist) is not sensitive to this financial fact of life. But gasoline lines! That's another matter.

Now we come to an uncannily prescient statement by Peter:

Looking at fuel economy from the point of view of natural resources, rather than individual savings, we get a different picture indeed.

In 1954 the gasoline used by automobiles in the USA amounted to 13.5 billion dollars (including 3.5 billion in taxes). All central power stations and railroads paid a fuel bill of 1.5 billion dollars.

A fuel saving of 10 percent in the automobiles of the country saves more fuel than the fuel cost of all central power stations and railroads.

Comment: Peter was talking about savings of money. But by bringing in the *national* use of gasoline, he may have been the first to recognize the impact of the automobile on national, or indeed worldwide, problems. Today the automobile's thirst for petroleum is a major contributor to worldwide inflation, political unrest, and, according to prophets of doom, World War III. Petroleum use for personal transporation is greater than usage for any other single purpose.

The highest-powered vehicles, while driven more frequently in the high speed ranges, are not driven at any greater maximum speeds than the lower-priced cars, except perhaps for those under 100 hp.

Comment: As true today as then. Those "300 horses under the hood," prevalent before Detroit began to make cars for lowered fuel consumption, were not there so that cars could go 120 mph, or more. Such useless and dangerous high top speeds were incidental. The engine was that powerful because it gave the car pep or rapid acceleration. "Pep" sold. At 30¢ per gallon, a mere 8 mpg was no problem. Acceleration pedals were, and still are, depressed heavily for acceleration and then relaxed as much as 75 percent for highway driving. (Readers who do *not* drive with a heavy foot will please excuse me. You are in a minority.)

All attempts to develop safety devices are good, but they cannot substitute for a sensible driver.

Comment: This is still true, despite all the safety features that have been introduced as a result of Ralph Nader and his followers. Seat belts are useless if they are not employed. The attempt to legislate their use in 1974 models by mechanical fiat (the driver and front seat passengers had to buckle up or the car wouldn't start) was a miserable fiasco. The law was repealed faster than any other in-out action of Congress since 1782. (Maybe I'm wrong here, but I'm not far off.) And air bags! There is still more hot air generated in debates on air bags than will be expended by their being inflated in anticipated car crashes during the next $\sqrt{e^\pi}$ years.

Now, development [of cars] is concerned with refinements, rather than dramatic changes. Utility is taken for granted.

Comment: As true today as in 1955. This complex mechanical device, a modern car, is taken for granted. Despite all the jokes and complaints, cars are amazingly reliable.

There have been no major developments in automobile technology since 1955. I do not consider eight-track tapes of major consequence, nor are electrically operated front seats an earthshaking advance (except possibly for teenagers).

During the last few years it has become fashionable to denounce the American automobile with a fervor usually reserved only for political and religious controversy.

Comment: How true. Recently, modish speakers decry USA cars with a passion normally assigned to discussions of state or church.

In Europe the automobile has not developed into a necessity and a household appliance . . . . In practice more than 90 European cars out of 100 are purchased with company funds, but used for both business and pleasure.

Comment: The Europeans are catching up with us to a substantial degree. We average 516 cars per 1,000 people. In Europe it's 244 cars per 1,000 people.

Outside America, there have . . . been few developments in motor roads. In France under 100 miles of new road have been built since the war, and in England expenditure has been almost entirely confined to the erection of "Danger" and "No Parking" signs.

Comment: My, how things have changed! England has a few thousand kilometers of "M" high-speed Motorways, similar to our Interstates, as does France with its Autoroutes. Because the countries are smaller than the USA, the total road lengths are much less. There the dissimilarity ends. The "Ms" and Autoroutes are multiple lane (up to five in some places), limited access, and free of intersections. The French and British are also blessed with bumper-to-bumper traffic in and around urban areas during daily rush hours and during homecoming hours on weekends and holidays.

Now let's look into Peter's crystal ball. Peter introduced his extrapolations with the following caveat:

C. F. Kettering suggests this method of predicting the future of automotive engineering: "Considering all the factors, use the best extrapolation you can, push it as high as you can, and, if you live to see it accomplished, you will be amazed that you missed it so far.

Peter predicted:

Gas turbines are here to stay; will find their most suitable use in trucks, buses, military and earth-moving vehicles, perhaps in racing and sports cars.

What happened: There is no commercial application of turbines in road transportation. There have been experimental buses and sports cars. In the 1970s a turbine car, raced in the Indianapolis 500, was in a position to beat the field by many laps, and to set a new Indy 500 record, when the turbine failed and the car did not finish. Turbines were subsequently barred from the Indy 500. Turbine superchargers are another story.

The federal government, first through the Environmental Protection Agency (EPA), and now through the Department of Energy (DOE), has spent more than $100 million to develop a turbine for the automobile. The goal of the EPA was a clean engine, spurred on by the fact that the Brayton cycle (fancy word for a turbine) is basically an external combustion engine. In an external combustion engine, burning can be more complete and cleaner than in an ICE (internal combustion engine).
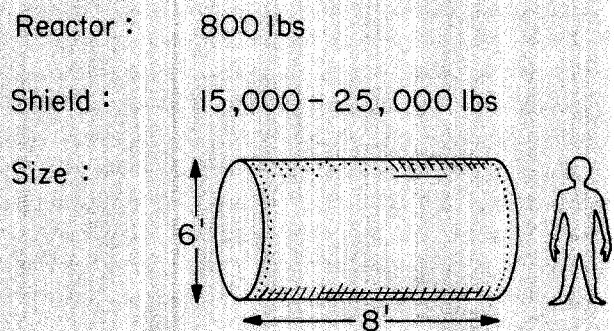
The DOE is excited about the prospects of the turbine because the Brayton cycle can be inherently more efficient than the Otto cycle (conventional piston engine) of the ICE. Cars will use less gasoline! Let's go at it!

The turbine has run up against a stone wall of technology and a paper wall of finances. The stone wall is more accurately a ceramic wall — an attempt to develop ceramic blades for the turbine. A turbine must operate at very high temperatures and speeds, and to date, only expensive metal alloys have been usable. It's OK for the metal in the turbine in the friendly skies, tucked inside the engine of a commercial jet, to cost $50,000, but the metal cost is prohibitive for automobile purposes. Ceramics, which could operate at high temperatures and high mechanical stresses, in principle could be cheap or at least inexpensive. So, hope springs eternal in the breasts of the contract issuers in Washington. Their jobs may also be eternal, but that's only conjecture.

Nuclear power could become an attraction if a small reactor can be built. With it the reciprocating steam engine may return from its somewhat undeserved oblivion.

Again, Peter was prescient, this time in discussing in an offhand manner the possibility of a cute little nuclear reactor to generate steam to operate a steam engine. For a lot of reasons the steam engine, for cars, remains in oblivion. But the nuclear power plant ranks with petroleum sources as a major problem in our industrial civilization. There are the problems of waste disposal, and potential terrorist use of nuclear weapons obtained from recycled fuel elements.

The cute little nuclear reactor for a car is out of the question. In 1967 I presented a paper to the AAAS (American Association for the Advancement of Science) session on "Man and Transportation in the 21st Century." The subject of the paper was "Electric or Nuclear Power for Automobiles?" The man-shield comparison shown below gives an idea of how large a reactor would have to be for

Reactor :	800 lbs
Shield :	15,000 – 25,000 lbs
Size :	6' tall, 8'

about 50 kW of useful output — just about enough to drive a reasonably sized car. The shield would weigh 8 to 12 tons. But this was for SNAP-8, a nuclear power plant in space that would be located 100 feet away from astronauts in a space capsule. In a car it would look like this:

Reactor

150'

I was flippant in that paper, saying that one of the problems would be accidents and the spilling of radioactive wastes. Also, what a field day for thieves who wanted to peddle enriched uranium for blackmail purposes! These two problems, radioactive wastes and nuclear proliferation, are the major hurdles to our being able to solve The Energy Problem in the world, but let's drop that subject so we can remain friends.

The electric car may come in for some attention for short haul service. We have learned a few things about batteries and should be able to produce an acceptable vehicle.

Peter hit the nail on the head!

Since 1970, within his time frame of 10-20 years from 1955, electric *vehicles*, not cars, have "come in for some attention for short haul service," particularly electric delivery vans. The DOE is implementing the Electric and Hybrid Vehicle Research, Development and Demonstration Act of 1976 with $160 million. It is hoped that, by pump-priming, an electric vehicle industry will grow in the USA. Currently 20 electric service vans are being successfully operated by AT&T in Culver City, California.

Since most cars are driven less than 40 miles per day in the USA (some areas of southern California are an exception to this and other American norms), battery-powered cars could satisfy much daily driving. This would be particularly true if there existed networks of electrical outlets for charging the batteries when the vehicle is not in use. This process, "biberonnage" (from the French, meaning "bottle feeding," or, more colloquially, "a quick one for the road"), is being experimented with seriously in Germany. Biberonnage makes electric cars practical with present lead batteries.

The "few things we have learned about batteries" have defied intensive efforts over more than a decade to come up with a commercially producible better battery. It has been "five years away" for the past 15 years, and still is according to General Motors.

By a curious coincidence, my involvement with electric cars was a direct result of the requirement for better batteries, Caltech's preeminence in chemistry, and Dr. DuBridge's interest in the concept of an electric car.

In 1962 I was approached by a well-known industrialist who had built 30 electric cars, converting Renault "Dauphines" by removing the engine, transmission, etc., and putting in batteries, an electric motor, and a speed control. His reasoning was that air pollution from cars was going to be a serious problem, and electric cars would solve the problem. He thought that at *least* electric utilities would buy the cars for meter-reading, where the driver does not have to go far nor fast. He expected the cars to sell like hotcakes. They sold like coldcakes. Why? A top speed of 35 mph and a range of 25 miles was not enough, even for meter-reading.

The industrialist had been told that the poor performance was due to the old-fashioned speed control that wasted too much of the energy stored in the batteries. He needed a modern, electronic speed control, which should be more efficient. I was suggested as knowledgeable in the field, as in 1962 I had presented a paper on what are called "switching regulators" at an IRE (now IEEE) conference. I examined the car, drove in it, and concluded that the problem was not the speed control, but the battery. There just was not enough energy stored in the batteries. I was asked if there was an inherent limitation to the electrochemical process that would make a better battery impossible. Good question, because if better batteries were not possible, he'd give up his electric car project.

I did not know the answer, but thought that someone at Caltech might. So I wrote to Dr. DuBridge, who replied that the consensus at Caltech was that theoretically much better batteries could be built. There are electrochemical couples available that have energy densities at least 10 times that of the lead battery. It was a matter of engineering ingenuity, time, effort, and money. The quantitative story of gasoline vs batteries is expressed in watt hours per pound (Wh/lb) of stored energy. Roughly, a pound of gasoline has 1000 watt hours, i.e., a 100-watt lamp would be lit for 10 hours, if 1 pound of gasoline is burned and converted to electricity. Lead batteries have 10 Wh/lb, i.e., a 100-watt lamp could be lit for 1/10 hour before a 1-pound battery would discharge.

More practically , if we can go 300 miles at 50 mph on

a tankful of gasoline, we would go 3 miles on a "tankful of batteries." Modern electric cars go 30 miles or more at 50 mph, because the batteries in the car weigh 10 times as much as a tank of gasoline.

The following chart sums it up:

MATERIAL	WH/LB
Lead batteries	10
Some experimental exotic batteries	100
Gasoline	1000

Dr. DuBridge's statement was accurate. By 1970, because of intensive work done in many laboratories, there were individual cells, such as liquid sodium and liquid sulfur with a ceramic electrolyte, yielding 100 Wh/lb. By 1980 there were produced modules of cells using high temperature (300°C) materials connected to provide 100 volts, which would propel an electric car 150 miles. So, we might project that by 1990 such batteries will be available as a physical reality. The price may be prohibitive. We discuss this when *I* do crystal-ball gazing at the end.

With the increase in super-highway mileage we will need automatic steering. The car might be rolling along a beam produced by a buried cable. Once on the beam, the driver would push the control out of the way and relax. There will have to be proximity warning devices and emergency over-rules but, all in all, this is not very complex. It will take a lot of monotony out of long distance driving; people can doze without wrapping themselves around trees.

These things are probably 10 to 20 years off.

The petroleum problem has put the kibosh on automatic steering even if it were technically feasible, which it is not at present. At 55 mph we can expect the driver to remain alert so as to avoid accidents. With fuel being expensive or rationed, commonplace cross-country trips will go the way of tail fins.

In the immediate future we will see more power, levelling off around 450 hp, the upper limit of what the two rear wheels can comfortably transmit.

Bull's-eye!

Gasoline injection will appear in one form or another – may well turn out to be a fad, rather than a real step forward.

On the nose again! Not a fad. Fuel injection was first introduced for emission control (uniform fuel mixture in all cylinders improves emissions) and now is a big thing for improvement of fuel economy (uniform fuel mixture in all cylinders improves fuel economy).

Compression ratios will go up and so will gasoline octane numbers. I am guessing at 15 to 20:1.

Optimistic. Compression ratios peaked at 11 or 12:1, and now have been backed off to 7 or 8:1, to reduce the nitrous oxides in the exhaust.

Why not Diesel? Because at any compression ratio the spark ignition cycle has a higher thermal efficiency than the compression ignition cycle of the same compression ratio. (Note to the readers who like to write to the editor. Before you break into loud snorts of indignation over this one, consult an elementary thermodynamics text.)

Where did Peter go wrong about the diesel? He didn't. The key is the phrase "of the same compression ratio." ICEs never did get up to the compression ratios of the diesel. Hence, the Volkswagen Rabbit diesel is rated at 42 mpg, whereas the gasoline-engine Rabbit, even with fuel injection, is only 32 mpg. Remember, these figures may vary with the degree of my laziness about going to the EPA mileage rating charts.

The possible gains in performance (acceleration, fuel consumption) from lightweight construction may well come in for some attention. This calls for more than a "material substitution program." It will require a lot of re-designing of components and development of aluminum die-casting of large parts.

Again, correct in all principles. Lightweight materials have been introduced, but plastics seem to be the glamor material rather than die-cast aluminum. But then, "The Graduate" *did* take place in California.

In any event, there are not going to be any dull moments.

Peter was right. There weren't, and now I make some bold predictions for the years 1990 and beyond (all subject to instant change, depending upon the political situation in the Persian Gulf):

There are not going to be any dull moments.

As I wrote in the AAAS paper in 1967, I believe the personal automobile is here to stay. Why? Because it represents freedom of mobility. We are not going to give up this freedom, which is fundamental to our socio-economic structure, as long as there is a choice. Without a car, our present way of life is doomed. There *is* a choice. Use a fuel other than limited fossil energy sources.

Between 1980 and 1990 it's easy to predict that cars will be smaller ("downsized" — ugh!). Liquid hydrocarbons, whether fossil or synthesized from coal, biomass, or the like, will be used more and more for societal purposes . . . to run agricultural equipment for food production, planes, interstate trucking and busing, etc. Batteries will be improved, and electrics and hybrids will be in car showrooms

by 1990, though not in large numbers, because of the price. The "better battery" will be expensive.
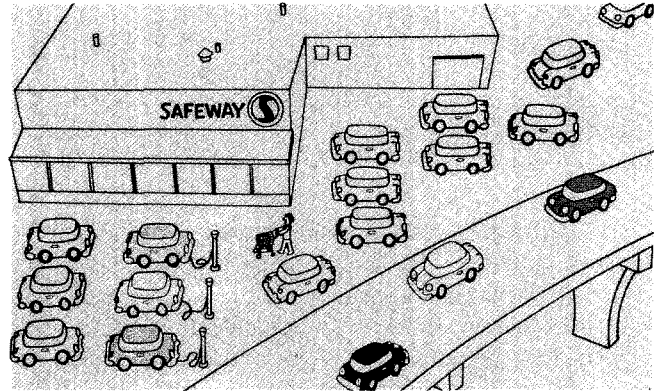
Now, for 25 years hence, and the year 2005: Specifically, we must recognize that our customary profligate use of petroleum in autos just can't continue. I disagree that the common car will be a two-seater microcar, using synfuels and getting 80 mpg, suitable for shopping, short commuter drives, and family activities usage. It doesn't make sense to use scarce liquid hydrocarbons for anything when electricity will perform the job.

I agree that the large-sized car for visiting grandma a few hundred miles away will be a rental, a community-owned car, or the like. If the full-sized car as we know it is to be commonplace, it will have to be a hybrid. The hybrid is a combination of a small ICE and electric drive, in a full-sized car. (See the article in *Caltech News*, Vol. 12, No. 6, September 1978.) The hybrid will have all the highly touted improvements of the conventional auto — light weight, better engine, and the like. In normal usage it will run mainly on battery power. For long trips, the $8 per gallon gasoline of the year 2000 will be rationed, and hoarded ration coupons splurged for the occasion.

The Jet Propulsion Laboratory is running a $10 million program for the DOE to develop hybrids. These vehicles do not have the range limitations of all-electrics and may save up to 80 percent of petroleum by letting the batteries discharge during the driving mission. The batteries are charged by biberonnage or overnight, at home. No gas is used on short trips. On long trips a smaller engine is satisfactory, averaging 40 mpg in a full-sized car. Over the course of a year, on-board fuel economy of 50 or more mpg is envisioned for a family-sized car.

The "first" car and "second" car will be all-electric by 2005. We will buy "range" when we buy a car, just as until recently we bought "performance" — that is, horsepower — after basic transportation needs were met. Since range is determined by battery size and weight, it will be ridiculous to buy a "100-mile electric" when 95 percent of the time the car is used for less than 40 miles. The extra battery weight for the 100-mile range will mean a cost of 10¢ per mile for electricity rather than 5¢ per mile. If you buy a car for status, OK; Caddy-electrics will be 120-mile-range vehicles, and Chevette-electrics will have 40-mile ranges.

I predict that biberonnage outlets will be widespread in

public parking areas. There is no reason why they cannot be installed in a supermarket parking lot, as shown above. The hosts at dinner parties will set out a long charging-outlet strip for the guests' cars in the driveway. The electricity consumed by those cars will cost only about 1/10th that of the drinks and wine consumed by the guests.

I will not belabor the point. Electricity is a fuel, or secondary source of energy, available almost anywhere in an industrialized society. The primary sources of energy to generate electricity are abundant, and some are inexhaustible. A ton of coal converted to electricity by burning in a power station will drive an electric car twice as far as will the same ton converted to synfuel to drive an ICE. Synfuels will be out for private transportation.

How soon can we implement electrics and hybrids? As soon as political pressure is great enough. That means we need the public to demand it, or at least to applaud the politicians' statements.

A member of the EPA said to me when my company was developing a hybrid for low pollution, "Don't waste your money. Even if the car works, so what? If Detroit won't build it, what good is it?" He was right. The car worked, had low emissions and good fuel economy. Detroit said, "Ho hum." When petroleum *really* begins to run out, they'll say, "Ho ho" to electrics and hybrids.

Finally, will there be any conventional automobiles, ICEs, or diesels? Yes, again for societal purposes — police, fire, military, sanitation, ambulances, for example. Maybe one of the more salutary results will be the drop of crimes such as bank robbery, because of the unavailability of high-speed getaway cars. I end on that upbeat note. □

New Materials
Atomic-Scale Architecture
of Metallic Solids

by WILLIAM L. JOHNSON

The world of solid objects can be divided into two categories according to how the atoms are arranged, whether in a regular periodic array called a crystal or in some other more random structural form. The overwhelming majority of solids that you see around you are noncrystalline — our bodies, this paper, the vinyl you are probably sitting on. But nearly all metallic solids, on the other hand — steel, aluminum or any of the common metals that we're familiar with — are invariably found to occur in crystalline form. There *are* also noncrystalline metals — metallic glasses. In this article, I will discuss how these are formed, what sort of properties you would expect them to have, and how atoms in such metals are arranged.

First, how are atoms arranged in crystals? Most of us are familiar with crystals, with things like diamonds, for example, which have well-defined geometrical shapes. You can recognize a crystal by its highly symmetric form, which is due to the fact that the atoms in a solid crystal are arranged in some regular fashion — in a periodic array. This was recognized in the 19th century. In an idealized crystal, the atoms sit on a regular array of sites. We can also have material consisting of many crystals that are conjoined along what are called grain boundaries. In each separate crystal the atoms are still situated in perfect array. Most metals that we deal with in the practical world are in this polycrystalline form.

What else can happen in a metal? Things aren't always in perfect array — there can be disorder or things out of place. It's very common to have vacancies in metals. A vacancy is a lattice site where one of the atoms of the crystal is missing. The effect of a vacancy goes beyond that of just taking out an atom. Taking out one atom tends to affect those around it as well; surrounding atoms shift their positions.

In the real world few materials are pure. Most materials consist of a mixture of atoms, and there are several ways in which atoms can mix together to form a crystal. One way is a substitutional solid solution, in which a foreign atom substitutes for a host atom in the perfect crystal. If it is bigger than the host atom and takes up a little more space, all the surrounding host atoms have to shuffle around to make room for it. So again, the influence of putting in the foreign atoms extends beyond the foreign atom itself and affects the neighboring atoms.

A second way in which foreign atoms can enter a crystal is through an interstitial solution. When the foreign atom is too small to substitute for host atoms, it can occupy instead one of the interstitial sites — the space between the host atoms of the crystal. Surrounding host atoms again react by moving away; since the interstitial site is small, they're pushed out slightly to accommodate the foreign atom.

As examples of these situations, a perfect crystal could be paralleled in the real world by a single crystal of iron. Stainless steel is a substitutional solution, and an example of an interstitial solution is carbon steel, or high strength steel.
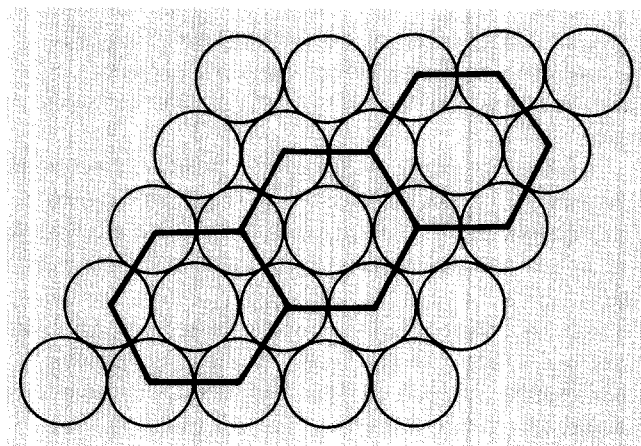
Actually, these perfect arrangements never occur in the real world either, because materials are at some finite temperature, typically ambient temperature, but sometimes higher temperatures, depending on the situation. When there is thermal energy available — that is, at higher temperatures — these atoms don't just sit at their equilibrium positions but move and rattle around somewhat. If we heat a metal hot enough and get very close to its melting point, we can still have a crystal with a periodic array of atoms, but it is now a highly agitated or disordered crystal. But when we melt a crystal, we lose that regular arrangement of atoms. In metallic glasses the atomic-scale architecture is similar to that of a liquid.

What do the atoms in a metal actually look like? Probably the best description of an atom in a metal is a round ball. That's not true of all atoms, and it's not true of metal atoms in all circumstances. The carbon atoms in a diamond are not well described as balls. They have some very specific kinds of geometrical shapes, which are de-

New Materials

fined by the way in which the atoms are bonded to their neighbors. In other words, not all atoms look round; some have very strong directionality in space. They have chemical bonds that stick out in particular directions and join them to their neighbors. Metal atoms generally don't do that. Basically, metal atoms are spherically symmetric. They have a round shape and they see their environment in a very nonpreferential and nondirectional way.

To understand why atoms in a metal arrange themselves the way they do, probably the best concept to start with is to think of how to put balls together in space to fill space most efficiently. That is, how could we take a random collections of balls and put them together to fill space in a way that would leave the least amount of unfilled volume? Let's assume we have a pure metal, iron, for example, with one kind of atom, and that we can think of it in two dimensions, even though it exists in three. The iron atoms prefer to pack themselves in a way to fill space most efficiently and consequently adopt a hexagonal close-pack geometry with each atom having six nearest neighbors.

Seen in two dimensions, the atoms of this "perfect crystal" of iron arrange themselves most efficiently by lining up with six neighbor atoms, creating a neat hexagonal symmetry.

What if we were to put foreign atoms in this perfect hexagonal crystal to form a substitutional solution? The foreign atoms that might be interesting in the case of an iron crystal would be atoms like nickel or chromium, which are the typical additions used to form various types of steels. Chromium atoms reduce the tendency of the iron crystal to oxidize, but chromium is a very expensive material and stainless steels are expensive to make.

We could also put interstitial atoms into the iron crystal. The one most commonly used is carbon, but other additions such as nitrogen and boron atoms are also used. Car-

bon atoms are considerably smaller than the iron atoms of the host matrix. The carbons tend to go interstitially and occupy the holes between iron atoms, creating steel. Steel is typically used in applications where a cheap, high-strength material is needed.

What makes steel strong? A pure piece of iron subjected to a force — say a shear force — deforms fairly easily, precisely because of its periodic arrangement. The atomic arrangement is along lines, called slip planes, along which the atoms can roll very easily. When you apply a shearing force to a perfect crystal such as iron, it will slip or deform along those planes, the atoms behaving like a string of ball bearings rolling over each other.

When you introduce carbon into the crystal, the configuration changes. Carbon is not really a metal atom. It doesn't look like a round ball; it forms chemical bonds with a strong directionality to the neighboring iron atoms. In two dimensions it looks like a little spur locked to its neighboring iron atoms. In three dimensions the carbon atoms occupy octahedral holes in the crystal, but they form the same kind of directional bonds and the same kind of considerations come into play. What the carbon atom does is prevent motion along the easy direction lines. If you try to deform this metal by pushing with a shear force, the carbon atoms will prevent the slip planes from moving with respect to each other, making steel stronger than the iron. A pure piece of iron has a strength of about 50,000 pounds per square inch (psi). With 1 percent carbon in it that typically changes to 150,000 psi. Even this small amount of carbon has a very dramatic effect on the ability of metal to resist being deformed. It increases its strength by a factor of three.

So if we wanted to make strong materials, materials that would resist wear, for example, or which would be useful in structural applications, the obvious thing to do would be to put more carbon in them. It turns out that that's not easy to do because nature is not very accommodating. The situation is similar to a common kitchen problem — dissolving salt in water. At room temperature water will dissolve only a certain amount of salt. When you try to put more salt in, it doesn't dissolve. Heating the water increases the ability to dissolve the salt. When it cools, the excess salt will crystallize out again.

Solid metals are much the same. At low temperatures, such as room temperature, the ability of iron to dissolve carbon is restricted to less than 1 percent. If you heat iron up to high temperatures, it will dissolve a little bit more carbon — about 1½ percent. If you then cool it down slowly, the carbon atoms like to leave the solution in the form of iron carbide.
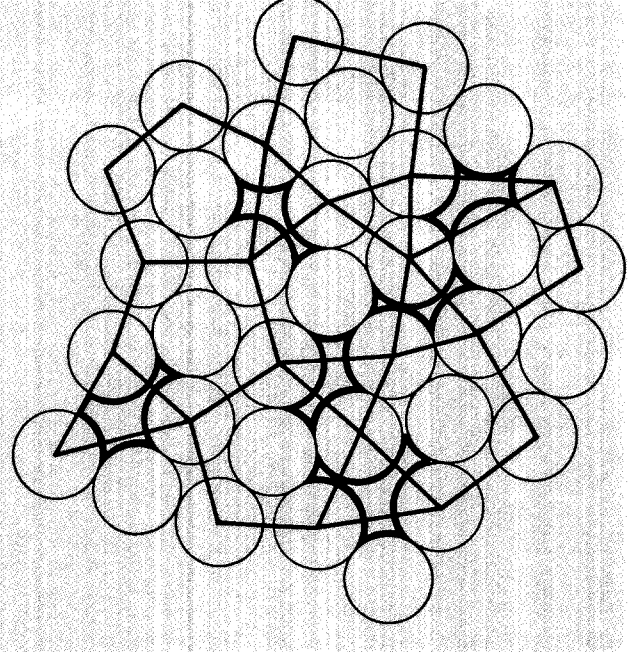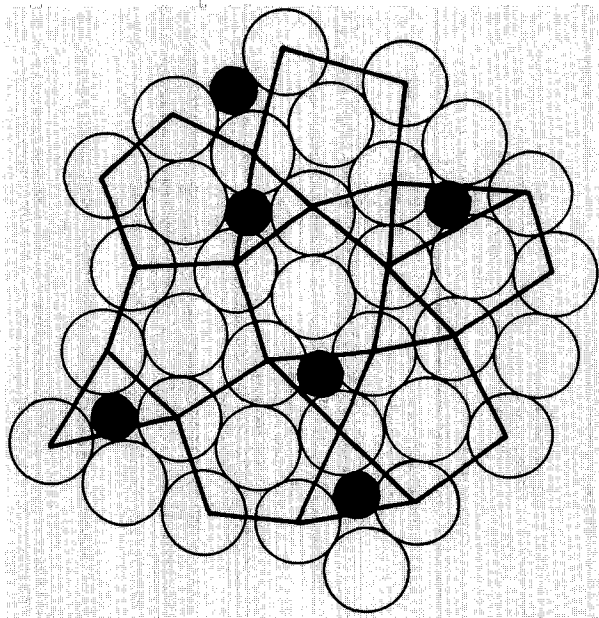
In a liquid metal, which has expanded, there will be many atoms that have only five neighbors. Conjoining five-sided figures doesn't work out as neatly as the hexagonal configuration; when you try to arrange the atoms in a regular periodic array, they fit together only by leaving holes, or defects — indicated here by the triangles between pentagons over the heavier lined spaces.

However, if you cool the iron crystal quickly enough, the carbon atoms might have too little time to find their way out. This is what a blacksmith does when he quenches a piece of iron. He loads it up with carbon by heating it in a furnace and then quenches it in water to retain the carbon. Roughly speaking, the more carbon he gets in, the stronger the material gets.

But you can only go so far. Even if you heat a piece of iron to its melting point, it still dissolves only about 1½ percent carbon. So there's a limit to how strong you can make iron by that method. In recent years scientists have found various other methods of getting around this problem by putting the carbon in by brute force; you just stuff the carbon in. One such technique is called ion implantation, in which carbon atoms are ionized, stripped of a few of their electrons, accelerated with an electrostatic field of very high energy, and then allowed to ram into the surface of the metal. Depending on its energy, the carbon can go quite a distance into the surface. But the distance is limited by the amount of energy you can give the carbon. Practical considerations restrict that. Typically in this method the surface of the iron sample becomes impregnated with a high concentration of carbon, but the carbon atoms are unable to penetrate beyond the surface.

This technique has been studied by several researchers and shown to be very effective in producing extremely hard surfaces on iron. There are applications in which only the hardness of the surface is important, for example, a very precise ball bearing. A very small amount of wear will render the ball bearing useless anyway. In this case hardening just the surface will increase the working life of the bearing.

Ion implantation is useful, but nevertheless restricted to the surface of materials. How can we beat the problem of getting carbon (or boron or nitrogen or any of these other small atoms that make iron very hard by occupying the interstitial holes) into bulk iron? In a perfect crystal the atoms arrange themselves to pack most efficiently in space. A solid metal will seek out this arrangement because it is the lowest energy state of the system. There are, however, many other ways to arrange atoms in two-dimensional or three-dimensional space that are still fairly efficient. One way is suggested by the phenomena that occur when a metal melts. A liquid metal no longer forms a lattice; it no longer packs atoms together in this highly symmetric arrangement. In a liquid metal there are many atoms that don't have six nearest neighbors; they could have just five (in two dimensions).

If we start with an atom and give it five neighbors on a perfect pentagon, we would have to conjoin that to a

second pentagon to fill space as shown in the figure above. Then we conjoin a third pentagon (a five-fold coordinated atom) and so on. With the hexagon we get right back to where we started from, and everything works out to give a perfect crystal. When we try to do the same with five atoms, we wind up with small triangular holes left over, as seen in the figure above. In a two-dimensional array with five neighbors, these holes will always occur as a basic kind of defect.

This same problem occurs in three dimensions. To visualize our hexagonal close-pack structure in three dimensions, the six-neighbor-packed planes can simply be stacked on each other. Gold and copper crystals look exactly like this — a sequence of six-neighbor layers. In three dimensions, five-fold symmetry is obtained by placing twelve nearest neighbor atoms around a central atom to form an icosahedron, a three-dimensional object whose faces are pentagons. The holes, or defects, also occur when icosahedra are used to fill three-dimensional space. Five-fold symmetry is linked up in a very fundamental way to the formation of metallic glasses, or noncrystalline metals, because perfect crystals cannot be formed from objects which have five-fold symmetry.

You might wonder why five-neighbor arrangements would ever tend to form. Why don't metals just exist in the neat six-fold packings? Under normal conditions of thermodynamic equilibrium, where nature is left to run its course, the atoms in solid metals are always found to form periodic arrangements. Something a little different happens when a metal melts. Because of all the extra thermal energy, the atoms of the metal tend to assume a highly disordered arrangement. In liquid metals — as in liquids generally — it is possible to form five-neighbor arrangements.

The holes in the liquid five-neighbor packing offer little extra spaces to put carbon or boron atoms (dark circles), forming an alloy, here $Fe_{80}B_{20}$ — 80% iron, 20% boron. This also stabilizes the configuration and makes it more competitive with the six-neighbor packing of the solid crystal.

Going back to our iron and carbon problem, we see an obvious way to exploit this atomic structure of liquids. A liquid will generally contain extra little spaces (or defects) in which to put carbon atoms, as shown in the figure above. In fact, liquid iron and other liquid metals can dissolve much carbon — 20 percent or more! We can't achieve that situation in a solid metal since the solid likes to seek out its lowest energy arrangement, which has six neighbors and little solubility for carbon. But carbon (or nitrogen or boron) fits nicely into the defect holes of a liquid metal. If we fill each of the triangular defects with a small atom, we have an alloy that fills space very efficiently. When these smaller atoms are added to pure iron, another effect is to lower the melting point. The liquid begins to compete a little bit more effectively with the six-fold neighbor solid state, since we have now filled space more efficiently.

But we're still in trouble, because if we cool the metal down far enough, it will still crystallize. In the case of carbon in liquid iron, the carbon will come out and form an iron carbide. It will leave the iron behind with only a little carbon and put us back where we started. If we go to low temperatures and solidify the material, we always lose the five-neighbor arrangement of things.

One factor we can work with is that atoms move at some finite velocity, so it takes them a certain amount of time to go a given distance. If we were to cool a liquid iron-boron (or carbon) solution very rapidly, so rapidly that the boron atoms didn't have time to get out, then we could possibly retain the desired kind of structure in a solid. If we succeed in that, then we can form a metallic glass, a noncrystalline metal, for example, a glass consisting of 80 percent iron and 20 percent boron (or carbon). Comparing this to the crystalline metal with only 1 or 2 percent carbon, we might expect to have produced an extremely strong material.
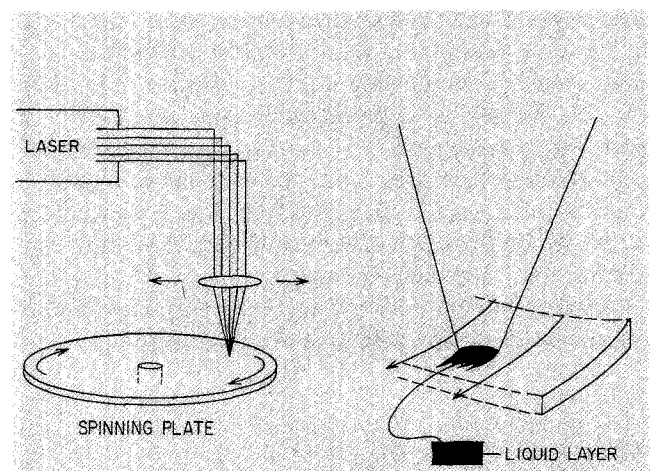
How would one go about actually cooling something fast enough to do this? About 20 years ago here at Caltech, Professor Pol Duwez showed that by rapidly cooling a liquid metal, by sandwiching it between two highly conductive pieces of cold metal and "splatting" it out into thin foil, it is possible to retain this glassy structure. A radio-frequency coil heats the liquid metal in a crucible. When a drop of liquid metal falls, it triggers a detector, and the falling drop is captured by a piston, which impacts at a very high velocity, literally splatting the drop between two copper surfaces. Copper is a very good conductor of heat. If you spread the drop out and make it thin enough, it takes very little time to remove the heat. The metal can be cooled at the rate of a million degrees per second.

To deform an object made of metallic glass would be extremely difficult for two reasons. The carbon (boron) atoms bond with the neighboring iron atoms like little braces and make the structure very rigid. Also since the atoms are no longer periodic in space, there is no direction along which the metal can give easily; that is, there are no slip planes along which the material deforms easily as it did in the case of crystalline iron. In the crystal, carbon atoms lock up the iron, but the crystal can still slip along the plane. Now we have even gotten rid of that problem. There is no preferred direction for the slip to occur.

The strength of many iron-base metallic glasses is between 400,000 and 500,000 psi. As mentioned earlier, the strength of pure iron is 50,000 psi, and a very high carbon steel is 150,000 psi. In a technological sense, we've made a leap forward in improving the strength of these materials. We've found a method of getting a high carbon concentration everywhere in the sample. This process produces a uniform, homogeneous material in contrast with techniques that add carbon to the surface only.

Of course, if you want to apply this technique to a practical problem, you are immediately faced with the question

Laser glazing can create a glassy metallic surface by melting the skin of a material and cooling it at a million degrees per second.
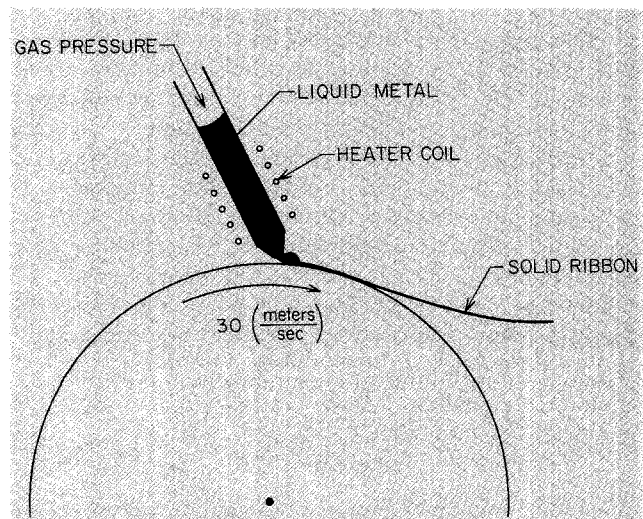

LASER

SPINNING PLATE

LIQUID LAYER

of how to produce a useful material in sufficient quantity by slapping drops between two plates. Such a process would not yield enough material to serve most practical applications. So the next obvious hurdle was to figure out how to cool metal with a lot of carbon or boron in very large quantities at a million degrees per second. Some of the ways this is currently being done incorporate technology that has been developed in the last decade.

One process employs a laser. Laser light is focused to a very fine point on a plate spinning at a very high velocity. As the material spins by and passes under the laser beam, it's melted very quickly, forming a little liquid pool on top of the cold plate. As this passes out of the laser beam, we have the same sort of situation as the drop being spread very thin over a copper surface. With the laser technique, the heat is conducted very rapidly into the material itself. The plate below the liquid skin remains cold, so the material solidifies very rapidly. With this technology a cooling rate of a million degrees per second can again be achieved, and an amorphous or glassy layer two or three thousandths of an inch thick can be made on the surface of a metal object. This is useful for making objects that resist wear well. Unfortunately, we're still on the surface, even though in a better position than with ion implantation since with that process we were limited to much thinner layers.

The Allied Chemical Corporation pioneered a new technology back in about 1971 and 1972, in which they studied the formation of metallic glass using what is called a melt spinning technique. In melt spinning the metal is melted with a radio frequency coil or heater coil, and gas pressure then forces the liquid metal onto a wheel that is rotating at extremely high velocity, say a surface speed of 30 meters per second or about 60 miles per hour — like a car down a freeway. The metal is pulled off by the rotating wheel into a thin layer and spun off as a ribbon. Miles of it can be produced in a very short time. This process works

Melt spinning produces metallic glass ribbon in large quantities drawn off and cooled by a rapidly rotating wheel.

extremely well, and the formation of a melt puddle with the ribbon coming off is an extremely stable configuration. An iron-based alloy or steel can be continuously cast at the rate of a million degrees per second.

These materials are extremely interesting from a technological point of view. General Electric Corporation is now fabricating ribbons of amorphous iron alloys that have very interesting magnetic properties. Cross-country power transformers step up the voltage on transmission lines to allow electrical power to be distributed over large distances. These are currently made from a crystalline alloy of iron and silicon; it's a very expensive material to produce and requires a lot of processing. The amorphous alloy spun off the wheel is a much better magnetic material for this application because it's magnetically softer. This means that the transformer made of metallic glass ribbons would dissipate less energy. In fact, you can reduce the power loss from the transformer by as much as a factor of 10. Since a significant amount of electrical power is lost every time power passes through one of these transformers, reduction of the loss could save hundreds of millions of dollars worth of energy every year. General Electric hopes to replace cross-country power transformers with transformers made from metallic glasses, by simply winding up ribbons to make transformers.

There are a number of other interesting applications and a variety of other unusual properties, some of which we're currently studying here at Caltech. Metallic glasses resist corrosion extremely well — much better than stainless steel. Many of the metallic glasses become superconducting at low temperatures. A superconducting material is a metal that at temperatures near absolute zero loses all its resistance to the flow of electricity. Many crystalline metals also have this property, but the glassy superconductors, such as alloys of molybdenum-ruthenium-boron, remain superconducting in extremely high magnetic fields. This is a very useful property in the construction of superconducting magnets and the building of magnets that will generate very large magnetic fields. The metallic glasses are also extremely strong and have very desirable mechanical properties. Crystalline materials that are good superconductors on the other hand don't generally have desirable mechanical properties.

Our group at Caltech is currently involved in research on many diverse aspects of metallic glasses. This is still a comparatively new field, and the technological impact of these materials is just now beginning to be felt. It's very probable that in the coming years the number of applications for these materials will expand at an exponential or geometric rate. ☐

Research in Progress

The View from Tibet

I ndia and China have been on a collision course for some time — not just ideologically as in recent years but quite literally, physically, for the past 200 million years. That's when, geologists now think, India broke off from Gondwanaland — the southern hemispheric supercontinent — and began drifting northward, finally slamming into Tibet about 40 million years ago. Last October, Professor of Geology and Geophysics Clarence R. Allen was chairman of a 10-man American delegation to Tibet, the first such group allowed into the region to observe the geological evidence of this collision, which created the Himalayan Mountains.

Unlike most mountain chains, the Himalayas, which are the world's highest mountains, sit squarely in the middle of a continent. It has been only in the last 20 years that a major revolution in geological concepts, plate tectonics, has enabled scientists to figure out what, Allen says, schoolchildren have long pointed out — that the Himalayas look like the folds of a tablecloth pushed forward between two fingers and bunched up at the corners.

Another geographic anomaly has also been known for many years — that two of India's major rivers, the Indus in the west and the Brahmaputra in the east, both rise in Tibet on the far side of the Himalayas, actually within a few miles of each other. Before flowing southward at opposite ends of the great range, they describe a broad linear zone characterized by a distinct belt of rocks. Allen wanted to find out what Chinese geologists had learned about these rocks and what the area might reveal in support of recent geological theories.

According to plate tectonics, molten rock is constantly flowing up from the earth's mantle along mid-ocean ridges; it solidifies and moves sideways away from the ridges until it is forced to dive, or subduct, under the relatively stable and rigid continental plates where it is reabsorbed into the mantle. This process can be traced by magnetic signals along the ocean floor, where the irregular flipping back and forth of the earth's polarity has left an important record, recognizable in distinct magnetic patterns in the newly formed crustal material. These characteristic magnetic stripes can be read like tree rings and allow geologists to reconstruct the history of the ocean floor.
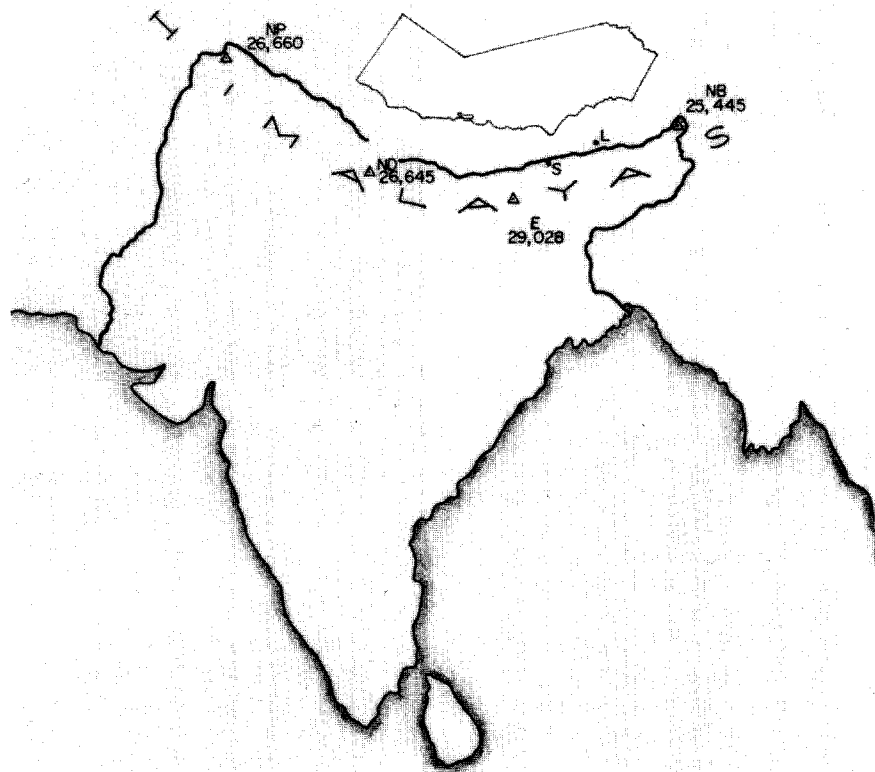
Although the floor of the Indian Ocean is more complicated than, for example, that of the Atlantic, which has a single spreading ridge, India's northward path can be traced through these magnetic clues. They show that between 80 and 100 million years ago India was approaching

the Asian continent; crust was subducting in a trench at what was then the Tibetan coast. But since a continental plate is too light to subduct (it rides like the froth on a wave), as the two continents neared each other, a lot of things began to happen. There is controversy over what actually did happen at the sub-crustal level, but Chinese observations of the rocks in the "suture zone" in southern Tibet substantiate the view that this is the primary line of impact.
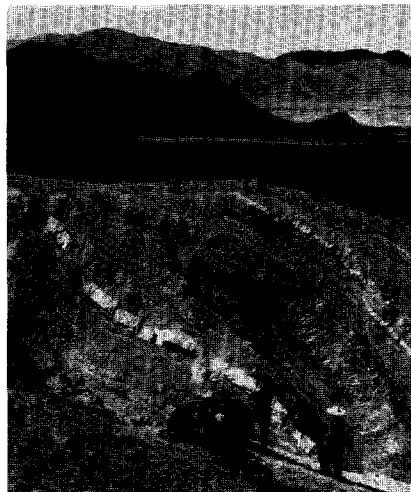
In addition to rocks more than 200 million years old, formed when India was still a part of Gondwanaland and now mangled and squeezed from impact, and granite in the Trans-Himalaya similar to that of California's Sierra Nevada, Allen observed ophiolites — rocks unique to the ocean floor. Examples of these ophiolites in the suture zone were pillow lava, molten bubbles extruded from submarine volcanoes; red cherts, porcelain-like siliceous material characteristic of deposits on the deep ocean floor and datable by fossils; and harzburgite from the mantle itself, rich in iron and magnesium. The presence of this unique set of rocks makes geologists certain that this area, now 15,000 feet high on the inland side of a mountain range, was once the floor of the Indian Ocean, squeezed and uplifted in the collision.

Even after the last gap of ocean was closed when the Indian plate rammed into Tibet 40 million years ago, India did not stop moving. It has continued to advance another 1,500 kilometers, creating in its forward push the highest mountains in the world. But even if you could iron out all the folds in the Himalayas, you couldn't account for the entire distance. Allen and other geologists suggest that the crust in Tibet is so thick that it is capable of transmitting clear up to Mongolia stresses that otherwise would have accumulated along the suture zone. The major earthquakes along faults throughout China are a means of relieving these pressures, which are still building up.

Allen, who is known primarily for his earthquake expertise, hopes to return to China next year to study the Red River fault in Yunnan to try and gain a better understanding of these stresses. □

The Indus River (left) and the Brahmaputra (right) rise quite close to each other and flow along the suture zone where India joined Asia 40 million years ago, piling up the Himalayan Mountains from the force of the collision. Some of the higher peakes are indicated with their elevations; the American geologists traveled between Lhasa (L) and Shigatse (S). California has not broken off North America and migrated to Tibet but is placed on the map for scale.

Folded sedimentary rocks near Shigatse provide evidence of the continental collision of India with Asia.

These pillow-shaped rocks were once a part of the Indian Ocean floor where they formed from cooling lava.
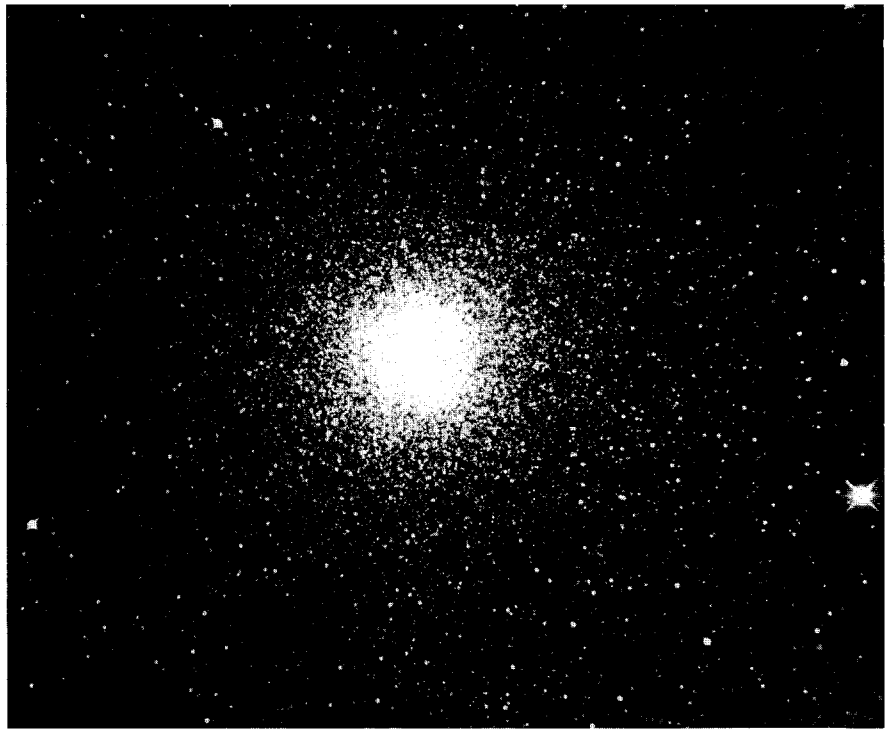
Research in Progress

Galactic Genesis

Globular cluster M3, an intermediate distance out in the Milky Way's halo, was photographed in 1910 at Mount Wilson. Clusters nearer the center of the galaxy are more difficult to distinguish from the field of background stars, and only with recent advances in technology have the spectra of their stars been analyzed, overturning theories that these had a considerably greater abundance of heavy elements and were younger.

Most of the billions of stars in the Milky Way are concentrated in a flat "pancake" or disk that is thought to have been formed by the collapse of a spherical cloud of gas. The disk is surrounded by a spherical halo that extends out about 65,000 light years and that contains relatively few stars. Most of the stars in the halo are closely packed together in globular clusters of up to 100,000 stars each. The halo and its globular clusters, of which about 150 are known to exist, are considered remnants of the primordial gaseous sphere. Astronomers believe the stars in globular clusters to be the oldest in our galaxy and have looked to them for clues to its chemical history. Those globular clusters concentrated near the center of the galaxy have been thought to have condensed out of the gas further along in the collapsing process — hence to be somewhat younger — than those out in the halo.

The age of these halo stars can be estimated by the concentrations of elements heavier than hydrogen and helium, which can be determined by analysis of their spectra. It had been generally believed by astronomers that the stars of the globular clusters near the center were richer in heavy metals than the clusters scattered in the outer reaches of the halo, and Judith G. Cohen (Caltech PhD '71), associate professor of astronomy, undertook to find out if this were true.

The stars in globular clusters are very faint, and their spectra are difficult to measure. Astronomers have also been led astray in measurements of presumed metal-rich clusters near the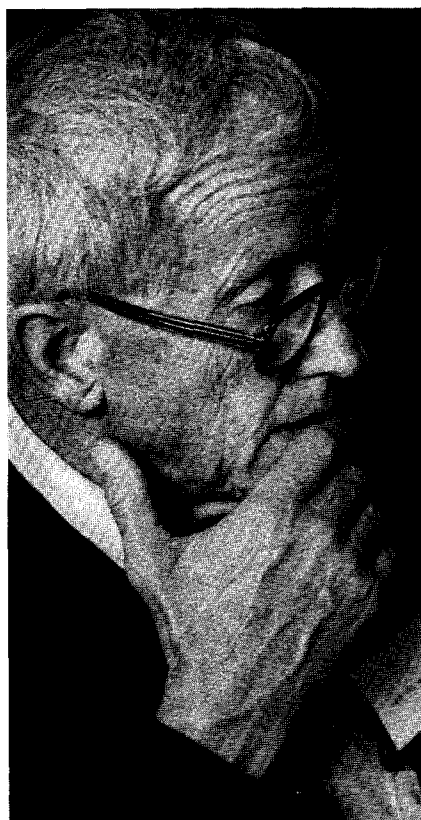 galaxy center by field star contamination; that is, when you're looking at a globular cluster through the Milky Way disk, it's very difficult to differentiate a star that is a member of a globular cluster from the numerous stars in between us and the cluster. So it was not until about four years ago that improved observational techniques — a combination of big telescopes and modern electronics — made accurate spectral measurements of these faint stars possible.

Using these new spectroscopic techniques, Cohen discovered that the metallicity of M71, a globular cluster near the center of the galaxy, was much lower than previously estimated. Recent studies of another such cluster support this finding. Cohen, who has studied several other globular clusters, speculates that all the clusters near the galactic center will prove to be relatively metal-poor, a fact that will change and in many ways simplify the picture of the galaxy's history. The previous picture contained a contradiction — there was a sizable overlap in assumed metal abundance of the close-in globular clusters and a large number of stars in the Milky Way disk, and that implied that they were about the same age. If the formation of globular clusters indeed preceded the final stages of collapse of the gassy sphere into a disk, there should be no overlap in metal concentrations or age. Cohen's picture does not have this overlap. She concludes that there is no correlation between the metallicity of globular clusters and their position in the halo; she believes they formed independently of one another within a relatively short period of time and with widely varying metallicities. The stars in the disk formed after all the globular clusters did, but those disk stars that formed first were equal in metallicity to the most metal-rich globulars.

While appearing to resolve one puzzle, her investigations also present a new one. It has been predicted that the ratios of the different heavy metals to each other would be significantly greater in the older stars. Cohen's observations show no such change in these ratios.

Besides globular clusters in our own galaxy she also studied some in Andromeda, a large spiral galaxy much like the Milky Way. Although the stars are so faint they cannot be studied individually, she believes from the integrated light of all the stars as a whole that Andromeda's globular clusters share the same range of metals, age, and mass distribution within the cluster as the ones in the Milky Way. She is planning observations of globular clusters in the Large and Small Magellanic Clouds, which are galaxies of a very different kind from ours in shape, size, and family of clusters associated with them. □

Max Delbrück

—How It Was

This is the second of two installments of the Max Delbrück Oral History, excerpted from transcripts of half a dozen interviews conducted by Carolyn Kopp under a special program of the Caltech Archives. In the last issue of E&S, Delbrück told of his youth and his early career in pre-World War II Germany, where he was first interested in becoming an astronomer and then turned to theoretical physics. In 1932, however, he heard Niels Bohr's famous "Light and Life" lecture, a bold intellectual step by Bohr suggesting that aspects of quantum mechanics might have applications to other fields. That concept challenged Delbrück and, he says, eventually constituted his motivation to take up biology. Delbrück came to Caltech on a Rockefeller Fellowship in 1937, and when war broke out two years later found himself a de facto refugee in the United States. In the following chapter he discusses some aspects of his life and work in the past 40 years.

Max Delbrück: When I went to Berlin in 1932 — to the Kaiser Wilhelm Institute for Chemistry — my job was to be a theoretical physicist, as it were consultant, for Lise Meitner, a very good experimental physicist working on radioactive substances. I was supposed to keep up with the theoretical literature and watch out what happened, and also presumably be productive as a theoretical physicist and write theoretical physics papers. And I did write a few papers, not very interesting ones — a very *learned* paper with Gert Molière on statistical mechanics and quantum mechanics and an appendix to a paper by Meitner and H. Kösters on scattering of gamma rays. I never heard of the latter problem again until about 20 years later, in the fifties, when I was long since in biology. Somebody told me that there had been published two papers in *Physical Review* on "Delbrück scattering," by Hans Bethe and some graduate students of his who had made some progress in calculating the scattering. So since then this name "Delbrück scattering" exists, and if you ask theoretical physicists then I am known scurrilously for that little incident.

Now, I came to Berlin in the fall of 1932, and during the winter of 1932 and the spring of 1933 was the takeover of power by Hitler, and with it very quickly the beginning of the emigration of a large number of colleagues, especially Jewish colleagues, and the harassment of those who didn't leave; they either lost their jobs, or were not permitted to come to the institutes anymore, or to attend seminars. It was quite ridiculous.

Carolyn Kopp: How did you begin research in biology?

MD: After awhile there was a group of, as it were, exiled — internal exiled — theoretical physicists, five or six of us, who met fairly regularly, mostly at my mother's house, to have private theoretical physics seminars among ourselves; at my suggestion we soon brought in also some other people, some biologists and biochemists. They were Gert Molière, Werner Bloch, Ernst Lamla, Werner Kofink, Kurt Wohl, Hans Gaffron, K. G. Zimmer, and of course, N. W. Timoféeff-Ressovsky, who was a staff member of the Kaiser Wilhelm Institute for Brain Research. We had Timoféeff over to my house a number of times, and we also went to his place just to see some flies, and talked about fly genetics and mutation research. His main line of research at that time was to study quantitatively the induction of mutations by ionizing radiations. In order to do this quantitatively, we had to have quantitative dosimetry of the ionizing radiation, and the person responsible for that was K. G. Zimmer. So out of that grew a rather lengthy paper, which summarized all the experimental data and methods, and then a big theoretical *Schmus* about interpreting it, for which I was mostly responsible. In a crude way one could say that the experimental results meshed together to the picture that the genes were relatively stable macromolecules.

The paper got a funeral first class. That means it was published in the *Nachrichten der gelehrten Gesellschaft der Wissenschaften* in Göttingen, which is read by absolutely nobody except when you send them a reprint. Timoféeff must have sent reprints around to all the major geneticists; when I came to Caltech two years later, A. H. Sturtevant, for instance, was

Max Delbrück

quite interested, although again, he didn't know enough physics. It was all a matter of bridging physics and genetics at that time — there just weren't any people who could do that. Sturtevant wanted to know what was in the paper, and so I gave a seminar here, and he was very pleased with that and said, "Now you have told us exactly what I wanted to know."

So this sort of black market research was going on, I mean it was moonlighting; I was supposed to be the theoretical physics adviser to Lise Meitner, but actually took all this time out to work in biophysics. During that time Otto Hahn and Meitner (who were great experts on radioactivity and the chemistry of radioactive substances for decades) followed up the discovery of Enrico Fermi that you could irradiate uranium with neutrons, and obtain quite a number of radioactive substances with apparently new chemical properties, which Fermi suspected to be transuraniums. Hahn and Meitner picked that up, and indeed discovered that when you irradiate uranium with neutrons, a large number of products arose which could be characterized by their half-lives and by the type of radiation that they gave off. These were interpreted to be elements 93, 94, 95, 96, 97, but very soon it became obvious that there were quite a few more than that, and so they were supposed to be isomers of the transuraniums.

I was very quick in interpreting all of these as isomers of these things, and in retrospect this was really immensely stupid of me; I should have guessed what was really going on, namely fission, but I, like everybody else, lacked imagination to see that.

CK: The theoretical physical problems never seemed to have really caught your wholehearted interest.

MD: Yes, that's true. Well, this wasn't really a theoretical physics problem; it was too trivial. It was something that any experimental physicist could easily have figured out. You didn't need any calculation; all you needed to know was that there was excess energy there; the neutron enters and there is enough energy there to blow the nucleus to pieces. You needed to just be able to add and subtract, and it just
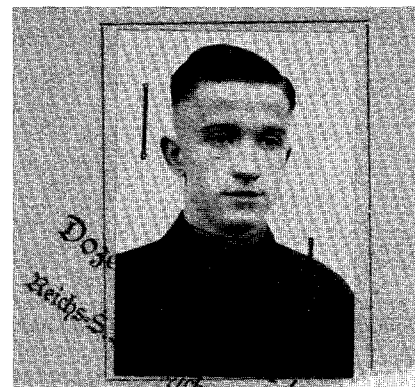
didn't occur to anybody; and it didn't occur to anybody until they were literally forced to this conclusion only the year after I left. I left in 1937 and came here to Caltech and gave here a seminar in physics which then a few weeks later turned out to be everything wrong.

CK: How did your second Rockefeller Fellowship come about?

MD: One day I got a visit from a gentleman of the Paris office of the Rockefeller Foundation, who was just checking up on what former Rockefeller Fellows were doing. I told him what I was doing, and since I was reading this book on population genetics by R. A. Fisher, he suggested, "Don't you want to go to London and study with these people?" And I said, "Well, why not?" And then, however, after I reconsidered, I said, "I'm not really that interested. If I want to do something like a Rockefeller Fellowship I would rather go to Pasadena." And to my surprise he acceded to that without batting an eyelid, and to my surprise Hahn and Meitner — not to my surprise; I knew that I had their good will and friendship — they acceded to it and facilitated it by giving me a guarantee that I could come back and get my job back — that's what Rockefeller insisted on. And so the next thing was to get an exit visa to get permission to leave Germany. Before the Nazis, this problem would not have existed. There was no such thing as an exit visa, but at that time already I guess you needed some sort of an exit permit, because they had reinstituted military service. I was beyond the age of military service; in 1937 I was 31.

CK: You say in the Royal Society biographical questionnaire that one of the reasons that you wanted to go to the United States was because it seemed as if political factors would bar you from further advancement in Germany.

MD: Yes. While I was the assistant of Lise Meitner, I also tried to become a lecturer at the university; this means *Habilitation,* become a *Privatdozent* and obtain a *venia legendi,* permission to lecture, but unsalaried. The Nazis very quickly made

Photograph taken in 1934 for Nazi indoctrination camp.

this procedure more complicated by dividing it into two steps. One, you were supposed to get an advanced degree, as it were, the Dr. habil.; that means essentially presenting all the publications that you have made, demonstrating that you are scientifically, scholarly, qualified. In addition, you were, however, supposed to pass also some political tests. To do so you had to go to a *Dozentenakademie,* an indoctrination camp, which was quite a fascinating thing — a "free" discussion group, you know, where you got lectures on the new politics and the new state. So we had "free" discussions, and after three weeks of "free" discussions they decided whether you were sufficiently politically mature to become a lecturer at the university.

My first one, I think, the very first one that they had run themselves, was at a very nice estate near Kiel. There were about 30 of us, and in a way it was a marvelous thing, because it was the first time in my life I got thrown together closely with people from other disciplines. I learned more about other sciences at this academy and at the next one than anywhere else. But of course there was also the business of having these wonderful lectures by reliable party members, and everybody was terribly nervous because you really didn't know what was going on, and what you could say and couldn't say. Anyhow I obviously was too incautious, and I was informed afterwards that I wasn't quite mature enough but that I could try again.

So I tried again. The next time it was in another beautiful place, Thüringen. There things ran much more smoothly; everybody knew by then what he could say and couldn't say and everything was much more relaxed. But still I must have shot my mouth off. It must have been transparent that I wasn't in great love with the new regime, so I don't know whether I was officially informed that I wasn't mature enough, or whether they just didn't answer my letters. I have forgotten now. Anyhow it was pretty clear that a university career was not likely to be open to me.

So when this Rockefeller thing came around in 1937 it seemed like a good idea to see something of the world and see what was going to happen, because at that time it was anybody's guess how long the mess was going to last. Some people said six months and some people said much longer. I was immensely lucky that I had this opportunity. Many nasty things have been said about those who could have left and didn't leave, like Heisenberg, he's the most outstanding case. I don't agree at all with these derogatory comments. I don't think that it was anything to my credit that I left at all. I think it was a question which could be answered one way or the other, and there is great merit on both sides.

CK: It seems that the choices seem to be much more clear-cut in retrospect than perhaps they were at the time.

MD: Of course, yes. It's not that the choices seem clear-cut in retrospect, but they seem clear-cut to people who have no sense of the reality of the situation. I mean going away was in any case only a chance.

I went via England and visited a Faraday Society meeting in Manchester, I think, and then took a boat to New York. In New York I visited the Rockefeller Foundation offices and then spent a post-season month in Cold Spring Harbor. There I talked mostly to M. Demerec, and learned a little about work on *Drosophila* cytogenetics, using salivary gland chromosomes with their wonderful banding. Demerec also made me do a little experimental work, that is, actually dissect *Drosophila* larvae and fish out the salivary glands and squash them and stain them,

and that's as far as I ever got with *Drosophila* genetics experimentally.

After that month I went West and made only one stop on the way in Columbia, Missouri, to visit Louis Stadler. Stadler was sort of the counterpart to Timoféeff, in the sense that he (Stadler) had discovered the mutagenic activity of ultraviolet light; this is in contrast to the work of the other people who worked with ionizing radiations. Then from there I continued by train and must have arrived in Pasadena on the Santa Fe train one evening in late October. I was met at the train station by one German fellow, George H. M. Gottschewski, a *Drosophila* geneticist, and somebody else. They took me out for a beer, and dropped me at the Athenaeum, and Gottschewski got me all upset, because he said that Thomas Hunt Morgan was very upset about my coming; he didn't know what to do with this theoretical physicist, and really thought it was crazy for a physicist to come. Well, that turned out to be entirely wrong, but it was sufficiently unsettling for me, having traveled 8,000 miles to get here, that I from that day on was utterly confused about north and south in Pasadena.

The next morning, then, I visited Morgan, who was very cordial, and I explained that I had done these somewhat theoretical studies with Timoféeff — Timoféeff did the experiments and I did the theory on mutagenesis and ionizing radiation in *Drosophila* — and that I wanted to learn more about the actual *Drosophila* genetics, and see how the whole subject could be advanced further. Morgan suggested that I should work with A. H. Sturtevant. I talked to Sturtevant, who was also very nice, and he suggested that it would be interesting to try to clear up some confusing results on linkage in the fourth chromosome. He gave me some reprints to read, which I tried and failed to understand. By then the *Drosophila* terminology had become so specialized and esoteric that it would have taken me weeks even to understand all their terminology.

I sat poring over these papers pretty disconsolately for some time in the room across from Calvin Bridges, who was another very wonderful *Drosophila*

geneticist. So I consulted with him quite a bit and became very good friends with him. Calvin Bridges lived a "hippie" type of life — very simple. He had a small frame house here on one of the streets nearby, cooked for himself and occasionally had friends come in, but all very unobtrusive and very friendly. He and I regularly went for lunch together, which consisted of going to the corner of Lake and California and buying there in the market for 10 cents some peanuts and for 5 cents a little bottle of milk, and then we walked back and sat on the bench at the bus stop, and consumed our peanuts and milk and chatted about everything, both science and many other human things. In the Old World I had never met a person so unpretentious in a way that only an American can be unpretentious, although he was a really outstanding scientist. He died a year later.

I consulted with him for quite a bit and tried to learn some *Drosophila* genetics and, as I say, I didn't make much progress in reading these forbidding-looking papers; every genotype was about a mile long, terrible, and I just didn't get any grasp of it. So then one day I read that a seminar on bacteriophage had been given by E. L. Ellis, while I was away on a camping trip with Frits Went, the plant physiologist. I was unhappy that I had missed it and went down to ask him afterwards what it was all about. I had vaguely heard about viruses and bacteriophages, and I had read the paper by Wendell M. Stanley on the crystallization of the tobacco mosaic virus before I had left Germany. I had sort of the vaguest of notions that viruses might be an interesting experimental object for a study of reproduction at a basic level.

Well, Ellis was very cordial and showed me what he had accomplished by then, which was really very impressive; starting from zero knowledge concerned with anything about microbiology, viruses, and so on, he had gotten together very primitive kinds of equipment — an autoclave and a sterilizing oven, a few dozen pipettes, a few dozen petri plates, and some agar — and had taught himself how to pour plates and to use sterile technique. He had gone down to see his

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friend, Carl Lindegren at USC, who was in the bacteriology department, and had gotten from him this strange organism that nobody had heard of before, called *E. coli,* which is now the thing that you hear about in grade school. And he had gone to the Los Angeles Sewage Department and gotten himself a liter of Los Angeles sewage, and from this sewage had isolated a phage active against *E. coli.* With that he had taught himself how to get plates that would produce nice plaques of the phage, and had, in essence, already shown something like a one-step growth curve. I don't know really how far he had gotten with that.

Anyhow I was absolutely overwhelmed that there were such very simple procedures with which you could visualize individual virus particles; I mean you could put them on a plate with a lawn of bacteria, and the next morning every virus particle would have eaten a macroscopic one-millimeter hole in the lawn. You could hold up the plate and count the plaques. This seemed to me just beyond my wildest dreams of doing simple experiments on something like atoms in biology, and I asked him whether I could join him in his work, and he was very kind and indeed invited me to do so. And so I did, after asking some other people like Bridges and Frits Went whether they thought this was a good idea. They encouraged me, so I dropped *Drosophila* and teamed up with Ellis. And that was just marvelous. We had a tremendous time; a tremendous time because it was all really new, at least to us and certainly to everybody in this building (Kerckhoff Labs), and pretty soon we also did a few things that were not generally known.

A few weeks or months afterwards Ellis gave a seminar on phage, and he brought some petri plates along to show these plaques; these were passed around and everybody said, "Ah!" A few days later I met Mrs. Morgan, who also did work in genetics, and I asked her whether she was impressed with these plaques. She said, "You know, the light was very poor. I couldn't see them." It turned out that nobody had been able to see them. Everybody had taken it on faith that there were plaques there, which I thought was quite

hilarious. It reminded me of the story of the emperor's new clothes.

Ellis and I worked together for a year, and after a year, unfortunately and to my great regret, Ellis dropped out of the phage thing, and went back to what he had done before — cancer research on transplantable tumors in mice. Apparently the fellowship under which he worked stipulated that it should be on cancer research. But he came into the lab and certainly continued to take an interest in what I was doing my second year here.

CK: Did you have any trouble renewing your Rockefeller Fellowship for another year?

MD: Not really. No, that was relatively simple. I came in the fall of 1937, and it was renewed to start in September 1938. This ran out after the war had started, which made it virtually impossible for me to go back to Germany; not that I was keen on going back, but it also left me high and dry without visible means of subsistence. For several months I lived on money borrowed from friends.

CK: There was no possibility of a position at Caltech?

MD: There might have been, but Morgan didn't come forward. He thought maybe that wouldn't have been a healthy thing to do; although I'm sure he had a high regard for me — this was not the way he handled things. However, then the Rockefeller Foundation itself took a mild interest, and drew my attention to this job at Vanderbilt. In fact, an arrangement was made by which the Rockefeller Foundation paid half of my salary — the full salary was $2,500 a year — in return for a a gentleman's agreement that I would have half time free for research and would not be just loaded down with teaching physics. So a few days after Christmas of 1939, I left Pasadena and drove East, and arrived in Nashville on New Year's Eve in a driving snowstorm.

I got myself again set up at Vanderbilt in biology. I used the incubator and the sterilizing facilities of the department of bacteriology, which was a one-man department. My room was sort of in a no

Cold Spring Harbor, 1955. The barber is Seymour Benzer, now James G. Boswell Professor of Neuroscience at Caltech.

man's land on that floor between the physiology department and the bacteriology department. I may have gotten my own equipment after a while. I diddled along there, and then, I don't know in what sequence, I was joined by other people.

CK: You met Salvador Luria in December of 1940.

MD: And he did not come to Nashville until nine months later. I don't know whether by then I had some other people working there. Some of the earliest were A. H. Doermann, who had just gotten his degree in *Neurospora* genetics with George Beadle at Stanford; and E. S. Anderson; and gradually we took up contact with A. D. Hershey, who was at that time in the microbiology department of the Medical School at St. Louis.

And Tom Anderson, the electron microscopist; we first contacted him one summer when he was in charge of using the RCA electron microscope at Woods Hole. He had an exhibit instrument there, and collaborated with anybody who wanted to use it. He and Luria had already started in the summer of 1942 working on phage, and I joined them also for a few weeks.

Cold Spring Harbor, 1953. With Delbrück is Salvador E. Luria, with whom he shared the Nobel Prize in 1969.

Actually, it turned out that the findings we made that summer had been made previously by H. Ruska in Germany, but during the war there was very little communication. So the fact that some of these phages had this very odd shape, with a head and tail, and very startling morphology, had been seen in the electron microscope by Ruska, and had been published in the *Naturwissenschaften*. We did it a little more quantitatively, since we paid great attention to controlling two things quantitatively; that is, really control the concentrations of bacteria and phage, and the time in which they interact, so we could be a little more precise as to the adsorption process.

CK: Could we talk about how the first course at Cold Spring Harbor was set up in 1945, when you got the idea for that?

MD: I don't remember who suggested it, but that must have been already the fourth summer then; the first summer that we did phage work in Cold Spring Harbor was

1941, and I think from then on we were there every summer. So in 1945 then we gave this first course, which had a marvelously motley crew of students.

CK: Would you say that there was a sense that you needed to convert people to join in the research?

MD: You mean why did we give this course? I think Luria was the promoter of that. Luria thought that if phage ever was to become an important line of research, and its potentialities really developed, more people would have to be brought into it. And therefore one should make an effort to bring more people into it this way, by giving the course. Anyhow, it helped, even though only a few of the people who took the course actually became phage workers. At least this way we recruited quite a number of people who could read the phage literature with understanding.

CK: When did you hear about O. T. Avery's isolation of DNA as the "transforming principle" in pneumococci bacteria?

MD: Avery made his great discovery in 1943, but we knew about his working on this problem for at least a couple of years before then, and I think both Luria and I had gone to visit with him. And also Demerec knew quite well that there was a very interesting problem. It had been shown that you could use an extract of one bacterium and expose another bacterial strain to it, and then get some kind of transformation, and the transformation was expressed in producing a particular capsular polysaccharide.

The feeling had been that the transforming agent was the polysaccharide itself, that somehow that was sort of a crystallization process, or rather, a nucleation process; you add a piece of this polysaccharide and then more is produced; that was the obvious interpretation at the time. If that was true, then it showed that here you had a genetic property which was not transmitted by genes, but by something more like a whole organism, you might say; like every little piece of polysaccharide was a little apple tree that could grow

into a big apple tree. However, this little apple tree did not contain genes but was just a form principle that had made it possible to accrete more in the same form — more like a crystallization process. If you dump into a saturated solution a crystal of a particular substance, then you can get more of that crystal; it's a nucleation process. And if that had been true, it would not have been so overwhelmingly interesting, because it was obvious that this could not be the general principle of genetics. So it came as a total shock and surprise when Avery and his associates discovered that the transforming principle was DNA. He communicated this discovery to his brother Roy Avery at Vanderbilt, who was in the department of microbiology in the Medical School, in a 17-page handwritten letter, which Roy Avery showed me just about the day he received it, and which I read there standing in his office in the spring sunshine, I think it was. It was quite an amazing letter and has been published.

This discovery, of course, was just the beginning of the battle, because immediately the scientific world split into those who believed that their experiments showed that DNA is an information storage molecule, and those who believed that the DNA preps were contaminated with a small amount of protein, that the protein was the important part. During the subsequent years it was essentially the work of Rollin D. Hotchkiss who gradually tightened the proof more and more to show that the DNA is the essential thing.

CK: I am curious as to whether, when the Watson-Crick structure of DNA came out, there was a general feeling among biologists that this really marked a revolutionary point in biology.

MD: Let's put this question into two questions: whether I thought so and whether there was a general feeling.

CK: I know you thought so. You wrote to Bohr that you thought it equaled the Rutherford discovery of the nucleus of the atom. So you still think so in retrospect?

MD: Oh sure. Easily. The other half of the question — I think there was consider-

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able hesitation as to whether the structure was true. Shortly afterwards there was a Cold Spring Harbor symposium, and some of the more knowledgeable chemists were quite doubtful, (a) whether it's true, and (b) whether it would ever be possible to prove that it was true.

Now it's an interesting fact that there are several aspects to the structure, and people have become aware that the alternative models that have been proposed cannot be dismissed out of hand, that this double-helicity has never been adequately proved. Well, then the next question was, granted that the model is true, is the replication occurring in the way the model suggests; namely, each strand making its complementary strand. And that immediately poses a problem as to how the two daughter double helices are taken apart, how their knots are resolved. And that problem is still unresolved, or incompletely resolved.

Another question was, what do you do with this information that is stored there in the DNA? How do you go from there to really making proteins? And that has been largely resolved in the sense that we know how the amino acid sequences in the proteins are coded for a template code, but here again in the last couple of years it has been found that in eukaryotes, all kinds of monkey business occurs; that the gene that codes for a certain messenger RNA — which then is translated into protein — that this gene contains interstitial pieces that are eliminated later, and the meaning of that nobody knows yet. So there are still surprises.

CK: To back up a bit: When Erwin Schrödinger's book *What Is Life?* was published in 1945, what was your reaction to it? Had you known that he had discussed the model of the gene that you had put forward 10 years earlier, in the paper with Timoféeff and Zimmer?

MD: No, it was a total surprise to me. I had not seen or heard anything from Schrödinger, or by Schrödinger, for years, and when the book came out it was other people who drew my attention to it. I was puzzled how he had gotten hold of the paper, which he obviously had read, and which then formed a central chapter in the

The award of the Nobel Prize in 1969 led to this press conference in Millikan Board Room and to worldwide recognition of Delbrück's pioneering studies of viral genetics — the way strains of the virus bacteriophage infect the bacterium *E. coli* and multiply there.

book. I have recently learned, I think from the historian of science Robert Olby or somebody else, that it was not I who had sent a copy of the paper to Schrödinger, but that P. P. Ewald had shown him a copy.

CK: Did that book have the effect of increasing people's interest in what you were doing then in 1945?

MD: Insofar as it was read by a large number of younger, and not so young, people and physicists, it was publicity for me, although not specifically publicity for phage, more for genetics and for the problems posed by genetics. I mean I didn't need publicity, I would say, but maybe I owe my job at Caltech to it, I don't know. I doubt that I did, because Beadle knew me personally quite well when he offered me the job, and also the people here in the division had seen me around for two years; I don't think they needed Schrödinger's book when the question came up whether they should offer me a job here, which was done in December 1946, and the book came out about a year earlier. But I don't know what went on here.

CK: You were happy at Vanderbilt but were quite sure that you wanted to move?

MD: When the question really came up, to stay or not to stay at Vanderbilt — I mean when the people at Vanderbilt realized that I was very much in demand after the war, and then I got offers from Illinois, and Cold Spring Harbor, and from here, and from Manchester, England — then all of a sudden they tried to really promise me anything, and I think I was quite willing to listen, but I think Manny, my wife, was not, as I recall. In any case,

when the offer from Caltech came, it was irresistible.

CK: You came to Caltech in 1947 and were Beadle's first faculty appointee in biology. I was wondering what changes you've observed in the biology division since 1947.

MD: Well, it got bigger, which is not necessarily fortunate, and its emphasis shifted to chemical biology when Beadle came, more to molecular biology at first; then very soon the psychobiology was added — the Roger Sperry group — and that was an interesting move. This was made possible by a fund that Caltech had received, the so-called Hixon Fund, which was obtained for research that would do something about juvenile delinquency. From year to year the Hixon Committee struggled to find something that could be interpreted as having even the remotest connection to juvenile delinquency, and at the same time be compatible with the general attitude at Caltech of doing basic research. After having struggled for a number of years with that — arranging conferences, having visiting professors, and so on — the committee disembarrassed itself by appointing Sperry as the Hixon Professor, so from then on *he* had to worry about how to reconcile this. (I was a member of that committee.) That was an important move, and the contributions of Sperry have been enormous.

CK: Do you think the division has made an effort to identify new and coming fields?

MD: Well, they considered bringing me here as being a new and coming field, and in recent years certainly they have in

In 1978 Delbrück was speaker for Caltech's commencement, an occasion that gave his colleagues a welcome opportunity to honor him for his many contributions to science and to the Caltech community. *Maximus est,* translated from the Latin, testifies that he is the greatest.

eukaryotic molecular genetics made several important appointments.

Then there was, of course, a period where Caltech went into animal viruses quite strongly. That was initiated in 1950 and, similarly to the Hixon business, came about through a stimulus from the outside — namely, a wealthy citizen who suffered from *Herpes zoster* was persuaded to offer Caltech $100,000 to start working on animal viruses.

CK: After the war you returned to Germany several times.

MD: My first visit back to Germany after the war was in 1947 when things were still very chaotic, *very* chaotic.

CK: What was the psychological state of the scientists that you met at that point? Was there much guilt among the scientists you met who had stayed?

MD: It depended on who. No, if anybody feels guilty, I feel guilty of *not* having stayed, because I have so many friends who I admire for having stayed, and having tried to save what was to save, rescue it across this disaster.

So this was 1947, and then I must have visited a number of times afterward, but the first time I came for longer was in 1954. Then I came for three months and went to Göttingen. I came back for three months in 1956, to Cologne as a guest of Josef Straub, who was a professor of botany and who wanted me to bring molecular genetics to the university there. At that time his institute was still in a bunker in the Botanical Gardens, sort of subterranean caves. At that time the first new university institutes were being built, his among them. In fact, I think I gave a phage course there in this new building.

They had no electric light yet, and no cement floors, but yet we moved in and gave a course there, which was quite a tour de force.

At the end of this stay, they wanted to offer me a job, and I just couldn't see myself moving from Pasadena to Cologne. In the end I made a mistake. Straub said always, "Name your conditions." So the last day I was there I named conditions which I hoped would be so astronomical that the matter would end there. But then due to the fantastic negotiating ability of Straub, the thing finally became a reality in 1961, and we went there from 1961 to 1963.

At that time already all over Europe there were new universities being founded, and similarly in Germany they created a number of new universities. By hook or by crook they involved me in the founding committee of one of them — in Constance — as a consultant for the natural sciences faculty. This led to a natural sciences faculty that was essentially all molecular biology — even the chemistry and the physical chemistry were all molecular biology. We went there at an early stage for the summer semester of 1969. That was my last long stay in Germany.

The German universities have had their revolution like the rest of the world's universities, but I haven't seen much of it. The Max Planck Institutes — the renamed Kaiser Wilhelm Institutes — have expanded enormously; I think they now have 80 institutes, some of them quite monster places — huge places, and I don't think they are as productive as they should be. On the whole I have a feeling that nobody there really knows whither research and education are going to move.

CK: Because things have just been getting larger and larger, and there must be a breaking point, or why?

MD: For reasons as explained in my commencement speech (*E&S*, September-October 1978). The pristine faith in science has been punctured, and it's obvious that science is not going to solve our problems. Science is just as much a destabilizing force as it is a stabilizing force in the world. That's a very general thing. Specifically in Germany it's weighted down with all these problems of institutional lethargy and vested interests that go with it.

CK: Do you find now that — as expressed in your commencement address — you really have strong doubts about pursuing science the way it has been pursued in this country and other countries for the last 20 years?

MD: Yes, the honeymoon is over.

CK: You mean, it's over in that there seems to be a sense that science does not solve all our problems, and there is also a distrust of science by the public?

MD: Even by the scientists. I guess one would like to know more where really our values come from. And so you can ask where do the values come from, and you can ask what should our values be, and if you have an answer to what our values *should* be, how do we get them to be our values. These are not questions of science, but they are the questions, the answer to which will decide the further course of history more than anything else. I think the further course of history will not be decided by further discoveries in science, but by these questions about human values.

CK: Do you think it's possible that science will continue but that scientists will become more involved in value questions?

MD: No. I think the scientist, insofar as he is a scientist, has to do what he did before. Scientific *institutions,* like Caltech, will have to become more involved in value questions. □
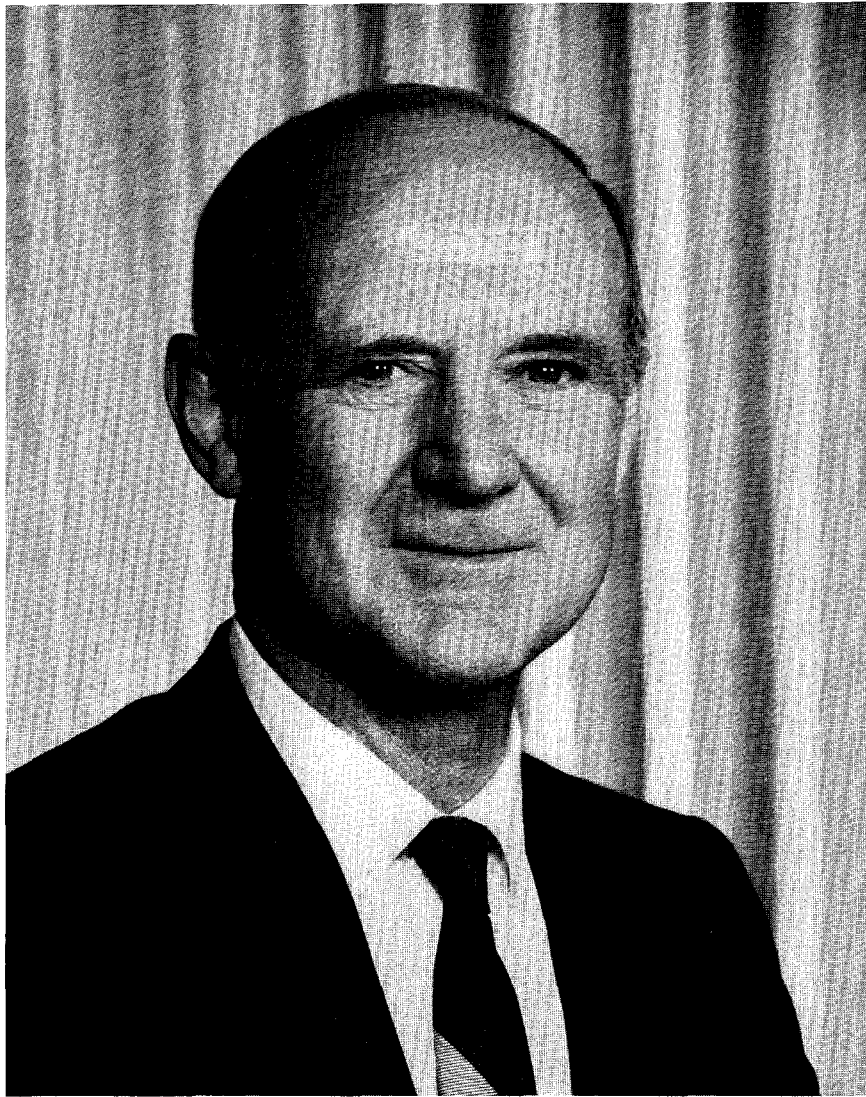
Retiring This Year

JESSE L. GREENSTEIN
Lee A. DuBridge Professor of Astrophysics

Jesse L. Greenstein becomes professor emeritus on July 1 after 32 years at Caltech. New Yorker Greenstein took his AB, AM, and PhD degrees at Harvard and then spent 11 years with the University of Chicago, at Yerkes Observatory. He arrived at the Institute in 1948 to create the graduate school of astronomy in conjunction with the new 200-inch Hale Telescope at Palomar Mountain.

For the next 24 years (9 of them semi-officially and 15 as the duly appointed executive officer) Greenstein diligently and successfully developed the department and observatory staff, served the larger Caltech community in many ways — was chairman of the faculty — and yet found time for distinguished research of his own, sharing, for example, in the discovery of quasars. He studied the properties of interstellar matter, notably its magnetic field, and the emission from gases in radio sources. He is noted for his research on the composition of stars through study of their spectra, the discovery of stars of peculiar composition and the explanations of these compositions from nuclear processes in their interiors. He is now especially interested in the final stage of star life — the white dwarfs.

In addition to professional societies, Greenstein is a member of the American Philosophical Society, the National Academy of Sciences (on whose council he has served), and formerly, the Harvard Board of Overseers. He is the author of nearly 400 technical papers and numerous popular articles. He has been an articulate spokesman for astronomy both to the general public and to many government boards and committees with whom he has consulted. He chaired the Academy Survey of Astronomy and Astrophysics for the 1970s. In 1964 he shared the California Scientist of the Year award with his colleague Maarten Schmidt. He has also received NASA's Distinguished Public Service Medal, the Gold Medals of the Royal Astronomical Society and of the Astronomical Society of the Pacific, and been Visiting Professor at Princeton, the Institute for Advanced Study, and the Bohr Institute. Jesse Greenstein has, in often quoted words, been "sitting up with the universe for 1001 nights" throughout a notable career. He hopes to keep on doing some of that, but also to find more time for other interests — for example, writing.

WILLIAM H. PICKERING
Professor of Electrical Engineering

Just over 50 years after he came as a student to Caltech in March 1929, William H. Pickering retired to become professor emeritus. He received a BS in 1932, an MS in 1933, and a PhD in physics in 1936, and then joined the faculty as an instructor in electrical engineering, becoming professor in 1947.

Pickering came to the Institute from his home in New Zealand, and in more ways than one he kept right on traveling — driving, for example, for six months in Europe as an undergraduate recipient of the Travel Prize. As a young faculty member, he did cosmic ray research with Robert A. Millikan, which meant ranging over most of the world to study geographical variations of cosmic ray counts.

In 1944 he became associated with JPL, and in 1950 he was made responsible for the development there of the U.S. Army's Corporal missile. In 1954 Pickering was appointed director of JPL, and for 22 years his name was synonymous with the development, first, of guided missile systems and then of space vehicles and missions — a considerable leap in travel outlook as well as in technological

achievement. Explorer I, the first U.S. satellite, was launched in 1958, just 83 days after JPL and the Army Ballistic Missile Agency were directed to prepare and orbit a satellite as a response to the USSR's Sputnik. Explorer was, of course, just the first in a succession of unmanned spacecraft developed at JPL under Pickering's leadership. There were also Ranger, Surveyor, Mariner, and Viking.

Pickering has been widely honored for his own achievements as well as those of the Lab. He is, for example, a member of the National Academies of Sciences and of Engineering. He has been awarded the National Medal of Science, the NASA Distinguished Service Medal, the Magellanic Premium of the American Philosophical Society, the Fahrney Medal of the Franklin Institute, the Guglielmo Marconi Award, and a dozen or so others — and he served as Grand Marshal of the 1963 Pasadena Rose Parade. In 1976 Pickering retired as director of JPL, but he has kept right on crisscrossing the globe, spending two years in Saudi Arabia at the University of Petroleum and Minerals and most recently visiting China to lecture on space research.

Retiring This Year

JOHN R. PIERCE
Professor of Engineering

John R. Pierce becomes professor emeritus this month after 9 years on the Caltech faculty — 2 of them as executive officer for electrical engineering. He also put in an earlier 7-year stint as a student at the Institute, earning a BS in 1933, an MS in 1934, and a PhD in 1936. In the 35 years between the two periods he carved out a notable career at Bell Laboratories where he became Executive Director, Research, Communication Sciences Division. At Bell, Pierce was in charge of work on mathematics and statistics, speech and hearing, behavioral science, electronics, radio and guided waves. His chief work was in electron devices, especially traveling-wave tubes, microwaves, and various aspects of communication.

Communication, in fact, is the thing he knows most about. The Echo communication satellite, launched in 1960, was his idea, and the Telstar satellite, which first sent TV across the Atlantic in 1962, grew out of his ideas. In 1963 he was awarded the National Medal of Science for his work on communication satellites, and in 1966 he received one of the first Distinguished Service Awards of Caltech's Alumni Association. He is the recipient of a good many other awards as well, including the Engineer of the Year award of the Institute for the Advancement of Engineering, the Medal of Honor of the Institute of Electrical & Electronic Engineers, the Marconi Award, and ten honorary degrees. Pierce is a member of the National Academies of Sciences and of Engineering, the American Academy of Arts and Sciences, and the American Philosophical Society, and a past member of the President's Science Advisory Committee. He is also a foreign member of the Royal Academy of Science of Sweden.

He is holder of approximately 100 patents, is the author of 15 books and many technical papers, and has been a science fiction author and devotee since he was in high school. (Another high school activity was building and flying gliders.) Among his other interests are Japanese culture, experimental psychology, and music (including electronic composition). He is currently working as chief technologist at JPL, and he has two more books under way — one of them is on the psycho-acoustics of music, that is, what goes into each of us that determines how we will hear music.

HOMER J. STEWART
Professor of Aeronautics

Homer Joe (for Joseph) Stewart came to Caltech as a graduate student in 1936 from the University of Minnesota. He has been associated with the Institute ever since, and this month he becomes professor emeritus. Stewart's 1940 PhD was in aeronautics and meteorology, and he began his faculty career teaching both of those subjects. In recent years his academic activities have primarily been concerned with problems of flight mechanics (including space flight missions) and theoretical and applied aerodynamics.

Part of Stewart's time has also been spent at the Jet Propulsion Laboratory, of which he was one of the founders. For several years he was chief of JPL's Research Analysis Section and participated in many pioneering rocket projects. Later he was division chief of the Liquid Propulsion Systems Division. He has also been a consultant to industry and a number of government agencies, including the Department of Defense and several Senate committees. While on leave for two years, 1958-59, he served in Washington as director of the Office of Program Planning and Evaluation for NASA.

In the early 1940s Stewart was a consultant on the famous Grandpa's Knob windmill in Vermont. While that machine produced more electrical power than any other ever had, it was not an economically feasible energy alternative at the time. Recently, with the rising costs of fossil fuels, the idea of energy from the wind has come round again, and Stewart has been in the vanguard of those trying to improve windmill efficiency and output. He has calculated, for example, that windmill blades up to twice as wide as present ones plus lower blade-tip speeds should increase efficiency up to 10 percent, especially at lower wind velocities.

With his colleague Ernest Sechler, in 1974 Stewart developed a course designed to provide a summary of windmill problems from a systems engineering viewpoint. These two also introduced "Case Studies in Engineering," a course that covered technological and managerial aspects of several large-scale engineering projects in detail. Stewart is a member of the American Meteorological Society, Sigma Xi, Tau Beta Pi, and the American Institute of Aeronautics and Astronautics.

IF YOU MISSED 1957.

If you missed 1957 you missed the Russians launching Sputnik I into earth orbit. You missed seeing a young singer continue his rise to the top of the record industry with a song called Jailhouse Rock. You missed the ballyhooed introduction of a car called Edsel. And you missed the birth of an industry when a group of talented young engineers and scientists formed the nucleus of Fairchild's semiconductor operations.

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