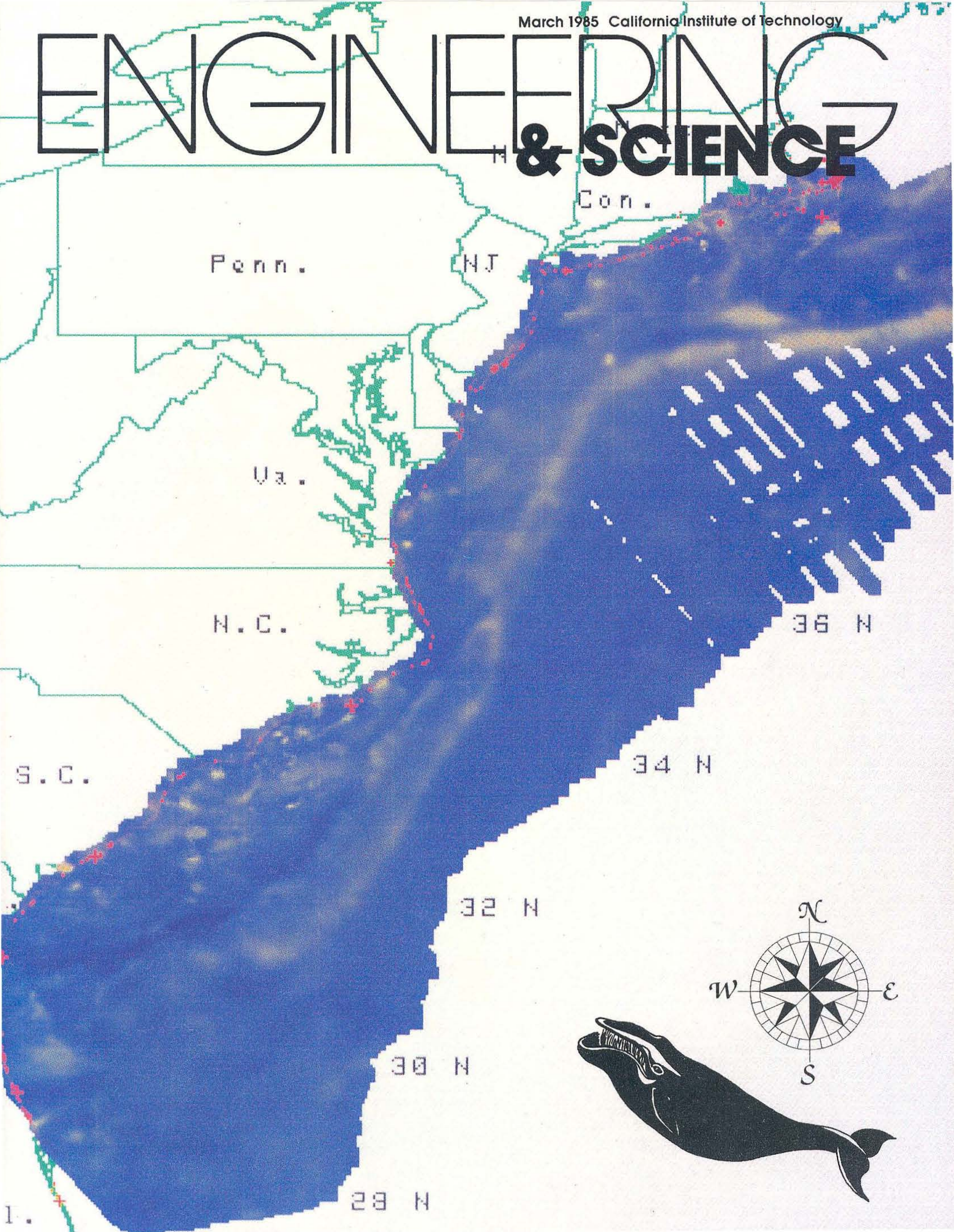
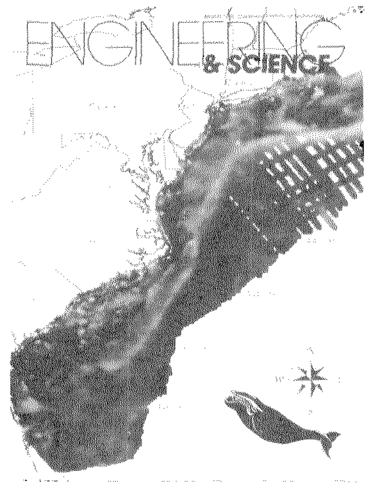
March 1985 California Institute of Technology

ENGINEERING & SCIENCE



In This Issue



Animal Magnetism

On the cover — the geomagnetic field off the Atlantic coast in a false-color image created at the Jet Propulsion Laboratory from U. S. Geological Survey aeromagnetic data. Magnetic field values range from dark blue (low) to yellow (high). Sites where whales have beached themselves are represented by red crosses and dots (based on data from the Smithsonian Institution), plotted on a high-resolution digital representation of the shoreline. (The whale in the corner was drawn by human hands.)

Joseph Kirschvink, assistant professor of geobiology, has found a statistical correlation between whale strandings and magnetic lows — the most remarkable of which can easily be seen here as the dark blue magnetic minima curving down from North Carolina and hitting the Georgia coast, precisely where 53 pilot whales have beached themselves. He explains this in "A Tale of Dead Whales," beginning on page 4, which was adapted from his January Watson Lecture.

Kirschvink earned two of his degrees at Caltech — a BS in biology and an MS in geology, both in 1975. After receiving his PhD from Princeton in 1979, he returned to Caltech in 1981. He was named a Presidential Young Investigator last year.

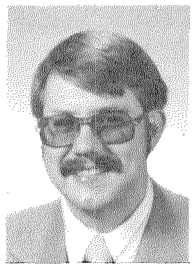
Luminous Birthday

The 75th birthday of Jesse Greenstein was the occasion for a symposium last October on low-luminosity stars. Greenstein, the DuBridge Professor of Astrophysics, Emeritus, has been a pioneer in the study of faint stars, especially white dwarfs, his most recent area of research in a long career. He came to Caltech in 1948 to create and develop the graduate school of astronomy in conjunction with the then new 200-inch Hale Telescope on Palomar Mountain, and he's been a much esteemed member of the Caltech community, as well as an internationally noted astronomer, ever since.

As such, Greenstein has lots of fans. Among them are Virgina Trimble and Judith Cohen, co-authors of long standing, who contributed the report on the birthday festivities (the science is in the report and the festivities are in the photos), "Some Faint Stars and a Bright One," beginning on page 10. Both received their PhDs from Caltech — Trimble in 1968, Cohen in 1971. Cohen is currently associate professor of astronomy here and was one of the conference organizers; Trimble is professor of physics at UC Irvine and is also visiting professor of astronomy at the University of Maryland.

Strange Moon

In introducing his Watson Lecture on "Io: Jupiter's Enigmatic Moon," Torrence Johnson dealt first with pronunciation — whether to call it "Ee-o," as most European languages would, or "Eye-o," as in standard American English. He quoted a JPL colleague, whose solution was not to pronounce it at all, but simply to spell it. While a printed article can avoid this problem, it should be noted that Johnson proceeded to spell it for the rest of his lecture.



Io is peculiar in ways other than its pronunciation — so weird, in fact, that science fiction has no need to embellish its features. Johnson explains some of these features and what has recently been learned about them in his article beginning on page 15. He has been involved with the Galilean satellites ever since his doctoral thesis, currently maintaining that involvement as a senior research scientist at JPL and visiting associate professor of planetary science on campus. He is a member of the Voyager Imaging Team and Project Scientist for Galileo, the spacecraft that will be launched toward Jupiter and its moons next year.

Johnson's PhD is also from Caltech (1970) — making all of this issue's faculty authors Caltech alumni.

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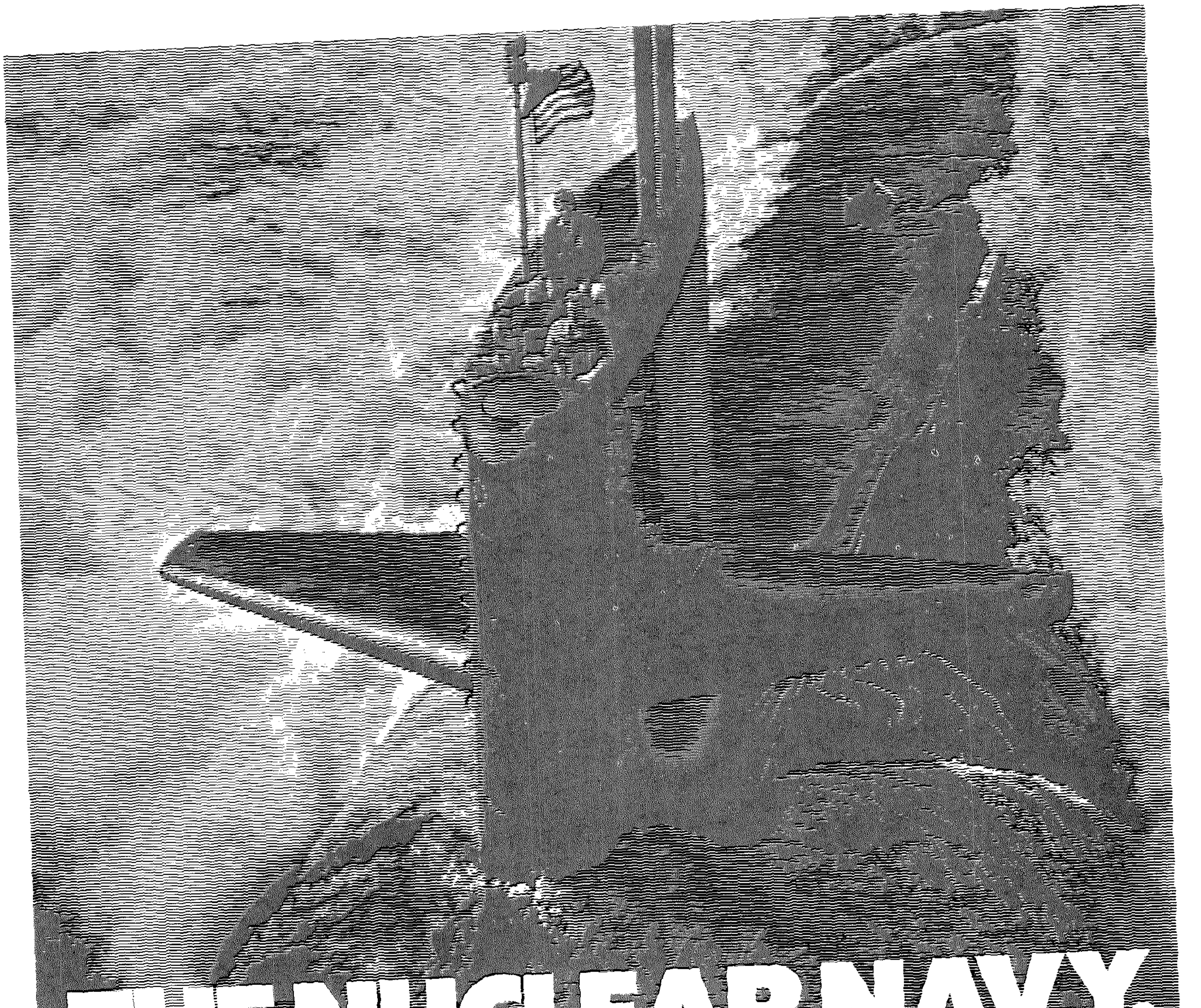

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A Tale of Dead Whales

by Joseph Kirschvink

THE ABILITY of migratory animals to find their way accurately over long distances in unfamiliar territory has puzzled man for centuries. In the past two decades behavioral experiments have shown that some birds and bees, as well as some bacteria and fish, can detect magnetic fields. It seems likely that they are using this sensitivity to the earth's geomagnetic field to help orient themselves and for navigation, but there have been few opportunities to test these assumptions in nature. Our recent studies of dolphins and whales, however, have given us a chance to discover why animals might want to detect the weak features of the geomagnetic field and how they could use them to find their way.

The modern study of biological sensitivity to magnetic fields goes back to the discovery in the 1960s of magnetite biomineralization by Heinz Lowenstam, professor of paleoecology, emeritus. Lowenstam was studying fossil reefs, which are often exposed as mushroom-shaped islands at or near the intertidal zone in the equatorial Pacific. He discovered that the undercut indentations in the rock were not caused, as had been previously thought, by wave action, but by the grazing of a primitive group of molluscs called chitons. These animals scrape algae off the surface of the rock with their long rows of teeth. In order to do this scraping the teeth need to be very hard, and Lowenstam found that they are made of magnetite, an iron oxide mineral that is strongly magnetic. These teeth stick like iron filings to a magnet (*E&S* June 1964). This was the first discovery of an example of biomagnetism.

But it was not known then how organisms make these magnets. In fact, when the phenomenon was first discovered, the petrologists objected because they thought that magnetite forms only at high pressures and high temperatures. They found it difficult to believe it could be produced in the mouth of a stupid little sea animal. Lowenstam and his students have shown over the years, however, that the magnetite grains are indeed true biochemical precipitates, not grains that are picked up in the sand and incorporated into the beast. They have been able to trace iron from the blood through the cells surrounding an anatomical structure called the radular sack, and then into the magnetite crystals that wind up in the tooth. So it's very clear that the chitons are making this stuff, and they make it for a purpose. The magnetite is very hard (it has a hardness of about 6 on the Mohs scale), hard enough to carve away the endolithic algae growing inside the surface of the rocks.

That remained the only known case of magnetite being made by organisms until 1975 when Richard Blakemore, then a graduate student at the Woods Hole Oceanographic Institute, discovered quite by accident that bacteria from the mud of a local pond moved when he waved a magnet near them. It turned out later that a whole variety of these organisms is found all over the earth. Blakemore and his collaborators have since done extensive transmission electron microscopy on these bacteria and discovered that each bacterium has a magnetosome — a linear arrangement of magnetite crystals. It's simply a bar magnet that swims.

These magnetic crystals look very peculiar when blown up to high resolution. They are usually hexagonal prisms — small, irregular cubes — whereas in nature it's typical for a magnetite crystal to occur as an octahedron. We can now recognize these biomagnetic crystals in the fossil record, and by studying these "magnetofossils" we hope to determine the time of origin of the magnetic bacteria. They also seem to be very important in understanding the fossil magnetism of sedimentary rocks.

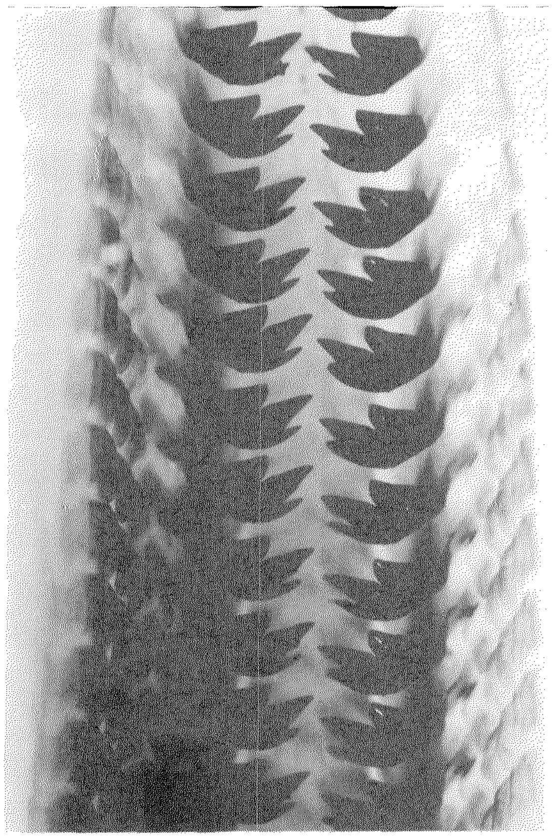
At about the same time, behavioral biologists discovered that bacteria were not the only organisms that were able to detect a magnetic field. Martin Lindauer and Hermann Martin in Würzburg, Germany, did a series of bizarre experiments with honeybees in the late 1960s and early 1970s. Bees normally build their honeycombs in vertical sheets, and somehow a swarm of bees working in the dark can decide the direction in which these sheets should hang. The German scientists demonstrated dramatically that a magnetic compass was involved by putting a swarm of bees in a circular barrel with a large magnet on top of it and letting them build their combs. In the presence of the magnet the bees could not agree on a single direction and produced a cone-shaped blob of wax rather than parallel sheets of honeycomb.

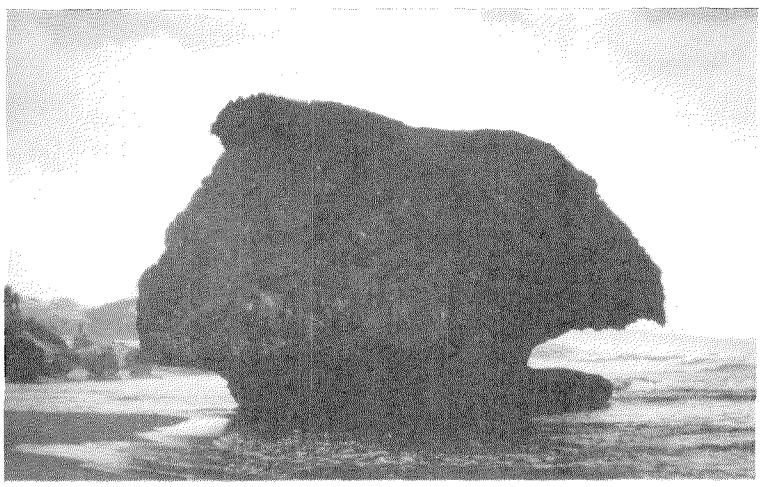
Magnetic sensitivity has been documented in homing pigeons as well as bees. Many of these experiments were conducted at Cornell by the late Bill Keeton and at the State University of New York at Stony Brook by Charles Walcott (now at Cornell). During the 1970s these two groups discovered a variety of magnetic effects on homing pigeons by attaching paired coils to the birds' heads or little bar magnets (or, as a control, brass bars) to their backs. With these coil-and-magnet setups they could change the direction of the magnetic field running through the birds' heads.

When they released the birds and tracked them home, they noticed that on sunny days there was virtually no difference between the birds with magnets and those with brass bars. Birds have a very nice sun compass. By looking at the position of the sun in the sky and comparing it with their internal clocks, they can get a very accurate estimate of where north is. On cloudy days, however, some very interesting things happen. The birds with the brass bars still fly more or less directly home, but the ones with magnets on their backs go off in random directions. They don't know where home is.

Changing the direction of the magnetic field through the head with paired coils also yielded interesting results. If the magnetic field goes down through the head, it approximates the normal direction of the field in the northern hemisphere. So you're actually adding to the present field, and in this situation the birds go preferentially toward home. But if you flip the field so that it runs up through the top of the head, you can make the birds fly away from home.

These experiments on honeybees and homing pigeons suggest that they have a magnetic compass sense. Somehow if the animal





The chiton's long rows of teeth (left) carve fossil reefs into mushroom shapes (right) as this marine mollusc scrapes the rocks in search of algae. The teeth are made of magnetite, an extremely hard, iron oxide mineral that is strongly magnetic.

is deprived of other information, it can figure out which way north is. Another of Walcott's experiments, however, involved releasing pigeons at magnetic anomalies. Normally when a pigeon is released it will fly in small loops before determining the straightest path to its home loft. When Walcott and his crew released pigeons at an iron mine magnetic anomaly (a peak in the local magnetic field, from 2 to 6 percent over the background field) the birds became very confused. A few of them eventually wound up going off in what might be the home direction, but a significant fraction just wandered off and flew randomly around the area, apparently unsure of which way to go. They seemed to avoid local magnetic highs in favor of the lows. This experiment has been repeated several times now, in Italy and Switzerland as well as in the United States, and it seems to be holding up. The birds appear at first to be trying to escape the magnetic anomaly and then trying to figure out where they are. Something about the perturbations in the field is disturbing them in some way, even on sunny days. These birds do actually get home eventually, but they take two to three times longer than does a bird released a few miles away.

How do the birds and bees do it? As an undergraduate at Caltech I worked with Lowenstam on the magnetic properties of chiton teeth and was quite familiar with biomineralization. Later, when I was a graduate student at Princeton, a group of biologists there successfully replicated some of the German bee experiments, and I got the idea that magnetite might also be responsible for their magnetic sensitivity. But how could we test this idea? Geologists routinely look at rocks for magnetic materials, and our favorite mineral in rock magnetism is magnetite. We applied standard geological techniques to this problem and soon had located small amounts of magnetite in bee abdomens and pigeon heads.

We have subsequently developed laboratory techniques for extracting and looking at these magnetic particles. We have some very sensitive instruments, for example, a superconducting magnetometer that uses something called a SQUID (superconducting quantum interference device), which can detect picogram quantities of magnetite. It's so sensitive that it can register the magnetic moments of dust particles in the air. But we needed just the right animal. The problem is that you can't use a metal knife to dissect the animal because the knife would leave a trail of highly magnetic metal particles. It is difficult, however, to conduct an accurate dissection on small honeybees or bird heads using only broken bits of glass and plastic. We needed to find a better animal to study, and the tuna turned out to be one of the best animals that we could find. These fish spend their entire lives in the deep sea, far away from all sources of terrestrial contamination. They also have large heads that are much easier to dissect that those of a bird or bee.

I started collaborating with Michael Walker and others at the National Marine Fishery Service (NMFS) in Hawaii to see, first of all, whether or not tuna have a magnetic sense, and second, whether we could locate any concentrations of magnetite in the fish. The yellowfin tuna was under particularly intensive study at the NMFS; its known migratory range extends over most of the Pacific. Walker did a lot of behavioral work, training fish to swim through hoops and using a conditioning paradigm — feeding the fish if it swims through a hoop at the right time and punishing it with no food if it does not. We could measure the ability of the fish to detect the magnetic field, then, by feeding it only after it swims through the hoop when a magnetic field is turned off rather than on. After a while the fish began to learn to go through the hoop to get the food when the field was right, which showed that the tuna have a good magnetic sense.

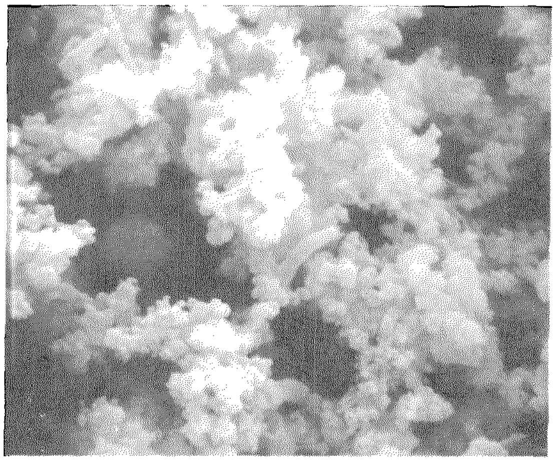
When we started looking into the head of the tuna, we found a reliable pocket of magnetic material within a sinus in the dermethmoid bone at the tip of the fish's snout. The magnetic particles, which turned out to be magnetite, are located in a structure a few millimeters long by a few millimeters wide, nestled within the top of this cavity inside the bone. The small, regularly shaped magnetic crystals are similar to those found in a few species of bacteria.

From other studies we've done we know that a nerve runs to this cavity from the lateral-line sensory system of the fish. This particular set of nerves usually goes to structures known as hair cells, which are very sensitive receptors for motion. The nervous system can detect a change of even a few angstroms in these hairs' position. Since we know that this particular type of nerve usually goes to such receptors, we're currently hunting for them in the tuna dermethmoids. The only problem is that the magnetite is present in concentrations of about five parts per billion of volume. The search entails going section by section throughout the whole structure where we found the magnetite. We hope that within a year or two we'll find something like hair cell receptors associated somehow with these magnetic chains.

All of the behavioral studies that I've mentioned so far have been contrived and are really not natural. For example, there's nothing in the experiments that shows us what a bird is actually doing when it's trying to fly somewhere. What we want to know is why these animals are using the magnetic field in the first place, and what possible information about navigational problems these weak field sensors can provide. And how can they be thrown off by just a weak anomaly? There seems to be something about the weak fluctuations in the magnetic field that interferes with the pigeons' ability to figure out their position.

So, in order to study the movements of naturally migrating animals, we have turned to whales. Whales are nice to work with because they're big. You can see them, even from airplanes, and when they land on the beach you can usually find them. There are national agencies, such as the Smithsonian Institution and the Coast Guard, that routinely report on the location of stranded whales. And there are systematic searches of marine animals and whales that are done over marine shelves within the 200-mile economic zone. So we have a data base to work with. Dead whales simply washed ashore are of no interest if you want to know whether whales have a magnetic sense or what they're using the magnetic field for. But the mass strandings, where whole schools of live whales come ashore for some unknown reason and beach themselves, are most interesting for our studies because they might tell us something about what the animals are doing at sea. In addition, Margaret Klinowska, a cetacean biologist at Cambridge University in England, has recently claimed that whales tend to avoid magnetic anomalies when they are about to beach themselves. An investigation of this with American data clearly seemed worthwhile.

About a year ago I started a cooperative project with James Westphal, professor of planetary science, and Andrew Dizon of the NMFS to see whether we could use these data to determine whether whales had a magnetic sense and, if so, how they are using it. We

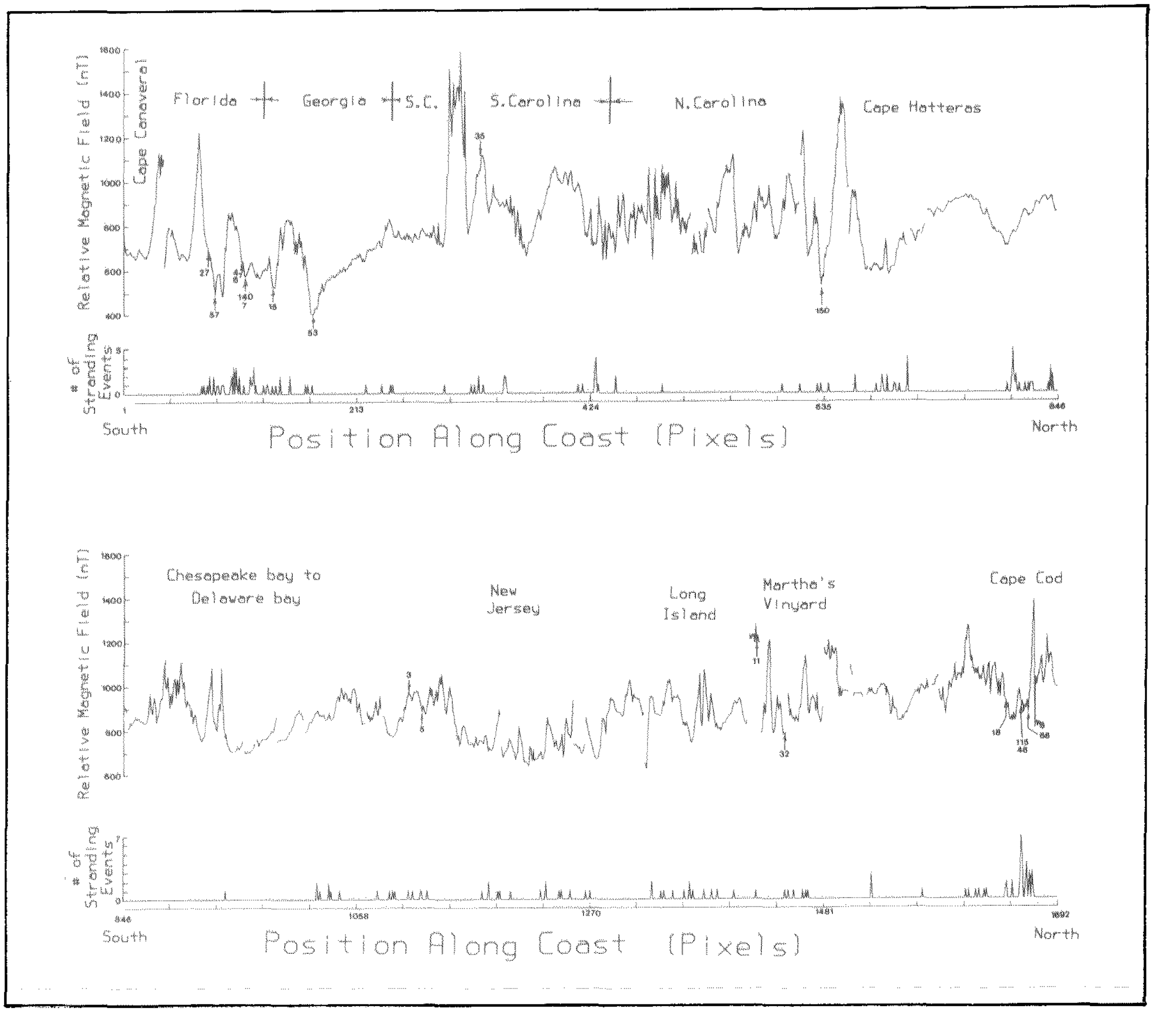


This scanning electron micrograph shows the magnetic material contained within a bone of the tuna's snout. This structure may send information on the Earth's magnetic field to the tuna's brain.

combined three data sets. During the 1970s the U. S. Geological Survey had done an extensive series of aeromagnetic surveys from Florida to Cape Cod. They just flew an airplane back and forth every few miles, towing a magnetometer to get a picture of the offshore magnetic field. The data from this survey are shown plotted in false color on the cover of this magazine. Magnetic field values go from low (dark blue) to high (yellow). On top of that we've plotted a high-resolution digital representation of the shoreline made by the U.S. National Center for Atmospheric Research. The red dots and crosses show the location of all whale stranding events, taken from the Smithsonian Institution's computerized directory. The large crosses represent more than 30 whales in stranding events, the small crosses between 3 and 30, and the red dots one or two whales. Visually there seems to be a strong association between points on the coastline where whales come ashore and the topography of the magnetic field. For example, the site where a large dark blue magnetic minimum — part of the East Coast magnetic anomaly — hits the Georgia shore is precisely where 53 pilot whales beached themselves. But since the range from deepest blue to brightest yellow represents only about a 4 percent change in the background field, the whales would have to be using an extremely high-resolution sense to be able to pick up the variations.

In all the years of cetacean work, nothing has been found until recently to explain why mass strandings occurred where they did. There's no bathymetric relief that characterizes these particular sites along the coastline; it's all the same depth. There are no obvious current or temperature differences. So a simple remaining hypothesis is that whales may

This diagram contains a plot of the relative magnetic field in sequential segments along the coast from Cape Canaveral, Florida, to Cape Cod, Massachusetts, along with a histogram showing the number of cetacean stranding events in each coastal pixel. Many strandings coincide with low points in magnetic field strength.



be following weak changes in the magnetic field and that this is reflected in their stranding locations.

We went about testing this hypothesis using a simple statistical analysis — by determining whether the associations of whale strandings with magnetic lows could be explained as random events. First we unraveled the Atlantic coastline and plotted the variations in the aeromagnetic data along it. This is shown in the diagram that plots this as well as the number of whale strandings at particular points along the coast. Just looking at it, some correlations seem obvious. It certainly looks nonrandom, but there are a number of statistical approaches that can test it.

The best statistical approach turned out to be to examine the local coastal neighborhood around each stranding event. Where a line of magnetic minima ("valley") intersects the shore, the total field should increase on either side of it. So if we assume that there is *no* relationship between magnetic fields and strandings, it implies that, on the average, the magnetic field along the coast should be neither higher nor lower than the field at the stranding sites, and the differences between field values at those sites and neighboring sites should be zero. Systematic departures from zero, on the other hand, would indicate correlation with magnetic highs and lows. This technique lends itself to a direct test of significance through Monte Carlo simulations — creating (in a computer) millions of random whales, throwing them on the beaches, and seeing how many randomly fall into the magnetic valleys.

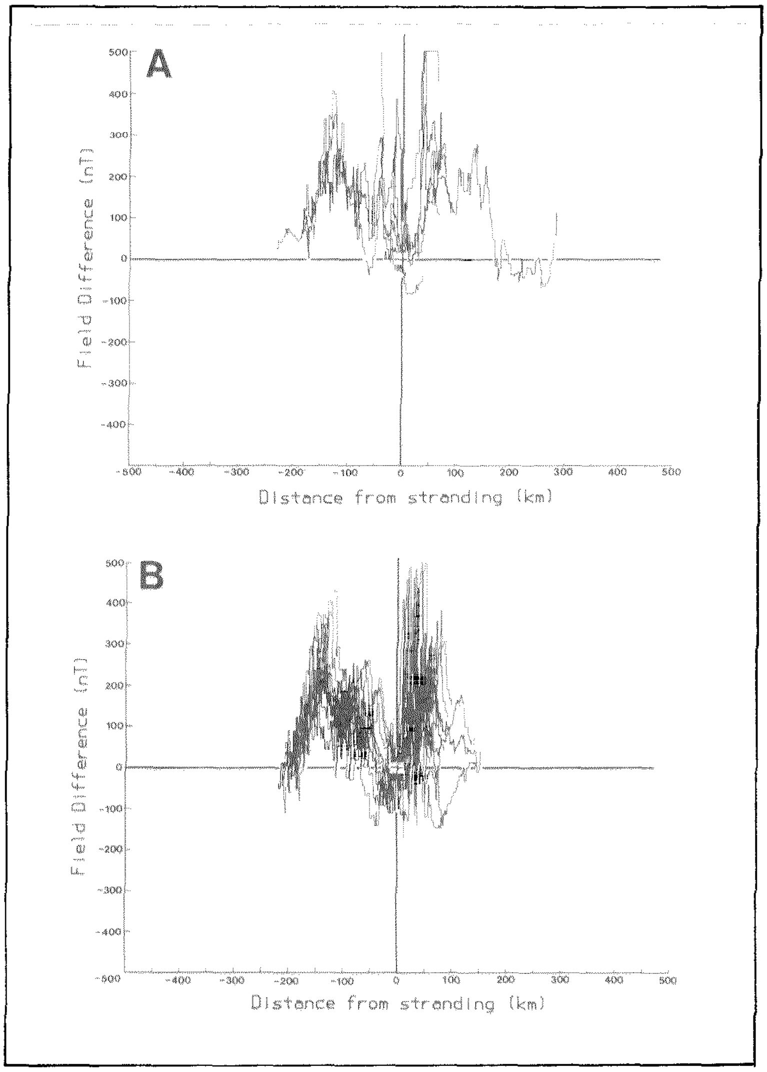
We ran this analysis on 212 stranding events from the Smithsonian records — separated by species and combined. Although there is variation among species (and species representing about 10 percent of the total appear to beach themselves at magnetic highs), the tendency for most cetaceans to strand near magnetic low spots is clearly seen in the combined results. Profiles of these results can be shown in what I call "spider" diagrams because of the long thin "legs" radiating out from a central point, which represents the intersection (0,0) of the axes of relative coastal distance and magnetic field. Random strandings would yield legs radiating in all directions, while stranding points at magnetic highs would send the legs into the bottom quadrants, and those at magnetic lows would have legs pointed upward into the top quadrants. These spider plots illustrate the data for two species, *Balaenoptera physalus* and *Lagenorhynchus acutus*, which gave the strongest results in our analysis. It is obvious that the average tendency is for the legs to point upward.

So it is clear that cetaceans possess a highly developed sensitivity to the geomagnetic field, which probably enables them to use it for guidance. We are currently looking at data from the Cetacean and Turtle Assessment Program, an aerial counting operation over the continental shelf financed by the Bureau of Land Management and managed through the University of Rhode Island. We have plotted the positions of various species by season to get an idea of migrating and feeding patterns and compared this to the magnetic field data. Visually, at least, there seems to be no strong relationship to the magnetic field where they are feeding, but when they are migrating up and down the coast there seems to be a much better one. Usually the migrating whales can be found in the magnetic valleys, as if they were following them.

Why might aquatic animals be interested in following the magnetic field? Imagine that you're left in a rowboat in the middle of the Atlantic Ocean without a compass, sextant, or any other instrument. How can you keep track of where you're going? You can determine latitude if you can see celestial cues, but measuring your east-west position is much more difficult. The early English navigators trying to circumnavigate the world had the same problem: If you can't measure longitude, you can't make maps. But a turtle dropped in the middle of the Atlantic knows how to head for home.

A clue to how aquatic animals may use the magnetic field has come to light in the past 30 years as marine geologists have studied the magnetic fields over the ocean floor. The ocean floor is formed at the mid-ocean ridges, where basaltic magma wells up and solidifies, spreading the sea floor apart. As the earth's magnetic field has reversed many times over the millions of years the ocean floor has been in the making, a record of those reversals has been left recorded by the volcanic rocks on the ocean bottom. These magnetic stripes trend north-south in a very regular manner away from the spreading ridges and have allowed geologists to date regions of the sea floor.

They also might be providing longitudinal reference points for magnetically sensitive animals. We haven't tested this hypothesis yet, but we think that when we can do a global study of migratory animals in the world's oceans, we will find the animals following the magnetic valleys, using them to keep track of relative longitude. Magnetic highs would be much more difficult to follow, since they are more prone to "noise" from positive susceptibility anomalies centered over seamounts and ocean fracture zones. If this proves true, it would have major implications for commercial fisheries that exploit magnetically sensitive fish, such as tuna and salmon, and should lead to better techniques for resource estimation and management. □



In these "spider diagrams", for (A) Balaenoptera physalus *(the fin whale) and (B)* Lagenorhynchus acutus *(the Atlantic white-sided dolphin), the central point represents the locations of strandings and the legs radiating from that point represent the magnetic fields of adjacent coastline. The upward radiation of the legs indicates that these species preferentially beach themselves at local magnetic minima.*

Jesse Greenstein (right) welcomes Willy Fowler to the symposium on low-luminosity stars.

Some Faint Stars and a Bright One

by Virginia Trimble and Judith G. Cohen

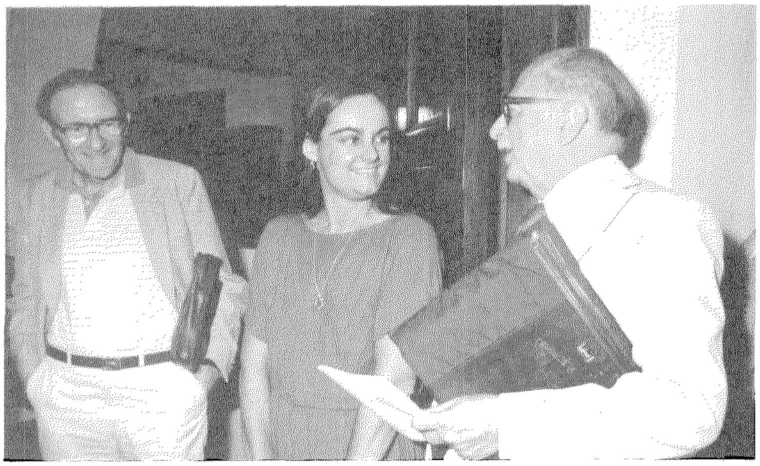
THE LOW-LUMINOSITY STARS are a wild assortment — hot and cool, old and young, dense and diffuse — almost as motley a crew as the 85 astronomers who gathered at Caltech on October 15-16, 1984 to discuss them and to honor the 75th birthday of Jesse L. Greenstein, the DuBridge Professor of Astrophysics, Emeritus, a pioneer in the study of faint stars, especially white dwarfs. The faint stars include both the small, low-mass subset of normal, hydrogen-burning stars, as well as the corpses of more massive ones that have essentially completed their nuclear reactions.

Inevitably, low-luminosity stars are not individually spectacular (not one is a naked-eye object), but they are astronomically interesting nonetheless. Low-mass stars are extremely numerous and collectively have a major fraction of the total stellar mass of our galaxy. Just how large a fraction is currently much debated. The compositions of the remnants of massive stars tell us about the nuclear processes that gradually transformed a few percent of the galactic mass from hydrogen and helium into the heavier elements found in the earth and in ourselves. These remnants are sometimes called degenerate dwarfs; they are so dense that their interiors are degenerate (this is a statement about the electron velocity distribution, not their moral principles). They thus provide a laboratory for studying physical conditions we cannot replicate on earth. And the faint stars are the longest lived of all, providing a probe of star formation in the distant past. In fact, as Greenstein noted at a 1968 meeting on the same subject, the astronomers who study faint stars will inherit the skies by default after 10^{11} years.

In the meantime, some of the issues on which participants at the October conference had recent progress to report included magnetic fields on the surfaces of some white dwarfs (stronger than any ever produced in terrestrial laboratories); the existence and modeling of a new class of pulsating, hot subdwarfs; and the interpretation of white dwarf compositions.

The magnetic field at the surface of the earth has a strength of less than one gauss; a household magnet measures about 100 gauss; and the strongest fields produced in the laboratory for research purposes (over very small volumes and very briefly) can read just about a million gauss. Thus the discovery about 15 years ago of circular polarization, implying a field of at least 100 million gauss on the surface of one white dwarf, made it immediately clear that we were seeing a new physical regime. The particular star involved (Greenwich +70°8247) was already famous for having absorption lines in its spectrum that had defied interpretation since Rudolph Minkowski first photographed them at Mt. Wilson in 1939.

The report of the strong field quickly triggered a flurry of papers attempting to identify the lines. The basic idea is that strong magnetic fields grossly perturb electron orbits, so that spectral lines split into many components, shifted by different amounts from their normal wavelengths. Some suggestions were intriguing (transitions in unbound He_2 molecules for instance), but none matched the observed lines well or obtained general acceptance. Sufficiently accurate quantum-mechanical calculations of the expected wavelengths were difficult, and there could be no laboratory data to check against. Another problem was that the wavelength shifts change sharply with field strength, so that variations in field in the star's atmosphere



Virginia Trimble contemplates suitable forms of birthday greetings for Greenstein with assistance from Robert O'Connell (Louisiana State University).

would smear features beyond detectability.

Two recent developments have finally sorted out the mess. The first was Greenstein's detection in 1983 of an ultraviolet line in the spectrum of Grw +70°8247, which he attributed to the simplest of all atomic transitions (ground state to first excited state in hydrogen, called Lyman alpha). Thus the calculations became more tractable. The second was the development of the idea of stationary components, lines whose wavelength stays nearly constant over a range of field values (owing to a turning point or inflection point in λ vs. B). This permits absorption to be concentrated in a single, narrow wavelength band over the whole stellar surface.

Observers Roger Angel (Steward Observatory of the University of Arizona) and Jesse Greenstein (Caltech), and theorists R. F. Henry and Robert O'Connell (Louisiana State University), and G. Wunner (Tübingen) were, therefore, able to report general agreement in attributing both optical and ultraviolet lines in Grw +70°8247 to transitions in the most common element, hydrogen. These lines include components of Lyman alpha and of Balmer alpha and beta, some never seen in the laboratory, because they exist only in the presence of a strong magnetic field. The surface field for Grw +70°8247 is in the range 2-6 x 10^8 gauss. Because of the stationary line concept, it cannot be determined exactly, but a mystery nearly 50 years old was solved.

Several other stars with unusual absorption spectra are currently under study and may also have fields in the range 10^8-10^9 gauss. For fields above 2 x 10^9 gauss, the normal hydrogen lines all move into the ultraviolet, and will typically not be seen. A few such stars could conceivably be hiding among those (called DC) that display no visible lines.

Stars whose brightness varies because of periodic changes in size or shape have been known for more than a century and have been understood since the 1920s. Such pulsational variables include the Cepheids, classic indicators of the cosmic distance scale. For most pulsating stars the driving force is a zone just below the surface in which hydrogen is partially ionized. The zone acts as a sort of faucet, letting light and heat leak out when the star is expanded and bottling them up when it is compressed. Such stars necessarily occur over a fairly narrow range of surface temperatures, just below the temperature at which hydrogen ionizes. They are particularly valuable astronomical tools because the periods, amplitudes, and shapes of the pulsation curves are sensitive to the distribution of density and composition inside the stars. Thus we are able to probe the density and composition in the invisible stellar interiors in addition to measuring total masses.

The white dwarf variables are called ZZ Ceti stars. They show normal (for white dwarfs) atmospheres of pure hydrogen and have surface temperatures near 12,000K and pulsation periods of 200-1,200 seconds. These can be modeled as shape distortions, driven by hydrogen ionization and restored by gravitation (called non-radial g modes). The surprise is that detailed matching of the driving force with pulsational properties requires that the hydrogen atmosphere be exceedingly thin, only about 10^{-7} of the total stellar mass. It is very difficult to get a star to burn or eject all of its hydrogen this efficiently.

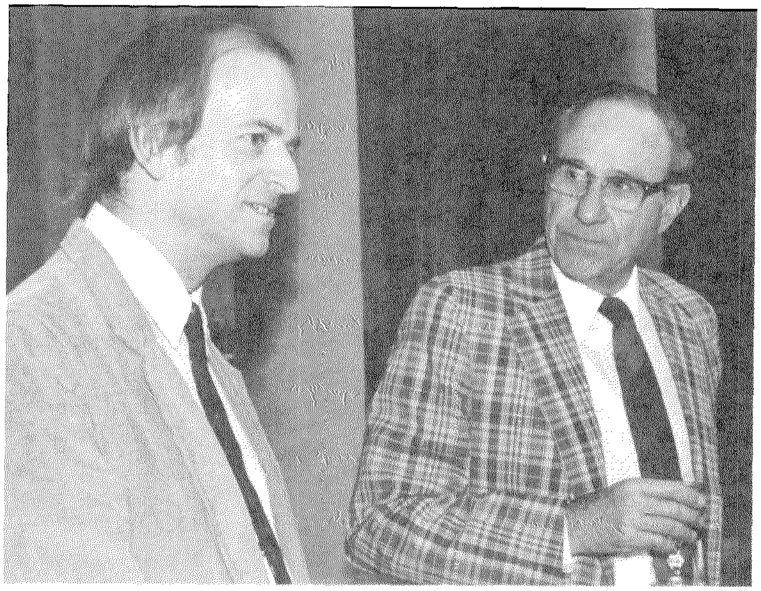
Georges Michaud (PhD Caltech 1970; University of Montreal) reported that this can be done via a new physical process — diffusive hydrogen burning, which is described later in this article — while Icko Iben, Jr. (University of Illinois) believes it to be quite impossible. Even with the new process included, his thinnest hydrogen layers are more like 10^{-4} of the mass of the star. An important difference between the calculations is the thickness of the helium layer intervening between the hydrogen envelope and the carbon-oxygen core where hydrogen can be burned. This and other disagreements between groups working on the problem remain to be sorted out.

A second class of pulsating white dwarfs, with higher temperatures and nearly pure helium atmospheres, is driven by a helium ionization zone and does not seem to present any special problems.

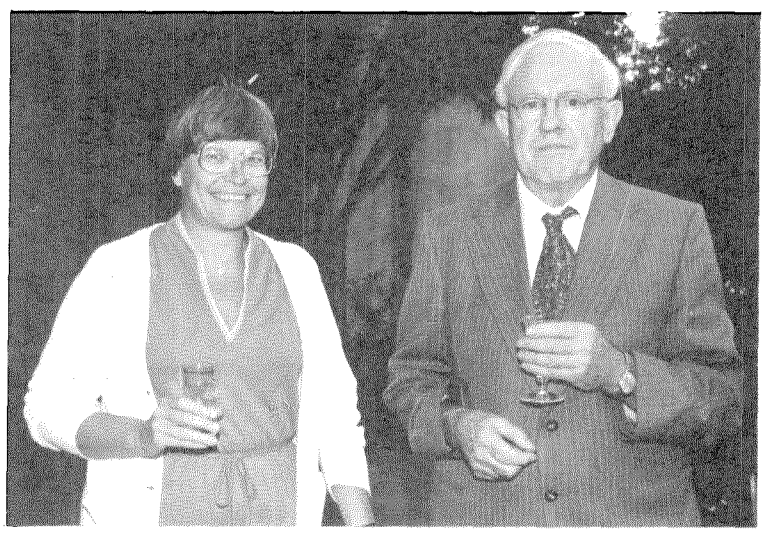
Most of the excitement centered around a new third class, one of whose four members has just ejected its outer layers in a planetary nebula and so must have completed nuclear reactions very recently indeed. These stars have surface temperatures of 100,000K or more, arguably the highest known for any sort of star, and their spectra show lines of helium, carbon, and oxygen. The class is named for its prototype, PG 1159-035 from the Palomar survey carried out as part of his dissertation by Richard Green (PhD Caltech 1977).

Two surprises about this new class surfaced. First, Donald E. Winget (University of Texas, Austin) reported that the pulsation period of PG 1159 is dropping at a rate of about 10^{-11} sec/sec, corresponding to an evolutionary time scale of only about a million years. Contraction of the star should make the period shorter, and cooling should make it longer. Thus the former must be more important right now. There will soon be more data on this star and other members of its class. Comparison of these with the pulsation calculations and evolutionary models should tell us the masses of the stars (probably larger than the general run of white dwarfs, according to James Liebert of Steward Observatory) and the rate at which they are cooling (hence the extent to which nuclear reactions are still contributing to their luminosity).

The second surprise, reported by Sumner Starrfield (Arizona State University), is that the pulsation properties of PG 1159 and of the pulsating planetary nebula nucleus K 1-16 require a composition that is about half oxygen (and the other half mostly carbon) almost to the very surface of the star. The stars must have been extremely efficient at getting rid of their hydrogen and helium envelopes, because the progenitors of white dwarfs (stars two to five times the size of the sun) have generally been thought to produce little oxygen. The burning of helium produces both carbon and oxygen in a ratio that rises with the temperature at which burning occurs. Greenstein expressed some skepticism that the requisite oxygen abundance could be produced in relatively cool, low-mass stars. William A. Fowler, Institute Professor of Physics, Emeritus, reminded the participants that a recent Kellogg remeasurement of the cross section for the reaction $C^{12}(\alpha,\gamma)O^{16}$ yielded a value about three times the previous one, thereby



Preparing for a birthday toast at the Athenaeum are Jeremy Mould and Marshall Cohen (above); Richard Green (Caltech PhD 1977), now at Kitt Peak National Observatory, and Judith Cohen (middle); and long-time friends and colleagues Anne Merchant Boesgaard (Hawaii) and Lawrence Aller (UCLA) in the bottom photo. Mould, Cohen, and Cohen organized the conference.



enhancing the oxygen/carbon ratio in the end-products of helium burning. Perhaps the PG 1159 stars are independent confirmation of the laboratory result.

Most degenerate dwarfs show strong spectral lines of only one element — hydrogen or helium or carbon. This has been understood to first order since 1945, when Evry Schatzman (Paris) pointed out that their very strong gravitational fields would force the lightest element present to float to the top in much less time than the stars take to evolve.

A few white dwarfs show hydrogen plus helium or some heavier elements — carbon, calcium, and magnesium are typical. They too are comprehensible, the surface abundances reflecting a combination of the accretion of fresh material (from surrounding interstellar gas or the wind of a companion) with Schatzman's gravitational settling and radiation pressure. Gary Wegner (Dartmouth) discussed a third group in which nearly pure helium atmospheres harbor some carbon (less than we see in the sun but more than should have survived gravitational settling). He pointed out that these stars have just the surface temperatures where currents of rising and falling gas should penetrate deepest into the star, dredging up material from the heavy-element-rich interior. And, as we saw a couple of paragraphs back, we expect the interiors of white dwarfs to be at least half carbon.

The really difficult case consists of the few stars with helium atmospheres contaminated by elements heaver than carbon (again, less than we see in the sun but more than should have survived settling). If the explanation is dredge-up, then nuclear reactions in the parent stars must have proceeded much further than we think they can in the sorts of stars that make white dwarfs. And if accreted interstellar gas is to blame for heavy elements, then where has the dominant hydrogen gone?

Michaud described a rather curious process that may provide the answer. Gravitational effects are trying to float any available hydrogen atop everything else. But there will be an exponentially decreasing tail of hydrogen concentration down into the star. For surface temperatures between about 10,000K and 30,000K, this tail penetrates into carbon-rich regions hot and dense enough for hydrogen to burn by the carbon-nitrogen-oxygen cycle. So efficient is the burning that it tries to establish an equilibrium hydrogen concentration even smaller than the exponential tail provides. Thus hydrogen continuously diffuses down and burns as fast as it accretes, leaving the atmospheric abundance below detectability. While gravitation tugs downward on the accreted metals, enough atoms pass through the atmosphere at any given time to make the lines we see.

Diffusive hydrogen burning therefore enables us to understand an otherwise incomprehensible mixture of elements in white dwarf atmospheres. It apparently solves two other problems as well. The first is the thinness of the hydrogen layer in the pulsating ZZ Ceti stars discussed above. The second is a gradual evolution from hydrogen-rich to helium-rich atmospheres as white dwarfs age. Since these stars have no internal nuclear energy sources, the oldest are necessarily the coolest and faintest. Greenstein's most recent compilations of Palomar white dwarf spectra show that the ratio of hydrogen-rich to helium-rich stars drops from 4:1 among the hottest to 0.5:1 among the coolest. It seems that diffusive hydrogen burning consumes enough during the billion-year-plus cooling time of an average white dwarf to turn one sort of atmosphere into the other, whether the hydrogen is accreted or left over from the parent star.

Because the burning turns off for surface temperatures below 10,000K, one would expect continued accretion to build the hydrogen-rich population gradually back up again. This hypothesis cannot yet be properly tested, because (as Greenstein seems to have been the first to note in 1969) such faint, cool degenerate dwarfs are exceedingly rare. We do not yet fully understand the reasons for this, but diffusive hydrogen burning, which slows down the cooling process, may again be part of the picture. Or perhaps there were fewer suitable parent stars born 6-8 billion years ago than more recently.

A number of other topics, some of agreement, and some of at least friendly disagreement, surfaced at the October meeting. Several, like the evolutionary status of hot white dwarfs with both hydrogen and helium in their atmospheres, the nature of the faint, high-latitude blue stars, and the masses of degenerate dwarfs are subjects to which Greenstein, his students, and collaborators have made major contributions. Readers interested in pursuing them will find a more complete report of the symposium in the Quarterly Journal of the Royal Astrophysical Society later this year. □

Io: Jupiter's Enigmatic Moon

by Torrence V. Johnson

THE WHITE QUEEN SAID to Alice, "Why, sometimes I've believed as many as six impossible things before breakfast." Meeting this quota is no problem if you merely whisper to yourself every morning — "Io exists!"

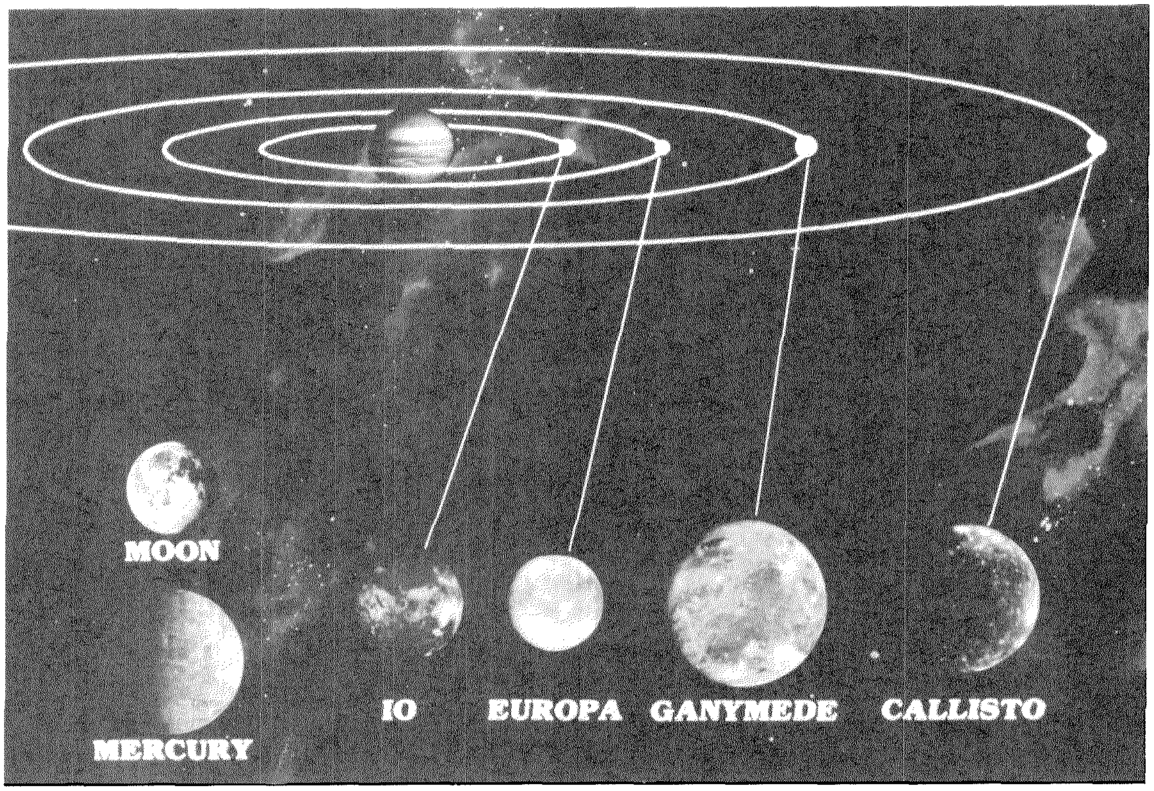
Scientific study of Io began in 1610 with its discovery by Galileo, along with the other three large moons of Jupiter — Europa, Ganymede, and Callisto. This article almost had a very different title, since Galileo proposed calling these new objects the "Medici stars." Acknowledgement of funding sources was as important then as now. It was Kepler and Simon Marius (who also claimed to have discovered the moons) who suggested naming them after Jupiter's lovers. They are now known collectively as the Galilean satellites.

In 1675 the Swedish astronomer Ole Roemer noted that the intervals between Io eclipses were shorter when the Earth was moving towards Jupiter than when it was moving away. The accumulated lag from one side of Earth's orbit to the other was about 16 minutes. Roemer deduced that this was due to the finite speed of light, which Galileo had tried unsuccessfully to measure, and calculated the first accurate value for this fundamental quantity.

Studies of the satellites of Jupiter continued to be of importance to the growing science of astronomy. In the 1890s A. A. Michelson tested his stellar interferometer by measuring the diameters of the Galilean satellites, values that are within 20 percent of the modern numbers in most cases. In the early part of the 20th century, the mathematical problem of describing the effects of mutual gravitation among the satellites on their orbits was solved, providing fairly accurate estimates of their masses.

In the latter half of the century, increasingly sophisticated astronomical observations continued to reveal strange things about the moon Io: its influence on the probability of certain types of radio noise outbursts from Jupiter; its yellow, highly reflective surface; and its "pollution" of the whole Jovian magnetosphere with clouds of atomic material.

All the studies from the time of Galileo until just a few years ago were limited to observations of a tiny spot of light in our telescopes. Detailed study of Io began with the first spacecraft observations by Pioneer 10 in



Io, the closest to Jupiter of the Galilean satellites, is almost as large as our Moon.

1973, which discovered evidence of a layer of electrons and ions high above Io's surface, suggesting at least a tenuous gaseous atmosphere of some type. Just a few years later, in 1979, our eyes were opened by the spectacular results of the Voyager flybys, which showed Io to be a world even stranger than most of us had imagined. Just how strange may be judged by the fact that even Arthur C. Clarke in his science fiction novel *2010* found Io entirely weird enough as it is and didn't embellish its nature at all.

Jupiter and its system of moons is five times farther away from the Sun than the Earth is. Io is about as far away from Jupiter as the Moon is from the Earth. This means that in Io's sky, the Sun is only a fifth the size that we are accustomed to, while Jupiter looms 40 times larger than the full Moon. Because the amount of solar energy reaching a planet decreases as the square of its distance from the Sun, Io receives only 1/25th the solar energy that the Earth does, and it's very cold. Noon temperatures on Io's equator do not rise above about -130° C, or about -200° F, and night temperatures drop to below -170° C, or about -300° F.

The Galilean satellites are relatively large objects (Io is the size of the Earth's Moon). Ganymede and Callisto are made of mixtures of ice and rock, but Io has a density a little heavier than the Moon's and must therefore be composed primarily of rock. But Io's yellow, reflective surface doesn't *look* anything like that of the Moon, the planets, or the other Galilean satellites. We believe that coatings of sulfur-rich compounds are responsible for its appearance. Solid sulfur dioxide has been identified in the spectrum of Io's surface, and sulfur dioxide gas was detected over one of Io's big volcanoes by instruments on the Voyager spacecraft. Elemental sulfur may be responsible for the odd coloration of much of the surface.

The most surprising feature of Io's surface is that it's geologically young and volcanically active. Instead of having the numerous impact craters formed when meteorites strike most planetary surfaces, Io's surface is covered with a variety of volcanic landforms and large eruptive plumes. But how did these peculiarities of Io's surface come about?

Since Galileo's time the Jupiter satellite system has been popularly thought of as a miniature solar system, and there are indeed many points of similarity. We believe that the planets in the solar system formed from material condensed from a cloud of gas and dust with the same overall composition as the Sun has now. In most current theories of cosmogony, the distance from the Sun in this nebula controls the composition of the solid material available to make up planetary objects, with rock and iron dominating the planets of the inner solar system and increasing mixtures of frozen volatile materials, primarily water ice, those beyond the asteroid belt.

The smaller, inner planets, such as the Earth and Mars, were probably unable to alter the conditions around them substantially from the general conditions prevailing in the nebula during formation. Jupiter, however, was massive enough to create a microenvironment in the solar nebula. As material gathered about Jupiter, gravitational contraction heated it to very high temperatures. Even today, the remnants of that early heat are leaking out from Jupiter's deep interior, so that the planet radiates almost twice as much energy as it receives from the Sun. Many theorists believe that this circumstance accounts for the lack of large quantities of ice on Io and Europa compared with Ganymede and Callisto — it was just too hot close to Jupiter for ice grains to survive and be collected into the forming satellites. Not all scientists accept this simple view, and Jupiter's role in focusing incoming debris into an intense collisional environment in the inner part of the satellite system has been suggested as a major modifier of the moons' early development. In any case, it seems clear that in the early formation stages of Jupiter and its moons the regions close to the giant planet differed significantly from the outer regions, helping to create the diversity of satellite compositions and characteristics we see today.

The life history of a planetary body is controlled by its energy balance. For most bodies, the energy coming from the sun is negligible in this balance in terms of its geological effect. The three main energy sources that control the evolution of most planets are the energy of formation, the energy from the decay of short-half-life radioactive elements, and the energy from the decay of long-half-life radioactive elements. The first two generally affect only the early history of a planet. The third is believed to be responsible for the current level of geological activity in most terrestrial planets.

The most important long-lived radioactive elements are uranium, potassium and thorium. These provide a heat source that builds up over billions of years, keeping the internal fires of the planet alive. Just how active a planet will be under the influence of this energy input is primarily a function of how fast it can lose the heat being generated in its interior; this heat is carried to the surface of the planet by a variety of processes and ultimately is radiated away into space. The efficiency of heat loss is tied to the surface area of the planet, while the amount of heat generated is linked to the planet's volume. This creates a situation that greatly favors big planets over small ones in the geological activity sweepstakes. The simple principle that bigger is hotter works admirably for the Moon, Mars, Earth, Venus, and Mercury. So we didn't expect active volcanoes on Io; we thought it had been geologically dead for more than 3 billion years, similar, in fact, to our own Moon.

There turns out to be, however, another source of energy that might drive current activity — tides. The story of how tides can heat a satellite, why this source was overlooked for so long, and how tidal heating might drive Io's volcanoes is an interesting one. First of all, if you think of tides as something only associated with the Earth's oceans and think of the gradual ebb and flow of the water down at the beach, you might not be inclined to think of tides as very important as sources of planetary heating. To really understand tides as important planetary forces, you have to consider three facts: tides affect all gravitating bodies; tides are raised in the entire body of a planet, solid as well as liquid; and the only way to get work out of tides is for them to change.

It is this third point that is responsible for the effects of tides on Io being overlooked for so long. If a moon is in circular orbit and is spinning at the same rate that it circles its planet, the net result is no change in tides. Io's orbit, averaged over a long time, is almost exactly circular, and its rotation is exactly the same as its orbital period. So no one worried much about tidal heating on Io, even though the idea had been kicked around as a heat source for our Moon, where the tides do change due to its noncircular, eccentric orbit.

The moons of Jupiter, however, are big enough to influence each others' orbits. This continual gravitational jostling has resulted in a relationship between the satellites' orbital periods, an effect first studied by the French mathematician Laplace. Europa, the next satellite out, orbits Jupiter with a period almost exactly twice that of Io, so the distance between Europa and Io varies in a systematic way, being at a minimum every two Io periods. Europa is very small compared to

Jupiter, and its gravitational pull is less than one ten thousandth of Jupiter's, even at its closest. So it doesn't do anything important itself in terms of tides. But the fact that it varies so regularly is extremely important. Essentially, every two Io orbits Io gets a small kick from Europa, always in the same place. This is known in physics as a resonance phenomenon; it's similar to giving a pendulum a small push, always at the same place in its swing. This will cause the amplitude of the swing to build up rapidly, just as a child on a playground swing can pump its legs in rhythm with the motion to go higher and higher. The result of this is that Io's orbit is not quite circular; it has a "forced eccentricity" of 4 parts in 1,000, enough to change the tides.

The tides on Io change in two important ways. First, since Io in its eccentric orbit is a little closer to Jupiter on one side of its orbit than on the other, the tidal force changes, and the deformation due to the tide goes up and down by about 100 meters as Io revolves in its orbit. Second, Io keeps spinning at a constant rate, but changes its speed as it goes around Jupiter in accord with Kepler's laws; this causes the point where the tide is maximum to wobble back and forth across the surface in the course of Io's orbit. Both of these changes translate into heat by creating friction in the crust and interior.

The implications of Europa's orbital *pas de deux* with Io were not realized until just before the Voyager encounter. Only weeks before the Voyager 1 encounter in 1979, an article by Stan Peale at UC Santa Barbara and two colleagues at Ames Research Center was published in *Science*, suggesting that Io may get more heating from tides than from radioactive isotopes. Peale and co-workers suggested that some volcanic features might be present on Io — a prediction amply confirmed by Voyager's pictures.

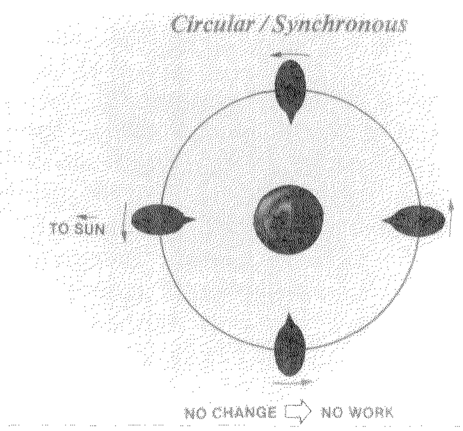
Even Voyager's first, long-range views of Io showed us that we were dealing with a very young surface. Io displayed no hint of impact craters or basins, unlike virtually all other planetary surfaces. And, as Voyager approached Io, we could see features that were clearly volcanic in nature, quite reminiscent of the volcanoes on Hawaii, seen from an airplane, but on a much larger scale.

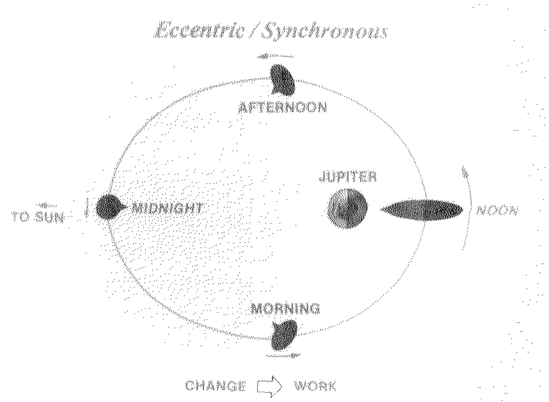
Finally came the discovery of some sort of active eruptions on Io. This was the real shocker. What was actually seen was what we now refer to as a "plume." Linda Morabito, a navigation engineer at JPL, found it on a deliberately overexposed picture a few days after the encounter; others were independently discovered by the imaging team the next week while reviewing the raw data.

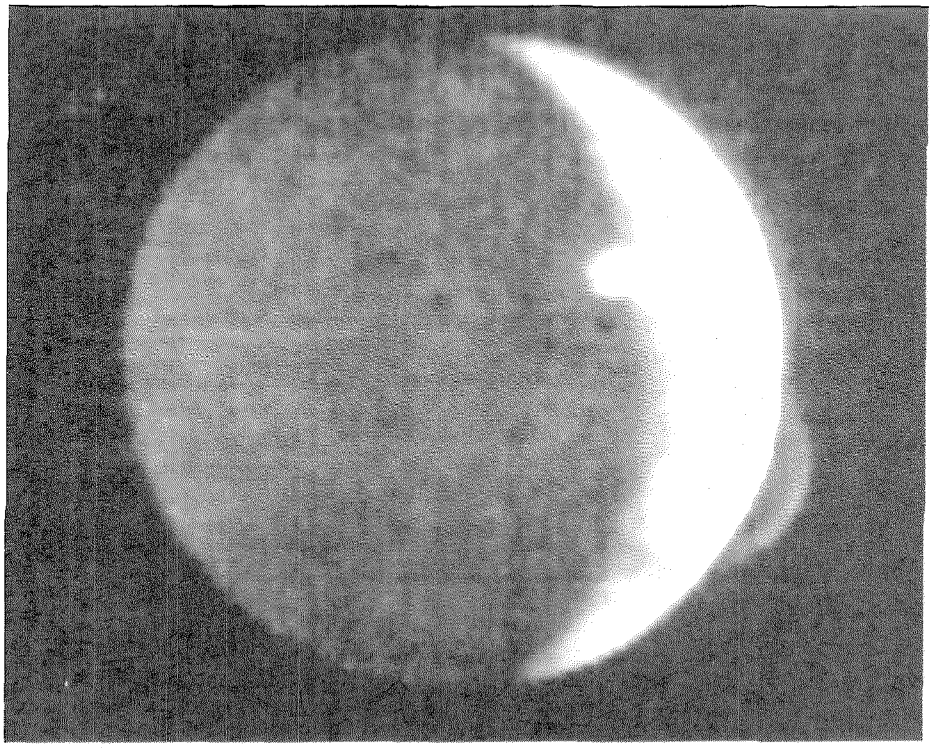
There has been a lot of confusion about what constitutes a "volcano" on Io. Volcanoes on the Earth usually have large volcanic collapse craters known as calderas in the tops of high mountains. Eruptions cause lava flows, which cascade down the side of the mountain and throw large amounts of gas and ash up into volcanic clouds or plumes. Io has high mountains, large calderas, lava flows, and plumes, but they are not related in the same way that they are on the Earth. The calderas are not in the tops of mountains; in fact we don't know for sure what builds up mountains on Io. The lava flows we see are not connected with the currently active volcanoes. The plumes we see are more likely to be giant geysers, driven by volcanic heat but not due to volcanic dust and lava thrown into the air.

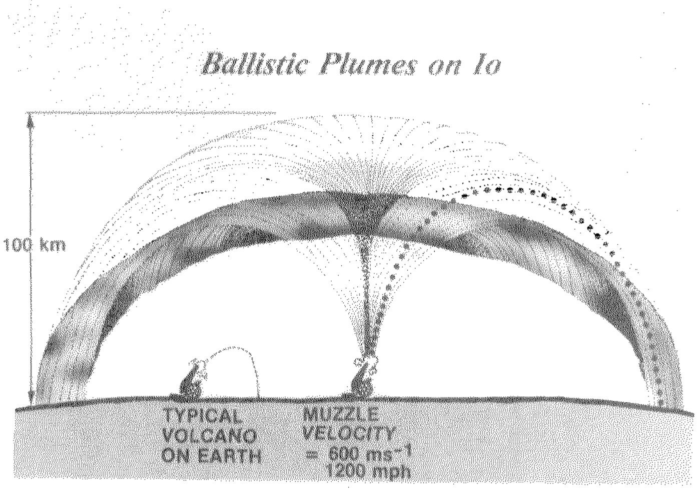
When we first saw the pictures of plumes on Io, our initial thought was that we were looking at large-scale explosive eruptions, similar to Vesuvius or Mount St. Helens. As with many quick intuitive ideas in science,

If Io's orbit were circular, its internal heat would be too low to produce its high geological activity. But the tiny "kick" it receives from Europa puts it in a slightly eccentric orbit. The tides produced in this way translate into heat by creating friction in its crust and interior.









JPL navigation engineer Linda Morabito first noticed Io's volcanism on this intentionally overexposed Voyager 1 photograph (top). In the diagram below, the ballistic plumes on Io are shown to be symmetrical, umbrella-shaped, and far larger than Earth's largest volcanoes.

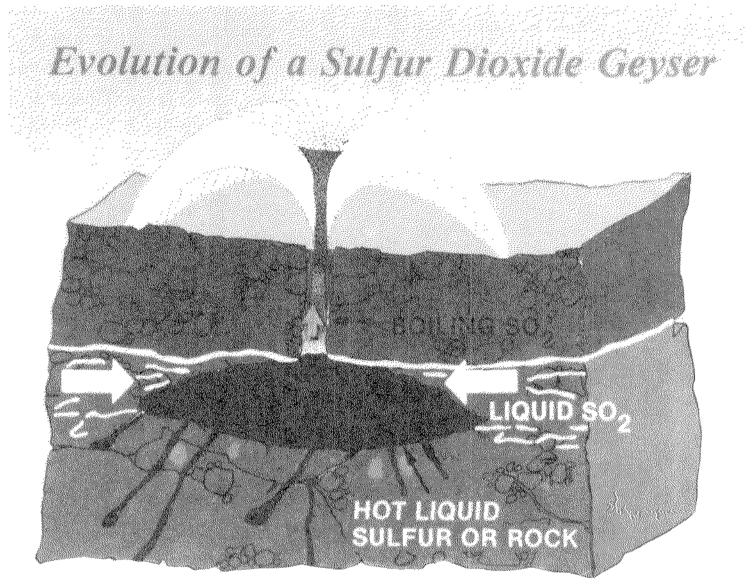
the rub came when we tried to plug numbers into this idea. There were two primary problems to be addressed — the size of the plumes and their long lifetimes. The observed plumes range from about 80 km to over 300 km in height, and most of them seem to have continued their eruptions over at least the four-month interval between the two Voyager encounters.

Io has essentially no atmosphere to impede the flight of volcanic debris, and its gravity is low — about one-sixth of the Earth's. Under these conditions, we expect material ejected from a small vent area to spread out in a nice symmetrical umbrella-shaped plume with each particle following a ballistic trajectory. Using simple ideas of ballistic flight, we can estimate the required exit velocities for particles at the base of the plume that will allow them to reach the observed height and range.

A typical plume on Io requires very high ejection velocities; a velocity of over 1 km/sec (greater than about 2,000 mph) is needed to match the largest plumes. Although typical volcanic ejection velocities on the Earth are far below this, some terrestrial explosive volcanism, such as massive steam explosions, can just about reach the necessary velocities. These events are usually very brief, however, lasting hours or a few days at most. Driving Io's plumes to such high velocities over periods of at least several months seems to require a totally different type of mechanism.

The answer seems to be that the plumes are, in fact, more like geysers. Geysers result when a fluid is heated at some depth below the surface and then is allowed to expand upward through a narrow vent to the surface — somewhat similar to a rocket exhaust. Geysers aren't very large or impressive on the Earth; even Old Faithful rises only about 30 meters from the surface during an eruption. The conditions are very different on Io. On Earth the geyser has to fight six times greater gravity, and it expands upward into a thick atmosphere. Sue Kieffer (PhD 1971) of the U. S. Geological Survey has studied both Old Faithful and Mount St. Helens in detail. She notes that if Old Faithful were placed on Io, it would erupt to a height of about 38 km — small by Io's standards but still impressive enough.

But what makes Io's geysers work? There is no trace of water in the spectrum of Io's surface or its tenuous atmosphere; a different working fluid is required to make the system go. The best candidates are sulfur dioxide and sulfur itself. Although both these substances are solid on Io's surface, they become liquid at relatively shallow depths in Io's volcanically heated crust. If liquid sulfur dioxide, at a depth of about 500 meters, comes into contact with either hot liquid sulfur or volcanic hot rock, it first begins to boil as it expands up the conduit to the surface; then, as the pressure and temperature drop lower and lower, solid sulfur dioxide will begin to "snow" out of the rising column as it reaches the surface. Kieffer and her colleagues have



Io's sulfur geysers are thought to originate when liquid sulfur dioxide boils at a depth of 500 meters. As it expands up a conduit to the surface, the temperature and pressure drop and solid sulfur dioxide "snows" out (above). The Loki caldera is shown below to scale with an earthly volcano (Hawaii).

calculated that a system such as this can easily achieve the exit velocities of 500 to 600 m/sec required to match typical plume characteristics. Even higher velocities can be reached if the sulfur dioxide is heated to higher temperatures, or if the fluid is sulfur heated by contact with hot rock.

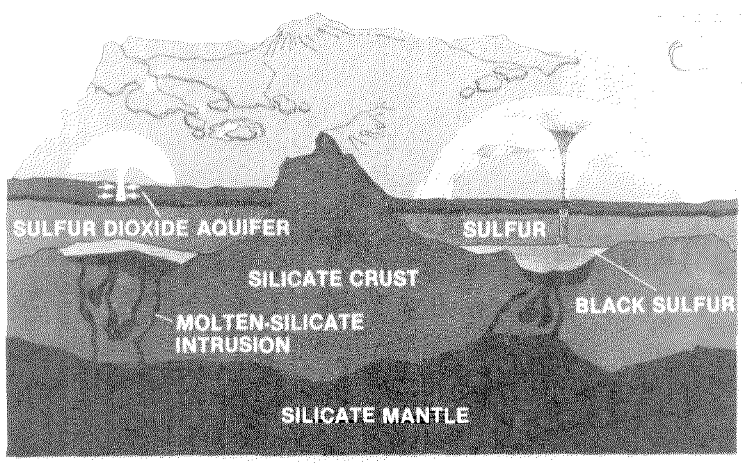
Next to the plumes, the most prominent volcanic features on Io are the many large calderas. Maasaw Patera, one of Io's medium-sized craters, is similar in dimensions to the summit area of Olympus Mons, the giant Martian volcano, but as far as we can tell, this crater and most others on Io are on fairly level volcanic plains. One of the most impressive and strangest features on Io is the Loki caldera. The size of this feature, over 200 km across, dwarfs the summit crater of even Olympus Mons. The Loki caldera, instead of being a deep hole in the ground, seems to be filled with darker material, presumably lava of some sort, although there are no obvious flows leading away from the edges.

Loki was the site of two other important Voyager discoveries. The Voyager infrared instrument found that this whole area was much hotter than its surroundings; much of the dark area is about room temperature instead of the usual frigid -200° F, and some portions of it are hotter than boiling water. In addition, spectra obtained by the infrared instrument showed the presence of small amounts of sulfur dioxide gas above Loki, the only direct evidence that we have for the composition of gas in Io's tenuous atmosphere. Loki is also the site of one of the active plume eruptions seen by Voyager. In many ways the Loki feature resembles an active lava lake, although of stupendous proportions. The amount of heat radiated from Loki accounts for about half of the total heat energy leaving Io through its volcanoes.

Loki and other active hotspots are hot enough relative to their surroundings that we can detect their presence even from the Earth. When we measure the infrared radiation coming from Io, we find that the flux between wavelengths 5 and 10 microns comes primarily from volcanic emissions. By measuring the variation of this flux as Io rotates, our group, in cooperation with scientists from the University of Hawaii, has found that Loki is still active and probably is one of the most important heat sources on Io.

There are two major unresolved problems associated with the calderas and the hotspots (the volcanically active areas): How much total heat is coming from Io compared with what is being put in by tidal heating? And what is the material in the calderas — sulfur or molton rock? When all the volcanic sources in our models are added up, Io's total heat output is about 10^{14} watts — an astounding figure. We thought that tidal heating must account for the volcanic activity levels on Io, but even this source has some limitations. The best theoretical estimates of these limits say that if Io has been heated by tides for the last 4.5 billion years, the average rate of energy input over that period can only be about one-half of the observed infrared power being radiated. This is an energy crisis in reverse. There are several possible solutions to this problem. There might be a flaw in the theoretical arguments from which the limit on tidal energy input is derived; we might be wrong in our estimate of the total power of volcanic sources that were not observed directly by Voyager; or Io may not always be as active as we see it now. Some interesting work on this last possibility is being done by Dave Stevenson, associate professor of planetary science, and graduate student Greg Ojakangas. They are developing a theory that suggests that the amount of tidal energy that can be turned into heat varies with time due to changes in the properties of rock as it becomes partially molten. In this theory Io becomes, in effect, a huge thermal oscillator, which is currently in a relatively hot state, but which has an average heat output over geological time that would satisfy the lower theoretical limits. There is a lot more work to be done with both observations and theory before we have a final answer to this problem.

The second unresolved problem associated with the calderas and hotspots is the debate about sulfur versus silicate volcanism, which goes back to the basic fact that Io's density indicates that it is made mostly of rock, while its surface layer seems to be dominated by sulfur compounds. Molten rock, heated by the friction of tidal changes, must be the ultimate source of volcanic activity on Io. The only question is whether most of what we see is more or less ordinary volcanism, colored by small amounts of sulfur, or whether the molten rock heats up large masses of sulfur, resulting in sulfur volcanic flows or calderas full of liquid sulfur rather than silicate lava. Certainly the mountains we see and the deep volcanic craters in some areas must be features in a relatively hard silicate rock crust; a deep surface layer of mostly sulfur would not be strong enough to support these features.



Io is composed mostly of silicate rock, but its surface is dominated by sulfur compounds. It's still uncertain whether the volcanic flows are made of liquid sulfur, or whether they are made of silicate lava colored by small amounts of sulfur.

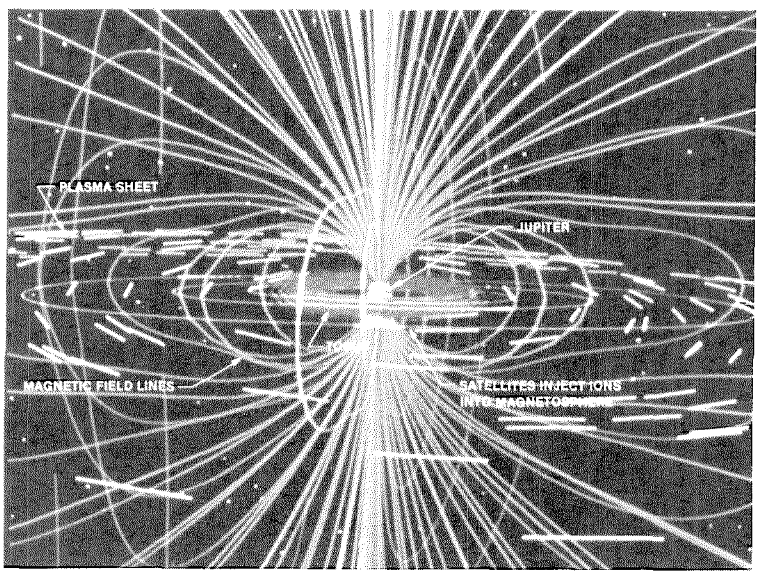
In support of the idea of at least some sulfur volcanism is the fact that the hotspot temperatures measured by Voyager and inferred from various telescopic measurements are too low to indicate the presence of large amounts of molten rock in the hotspot regions. On the other hand, these temperatures are quite reasonable for various forms of molten and cooling sulfur. In my opinion, silicate volcanism occurs on Io, and many of the lava flow features around calderas such as Maasaw are probably flows of rock lava. However, the continuous heat-producing areas associated with the dark calderas may very well be examples of large pools of molten sulfur heated from below by deeper seated silicate volcanism.

One of the consequences of volcanic heating is the large amount of material involved in resurfacing the moon, as indicated by the absence of impact craters on Io. Craters must be buried more rapidly than they are formed, leading to the conclusion that about one millimeter of new material must be added to Io's surface every year. This translates into an eruption rate of at least several thousand tons per second, and the actual rate may be even higher. At this rate Io's volcanoes have to put out as much material every month as was involved in the massive explosion of Mount St. Helens.

Over geological time the eruption rates implied by Io's lack of impact craters suggests that at least the upper portions of Io's interior have been heated, brought to the surface, buried, and reheated many times. This probably explains the lack of any water ice on Io's surface; even if Io originally formed with some ice or water incorporated in its rocks, this continual heating and recycling of the crustal material would have sweated out all the water, which would have broken apart into hydrogen and oxygen and subsequently escaped to space. Sulfur, although volatile, is harder to get rid of than water, and so we might expect a buildup of sulfur-rich material in Io's crust over time, making all of Io's surface one huge geothermal-type mineral deposit.

Another consequence of the volcanic activity is that volcanic gases, particularly sulfur dioxide, are continually being supplied to the surface to form a tenuous atmosphere. Sulfur dioxide will freeze at noontime temperatures on Io's equator, but even though it is very cold, there will always be some amount of gas evaporating from this frost and forming an atmosphere. This idea is quite similar to the one that Bob Leighton, the William L. Valentine Professor of Physics, and Bruce Murray, professor of planetary science and former director of JPL, came up with after the first Mars mission to explain that planet's carbon dioxide atmosphere and its relation to the polar caps. The major difference here is that while Mars has a surface pressure a few thousandths of that of the Earth's, Io has a maximum possible atmospheric pressure from this source of less than one ten-millionth of the Earth's. An atmosphere this thin is just a short step away from being a pretty good vacuum, but it is still thick enough to be interesting. Another difference is that the sulfur dioxide frost is stable all over the planet, not just in the polar regions, as is the case with carbon dioxide on Mars.

Sulfur and oxygen atoms escape from Io's surface, producing a torus around Jupiter.



One of the things we do know about the fate of material brought up by Io's volcanoes is that at least some of it escapes from Io and forms a potent source of charged particles in Jupiter's radiation belts. Before Pioneer and Voyager it was expected that most of the particles in the radiation belts would be protons and electrons, with a small amount of heavier elements. When Voyager measured the composition of the radiation belt particles, we found that instead of hydrogen nuclei, most of the positive ions were sulfur and oxygen nuclei, with a little sodium and potassium thrown in. This material must have had its origin at Io.

Voyager's ultraviolet spectrometer was able to detect a huge doughnut-shaped region, known as the Io torus, following Io's orbit. It's filled with sulfur and oxygen atoms emitting intense ultraviolet radiation. We can also see some of this material from the Earth. We don't know exactly how material escapes from Io, but it is probably kicked off the surface and out of the atmosphere by the impact of other particles in the radiation belts. A lot of the material probably comes off Io as neutral atoms, which are then later stripped of some of their electrons by other radiation belt electrons. Fortunately, Io provides us with tracer atoms, which, although they are less abundant than the sulfur and oxygen atoms, can easily be seen from the Earth. These tracers are sodium atoms, which glow brightly in sunlight, emitting two intense spectral lines. These sodium emissions were first detected several years before the Voyager encounters, but their full significance was not realized until they were placed in the context of the Voyager discoveries and other studies of the torus.

The next step in understanding Io will come in 1988 with the Galileo mission. The Galileo spacecraft, which is currently being built at JPL, will be launched from the space shuttle in May 1986. When it arrives in December 1988, it will spend another two years orbiting Jupiter (it will pass Io at an altitude of 1,000 km, 20 times closer than Voyager) sending back data that should help solve some of the mysteries of Io I have mentioned here — and will almost certainly deliver still more surprises. □

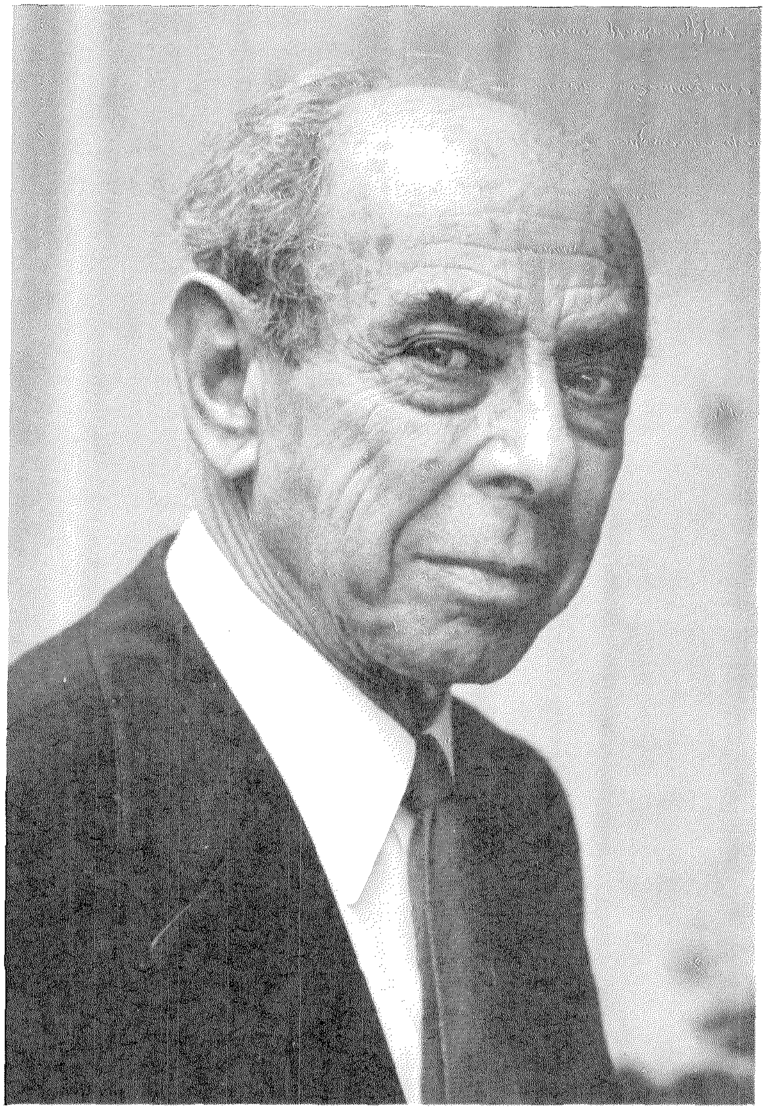
Oral History

Frank Oppenheimer

When he died on February 3 of this year, Frank Oppenheimer was director of San Francisco's Exploratorium, which he also founded (in 1969) and which many have called the best science museum in the world. For his achievements Oppenheimer received, among many other honors, Caltech's Distinguished Alumni Award in 1979.

Oppenheimer was a graduate student at Caltech for four years (in the early days of Kellogg Laboratory), earning his PhD in 1939. Before that he had graduated from Johns Hopkins and spent two years doing research at the Cavendish Laboratory in Cambridge, England, and the Instituto di Arcetri in Florence, Italy. After receiving his PhD, he worked with E. O. Lawrence at UC Berkeley and in 1945 joined his brother Robert at Los Alamos. In 1949 he was forced to resign from the University of Minnesota and banished from academic physics as a result of harassment by the House Un-American Activities Committee. He spent the next ten years as a cattle rancher in Colorado. He was eventually drawn back into teaching at Pagosa Springs High School and from there returned to mainstream academia at the University of Colorado at Boulder, where he directed research in high energy particle physics.

The late 1930s was an exciting time in physics at Caltech. Politically this was also an interesting time, and it was Oppenheimer's political activity in Pasadena that was to have such bitter consequences later on. Caltech Archivist Judith Goodstein talked to Oppenheimer last November about his years at Caltech.



Frank Oppenheimer: I met Charlie [Charles C. Lauritsen] before I came to Caltech, because he used to come to the place that my brother and my family rented in New Mexico, Perro Caliente. So, when I got to Caltech that fall [1935], he recognized me as I walked into the building. He happened to be right at the door. And he said, "Frank, do you want to smell a vacuum?" [laughter] He had just taken the x-ray tube apart. Of course, in those days, you used shellac to stop leaks, and that sort of decomposed. And it had a real foul smell, even though it was full of air at the time.

Judith Goodstein: You knew when you came to Caltech that your subject was going to be nuclear physics?

FO: Yes. I had worked in Cambridge on beta and gamma ray spectroscopy, using internal conversion and the photoelectric effect to develop energy level systems for those heavy nuclei, which was ridiculous and too complicated. So I sort of knew what I wanted to go on with, which was a wonderful thing for a graduate student, because most graduates just take courses, and then somewhere along the way somebody tells them what to do or they get interested in something. So, almost immediately, I told Charlie that I would like to make a beta ray spectograph, since they didn't have one.

JG: Had you made one?

FO: No, but I'd used one. I used the one developed by [C.D.] Ellis. That seemed a good thing because it was a hole in Charlie's nuclear physics facility.

JG: How long did it take to build it?

FO: Well, I didn't get it put together until about halfway through my second year, I think. I had to build the amplifiers and design the magnet. There was a local place where we could get crescent-shaped magnets, because they gave us a much better field. They were made in a forging place down south of Pasadena.

JG: Did you actually go into the foundry?

FO: Yes. That's why I know it was in my second year, because my wife and I went and watched him, and they had lots of ovens all along with a little cart that went along, and a man with a fork would go over and open the door and take out the forging as he would a cake or something, look at it, and see if it was ready. If not, he'd put it back. [laughter] It was beautiful to see that. The research I did was not terribly good.

JG: Why do you say that?

FO: Well, because I thought I had licked the problem of scattered electrons in this by various veins. I was looking for a gamma ray from nitrogen 13 that somebody had found, and didn't find it, but I found a peak in the electron energy spectrum, which said that my apparatus was scattering electrons. The spectra were good at the higher energies. I looked at the shape at higher energies, and it agreed with other people's conclusions that the neutrino had very little mass.

JG: Did you have much contact with the other graduate students? I think Charlie Lauritsen's son [Thomas] was a graduate student at the same time you were.

FO: He was an undergraduate, I think, when I first started, and then was a graduate student after that.

As I remember, you'd come into the building from an alleyway, and then over here was the x-ray facility. My office was the second one along there, looking out into that courtyard just in front of the Kellogg Lab. So it was right where everybody else was. Then, Charlie at that time, as I remember, had his students over to his house every Friday evening, or maybe every other.

JG: Is that really where most of the seminars took place — at his house?

FO: There were other seminars, but on Friday nights there was always a discussion of the work that was being done. They would talk about that, and then it would gradually develop into a party of some sort. They just said what they were doing, or talked and argued about things. I remember one time, Jackie, my wife, listening to this, asked a question. It was a question of something about nuclei, and everybody just stopped talking and stared at her for a moment, and then without answering just went back to talking. [laughter] They were just so astonished that some stranger would interrupt them. In addition to this, I saw them an awful lot — Willy Fowler and [Lewis A.] Delsasso also. It was a very nice group. Tom Bonner was working mostly with neutrons, almost exclusively with neutrons. And Charlie and Willy were looking at energy levels of light nuclei.

JG: Did you ever find yourself attracted to that? To going into working with the nuclei of the light elements?

FO: Well, in a way I was, but doing it indirectly through the gamma rays.

JG: Do you remember any interest in nuclear astrophysics on the part of the Kellogg group before World War II? Bethe's paper would have come out in '38 or '39. And, of course, then the war intervened. But I was wondering if they made the connection.

FO: I think they did, and I think the groundwork was laid for saying, "We ought to find out how to measure some of these cross sections," but the technique for measuring the cross sections — the sensitivity — wasn't there.

JG: That's why, apparently, they decided to build an electrostatic generator.

FO: Well, the Van de Graaff was already built when I arrived. Then they [Fowler and Tommy Lauritsen] built a second one.

JG: When you left, what happened to the apparatus that you built?

FO: Well, another graduate student, [E. P.] Tomlinson took it over.

JG: Was it used for a long time afterwards?

FO: I think for a while. I don't remember when it got to be disused. It probably wasn't used much during the war.

JG: No, I think they essentially shut down the lab. I noticed you told Charlie that you weren't really learning much about nuclei from the experimental approach at the Cavendish. What was different about it?

FO: Well, I think what I was doing was different, because [John D.] Cockcroft and [Ernest T.S.] Walton were learning a lot about deuterium and whatnot. But I had done a nuclear level system for radium C, and it broke up, for some reason, into two groups, each with about 12 levels, and I couldn't give assigned quantum numbers to these levels from the transition rates. But it was like looking at the iron atomic spectrum rather than at the hydrogen spectrum.

JG: So your comment was more about your own work.

FO: Yes. We did learn — and corroborated by making the level system — that the radium C has two branches. One is an alpha emitter and then goes to radium D. And the other's a beta emitter, and then it goes with an alpha emission. And so there's these two ways around that end up with the same product, and one has to find out whether the energy emitted is equal for both ways around. In one case some of the energy was emitted through a gamma ray, and the level system showed that had to be so.

JG: Where did you actually do your experiments?

FO: In that office. Because they would rush radioactive material, even if short-lived, over from Kellogg. But also, at that time, I checked the apparatus because they had a radon source which could put down radium [radium A] that just gave alpha particles and decayed to radium C and C', so I could check the gamma rays from those radioactive nuclei that I was familar with, and they really gave nice curves, so I knew the thing was working all right.

JG: Who made the radioactive sources for you?

FO: Well, they had a little facility in which you could go in yourself and collect them on the little buttons you were going to put in the apparatus.

JG: So it was a do-it-yourself operation. Once you made it, you had to rush right over and use it.

FO: Well, those had fairly long lives. But the ones that were artificially made, Willy would make for me. The natural radionuclides were used mostly to test the apparatus and the artificial ones to learn something new.

JG: Did you get involved at all in the cancer work?

FO: No. I knew about it, but I wasn't doing any of it.

JG: Someone like Willy, when he came, didn't receive any graduate stipend in money. He received it in services. So he had to do some work on the cancer therapy project. Then he received his room and board in the Athenaeum. Now you were different from him in that sense.

FO: Yes, because I didn't have to pay any tuition, but I used family money to live on. I finally made some money there. I made a huge, eight-channel coincidence analyzer. You could measure anything you wanted. You could put some in anti-coincidence and some in coincidence. You could use it in coincidence with the biology analyzer that belonged to [Henry] Borsook [the biochemist]. He paid me for making it.

JG: What were your contacts with other people at Caltech?

FO: Well, quite general in a way. Through my brother, probably in part. But I got to know the Tolmans very well. And Ruth [Tolman] and I played piano and flute over at Ruth Valentine's house almost every Friday. The first year Jackie and I were married the Tolmans came to dinner at our house. I don't know how I got to know Borsook; he was a great vitamin B enthusiast at that time. My wife had stomach aches. He thought she should eat wheat germ. And we treated it as a cold cereal. And he finally told us it wasn't doing any good. We had to cook it to break down the vitamin B. [laughter]

JG: Did it work when you cooked the wheat germ?

FO: No.

JG: Did you know Thomas Hunt Morgan?

FO: Yes. I didn't know him well. But then, von Kármán was there, and I knew one of his students, the Chinese fellow [Hsue-shen] Tsien, very well. Then there were other people. I knew Frank Malina very well and Sidney Weinbaum.

What happened with the Communist Party was, I had been close to sort of slightly left-wing things starting in high school. I remember once I went with some friends to hear a concert at Carnegie Hall that didn't have a conductor. It was kind of a "down with the bosses" movement. [laughter] I was also doing the Al Smith campaign during my high school years. Then when I went to Hopkins I knew quite a few people who I didn't know whether they were party members at all, but they were interested in left-wing politics, and I learned about it. And then a little more on the fringe of it when I was in England, and then went to Italy, and there were people there of varying degrees of leftness. Occhialini was quite left. Fascism was in Italy when I was there — the year before the Abyssinia war. There was a brigade of soldiers just below the lab there who were always singing and cheering.

JG: Were they dressed in their black uniforms?

FO: Yes. But they weren't threatening like the people in Germany somehow. I asked my colleagues about Italian fascism. And I think it was Bernardini who said that he didn't think it was a dangerous thing, that it wouldn't have any serious effect on repression of Italy.

Frank Oppenheimer at Pagosa Springs High School in Colorado, 1959.

JG: Did you feel threatened by fascism at all?

FO: No. Although I had been close to this, I wasn't terribly knowledgeable about what was happening. I did when I was in Germany, very much. The year before I had gone to see people marching down the streets, and really sort of lashed out at this behavior in the bars, and the whole society seemed corrupt. And then I had some relatives there who could tell me some of the terrible things. But in Italy the soldiers didn't seem especially aggressive. I never saw any of them marching. The policemen weren't any different, and were probably gentler, than New York policemen. The town seemed very relaxed.

JG: Did you ever see any of the Rome physicists in Florence?

FO: Not then, no. So I never met Fermi until many years later. One of the nice, interesting things was that Occhialini and I got to be very good friends. When I left, he gave me a farewell party in a cave. We walked way back in and came to a huge room and had the party. But Bernardini said something to me in that time that was really surprising. He was 29 and I was 23. And he said, "You know, Frank, I thought I'd reached the age beyond which it was possible to make friends. Now I've learned differently." It was really scary to think you might, at 29, have reached that end.

JG: Was there any choice of where you might have gone after that?

FO: Yes, I had a terrible time deciding whether to leave there or not, or to go to Caltech. I don't know, I think I must have written them about my wanting to come there. And my brother probably did something. But I don't remember any elaborate applications to go to Caltech. The same thing was true with going to Johns Hopkins. I had a friend who suggested I go there, and I probably wrote a letter and got one back. And that had nothing to do with my brother or my friend either.

JG: So you came to Caltech. Characterizing discussions there with graduate students and professors — were they aware of what the political climate was like in Europe? Did they care?

FO: Yes, because of Spain. We were all talking about Spain. Ruth Tolman and I even gave a benefit concert for Spain. [laughter] I think they were aware. You see, my wife, when she was a student at Berkeley, had been exposed to radical influences and had been a member of the Young Communist League. And we saw an ad in one of the newspapers, asking people to join, and we clipped it and sent it in. And it was months before anybody came by. [laughter] I think we had to send a second one. So it was that kind of a casual thing. But then we became very active.

JG: While you were still a graduate student?

FO: Yes, first in the city, in what was called a street unit, in which there were mostly inhabitants and a lot of black people who lived in Pasadena at that time. We had meetings regularly and discussion groups. There were various organizations connected with the New Deal. One of them, the Worker's Alliance, was an organization of the unemployed, and many of these people were unemployed.

JG: Did you have the feeling that there was a great deal of unemployment in Pasadena?

FO: Among the black people there, yes. They were so poor, but you didn't see them, and it wasn't like New York, where you'd see them out in bread lines. We tried to integrate the Pasadena city swimming pool, I remember. And it's really hard to imagine — they just allowed blacks in Wednesday afternoon and evening, and then they drained the pool Thursday morning.

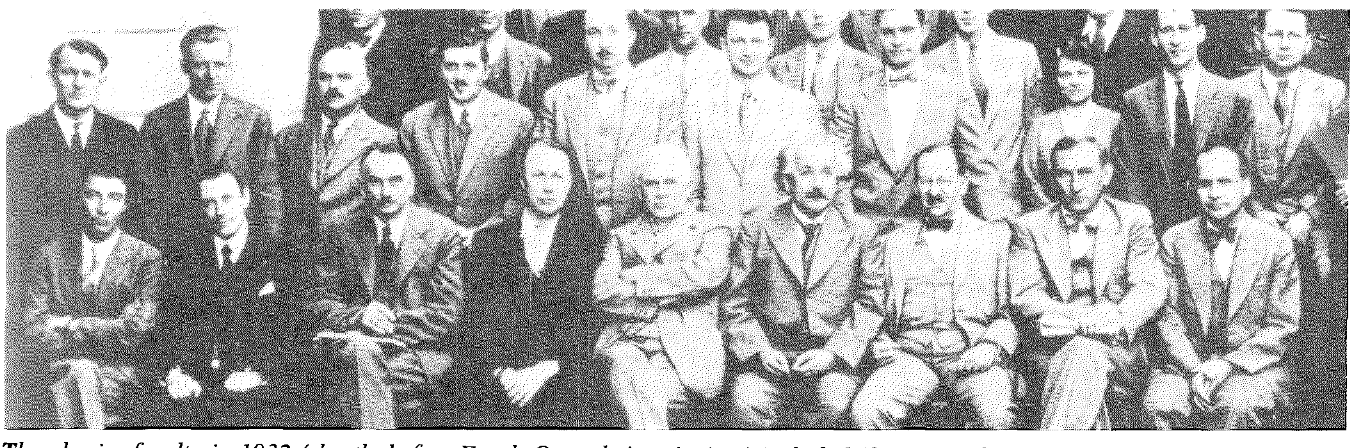
JG: How successful were you?

FO: It wasn't successful. But then I was asked by the Party organization to try and organize a Party group with people at Caltech. So Jackie stayed with the street group for a while, and then later on she moved into the other group.

JG: So, how much success did you have organizing a group at Caltech?

FO: Well, I don't know. There were six, or eight, or ten people.

JG: All the people who were caught up in the McCarthy period, were they all members of your group? I mean, Malina left JPL after the war. Weinbaum went to jail. Tsien left eventu-



The physics faculty in 1932 (shortly before Frank Oppenheimer's time) included (front row, from left) Robert Oppenheimer, Harry Bateman, Richard Chace Tolman, William Houston, Robert Millikan, Albert Einstein, Paul Epstein, Fritz Zwicky, and Earnest Watson. Charles Lauritsen stands directly behind Oppenheimer.

ally. They're all gone.

FO: And a few others.

JG: Was it secret?

FO: It depended on the person. Jackie and I were quite open about it. And I would rent meeting places. Other people on campus were very secretive about it. A lot of people would get into political discussions, and some avoided anything political. So there were all kinds of things. But it was essentially a secret group.

JG: Did that bother you?

FO: Well, it did me, but I wasn't secret about it.

JG: So, are you saying the secrecy was imposed by most of the people who belonged to it?

FO: That's right. Because they were scared of jobs.

JG: Would you often meet on the campus?

FO: We would meet in people's houses.

JG: If you had to do it all over again, would you have done the same thing?

FO: I can't say. I know things now. Jackie and I finally left the Party because they had something that they called "democratic centralism," in which, if there was a policy, the groups were supposed to discuss it, to let the leadership know — a back and forth thing. But it really wasn't; there was "centralism," but no "democratic." So, we got fairly upset with that. And also, certainly after the war, the attitude of the Party was not at all concerned with nuclear weapons. And it was pretty much just a duplicate of the Soviet policy.

JG: When did you leave the Party?

FO: We actually left in the spring of '41 or the fall of '40 — I think it was the spring of '41.

JG: When you were at Caltech, was Linus Pauling already politically active?

FO: Yes.

JG: Did he come to any of your groups?

FO: No. I don't think he gave speeches, but in private conversation he was very interested in what was going on. One of the big issues then was support for the Spanish loyalists. And I don't remember exactly whether he did or didn't support them, but it would seem likely that he was interested.

JG: Was that an issue that polarized the campus in any way?

FO: I don't think so — not the people I knew. One of the things about my work in the Party is that I would go with my brother to New Mexico in the summer, so I would be gone for two months. Then, the three years that I was at Caltech I was really quite active with the Party, so I didn't work nights as much as I did the first year. With going away in the summer and without working nights, you really don't get enough done. Charlie didn't object to that, never chastised me for not being there. He made one of his wonderful cracks one time when he came in and I had just gotten a cigarette holder — I smoked a lot then — and I was soldering away on the amplifier. He remarked on the cigarette holder. And I said, "I had to get it because smoke gets in my eyes." And he said, "Well, if you really have a choice, I'd give up soldering." [laughter]

JG: *He* did work night and day?

FO: Yes. I think I did, too, quite often, but not as much as I could have. And I think all the other graduate students knew about Jackie's and my radicalism, and we'd argue about it. But there was never any sense of "you don't belong here" with that community. I'm sure with [Robert] Millikan it didn't sit very well.

JG: After '33 in Germany, and after '38 in Italy, of course, there were many scientists who lost their jobs. Was there ever any discussion of bringing some of these people to Caltech?

FO: Yes, though I can't remember the details.

JG: When you look at the statistics, not all that many émigré scientists really found jobs in the U.S. And there were none at Caltech.

FO: I wonder why, because I remember talk of it. But I think that may have been something that Millikan wasn't all that enthusiastic to do.

JG: I was wondering, for example, would Charlie Lauritsen have proposed somebody, and did you ever hear anything about it?

FO: I have a vague recollection of yes, that there were talks about it, that whatever was talked about didn't happen. But I don't remember why or who.

JG: Was Lauritsen a good adviser?

FO: Yes, a very good adviser, and you could talk with him when you wanted to. He didn't come in and ask you what you were doing very much. But, of course, he didn't have to, because of the Friday meetings where you told what you were doing or not doing. I think I discussed what I was doing more often with Willy than Charlie.

JG: Do you have any sense of what Charlie liked best about his work? Did he like the physics as much as building the equipment, making it work?

FO: Yes. He liked the logic of the physics.

JG: Was he a good lecturer?

FO: I don't know.

JG: Well, they used to have the Thursday afternoon colloquium in physics.

FO: Yes, but I don't think Charlie ever gave one. Willy gave one, I gave one — mostly other physicists. I don't think I ever heard Charlie give a lecture — maybe it's just a lack of memory. Willy was nice. I said, "Are you going to give a colloquium?" and he said, "No, I'm writing it on the board, and I'm going to point at it."

JG: When you were there, do you remember anybody coming down from Berkeley, from the nuclear physicists? Did [Ernest O.] Lawrence ever come down and give a talk, or any of his boys?

FO: Well, there was that group from my brother that would come down. I remember Lawrence coming to the Cavendish and giving a talk, once, when he was really confused about what was going on, when they first found the neutrons from deuterons, but I don't remember his coming down to Pasadena and giving a talk.

JG: Did you ever hear Millikan lecture?

FO: Yes. Very dogmatic! Terribly dogmatic! Pretty clear, but not always believable. [laughter]

JG: I was going to ask you about some of the professors you had. What was [Fritz] Zwicky like? He's sort of legendary around the campus.

FO: Well, I had a problem there. My first year in Hopkins, I took a sophomore or junior course in mechanics at the same time I was taking calculus. And I think I must have found it sort of hard. I got good grades in it, but I didn't like the subject. And I tried to make up for that by always, whenever I went to Paris, I'd take my mechanics work with me and do it. [laughter] I must have really hated it. But then Zwicky's course in mechanics came at eight in the morning. It was the first year I was married, and I didn't get there very often. So he was going to give me an F. And I said, "Well, give me a test in it." And he gave me a test in it, and I passed the test and didn't get the F. So I really didn't like Zwicky very much. [laughter] And I didn't like the way he taught the course, the mechanics, either.

JG: Was he dogmatic?

FO: Well, I don't know. It wasn't so much dogmatic. It was all in the formulas and not in the meaning or utility of the subject, as I remember it.

JG: Did Lauritsen ever instruct you in the details of nuclear physics right there in the lab? Was he good at that?

FO: Yes. He'd explain things. He was very clear. That's what I meant — he really loved the logic of the subject.

JG: How was Epstein for thermodynamics?

FO: I liked him, I really did. And I liked the subject. It's a strange subject; it always seemed like sort of a swindle — sort of results without any physical input. But I liked the subject, I liked his teaching, and I liked his accent. [William Ralph] Smythe, I thought, was a little more boring, for a really juicy subject [electricity and magnetism]. And he had a real prejudice against vectors. [laughter] You know, his book is all X, Y, and Z written out.

JG: Did you go to the course that your brother taught? Carl Anderson took it.

FO: I listened to many of his lectures, but I don't think I took anything for credit. The course work at Caltech was nice because it came as not something you had to do before you could do any physics. I was really glad that I'd been to the Cavendish.

JG: Of the three labs, which did you enjoy the most?

FO: They were so different. Probably the Cavendish in some ways because there was so much going on there, and it was all new to me. And they met every day for tea, and you could listen and hint at what was going on. [Petr L.] Kapitsa was there, [Sergey P.] Karpoff, [James G.] Mauldon, [George] Gamow would come around. It was a very exciting place.

JG: And you heard every day what everybody was doing.

FO: That's right. I don't know why we don't stop and talk to each other more.

Caltech was a lovely place to go to get a degree, partly because of Charlie's group, but partly because of the contacts with other people.

JG: Was the general level of the Institute one of excitement?

FO: Yes. It was a small place, but aeronautics was doing interesting things, and I knew them through Malina and Tsien. And Carl [Anderson] and Seth [Neddermeyer]. And then [Jesse] DuMond — I didn't have much contact, but enough to know what he was up to.

JG: So there must have been informal ways to know what everybody was doing.

FO: That's right. I don't know what they were exactly.

JG: The story is that if you came to campus any season, there would always be work going on here at Kellogg.

FO: Yes. And that's why I say it was very strange to be away in the summertime.

JG: Well, in fact it doesn't seem to have held you back. You seem to have taken your PhD in about the right amount of time.

FO: Right. But I think I could have done better work. At the Cavendish Lab they closed the place down at night unless there was some kind of apparatus or experiment that had to be looked after. Rutherford's law (at least I never heard him say it differently) was that if you haven't done enough in the day to think about it at night, it is not worth coming back at night.

JG: Well, what do you think? Now, there you have a study in contrasts, because in Kellogg it's the opposite.

FO: Yes, it was just the opposite. In most places it was just the opposite. I think Rutherford's idea came from doing somewhat more straightforward experiments with radioactivity. I thought Rutherford was never really interested in theory. But when you read his works, he was always trying to think, "What does this mean?"

JG: What about Lauritsen? Was he also interested in theory?

FO: Not so much, I don't think. I think he wanted to think about what was happening, but the theory had gotten — well, there was lousy theory in those days. And I think he knew it. [laughter] I wasn't much help.

JG: What did they do in Italy? Did they run late at night there?

FO: No, not especially. When we were fairly late, maybe six or seven o'clock, a group of us would walk to some place where everybody had to go in different directions, and they'd stop and sing for a while.

JG: Were they traditional songs or were they political songs?

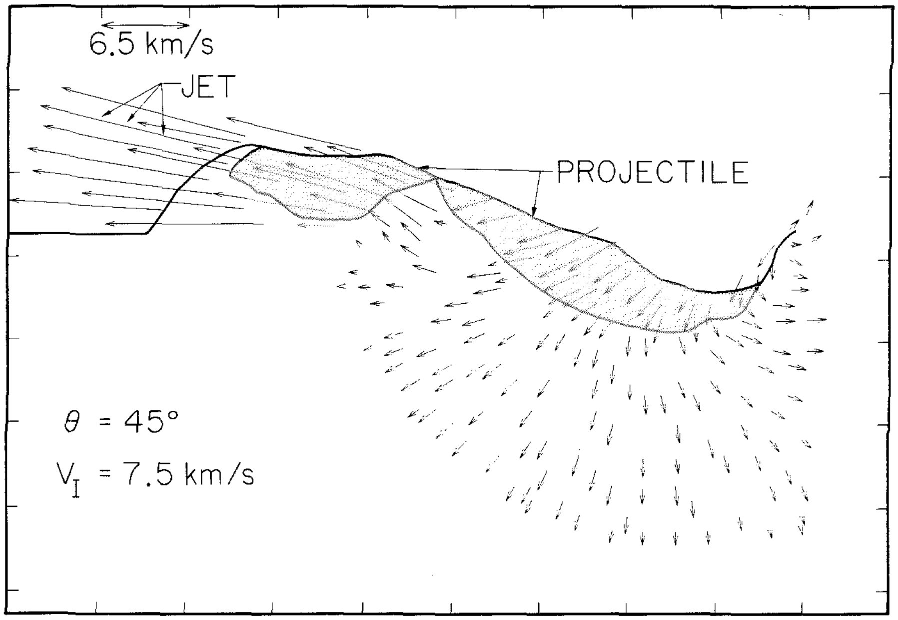
FO: They were traditional songs, I think. □

Research in Progress

The Martians Have Landed

BETWEEN 1968 AND 1980 NASA spent $979.1 million on the spectacularly successful Viking program, which sent two unmanned spacecraft to study the planet Mars. It now appears that during all that time at least eight pieces of Mars may have been right here at home. They apparently fell to Earth as meteorites and are currently under intense study by scientists around the world.

Although these meteorites differ significantly from the 10,000 or so others in the world's museums, not all scientists are convinced that they could have come from Mars. It isn't their chemical composition or their age or any of their other characteristics that make researchers reluctant to assign these rocks a Martian origin. Rather, the stumbling block to full scientific acceptance is the question of how they could have left Mars in the first place and how they could have arrived here in such good condition. Recently, however, Thomas J. Ahrens, professor of geophysics, and John D. O'Keefe, visiting associate in planetary science, have devised a scenario which just may convince their skeptical colleagues that these meteorites really did come from Mars.



In this simulated meteorite impact, a spherical object has just hit the Martian surface at 7.5 km/sec and at an angle of 45°. The impact produces a high-velocity jet of hot gas that can lift large boulders into interplanetary space. The arrows point in the direction of motion. Lengths are proportional to velocity.

Except for their relatively pristine condition, everything points to a Martian origin. They all crystallized from molten rock about 1.3 billion years ago, as determined by the ratios of samarium to neodymium, rubidium to strontium, and potassium to argon. The asteroids, source of almost all other meteorites, cooled completely about 4.5 billion years ago, and volcanic activity on the moon ceased about 4.2 billion years ago, so both lunar and asteroidal origins seem to be ruled out. In addition, these meteorites have spent only between 2 and 10 million years in space, as judged by their cosmic ray exposure times. (Meteorites derived from asteroids typically spend far longer times exposed to cosmic rays on their transit to Earth.) And Donald Bogard of the Johnson Space Center has determined that the composition of noble gases trapped in the meteorites is consistent with what we learned about the Martian atmosphere from instruments on the Viking landers.

Together, these putative pieces of Mars are called the "SNC meteorites" after the locations at which three of them were found: Shergotty (India), Nakhla (Egypt), and Chassigny (France). Two others were found in Antarctica and one each in Nigeria, Indiana, and Brazil. The SNC meteorites are members of the achondrite class of igneous meteorites but differ significantly from the eucrites, the most common achondrite type. In composition, some of the SNC meteorites are olivine, some are clinopyroxene and olivine, and the rest are pyroxene and plagioclase. These rocks resemble samples of the crust and upper mantle of the Earth. Their textures show the clear imprint of having cooled from a melt in a gravitational field, one that must have been far stronger than those associated with even the largest asteroids.

But though their chemical and geological characteristics all point to a Martian origin, it's not easy to understand how the SNC meteorites could have left the red planet's surface. Martian escape velocity is 5.05 kilometers per second (11,300 miles per hour), and some process would have had to accelerate large boulders to this speed in order for them to have escaped Martian orbit. At first glance, the most likely mechanism for this would be the impact of a comet or asteroid directly perpendicular to the Martian surface. In response to such an impact, material could have rebounded from Mars at sufficient speed. Unfortunately for this model, however, any such material would have been pulverized, or at the very least thoroughly melted. Some SNC meteorites do show evidence of shock-induced melting, but this melting is not nearly extensive enough to accord with the direct impact model.

Another possibility is that the material ricocheted off Mars after a grazing meteorite impact. Although relatively rare, such impacts are by no means unknown; Mars has more than 170 large elongate craters with characteristic "butterfly" ejecta patterns. But according to Ahrens, this too is an unlikely scenario. "It's a good idea, but it doesn't accelerate materials to high velocities. In fact, it doesn't even produce material that's *molten* at 5 kilometers per second. Although at first blush you might expect it to be a good way of launching material, because you do launch a lot of material preferentially along the direction of impact, the velocity that it's launched at is relatively low."

Ahrens's work involves the study of materials undergoing high-velocity impact. His laboratory, the Helen and Roland Lindhurst Laboratory of Experimental Geophysics, contains two powerful guns capable of accelerating projectiles to high speeds. When the larger of these, a 106-foot-long, 35-ton, light gas gun, is fired, all South Mudd shakes, and a one-ounce plastic and tantalum bullet attains a speed as high as 7 kilometers per second (16,000 miles per hour). According to Ahrens, this apparatus can produce impacts with dynamic pressures as high as any occurring anywhere in the solar system.

Using the data gathered in this way, O'Keefe and Ahrens build "equations of state" that describe the behaviors of various materials under various impact conditions. These equations are then fed into a computer program that, in Ahrens's words, "conserves energy, mass, momentum, motherhood, and apple pie," to simulate the effects of meteorite impacts on the Martian surface.

The results of these simulations suggest that the best way to get material off Mars involves neither perpendicular nor extremely shallow impacts. Rather, material is most likely to be accelerated past escape velocity if the impact angle is between about 25 and 45 degrees from the horizontal. Such impacts vaporize the water, carbon dioxide, and other volatiles trapped in the Martian crust and produce a jet of hot gas that can reach a velocity of 20 kilometers per second. This is sufficient to sweep rocks ranging up to one meter in diameter off the surface of Mars, at speeds greater than 5 kilometers per second, without damaging them in any appreciable way.

Although these rocks escape the Martian gravitational field, says Ahrens, "they probably don't get a high enough velocity to escape the solar system and they go into orbit around the sun. Objects that are launched into somewhat eccentric orbits become perturbed in orbit as a result of the very slight gravitational effects of the planets. Probably most of these objects go into Mars-crossing orbits and, in time, fall back to that planet. But a small number leak out of the zone around Mars and eventually go into Earth-crossing orbits." And a small number of those fall to the ground to be found and puzzled over by terrestrial scientists. □ — *RF*

Books

Goodwin Wharton

by J. Kent Clark

Oxford University Press........$29.95

In the marvelous musical show he wrote for Lee DuBridge, Caltech's J. Kent Clark, professor of literature, introduced a song entitled "The Woman Behind the Man." The lyrics were so complicated only Kent could sing them. "There's a woman behind the man behind the plot behind the plan behind the triumph or the random situation," the song began. Now he's written a book to illustrate his point.

The man was Goodwin Wharton, second son of Philip, Lord Wharton, who served as a lieutenant in the Parliamentary Army during the Puritan Revolution against Charles I and became a leader of the Whig coalition after the restoration of the Stuarts. Lord Wharton's children found their father, as Kent writes, "easier to honor than to obey, easier to obey than to love, and easier to love than to please." This was particularly so for Goodwin after his father forced him to surrender his claim to the manor of Wooburn, a fine estate his mother, Jane Goodwin Wharton, who died in 1658 when Goodwin was but five years old, had arranged for him to inherit. Goodwin didn't get along with his father, in part because Lord Wharton favored Tom, Goodwin's older brother, and in part because Goodwin seemed always to need financial aid. He was kept impoverished by a series of disastrous business ventures including unsuccessful but expensive experiments designed to turn baser metals into gold.

Goodwin's fortunes did not

improve until 1689 when, at the age of 36, he was elected to the House of Commons. King William had succeeded James II, and Goodwin's younger brother, Henry, had died, leaving a safe Wharton seat open in Parliament. Goodwin's hard work thereafter was rewarded as his father paid his debts and King William made him a Lieutenant Colonel of the Cavalry and ultimately Lord of the Admiralty. Like his brother Tom, Goodwin became a respected Whig politician. Following the death of Lord Wharton in 1696 (after a reconciliation between father and son), moreover, though Goodwin did not inherit Wooburn, he became wealthy and lived well until his own death in 1704.

The Woman Behind the Man was Mary Tomson Boucher Lawrence Parish, whom Kent describes as "one of the most imaginative and versatile women that England has ever produced." Kent might have added that Goodwin was one of the most gullible men, for Goodwin believed whatever Mary told him regardless of how outlandish. Mary told him, for example, that she talked regularly with a George Whitmore who had been hanged for highway robbery outside of Ludgate Prison some years before. Through George, Mary said she learned wondrous things including the location of hidden treasures and of a fairy kingdom near Heathrow. Needless to say, Goodwin wanted to speak with George directly, but Goodwin couldn't see George even when "Mary announced George's presence and indicated that he was 'standing before her'. . . ."

> Goodwin was forced to launch his arguments at the apparently empty air. Then, since George refused to talk with him directly, he was obliged to leave the room while George made his answers, which could sometimes be heard, indistinctly, through the door and which Mary later repeated for Goodwin's benefit.

Goodwin met Mary, a very common woman, "'in her sorry little lodging in a poor beggarly alley and a very ill house'" shortly before his 30th birthday. He was referred to her because she had a reputation "for wisdom in abstruse matters" such as speaking to angels. "Whether the subject was astrology, alchemy, buried treasure, pharmacology, or the making of lucky charms," Goodwin found that she gave "very smart answers" to all his questions, and he soon proposed that she "detatch herself from the lowly people who sought her advice" and form a partnership with him. For her part, Mary found security in her alliance with this young nobleman. For 20 years, Mary led a willing Goodwin through a maze of inaccurate predictions and occult misadventures, including a love affair with Penelope, the invisible queen of the fairies, and conversations with angels and the Lord God Himself, who usually spoke (through Mary) in verse:

> For when tomorrow thou has got a name
> It shall be to thy family an everlasting fame
> When they see that they will their pardons crave
> And be much sorry for the ill that to thee done they have.

Mary even induced Goodwin to believe that he was a father several times over. The day after Goodwin and Mary first made love,

> . . .Mary announced that she was pregnant. Her condition, she explained, had been easy to diagnose, since she had been pregnant many times before. Furthermore, she said, she knew the child would be a boy! Since she had learned "by a maxim" how to produce children of either sex. . . .

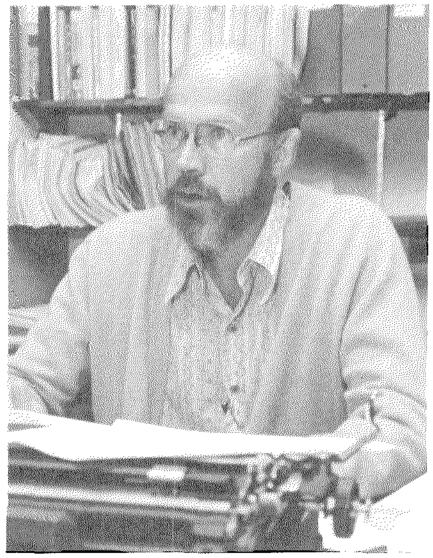
Mary was then 53; Goodwin was 30. "And only two nights later, she reduplicated her feat. . .[and] announced she had again conceived of another boy." Over 20 years, Mary claimed to have borne something like 50 of Goodwin's children, only one of whom Goodwin ever saw, and he was borrowed for the occasion.

For 20 years Goodwin believed he could work miracles, including seducing three successive queens of England, raising sunken treasure, and winning great military victories. Mary's prophesies were almost always wrong, but Goodwin believed in her and, therefore, in himself. He was a marvelous example of a late 17th-century Englishman, full of superstition and exuberance.

The evidence that Goodwin believed what Mary told him is chronicled in a 530-page autobiography and journal Goodwin wrote largely for the benefit of his oldest, non-existent son. This autobiography-journal is in the Huntington Library in San Marino, where Kent labored for more than a decade between Caltech committee meetings, classes, and other chores. Kent also spent considerable time in England and France searching archives, libraries, and galleries and walking the ground Goodwin had walked. His scholarship is impressive and his product is factual, instructive, entertaining, wonderfully whimsical, and, because it is written as only Kent can write, delightful.

Many regard Kent Clark as Caltech's unofficial Poet Laureate, and many of Caltech's extended family will want to enjoy this *magnum opus* of this perceptive, sensitive, literate, literary professor. □ — *Robert W. Oliver, Professor of Economics.*

Opinion



by Robert H. Bates
Professor of Political Science

Bates is the author of the book "Markets and States in Tropical Africa." This essay appeared in the Washington Post *in February of last year.*

SOUTHERN AFRICA faces "the worst drought in a century," according to the Food and Agricultural Organization of the United Nations. Almost 20 other nations in eastern, western, and central Africa face catastrophic food shortages.

This year's disaster highlights an important fact: For years Africa has been a continent that does not feed itself. The crisis extends from food crops to export crops. There is a crisis in African agriculture.

Since the 19th century, Africa has produced vegetable oils for soap, cosmetics, margarine; beverages, such as coffee, tea, and cocoa; and fibers, such as cotton and sisal. While in the colonial period these crops provided the foundations for the economies of Africa, recently farmers who produce these crops grow less, export less, and earn less in foreign markets.

The decline of agricultural exports means less foreign exchange. In countries where taxes are levied largely from foreign trade, it also means a loss of public revenue. Foreign payments and fiscal crises — these are the results of the decline of agricultural exports.

There are many reasons for the crisis in African agriculture. Belying the myth of the lush tropics, the environmental realities facing Africa's farmers are harsh. Yet many experts now lay the major blame for the current crisis on the economic policies of African governments. These policies place heavy burdens on African farmers and create strong disincentives for farming.

To secure imports for new industries — and for elites with a taste for foreign products — African governments overvalue their currencies. As a result, the African food producer must now compete with the American farmer, because imports of foreign foods appear inexpensive to African consumers. A parallel consequence is that the producers of export crops, such as cocoa or palm oil, earn less. Overvaluation of local currencies means that the producers of exports earn foreign exchange that is now worth less in domestic currencies. Overvaluation for the sake of imports for urban and domestic industries places an economic burden on the African farmer.

So do government taxes. In many African nations, farmers producing exports are compelled to sell through government monopolies. These market their crops on the world market, retaining much of the proceeds in the form of taxes and returning only a portion (often less than half) to the farmer.

Government efforts to protect their new urban industries place further burdens on the farmers. By protecting the domestic market against imports of cheaper foreign goods, governments force farmers to pay higher prices resulting from tariff protection. Moreover, governments insist on low food prices. They do so because higher prices mean higher wages and lower profits for industry, as well as political unrest in the cities.

A range of government policies thus harms the economic interests of farmers. African farm families are like the rest of us. If an economic endeavor fails to offer an adequate return, they move out of it and into another. Some leave the countryside. Others remain but educate their children and send them to the cities. Others don't work as hard, earning subsistence but seeing little profit in producing a marketable surplus. Few invest in new technologies to improve their farming. There is too little return to justify such investments. In the face of few economic rewards, farm families devote fewer and fewer resources to farming. The result is an African continent that cannot feed its people.

Criticism of the policies of African governments have been mounted from every side. Most often they have been rejected. One reason is that while these policies may be economically costly, they have appeared to be politically necessary. Insecure governments promote "cheap food" policies to placate urban consumers who might otherwise back the political opposition. And they use the taxes from exports to provide benefits demanded by restless constituents.

But now the overwhelming reality — political and economic — is one of shortage. In the face of famine, governments may have little choice but to change their farm policies. African governments have to devise ways of rewarding the economic interest of farmers. As a result of current suffering, Africa's rural population may at last be incorporated into the basic political arrangements that underpin the economic order of the continent. □

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