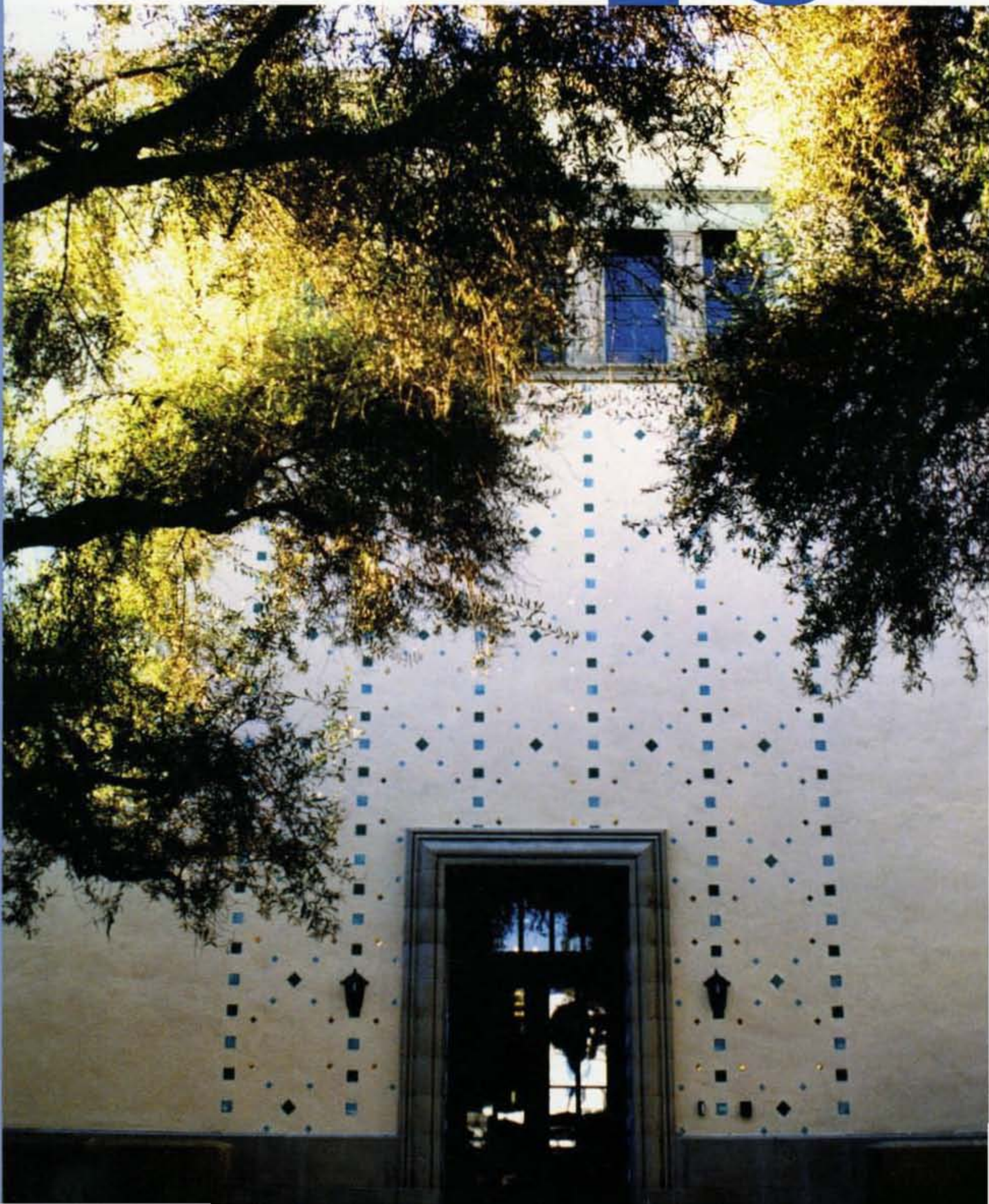
California Institute of Technology

E&S

ENGINEERING AND SCIENCE

Volume LXVII, Number 3, 2004

IN THIS ISSUE

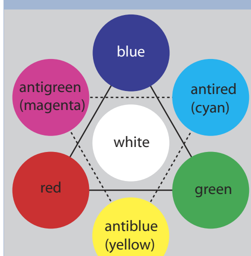


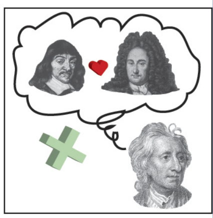
***The Tender Land: A Pasadena Festival of Art, History, Music, and Science*, opened on October 9. The collaboration of 14 local organizations takes its title from the Aaron Copland opera, and this "tender" land is fragile as well as nurturing. Caltech's participation includes this installation, *Stellar Mapping I*, by Lita Albuquerque, on the Moore Walk by Avery House. A meditation on the theory of stellar nucleosynthesis, for which Caltech physicist Willy Fowler (PhD '36) shared a Nobel Prize in 1983, it consists of bell jars filled with material representing the chemical elements formed in supernova explosions. The jars are arranged in the shape of the constellation Lupus, where the brightest supernova in recorded history occurred in the spring of 1006 A.D. (Recent calculations put its visual magnitude at about -7.5, roughly halfway between that of Venus and the full moon.) The festival runs through January 31. For a full listing of events, see www.tenderland.org.**

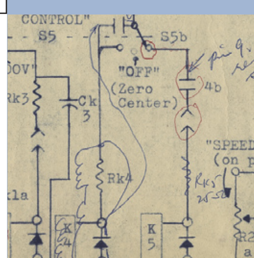
California Institute of Technology

ENGINEERING AND SCIENCE

Volume LXVII, Number 3, 2004







On the cover: When he drew the plans for Dabney Hall, architect Bertram Goodhue designed a pattern of colored tiles to ornament the building's north façade. For one reason or another the tiles were scrapped, and the stucco wall faced the campus naked and unadorned. When Dabney was restored during the past year, Caltech architect Brad Smith followed Goodhue's original vision, to stunning effect. The restored Dabney Hall was rededicated on September 17; see the inside back cover.

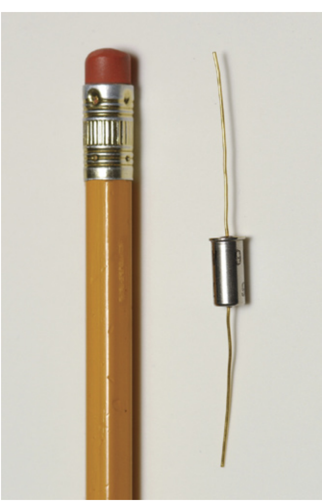
Engineering & Science (ISSN 0013-7812) is published quarterly at the California Institute of Technology, 1200 East California Boulevard, Pasadena, CA 91125. Annual subscription $10.00 domestic, $20.00 foreign air mail; single copies $3.00. Send subscriptions to Caltech 1-71, Pasadena, CA 91125. Third class postage paid at Pasadena, CA. Published by Caltech and the Alumni Association. Telephone: 626-395-3630.

PICTURE CREDITS: Cover – Jane Dietrich; inside front cover, 22 – Bob Paz; 2 – NASA/JPL; 3 – NASA/JSC, Warner Bros.; 4-5 – NASA, JPL, SSI; 7 – Ryan Petterson; 12, 13, 21 – Doug Cummings; 12 – Fiona Cowie; 13, 23, 25-26, 28, 32 – Caltech Archives; 14, 15, inside back cover – Herb Shoebridge; 19 – Smithsonian; 20 – Kate Quirk; 21 – Vienna Veterinary Dept.; 34, 35 – Roscoe Dickinson; 35 – John Otvos; 36, 37 – U.S. Army; 38 – Hal Johnston; 38 – Bob Mills; 41 – Floyd Clark; 42 – Jean Westphal

Visit us on line at **http://pr.caltech.edu/periodicals/EandS/**

GENESIS BOUNCES BACK

U. S. Air Force 388 Range Squad



NASA/Lockheed Martin

Above: The pocket-watch-shaped capsule hit the desert floor edge-on and buried itself to about half its diameter.

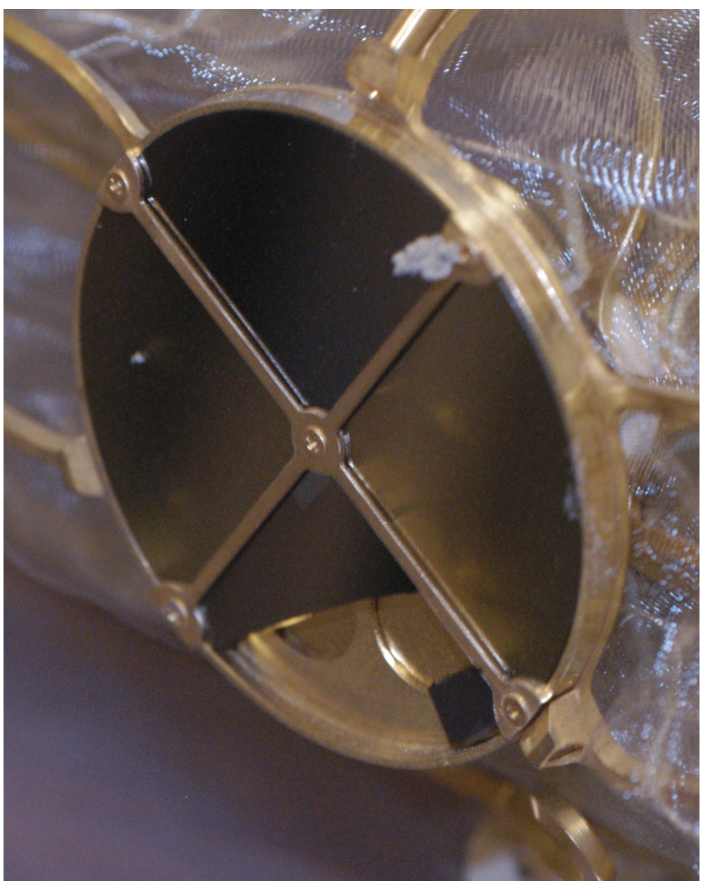
Left: The culprit is believed to be a gravity switch like this one, which was supposed to sense the capsule's deceleration when it hit the top of the atmosphere and initiate a sequence of events that would have deployed a drogue chute followed by a winglike para-foil such as skydivers use. The switch was apparently installed backward.

JPL's Genesis mission to bring back atoms from the solar wind deviated from the script when it delivered its cargo to the empty expanse of the Utah Test and Training Range on September 8. As written, one of a pair of helicopters piloted by Hollywood stuntmen was to snatch the parachuting capsule out of the sky to avoid jarring the delicate contents. But the chute missed its cue and the payload "geobraked" into the desert floor at a speed of 311 kilometers per hour.

Things didn't look good when the recovery team arrived minutes later. The shell had split open, and the inner canister was cracked. Switching to Script B, the crew secured the charge that should have released the parachutes. Only then could the capsule be dug out and trucked to a clean room at the nearby Dugway Proving Grounds. The bulk of the wreckage was sent on to Genesis's builder, Lockheed Martin, for analysis, while the science team tried to salvage something—anything!—from the 26 months' worth of material that had been collected atom by laborious ion and whose chemical composition mirrors that of the material from which the solar system formed.

But this cliffhanger has a happy ending. The top scientific priority, the concentrator that collected oxygen atoms to measure their isotopic ratio, was in pretty good shape. The second priority, a sheet of gold foil to collect nitrogen atoms for the same purpose, was undamaged. And although most of the wafers of sapphire, diamond, and other brittle materials that made up the five rotating collector arrays were in smithereens, the wafers from each array were of a different thickness, so each piece can be traced back to its original array—essential for determining which sample had been collected when. The fragments were meticulously photographed, numbered, and packaged—in more than 3,000 small containers—and shipped to the Johnson Space Center for decontamination, archival preservation, and eventual distribution to various labs. Since the latter would have involved sawing up the wafers anyway, one could say we're actually ahead of the game. □—*DS*

Left: Caltech professor of nuclear geochemistry Donald Burnett, Genesis's principal investigator and lead scientist, picks through desert dirt for bits of the arrays, each of which was designed for a different particle stream. Below: The shards were sorted, and samples from all 14 types of wafer were recovered. A few of the silicon-on-sapphire and gold-on-sapphire wafers actually survived intact. Bottom: Three of the solar wind concentrator's four segments were unscathed. About 85 percent of the fourth segment was salvageable.

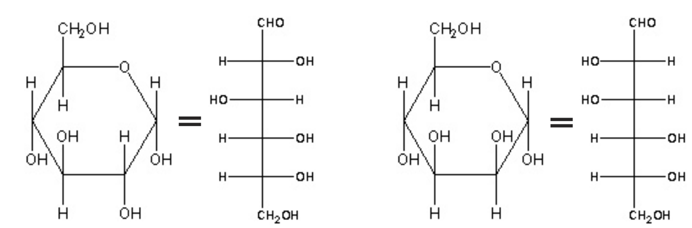


Above: In the *Friends* series finale last May, dimwit actor Joey Tribbiani (Matt LeBlanc, at right) moved to Hollywood. He now shares an apartment with nephew Michael (Paulo Costanzo, center), a grad student at guess where? Clever, sympathetic writing gives this odd couple plenty to do, and Joey's brassy sister Gina (Drea de Matteo, late of *The Sopranos*) and neighbor Alex Garrett (Andrea Anders, at left) earn a lot of laughs as well. *Joey* airs Thursdays at 8:00 p.m. on NBC.

Atkins, Schmatkins

A health-conscious public may be shunning carbs, but Caltech chemists are embracing them. Professor of Chemistry David MacMillan and his graduate student Alan Northrup have discovered a simple way to make their basic unit—the six-carbon sugar—in two steps. This is a major improvement over current methods, which can require a dozen or more reactions. Natural carbohydrates range from these simple sugars up to long-chain molecules containing a thousand or more sugar units. (The latter include chitin, the stuff of insect exoskeletons; glycogen, a source of ready energy for muscles and other tissues; starch; and cellulose.)

"For the last 100 years, scientists have needed many chemical reactions to differentiate five of the six oxygen atoms" in the basic sugar unit, explains MacMillan. "We simplified this to two steps



Above: Six-carbon sugars have five carbon atoms that can be either "right-handed" or "left-handed." This "handedness" determines which groups of atoms end up above and which end up below the ring plane; the rings have also been drawn unclosed. Glucose is at left, mannose at right.

by the invention of two new chemical reactions that are based on an old but powerful chemical transformation known as the aldol reaction." There are eight naturally occurring sugar structures in carbohydrates, and the method works for four of them.

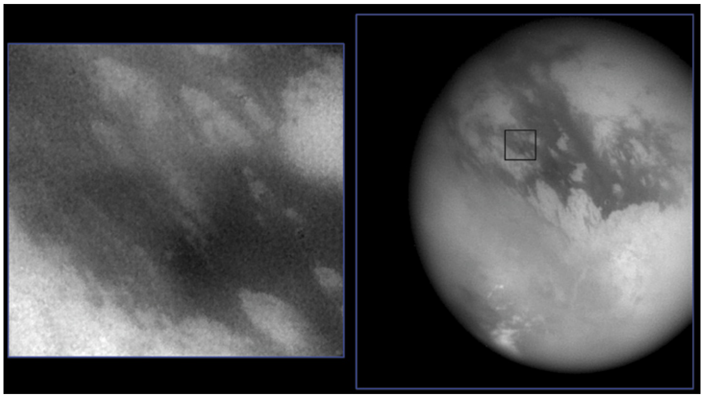
The method also allows easy access to sugars not found in nature, which is invaluable for medicinal chemistry, biology, and a number of diagnostic techniques. For example, a rare form of carbon known as carbon-13 is widely used as a tracer. Starting with ^{13}C-labeled ethylene glycol, which is cheap and readily available, MacMillan and Northrup made a sugar molecule that contained ^{13}C exclusively in only four chemical steps—a feat that previously took 44.

"Carbohydrates are essential to human biology, playing key roles in everything from our growth and development to our immune system and brain functions," says John Schwab, a chemist at the National Institute of General Medical Sciences, which supported the research. "They also play critical roles in plants, bacteria, and viruses. But because they are so difficult to work with, carbohydrates are not nearly as well understood as DNA and proteins. MacMillan's technique will allow scientists to more easily synthesize and study carbohydrates, paving the way for a deeper understanding of these molecules, which in turn may lead to new classes of drugs and diagnostic tools."

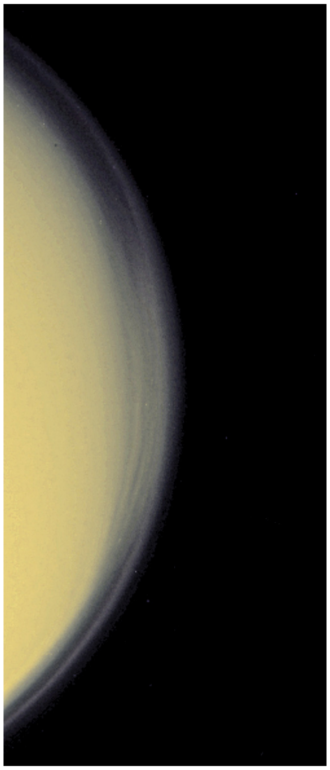
The paper was published online August 12 by the *Science Express* website, and will appear in the journal *Science* at a later date. □—*RT*

Postcards from Titan

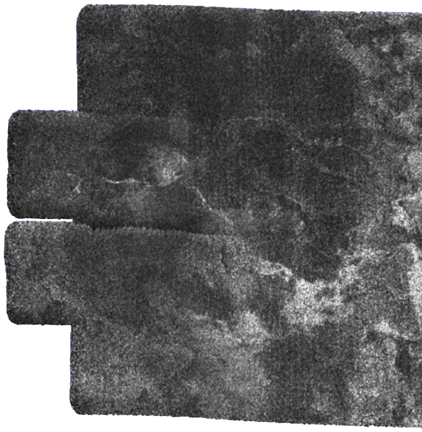
JPL's Cassini spacecraft had its first close encounter with one of Saturn's moons on October 26, when it buzzed by Titan at an altitude of 1,200 kilometers. Larger than Mercury, Titan fascinates scientists because its dense, opaque atmosphere of nitrogen and hydrocarbons—thicker than a Stage 3 smog alert—is believed to resemble the one from which life's precursor compounds formed on Earth. (But with a surface temperature of −180 °C, we don't expect Titan to be inhabited.) Here are a few of the first pictures released from the flyby.



Above: These near-infrared images show the surface at a wavelength sensitive to methane. The large, bright feature in the center right of the right-hand image is a "continent" named Xanadu, although whether the dark stuff adjoining it is really a slushy methane ocean remains to be seen. The dark, narrow streaks in Xanadu may be rivers or glaciers of gasoline. The fact that they and the broad dark features run west to east (upper left to lower right) means that they are probably wind-driven. On its next visit to Titan, Cassini will release the European Space Agency's Huygens probe, which on January 14, 2005, will parachute into the black square, a region shown enlarged in the left-hand image. Huygens will explore Titan's atmosphere all the way down, and if the probe doesn't drown—it has legs but is also designed to float—it may transmit data and pictures from the surface for up to 30 minutes before it freezes to death.



Above: This near-ultraviolet photo of the global haze that hangs hundreds of kilometers above Titan has been processed to approximate natural color.



Right: Cassini's radar scanned a region some 150 by 250 kilometers at a resolution of about 300 meters. Few, if any, craters can be seen, showing that the surface is geologically young. But aside from the fact that it's clearly a very complex place, and that the darker areas are probably smoother than the bright ones, not much else has been deduced yet.

The SWAT Team

If you think it doesn't do much good to swipe at the fly that's going after the potato salad, guess again. You may be discouraging the fly's colleagues from taking up the raid. Caltech's David Anderson, the Sperry Professor of Biology and an Investigator at the Howard Hughes Medical Institute; Seymour Benzer, the Boswell Professor of Neuroscience, emeritus, and a Crafoord Laureate; along with Columbia University's Richard Axel (who shared this year's Nobel Prize in Physiology or Medicine for work on the olfactory system) have found that the act of shaking or shocking flies causes them to emit carbon dioxide as one component of a previously unknown chemical signal that makes other flies avoid the space in which the stressful event occurred. The research involved the fruit fly, *Drosophila melanogaster,* which has been used for decades in genetics experiments. However, the mechanism could be more widespread.

The team, led by Caltech postdoc Greg Suh, found that CO_2 activates a single class of sensory neurons in the flies, and that these neurons seem to be dedicated to the sole task of responding to it. By inhibiting these neurons' synapses, the researchers were able to block the ability of flies to avoid CO_2. "This shows that there is probably a genetically determined, or 'hard-wired' circuit mediating CO_2 avoidance behavior in the fly," Anderson says.

Mosquitoes, on the other hand—or perhaps arm—are attracted to the CO_2 exhaled by their warm-blooded hosts. "Given the evolutionary conservation of olfactory mechanisms in insects, if we learn about the molecular details involved in CO_2 sensing in fruit flies, it could potentially lead to repellents that act by interfering with the reception of CO_2," Anderson says. Such a repellent could benefit third-world countries where mosquitoes carry malaria—or even in the United States, where the mosquito-borne West Nile virus has become a concern.

The paper appeared in the October 14 issue of *Nature.* □—*RT*

It's IST!

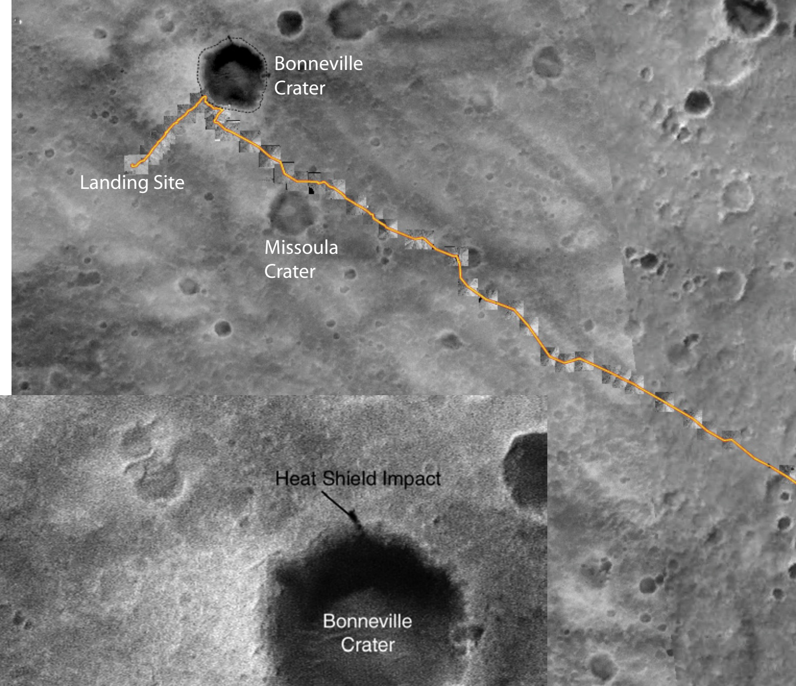
Information is everywhere. It's not just facts and words, but things like the instructions in our genome that tell our cells when to divide and when to die, and the daily flow of data into the stock market that somehow motivates people to buy and sell. In an unprecedented effort, Caltech has launched a university-wide initiative called Information Science and Technology (IST) to draw back the curtain on the nature of information itself and how we use it.

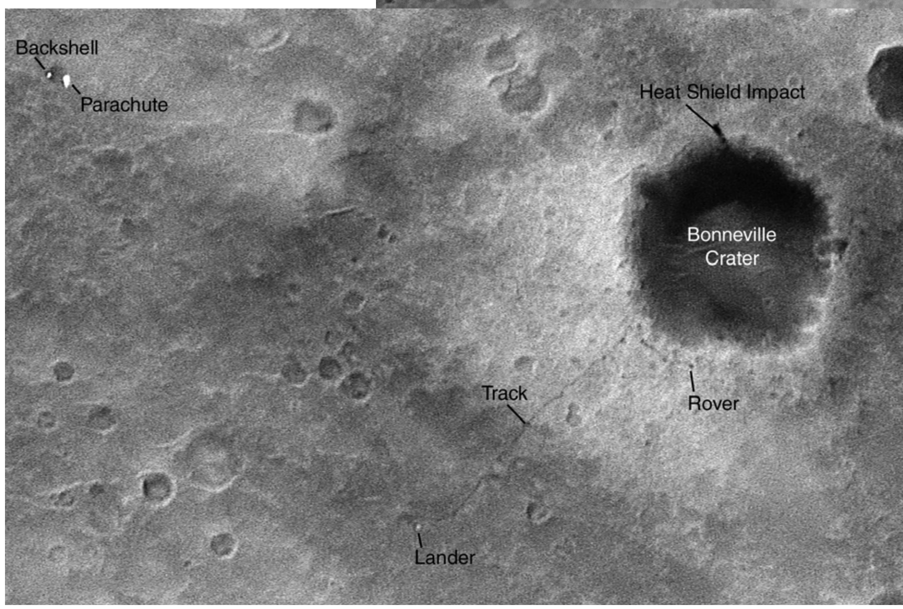
While other universities offer computer science and software development, IST will build information-based research and instructional programs across the academic spectrum. "To maintain preeminence in science, the U.S. needs new and unified ways of looking at, approaching, and exploiting information in and across the physical, biological, and social sciences, and engineering," says Jehoshua (Shuki) Bruck, the Moore Professor of Computation and Neural Systems and Electrical Engineering and founding director of IST.

In the same way that the printing press heralded the start of the Renaissance, and the study of physics helped to foster the Industrial Revolution, technological advances in computation and communication in the 20th century have set the stage for the Age of Information. Scientific and technological changes are outpacing our institutions— schools, media, industry, and government among them—which were originally designed for the Industrial Age. "We need a new intellectual framework to harness these advances," says Bruck.

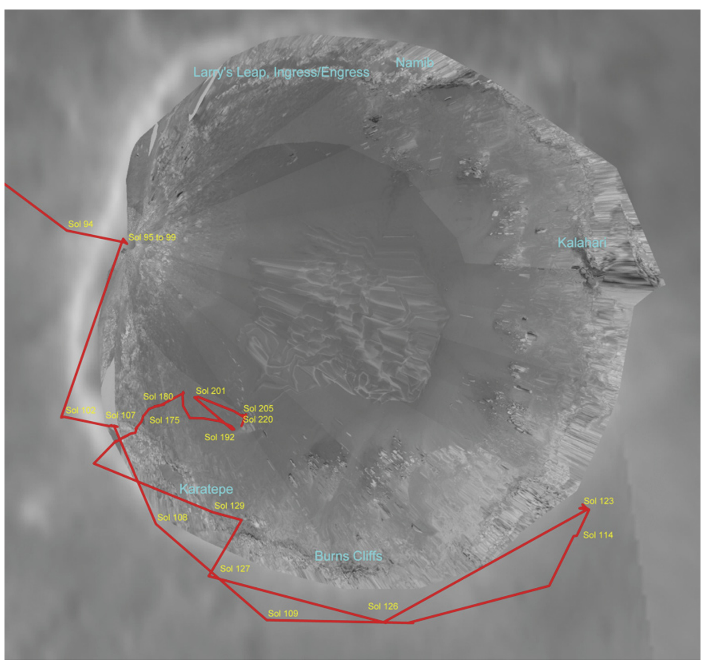
"Some say biology is the science of the 21st century, but information science will unify all the sciences," says Caltech president and Nobel Prize-winning biologist David Baltimore. "It will be like physics in the 20th century, where Einstein went beyond the teachings of Newton—which were enough to put people on the moon—and allowed us to reach into the atom and out into the cosmos. Information science, the understanding of what constitutes information, how it is transmitted, encoded, and retrieved, is in the throes of a revolution whose societal repercussions will be enormous. The new Albert Einstein has yet to emerge, but the time is ripe."

Caltech has committed to raising $100 million for IST as part of the Institute's five-year, $1.4 billion capital campaign. Nearly $50 million has already been raised, in the form of separate grants of $25 million from the Annenberg Foundation and $22.2 million from the Gordon and Betty Moore Foundation. The Walter and Leonore Annenberg Center for Information Science and Technology, expected to be completed in 2007, will join Watson and Moore labs as the physical home of IST. The Moore gift will provide seed money to establish four new interdisciplinary research centers, joining the Lee Center for Advanced Networking and the Center for Neuromorphic Systems Engineering as IST's conceptual anchors. The Center for Biological Circuit Design will address how living things store, process, and share information; the Social and Information Sciences Laboratory will investigate how social systems such as markets efficiently process immense amounts of noisy information, and how a better understanding of them can help improve society; the Center for the Physics of Information will examine the physical qualities of information and design computers and materials for the next generation of information technology; and the Center for the Mathematics of Information will formulate a common language of information that researchers from different fields can use. □—*JP*



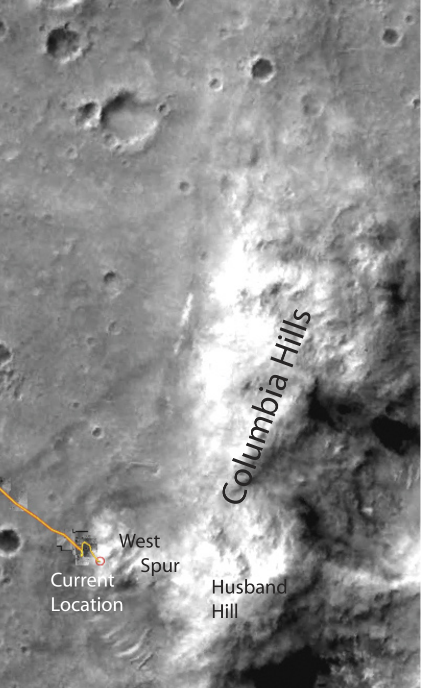


NASA/JPL/MSSS



NASA/JPL/Cornell/OSU/MSSS

JPL's Mars rovers keep on truckin' despite wonky wheels and other signs of middle age. At top is Spirit's trek as of its 238th martian day, or sol (September 3), superimposed on images from JPL's Mars Global Surveyor. Spirit continues to climb into the Columbia Hills, and now has more than two miles on its odometer. Meanwhile, the folks who run the Mars Orbiter Camera have figured out a way to sharpen its gaze so that the rover and its tracks can actually be seen from orbit (above left), as in this shot from March 30. And Opportunity's exploration of Endurance Crater from sol 94 through sol 205 (August 21) is shown at left. In honor of the rovers' unprecedented successes, on September 28 asteroids 37452 and 39382 were officially renamed Spirit and Opportunity, respectively.



NASA/JPL/New Mexico Museum of Natural History

Science By the Seat of the Pants

Sliding down a sand dune on your derrière might at first take seem a bit undignified for a Caltech professor. But for Professor of Mechanical Engineering Melany Hunt, it's all in the name of science. Hunt wants to know why many desert sand dunes give off sound—and a loud, droning sound to boot—whenever the dune avalanches, or a strong wind blows, or a scientist slides down its side. While the phenomenon has been known for centuries, what causes it remains a mystery. Most believe the answer is friction as grains of sand rub together. But the sound continues even after the movement has stopped, and differs from winter to summer.

Hunt, who studies the flow of particulates and granular materials, has spent the last few summers investigating booming dunes as a mentor with Caltech's Summer Undergraduate Research Fellowships (SURF) program. Several times each summer, Hunt, Professor of Mechanical Engineering Chris Brennen, and their students make the long drive to the Eureka or the Dumont Dunes in the Death Valley area, or to the Kelso Dunes in the Mojave Desert. Once there, they slog up to the dune's crest line, carting a ground-penetrating radar unit, geophones (a type of microphone), and lots of drinking water. The radar was used to confirm the subsurface existence of a band of wet, hard sand; the geophone recorded the noise as students and faculty alike slid down the dune.

Hunt believes the sound is a resonance effect, much like a string being plucked on a musical instrument. Over time, whatever rain falls percolates into the dune, eventually forming a band of moisture some two meters down. In time this sand hardens, says Hunt, forming a cement-like crust. When the dune's surface is disturbed, friction between sand grains creates reverberations between the dry sand on the surface and the wet sand below.

"That may be why smaller dunes don't make sound," says Hunt, "because they haven't been around long enough to form that hard layer of sand." The minimum needed is about two meters of thickness, she says. The loudest dunes are the tallest and the steepest, those with a maximum 30-degree angle of repose; that is, the steepest the dune's face can be without collapsing. It's also the reason she believes the sound varies by the season, which affects the moisture content. □—*MW*

Below: Brennen holds a sand sampler as grad student Angel Ruiz Angulo pounds it into the Dumont Dunes and Hunt hunches over her notebook. The operation is being taped for the debut of a new PBS series called *NOVA Science Now*. The show is set to air on January 25, 2005. Check your local listings.

If a hydrogen atom were Earth-sized, the proton that is its nucleus would fit comfortably in the Rose Bowl, and a quark would be smaller than a softball.

Quark Tale

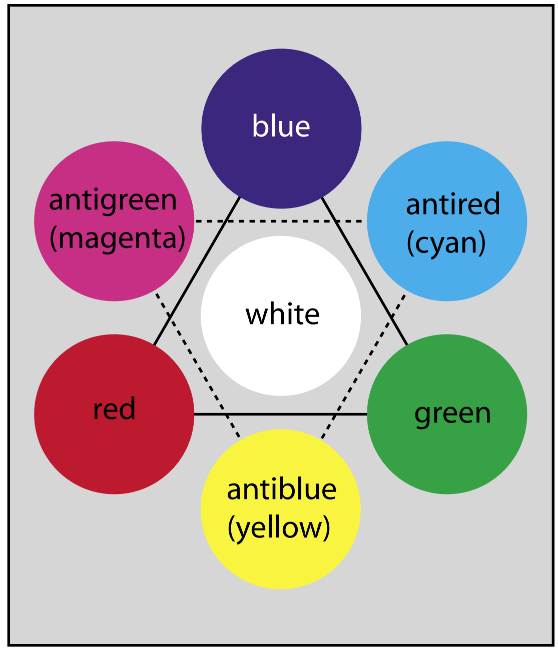
by Douglas L. Smith

Professor of physics David Politzer is Caltech's newest Nobel laureate.

In our world, forces get stronger as the objects that feel them get closer together. Drop a bowling ball off the Empire State Building, and it accelerates as it falls to Earth. Hold two powerful, opposing magnets at arm's length and slowly bring them towards each other, and at some point they'll leap out of your grip and clang together. This is quite reasonable, logical, and natural. But deep within the atom, the strong nuclear force that holds quarks together to make protons and neutrons behaves just the opposite: it increases as the quarks are pulled apart, as if the proton were wrapped in a stout rubber band. The harder you pull, the harder they snap back. But if the quarks are rubbing up against one another, as it were, the band goes slack. On October 5, H. David Politzer, professor of theoretical physics, shared the Nobel Prize in physics with David Gross at UC Santa Barbara, and Frank Wilczek at MIT, for explaining why this is so—a property known as asymptotic freedom.

To follow the trail leading to this discovery, we need to back up a bit. By 1964 the number of "fundamental" particles being discovered had gotten entirely out of hand. So Caltech's Murray Gell-Mann, and George Zweig of CERN, the European Organization for Nuclear Research, independently proposed that protons, neutrons, and an entire bestiary of other particles collectively known as hadrons were themselves made up of smaller, really truly fundamental particles that Gell-Mann dubbed quarks. (Of course, if superstring theory pans out, it will show that quarks aren't fundamental after all, but superstrings are. Honest, they are.) Gell-Mann postulated that a proton is made up of two up quarks (each with an electric charge of $+2/3$) and one down quark carrying a charge of $-1/3$. These bizarre fractional charges were needed to give the proton its +1 charge and the neutron, which consists of two downs and one up, its zero charge. A third "flavor" of quark was needed for the class of "strange" particles that Gell-Mann had described earlier, so this he of course called the strange quark.

The quark color wheel. A color plus its anticolor make a colorless, or white, entity. One blue, one green, and one red quark also add up to white, as do one of each of the three anticolors.



Below: A few members of the hadron zoo. Hadrons are made up of pairs or triplets of quarks and antiquarks; some are quantum superpositions of quark pairs. Here "u" is the up quark, "d" stands for down, and "s" is for strange. Antiquarks are marked by horizontal bars. The "rest mass," given in millions of electron-volts, is the amount of energy needed to create the particle with zero momentum. The particle's mean life is 1.44 times its half-life.

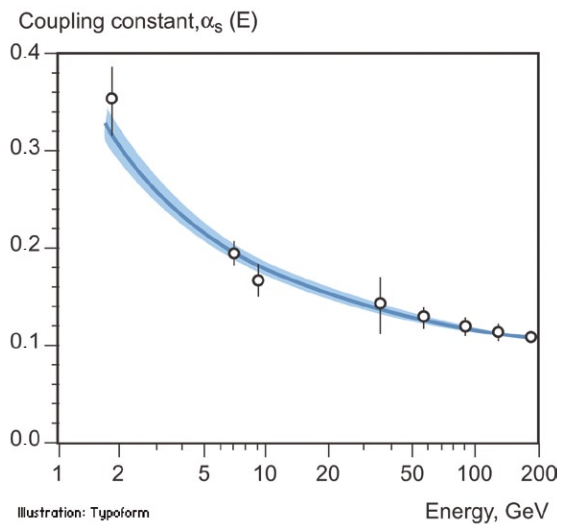
Since then, as accelerators have reached higher and higher energies, more and more particles have been found. The quark inventory is now up to six, with the addition of the charm, bottom, and top quarks. (The latter two are also sometimes known as beauty and truth.) But that's OK. The way the theory is structured, it can accommodate up to 16 flavors—enough for a respectable ice-cream parlor. Gell-Mann won the physics Nobel in 1969, although not for quark theory.

Just as electrons interact by exchanging photons—the carrier of the electromagnetic force—quarks interact by exchanging gluons, the carrier of the strong nuclear force. (Or strong interaction, as they like to call it nowadays.) Constant gluon swapping makes quarks stick together to form protons, neutrons, and whatnot, and even overcomes the mutual repulsion of positively charged protons to bind them with neutrons into atoms. Which gets us to why it's called the strong force. The electromagnetic force that keeps the proton and the electron together in a hydrogen atom is 10^{41} times stronger than gravity at that range. At the boundary of the proton, the strong nuclear force is stronger still—roughly 100 times stronger.

PARTICLE	QUARKS	CHARGE	REST MASS (MeV)	MEAN LIFETIME (seconds)
proton (p)	uud	+1	938.280	stable
neutron (n)	udd	0	939.573	898
Λ	uds	0	1116	3.8×10^{-9}
π^+	$u\bar{d}$	+1	140	2.6×10^{-8}
π^-	$\bar{u}d$	−1	140	2.6×10^{-8}
π^0	$u\bar{u} - d\bar{d}$	0	135	8.7×10^{-17}
K^+	$u\bar{s}$	+1	494	1.24×10^{-8}
K^0 long	$d\bar{s} - \bar{d}s$	0	498	5×10^{-8}
K^0 short	$d\bar{s} + \bar{d}s$	0	498	8.6×10^{-11}

And now things start to get messy. There are only two magnetic poles, north and south, and two forms of electric charge, positive and negative. But the strong force comes in three forms of charge, called colors: blue, green, and red. These aren't real colors visible to the eye, of course, but they do exhibit a similar bit of behavior—one blue, one green, and one red quark add up to be colorless, just as equal parts of blue, green, and red light add up to white light. All observable particles—your protons, neutrons, pions, kaons, and what have you—are color-neutral. And just as all particles, including quarks, have antiparticles, colors have anticolors: antiblue (yellow), antigreen (magenta), and antired (cyan). A bound pair of a color and its anticolor is also color-neutral. To make things really interesting, every gluon carries two units of color charge—a color and a (generally different) anticolor—and when quarks trade gluons, they usually change color as well. By analogy with quantum electrodynamics, which explains electromagnetism on a quantum level, Gell-Mann christened this *Trading Spaces* nightmare quantum chromodynamics, or QCD.

Quantum electrodynamics, or QED, had been independently proposed in the late 1940s by Sin-Itiro Tomonaga of the University of Tokyo, Julian Schwinger of Harvard, and Richard Feynman, who was then at Cornell but left for Caltech almost immediately afterward. The three would share the Nobel for QED in 1965. QED solved a stubborn problem in quantum field theory, which had been invented at the dawn of the quantum age to deal with electromagnetism. Unfortunately, the mathematics that was so spectacularly successful in calculating electromagnetic interactions proved completely useless for the strong force. In a



The coupling constant, or strength with which force-carrying particles interact, depends on the amount of energy they have. The values predicted by QCD are shown in blue, and the actual measured values are shown as open circles, plus or minus the vertical error bars. GeV stands for billion electron-volts. The line slopes downward, which is the hallmark of a negative beta function.

nutshell, QED calculations assumed that the larger the number of particles involved in some event, the less likely it was to happen. The law of diminishing returns sets in pretty rapidly, so you could make the calculation as accurate as you pleased by deciding where to draw the line. This worked for QED because photons have no electric charge, so they don't "feel" other photons. But gluons do have a color charge, so they create new gluons to trade among themselves. As quarks get pulled apart, they barrage one another with gluons in a frantic effort to stick together. Each gluon begets more gluons, and quantum field theory goes straight down the drain.

Efforts to apply a QED-type field theory to the strong force was quickly abandoned, but not everyone gave up on it completely. In May 1964 Gell-Mann wrote, "We [may] construct a mathematical theory of the strongly interacting particles, which may or may not have anything to do with reality, find suitable algebraic relations. . . and then throw away the model. We may compare this process to a method sometimes employed in French cuisine: a piece of pheasant meat is cooked between two slices of veal, which are then discarded." However, since experimentalists were unable to observe free quarks directly, there was very little evidence that they were anything more than a handy bookkeeping device.

That is, until 1969, when researchers at the Stanford Linear Accelerator who were hurling electrons into protons at a significant fraction of the speed of light got some very odd results. This behavior could best be explained by Feynman's suggestion that, at the very high energies equivalent to very short distances, the proton acted as if it were made up of freely moving, point-like particles—although Feynman, being Feynman, called them "partons" instead of quarks. (If a hydrogen atom were Earth-sized, the proton that is its nucleus would fit comfortably in the Rose Bowl, and a quark would be smaller than a softball.) In other words, you could liberate quarks by squeezing them together. Freedom in confinement is a very Zen notion, whose mathematical equivalent is something called a negative beta function. In a negative beta function the coupling constant, or the strength with which objects interact, increases with distance—or decreases with energy, as it takes more and more energy to force particles closer and closer. When you get close enough, the coupling constant essentially vanishes. Alas, several people had already "proved" that a negative beta function was physically impossible.

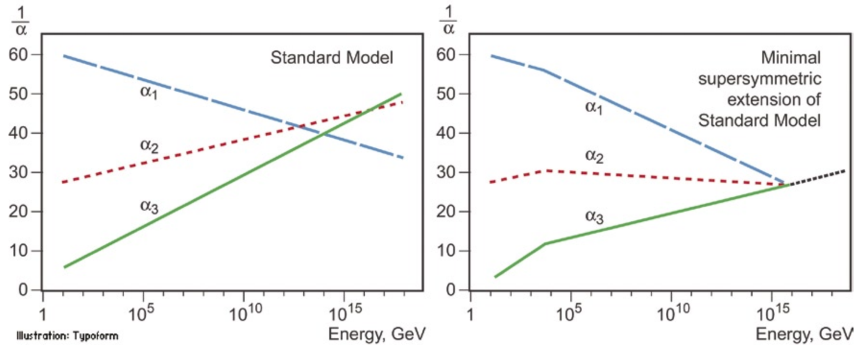
In the spring of 1973, David Politzer, then a graduate student of Sidney Coleman at Harvard, was attempting to caculate the beta function for something called Yang-Mills theory, a forbidding and little-explored realm at the time. Coleman, meanwhile, was visiting Princeton for the semester, where Politzer's co-winners, David Gross and his graduate student, Frank Wilczek, were working on the same calculation. The approaches were different, but the results, compared via Coleman, were the same, and back-to-back papers describing what is now called asymptotic freedom appeared in the June 25 issue of *Physical Review Letters*—in Politzer's case, his first-ever publication. An asymptote is a mathematical term for a line bounding a curve that the curve approaches but never quite meets.

The mathematical picture led to a physical one. Explains John Preskill, the MacArthur Professor of Theoretical Physics, "The crucial difference between the two theories is that while the photons of QED carry no charge of their own, the gluons of QCD are themselves colored particles. A quark is surrounded by a sea of 'virtual' gluons that arise due to quantum fluctuations, and the color of the virtual gluons enhances the quark's own color. A probe coming closer and closer to the quark is influenced less and less by the virtual gluons, so that the effective color charge of the quark seems to weaken; this is asymptotic freedom."

And because the coupling constant increases as you separate the quarks, it soon becomes insurmountable. The rubber band snaps, but instead of spilling forth the quarks it restrained, two new rubber bands form, each binding up a new particle. The fresh quarks needed to round out the new doublets or triplets are conjured out of the energy imparted to them—$E=mc^2$ and all that.

Armed with the proper beta function, the interactions resulting from the strong nuclear force suddenly became calculable in full detail. As Gross wrote in *Twenty-Five Years of Asymptotic Freedom* in 1998, "During a very short period, a transition occurred from experimental discovery and theoretical confusion to theoretical triumph and experimental confirmation." Perhaps the most spectacular confirmation came from the DESY (Deutsche Elektronen-SYnchrotron) particle accelerator in Hamburg, Germany, in the late 1970s. Experimenters studying the annihilation of the electron by its antiparticle, the positron, found that this

If you use the Standard Model (right) to plot the energy dependence of the coupling constants of the strong nuclear force (green), the weak nuclear force (red) and the electromagnetic force (blue), the three lines cross but do not meet at a common point. But supersymmetry (far right) bends the lines, causing them to meet at a point where all three forces have equal strength. At that point, the forces become unified, and one equation will describe all three.



sometimes created a quark, an antiquark, and a gluon, each of which became the source of a shower of stuff that could be traced backward to identify the three original particles. QCD now stands with QED as two of the three pillars of the so-called Standard Model of physics that also describes the weak nuclear force, which is responsible for some forms of radioactivity and is carried by the W and Z particles.

What are the implications? Well, as Mark Wise, the McCone Professor of High Energy Physics and a collaborator of Politzer's, said at Caltech's press conference about the Nobel—which Politzer did not attend—"I don't think that we're going to have a QCD car in the near future, despite the high price of oil." However, the mathematics of QCD and QED are quite similar, and QED and the weak nuclear force have been unified into an "electroweak" theory that has itself produced a clutch of Nobels. So theorists have been floating schemes to unify the strong and electroweak forces—into the "streak" force, presumably—as the last stop en route to the hypothesized Grand Unified Theory, which would incorporate a quantum treatment of gravity as well. These petit unified theories note that if you plot the dependence of each component theory's coupling constant versus energy, all three lines almost cross at one point—in an energy range just out of reach of today's accelerators. And if you invoke something called "supersymmetry," which we won't go into, you can bend the lines until they meet. At the point where the lines cross, the three forces have equal strength, and this equivalence means very interesting things should occur. A whole spate of brand-new particles should appear, and their characteristics will tell the theorists which, if any, of their unified models is correct. The Large Hadron Collider, currently under construction at CERN, is designed to reach these energies. It is slated to begin operating in 2007.

And, as Gross said at *his* press conference at UC Santa Barbara, "Another application of these ideas is tracing the history of the universe back to the Big Bang. . . . We know from observation that the universe is expanding, so early on it was very dense and hot. And without a theory like QCD, we wouldn't be able to say anything about how matter behaves under those circumstances. . . . With asymptotic freedom, with QCD, we can say what happened. In fact, what happens is remarkable. Protons—these bags of quarks which are held together by this strong force—dissolve, and the quarks and gluons get liberated and form a kind of plasma in the earliest moments of the universe."

As Caltech president and fellow Nobel laureate David Baltimore said at the press conference, Politzer "has now been recognized as one of the seminal figures in the history of physics by this prize." Added Wise, "He did a very difficult calculation—at the time it was very difficult; now graduate students can do it. In fact, I'll assign it to my students this year. . . . The smart money was on this [prize]. You could have looked at the Nobel futures market."

Politzer himself, meanwhile, continues to enjoy his prerogative to decline interview requests. □

What Do Babies Know About Language?

by Fiona Cowie

Nativism is the view that there are ideas, beliefs, knowledge, or concepts that are inborn or innate. It's not just the notion that we have innate capacities to acquire knowledge from our experience; instead, it's the idea that some of what we know is already in us to start with. Some very eminent thinkers have held this view. Plato thought that ideas of the Good, the Beautiful, Virtue, and Justice were all innate; René Descartes thought our ideas of God, mathematics, and logic were innate; and Gottfried Leibniz thought that our ideas of necessity and possibility were innate.

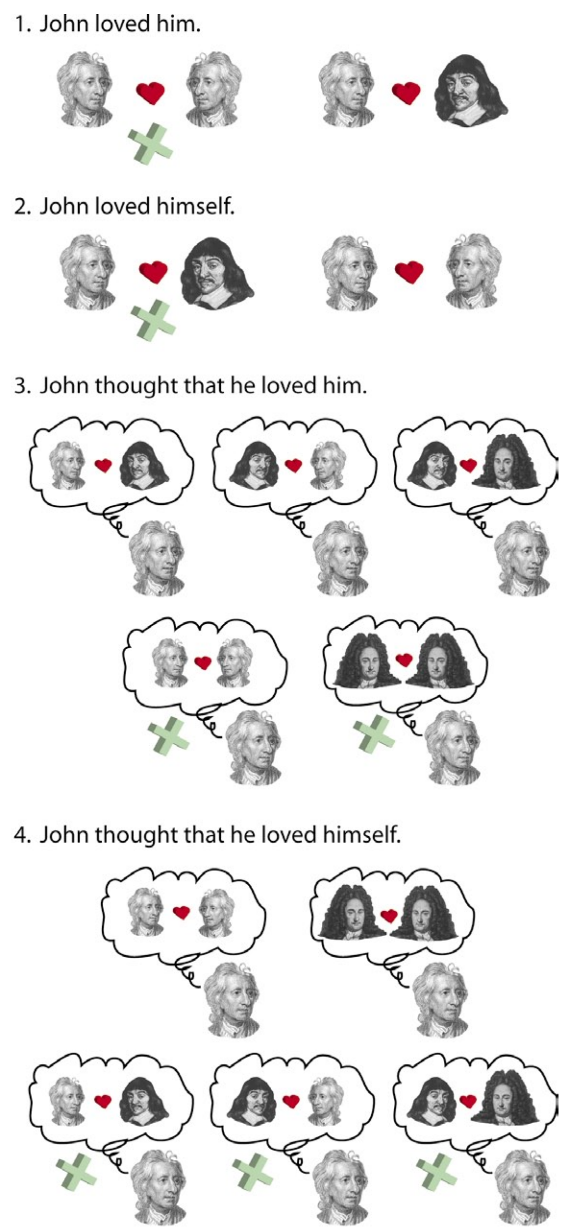
Of course, not everyone shares this view. Prominent nonnativists include Aristotle, the Enlightenment philosophers David Hume and John Locke, and the 20th-century psychologist B. F. Skinner. I don't claim to be in the same league as these people, but I, too, am a nonnativist, or empiricist. We have in common the idea that most of what we know is empirical—that is, comes through learning. This could strike many of you as somewhat uncontroversial because, after all, learning is such a ubiquitous feature of our lives. So you might wonder why anyone, especially the eminent nativists I've named above, would deny that learning, at least in some areas, is possible.

Nativists often support their case by an argument that, in general terms, goes as follows. We know about something, X, where X could be God, the truths of mathematics, what virtue is, what goodness is, or many other things. But, it's claimed, there's too little information about X in the environment to enable us to have learned what we know about it. So if our knowledge of X couldn't have been learned, it must have been inborn. After all, there's nowhere else it could have come from! This is called the "poverty of the stimulus argument."

Which brings us to my topic, for the MIT linguist Noam Chomsky uses this argument to reason that linguistic knowledge is innate. We know facts about language, he argues, that we couldn't possibly have heard people say to us, or overheard people saying around us, at the time we were learning to speak. Nor could we have inferred these facts from what we heard around us. These facts could not have been learned, so they must be known innately—we were born knowing them. To give you an idea of how this argument goes, let's look at a particular case, the four sentences shown in the illustrations. You'll be surprised at what you know about them.

Take the simple sentence "John loved him." You know it can't mean that John loved John. It has to mean that John loves somebody else, perhaps René. What about the next sentence, "John loved himself"? You know without even thinking about it that it can't mean that John loved René; it has to mean that John loved John. What about "John thought that he loved him"? You know that it could mean a bunch of things. It could mean that John thought that he, John, loved René. Or it could mean that John thought René loved John. It could also mean that John thought René loved some third person, Gottfried. But you also know that it can't mean certain things too: it can't mean that John thought that he, John, loved John, and it can't mean that he was thinking about Gottfried's self-obsession. What about "John thought that he loved himself"? It could mean that John thought that John loved John, or it could mean that John was thinking about who Gottfried's object of affection was, namely Gottfried, but it can't mean a lot of other things. You know this without even thinking about it—you just automatically understand these sentences and can tell what are the possible meanings and what aren't.

The rules of grammar that govern when two terms, like "John" and "he" can refer to the same



We know what the sentences on the right can and can't mean without having to open a grammar book. Are humans born with this knowledge, or is it learned? Author's son Jacob, left, can't wait to find out.

object and when they can't are known as binding theory. Here are the principles of binding theory:

A. Anaphors (like "himself") are bound in their binding domain.
B. Pronominals (like "he") are free in their binding domain.
C. R-expressions (expressions, like noun-phrases,that are used to refer to things and events in the world) are free.

(The binding domain of a noun-phrase is the smallest clause that contains the noun-phrase, its case-marker, and a subject. An expression is bound if its reference is the same as the reference of some other expression within the binding domain.)

Got that? I don't really need to explain what it means, do I? Because, according to Chomsky, you already know these principles! You're not conscious that you have this knowledge, of course, and you may not even be able to understand what binding theory, as formulated here, is telling you. But this is the knowledge that apparently underlies your ability to understand the meaning of those sentences about John, René, and Gottfried.

Now it's almost certain that, unless you've studied linguistics, no one ever told you these principles till now. Yet you've been using them since you were a child. How did you acquire this subtle linguistic knowledge? Surely children can't figure out such complicated principles just from listening to what people say around them? In order to do that, they'd surely need to know what sentences containing anaphors and pronominals *can't* mean—for instance, that "he loves himself" cannot mean that he loves some other person. But no one ever tells you what sentences can't mean. So it's mysterious how you could possibly have inferred these difficult principles from the information you had access to as a child. Chomsky argues that you couldn't have and that you didn't. He thus concludes that binding theory must be known without learning—it must be innate knowledge.

Chomsky and his colleagues (like Steven Pinker, a psychologist at Harvard and author of an excellent popular treatment of these issues, *The Language Instinct)* run similar kinds of poverty-of-the-stimulus arguments for the innateness of various other principles of what is called "universal grammar." These are principles of structure and organization held to apply to all natural languages, no matter what their superficial differences of vocabulary and syntax. Chomsky holds that our innate knowledge of universal grammar is embodied in a special language-specific learning device, or module, that evolved only in humans, presumably by natural selection—although he refuses to comment on how exactly this language module developed in our brains. If humans have a specialized language module that embodies their knowledge of universal grammar, then there's no need for them to learn all the deepest and darkest properties of natural language, like binding theory, for they know it already. All that children have to learn as they listen to people and try to talk to them are the superficial features of their language, such as the vocabulary and the rules governing such things as word order or past-tense formation. As a result, language learning is quick, efficient, and easy.

Having given you a review of the reasons why a nativist like Chomsky holds that a large amount of linguistic knowledge is innately known, I'd like to give you some reasons why I don't believe it is. First of all, the poverty-of-the-stimulus argument might seem convincing at first glance, but it doesn't provide any real data showing that children *don't* get adequate linguistic information. Literally none of the Chomskyans' specific claims about what children do not hear are supported by developmental studies, and many of the claims have not withstood careful scrutiny by developmental linguists. And the more general claim that children have very little access to information about language is one of those "facts" that looks plausible or not from different points of view. A child learning language hears

With five-year-old sister Katie around, Jacob, aged three, sure doesn't suffer much from an impoverished linguistic environment. Katie and Jacob are the author's children.

about 7,000 utterances a day over the six or seven years that language learning typically takes. Is that a *little* information, as Chomsky maintains, no doubt thinking of the infinitude of other sentences that a natural language contains? Or is it rather a lot, as it seems to me, thinking that people routinely master other infinite areas such as arithmetic, logic, or even cooking, in much less time and with much less input and practice than a child spends on learning a language? But it's not the sheer *bulk* of information coming in that's important, as anyone knows who's had a bad teacher or read a bad textbook, it's whether the information is of a kind that a person *can make use of*: a lot can be learned from very little, given the right preparation.

This brings me to my second quarrel with the argument from the poverty of the stimulus, which is that it is based on a very simplistic concept of learning. It concludes that Mother Nature has prepared us for language learning by building in most of what we end up knowing. But in stating its case for this conclusion, the argument overlooks the other kind of "preparation" with which Nature might have furnished young minds, namely, a more general, non-language-specific suite of learning capacities, abilities that allow children to take information from the environment, organize it, analyze it, and render it in forms that are more useful to them.

Proponents of the argument talk of how little children can "learn" from what they hear, but they don't take account of the fact that learning is not just a matter of what the philosopher Karl Popper referred to as "bold conjecture" and refutation. For instance, their idea that childrens' hypotheses about language may be constrained by their ability to perform sophisticated inductive and statistical inferences is not followed up. The argument simply *assumes* that children are not good at analyzing large amounts of data, nor at making accurate generalizations going beyond the data they have access to. Yet this is now known to be false: an impressive body of experimental work by psychologist Jenny Saffran of the University of Wisconsin, Madison, and colleagues has shown that even very young babies take extraordinarily little time to extract high-level regularities from their analyses of the statistical properties of rule-generated inputs, linguistic and otherwise. For example, Saffran showed that after a mere two minutes' exposure to a stream of artificial speech, eight-month-old infants are able to recognize what is and is not a "word" of the artificial language, based solely on the probabilities of certain sounds going together.

The argument also ignores the fact that children are able to use myriad kinds of information to evaluate their hypotheses about how language works: the "linguistic data" that kids have access to is not just a set of sentences, a list of what other people say. Instead, it includes information about meaning and context, information about which things are sometimes said differently (and what these differences imply), and information about what is not said and when and why. It includes information about how children's own linguistic sorties and those of others are received, and whether their or others' demands, requests, and questions are understood and effective. It includes, in other words, information about what chunks of language are *for*, and about what they can *do*—language being *for* communication and able to *do,* well, just about anything, from getting someone to buy you a toy to starting a war.

Here's the point: the poverty of the stimulus argument in effect contends that if you took a child who lacked innate knowledge of universal grammar, locked her in a room for seven years, and made her listen to recordings of around 18 million sentences of, say, English, then she would come out of the room unable to speak or understand that language. Well, maybe so. But what the argument does *not* show is that if you took a child who lacked innate knowledge of grammar, put her in the world, and gave her the vast amounts of information about language and its works and workings that actual children have access to, she would fail after seven years to have learned her language. By putting an impoverished conception of the data together with an impoverished conception of children's remarkable abilities to learn about their world, Chomsky's argument looks overwhelming. But once you enrich your conception of the child, and of the linguistic data, the argument seems a lot less compelling.

Although nativists about language may have given poor arguments for their view, we can nonetheless test the hypothesis of innate linguistic knowledge in another way—on its merits. How well does linguistic nativism fare? Not well at all, or so I will try to convince you. In order for a scientific theory to be fully validated, it needs to do two things: it needs to explain or account for the data within its area, and it needs to be consistent with other things we know. Linguistic nativism fails on both fronts.

What we want from a theory of how language is learned is—a theory about how language is learned! That is, we want a theory about the psychological mechanisms used in language acquisition, and about the data used by children, that accords with what we know about children's psychology and the data they have access to, and that predicts the actual course of language acquisition.

Nativists say that our innate knowledge of universal grammar, together with a theory of parameter setting (a process in which the—very few—variables in universal grammar are nailed down to a particular value, as when the basic word order within phrases is determined), explains how language is learned, given the paucity of linguistic information and the stupidity of young children. However, nativists have failed almost completely to provide any detailed, testable theories about how actual children go about the task of learning their language. (A notable exception here is Steven Pinker, who developed a theory about verb-learning in the 1980s that initially looked promising but is now widely held to be inadequate.) So nativists' theory of the innateness of a language organ embodying universal grammar has not delivered on its promise of productivity: while it explains how language acquisition might in principle work, it has not even attempted to tell us in any kind of detail how this story is supposed to explain the actual course of language learning. It's as if Newton had rested content with "There's this weird force out there that explains how planets and other things move. Let's call it gravity." But scientific validity, not to mention God, is in the details. So nativism fails the first test: as it stands (indeed, as it's stood for almost 50 years), nativism is not clearly enough articulated to provide an adequate scientific explanation of language acquisition.

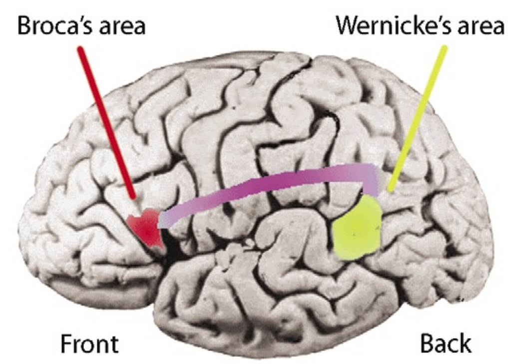
Nor is nativism consistent with other things we know. A group of psychologists, including Jeff Elman and the late Elizabeth Bates, both of UC San Diego, Michael Tomasello of the Max Planck Institute for Evolutionary Anthropology in Leipzig, and Annette Karmiloff-Smith of University College London, have been developing an alternative theory of how language acquisition works, and my current research is aimed at bolstering their case. My aim is to show how this alternate theory coheres better with other areas of the mind sciences, especially developmental psychology and neuroscience, and to bring home the implications of this diverse body of research for the orthodox nativist position.

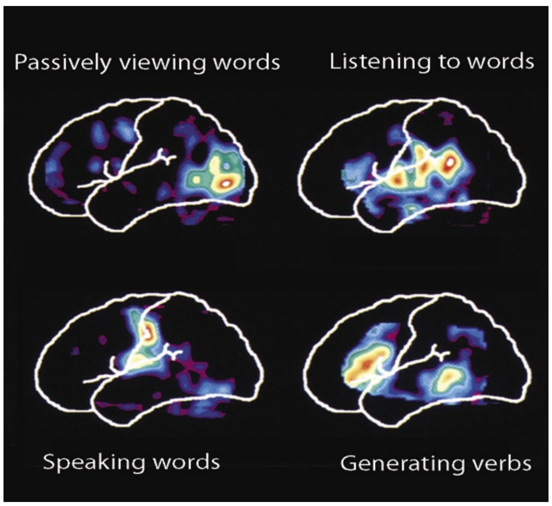
In this alternative, "constructivist" view of language learning, there is no evolved, specialized language module. Instead, numerous faculties with different evolved functions cooperate to make language learning possible. For instance, children have the capacity to focus their attention on the same thing that somebody else is attending to, and this underlies their earliest attempts at word learning. They can perform extremely sophisticated statistical inferences from data, as we have already seen in the Wisconsin studies, and this accounts for their initial ability to extract words from the incoming stream of "noise" and their progressive understanding of ever-more-general rules about how language works. They also have the capacity to understand other peoples' intentions, particularly their communicative intentions. This again is a critical skill for a language learner: language is, after all, primarily a vehicle for communication. And they have the ability to learn by imitation, an ability that is exhibited in the virtually ceaseless stream of "practice language" that is both the pleasure and despair of the parents of young children. This last may be a peculiarly human ability—it's not clear whether any other animals can learn by imitation, though many researchers believe that if they can, they find it very, very difficult—but it's not an ability that's specific to the task of learning language. On the contrary, it plays a role in many other kinds of learning as well. Finally, children also have the ability to perform what's called "categorical perception," which I'll elaborate on later. The key feature of this alternative view is that all the capacities that are used in language acquisition are also useful for other tasks. There may be innate knowledge and innate capacities that enable us to learn a language, but none of them are *specific* to the task of learning language.

One of the nice things about being at Caltech is you don't have to stay in your own disciplinary pigeonhole, which is just as well, because in order to defend this view of language acquisition I've had to become familiar with, or at least know people who are familiar with, a lot of different things outside philosophy: neuroscience, genetics, psychiatry, developmental psychology and psycholinguistics, historical and comparative linguistics, anthropology, and evolutionary biology.

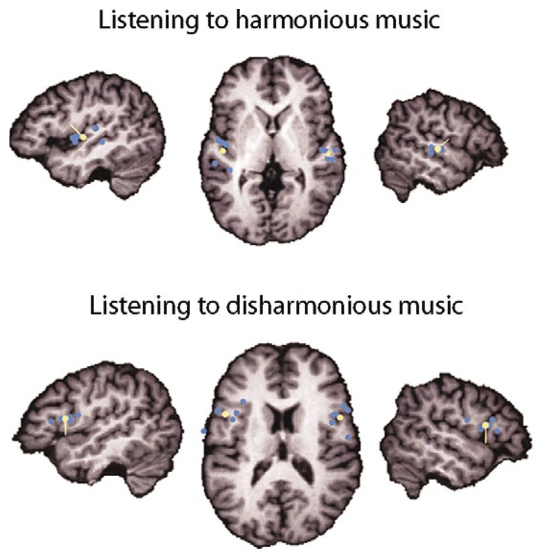
Let's look at the evidence from neuroscience. If linguistic knowledge were innate, you'd expect it to be expressed somewhere in the brain. Indeed, until

Right: Until recently, it was thought that two areas of the brain's left hemisphere shared language duties, with Broca's area responsible for syntax, the production of sentences, and Wernicke's area handling semantics, the meaning of what's being said. It's true that the two areas, connected by a thick bundle of nerve fibers (pink), work closely together, but they're not the only areas involved. In the PET scans below left, the brain of someone looking at words, listening to words being spoken, speaking words, and turning nouns into verbs, lit up all over the place. And Broca's area also analyzes things other than language syntax. In the experiment below right, Broca's area and its right-hemisphere equivalent lit up (as measured by magnetic field strength) when listeners heard music that unexpectedly hit a dud chord. The brain was probably trying to work out what had gone wrong with the expected harmonic syntax.





From *Images of Mind* by M. J. Posner & M. E. Raichle. ©1994, 1997, Scientific American Library, reprinted by permission of Henry Holt & Co., LLC.



From B. Maess *et al.*, *Nature Neuroscience*, 2001, Vol. 4, 540-545, with permission.

late last century, studies of brain lesions and aphasias (a condition where people lose the power to use or understand language) were thought to show that language was relatively localized to Broca's and Wernicke's areas of the left hemisphere. Broca's area was thought to be responsible for syntax (the form of the utterance) and Wernicke's area for semantics (what the utterance means). This apparent localization of language function in the brain appeared to support linguistic nativism: Broca's and Wernicke's areas were plausible candidates for the repositories of our innate linguistic knowledge.

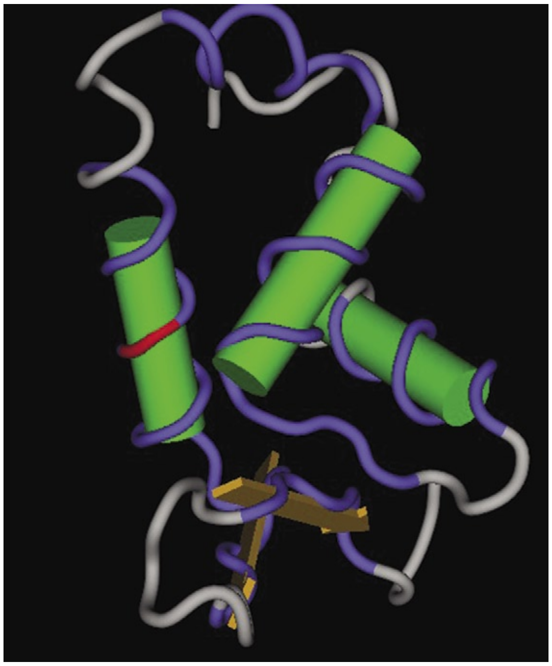
However, it's not actually clear that functional localization tells us very much about whether or not the function is *innate*. As Elman and Bates, among others, have argued, functional localization can occur from virtually any developmental trajectory—learning, genetic determination, and everything in between. We know, for example, that the brain has a lot of plasticity, and can adapt to changed circumstances. The congenitally deaf use their auditory areas for the processing of sign language, which is a visual task, and the congenitally blind use their visual cortex for Braille reading, which is a tactile task. This suggests that functional specialization in the cortex is determined less by genetic than by experiential factors. So if there's localization for language in the cortex, it's an open question where that specialization came from.

In any case, it's beginning to appear that there really isn't much localization of language in the brain. New imaging techniques developed in the last few years have revealed that language processing is much more widely distributed than the earlier picture supposed. You can see this in PET scans of someone passively viewing words, listening to words, speaking words, and generating verbs from nouns. When I look at this kind of scan, I think to myself, where *is* the language module? It seems to be everywhere! Some nativists have responded to these kinds of brain imaging data by saying it doesn't matter if there are lots of language areas in the brain; the important thing is that *some* areas of the brain are destined to encode the specialized linguistic knowledge that our genes represent. The genes can put linguistic knowledge in the brain wherever they like, so long as they *do.*

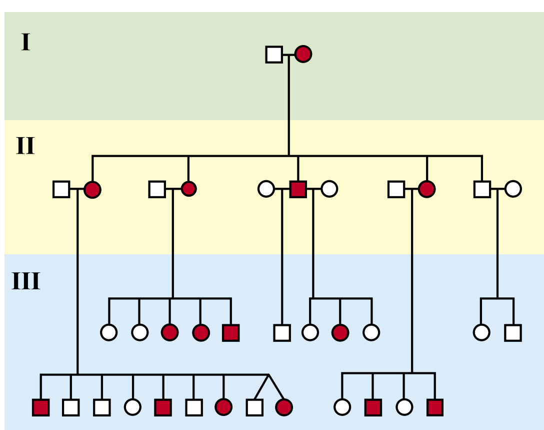
The idea that it is *language-specific* information that the genes encode in the brain is brought into question by the fact that areas once thought to be specialized for linguistic tasks, such as Broca's area, can also perform tasks other than the processing of linguistic syntax, as shown by a recent study in which this area lit up on an MEG (magnetoencephalography) scan while people were listening to harmonious and disharmonious music. It even lit up in one place when harmonious music was played, and in another place when disharmonious music was played. What Broca's area seems to be doing is processing not just linguistic form, but also musical form. This kind of functional overlap, like the fact that language processing seems to be "smeared out" over much of the brain, suggests that the processes responsible for language are not *specific* to language. The evidence from neuroscience seems to support an empiricist rather than a nativist view.

What about the nativist counterargument that it doesn't matter *how* language is implemented in the brain; what matters is that linguistically specific knowledge encoded in the genes is expressed during language acquisition? First, it's not clear how knowledge of universal grammar could actually be "encoded" in the genes. For one thing, as Bates pointed out, half facetiously, there may not be enough of them! Recent estimates give us around 20–25,000 genes, which have a lot more to do beside encode for universal grammar. In addition, many noted biologists and philosophers, including Richard Dawkins of Oxford University and Peter Godfrey-Smith, of both the Australian National University and Harvard, argue that although genes can be said to code for proteins and transcription factors, they do not in any real sense "encode" higher-level traits like knowledge of universal grammar at all (though they are certainly involved in producing them). More damagingly, nativists have never given even the barest hint as to *how* linguistic knowledge (or any other knowledge, for that matter) might be genetically encoded. What, exactly, are the processes, genetic and otherwise, by which this genetically coded information gets expressed?

Worse still, recent attempts to locate genes specialized for language have resulted in the discovery of genes whose functions are *non*-linguistic. Let me give you an example of this. In England there's a family called the KE family who have an inherited language disorder. As you can see in the pedigree, about half the people in the family have what's called Specific Language Impairment in quite a severe form. They have deficits in the production of various grammatical morphemes like the "s" at the end of a plural, the "ed" at the end of a past tense, the "ing"—all those niggly little bits of language that carry certain kinds of grammatical and semantic information. In 1991, to great fanfare, the Canadian linguist Myrna Gopnik suggested that the gene responsible for the family's language problems was a "grammar gene" encoding grammatical morphology, based on the argument that the disorder showed a Mendelian inheritance pattern corresponding to a single dominant gene, that a fault in this gene resulted in grammatical deficits, and that it must therefore be a gene for grammar. The faulty gene was recently identified as *FOXP2* on chromosome 7. But it's not obvious that *FOXP2* can be called a gene for grammar. For one thing, other animals also have this gene, yet we're the only species (as far as we know) that uses language. Other species communicate symbolically, to be sure, but it's generally thought that



Top: Part of the first "language" gene to be identified, transcription factor *FOXP2*, with the mutation that causes a severe speech and language disorder colored red. *FOXP2* is a gene orchestrating the development of brain circuitry for the precise coordination of movement in mammals. When it's faulty, humans lose the ability to accurately control the muscles used in speech. The image is from Dr. Simon Fisher of the University of Oxford, who was part of a team that identified this gene with the help of three generations of the KE family, whose pedigree diagram is shown below. Family members with the inherited disorder are shown by the red squares (males) and circles (females).



because their symbols cannot be recombined to express different thoughts—compare "The dog bit the man" with "The man bit the dog"—their communication systems are not languages proper. For another, this gene seems to play a role in motor development, rather than linguistic development per se. In the rat, it encodes a transcription factor implicated in the normal development of the corpus striatum, a part of the brain involved in the planning and sequencing of motor behaviors. This supports an alternative view of what is wrong with the KE family. It's not that they lack a grammar gene, it's rather that they have an articulatory problem in moving the mouth, lips, and tongue so as to form certain language sounds. On this alternative view, it is this articulatory problem that in the first instance hinders the affected family members from learning some of the relevant grammatical rules (lack of practice makes imperfect) and in the second instance prevents them from expressing what linguistic knowledge they do have. The gene isn't language-specific, and it isn't even species-specific, so there's no support for the innateness of language here. On the contrary, the fact that a gene concerned with motor development is closely implicated in a disorder of language supports the empiricist view that lots of different abilities have come together to enable language learning.

To defend the alternative argument that children learn language from the information they hear around them rather than having large chunks of it built in, we also have to explain why human languages across the world are so similar to one another. Proponents of the innateness hypothesis have argued that all languages—described at a suitable level of abstraction, anyway—are the same. They reason that this is because all people have a universal grammar embedded in their heads and, of course, all languages conform to this universal grammar. The features that are common to all the world's languages, the linguistic universals, are somewhat controversial, and if you had five linguists in a room, they wouldn't reach any agreement about what they are. But one relatively uncontroversial feature common to all, or nearly all, languages is the syntactic distinction between nounlike words and verblike words. Most languages treat nounlike words—words that refer to things—differently from the way they treat verblike words—words that refer to actions, processes, or states. Nativists claim that these similarities across languages arise because universal grammar is known innately. But there are other explanations.

For example, some broad similarities among languages are almost certainly due to universal features of the communication situation. We use language to communicate, so precious necessities for communication are going to shape everybody's language. Indeed, in 1921, the linguist and anthropologist Edward Sapir proposed that the noun-verb distinction arose because to communicate, you need a way of picking out something as the topic of your utterance (e.g., the bee), and then a way of saying something about it (stings). So, of course, all languages are going to develop ways to do those things.

Was language invented only once, in Africa, about 125,000 years ago by a group of early *Homo sapiens* that eventually populated much of Asia and Europe? The 90,000-year-old skull shown here was found in the Skhul cave near Mount Carmel, Israel.

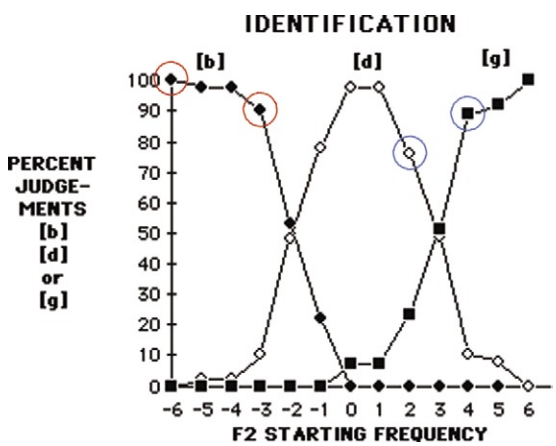
Other common features of language are probably due to nonlinguistic features of human cognition, such as processing or attentional constraints. Most people don't use sentences that are 15,000 words long, and it's not because of anything deep, it's because peoples' memories and attention spans just don't last that long.

Some features of language may just be historical accidents, like driving on the right. There's nothing inherently correct about driving on the right side of the road as opposed to the left, but someone, somewhere, just decided that was how we were going to do it, and we all conformed (except the British, some of their former colonies, and the Japanese, who are still holding out), because it was easier to do so than to effect a change to the other side of the road.

The same could be true of some language universals, particularly those that seem inexplicable in terms of communicative necessities or general features of our brains and minds. What matters for communication is not so much *what* rules we all follow, but that we all follow the *same* rules. Seemingly arcane or strange rules might thus be adopted, and might subsequently persist, because changing our linguistic conventions would lead to communicative breakdown. If certain rules became fixed in a common ancestor language, and if changing those rules was more bother than it was worth, it could explain why all languages spoken today share certain features. Is there evidence for such a common ancestor language? It used to be thought not, but recent developments in historical linguistics, archaeology, and genetics suggest that all human languages are descendants of the language spoken by a group of people coming out of Africa about 125,000 years ago. Arbitrary convention plus common descent, rather than constraints imposed by an innately known universal grammar, can explain linguistic universals.

The crux of the issue between nativists and their opponents is this: Are the processes by which we learn language specific only to learning language, or not? The nativist says Yes: after all, innate knowledge of universal grammar would be useful for learning language, but not for much else. The empiricist or constructivist says No: there's innate stuff involved in language acquisition, of course, but that stuff is used for other learning tasks as well.

As a kind of test case, let's look at phonological learning, which has for many years been touted as a convincing defense for nativism. Phonemes are the smallest linguistic units relevant to meaning.



I. G. Mattingly, *American Scientist*, 1972, 60, 327–337, with permission.

Listening to a series of computer-generated sounds in which the frequency at the start of each sound changed over a continuum, the subject in the experiment at left heard only three syllables: ba, da, or ga. And although the difference in frequency between the red-circled markers was more than that between the blue-circled ones, both reds were heard as ba, while one blue was judged to be da, and the other ga. The brain "chunks" these sounds into familiar categories and doesn't hear the nuances in between. This is also true of newborn babies, like 10-day-old Ella, below, and even of the chinchilla on the facing page.

In English, they are sounds like be, ke, pe, te, and ah (which are often written as /b/, /k/, /p/, /t/, and /a/). According to nativists, all phonemes for all possible languages are represented in our brains at birth, and all that our experience does during language learning is to prune away the phonemes we don't need for the particular language we're learning. If we were learning Japanese, for example, the distinction between the English /l/ and /r/ sounds would be pruned away. There is some support for this account. Phoneme perception begins in the womb, and newborns prefer the sound of their mother's voice and the sound of their parents' language minutes after birth. Infants aged between one and six months can reliably discriminate many different natural-language phonemes, even ones not occurring in the language being spoken around them, but after 12 months they have lost that ability. So although a Japanese six-month-old can discriminate /l/ and /r/ sounds, a one-year-old can't. It does look as if we're all born with innate representations of these sounds and they wither away if we're not using them.

The nativist position is undermined, however, when you look at the mechanism by which phoneme perception occurs. Then you find that it's initially inborn but shaped by learning, that it's not language specific, and that it's not even specific to our species. Our brains distinguish phonemes by a mechanism called categorical perception. The brain takes the continuous speech stream, which is a continuously varying acoustical signal—a bunch of noise, basically—and segments it into chunks that map onto the phonemes of our language. It's a very complicated process, as you can see by looking at the graph of an artificially engineered acoustical signal that varies continuously along one dimension, and what peoples' response to that sound is. When the starting frequency is –6,

–5, –4, and –3, people judge it's the sound of the letter /b/, then when it gets up to 0, they judge it to be /d/, and at +4, +5, and +6, they think they're hearing /g/. Moreover, the two sounds that have red circles differ from one another physically much more than the two markers encircled in blue, yet both red sounds are judged to be the sound /b/, whereas one blue sound is judged to be /d/ and the other is judged to be /g/. Such responses are characteristic of categorical perception, in which some things that are physically or acoustically different are counted as being the same, while other things that differ physically by exactly that same amount are counted as being different.

The brain also chunks faces into familiar categories, as when George W. morphs into Arnie.

The ability to perform categorical perception is inborn, but it's not language specific. The "chunking" of continuously varying stimuli into discrete categories is a general feature of human perception, and we do it with meaningless sounds such as cheeps, chirps, and bleats, we do it with musical sounds such as those from a violin, and we do it with faces. For example, if a digitized picture of George W. Bush's face is "morphed" gradually into one of Arnold Schwarzenegger, there will come an abrupt point when people change their response from "It's George" to "It's Arnold." No in-betweens.

It's also not specific to our species; crickets, birds, chinchillas, and other animals all "chunk" their acoustical input. Indeed, chinchillas respond to human speech by chunking it into /b/, /t/, and /d/ in exactly the same way newborn babies do. So even the case of phonological knowledge, in which innate abilities do figure largely, does not support a nativist picture of language acquisition. Instead, it supports an alternative picture whereby our linguistic abilities are cobbled together out of preexisting and nonlinguistically specific mechanisms.

The same is very likely true of our other linguistic capabilities. It's unlikely that there's a highly specialized language-acquisition mechanism and much more likely, I think, that language acquisition draws on mechanisms of far more ancient lineage such as the ability to "chunk" incoming perceptual signals into larger units, the ability to recognize statistical regularities among these signals and generalize from them, the ability to deploy attention to important tasks, the ability to share attention with others of the same species, and (of more recent origin) the ability to figure out what other people are thinking, to learn by imitation, and to use tools (like language) as a means of manipulating the world.

To be sure, these abilities would have been honed by the positive selection pressure that came into play as soon as language got up and running, because language is so useful that any trait that enhanced the ability to learn it would have been massively selected for. But it's unlikely that natural selection created a radically new language organ embodying knowledge of universal grammar. Which is just as well, since, as I've argued here, there's not much reason to think we'd need one. □

A native of Sydney, Australia, associate professor of philosophy Fiona Cowie gained a BA at the University of Sydney in 1988, followed by an MA and PhD in philosophy at Princeton in 1992 and 1994, respectively. She came to Caltech as an instructor in 1992, became an assistant professor in 1993 and an associate professor in 1998. Her book What's Within? Nativism Reconsidered *gained her the 1999 Gustave O. Arlt Award in the Humanities from the Council for Graduate Studies. She is presently working on another book with James Woodward, the Koepfli Professor of the Humanities, to be entitled* Naturalizing Human Nature: Beyond Evolutionary Psychology. *This article is adapted from a talk given on Seminar Day in May.*

PICTURE CREDITS:
12, 13, 21 – Doug Cummings; 12 – Fiona Cowie; 13 – Caltech Archives; 14, 15 – Herb Shoebridge; 19 – Smithsonian; 20 – Kate Quirk; 21 – Vienna Veterinary Dept.

Godfather of the Hybrid

Victor (left) and Herman Wouk enjoy a celebration in their honor last April in the Athenaeum. Victor was his "bypass," Herman said in remarks praising his brother's contribution to his novels, around the "one-way valve that normally flows from the humanities to science and then shuts."

Born in the South Bronx in New York City in 1919, Victor Wouk earned his bachelor's degree from Columbia University in 1939 before heading west for graduate school. In choosing between Stanford and Caltech, he picked Caltech because, as he said recently, it had open-book exams. Drawn to Caltech's state-of-the-art High Voltage Lab—the first such laboratory in the country—Wouk received his MS in electrical engineering in 1940 and his PhD in 1942. His first company, Beta Electric Corporation, which he formed in 1946, grew to become in a decade the largest manufacturer of high-voltage power supplies. Then he went on to found other companies, leading to an interest in and then a passion for electric and hybrid automobiles. He holds more than 10 patents, most of them on various features of electric and hybrid vehicles.

He was a strong supporter and guiding spirit of the 1968 cross-country electric-car race between Caltech and MIT, won by Caltech's Wally Rippel, BS '68, (see *E&S*, October 1968).

Wouk recently donated his papers to the Caltech Archives—35 linear feet, much of which relates to the day-to-day running of two of his companies, Victor Wouk Associates and Petro-Electric Motors, Ltd. The collection, according to the Archives, "provides a good window on the life of a research scientist, engineer, and entrepreneur, as well as his extraordinarily diverse activities and passions." Wouk and his wife, Joy, have established a fund to support researchers interested in working on these papers.

The collection also contains many decades of Victor's correspondence with his brother, Herman, the novelist. Herman Wouk's latest novel, *A Hole in Texas*, is dedicated to Victor, and its hero is a scientist at JPL. (The character Palmer Kirby, a Caltech alum, in *War and Remembrance* was actually based on Victor.) At an April 14 luncheon in the Athenaeum to honor both Herman's new book and the Archives' new Wouk collection, Herman Wouk described how his brother had brought him into the Caltech family. The opportunity to talk to Caltech faculty, he said, like Hal Zirin, Jesse Greenstein, and Richard Feynman, was "the closest I can come to being a scientist."

Last May, Caltech Archivist Judith Goodstein interviewed Victor Wouk in New York. In that oral history, he described, among other things (including his grad school days at Caltech), the hybrid car he built in the early '70s, a quarter of a century before hybrids finally rolled onto American roads. That section of the interview is excerpted below.

Wouk explains to Congressman Edward Patten the function of the nickel-cadmium batteries in the power system of the Gulton–American Motors electric car in November 1969.

Wouk sold his first company, Beta Electric, in 1956 and formed a new one, Electronic Energy Conversion Corporation, in 1960 to make smaller, higher-efficiency AC-to-DC converters. In 1962 this company came to the attention of Russell Feldmann, president of the National Union Electric Company and one of the founders of Motorola, who had bought a fleet of 30 Renault Dauphines in which he installed batteries and electric motors. But he had trouble with the speed control, and thought perhaps Wouk's efficient DC power supply would solve his problem. Wouk inspected and drove Feldmann's cars and told him basically that "the problem wasn't the energy wasted in the speed control; it was just that the batteries didn't have enough energy to take the car far or fast." Feldmann dropped his project, but Wouk kept going. He contacted Caltech president Lee DuBridge, who convened an informal seminar of physicists and chemical and electrical engineers to explore the question of building better batteries, coming to the conclusion that there were still many problems to be solved before that could be done. But after Caltech's Arie Haagen-Smit showed that Los Angeles smog was due mainly to gasoline exhaust, Wouk thought electric cars might still have a future.

Victor Wouk: In 1963 I sold the Electronic Energy Conversion Corporation to Gulton Industries, a company that was making nickel-cadmium batteries and had a subsidiary in California making power supplies based on the principle of what I call the Convertron—putting in AC, changing it to DC, and then chopping it up at high frequency. The Electronic Energy Conversion Corporation was now a subsidiary of Gulton, operating out of my old office and lab at 342 Madison in New York. I was very happy with that.

Then one day Dr. Gulton calls in the section managers (I was head of electronic research) and said, "I want more applications for nickel-cadmium batteries that we are now building for the air force."

So at the meeting I said, "Oh, Dr. Gulton, maybe electric cars would be a good application."

"Why?"

"Well, you can get much more current, so the cars need not be sluggish." This had been the *big* objection. People would say, "Well, I don't care about the range, but they're sluggish." And Gulton said, "Fine idea—start working on it."

I thought about it and realized that if we're going to get some performance and the vehicle is going to be quasi-experimental, I want a big car—a station wagon. And Gulton wanted a tie-in with some automobile company in Detroit. We couldn't do it with GM or Ford—they had their own electric programs. Same problem with Chrysler. But American Motors was losing money in those days. And after some negotiation, a contract was drawn between Gulton and American Motors. Gulton Industries would develop a new battery based on lithium and, using my speed controller, a wonderful car.

Judith Goodstein: What brand of car?

VW: This was an American Motors station wagon. So I put a lot of batteries in the back, put other things in the back, and you could still have at least two people up front and three people behind. I began to build this machine, because I liked the idea and there was this great potential.

Then along came the Clean Air Act of 1970.

JG: How long had you been building this car?

VW: I started building the car in about 1967. That is, I set up breadboards of a speed controller and this and that and the other thing. I had to go through a lot of stages, testing things that were absolutely new.

Wouk soon realized that electric cars were probably not going to catch on with the American consumer. Speed and acceleration were tricky, the cars were *unreliable, and the better-battery problem wasn't going to be solved anytime soon. But a car that combined a conventional internal combustion engine with an electric engine—now, that just might work. He began to try to stir up interest in hybrids.)*

Then in 1968, Washington began to legislate—and California already had legislated—emission limits on vehicles. So everyone immediately thought of electric cars. And I had to go to various people to disabuse them: It isn't the smart controls. It's the battery. Until we multiply the battery capacity by at least a factor of three, and preferably eight, we'll be no competition for conventional cars. And I would be told, "Oh, you don't have any faith. It's got to be all electric." I was actually being accused of being anti-electric car. I'd say, "It's not that I don't want electric cars, I want cars that will work!" And they would say, "If it's a hybrid, you've still got an internal combustion engine; you're going to have some emissions. We don't like the idea."

The 1970 Clean Air Act required that by 1976 emissions be reduced by a factor of 95 percent. And I can interject here that at one time, while Dr. DuBridge was still the science advisor to President Nixon, he was following my program, because I let him know what was going on. Oh, and of course he knew about the earlier business with the battery. I told Dr. DuBridge about the hybrid, and he thought that was a great idea. And he said, "Victor, do you know why the pollution regulations require 95-percent reduction and not 80 percent or 99 percent? "

I said I had no idea.

"Were you ever in California in the 1930s?"

And I said, "Yes, I went to summer school in 1937."

"Where were you?"

"UCLA."

I was actually being accused of being anti-electric car. I'd say, "It's not that I don't want electric cars, I want cars that will work!"

"Oh, then you will remember that on a clear day, you could see Catalina."

"Yes, sir."

"And you could see it rather often."

"Yes, sir."

He said, "We made the specifications on the basis of some computations as to how much we would have to reduce current pollution to get down to where we could see Catalina. So, the number of cars has increased, the mileage they've gone has increased, and when you put the two together, it turns out that pollution from cars is about 20 times greater than in 1937. So we want to reduce emissions 95 percent." That's the explanation.

I spoke to several organizations about hybrids, and nobody was interested. By this time, Dr. Gulton was retired from Gulton Industries, and his successor wanted nothing to do with hybrids because there was no government program for hybrids.

JG: No source of funding.

VW: Exactly. So then all of a sudden we hear about the Federal Clean Car Incentive Program (FCCIP), which was initiated in 1970. I forget how I heard of it; maybe Charlie Rosen heard of it first, because he and I worked on the electric car also. He was a chemical researcher at Gulton.

Now, for some reason I had been out in Ann Arbor several times and I knew the EPA people who were going to run this program. And they said, "Hey, Vic, let 's talk about your hybrid car. How about proposing?"

JG: Were you the first to propose the name "hybrid vehicle"?

VW: No. Some of the early cars in the early 1900s would be referred to as "dual powered," and I think the word "hybrid" was in one of the early patents.

JG: And those early cars, when you speak of dual power, what were the two sources of power?

VW: Same thing—batteries and internal combustion engine. There had been some studies, underwritten by some program or federal agency or other, by Aerospace Corporation, in the Los Angeles area. They had a contract to study all types of hybrid. I knew the president of Aerospace Corporation, Ivan Getting. So he and I communicated. I said "I'd like to get more information," and arrangements were made. I went to see him and got the information. And they more or less agreed with what I had been thinking—that the hybrid *could* do this, cut down emissions, cut down fuel usage. So I went to the president of Gulton and said I'd like to bid on this. And again, for various reasons much too complicated to discuss here, we were not even allowed to think about the Federal Clean Car Incentive Program, number one, and even if we did, it would not be hybrid.

So, Charlie and I decided we were not going to work on the DC sources anymore. We wanted to work on a car, which Charlie and I were confident would at least meet the specifications. So Gulton said, "OK, goodbye, thank you very much," and I had to start a new company. I didn't know quite what to call it. My brother Herman came up with the idea. He said, "You use petroleum, you use electric. So, Petro-Electric Motors. And 'Ltd.'—Limited, which makes it sound very fancy." So Charlie and I worked on the proposal for the

FCCIP. It took us about a year. And we prepared this and bid.

What we were asking for was the privilege of building this vehicle at our own expense and having it tested at our own expense, to prove that it would beat the 1976 requirements on emissions. What the EPA would do is, after we had called them in and said, "Hey, this meets the specs as indicated by a test at such-and-such a lab," which was certified, they would give us one dollar for having made the preparation and bid. And when they're finished with the tests, they'll give us $30,000. *[Actually $37,351, see contract at left.]*

JG: But all of the R&D is on your nickel?

VW: Correct. Now, where's our incentive? The incentive is that if the vehicle really works, they'll then order 10 of them at a price that might write off most of our R&D. No guarantee, but it might. And the EPA said: "We'll test them, and if after a year they are still low emissions, low fuel users, we will order 350 for government offices throughout the country. And as a *real* incentive, we will pay you twice the price that the government would normally pay for an automobile." My real incentive was mainly to prove the damn thing worked. After that, we wanted to be able to move ahead, let someone buy us out, and I'd get out of it.

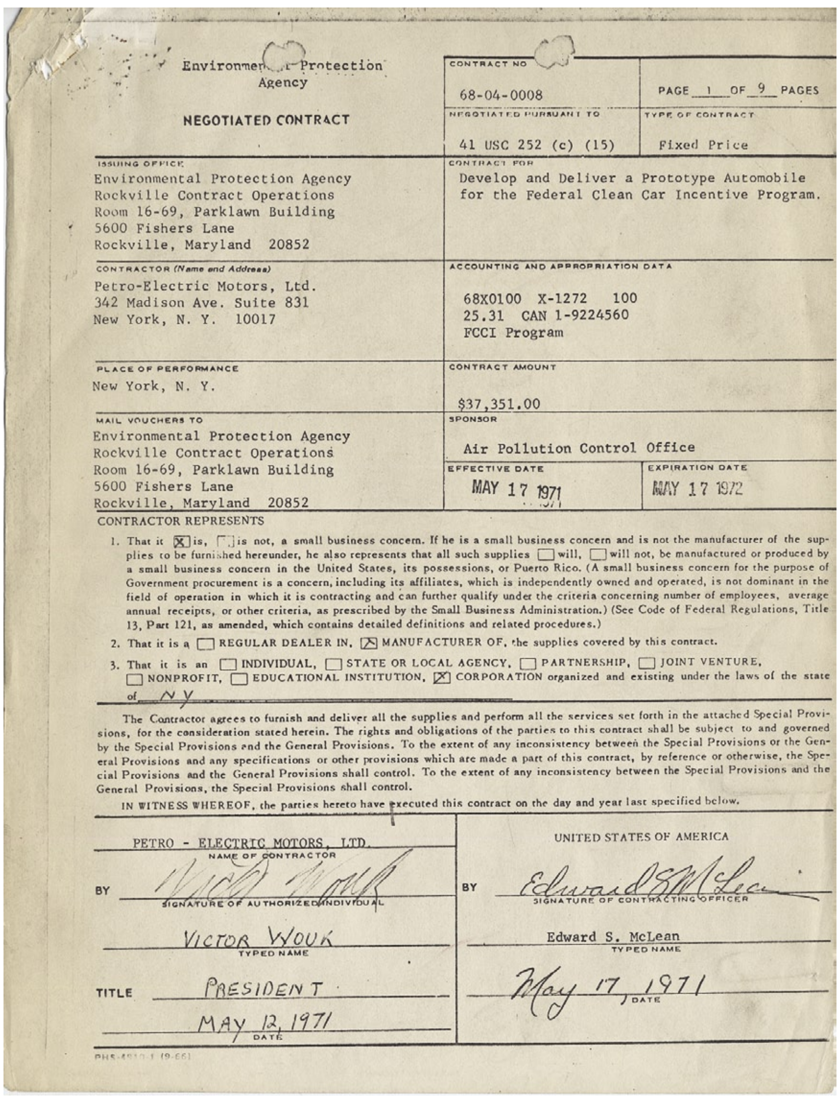
So we sent the proposal in, and I forget how long it was—two months, three months—before we get a phone call and a letter, saying, "We like your proposal. Very interesting. Technically feasible. You are hereby given the contract. Get started."

There were six other vehicles in the program. There was our hybrid, one electric car, one diesel, one with a simple exhaust filter.

JG: You're the only hybrid?

VW: We're the only hybrid. And we start building it. It was a long, uphill struggle because I'm not an automotive engineer, nor is Charlie Rosen. Rosen is a thermodynamicist; got his PhD in thermodynamics engineering at what was then Brooklyn Poly, now just Polytechnical Institute. We divided the work—I would be doing all the electronics; he would be doing the emissions reduction. When we had a vehicle pasted together, I contacted a Professor Smith at the University of Michigan, in Ann Arbor, whom I knew very well, who was familiar with my background on electric cars, and who liked the idea of hybrids. So he spoke to the chairman of the automotive engineering department, David Cole. (And by the way, at present Cole is head of the Center for Automotive Research, which studies the automotive industry throughout the world—a very highly respected man.) Smith got permission for us to bring a car out to Ann Arbor to do the final tuning up of the engine and the electronics and everything, so that we would have a working vehicle with low emissions, and it would

My real incentive was mainly to prove the damn thing worked.


Environmental Protection Agency

NEGOTIATED CONTRACT

CONTRACT NO 68-04-0008	PAGE 1 OF 9 PAGES
NEGOTIATED PURSUANT TO 41 USC 252 (c) (15)	TYPE OF CONTRACT Fixed Price

ISSUING OFFICE Environmental Protection Agency Rockville Contract Operations Room 16-69, Parklawn Building 5600 Fishers Lane Rockville, Maryland 20852	CONTRACT FOR Develop and Deliver a Prototype Automobile for the Federal Clean Car Incentive Program.
CONTRACTOR *(Name and Address)* Petro-Electric Motors, Ltd. 342 Madison Ave. Suite 831 New York, N. Y. 10017	ACCOUNTING AND APPROPRIATION DATA 68X0100 X-1272 100 25.31 CAN 1-9224560 FCCI Program
PLACE OF PERFORMANCE New York, N. Y.	CONTRACT AMOUNT $37,351.00
MAIL VOUCHERS TO Environmental Protection Agency Rockville Contract Operations Room 16-69, Parklawn Building 5600 Fishers Lane Rockville, Maryland 20852	SPONSOR Air Pollution Control Office EFFECTIVE DATE MAY 17 1971 — EXPIRATION DATE MAY 17 1972

CONTRACTOR REPRESENTS

1. That it [X] is, [ ] is not, a small business concern. If he is a small business concern and is not the manufacturer of the supplies to be furnished hereunder, he also represents that all such supplies [ ] will, [ ] will not, be manufactured or produced by a small business concern in the United States, its possessions, or Puerto Rico. (A small business concern for the purpose of Government procurement is a concern, including its affiliates, which is independently owned and operated, is not dominant in the field of operation in which it is contracting and can further qualify under the criteria concerning number of employees, average annual receipts, or other criteria, as prescribed by the Small Business Administration.) (See Code of Federal Regulations, Title 13, Part 121, as amended, which contains detailed definitions and related procedures.)
2. That it is a [ ] REGULAR DEALER IN, [X] MANUFACTURER OF, the supplies covered by this contract.
3. That it is an [ ] INDIVIDUAL, [ ] STATE OR LOCAL AGENCY, [ ] PARTNERSHIP, [ ] JOINT VENTURE, [ ] NONPROFIT, [ ] EDUCATIONAL INSTITUTION, [X] CORPORATION organized and existing under the laws of the state of N Y

The Contractor agrees to furnish and deliver all the supplies and perform all the services set forth in the attached Special Provisions, for the consideration stated herein. The rights and obligations of the parties to this contract shall be subject to and governed by the Special Provisions and the General Provisions. To the extent of any inconsistency between the Special Provisions or the General Provisions and any specifications or other provisions which are made a part of this contract, by reference or otherwise, the Special Provisions and the General Provisions shall control. To the extent of any inconsistency between the Special Provisions and the General Provisions, the Special Provisions shall control.

IN WITNESS WHEREOF, the parties hereto have executed this contract on the day and year last specified below.

PETRO - ELECTRIC MOTORS, LTD. NAME OF CONTRACTOR	UNITED STATES OF AMERICA
BY SIGNATURE OF AUTHORIZED INDIVIDUAL	BY SIGNATURE OF CONTRACTING OFFICER
VICTOR WOUK TYPED NAME	Edward S. McLean TYPED NAME
TITLE PRESIDENT	May 17, 1971 DATE
MAY 12, 1971 DATE	

PHS-4910-1 (9-66)

Relations with the EPA have not yet started to go downhill, as Wouk poses proudly with his 1972 hybrid Buick Skylark at the EPA test site.

then be up to the two of us back in New York to tweak everything so that we really get the best out of it.

We took the car from New York to Ann Arbor by truck. And then we tweaked the car, back in New York. How did we get the car *back* to New York? This is now 1973, and there was going to be a hearing in Washington about some new legislation about emissions and fuel economy. I was planning to drive the car, with Charlie, from Ann Arbor to Washington. (*A huge snowstorm forced them to put the car on a plane for the trip to Washington.)* We had driven it quite a bit before, and it worked. People ask, "What was the top speed?" I say, "I never went over 85 miles an hour," because there was some rattling. The car was not assembled the way they would do it at GM.

JG: And what kind of a car was this, now?

VW: This was a 1972 Buick Skylark.

JG: Who provided the car? Did you buy it?

VW: The car was provided by General Motors. Once we got our contract, I went looking around for the car I wanted. I went to various showrooms in New York. I looked under the hoods. The Buick Skylark seemed to have the most volume under the hood. And not knowing exactly how much space we were going to need, I wanted a car with the largest volume under the hood.

So I went to a dealer and said I wanted a Buick Skylark. And he said, "Very sorry, but the one here is already sold."

"So how about a new one?"

He said, "I don't know. The line is closed." So I had to send a letter to Dr. Thompson, director of the GM Technical Center, in Warren, Michigan. I knew him because I was on a panel with him at the American Bar Association building in Manhattan, discussing low-pollution cars. Eric Stork, who was in charge of the mobile systems emission section of the EPA, was on this panel. And when it was all over, Thompson came to me and said, "I certainly appreciate what you said about electric cars being extremely limited and that's why you're talking about the hybrid. Most people involved will not admit the serious hurdles that have to be overcome." So that's how I knew him.

So when I explained this situation to him—that I wanted a Buick Skylark and there were none to be had in the country, they'd all been sold out—I got a telephone call or letter saying, "Let me investigate this." And about two weeks later, I get a telephone call from a Buick manager in New York saying, "I don't know what's going on here, but I've been told that I'm going to be getting a Buick Skylark, and it's for you. But you have to buy it; it's not being given to you for nothing." It was $2,700 in those days. Obviously the reason for their not giving it to us is because it would mean they were neutral; they're just helping out some young fellows with some cute ideas.

Getting back to the hearing in Washington, I gave my testimony, and we showed the car to a lot of people, who were very impressed, and then we drove it back to New York. No problem at all. And there we were given the run of the labs in Brooklyn—the Clean Air Department of New York City had this very good car-testing laboratory in Brooklyn. You could get on a dynamometer, you could do all sorts of things; they measured the emissions and the fuel economy. And tweaking the car for optimum emissions was a pretty tough thing, because we were using the Wankel engine.

I have to backtrack here. The reason I wanted a Wankel engine is that it was squat for the same amount of horsepower as a conventional piston engine. And as I mentioned, I didn't know how much electronics I would have to put under the hood or how big an electric motor, so I wanted something that was squat, and Mazda's Wankel engine fitted this requirement absolutely beautifully. The Mazda had proved to be a sensation on the West Coast, where it was being sold. Because here was this little car—a pipsqueak—and if it was up against a Caddy, and they were both stopped at a red light, the Caddy would slam the accelerator all the way down and by the time he took his foot off the accelerator, this pipsqueak would be a half a mile ahead of him. Why? Because it was a small car, lightweight, and had this Wankel engine, which developed twice as much power per unit volume as a conventional car.

But you couldn't go to the corner store and buy a Wankel. GM had paid $50 million for some contract with Mazda to do something with the Wankel, and Mazda had some sort of arrangement with Curtiss-Wright to do something with the engine on the East Coast—maybe apply it to propeller-driven planes. So there was a Mazda representative on the East Coast. Now someone who knew Curtiss-Wright very well introduced me and said, "This gentleman would like a Wankel engine." In fact, I made a presentation to the top brass at Curtiss-Wright and they said, "It sounds good. We'll see what Mazda has to say back in Hiroshima."

Three or four weeks later, I got a phone call saying Mazda liked the idea and they were going to send me *two* engines, so if something didn't work well on the first engine…. Well, I'm absolutely flabbergasted, they're sending us two engines! Which they did—complete engines with all the auxiliaries. So here I had this nice squat engine and we were able to put that in the Buick Skylark. Now the only car on the East Coast that was using the Wankel was our Petro-Electric Motors hybrid. When people say the automobile and gasoline companies won't give you the time of day, the answer is, "On the contrary!" They were very interested in what was being developed, because

He thought—and he expressed the opinion—that the function of a government agency is to set standards and regulate. It is not to help a company pass tests and everyone become millionaires.

the thing worked. The Wankel engine is still used today, I think, by Mazda on their RX.

So, anyway, we needed to get the operation perfect, and we had use of this enormous lab in New York City. We had been given the run of the place because the man in charge of the Clean Air Act in New York City—one Brian Ketcham—I had met, because at that time I was one of the founders of and active in the Citizens for Clean Air in New York. He liked the idea of a hybrid and he said, "OK, you can have it whenever you want." So, over a period of about two or three months, we were in and out of the labs in Brooklyn. And soon the car was ready to be tested.

We called the EPA in Detroit and said, "We're going to be testing the car. What do you want in the way of certification, so that one of your people from Ann Arbor can come see the vehicle and run the tests for us?" And the only lab that the EPA would accept in the New York area had to be an independent lab. It couldn't be the New York City department, because we were New Yorkers; it couldn't be the facilities at, let's say, Mobil Research, because that's a gasoline company. The closest independent lab was opposite a big town in New Jersey, on the other side of the Delaware River, in Pennsylvania. We went down there two or three times to have tests made.

JG: Did you drive the car there?

VW: That's where I had the 85-miles-per-hour maximum. We would drive the car occasionally, just Charlie and I, and sometimes other people. We went for the tests; all the specs were met. And we called and said we're ready for someone to come. They said, "OK, we'll send . . ."—I forget his name, some other Charlie. At that time, the car was garaged in Charlie Rosen's garage, in Teaneck, where he lived, and his sons began pestering this EPA man. "Come on, are you going to say 'OK we'll test'? Or 'Not OK, we won't.'" He couldn't tell them to go jump in a lake, so he said, "Yes." This was around the beginning of January 1975. He looked at the data. We took him for a ride. And he said, "When I get back to Ann Arbor, I will report to John Brogan, who's the head of everything."

It turned out that the EPA, through a certain "Mr. X," wanted to drop the program.

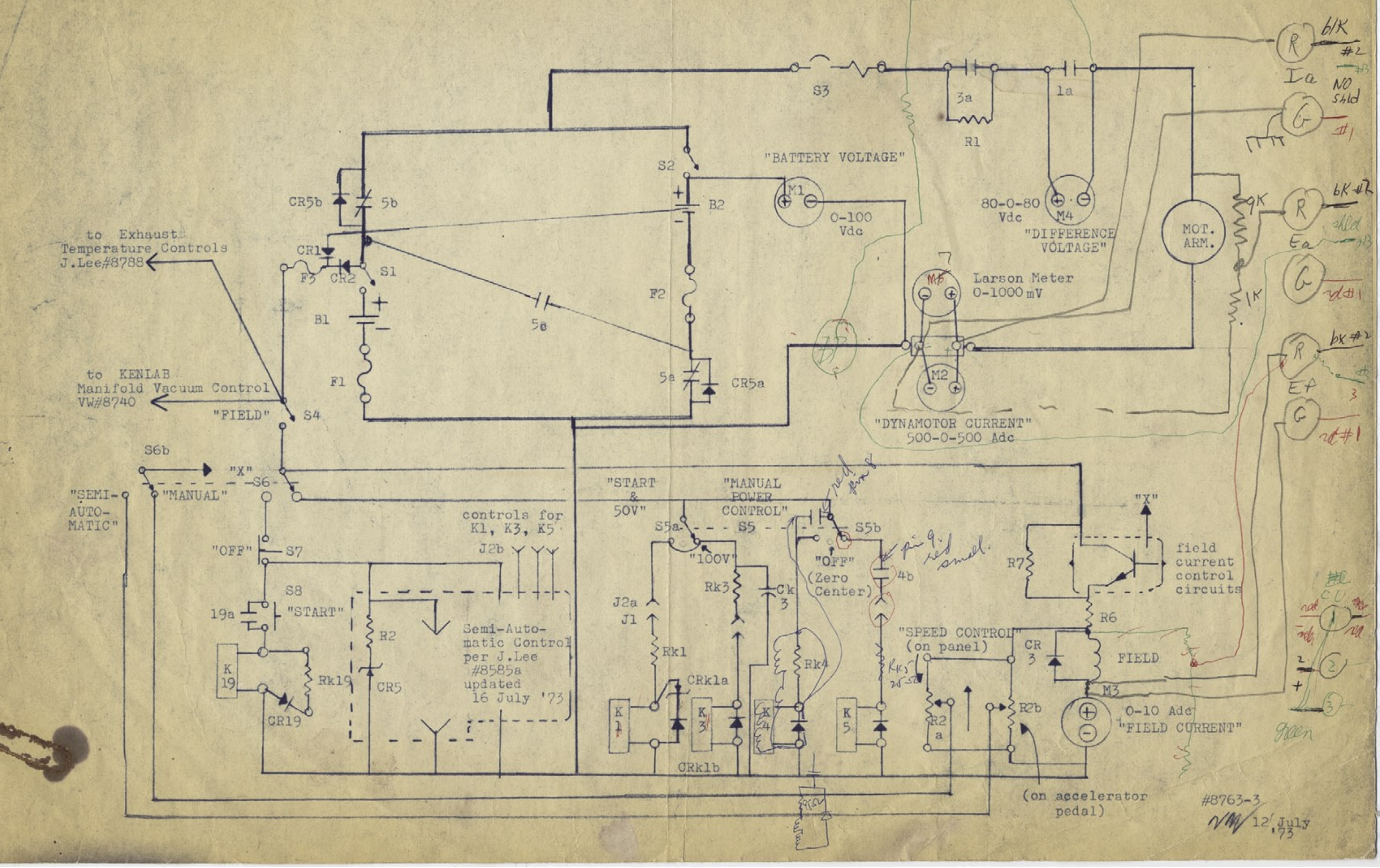
JG: This was before your car had been tested?

VW: Yes, before the car had been tested, before we had even built the final. Meanwhile we were the only ones left in the FCCIP who could possibly even be tested. The others had dropped out for one reason or another.

JG: And just on principle, Mr. X wasn't willing to have it tested?

VW: That's right. He thought—and he expressed the opinion—that the function of a government agency is to set standards and regulate. It is not to help a company pass tests and everyone become millionaires.

We made arrangements to go to Ann Arbor—this was now something like the middle of January. We might have to do some final tuning up at the University of Michigan, where we were doing the testing. We brought the car out. There were some slipups in the beginning, and it looked as though my idea was not a very good one. Then I realized what the mistake was. I had the mistake corrected



Among the papers in the Archives' Wouk collection is this detailed circuit diagram of the electrical system for powering the motor of the Petro-Electric Motors hybrid. Wouk's initials and the date are in the lower right corner.

at the university and the car breezed through the tests, except for one thing: Every now and then, there would be a spike of emissions and that would vitiate the entire test. All you needed was a little spike of emissions for one half-second and the average emissions would be above what was allowed. We eventually found out what that problem was, and that was going to require some more tweaking of the emission control.

So here we have these little spikes, and we needed help to do something about the emission control. And Charlie Rosen said that what we needed was a richer mixture, which should come down when the vehicle starts running, because otherwise with a richer mixture all the time, the fuel consumption would be too high. So we did that. In about a month we finally got a beautifully operating thing.

We made the final tests at the EPA in Ann Arbor, and most of them were well within the range. Then they said: "We'll determine whether you go on to Phase II of the program." So we see the report about a month later from the EPA people as to why we did not meet the specifications.

JG: Were you shocked?

VW: No, no. And that is something I'm glad you asked. When we were near the end of our tests at the EPA, we had become very friendly with the engineers who were supervising. There was one who was particularly upset that we were sunk from the very beginning. He said that Mr. X had come in and said, "Under no circumstances is the hybrid to be accepted."

JG: Mr. X said that to the engineer?

VW: Yes. Before we finished. "Under no circumstances." Why? Again, he thought that the government should regulate, not make people rich—"If you think you're so smart, build the car and build lots of them and we'll buy them. Don't have us test them."

JG: Didn't you have a contract to do just that?

VW: Yes. So the question was in the interpretation of the contract, as to whether we met the requirements. There was a lot of Mickey Mousing. And the record of letters back and forth is half-an-inch thick. Now the Archives has them. What you don't have is, unfortunately, the smoking gun.

JG: OK, tell us about the smoking gun.

VW: It was a two-page letter from Mr. X to me in 1976 on federal letterhead saying, "You have

If we must reduce automobile pollution and reduce automobile fuel consumption a large amount in a short period of time, the only thing you should do is use existing technologies . . . and as these technologies improve . . . you just go ahead.

a very good thing; it works beautifully. It cuts emissions, cuts fuel consumption. But basically I think it's the wrong approach. And if I'm proved to be wrong, I will be the first to admit it." So I may still either (a) find the letter or (b) I don't have to find the letter but send him a letter—registered of course. As soon as I find the letter, I'm going to tell him I'd like him to fulfill his statement and have a full-page ad in the *New York Times*, *Wall Street Journal*, and *Washington Post*. I won't insist that he have it in the *Los Angeles Times*. (*The letter disappeared in photocopying before Wouk's papers were sent to the Caltech Archives.)*

JG: Suppose Mr. X had been a different sort of person, not as committed to his point of view. Do you think it would have meant a different outcome for this country and the evolution of hybrid cars?

VW: That is my firm belief, and that is what I have been espousing for almost 30 years—after the first tests at the EPA and others. As I always said, the hybrid is the way to go if—if, if, if. If we must reduce automobile pollution and reduce automobile fuel consumption a large amount in a short period of time, the only thing you should do is use existing technologies, base your design on existing technologies, and as these technologies improve, even when you're implementing the design and improving your design, you just go ahead. The principle was proved by our tests at the EPA. But nobody did anything about it until, independently, the Japanese—Toyota and Honda—did it.

JG: How many decades did you talk about this?

VW: Oh, from 1970 through 1980 and from 1990 up until about 1997, when Toyota came out with the Prius.

JG: So, do you consider yourself the godfather of the Prius?

VW: I may or may not be. *(An SAE book credits Wouk as the father of modern hybrid programs.)*

JG: Who are the bad guys in your opinion?

VW: *The* bad guy is Mr. X, who told his men that they had to flunk the Petro-Electric Motors vehicle, before any tests. And Mr. X was told by some of the engineers at Ann Arbor that it would never work anyway, because this was complicated, that was complicated, so "stop worrying about it, Boss." But it did work. And the orders had been given, somehow or other, "You've got to flunk them." So there was this report by Ann Arbor EPA, back to Washington. We were sent a copy of it. I had to write a 75-item rebuttal. And it just dragged on and on. And then finally some of the things I objected to were used in the final meeting on the subject. And then I quit.

JG: Then you quit. Did you close down that company?

VW: Yes. We go back now to that meeting with the NSF. Herman was there, and Brogan and Mr. X. Brogan whispered to me in an aside. He said, "Vic, you are getting screwed. And I'm going to see to it that you get some money back." So on the basis of some tests, he told Mr. X that it would be a good idea to give us another chance. So that in addition to the $30,000 we got for the first series of tests, because some of our tests had looked very, very good, we were able to do another series of tests, for which we got $50,000. The last $50,000 went to Gould, a big manufacturer of batteries, who ran a lot of good things for me. But by 1976 I was so disgusted, I lost so much energy, that I gave up and went into straight consultation.

JG: Do you feel vindicated today?

VW: Absolutely. And not only do I feel vindicated, but people high up in automotive technology—to whom I had forgotten that I'd mentioned things—would come to me and say, "I'm sorry I didn't agree with you then. It was just a professional opinion. It didn't affect anything." Like people on the IEC committee or the SAE committee, others like that. "You've been right all along and we've been wrong."

So I feel vindicated. But I won't feel fully vindicated until I get that mea culpa letter from Mr. X into the *Times*. □

PICTURE CREDITS:
22 – Bob Paz; 23, 25-26,
28 – Caltech Archives

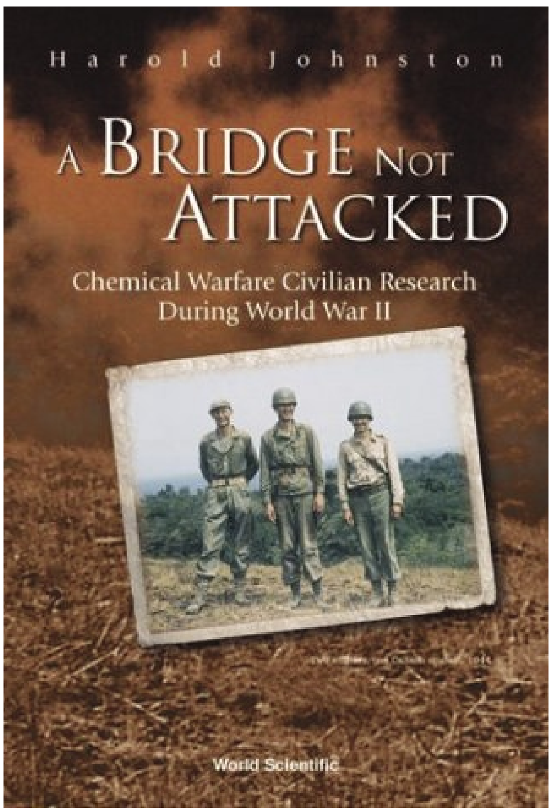
A Bridge Not Attacked

by Harold Johnston

Harold (Hal) Johnston graduated from Emory University in 1941 with a bachelor's degree in chemistry. He entered Caltech that fall and, like most of the campus, soon became involved in the war effort. What started as a laboratory project to improve gas masks soon led to a three-year campaign of field studies in places as far-flung as Florida and Panama. This article is adapted from his recent book on the subject, which is part personal memoir and part historical document.

Johnston resumed his graduate studies when the war ended, earning his PhD in chemistry in 1948. But the years spent tracing the dispersion of poison-gas clouds changed him from an inorganic chemist to an atmospheric one: he was one of the pioneers in the field in the 1950s. He was at Stanford until 1956, returned to Caltech for a year, and spent the rest of his academic career at UC Berkeley. Over the years he worked on the reaction kinetics of ozone, fluorine, chlorine, and the oxides of nitrogen, publishing 165 papers and one book.

Johnston was one of the first to recognize that human activities can have global atmospheric effects. A paper he published in Science *in August 1971 showed that the nitrogen-dioxide exhaust from a proposed fleet of supersonic transport aircraft (SSTs) could, depending upon the exhaust's vertical distribution, lead to global stratospheric ozone reductions of from 3 to 23 percent. This led Congress to set up the first major stratospheric research program—the Climate Impact Assessment Program, or CIAP. CIAP, in turn, provided the basis for F. Sherwood Rowland and Mario Molina's Nobel Prize–winning discovery (with Paul Crutzen) of the effects of chlorofluorocarbons on the ozone layer. Johnston was elected to the National Academy of Sciences in 1965 and the American Academy of Arts and Sciences in 1972. His awards include the Tyler Prize for Environmental Achievement (1983), the National Academy of Sciences Award for Chemistry in Service to Society (1993), and the National Medal of Science (1997).*



A Bridge Not Attacked:
Chemical Warfare Civilian Research During World War II
By Harold Johnston
World Scientific Publishing Co.
2003
276 pages; $50 hard cover, $20 paperback

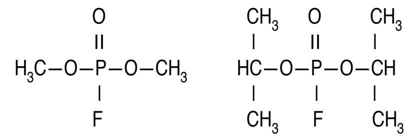
PICTURE CREDITS:
32 – Caltech Archives; 34, 35 – Roscoe Dickinson; 35 – John Otvos; 36, 37 – U.S. Army; 38 – Hal Johnston; 38 – Bob Mills

I entered Caltech on September 20, 1941, three weeks before my 21st birthday. At the end of the winter quarter, I joined Professor of Physical Chemistry Roscoe Dickinson's secret project for the National Defense Research Committee, or NDRC. (Dickinson had received the first PhD ever conferred by Caltech, in 1920.) The NDRC was a civilian organization formed in 1940 to perform contract research for the military on a wide range of problems. In the realm of chemistry alone, NDRC scientists developed such diverse products as napalm and hydraulic fluids.

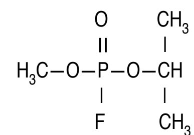
"One breath of it and you die within a day, drowning in your own water and blood, and there is nothing anybody can do about it." I felt an almost pleasant tingle of fear.

Our task was to determine how long a gas mask's charcoal filter would give protection. To do this, we flowed a stream of air and the poison gas through a bed of charcoal and then through counterflowing distilled water. Any unfiltered gas would dissolve, changing the water's conductivity, which was continuously recorded on a strip of chart paper. Thus we could measure the time it took for the gas to break through the bed. We repeated the tests with different amounts of the gas, at different temperatures, and with different charcoals. The War and Navy Departments carried out experiments of this sort by collecting about a dozen samples of the outflow from the bed as time passed, and submitting each sample to a tedious chemical analysis called titration. Dickinson's method was at least a hundred times faster and gave much more information.

The gases were kept in a fume hood in 65 Crellin, in glass bulbs immersed in Dewar flasks filled with dry ice. Though the bulbs' contents were gaseous at room temperature, they were liquid or solid at dry-ice temperature. The first bulb contained phosgene, Cl_2CO; it was our calibrating gas. It was used as a war gas in World War I. Phosgene dissolves slowly in water and passes through the nose and throat with only moderate irritation, but it reacts in the lungs to form two units of hydrochloric acid, HCl, which corrodes the lungs and can lead to death within 24 hours. The second bulb contained chlorofluorophosgene, ClFCO, which was much more toxic. It breaks down in the lungs to form one unit of hydrochloric acid and one unit of hydrofluoric acid, HF, which is a weaker acid but does more physiological damage. The third bulb contained sulfuryl chloride fluoride, $ClFSO_2$. It gives one unit of sulfuric acid (H_2SO_4) plus the acids carried by chlorofluorophosgene. The material in the fourth and fifth bulbs had arrived recently and came with special warnings. These organofluorophosphate gases quickly shut the pupils of the eyes, paralyzed the lungs, and were fast killers.



"Dimethyl poof," or dimethyl fluorophosphate (above left), and "diisopropyl poof" (above right) are close chemical cousins to sarin (below). Sarin was developed by the Germans during World War II, but was never deployed by them. It gained notoriety when used by a Japanese cult in a 1995 rush-hour attack on the Tokyo subways that killed a dozen people and injured thousands more.

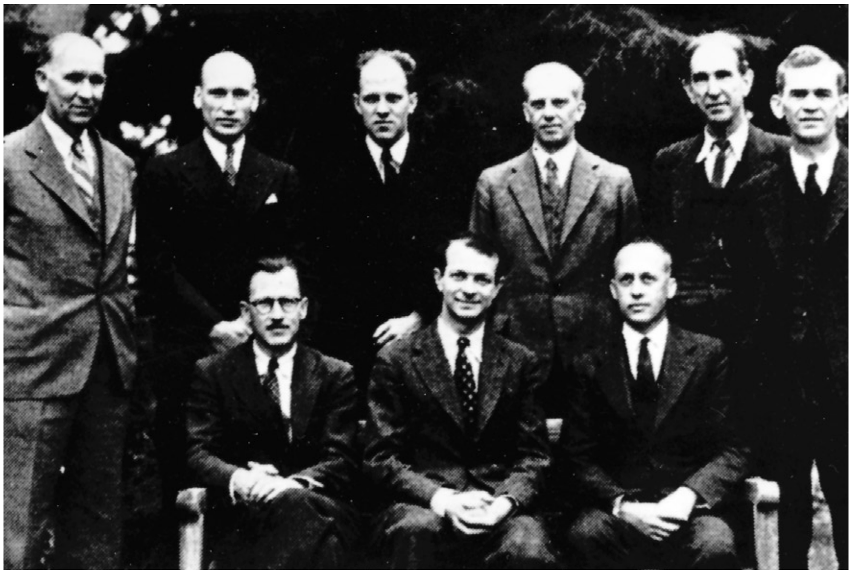


Our group nicknamed them "dimethyl poof" and "diisopropyl poof," with the "poof" standing for the PF in fluorophosphate.

My immediate supervisor was John Otvos [PhD '43], a skilled and patient teacher, even to one as green as I. John was a third-year graduate student, or would have been if he had not joined this NDRC project. On my first day, John showed me around. When we came to a second hood, he lifted a small sealed glass tube out of its dry ice slush; it was about half full of a clear liquid. He said in a matter-of-fact voice: "That is sulfur decafluoride, S_2F_{10}, or S-10. It is a close relative to sulfur hexafluoride, SF_6, which is about the most unreactive compound known to chemistry. S-10 is colorless, odorless, and four times as poisonous as phosgene. One breath of it and you die within a day, drowning in your own water and blood, and there is nothing anybody can do about it." I felt an almost pleasant tingle of fear.

S-10 does not dissolve in water, or strong acid, or strong alkali. We had to pass it down a hot Monel metal tube to break it down so we could detect it in the conductivity cell. I wondered how such an inert molecule could do so much damage. The molecular structure is $F_5S\text{-}SF_5$, two identical twins joined together. I came up with a theory—*disproportionation*: one goes up and one goes down. Somehow in the lungs S_2F_{10} disproportionates into SF_6 and SF_4. SF_6 is stable. SF_4 is a reactive beast. It tears water apart to make four molecules of hydrofluoric acid and one molecule of sulfurous acid (H_2SO_3).

One of our tasks was to manufacture enough S-10 for the army to test in a small bomb. Associate Professor of Inorganic Chemistry Don Yost [PhD '26] had come up with a relatively high-yield synthesis that used the large fluorine generator in 121



Some of Caltech's chemistry faculty in 1937. Rear row, from left: Howard Lucas, associate professor of organic chemistry; Arnold Beckman (PhD '28), assistant professor of chemistry; Bruce Sage (MS '31, PhD '34), assistant professor of chemical engineering; Stuart Bates, professor of physical chemistry; James Bell, professor of chemistry; and Yost. Front row: Dickinson, Linus Pauling (PhD '25), professor of chemistry; and William Lacey, professor of chemical engineering.

Gates—up a flight of steps and down two hallways. John and Arthur Stosick [PhD '39] developed the procedure, and they manufactured some S-10 every day. The dangerous job of transferring the S-10 from the small bulb of each day's work was done only by Professor Dickinson or, sometimes, by Dr. Stosick. It took more than a month to fill a large bulb in Room 65 with the amount of S-10 the army needed.

The army came to collect the gas in their own glass flask attached to a carrying rack. The rack was too tall to fit in the hood, so Stosick clamped it to a heavy wooden desk. The S-10 was transferred by vacuum distillation: we connected the army bulb to the Caltech bulb, evacuated the system, and cooled the army's bulb to draw the S-10 over. Stosick mounted a large-mouth Dewar containing liquid air on an automobile jack, and slowly turned the crank. Upon contact with the army's bulb, some liquid air vigorously boiled away and formed a cold fog, which settled to the floor, spread out, and evaporated. Dickinson watched the S-10 in the hood and told Stosick at what rate to raise the liquid air. The whole process was over soon. The glass line to the army bulb was sealed off and disconnected; Dickinson was a master glassblower. Stosick slowly lowered the Dewar and removed it. They took off their glass-blowing goggles, removed the gas masks from their belts, sat down and smoked one cigarette after another.

We had a local telephone in the laboratory, but to make an outside call, Dickinson had a private line in his office nearby. He announced: "I'm going to telephone them to come pick it up. Art, why don't you take a walk outside and take it easy for a while." He added emphatically: "Everybody else stay away from it." A few minutes later, he came back: "They won't pick it up until about two p.m."

The laboratory was calm. Stosick was fabricating some electronic device. We did most of our analyses by ingenious circuits he had designed. Dickinson sat at his desk, busy with paperwork. John and I began work on one of the "poofs." The S-10 stood on the desk about half a meter from the hood and about three meters from John and me.

After lunch, I returned early, bypassing undergraduate students as they lined up to get into their one o'clock class. Dickinson and Stosick, talking together, got back somewhat before two o'clock. I heard loud and then soft exclamations from Dickinson. "*It's all gone*. Cracked. Hours ago, probably. I'll go call the army."

Dickinson told John and me, "They didn't anneal their glass bulb. It cracked and leaked. It's all gone."

Fear grabbed my throat and guts, my heart raced in irregular thumps. In my mind John was saying, "Colorless, odorless, and four times as poisonous as phosgene." Then I glanced quickly at the others. They showed no fear, and so I concentrated on not showing mine. I thought, "This is how soldiers can jump out of the trench together and rush at the enemy machine gun."

Dickinson conjectured that it probably had happened during lunch, because we hadn't heard anything. Furthermore, he said, the laboratory had excellent ventilation, air came in at the middle and went out the hoods, the S-10 had been close to the hood, and it probably had gone up the stack. Dickinson told John and me to go up to Gates and make another batch of S-10, while he and Art would call the army, return to clean up the laboratory, and carry on the tests John and I had started.

I recalled a preacher who had said he was once asked what he would do if told he would drop dead tomorrow, and he listed, one after the other, exactly those things he planned to do anyway. The preacher had said one should live each day as if it were the last. I silently noted that Professor Dickinson was advocating such a course for this day.

My thoughts continued along another path: Professor Dickinson had said that the ventilation system had carried it all up the hood, but I didn't believe it. Dickinson smoked much of the time and could blow elegant smoke rings. I had watched many a smoke ring distort, fold, stretch out, and drift around the lab until it became so dilute that it could no longer be followed. Thus, I knew that the laboratory air did not march from the fresh-air input to the hoods. It swirled and swooped all over the lab. And when the liquid-air fog had formed on the table just in front of the hood, the fog fell to the floor—it didn't go into the hood. As we walked through the hall toward Gates, I avoided looking directly at John and hoped John was not looking too closely at me. As we passed graduate-student friends in the hall, we did not speak to them. By the time we reached 121 Gates, the alarming fear of instant death had faded into a low-intensity, sad, sick feeling.

The fluorine maker was an awkward-looking machine. A large pot contained sodium fluoride

(NaF) and hydrofluoric acid. The pot had to be brought up to a moderately high temperature to melt the salt and acid mixture, and a direct electrical current was applied to form fluorine gas, F_2. The F_2 was diverted to a reaction cell that was heated on one end and cooled on the other. There the fluorine reacted with sulfur to make harmless SF_6 and highly poisonous S_2F_{10}. Finally, the freshly prepared S-10 was slowly vacuum distilled to remove trapped HF and SF_6. The process took three hours to complete, and then the system had to be cleaned up.

On the laboratory bench were two large open jars of a paste to be used if any HF got on a person's skin. HF causes deep ulcers that take a long time to heal. Safety called for rubber gloves and face shields, but operating the machine called for fast, free fingers and a clear sight of what went on. We wore goggles, but took our chances with bare fingers even though the machine sometimes spat out specks of hot sodium fluoride, smoking with HF. To see if the machine was making fluorine, John would pick up a small cotton ball with long metal tweezers, moisten it lightly on one side with alcohol, and hold it up to the exit. When the cotton ball caught on fire, it meant that fluorine was coming out.

We had been proceeding with this particular batch for a while when Professor Dickinson knocked on the door, then opened it with his key and asked how things were going.

John replied that there were no problems.

Later Dickinson checked on us again.

When we were done, John put the sample in a padded box, and we took it down to 65 Crellin.

I recalled from one of the hazards sheets that "the first symptom of acid poisoning of the lungs shows up as a bitter taste in the mouth when one smokes a cigarette." From the dense haze in the room it was clear that Stosick and Dickinson had been smoking heavily, and they continued to do so. Dickinson said, "We have had a long day, but we need to stick around until six at least. Have a seat, boys. If I had some beer I would offer it to you."

Then, uncharacteristically, he began to talk about himself.

I silently noted that somebody else had been thinking that afternoon. Reminiscences continued as did the smoking, with an occasional set of smoke rings. No one complained of a bitter taste. No one referred to why we were sitting around waiting, nor to any aspect of the S-10 spill.

Eventually, a telephone call came. Captain Everett, the army doctor, wanted to see me. He listened to my chest, front and back. "Lungs completely clear. No change at all. You and John were closest to the leaking bulb. Since you are clear, the others must be also. Tell me, where do you think that gas went?"

With a detached sense of relief I said, "Right up the hood, I guess."

Everett asked jovially, "Well, Hal, were you frightened today?"

"Part of the time."

The doctor chuckled, "Part of the time, eh. Well, I was frightened for the four of you all afternoon."

The army asked us to replace the S-10. Within a month, we did it. Professor Dickinson selected a Pyrex bulb, added attachments for conveniently filling and emptying it, then thoroughly annealed it. He packed the full bulb in an insulated wooden box made to his specifications by the carpenter shop. We were allowed to follow him to his automobile, but he would not let anyone go with him to deliver the material.

It was another month before the army got around to testing it. The explosion took place in a barn. The rats and goats died as expected, but the explosion produced a strong sulfurous odor, wiping out the gas's advantage of being odorless. Interest in S-10 ended after this test. Our regular work continued, and soon expanded to include the cyanides.

* * *

In July 1942, a group of university scientists who were working on chemical-warfare problems met in Evanston, Illinois; this group later became NDRC Divisions 9 (Chemistry) and 10 (Adsorbents and Aerosols). (By war's end, the NDRC would have 20 divisions; Division 10 alone had 77 projects going in 18 universities and five industrial laboratories.) Many potential chemical-warfare gases had already been identified, synthesized, tested for toxicity, and tested for how well gas masks stopped them. The group discussed what else should be done. Professor Yost spoke forcefully to the effect that, having solved our most urgent defensive problems, we should learn how to carry out offensive gas warfare if that was to our advantage. He added that we did not know how performance was affected by wind, temperature, time of day, cloudiness, and terrain. Professor Wendell Latimer of the University of California at Berkeley supported Yost's position, and it became the policy of Division 10, which was formed soon after.

Our first job was to learn something about meteorology and to design and build portable instruments to measure air temperature and wind speed from heights of zero to four meters. Such ground-level meteorology was called "micrometeorology." We were to obtain measurements near cities from the Mexican border to San Luis Obispo to see if there were any regions especially vulnerable to gas attack. Professor Latimer's group was to make similar measurements from San Luis Obispo to the Oregon border.

Measuring air temperature out of doors is not a trivial task. If a bare thermometer is placed in the

Safety called for rubber gloves and face shields, but operating the machine called for fast, free fingers and a clear sight of what went on.

air, it will absorb sunlight and read too high. If it is shaded from the sun, it will absorb infrared rays from the ground and again give a spurious temperature, too high or too low. If the thermometer is shaded above and below, the shades may absorb sunlight or ground heat and change the local air temperature. To obtain true air temperature, we mounted a small copper-constantan thermocouple within two concentric tubes of thin metal and rapidly aspirated air through the tubes and across the thermocouple. We built a mast of four-inch aluminum pipe in three detachable sections, affixing the thermocouple tubes at heights of one-third, one, two, and four meters. We used a direct-current motor powered by two storage batteries to drive a large vacuum cleaner that sucked air through the tubes and out the base of the mast. These masts were later manufactured by the Wheelco Instrument Company for other researchers.

To measure wind speed, we bought a set of commercial cup anemometers, but they gave results in terms of numbers on a dial. It was a nuisance to use a stopwatch to start and stop the anemometer readings, and then to subtract the numbers in a notebook. Professor Yost's group had the Caltech machine shop put three aluminum cups on a vertical axis that spun on a jewel mount, and at every complete round a small needle contacted a drop of mercury to give a brief electrical pulse. Art Stosick hooked together a set of vacuum tubes that counted the pulses to give a continuous recording of the wind speed. The anemometers were mounted on a mast of their own, at one- and two-meter heights. The Lane-Wells Company later manufactured 36 of these instruments for other groups.

The machine shop also built a few British "gustiness meters." This was a wind vane free to move up and down and left to right. It had a lightweight pen on its downwind side, which put ink marks on a sheet of curved paper. We let it operate for two minutes, and the pattern of scribbles indicated the degree of horizontal and vertical turbulence.

Otvos (on ladder) and Johnston set up the micro-meteorological gear near Rosamond Dry Lake in the Mojave Desert some time in 1943. The thermocouple mast and its suction system are visible behind Otvos' ladder. The center mast has two anemometers on it, and the mast in front of Johnston carries a British gustiness meter. Note the Buick in the background.

New cars were not available for nonwar work, but Dickinson was able to buy a new 1942 Buick station wagon for our project. It had real leather seats and real wooden panels on the outside. It was an impressive automobile. It had two fold-up seats between the front seat and regular back seat, so that the car could carry eight people. The back seat could be folded down to give a large storage area, and there was a storage rack on the roof. Our entire meteorological station could be dismantled and stored in the back of and on top of the Buick.

We measured local conditions at the beach, in a desert, on a site up the side of the mountain, and at various spots in the city. We quickly discovered what meteorologists already knew: during much of the year there was a pronounced breeze from the ocean to the interior during the day. The cold ocean air slips like a wedge below the warmer land air, thereby creating a large-scale temperature inversion that would trap poison gas or, nowadays, smog. Greater Los Angeles was especially vulnerable to gas attack.

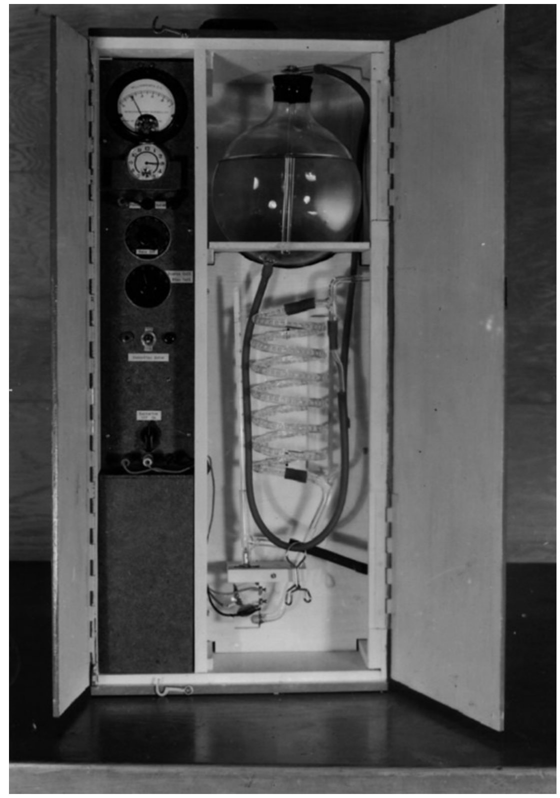
We could actually see these inversions on a small scale. We'd pack red phosphorus in cardboard cylinders about three centimeters in diameter and fifteen centimeters tall; when ignited by a thin magnesium ribbon as a fuse, each one produced a copious amount of smoke for a few minutes. During a temperature inversion, the smoke would boil up a few feet above the ground and then slowly spread gauze-like in broad flat sheets with little vertical mixing. We photographed these patterns of motion. Thus we had four different ways to characterize turbulence: temperature and wind speed profiles, the gustiness meter, and smoke patterns.

* * *

Professor Dickinson suggested we reduce the size and complexity of our gas-analyzing system so that it could be used in the field. In the laboratory, we had a 20-liter carboy of distilled water on a shelf. This water flowed by gravity along the inner surface of a half-meter-high burette and scrubbed soluble gases out of a counterflowing stream of air. The water then flowed through a cell that measured electrical resistance and recorded it on an Esterline Angus meter. It appeared to be a difficult job to miniaturize this system.

John Otvos did most of the work, but I made one contribution. I had grown up in rural northern Georgia, so Dickinson jokingly said I should use some feature of watering chickens to do our job. With that challenge, I played around with water and gas flows for a couple of days. I used a long, straight glass tube instead of a burette and attached an open T-tube at the upper end. I tilted the long tube slightly and let water slowly flow into the upper end. By controlling the tilt and by applying suitable constrictions to the incoming and

A red phosphorus flare going off in a stiff breeze at Rosamond Dry Lake, part of Edwards Air Force Base. In this vastness Yost's group could release moderately toxic sulfur-dioxide gas that they traced by chemical analyses, melding the dispersion data with Dickinson's micrometeorology.



Egbert. Outside air is drawn into the glass T at upper right of the glassware compartment by water flowing from the two-liter flask up top. As the string of bubbles proceeds down the spiral glass tube, any water-soluble war gas gets scrubbed out. This changes the water's conductivity, which is measured through the wire leads that are connected to the glass cell at the bottom.

outgoing flow of water, I could get air to enter the T and be picked up by the down-flowing water. With more adjustments I could get the incoming air to break up into round bubbles, and the flow down the tube looked like a string of pearls. The flowing water sucked in outside air, and the bubbles allowed any soluble gases to dissolve. This eliminated the need for an air pump, and replaced the heavy carboy with a compact two-liter bulb.

John replaced the straight tube with a spiral. He and Art Stosick designed and had the shop build a miniature resistance-measuring cell. The woodworking shop built a box that held the spiral tube in one compartment and the electronics, which Art developed, in another. A dry-cell battery provided enough power to run the machine for several days. John named the new machine "Egbert." More than 100 Egberts were eventually manufactured by the Caltech wood, glass, and machine shops, and they saw widespread use at several NDRC test sites.

* * *

In 1943 the United States and its allies began the difficult task of retaking islands in the southwest Pacific from the Japanese. These tropical jungles were, of course, different from the deserts of California and Utah and the pine forests of Idaho, where the NDRC and the Chemical Warfare Service (CWS) had tested chemical weapons and studied the travel of gas clouds. A 50,000-acre site in central Florida was chosen—the Withlacoochee Development Project of the Soil Conservation Service, U.S. Department of Agriculture. That November, Dickinson, Otvos, new recruit Bob Mills [MS '48], and I, along with Phil Hayward [PhD '49], postdoc Mike Kraus, and Ted Gilman [MS '46] from Yost's lab, packed 30 Egberts and assorted other gear into the Buick and a two-ton Ford flatbed truck and drove cross-country to Bushnell, Florida, about nine miles from the test site. There we joined civilians from other universities and a CWS detachment from the Dugway Proving Grounds in Utah. The commanding officer was Captain Jake Nolen, who was in his early 30s and had a PhD in chemical engineering from MIT. The NDRC group and the army officers lived in rented rooms, apartments, and houses in Bushnell. We had our meals at the local restaurant except during field tests, when we got in line and ate army chow. (After two years in California, I had come to dislike much Southern cooking, where vegetables were typically boiled for hours in the presence of salted fat hog-belly. I sometimes shook a heavy dose of pepper at spots in the food—under lifted portions of the mashed potatoes, and in turnip greens and meat—and then spun the plate. I would never know as I took a bite whether it would taste like black pepper or hog fat. That provided variety.) The enlisted men lived in tents in a flat area just south of town.

At the test site, soldiers marked the ground with whitewash to show where the bomb would be located and where they would place goats. We defined a grid around the bomb where the Egberts and the hot-wire anemometers from Berkeley would be placed. We set up one micrometeorolog-

ical station in rough meadow terrain, and another in a grove of tall trees, which towered high above our thermocouple mast. For six weeks, we had three to five tests every week. The gases included phosgene, hydrogen cyanide (HCN), and cyanogen chloride (ClCN). Immediately after the bomb or bombs exploded, we put on gas masks and went in to check the instruments. Then, after a prescribed time, we'd return and turn off the recording meters, label and remove the chart paper, tightly wrap the instruments with waterproof canvas, and place them on the truck. Any malfunctioning instrument was brought back to town for overnight repairs. In one experiment, an air-dropped bomb made a direct hit on one of the Egberts.

NDRC scientists measured the travel of the war gases by concentric circles of automatic chemical samplers. Military personnel staked out goats on a grid of their own. The goat detail was handled by an old-time major from the chemical-warfare corps. After one large test, the chemists said that no significant amount of gas got beyond the 100-yard circle. But two goats on the 200-yard circle died shortly after being brought back to their pens. There was a conference to decide why we had the discrepancy between methods. The chemists defended their instruments. The major suggested that the gases might have passed the 100-yard circle in a highly dilute state and then come back together to be concentrated enough to kill the goats on the 200-yard line.

I naively exclaimed, "On the basis of the second law of thermodynamics, I can say for sure that is impossible."

"Well, I don't know nothing about the second law of ther-mo-dy-namics, but I do know a dead goat when I see one." The major laughed heavily and was joined by some others.

I naively exclaimed, "On the basis of the second law of thermodynamics, I can say for sure that is impossible."

"Well, I don't know nothing about the second law of ther-mo-dy-namics, but I do know a dead goat when I see one." The major laughed heavily and was joined by some others.

Captain Nolen probed further: "Tell me, Major, sir, how many goats were on that 200-yard circle?"

"Eight," said the major confidently.

"How many died, sir, after they were brought back to their pens?"

"Two," was the reply. "Two of them died during that night."

"How many goats are in the pen that have not been exposed yet, sir?" Captain Nolen asked.

"How many do we have, Sergeant?" the major asked his assistant.

"They's only fifteen left now, sir. We're getting sort of low on goats," the sergeant said.

Captain Nolen, who signed every purchase order, including those for goats, pushed on: "Sir, how many goats have died in the last few days that were *not* exposed in the field?"

"Why, none, I don't think," the major exclaimed. "We haven't had any of them die recently, have we Sergeant?"

"Sir, the last few days we've had a right smart of Texas fever in them there goats," the sergeant said, nervously glancing back and forth between the captain and the major.

Captain Nolen pursued the point directly with

Opposite: The NDRC Division 10 group on San José Island in August 1944. Back row, from left: George Doyle (MS '48); George Cleland (PhD '51); Jim Pitts, of Northwestern; Ted Gilman (MS '46); John Thomas, from UC Berkeley; Lewis McCarty, University of Rochester; and Bill Roake, of Northwestern. Middle row: Bob Brinton, of Northwestern; Bill Shand (PhD '46); Bob Mills (MS '48); Phil Hayward (PhD '49); Pat O'Conner, from the University of Illinois; Clive Countryman, from Berkeley; Chet O'Konski and J. M. Thomas, both from Northwestern. Front row: R. J. Grabenstetter and Dave Volman, of Northwestern; Dickinson; Francis Blacet, the UCLA professor in charge of the group; Bill Gwinn, of Berkeley, the second in command; Jack Roof, of Northwestern; Otvos; and Caltech postdoc Mike Kraus.

the sergeant: "How many goats left in the pen died since the test four days ago?"

"They's been six of 'em died from Texas fever these last four days," the sergeant said.

Captain Nolen spelled out the conclusion: "Six out of fifteen died from Texas fever in four days, and they never got near the field. That's even more than two out of eight on the 200-yard circle. I think, sir, they probably died of Texas fever too."

The second law of thermodynamics was saved.

* * *

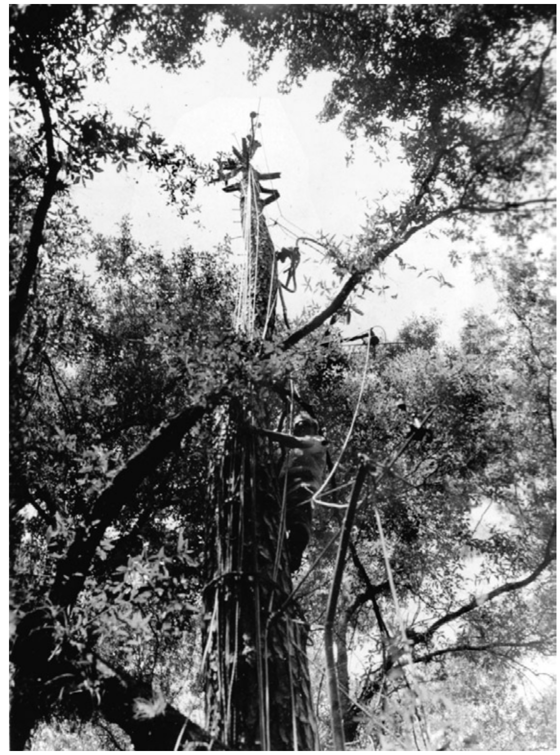
It was soon concluded that the semitropical forests of Florida were not a satisfactory substitute for the jungles of the southwest Pacific, and in early 1944 the Division 10 work was moved to San José, an uninhabited island off the Pacific coast of Panama. I remained in Bushnell, where the work switched to "persistent" gases, such as mustard, under Division 9. The persistent-gas group were mostly organic chemists, under the direction of Carl Niemann, another Caltech professor.

At room temperature, mustard is a heavy, oily liquid, but it has some vapor associated with it. Mustard vapor, if breathed, destroys lung functions in a manner similar to and more severe than phosgene. A person wearing a gas mask can still be killed by vapor absorption through the skin. A droplet of mustard is irreversibly absorbed in less than a minute, producing large, crippling blisters. Heavy contact of the liquid on skin can be rapidly fatal. Contaminated ground or logs remained dangerous for weeks or more, as certain poachers discovered.

Since our observations only went up a few meters, while the forest canopy was about 20 meters high, I asked Captain Nolen if we could obtain a steel tower that would go up above the treetops. He said we had been assigned one, but it had been erected in Dade City and there was no chance of our getting another. I had noticed some tall pine trees that went above the canopy of the hardwood forest—could we strip one of them and put in steel climbing rods like those on telephone poles? A suitable one was found about half a mile from the test area, and in short order it was instrumented.

Under clear skies, our meadow station recorded strong temperature inversions at night, and during the day showed an "unstable lapse rate," that is, the air temperature decreased with height. This is convection at work—air cools as it rises—and had been well documented over grass lawns, cow pastures, barren hillsides, and desert soils. After we converted the tree into a micrometeorological tower, I wanted to make measurements there 24 hours a day, whether tests were being conducted or not. I was curious about the magnitude and timing of temperature inversions in the forest and of the relation between meadow and forest. I made arrangements with Captain Nolen to have recharged batteries delivered twice a day at both the meadow and forest stations.

We recorded continuous measurements through-



The micrometeorological tree tower, deep in the forest some 100 yards off the main road into the test area, had vacuum-aspirated temperature sensors at 0.3, 5, 10, 15, 20, and 25 meters above the ground; anemometers at 2, 5, 10, 15, and 23 meters; and a wind vane at 25 meters. All the instruments were mounted at least a meter away from the tree trunk on stubs of branches or on steel rods. The trunk itself was festooned with wires and vacuum lines leading to a small shelter on the ground that held the industrial vacuum cleaner, the two heavy-duty truck batteries that powered it, and the recording and calibration equipment. Note the soldier on the trunk about one-third of the way up from the bottom of the photo.

Micrometeorological tests resumed at Rosamond Dry Lake in the late spring of 1945, using the new gustiness meter. Here Mills sets out an anemometer early in the morning.

Observations were made at three locations: on a smooth, flat, vegetation-free surface; on a surface of cracked dry clay with upturned edges about an inch high; and in desert brush. Mills and Johnston commuted between the stations on a bicycle, and in the interim between the two camping trips, they built a sail for it. Here Johnston sails across the cracked clay.

out February and March 1944, when there were few leaves on the deciduous trees, and May and June, the time of maximum foliage density. The temperature differences between air in the forest canopy and the air above the canopy showed the same pattern as the meadow: inversion during the night and unstable lapse rates through the day. But from 0.3 to 15 meters above the ground surface, the patterns were totally different. During May and June the forest air showed a temperature inversion every hour of the day and night. During February and March, air in the forest had an unstable lapse rate from about 1130 to 1530 Eastern War Time, and a temperature inversion at all other times. The daytime inversion in the forest was a mystery.

We explained it as follows: sunlight absorbed by the leaves warmed canopy air to temperatures higher than the air above to produce an unstable lapse rate above the canopy during the day, as in the meadow. This sun-heated air in the canopy was warmer than the shaded air in the lower forest, which constituted the daytime inversion. During the early morning and late afternoon, foliage cut out almost all of the slanting sunlight, and the inversion was large. When the sun was nearly overhead, some sunlight penetrated to the ground to reduce or break up the temperature inversion during the middle of the day. We concluded that the denser the canopy, the greater the inversion, which would probably be highly important in tropical jungles. Poison gas clouds would persist much longer there than in open areas.

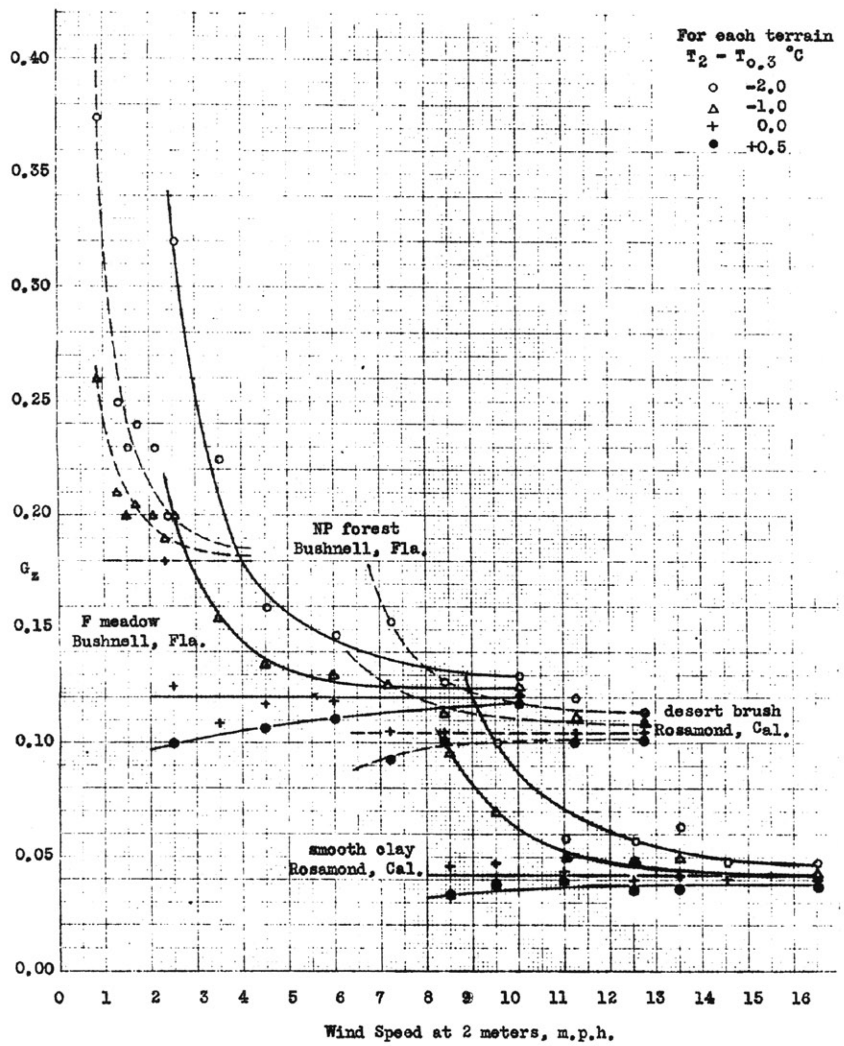
We ran into one difficulty with our overnight recordings. Well into our program, we found one or more of the ink traces would go dry in the early evening. We became doubly careful to fill the ink reservoirs, but we still lost some records. I drove out one night, brought along a strong flashlight, and set up a chair near the recorders. For a few hours I heard owls and other night noises, and then I heard a rasping sound in our box of meters. I turned on the flashlight and found the biggest cockroach I had ever seen. It was as big as a cigar and about that color, five or six inches long, and when I opened the case and tried to catch it, it flew away on big whirring wings. Later, someone told me it was a "blabberous cockroach." The next day my sergeant took a soldier skilled in carpentry to the site. He installed two tight wood-and-screen doors, and stopped the other possible entries with putty. He did a good job, because we had no more trouble with ink-guzzling cockroaches.

* * *

Contour maps of the spread of mustard-gas clouds correlated best with the gustiness-meter readings. But after a two-minute exposure, the center of the paper was coated by a coil of overlapping red ink tracks. We measured the widths of the "average extreme excursions," but it was difficult to say what the record meant. We needed a quantitative measure.

I designed a two-vaned instrument that measured vertical gustiness. One vane aligned the system with the wind; the other, streamlined to give a laminar flow around it, swiveled up and down on high-quality jewel mounts. The tangent of the angle of the vane from the horizontal plane was a measure of the instantaneous gustiness. Electrical contact was maintained with the vane, and the data could be read off a milliammeter or recorded on chart paper. The machine shop at Caltech built several copies of our new meter.

The British had developed a statistical diffusion theory to predict the three-dimensional dissipation of a gas cloud downwind of an explosion. Their



This plot from the Bushnell project's final report shows vertical gustiness (G_z) versus wind speed at two meters off the ground for four different types of terrain. Each line represents the temperature difference between 2.0 meters and 0.3 meters above the ground: open circles are a difference of -2 °C and triangles are -1 °C (unstable lapse), crosses are 0° (thermally neutral) and solid circles are 0.5° (inversion). The thermally neutral lines are horizontal—the gustiness does not change, regardess of the wind speed. This is the "mechanical gustiness," which is caused by frictionally induced turbulence and is thus related to the terrain's roughness. The other component of vertical gustiness is convection-driven, which is greatest at low wind speeds. As the wind picks up, the convection cells are distorted and eventually destroyed, and the gustiness approaches the thermally neutral value.

spreading parameter, *n*, determined how strongly the dosage near the source decreased with distance, and we felt that should depend strongly on vertical turbulence. The British had found that the "R-value," the ratio of wind velocity at heights of two meters and one meter above the ground, was a good measure of vertical turbulence over smooth lawns, but we found it was not good in forests or rough meadows.

Bob Mills returned to Caltech from Panama in September 1944 and worked on Professor of Chemistry Linus Pauling [PhD '25] and Senior Research Fellow in Structural Chemistry Robert Corey's rocket-propellant project. In the late spring of 1945, I visited Pasadena. Bob and I borrowed a Caltech panel truck, and on two trips we camped out at Rosamond Dry Lake for several days, making measurements with our new gustiness meter and other instruments. It was windy day and night, especially in the afternoon, and we noted that our eight-foot ladder blew over when the wind reached 25 miles per hour. We slept outside on army cots. The truck was tall enough for us to stand up in, and we used it for our kitchen.

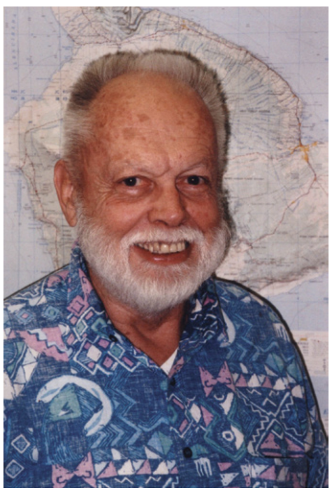
We were able to verify that the vertical gustiness correlated inversely with the spread of a gas cloud as it moved downwind. Furthermore, plotting the gustiness versus wind speed revealed a parameter, which we called "mechanical gustiness," in air of zero temperature gradient, i.e., where the temperature remained constant regardless of height. This parameter was the same for all wind speeds but different for each class of terrain: largest in the forest and jungle, much less in rough, bushy meadow, somewhat less again in desert brush, and very low on the dry lake bed. In other words, it measured the site's roughness.

Robert Merrill of the University of Chicago, Bob Mills, and I wrote this up in our project report in the summer of 1945. It was submitted to Major Nolen (he had been promoted) just after the atomic bombings of Hiroshima and Nagasaki that August. The report was classified Confidential, filed away, and forgotten.

So was it worth it? Professor Dickinson was a brilliant scientist, an artist, and a generous, liberal person. He felt good about working with terrible poisons to provide better gas masks. When the NDRC shifted from defense to offense, he supported the new emphasis, accepting that a recognizable capability to go on the offense was a necessary part of defense. Early in 1942, he said to me, "We are guarding a bridge that may never be attacked; we hope it will not be. If it is not attacked, our work has succeeded." □

"He had the extraordinary ability to grasp the essence of a physical problem and come up with a simple, cheap, and very effective way to study it."

James A. Westphal
1930 – 2004



Jim Westphal, professor of planetary science, emeritus, whose originality and instinct for designing instruments were legendary, died September 8 at his home in Altadena. He was 74.

Born in Dubuque, Iowa, Westphal spent his early years in Tulsa, Oklahoma, where his accountant father leased a filling station during the Depression. During the war, they moved to his grandparents' farm in the small mountain hamlet of Petit Jean, Arkansas, where Westphal attended school in nearby Morrilton, not much larger. For high school, his parents sent him off to relatives in Little Rock, where he discovered in the library a book on amateur telescope making. He and a friend built an eight-inch telescope and put it on top of the bandstand tower at Little Rock High School. It was the beginning of a long and distinguished career.

Westphal returned to Tulsa and pumped gas for awhile, but, as he recalled in his oral history for the Caltech Archives, he was "hell-bent to go to college." An uncle got him a more lucrative job with a seismic exploration crew, and in a year and a half he had saved enough money to enter the University of Tulsa, where he earned his BS in physics in 1954, continuing to work summers in oil exploration. He never thought that working his way through school was a bad thing at all: "I took a lot of pride in the fact that I could do this all myself."

After graduation, he stayed on with the Seismograph Service Corporation, dropping instruments down into Mexican oil wells for a year, and then joined Sinclair Oil Company's new research lab in Tulsa, where he hung out with other amateur astronomers and with fellow members of the Society of Exploration Geophysicists. It was to this body that C. Hewitt Dix, professor of geophysics at Caltech and considered the father of exploration geophysics, gave a talk in Houston in 1960 on using seismic waves to measure the distance to the bottom of the Earth's crust. Westphal's reaction was: "I bet we can do some of that stuff," and he promptly figured out a simple and clever way of communicating between the seismic waves and the recording equipment. Dix, who could spot talent when he saw it, was impressed and, after spending a summer with Westphal in Tulsa, invited him to Caltech to build an instrument for analog Fourier analysis. Westphal took a four-month leave of absence from his oil job and arrived in Pasadena in January 1961.

Dix also urged him to take a course in applied mechanics from Julius Miklowitz. "He gave me a D+, and I was the happiest man on the face of the Earth." Westphal decided against further education, but meanwhile, his reputation for making clever devices spread quickly beyond geophysics. His scuba-diving pictures, made with a strobe camera he invented, caught the attention of the late Heinz Lowenstam (professor of paleoecology), who used them in his studies of undersea life. (Westphal also figured out that the excessive mortality in Lowenstam's aquariums was due to brass hinges.) Bruce Murray, then a research fellow in space science, wanted to know if astronauts landing on the moon would sink over their heads in a deep layer of dust, and Westphal had an idea how to find out. "A lot of things were happening, and it was all just tremendous," remembered Westphal in the oral history interview. "I was having the time of my life."

Toward the end of the visit, one day Westphal was in Dix's lab "building stuff" when someone came up behind him. "I turned around and said, 'Can I help you?' And he

Westphal works in his lab in 1974.

said, 'Are you worth a shit?' And I said, 'Pardon me?' And he said, 'Do you do good stuff?' And I said, 'Well, some people think I do.' And he said. 'Oh.' And turned around and walked out." It was Jerry Wasserburg (now the MacArthur Professor of Geology and Geophysics, Emeritus). A short 10 minutes later, Bob Sharp, then the division chair of geology, called Westphal in and said, "We want you to stay," offering him a better salary as a "research engineer" than he had been making in the oil business. When he asked what his job was, Sharp told him it was "to decrease research resistance around here." Westphal sold his Tulsa home and never looked back.

Westphal's wide-ranging inventiveness and interests found a fertile field at Caltech, and his background proved surprisingly applicable. For Murray's question, to prove that the astronauts would step out onto luna firma, he rigged up an infrared photometer, similar to one he had built at Sinclair to look for pipeline leaks. For Lowenstam (who also had a background in oil exploration) he built an aquarium that simulated the pressures at various ocean depths (Westphal's experience with oil pumps came in handy). And he developed a Schlieren technique for photographing how water flowed through Lowenstam's ancient shellfish; it was published in *Science* in 1965.

He digitized a quasar spectrum for Maarten Schmidt, now the Moseley Professor of Astronomy, Emeritus, which was published as "Some Astronomical Applications of Cross-Correlation Techniques" in 1965. This was, Westphal said, a technique used in the oil industry all the time: "I was flabbergasted that these people didn't know all about this."

But Westphal didn't just build instruments; he was actively involved in the science that his instruments made possible, particularly in astronomy and planetary science. In the '60s, he published several papers on infrared observations with Bruce Murray (later director of JPL and now professor of planetary science and geology, emeritus) and Gerry Neugebauer, the Millikan Professor of Physics, Emeritus. Over his career he wrote 133 scientific papers.

"What I liked most about Jim," says Neugebauer, "was how much he enjoyed science. He did fun stuff superbly and was interested in the way things really worked. Whatever he did, he saw new applications in totally different fields and was not afraid to try out a new technique or idea."

Murray considered him the "cleverest instrumentalist (if I can create a word) I ever knew at either Caltech or JPL. He had the extraordinary ability to grasp the essence of a physical problem and come up with a simple, cheap, and very effective way to study it."

While working with Barclay Kamb trying to measure the thickness of the Blue Glacier, in Washington, Westphal jury-rigged an oscilloscope with a camera on it to lower into the glacier. He wrote it up as a paper, "In Situ Acoustic Attenuation Measurements in Glacial Ice," which Caltech decided to consider his "dissertation." In 1966 he was promoted to senior research fellow. The next thing he knew, he had been made an associate professor of planetary science in 1971, and by 1976 he was a full professor with tenure—sans PhD.

Rich Terrile (MS '73, PhD '78), now a planetary scientist at JPL, was Westphal's first graduate student. He said (in a profile of Westphal in *Caltech News*, 1995) "It's a real credit to Caltech that they'd recognize intellectual brilliance over academic credentials. . . . Jim is a genius."

Westphal didn't hesitate to apply his genius to practical ends. During the energy crisis in 1973, he suggested removing one fluorescent tube out of each pair in every light fixture on campus and replacing it with a capacitor. He first tried it out in the business services building, coming in on a Sunday and replacing half the tubes with "phantoms." No one noticed. Caltech implemented this scheme campus-wide and cut lighting expenses almost in half. This led to a patent on the energy-saving phantoms and a $10,000 national award for Westphal.

But astronomy was to be his ultimate destiny. He developed silicon sensing devices for Palomar, the first application of such devices in astronomy. When, in 1973, he and astronomer Jerry Kristian fitted his Silicon Intensified Target onto the prime focus of the 200-inch Hale Telescope, Kristian couldn't identify the star field and questioned, "You got this damn telescope pointed right?" Then they suddenly realized that they were seeing stars that were too faint to be picked up on photographic plates. They were seeing "deeper in the sky than anybody had ever seen before," said Westphal in his oral history.

That exhilarating experience made him a convert to silicon. When he got wind of the new silicon detectors (CCDs: charge-coupled devices) being developed at JPL for the Galileo mission to Jupiter, he and Jim Gunn (then at Caltech, now professor of astrophysics at Princeton) desperately wanted "to get our hands on some of those things" for the 200-inch, correctly calculating that CCDs were "going to wipe out every other detector astronomers use" and revolutionize astronomy.

CCDs were at the heart of what was to become Westphal's most widely known achievement—the Wide Field and Planetary Camera on the Hubble Space Telescope. Initially, he was not enthusiastic about working with NASA. When Gunn suggested that they make a proposal to build the instrument, Westphal told him (as quoted in *E&S,* Summer 1990): "You're out of your mind. Neither one of us works in that world; we don't want to spend our time up there dealing with that bureaucracy and counting beans and making viewgraph presentations and not being allowed to make marks on a blackboard and all that sort of stuff."

With Sue Kieffer, Westphal prepares to lower his camera into the steaming mouth of Old Faithful and (below) watches the launch of the Hubble Space Telescope with the late Clair Patterson, professor of geochemistry, emeritus.

Kicking and screaming, he was dragged into the project; Gunn even persuaded him to be the principal investigator of the WF/PC, pronounced "Wiffpick," to be built at JPL. (Gunn and Westphal tested the Wiffpick's design on an instrument for the 200-inch: the "four-shooter," which used similar optics and arrays.)

"The facility with which Jim negotiated the NASA bureaucracy during the construction of WF/PC was astonishing," says Gunn. "I think they had never quite run into anyone like him before; they wanted badly to dismiss him because of his background and lack of pedigree, but were burned almost instantly because of his awesome intellect and deep understanding of essentially all the problems that interested and confronted them. It was impossible not to like Jim, and NASA managed both to do that and have a healthy respect (read fear) of him."

Although the culture and style of a government lab was not to Westphal's liking, to say the least (and vice versa: there were times, said Westphal, when he thought the NASA people would gladly deport all the scientists to Chile and leave them there), the perfect instrument that was born of that collaboration became a hero of our time. Not only did Wiffpick diagnose the spherical aberration in Hubble's mirror, but Wiffpick II was designed as a corrective lens to salvage the near-disaster and made possible Hubble's glorious images of space that have been such a scientific and popular success.

For his 17 years' work (it was supposed to be four) on the Space Telescope, Westphal and his team earned 320 hours of observation time and billions of frequent-flier miles. He and Gunn also achieved their original goal of fitting CCD detectors to the 200-inch Hale Telescope—and ultimately to most of the major ground-based observatories in the U.S. and Chile. Nowadays, the technology is as close to hand as the ubiquitous digital camera.

When he was informed, by phone, in 1991 that he had won a MacArthur "genius" award, he replied "something unprintable, something that meant 'no kidding,'" and then, sensing it wasn't a hoax, "started apologizing all over myself for what I had said." He later "realized it gave me a strange sense of freedom," even though he had always thought that he had all the freedom anyone could ask for. (He once wrote of himself in a short biographical sketch: "His job is his hobby is his job.") After hiding out for awhile, avoiding all committees, he settled on a project.

He had long been interested in the geophysics of volcanoes (after Mount St. Helens blew in 1980, he designed cheap, sacrificable tiltmeters encased in styrofoam pellets and mounted in plastic garbage cans to study its future rumblings.) Geysers were something similar, not to mention being related to his first career dropping instruments into holes in the ground. So Westphal used the MacArthur money to find out how geysers work. Collaborating with geologist Sue Kieffer (PhD '83), he got beautiful data on Old Faithful in Yellowstone National Park, but they were unable to come up with a model of what happens down in the geyser between eruptions. Finally he put a tiny video camera inside a vacuum-insulated housing to keep it cool in the boiling water and lowered it down the crack. The "gorgeous footage" that resulted showed that the pressure from large blasts of water coming in from side vents and fissures was forcing the superheated water up the cylindrical pipe and out the top.

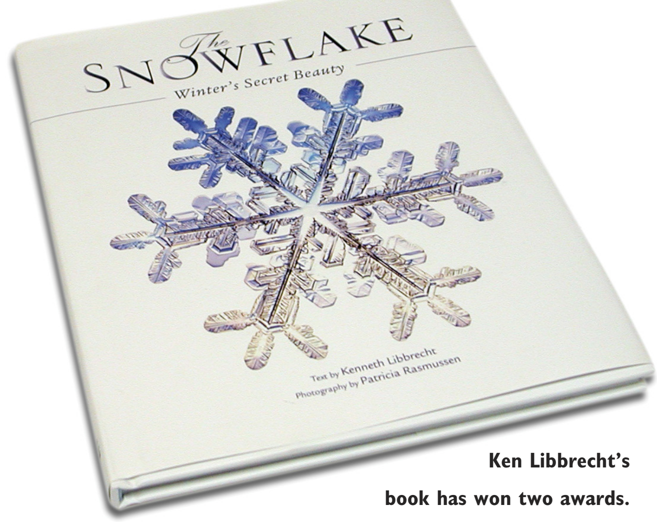
In 1994 Westphal was asked to take on the directorship of Palomar Observatory. He considered it a "service job. It's a job to keep the trains running on time," with no academic responsibility. He agreed with great reluctance to serve for three years, because of his love for the great telescope. The most vexing problem of his tenure was hiring cooks (he finally hired the local Hare Krishnas), and it was with great relief that he relinquished the job to Wallace Sargent, the Bowen Professor of Astronomy, in 1997, before taking emeritus status the following year. He remained active on campus, still "building stuff," until about a year ago, when a degenerative neurological disease finally made it impossible to continue. But he faced that, too, with characteristic good nature and good sense.

"I think," says Gunn, "the most far-reaching thing I learned from Jim, who was also one of the most fearless people I have ever known, was not to be afraid of anything technical just because of ignorance of the subject or device or any preconception about the difficulty of the task. It is quite OK to deem a task impossible (and he did a very few) but not without knowing how hard it really is."

Terrile, his former graduate student, remembers many nights getting to know Westphal inside the east arm of the Hale Telescope, where "he not only taught me about astronomy and science, but also about more down-to-earth topics like self-reliance, dealing with people, and how to keep focused when things go bad. Jim had a wonderful way of reducing a problem to its most basic form. He said, 'There are always two ways to deal with a problem: You can get angry and upset and then try and fix it, or you can just fix it. Which way would you rather work on it?'"

Westphal is survived by his wife, Jean; a son, Andrew; two stepdaughters, Robin Stroll and Susan Stroll; and two granddaughters. A memorial service will be held December 9 at 4:00 p.m. in Dabney Lounge. □ *—JD*

Faculty File



Ken Libbrecht's book has won two awards.

Honors and Awards

Michael Alvarez, professor of political science, has been selected by the board of editors of *Scientific American* magazine for inclusion in the third annual Scientific American 50, which honors 50 individuals, teams, companies, and other organizations, whose accomplishments in research, business, or policy making during 2003–2004 demonstrate outstanding technological leadership.

Alexei Borodin, professor of mathematics, received the Prize of the Moscow Mathematical Society for 2003, and **Vadim Kaloshin**, associate professor of mathematics, received the Prize of the Moscow Mathematical Society for 2002.

Clive Dickinson, postdoctoral scholar in astronomy, is the 2004 winner of the Michael Penston Astronomy Prize, given by The Royal Astronomical Society for the best astronomy thesis in the United Kingdom. Dickinson earned his PhD from the University of Manchester and came to Caltech this year.

Charles Elachi, professor of electrical engineering and planetary science, and director of JPL, has received the NASA Outstanding Leadership Medal for "outstanding leadership of the Jet Propulsion Laboratory, whose legacy of excellence in planetary exploration continues with the awe-inspiring Spirit and Opportunity missions to Mars."

Ali Hajimiri, associate professor of electrical engineering, has been named to the 2004 list of the world's 100 Top Young Innovators by MIT's *Technology Review*. Nominees are recognized for their contributions in transforming the nature of technology and business in industries such as biotechnology, medicine, computing, and nanotechnology.

Babak Hassibi, associate professor of electrical engineering, has received a Presidential Early Career Award for Scientists and Engineers for his "fundamental contributions to the theory and design of data transmission and reception schemes." The award "recognizes outstanding young scientists and engineers who, early in their careers, show exceptional potential for leadership at the frontiers of knowledge," and provides five years of grant support.

Leroy Hood, BS '60, PhD '68, visiting associate in biology, became the sixth recipient of the annual Biotechnology Heritage Award at the BIO 2004 Annual International Convention, held June 6–9 in San Francisco. Hood is the cofounder and president of the Institute for Systems Biology, as well as the cofounder of Amgen, Applied Biosystems, and other biotechnology companies.

Ken Libbrecht, BS '80, professor of physics and executive officer for physics, has received a 2004 Benjamin Franklin Award for his book *The Snowflake: Winter's Secret Beauty*. The awards recognize excellence in independent publishing, and Libbrecht's book was honored in the Science/Environment category. The book also won the nature and environment category of the 2004 National Outdoor Book Awards.

Robert Phillips, professor of mechanical engineering and applied physics, has been named by the National Institutes of Health (NIH) as one of nine recipients of the first annual Director's Pioneer Award. The award will provide Phillips, an authority on the nanoscale mechanics of biological systems, with $2.5 million for the next five years as part of the NIH's new "Roadmap for Medical Research" program.

Re'em Sari, associate professor of astrophysics and planetary science, has been awarded a David and Lucile Packard Fellowship for Science and Engineering. Paid over a five-year period, the fellowships were established in 1988 by the David and Lucile Packard Foundation "to allow the nation's most promising professors to pursue science and engineering research early in their careers with few funding restrictions and limited paperwork requirements."

John Schwarz, the Brown Professor of Theoretical Physics, was selected to deliver the keynote speech for the opening ceremony of the Center for Mathematics and Theoretical Physics at the Shanghai Institute for Advanced Studies (administered by the University of Science and Technology of China), which took place July 30–31; the honor included the granting of an honorary professorship.

Mark Simons, associate professor of geophysics, and **Brian Stolz**, assistant professor of chemistry, have received Presidential Early Career Awards for Scientists and Engineers. The award "recognizes outstanding young scientists and engineers who, early in their careers, show exceptional potential for leadership at the frontiers of knowledge," and provides five years of grant support.

Kip Thorne, the Feynman Professor of Theoretical Physics, was named in June as recipient of the 2004 GSC (Graduate Student Council) Mentoring Award. **Wilhelm Schlag**, professor of mathematics, received the GSC Teaching Award. The award for teaching assistant went to **Francesco Ciucci**, a graduate student in mechanical engineering.

Alexander Varshavsky, the Smits Professor of Cell Biology, has been named corecipient of the Protein Society's 2005 Stein and Moore Award. Presented annually, the award recognizes the "revolutionary work" of Varshavsky and Avram Hershko, Distinguished Professor at the Technion—Israel Institute of Technology, "in discovering the ubiquitin system of protein degradation, its mechanisms, and its significance to living cells."

Theodore Yao-Tsu Wu, PhD '52, professor of engineering science, emeritus, was awarded the American Society of Civil Engineers' 2004 Theodore von Kármán Medal on June 15 at the annual Engineering Mechanics Division Conference. He also received the American Society of Mechanical Engineers' 2004 Lifetime Achievement Award on June 22 at the International Conference on Offshore Mechanics and Arctic Engineering.

Yuk Yung, professor of planetary science, was awarded one of three NASA Exceptional Scientific Achievement Medals, given for "original thinking that has contributed to our knowledge about the Earth and the solar system through basic scientific research and developing new approaches for scientific study." Yung studies the chemistry of planetary atmospheres. □

FARLEY NEW CHAIR OF GPS

Ken Farley

On September 1, Kenneth Farley, the W. M. Keck Foundation Professor of Geochemistry took over as chair of the Division of Geological and Planetary Sciences after Ed Stolper's 10-year stint. Hailed as a "young, dynamic scientist" by George Rossman, professor of mineralogy and leader of the search committee, and as someone who is highly respected by his colleagues for his integrity and conviction by provost Paul Jennings, Farley says he relishes the chance to work with the faculty to find out what their problems are and help them to succeed. He will also be overseeing some major new initiatives in the division, including a new Center for Plate Boundary Studies, a project to locate extrasolar planets, and a Computational Infrastructure for Geodynamics initiative, as well as the move of the environmental scientists to Robinson when the astronomers move to their new building in 2008.

Farley earned his BS at Yale in 1986, and his PhD at the Scripps Institute of Oceanography, UC San Diego in1991, before joining the Caltech faculty in 1993 as an assistant professor of geochemistry. He was appointed professor of geochemistry in 1998 and received his named chair last year.

His research on noble gases—gases such as helium, neon, argon, krypton, and xenon that don't form chemical bonds with other elements—has taken him as far afield as Tibet and Robinson Crusoe Island off Chile in search of clues to the evolution of the Earth's interior and atmosphere. Although his duties as division chair mean he will have to scale back on his research, he plans to keep his program going, and will even continue to teach. Perhaps he will also still find time to run off the frustrations of the week in running marathons and beyond (ultramarathons). While in Tibet, he ran over the Taggalu pass, 17,000 feet above sea level. □
—BE

Dabney Hall Complete!

Above: Restored tile work and wrought iron painted in the original colors now adorn Dabney Hall's east facade.

Top, right: At the rededication, Jean Ensminger and David Baltimore ceremoniously open the gates on the building's west door.

Caltech celebrated the completion of the Dabney Hall restoration project with a special rededication ceremony on September 17. The first capital project of the "There's only one. Caltech" campaign, the one-year renovation project not only restores Dabney Hall to its early grandeur, but also reestablishes this historic building as home to Caltech's humanities faculty.

Approximately 300 Caltech friends and faculty attended the special event, which opened with remarks from Caltech's president David Baltimore and Jean Ensminger, chair of the Division of the Humanities and Social Sciences. These renovations "not only help reclaim the beauty of an extraordinary building [but also] invigorate the humanities for students through space enlivened by study, research, lectures, and performance" said Baltimore.

A generous distribution from the estate of George F. Smith (BS '44, MS '48, PhD '52), who was a life member and past president of the Associates, funded a matching challenge to all of the group's members that resulted in naming the building's connecting garden area the "Garden of Associates." Under the leadership of current president Ted Jenkins (BS '65, MS '66) and the Associates campaign committee, the organization reached its $250,000 goal in October to meet the challenge and fulfill the $1-million naming opportunity.

Other gifts toward the $12 million project have funded the third floor faculty lounge, named for the late Roger Stanton, professor of English until 1966, thanks to the generosity of Capt. Tyler Matthew (BS '39); the third floor loggia, given by Marjorie and Roger Davisson (BS '65, MS '66) in recognition of past and present humanities faculty; and an office space, named for benefactors William F. Horton (BS '46, MS '48) and Glenna Berry-Horton. Several other naming gifts—such as the library, main lounge and north patio—are still being sought in the fund-raising effort.

Dabney Hall of the Humanities is one of Caltech's oldest and most distinguished buildings. The changing needs of the Institute over the years resulted in numerous modifications to the hall's initial architectural layout to create additional administrative workspace. The renovation, however, has restored many architecturally significant elements of the original building while creating new instructional facilities and offices designed to reunite the humanities faculty and encourage collaborative scholarship. Significant features of the project include a restored and technologically updated library, recovery of the original "Treasure Room" for meeting space, an updated communications infrastructure, and an elevator for improved access for all members of the Caltech family.

Dabney Hall of the Humanities was named for Joseph B. Dabney, former Caltech Trustee and one of the Associates' earliest members, in recognition of his $250,000 gift to the Institute in 1927. □ — *Vannessa Dodson*

For more information on this and other campaign priorities, please contact:

California Institute of Technology
Development and Alumni Relations
Mail Code 5-32
Pasadena, California 91125
Phone: 1-877-CALTECH
http://www.one.caltech.edu

Engineering & Science

California Institute of Technology
Pasadena, California 91125