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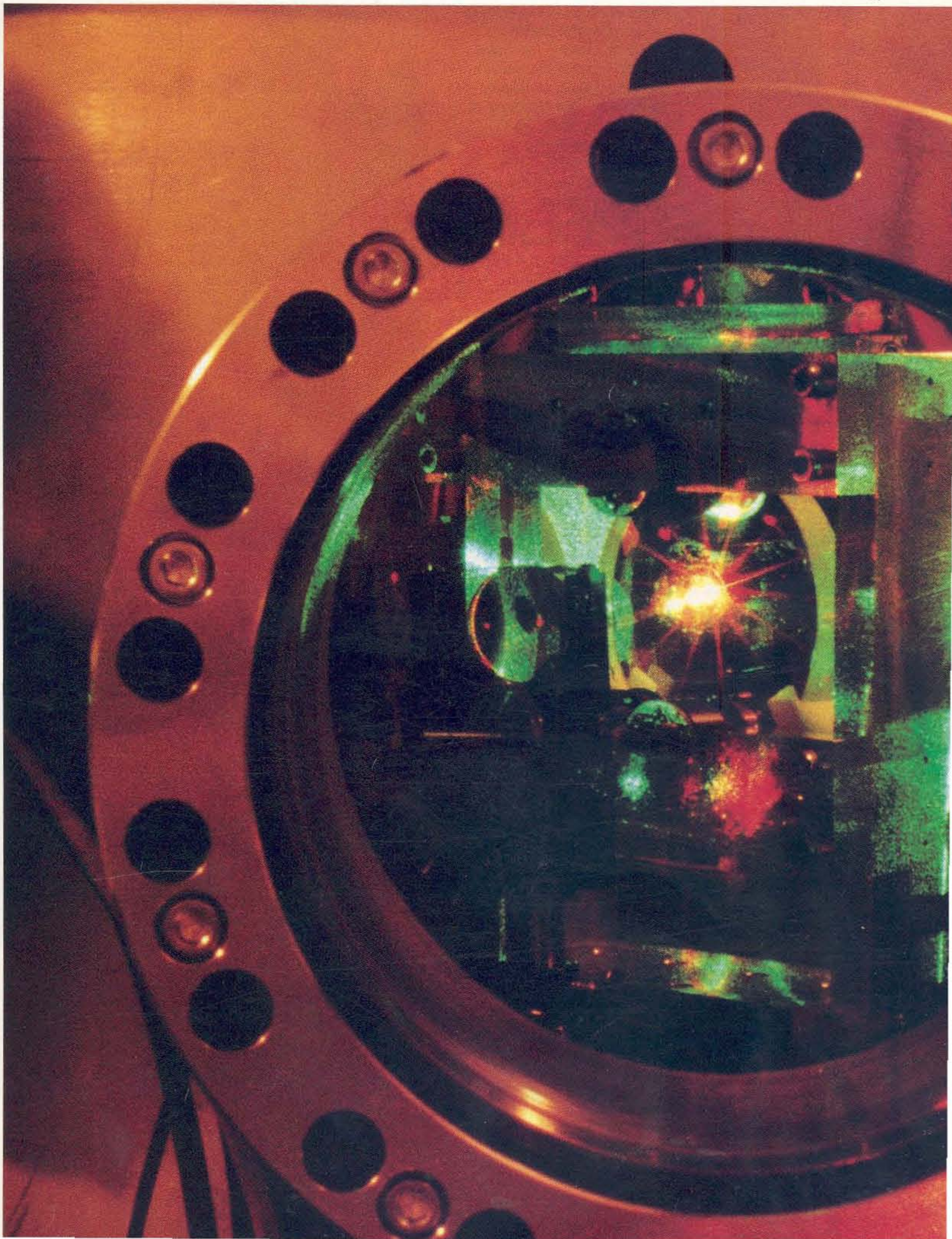
Engineering & Science

Summer 1991



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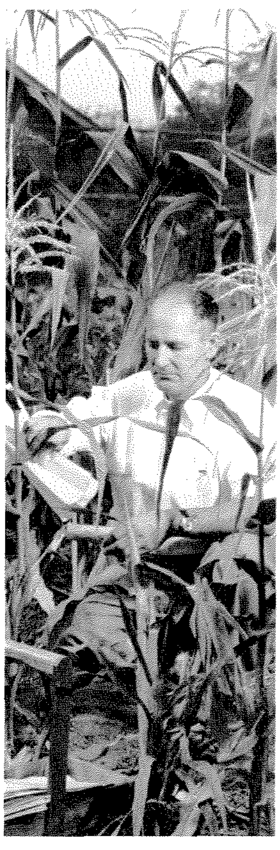
In Caltech's 100 years of existence both Thomas Hunt Morgan (right) and George Beadle (below) were giants in the history of Caltech's biology division and in the history of 20th-century genetics. Morgan founded the division, and Beadle succeeded him as chairman in 1946. Morgan won the Nobel Prize in 1933 and Beadle in 1958.



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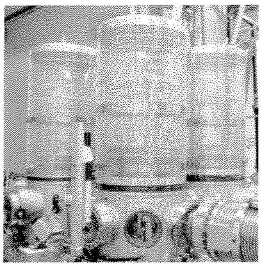

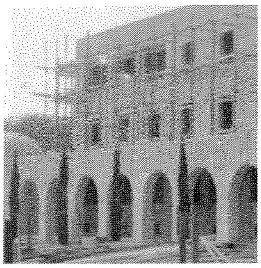

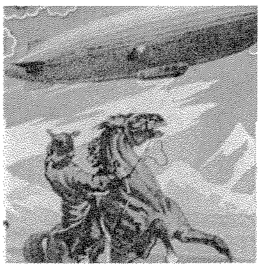

On the cover: This beam splitter—the circular plate with the starburst on it—is the heart of a prototype detector for the gravitational waves predicted by Einstein, but as yet undiscovered. The green light comes from a laser that measures the waves, while the red light comes from another laser that controls the beam splitter's position. The story of the full-size detector, which Caltech and MIT hope to build, begins on page 2.

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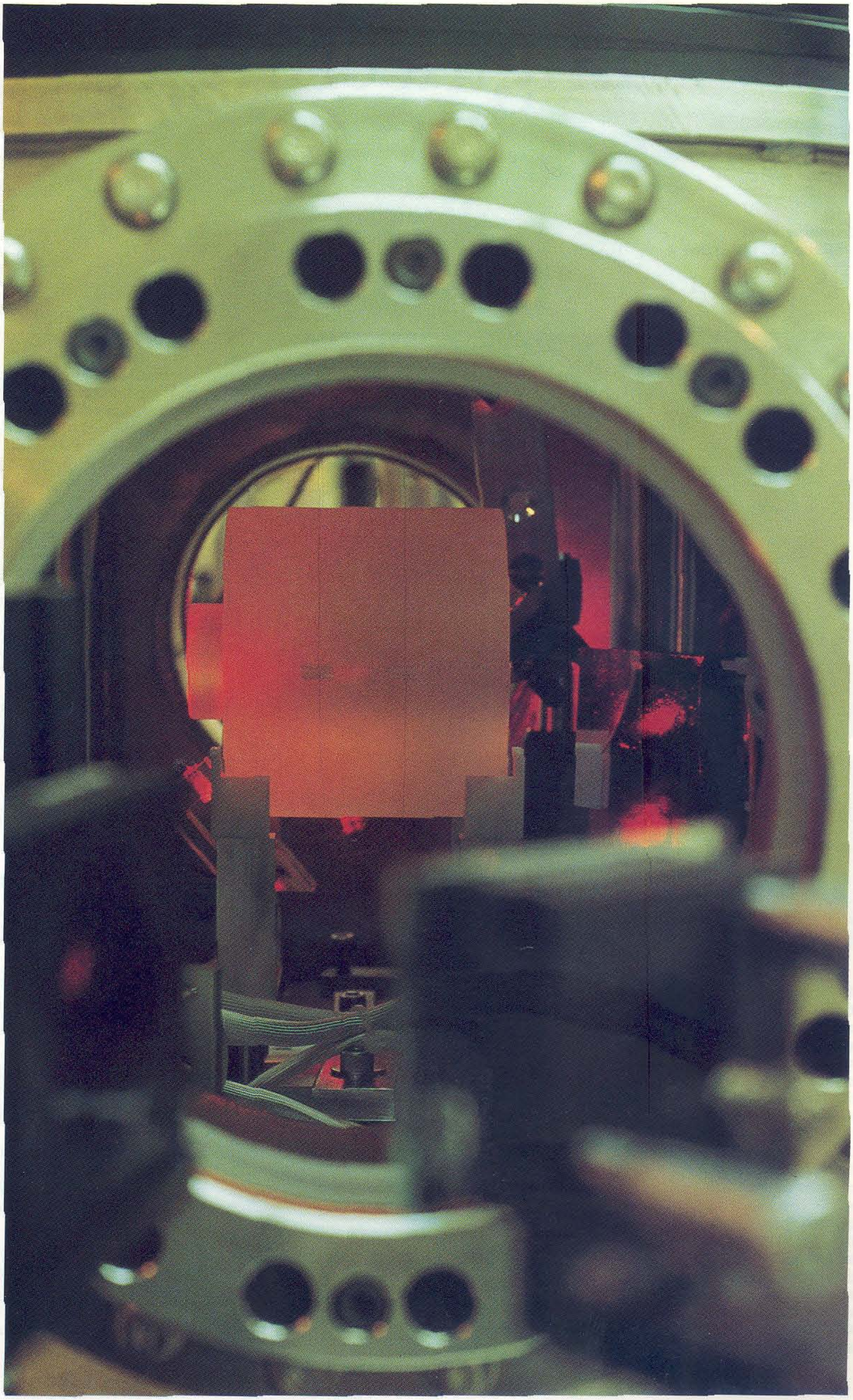


Lasers, Mirrors, and Gravitational Waves

These waves, which bear tidings of cataclysms like the deaths of stars and the collisions of black holes, have so far escaped our view.

by Frederick J. Raab

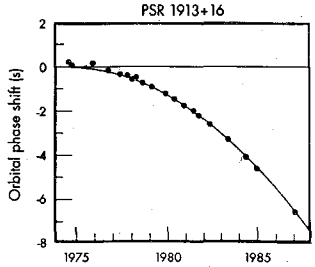
A three-pound test mass of fused silica—glass—hangs in a vacuum chamber from wires three-thousandths of an inch thick. The wires can be seen as fine vertical lines that divide the mass into thirds. The mass is a four-inch-diameter horizontal cylinder; the mirror is visible on its left side. The ruby suffusion bathing the mass comes from a secondary laser system, part of a servomechanism that keeps the mass in alignment. The mass hangs above, but does not touch, the legs visible in front of it. These legs prevent it from swinging wildly in the event of an earthquake or other sudden jolt that could damage the system.

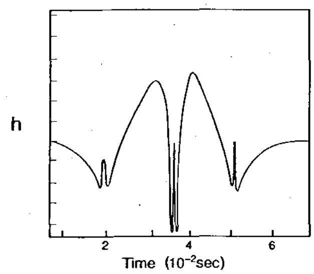
We know of four fundamental forces in the universe. Two of them, called the "strong" and the "weak" interactions, are the stuff of particle physics, and are very short-range forces. There are also two long-range forces, electromagnetism and gravity. The electromagnetic force can propagate as waves in an electromagnetic field, which we perceive as photons of light or as radio waves. Einstein's theory of general relativity predicts analogous gravitational waves in the gravitational field. In the language of relativity, the fabric of space and time will be distorted in the vicinity of a massive object. Picture Earth as floating on the surface of a pond that represents space-time, and putting a little dimple—its gravitational field—on the water's surface. The dimple, in turn, "attracts" nearby objects whose motion is affected by the dimple's shape. If Earth made violent motions, it would create ripples that would propagate to the far edges of the pond. In this metaphor, the ripples are gravitational waves.

Both electromagnetic and gravitational waves carry information about the physical processes that created them. Astronomy has principally been concerned with interpreting the electromagnetic spectrum, which is imprinted by things that happen in the atmospheres of stars, in gas clouds, and in other places where photons are created or scattered. But gravitational waves are created by the bulk motions of matter. These waves, which bear tidings of cataclysms like the deaths of stars and the collisions of black holes, have so far escaped our view. Astrophysicists and other people interested in such violent events would like to be able to detect these waves and read the stories written on them.

To this end, we hope to start building LIGO—the Laser Interferometer Gravitational Wave Observatory—in the next few years. LIGO funding has been proposed in President Bush's budget for 1992, which is pending before Congress. LIGO is a Caltech–MIT joint project, directed by Rochus Vogt, Avery Distinguished Service Professor and professor of physics; and including Stan Whitcomb as deputy director; Ronald Drever, professor of physics; Kip Thorne, Kenan Professor and professor of theoretical physics; me; and Rainer Weiss, professor of physics at MIT, plus an excellent technical staff of engineers, physicists, and students at both institutions. A LIGO facility—there will be two of them—consists of a central building containing lasers and detectors, plus two four-foot-diameter vacuum pipes that stretch approximately two and a half miles (four kilometers) from the central building to form an "L." These pipes carry laser beams that sense the separation between test masses—20-pound masses at first, to be replaced by one-ton masses later on—hanging in the central building and in buildings at the far end of each arm. We hope that by monitoring their separation very carefully, we will see them move infinitesimally as gravitational waves pass.

There would be two such detectors, operating in concert, on opposite sides of the United States. We need at least two well-separated stations to know when we have seen a gravitational wave, as opposed to some local disturbance. A real wave would trigger both detectors within one-sixtieth of a second of each other. We can use this differ-





Top: PSR 1913+16's gradually decaying orbit has offered the first indirect evidence for the existence of gravitational waves. [Taylor and Weisberg, 1989]
Bottom: A collapsing supernova's gravitational wave might look like this ("h" is the fractional change in separation between test masses the wave causes). The complex shape arises because different parts of the core collapse and rebound sequentially. The collapse begins along the star's rotational axis, where material isn't supported by centrifugal force. [Saenz and Shapiro, 1978]

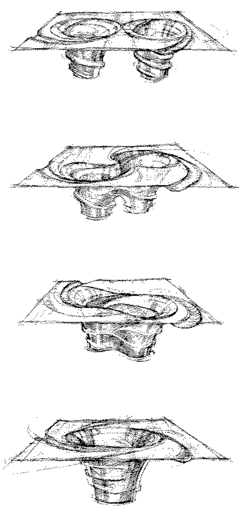
ence in arrival times to determine where in the sky the source is. Unfortunately, having two detectors merely specifies the source as being within a ring of sky. Three detectors would cover most of the sky unambiguously, and four would cover the whole sky. The Europeans, Australians, and Japanese are considering building similar detectors, so eventually there should be an international network, of which LIGO will be a part.

Sanduleak was a very interesting star, although no one knew it then, because it had died 160,000 years ago. That night, the signal from its death reached Earth.

You may be asking yourself, "It's pretty easy to believe in electromagnetic waves, what with TV and all, but should I buy any of this stuff about gravitational waves? Do they really exist?" Let me show you a system whose gravitational radiation can be calculated rather accurately. . . a very simple system consisting of two masses—two stars orbiting each other. If Einstein is right, gravitational radiation would carry energy away from the system, so that the orbits would gradually decay and the stars would spiral in toward each other. Around 1975, science got lucky—although luck is always due to very good work—and Joseph Taylor and his colleagues discovered a very interesting object, called PSR 1913+16, that consists of two neutron stars orbiting each other. (Neutron stars are several-mile-diameter balls of essentially pure neutrons. They're the compressed remnants of stars that have long since died.) One of the pair is a pulsar that sends out a narrow beam of radio waves. As the pulsar spins on its axis, the beam sweeps across Earth like a lighthouse beacon, and one can measure the interval between flashes with exquisite precision. From these data, one can derive the orbit's phase shift. That is, one can measure the interval between successive times when the two stars have a given alignment as seen from Earth, and see if there is any drift in that interval. In the topmost graph above, the horizontal line at zero seconds is what would be observed if there were no energy loss from the orbiting pair, and certainly the data points don't agree with it. The curved line is what we would expect if the energy in the orbit was being lost—as general relativity predicts—by the radiation of gravitational waves. In fact, Taylor and his colleagues have measured a drift of about eight seconds over the years. You can see how well the data fit that curve, so it's a fairly safe bet that we've already seen the result of gravitational radiation in the orbital decay of this particular pulsar.

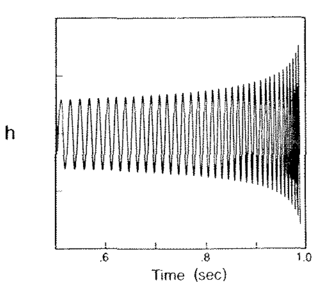
What kinds of events might one study using gravitational waves? One promising event is the supernova—the death throes of a large star. For instance, on February 23, 1987, astronomers Robert McNaught and Ian Shelton were separately photographing a region of sky that happened to contain a star called Sanduleak. Sanduleak was a very interesting star, although no one knew it then, because it had died 160,000 years ago. That night, the signal from its death reached Earth and was recorded on film—the famous Supernova 1987a—the brightest one seen from Earth in 300–400 years.

Now, normally a star burns nuclear matter by fusion—the star's tremendous gravity squeezes atoms together until they fuse—gradually turning the star's hydrogen and helium into the elements that we know and love and are made of. But at some point the fuel runs out, and the star's



Above: Two black holes orbiting each other eventually coalesce into one.

Below: In their final *moments as separate* entities, just before each one's immense gravity starts to disrupt the other, the merging black holes should produce this gravitational-wave signature.



nuclear engine can no longer provide the outward pressure needed to keep the star from collapsing under its own enormous weight. A star the size of our sun will just fizzle out. Much more massive stars undergo a gravitational collapse that is one of the truly spectacular events in the universe. The star's iron core, which is about the mass of our sun, collapses to a few miles in diameter in less than a second. The collapse releases subatomic particles called neutrinos. These interact fairly weakly with matter, but enough so that it takes them a few seconds to boil out through the star, and in the process they deposit enough energy in the star's outer layers to blow them apart. The supernova that appears on photographic plates is a record of the photons released from this explosion. This visible display begins to appear hours later, when the photons finally escape. The material gets dispersed through the heavens, and it later forms planets and people. This is where the stuff that we're all made of comes from.

One can watch the supernova's light show with all the tools for detecting electromagnetic radiation—optical telescopes, radio telescopes, gamma-ray detectors, and so on. (In the case of Supernova 1987a, the neutrinos were detected as well.) One then practices forensic medicine, examining the corpse of the star for clues about the manner of its demise.

But if a gravitational-wave detector catches a supernova in the act, we can glimpse the inner workings of a collapsing star. The waves released as the core collapses and rebounds pass right through the star's outer layers as if they weren't there. Even after the collapse's waves are long gone, the object left behind may still emit gravitational waves. This object may be a neutron star or a black hole. It can remain hidden from our electromagnetic view for years by the veil of exploded stellar material, or it may fail to emit electromagnetic waves.

Another thing we can do with a supernova is determine whether gravitational waves travel at the speed of light, or merely very close to it. This question is related to whether the graviton—the hypothetical object that "carries" gravitation the way photons carry light—has mass or not. The fact that gravity is a long-range force is interpreted to mean that the graviton is massless, like the photon. But we don't know absolutely for sure that the graviton has exactly zero mass. It could have a very small mass, which would mean that the range of gravity isn't infinite after all, but merely very long. We could answer this question by racing gravitons against something whose mass is known, like photons, because the maximum propagation velocity of an object is related to its mass.

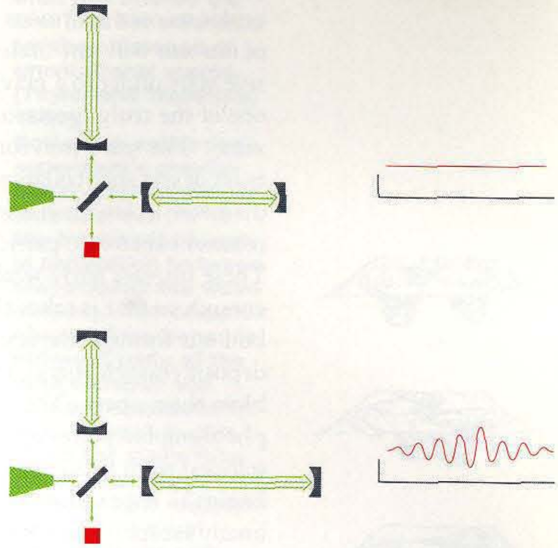
This has already been done with the neutrinos and photons emitted from Supernova 1987a, which had to travel 160,000 years to get here because Sanduleak was 160,000 light-years from Earth. It's like watching sprinters. Carl Lewis will always beat me in a foot race, even if I jump the gun a bit, as long as my head start is small compared to the length of the racecourse. Similarly, photons will always beat massive neutrinos if the race is long enough. By timing when the photons and neutrinos from a supernova arrive at Earth, we get an upper limit on the speed and mass of the neutrino relative to the photon, even though the neutrinos get a head start. This has confirmed that the neutrino's mass is less than about 20 electron volts. Once gravitational waves have been detected, we can use a similar technique to actually measure their speed and thus test the theoretical underpinnings of relativity.

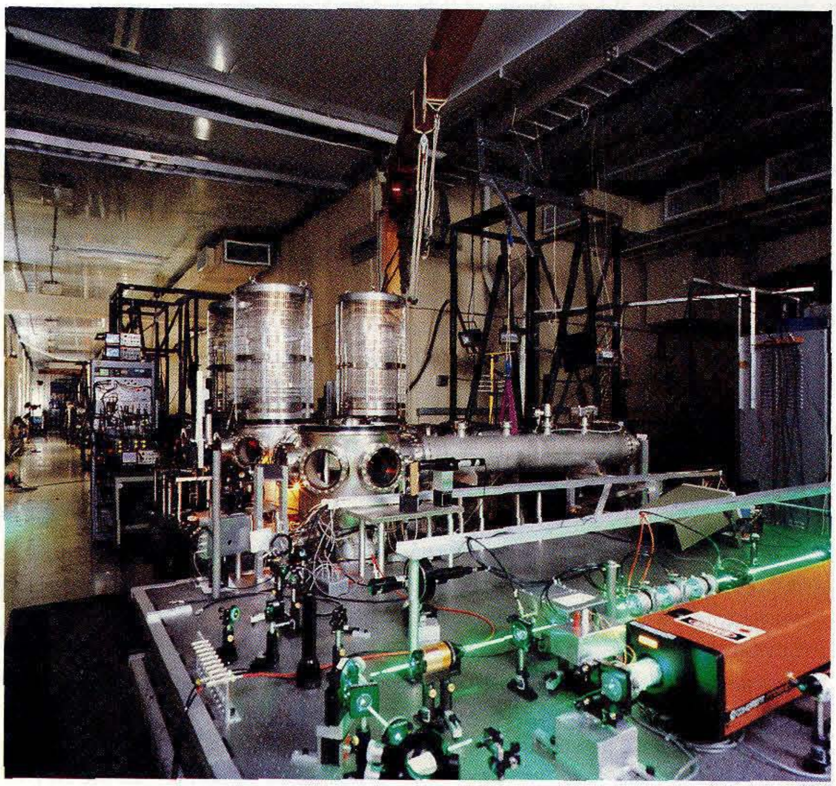
We can also look for binary systems made up of two black holes. (Black holes are regions of gravity so strong that even light can't escape.) We hope to see the waves emitted when two black holes capture each other. Again, their orbits gradually decay through the emission of gravitational radiation until the black holes become so close that they start to disrupt each other tidally. We can predict what the waveforms will look like before the tidal distortions start. Life becomes much more complicated for theorists after that, but, with the help of supercomputers, they will be able to calculate the details of the death spiral. These waveforms could be used as a signature—their detection would be proof that black holes actually exist.

We could look for all kinds of waveforms, in fact. Deciphering a waveform's details should tell us what created it. A supernova collapse and a black-hole binary coalescence should look very different. There are other things that we know should make gravitational waves, and there are probably just as many other things we can't even dream of yet. When detecting these waves becomes routine, we will have a powerful new tool for astronomy, comparable, perhaps, to the advent of radio astronomy, which made possible the discovery of objects like pulsars and neutron stars in the first place.

Clearly there's a lot to be gained from making the routine detection of gravitational waves a reality. The hard part is building detectors with the requisite sensitivity. The basic LIGO detector is a system of suspended masses that are free to move horizontally; when a gravitational wave comes by, it perturbs the distance between them

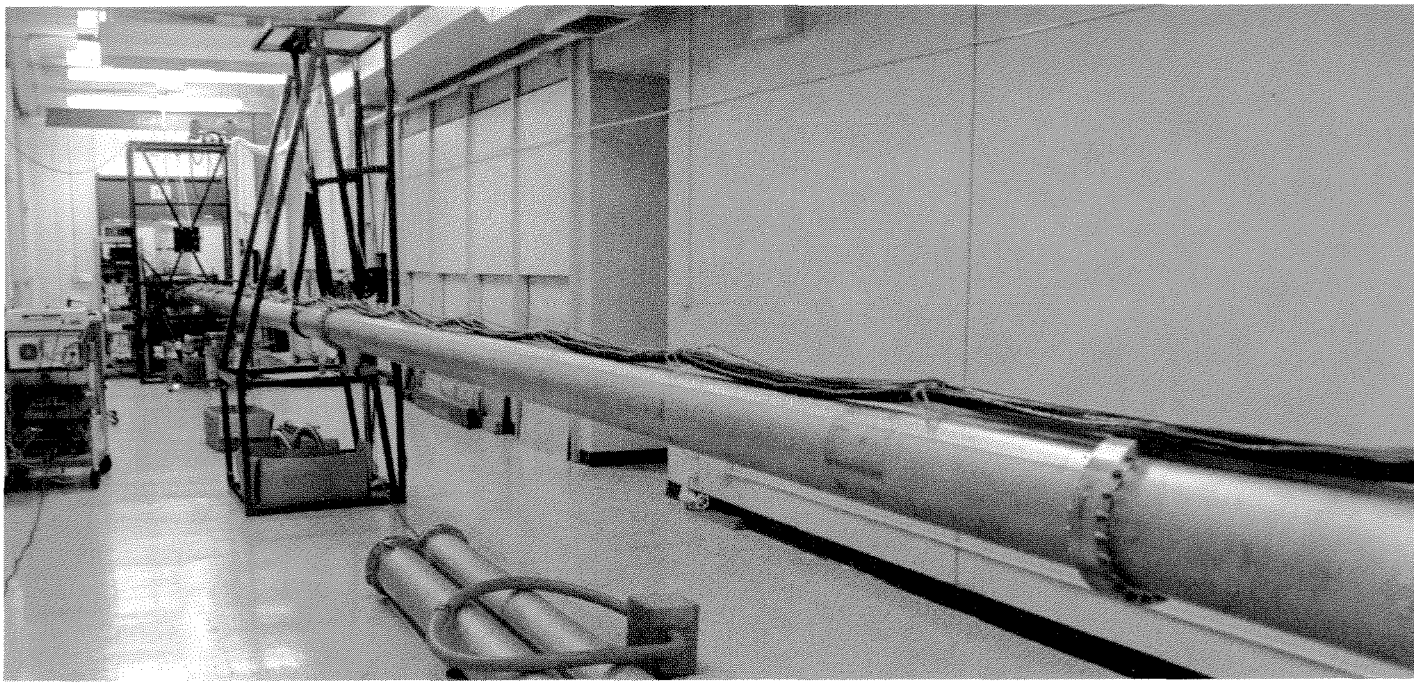
Right: How an interferometer works. When no gravitational waves are passing by (top), the distances between the two sets of mirrors are the same, the reflected beams cancel each other, and no light reaches the photodetector. When a wave comes by (bottom), it nudges one set of mirrors slightly apart and the other set slightly together. The beams no longer cancel, and the photodetector records a signal whose shape follows the wave. Below: The 40-meter prototype. The optical setup in the foreground filters and stabilizes the laser beam, which enters the interferometer via the right-hand end of the horizontal pipe. Of the three mesh cages, the middle one houses the beam splitter and the other two house test masses. Note the 6' 3"-Raab standing at the arm's far end.





as measured by a laser beam. Imagine sending the beam out from one mass, reflecting it off the other one, and measuring how long the trip took. Since we know the speed of light, we can figure out the distance. LIGO has two pairs of masses in perpendicular directions, forming the two arms of an "L." A passing gravitational wave will change the distance between the masses by some fraction of that distance. Think of the masses as being glued to a rubber sheet. If the sheet stretches in one direction, moving the masses apart, it will shrink in the other, moving the masses together. Thus we need only compare the lengths of the arms to detect the waves.

This comparison can be done by actually splitting the laser beam, sending half of it down each arm of the L, and then recombining the reflected beams by passing them through a device called a beam splitter. This setup makes use of something called interference—hence the word *interferometer* in the name LIGO. Light waves are just oscillations in the amplitude of an electric field, and the beam splitter adds the two recombining fields together. Normally, the crests of the field from one arm line up with the troughs of the field from the other arm—or if they don't, the masses can be moved slightly so that they do—and the beam splitter adds a plus field and a minus field to get zero. This is called destructive interference. No light reaches the photodetector. But if a gravitational wave makes one arm a little longer and the other a little shorter, then crests start to line up with crests, and troughs with troughs. Now when the beam splitter adds the waves, it gets a field with some amplitude. This



The view down the hall: looking along one of the 40-meter interferometer's arms toward the test-mass chamber at its far end. The photo was shot from about the arm's midpoint.

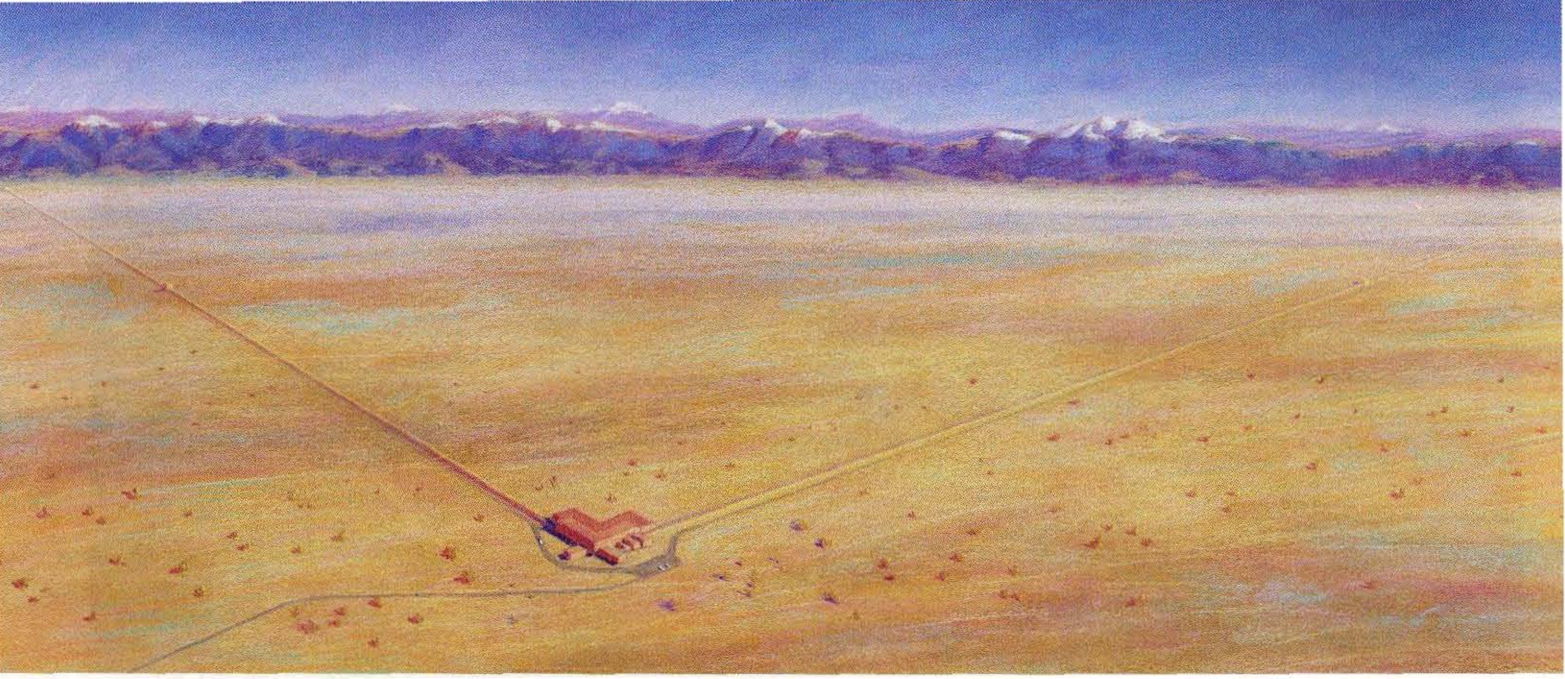
field hits the photodetector, which typically detects the square of the field, and we get a curve that follows the shape of the gravitational wave.

Our group has built a working prototype gravitational-wave detector. Its arms are 40 meters long, and it's housed in a prefab building wrapped around two sides of Caltech's Central Engineering Services building. In fact, we've prototyped almost all of the techniques I'll describe, in either this 40-meter system or in some other apparatus. When Ford builds a prototype vehicle, they don't do it because the CEO wants to drive to the store for a loaf of bread; they do it to find out how to build a better car. Similarly, our prototypes are not likely to detect gravitational waves; we use them to develop and test the technology that will eventually be incorporated into LIGO's detectors. This technology works on paper, but we want to see it work in the real world, and learn how to operate it. Our 40-meter prototype has masses suspended very much as they would be in LIGO. A number of optical schemes—I'll describe them presently—have also been prototyped. Since we don't need the suspended masses to test purely optical properties—just the detector's lasers, mirrors, and such—we build these kinds of prototypes on an ordinary optical bench.

I have one big problem. Remember the supernova, and the star's outer layers that the gravitational waves went right through? We're all made from that material, and so are any gravitational-wave detectors we might build. If the waves zipped through several times the mass of the sun with impunity, they sure won't move any test masses much. With LIGO as it is presently designed, and considering the sources we want to see, we expect fractional changes in the distance between the masses of about 3×10^{-22}. That is, the distance change between masses two and a half miles apart will be about one-thousandth of the diameter of an atomic nucleus. This could give you pause. It would give *me* pause, except that we're operating a prototype where we're close to detecting that small a change now. We've gotten down to 10^{-18}—about one-thirtieth of a nuclear diameter over a distance of 40 meters for an event that lasts for a few thousandths of a second. So if we make the distance between masses 100 times greater so that they would move 100 times farther in response to a passing wave (as planned for LIGO), and if we can improve the precision of our measurement of small displacements by a factor of 30 (which we think we can do), we can make this scheme work. It's a challenging program, but the technology to do it is within reach.

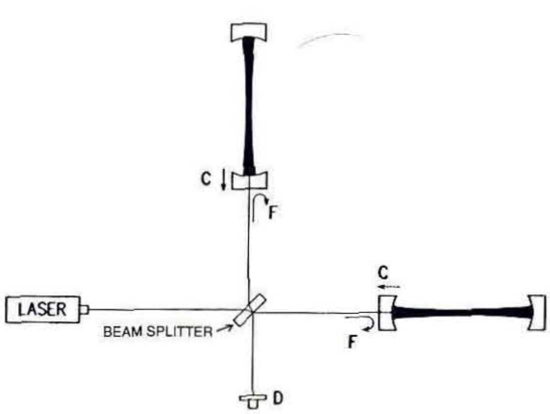
We would like to separate the masses as far as possible, because the masses' motion is proportional to their separation, but this isn't as easy as it sounds. With LIGO's four-kilometer arms, we're already running into problems with Earth's curvature, not to mention acquiring the real estate. Instead, we put mirrors at both ends of each arm to bounce the light back and forth many times, so that we get more signal for a given mass motion. The setup we use is called a Fabry–Perot cavity.

In each arm the front mirror—the mirror closest to the laser—is only partially reflective,



Above: How a real LIGO installation might look. The L-shaped building in the foreground houses the lasers, control equipment, offices, and such. The interferometer arms would be protected by concrete culverts.

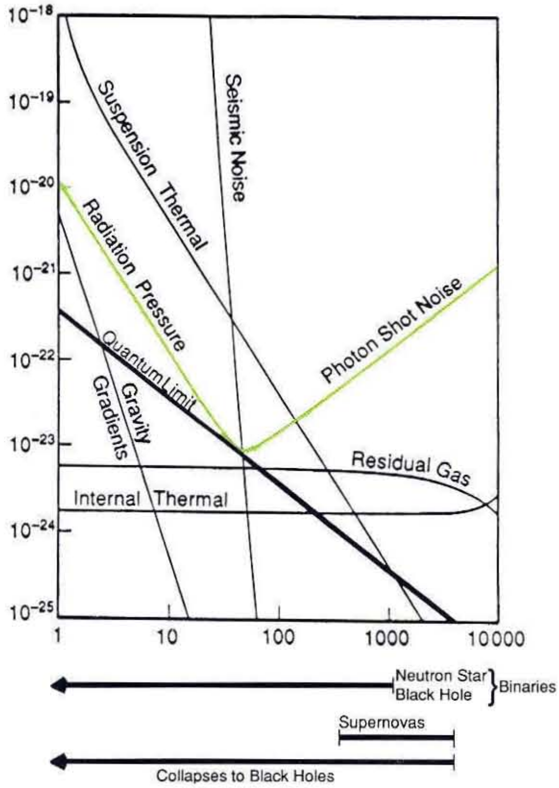
Below: Schematic diagram of an interferometer made from two Fabry-Perot cavities. "F" is the front field, "C" is the cavity field, and "D" is the photodetector.



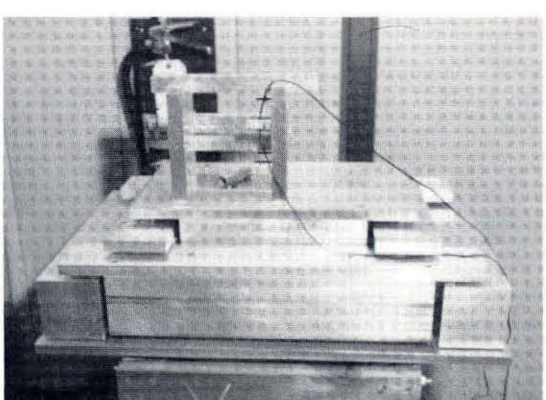
to let the light into the arm. However, some of the light bounces back toward the laser. Let's call the electric field associated with the light reflected from the front mirror "F." The light that enters the interferometer arm bounces off the second mirror and rattles around between the mirrors. But with each round trip, a little light leaks out of the cavity through the front mirror. I'll call the leaking field "C." Once again I can invoke interference—in fact, I'm going to invoke it frequently now. If I set the front and back mirrors at the right distance from each other, the crests of wave C and the troughs of wave F line up—destructive interference occurs—and behold, there's less light reflected from the front mirror back to the laser. More light goes into the arm, where we want it, instead. (This is how the antireflective coating on your camera lens works. A coating on the front surface of the lens gives an F reflection that cancels the C reflection from the lens itself. That's why you don't see your face very well when you look into your camera.) We typically bounce the light back and forth about 3,000 times in our prototype. This feat requires extremely high-quality mirrors; in the longer LIGO fewer bounces will be required.

There are various so-called noise sources that, depending on how cleverly we deal with them, can determine the smallest gravitational wave we can measure. (See graph, opposite.) These generally fall into two classes: noise sources that prevent the masses from hanging motionless when no waves are present, and sources that affect our ability to detect very small motions of the masses; these latter sources are known as sensing noises.

Right: Noise from many sources can hide a gravitational wave. The bars below the graph show the frequency ranges at which various types of sources are expected to emit gravitational waves. The vertical scale shows the smallest wave detectable in the presence of various noise sources. For a one-second-duration wave at a given frequency to be detectable, its amplitude at that frequency must fall on the highest line. (Longer-duration waves can be detected below that line as their signal accumulates.)



The evolution of a vibration-isolation stack. Clockwise from the top:
1. The first stacks installed in the 40-meter prototype had horseshoe-shaped slabs supported on erasers—a convenient source of uniform pieces of soft rubber.
2. A newer test setup uses pairs of bars stacked crosswise like the logs in a cabin. In the search for elastomers with better vacuum properties, the erasers have given way to silicon-rubber cubes.
3. The eraser and the cube.



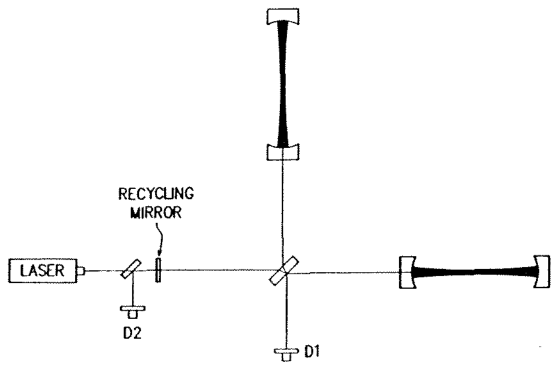
"Seismic noise," which includes the rumble of passing trucks as well as natural ground motions such as earthquakes, falls into the first class of noise sources. We can make the test masses very insensitive to seismic noise by the clever use of vibration-isolation systems. The basic principle is that a mechanical oscillator—a pendulum, or a mass supported by a spring—doesn't move much when its support is pushed by a force at a frequency higher than the oscillator's "fundamental resonance." (The fundamental resonance is the frequency at which the pendulum naturally swings or the mass naturally bounces.) Therefore, we hang each test mass like a pendulum, in slings of fine wire hanging from a frame that sits atop a stack of metal slabs. Each slab is separated from its fellows by springs of a resilient, rubbery elastomer not unlike the caulk around your bathtub. The springs isolate the upper slab and its test-mass cargo from the lower slab and its eventual connection to the outside world, much the way that your car's suspension isolates the chassis from the pounding of the tires on a bumpy road.

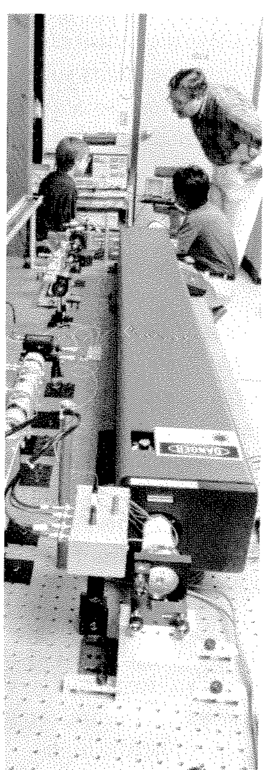
Since LIGO detectors will operate at room temperature, the mirrored surfaces of the test masses will move randomly in response to thermal excitation of the wire slings ("suspension thermal" on the graph) and internal vibrational modes of the test masses themselves ("internal thermal"). Seismic noise and thermal noise can be reduced by technological improvements such as the development of better vibration-isolation systems and higher-mechanical-quality materials. Unfortunately, gravity gradients caused by density fluctuations underground and in the atmosphere cause motions in the test masses that can't be shielded, and will limit the low-frequency sensitivity of Earth-based detectors.

A familiar example of a sensing noise is the apparent motion of distant, stationary objects on a hot day, due to density fluctuations in the intervening atmosphere. The space between LIGO's test masses must be evacuated to a pressure low enough that the density fluctuations in the residual gas left in the pipe ("residual gas noise") don't become confused with actual test-mass motion. Once again, this isn't as easy as it seems, because stainless steel absorbs hydrogen gas during the manufacturing process. This gas could leak out of the steel, and into our vacuum system, for years. Other projects—particle accelerators, for example—that use steel piping to hold a high-purity vacuum generally bake their components out at temperatures up to 1600 degrees Fahrenheit to expel the hydrogen. That's impractical for us—the electrical bill alone would be truly astronomical. So we've collaborated with industry to

Right: Installing a recycling mirror between the laser and the beam splitter sends the light back to the interferometer's arms. D_1 is the photodetector. D_2 is a secondary detector used to adjust the recycling mirror's position for maximum destructive interference. Placing another recycling mirror between D_1 and the beam splitter would create the dual recycling interferometer described on page 11.

Below: The optical components in the foreground are part of the system that filters and stabilizes the laser light. In the background, undergrad Maggie Taylor (seated), LIGO scientist Seiji Kawamura (kneeling), and Raab confer.





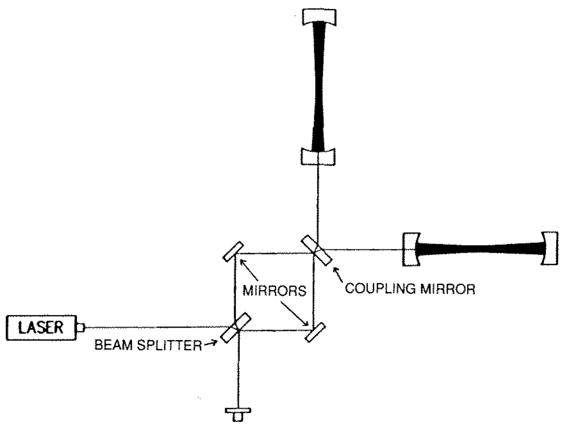
develop a special process to manufacture low-hydrogen stainless steel.

And finally, the green curve shows how the quantum nature of light—the fact that it's made of photons—affects sensitivity. "Photon shot noise" is very much like hearing rain on the roof. We hear a patter because the rain comes as droplets instead of a continuous flow of water. Similarly, with light we may be measuring a million photons per unit of time, on the average, but in any given interval, the million will be either in excess or shy of photons by about a thousand. The fluctuation during any interval generally depends on the square root of the total number. The other arm of the curve is "radiation pressure noise." Every time a photon reflects from a mass, there is a recoil momentum—radiation pressure—given to the mass. If the number of photons striking the mass fluctuates, then the radiation pressure fluctuates. At any given power level, there's a frequency at which a minimum occurs between shot-noise and radiation-pressure fluctuations in the interferometer. On one side of the minimum, we're not sensing accurately enough because we're not using enough photons, and on the other side, we're kicking the masses around because we've got too many photons. If we turn the power up, the shot noise's contribution to sensitivity decreases and the radiation-pressure contribution increases, so that the minimum moves to a higher frequency. The curve labeled "quantum limit" is the line of all possible minima for a 20-pound test mass, and it's set by the Heisenberg uncertainty principle. Unfortunately, the quantum limit, like the speed limit, is not just a good idea, it's the law. And it's a lot more rigidly enforced than any speed limit.

It takes very high light levels inside an interferometer to reach the quantum limit at higher frequencies. But there are some tricks we can play with the interferometer's optics to help reach this sensitivity. We can use our light more efficiently by recycling it. Once again, it's all done with mirrors. Remember that the light from the arms eventually hits the beam splitter, which, in the absence of a gravitational wave, delivers no light to the photodetector. By then, the light has already done its job of measuring the arm's length. But where did the light's energy go? It turns out that, by the law of conservation of energy, when destructive interference happens for light traveling in one direction, constructive interference must occur for light traveling in another direction. In this case, the beam splitter sends the constructively interfered light back toward the laser. It would have been nice if the photons didn't have to leave, because the interferometer loses energy that way. In fact, as my colleague Ron Drever discovered, they don't have to go. We can install a recycling mirror between the beam splitter and the laser and reflect the photons back in. After all, this is the age of conservation.

It works just like the Fabry–Perot cavity. There's an F wave, which is the direct reflection off the recycling mirror. In the absence of a gravitational wave, the beam splitter and the two interferometer arms act like a single mirror, sending the light back toward the laser. Now if we put the recycling mirror in the right place, the light that leaks through it—the C wave—can interfere destructively with the F wave. As a result, there's no light reflected to the laser—it all goes back into the interferometer. Believe it or not, this actually works. We've demonstrated 20-fold recycling factors in benchtop experiments. In principle, we ought to be able to make LIGO's laser appear a thousand times more powerful this way.

This recycling method is useful when we want to capture gravitational waves with high fidelity over a broad frequency spectrum. In many cases we won't know the wave's frequency ahead of time, or we may simply want to hear something go "bang!" But gravitational waves don't just come from burst sources. There are other sources—rapidly rotating neutron stars, for instance—that can broadcast gravitational waves continuously on one frequency. In this case, we might know the frequency, especially if the neutron star is a pulsar, but we won't know the amplitude that the wave should have. To study



Resonant recycling interferometer.

Like the 200-inch Hale telescope at Palomar, which has stayed on the cutting edge of astronomy over the years as its photographic plates have given way to sophisticated electronics, LIGO is not a single experiment but an experimental facility.

this kind of periodic source, we basically want to make the equivalent of a radio receiver—something that detects a very narrow frequency band, but is very sensitive at that frequency. There's a trick that works for that, and—surprise!—it involves recycling photons.

It's called the resonant recycling interferometer. Here's the trick: fill the arms with light that leaks out in a time equal to half the period of the gravitational wave we want to find, then pass the light leaking out of one arm into the other. For half of its period, the wave is stretching one cavity and shrinking the other. In the next half period, the motion reverses. If we switch the light back and forth between arms in synchrony with their alternating stretching and shrinking, so that one batch of photons is always in the long arm and the other batch is always in the short arm, the signal can build up over a long time. Ultimately, light appears at the photodetector if there actually is a gravitational wave at the chosen frequency. Otherwise, the photons dissipate among the mirrors in the system.

A new trick, called dual recycling, was just demonstrated this March by Brian Meers at Glasgow University. And yes, it's also done with mirrors. It's a modification to the broadband recycling interferometer that gives it the narrowband character of resonant recycling. In a broadband recycling interferometer, a periodic gravitational wave will immediately induce a periodic signal at the photodetector, although it may be very small compared to the background noise. The new mirror recycles this periodic signal, allowing it to build up inside the interferometer, so that each passing crest of the wave gives it a boost before it finally leaks out to the detector. We call this dual recycling, because we recycle both outputs from the beam splitter. We can now set the detector's bandwidth by choosing the reflectivity of the signal-recycling mirror. If the reflectivity is zero—as if the recycling mirror wasn't there—then we're back to the broadband recycling interferometer. If the reflectivity is high, then the bandwidth becomes very, very narrow, approaching the sort of frequency response of the resonant recycling interferometer in the previous case.

Perhaps LIGO's most important feature is that it will evolve. Like the 200-inch Hale telescope at Palomar, which has stayed on the cutting edge of astronomy over the years as its photographic plates have given way to sophisticated electronics, LIGO is not a single experiment but an experimental facility. The graph on page 9 shows the noise levels that we anticipate in LIGO's first generation of interferometers, which will be built from readily available components. For instance, the photon shot noise was calculated for an argon laser producing five watts of green light—the laser seen on page 6, in fact—installed in a broadband recycling interferometer with a 30-fold recycling factor, close to what we've already demonstrated on the benchtop. LIGO, in this incarnation, would have significant scientific potential—it would have been wonderful to have had it to view Supernova 1987a. Finding coalescing neutron-star or black-hole binaries will likely require a more advanced interferometer, one incorporating the improved lasers, optics, and materials that are now being developed, as well as the valuable experience gained by running LIGO's first detectors. If we start now, the first LIGO detectors will be on line in the latter half of the 1990s. This will mark not the closing of a project, but the opening of a new window through which to look at the universe. □

Assistant Professor Fred Raab joined Caltech's LIGO project in 1988. He had previously been at the University of Washington, where he worked on a variety of high-precision measurements of fundamental forces. He earned his BS from Manhattan College and his MS and PhD from SUNY, Stony Brook. This article was adapted from his Centennial Seminar Day talk.

The Thomas Hunt Morgan Era in Biology

Why did Caltech officials pursue a biologist so near retirement to establish the school's division of biology?

by Judith R. Goodstein

Thomas Hunt Morgan, with flies, at Columbia University in 1917.

This story of the genesis of biology at Caltech is excerpted from Millikan's School: A History of the California Institute of Technology *(©Judith R. Goodstein) and is reprinted here with permission of the publisher, W. W. Norton & Company. Probably no one was in a better position to write Caltech's history than Judith Goodstein, who, since becoming the Institute's first archivist in 1968, has had an inside track on learning what went on when. In the years since, she has built the archives, now housed in expanded new facilities in the Beckman Institute, into a notable resource in science history—and also knows where all the bodies are buried. Research for the book, which appropriately appears in time for Caltech's Centennial, was supported by the Haynes Foundation. In addition to her post as archivist, Goodstein has also been a faculty associate since 1982 and registrar since 1989. She earned her BA from Brooklyn College in 1960 and PhD from the University of Washington in 1969.* Millikan's School *will be available in bookstores in October or may be ordered from the publisher with the coupon on the back cover.*

> The establishment of a Department of Biology, rather than the traditional departments of Botany and Zoology, calls for a word of explanation. It is with a desire to lay emphasis on the fundamental principles underlying the life processes in animals and plants that an effort will be made to bring together, in a single group, men whose common interests are in the discovery of the unity of the phenomena of living organisms rather than in the investigation of their manifold diversities.
> -THOMAS HUNT MORGAN, 1927

The biochemist Henry Borsook liked to tell about a conversation Thomas Hunt Morgan had with the physicist Albert Einstein, a campus visitor in the early thirties. At a point in the conversation, Einstein supposedly asked, "What in hell are you doing in a place like this?" "The future of biology rests in the application of the methods and ideas of physics, chemistry, and mathematics," replied Morgan. The physicist persisted. "Do you think you will ever be able to explain in terms of chemistry or physics so important a biological phenomenon as first love?" "What did you say to that one?" Borsook asked Morgan afterward. "I tried to explain something about the connection between sense organs and the brain and hormones." "You didn't believe that yourself, did you?" Borsook asked. "No," said Morgan, "but I had to say something to him."

What he "had to say" says a lot about his plans, however. Thomas Hunt Morgan was 62 when he came to Caltech in 1928. By then, he had earned a worldwide reputation as a remarkable teacher, a clear writer, and an impressive researcher. In 1933, he would win the Nobel Prize for his discovery of the chromosomal mechanism by which character traits are passed on from parent to offspring through the interaction of genes.

All of that work had been accomplished in one room at Columbia University that held a bunch of bananas hanging in the corner and eight desks crammed into a space measuring 16 feet by 23 feet. In the fly room, as it was known, Morgan had elevated the lowly fruit fly, the *Drosophila*

Contributing to the squalor was Morgan's habit of squashing his flies (after he'd finished counting them) on his counting plate, which he left unwashed on his desk.

melanogaster, into the most famous experimental organism in the world.

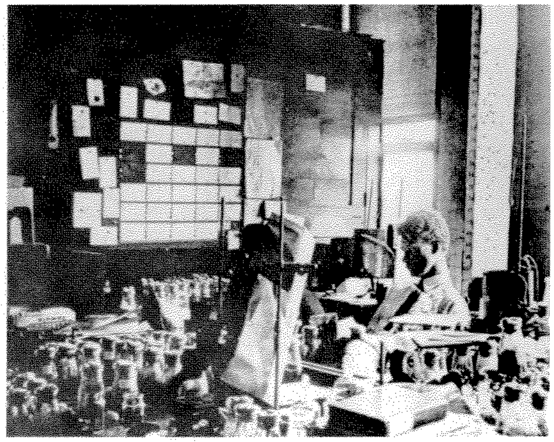
Why did Caltech officials pursue a biologist so near retirement to establish the school's division of biology? The answer to the question begins with the ways and means of Morgan's *Drosophila* group at Columbia.

According to the Russian geneticist Theodosius Dobzhansky, Morgan ran the fly room by his own rules. Traveling on a Rockefeller-financed International Education Board fellowship, Dobzhansky in 1927 arrived in New York from Leningrad thinking that Morgan was "just next to God" and his laboratory "close to Heaven." To his dismay, he found what he called "a very small, poorly equipped, and positively filthy" laboratory, run by a man obsessed by "pathological stinginess." The Morgan operation made Dobzhansky's laboratory facilities back home in Russia look very good in comparison. Morgan's longtime co-workers Calvin B. Bridges and Alfred H. Sturtevant sat and worked in the same room, along with graduate students, postdoctoral fellows, like Dobzhansky, and assorted visitors, ranging from Yoshitaka Imai, a Japanese geneticist, to Alexander Weinstein, a recent fly-room PhD. Often, all the desks were occupied, including the two reserved for guests.

Cleanliness was unknown here. The workers competed for space with cockroaches that reproduced in awesome numbers. Contributing to the squalor was Morgan's habit of squashing his flies (after he'd finished counting them) on his counting plate, which he left unwashed on his desk. As the pile of lifeless flies grew, so did the mold it attracted. Along one wall stood a kitchen table used by the student hired to wash bottles and prepare fly food. This area of the room was Morgan's only concession to hygiene.

Bridges, an unremitting tinkerer, sat at a desk covered with odd-looking pieces of apparatus he had made from items at hand. Capable of long periods of routine work and intense "fits and spurts" of ingenuity, Bridges gradually overhauled Morgan's primitive laboratory techniques: he designed a binocular microscope (most workers used a hand lens to examine flies; Morgan used a jeweler's loupe), invented new ways to etherize flies, developed new incubators, improved culture bottles, and whipped up alternative foods for flies.

Bridges had his faults, but jealousy, according to Morgan, was not one of them: "In fact, one of his most admirable traits was his freedom from priority claims of any kind." Morgan first met Bridges in 1909 when he took Morgan's courses in general biology and embryology. Hoping to find research work, Bridges put himself through Columbia on scholarships and odd jobs. Morgan hired him in 1910 to wash glassware, but gave him a desk and promoted him to the job of breeding flies and looking for mutants after Bridges spotted a fly with bright eyes in a dirty bottle. Bridges excelled at finding new mutants, which he "immediately announced." This skill (Sturtevant insisted he "had the best 'eye' for new types") paid off in 1916, when he published, in the first issue of *Genetics,* a detailed paper dealing with flies that had extra and missing chromosomes. Not only could Bridges explain



Left: Morgan's fly-room crew didn't flinch from eating among their specimens. Here a lunch celebrates the return of Sturtevant (front left, holding beer bottle and cigar) from the Army in 1919. Clockwise around the table from *Pithecanthropos* wearing Sturtevant's old uniform are Muller, Morgan, Lutz, Mohr, Huettner, Schrader, Anderson, Weinstein, Dellinger, and Bridges (with book).
Right: Calvin Bridges, who began his career with Morgan as a bottle washer, was promoted to the job of breeding flies, and given a desk, which he covered with handmade pieces of apparatus as well as bottles of flies.

these exceptions; he provided convincing proof of the chromosome theory of heredity. Bridges delighted in building up and studying the *Drosophila* stocks and mapping the position of mutant genes in each chromosome. He "was so good at this that he contributed many more mutants than did the rest of us," Sturtevant once admitted.

Sturtevant owed his desk in Morgan's laboratory to a childhood passion for recording the pedigrees of horses. A book on Mendelism that he read as a college sophomore opened new worlds, he later wrote, "for I could see that the principles could be applied to the inheritance of colors in the horses whose pedigrees I knew so well." He wrote up an account of his findings and submitted it to Morgan, his biology teacher. Much impressed, Morgan urged the young man to publish the account, which he did in due course. Sturtevant always believed this was the reason why in the fall of 1910 Morgan invited him into the laboratory and gave him a desk and some *Drosophila* to work on. By that time, Sturtevant knew for sure that he wanted to do genetics.

Jammed with people and paraphernalia, Morgan's laboratory, in short, was an ideal training ground for budding experimental biologists.

Sturtevant was the bookish one. Piles of books and reprints, stacked high, covered his desk. In the course of cleaning the room one summer, so the story goes, a workman found it necessary to rearrange some of Sturtevant's papers, uncovering a shriveled mouse.

It didn't take Dobzhansky long to discover what made the fly room tick. He later told an interviewer, "So this one room had six people working in it, a situation which doubtless had a great many advantages, particularly for a foreign guest. You can ask anyone a question you wish to enlighten yourself on any problem which arises. You also listen to the conversation between the people. As far as training is concerned, nothing better can really be imagined." Jammed with people and paraphernalia, Morgan's laboratory, in short, was an ideal training ground for budding experimental biologists, good for everything from selecting projects on which to base PhD research to testing new techniques and analyzing experimental data.

Sturtevant tells a similar story. "Everybody did his own experiments with little or no supervision," he wrote on one occasion, "but each new result was freely discussed by the group." Morgan's *Drosophila* group did not go in for organized coffee breaks, nor did it set aside a certain time of the day for laboratory discussion. "Instead," recalled Sturtevant, "we discussed, planned, and argued—all day every day." He added, "I've sometimes wondered how any work got done, with the amount of talk that went on." But Morgan did have one cardinal rule: you had to pick your own research topic.

To do otherwise could be academically fatal, as one aspiring *Drosophila* geneticist, Edgar Altenburg, discovered. Having been given desk space in the fly room, Altenburg asked Morgan to suggest a fruit fly problem for graduate work. Close by Morgan's office was the aquarium room. He took Altenburg there, dipped his finger in a tank of stagnant water, and held it up to the light. "There are a lot of *Daphnia* in here," he said to Altenburg. "Why don't you work on them?" Humiliated by the experience, Altenburg quickly switched to plant genetics.

Dobzhansky nearly made the same mistake. Once unpacked, he wasted no time in asking Morgan "to suggest a topic." "After all I was coming from afar, and although I knew what they had published earlier, I didn't know what they were doing at the time, less still . . . what they were planning to do" in the future, he said, adding, "I did not know how foolish that was." At first, Morgan brushed him off with a joke. When Dobzhansky asked him again for something to read, "the Boss" reached into his desk, took out a reprint dealing with the effects of temperature on the development of *Drosophila* (a subject of no interest to the Russian biologist), handed it over to the newcomer, and turned away. A zoologist by training and a geneticist with an expert's knowledge of the natural variations in lady beetles and with a keen desire to study problems of evolution, Dobzhansky quickly made his peace with Morgan's managerial style.

Morgan "thought that everybody should work on the problem which he sees fit," and Dobzhansky knew he was "perfectly capable of choosing" what he wanted to do in Morgan's laboratory. It was a perfect fit.

Morgan was a southerner, born in Lexington, Kentucky, in 1866. His maternal great-grandfather was Francis Scott Key, author of "The Star-Spangled Banner," and his father's brother, John Hunt Morgan, had been a notorious Confederate general. Preferring natural history to politics, Thomas Hunt Morgan in his youth combed the backwoods and byways of rural Kentucky and western Maryland, collecting fauna and fossils. One summer, he earned his keep working for the U.S. Geological Survey, tracing coal seams. After graduating in 1886 with a BS in zoology from the University of Kentucky, Morgan spent the summer months working in a marine biological laboratory at Annisquam, Massachusetts, and then enrolled as a graduate student at Johns Hopkins, earning his PhD—his dissertation involved studying different species of sea spiders—in 1890. By the time Morgan joined the Columbia University faculty, in 1904, he was known far and wide for his work in experimental embryology and regeneration.

The studies that brought lasting fame to Morgan were those connected with Drosophila. *He had begun breeding fruit flies in 1908 in an effort to determine what role . . . chromosomes played in the transmission of physical traits from one generation to the next.*

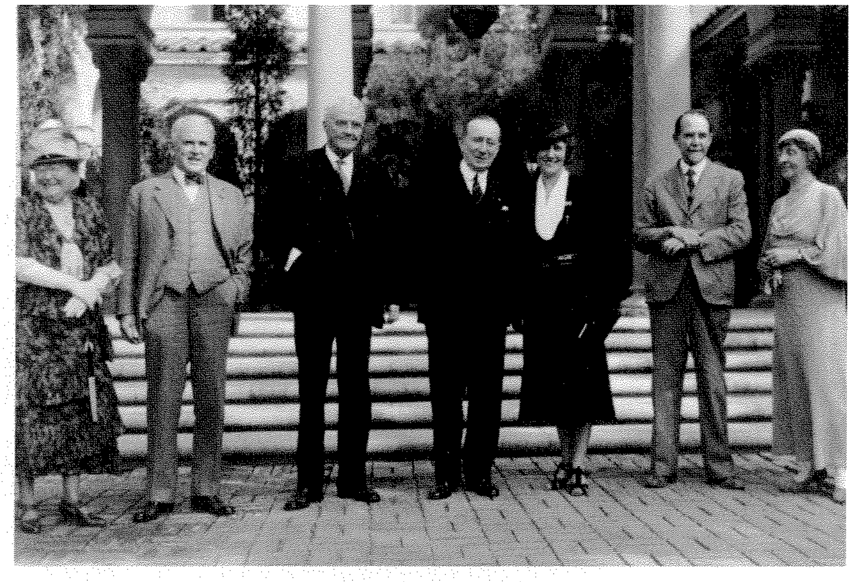
But the studies that brought lasting fame to Morgan were those connected with *Drosophila*. He had begun breeding fruit flies in 1908 in an effort to determine what role—if any—chromosomes played in the transmission of physical traits from one generation to the next.

Morgan began his research with *Drosophila* just as biologists were beginning to appreciate for the first time the long-neglected findings of the 19th-century Austrian monk Gregor Mendel. Mendel's genetic experiments on plant hybrids, published as a short report in 1866, led him to conclude that traits in garden peas such as seed shape, pod color, and plant-stem length were determined by fundamental units of inheritance, which he called "elements." Alternative forms existed for every hereditary trait as well: round seeds and wrinkled; green pods and yellow; short stems and long—one of which always stood out decisively in the pea plant. Today every schoolchild knows how Mendel, toiling alone in his monastery's vegetable patch, theorized the existence of dominant and recessive traits, through repeated crossings of innumerable pea plants. Yet Mendel's work was ignored and forgotten until 1900, when it was rediscovered independently by three botanists.

Nevertheless, many researchers, Morgan included, were initially reluctant to accept the notion that Mendel's "elements" (the term *gene* was coined only in 1909) were parts of chromosomes, unless they had evidence rooted not in statistical studies of monastery peas but rather in observable laboratory phenomena. Thus, the question facing Morgan and like-minded colleagues was twofold: First, how far could Mendel's work be taken as an authentic description of heredity in organisms? Second, how correct was the theory that chromosomes were indeed the physical basis of inheritance? The validity of the chromosome theory took Calvin Bridges—first Morgan's student and later his collaborator—only a few years (1914-16) to establish. The evidence needed to convince Morgan that Mendel's "genes" were indeed carried on chromosomes took longer to accumulate.

Of this period (1910-11) in Morgan's scientific life, the biologist John Moore has remarked, "Whereas it was difficult for Morgan to accept the data of others in suggesting that genes are parts of chromosomes, it was not nearly so difficult when his own data showed the same thing." In the end, Morgan's studies with *Drosophila* convinced him of the necessity of associating specific hereditary characteristics with specific chromosomes. He equated Mendel's elements of heredity with invisible genes at known locations in visible chromosomes and in the process created a new science of genetics.

Fruit flies have been called the geneticist's best friend. They reach maturity quickly, reproduce themselves frequently, and are inexpensive to rear. Shortly after he had begun research on *Drosophila*, in the autumn of 1910, Morgan recruited Bridges and Sturtevant, both then still undergraduates, to help with the fly work. Two years later, he brought a graduate student, the physiologist Hermann J. Muller, into the fold, an association that was to have less happy consequences. Muller was a good scientific choice—his work was clearly outstanding—but he and Morgan made a poor match in temperament. From the first, Muller's relations with the group, and with Morgan in particular, were strained. Part of the problem was the pecking order. Scientific decorum mattered a great deal to Muller, who came to feel that others in the fly room got credit for *his* ideas and experimental work. In conversations with the psychologist Anne Roe in the 1940s, Sturtevant testified that "Muller was a very essential part of that group," adding, "We didn't see eye to eye but I got a lot out of him." By the time Morgan moved his laboratory, *Drosophila* stocks, and research group to Pasadena, in 1928, Muller had long since left Columbia and launched his own research team at



On October 21, 1933, a luncheon at the Athenaeum hosted by Robert Millikan in honor of distinguished visitors Guglielmo Marconi, inventor of the wireless, and his wife (third and fourth from right) also turned into a celebration of Morgan's Nobel Prize, which had been announced the previous day. A pleased-looking Morgan stands second from right, next to Mrs. Millikan. Millikan, (second from left) is flanked by Mr. and Mrs. Allen Balch, who, in addition to financing the building of the Athenaeum, also gave $1 million toward the biology laboratory that helped bring Morgan to Pasadena (although Kerckhoff got his name on it).

"Our program, when we get it going, should speak for itself. . . ."

the University of Texas in Austin, where he became the first geneticist to demonstrate that x rays cause mutations. Like Morgan, Muller eventually won the Nobel Prize, but the personal rift between the two was never entirely repaired.

The discovery that genes are arranged in a single line in chromosomes like beads strung together on a loose necklace was made by Sturtevant in 1911. At the time, he and Morgan had been talking about the meaning behind some diagrams by H. E. Castle of coat colors in rabbits. The diagrams, they decided, were meant to be a representation of the spatial relationships of the genes on a given chromosome. How nice it would be to figure out the geometrical relationship between genes and chromosomes! "I think I can do it," Sturtevant told Morgan all of a sudden. "I suddenly realized," he recalled 50 years later,

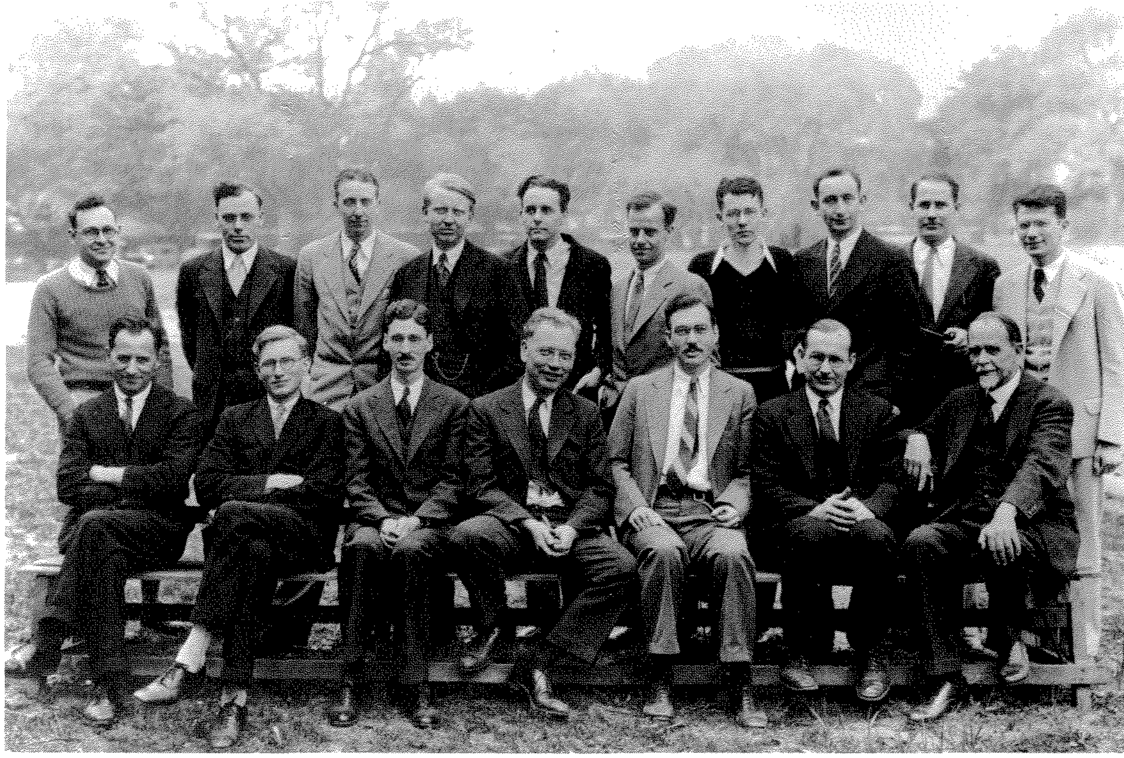
> that the variations in strength of linkage, already attributed by Morgan to differences in the spatial separation of the genes, offered the possibility of determining sequences in the linear dimension of a chromosome. I went home and spent most of the night (to the neglect of my undergraduate homework) in producing the first chromosome map, which included the sex-linked genes y, w, m, and r, in the order and approximately the relative spacing that they still appear on the standard maps.

He published his results in 1913. Sturtevant later told an interviewer that the discovery of the linear arrangement was the most exciting thing he had ever done scientifically. He went on to make other significant discoveries both at Columbia and at Caltech, but none came close to matching the thrill of his "first job on *Drosophila*."

Two years later, Morgan, Sturtevant, Muller, and Bridges joined forces to produce the first textbook on *Drosophila* genetics, which they entitled *The Mechanism of Mendelian Heredity*. A landmark in the history of 20th-century biology, the volume quickly became the bible of the new science of genetics. In the hands of Morgan and his co-workers, the genetics of *Drosophila* involved a rigorous and experimental search for the secrets of life that lay sprinkled within the chromosomes of a tiny fly.

By this time, Morgan had largely turned his attention elsewhere. He left the day-to-day operations of the fly room in the care of his students, who were technical virtuosi of the first order. A great synthesizer, Morgan distilled their findings, popularized their work, and shouldered the responsibility for bringing their results to a wide, frequently nonscientific audience. In fact, by the time he left Columbia in 1928, Morgan could no longer follow in detail what his younger colleagues, Bridges and Sturtevant in particular, were doing.

This happens to scientists, even to the best of them. And it's often sad to observe. In Morgan's case, however, distance worked to his advantage. As one geneticist familiar with Morgan's working habits put it, "For Morgan himself the *Drosophila* work was only one aspect of a biologist's searching." Professor of experimental zoology at Columbia for 24 years, Morgan in 1928 was still



In 1931 Caltech's Division of Biology included (front row, from left): H. Borsook, H. Dolk, H. Sims, A. H. Sturtevant, S. Emerson, H. Huffman, T. H. Morgan; (back row): H. Schott, C. Burnham, W. Lammerts, K. Lindström-Lang, E. Ellis, G. Keighley, J. Bonner, A. Tyler, G. W. Beadle, and J. Schultz. Missing from photo are E. Anderson, K. Belar, T. Dobzhansky, R. Emerson, and K. Thimann.

searching, as is plain from the following lines he wrote to George Ellery Hale, shortly after accepting the Caltech job:

> . . . I am writing to you something of the ideas that are shaping themselves in my mind about the organization of our biological work. Would it not be a good plan to think in terms of "The Biological Laboratories," rather than of a "Biological Department." This would allow greater freedom in giving each group an independent footing and allow greater flexibility in the future. As I have intimated to you, I think, I have no ambition to "boss the job," but rather to get together the best men available, to settle down to my own work, and then do all I can to coordinate and help matters forward along constructive lines.
>
> Our program, when we get it going, should speak for itself. . . . And, while I am anxious to emphasize the dynamic or physiological character of the work, I shall try to avoid the criticism that we are leaving the older and less important sides of biology in the background. This can best be done, perhaps, if we point out that we are not so much attempting to duplicate work that is being done well elsewhere, as in furnishing opportunities for the more advanced and less well developed lines of modern research.

Morgan's days of "the fly room" mentality were behind him. "Only through an exact knowledge of the chemical and physical changes taking place in development can we hope to raise the study of development to an exact science," Morgan told Caltech's elders in 1927, shortly after they had approached him about organizing work in biology in Pasadena. "The best chance" for success, he indicated, would be "to put some physicists in the biological laboratory, and some biologists in the physical laboratory."

Morgan's prophetic remarks set the tone of biology at Caltech for the next half century. It was, for example, Caltech's physicist-turned-biologist, Max Delbrück, himself winner of the Nobel Prize in 1969, who helped lay the foundations of modern molecular biology and the brave new world that we've only begun to glimpse.

Morgan kept his word to Hale "to get together the best men available," starting with three he knew—Sturtevant, Bridges, and Dobzhansky. He also recruited as teaching fellows three graduate students from Columbia, including Albert Titlebaum. From the University of Michigan, he plucked Ernest Anderson and Sterling Emerson, both PhDs in plant genetics, but well versed in the genetics of animals as well. Anderson, 37, came as an associate professor of genetics; Emerson, 29, as an assistant professor. Another geneticist from Columbia, Alexander Weinstein, did not get to Caltech—in a way that says much about the school's early ways.

Weinstein, Morgan told Millikan in the spring of 1928, had been working in the fly room and had just successfully repeated Muller's use of x rays to induce mutations. If appointed to Caltech, he would continue the work in Pasadena and teach the introductory course in biology. Morgan was proposing to make Weinstein an assistant, perhaps even an associate, professor, at an annual starting salary of $3,500. Emerson's starting salary was $3,800 and Anderson's

"The best chance {for success would be} to put some physicists in the biological laboratory and some biologists in the physical laboratory."

Morgan and Arie Haagen-Smit, who later pioneered studies in the chemical nature of smog and its sources, in 1938. Morgan had persuaded Haagen-Smit to come to Caltech as associate professor of bio-organic chemistry the previous year.

$4,000. As professor of genetics, Sturtevant received $6,500, placing him near the top end of the Caltech pay scale; Morgan was hoping to raise Sturtevant's salary to $7,500. (As head of the new department of biology, Morgan himself made $10,000, the same as Millikan and Noyes.)

A scientist "with distinct literary ability," broadly trained in biology, fluent in mathematics and physical chemistry, Weinstein ("a fine type, not aggressive") struck Morgan as the right man for the Caltech job. "I have hesitated a long time before bringing his name forward," Morgan admitted in his letter to the school's head, "but I think for the position proposed he is the most suitable man at present available."

No one on the faculty, save for members of the National Academy of Sciences, replied Millikan, made more than $7,000 a year. In Sturtevant's case, Millikan agreed, Morgan might have to pay that much or more to get him, but he might first try less expensive inducements—paying traveling costs to meetings back east or moving expenses. Weinstein's case was scarcely different. Millikan had at least "three brilliant young men" in the assistant professor ranks, all making between $3,000 and $3,300. Offer him $3,500, Millikan counseled, but then "give him a chance to match his pace to an associate professorship with these other men of about his age." In any case, he left all decisions about rank and salary in Morgan's hands.

Morgan did not change Sturtevant's starting salary. He offered Weinstein $3,500 as an assistant professor, which the seasoned fly-room veteran refused, pointing out that Emerson, barely out of graduate school, was making more and that Anderson and Sturtevant, who were about his age, were getting higher faculty positions. Morgan bristled. "I . . . consider the matter finished, as I do not think we want to have a man who makes points like that," he informed Millikan by letter that May. Too cheeky perhaps for Morgan's taste, Weinstein went on to teach genetics at Minnesota, then branched out into zoology and the history of sci- ence at Johns Hopkins. He later taught physics at City College of New York and eventually wound up at Harvard.

Another possible appointment in biology was Leonor Michaelis, a prominent biophysicist at Johns Hopkins. Michaelis, however, had several strikes against him, according to Caltech's new biology head. One was his apparently pronounced ethnicity. In the same letter of 1928 to Millikan recounting his dealings with Weinstein, Morgan lamented that Michaelis already had "collected about himself a few young Jews." "He himself is markedly Semitic," added Morgan. "I have my doubts whether we should want to start under all these conditions, and shall make no moves." Morgan recommended against hiring Michaelis. Fifty years later, Leonor Michaelis's daughter read the discussion about her father in the Caltech archives and said "it was shocking" to learn that the call never came because, as she put it, "he was a Jew." People, scientists included, rarely take the time to write shocking letters any more; they simply talk on the telephone instead.

Dobzhansky, who worked side by side with Morgan for many years, described him as a biologist with a razor-sharp mind, "a man of wide education which should have made him very broad, but curiously enough, did not." "In many ways," his Caltech colleague once recalled, "he was a very contradictory person." He'd had a number of Jewish co-workers at Columbia—Weinstein, Muller (he claimed one Jewish ancestor), Tyler (born Titlebaum, he changed it after moving to Pasadena)—and he brought many others to Caltech, including Henry Borsook, Jack Schultz, and Norman Horowitz, who many years later said,

> The question of Morgan's alleged anti-Semitism bothers me. I was closer to him than most graduate students during 1936-39, because he and Tyler and I spent every weekend at the marine station. I never noticed any anti-Semitism whatsoever on his part. On the contrary, he was always nice to me, and I have always believed that it was he who got me a National Research Council Fellowship when I finished my PhD in 1939.

"But time and again he would make, especially when irritated, anti-Semitic remarks of the most crude sort," remembered Dobzhansky.

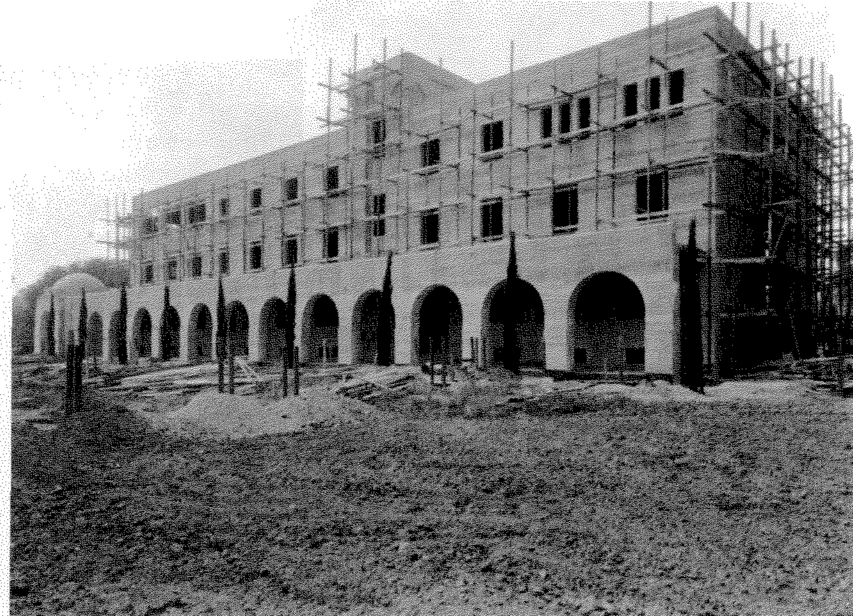
In the genetics laboratory, the atmosphere of the original fly room was soon re-created.

Morgan had a reputation for making outrageous remarks, for teasing those with different beliefs. "But he was never mean," insisted Sturtevant. Robert Millikan was often the butt of Morgan's gibes, for, unlike the atheist Morgan, Millikan was a pious Protestant. But Morgan's penchant for saying the wrong thing eventually caused an international flap in scientific circles. In 1934, Morgan went abroad, partly to pick up his Nobel Prize in Stockholm, partly to recruit new staff members. As Morgan had told a Rockefeller Foundation official beforehand, he wanted "to look over the ground at first hand and make sure that the men we have in mind are the kind we are looking for." While in London, Morgan attended an elegant reception hosted by the Royal Society. "He has announced to all who will listen," an eyewitness later reported, "that the Rockefeller Foundation has given him money to secure the services of a physiologist. He is combing England and the Scandinavian countries to find one who is not Jewish, if possible." The informant added, "From the English reception of this announcement, I am inclined to believe that he will have difficulty in finding a first-rate Englishman who will be willing to go to Pasadena." Indeed, Morgan was unable to recruit anyone in England or Scotland. Just before sailing home, he hired a Dutchman, C. A. G. Wiersma, who evidently had the right pedigree. According to a Rockefeller official in Amsterdam, Morgan "gave somewhat the impression of being 'desperate.'"

In the fall of 1928, the founding members of Caltech's biology division assembled for the first time in Pasadena. Traveling by the Santa Fe Railway, Morgan and his wife, Lilian, also a biologist, left for Pasadena on September 6, 1928, after spending the summer, as always, at the Marine Biological Laboratory in Woods Hole, Massachusetts. Dobzhansky, his wife, and Miss Wallace, Morgan's illustrator and secretary, left Woods Hole and joined him in Pasadena several weeks later, carrying among them "the sacred flame," the *Drosophila* stocks. Morgan met them at the train station. Bridges turned up soon afterward. Emerson had gone fishing with his father-in-law in Canada meanwhile and arrived at the end of October, a month after the academic term had begun. Sturtevant's wife was expecting a baby, and Sturtevant "thought she had a right to be born in the East." They arrived two months late. Anderson was completing a research trip to Berlin and arrived even later, close to Christmas.

Finding the new biology building unfinished, Morgan and his group set up a makeshift laboratory in the chemistry building, in Arthur Noyes's office. By the time classes began, two rooms in Caltech's new Kerckhoff Laboratories of the Biological Sciences were ready for occupancy. Tucked away by itself in the northwest corner of the campus, the building was a brisk five-minute walk to the physics, chemistry, and engineering laboratories across the quadrangle. A student who took courses there in 1930 recalls that the building "was connected by a boardwalk to the rest of the campus. In the winter, the territory between Gates and Kerckhoff became a sea of mud, known generally on the campus as Lake Kerckhoff."

Caltech officials had promised the biologists that they would not have to teach any courses that first year. But under pressure from the undergraduates, they offered one—in beginning biology—in the spring term. Morgan and



Above: Russian geneticist Theodosius Dobzhansky, who joined Morgan at Columbia in 1927 and came with him to Caltech, enjoyed camping and often used the pursuit of biological specimens as an excuse to pursue his favorite pastime.
Above right: Kerckhoff Laboratory was still under construction when Morgan, his researchers, and his flies arrived in 1928. The sea of mud in front of it became known as Lake Kerckhoff.

Sturtevant divided up the lectures, while Anderson, Emerson, and Sturtevant ran the laboratory associated with it. Partial to Darwin, Morgan in his homework assignments often asked students to read a portion of his masterpiece, *The Origin of Species,* and to write a report on it.

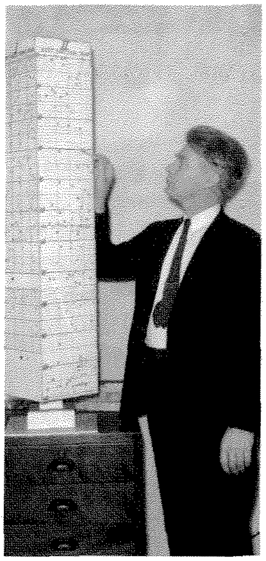
In the genetics laboratory, the atmosphere of the original fly room was soon re-created. A long bench stood in front of the two windows. Dobzhansky and Sturtevant sat at opposite ends of it looking at their flies—Dobzhansky on the left, Sturtevant on the right. "The students sat in between and listened to the wise conversation and contributed to it when they could," one former student remembers.

Intrigued by Muller's x-ray work, Dobzhansky had used his time at Woods Hole during the summer to irradiate flies. He spent fall and winter in Kerckhoff studying the chromosomal aberrations caused by the x rays and arranging them on a chart, using genes as markers. "Just what I expected to see in chromosomes, I don't remember," Dobzhansky later told an interviewer, but he decided one day to look under a microscope at the rearrangements he had projected would be observed between the fly's third and fourth chromosomes. Practiced in dissecting beetles, Dobzhansky removed the ovaries of a young female fly, embedded them in paraffin, and sectioned and stained them. It was a long, tedious process. He looked through the eyepiece. "Suddenly I saw an incredible thing," he later recalled, "namely, I saw a chromosomal plate which had just one little dot . . . and a chromosome never seen before, a long rod, which clearly meant that a piece of the third chromosome had become attached to the tiny fourth." The ancient dream of geneticists—direct evidence of the serial order of genes on chromosomes—stared Dobzhansky in the face. "I don't remember whether I emitted a loud yell," he later said.

By spring 1929, Dobzhansky had produced the first cytological map of the fly's long, rodlike third chromosome. To his joy, when he compared the linkage map, which summarized statistical data based on many genetic experiments, to the cytological map, he discovered that the two maps agreed with each other. The ability to predict the inheritance of certain characteristics, Morgan once said, justified the construction of genetic-linkage maps, "even if there were no other facts concerning the location of the genes." Dobzhansky's work in Kerckhoff Laboratory offered irrefutable, direct evidence of the correctness of Morgan's classical theory.

Meanwhile, time was running out on Dobzhansky's postgraduate fellowship. Morgan had succeeded in getting him a six-month extension, which meant he had to wind up his research and return home to the Soviet Union at the end of June 1929. One day, Morgan walked into the genetics laboratory and, according to Dobzhansky, "asked the question, 'Dobzhansky'—or rather, he called me to the end of his days, he could not pronounce this name, which of course I don't blame him, it's a devil of a name—he called me 'Dobershansky'—'Would you like to join our staff as assistant professor?'" It took him no time at all to answer yes.

In educational matters, Morgan did not be-



Right: Morgan and Albert Tyler, then a grad student and later a member of the faculty, examine fertilized eggs of marine organisms at the Corona del Mar marine laboratory in 1931.
Left: Also in the early thirties, Calvin Bridges points out a mutant on his "totem pole," a map classifying mutations in a particular chromosome as "best, good, poor, dead" for the purpose of doing genetic experiments.

lieve in graduate courses; he believed in reading. But even in Morgan's day, graduate students took courses, whether required or not. Seminars abounded, including the general biology seminar each Tuesday night, which Morgan always attended (and at which he introduced the speaker).

In one of these seminars, in 1933, a visiting biologist reviewed a paper by two German researchers on the salivary-gland chromosomes of flies known as *Bibio*, or March flies, so named because they are commonly abundant in spring. The Germans had observed in the cell nuclei of the larval salivary glands rope-like structures, which they correctly interpreted as "giant chromosomes." A number of geneticists were in the audience that day, Morgan and Bridges included, but they did not get excited by the report. Their attitude changed overnight when Theophilus Painter, a geneticist at Texas, drew and published the first map of these chromosomes for *Drosophila* and pointed out how the banding pattern could be used to study the break points of any chromosomal rearrangements.

As Dobzhansky tells the story, Bridges showed up in his laboratory one morning just thereafter and said, "Dobzhansky, show me the salivary glands." Although he probably knew more about *Drosophila* than anyone else, Bridges had no experience in dissecting larval fruit flies. Indeed, he had never even seen the salivary glands. Dobzhansky dissected a larva and showed the results to his visitor. And Bridges jumped into the study of these giant chromosomes with a vengeance. He set about identifying and extending the number of visible bands of these chromosomes. He went on to produce a series of drawings that are still consulted by fly geneticists. "Bridges's map," the *Drosophila* whiz Edward Lewis remarked more than 50 years later, "is still a masterpiece."

Keen competition existed between the Caltech geneticists and Painter's research group at the University of Texas. In 1934, Painter wrote an indignant letter to the editor of the journal *Genetics*. According to Painter, the two groups were "in a sense competing," and the Texas group had already hit two "home runs": Muller's discovery of x-ray induced mutations, and Painter's own work on the salivary glands. But now he was upset that Bridges had reviewed his salivary-gland manuscript. (Did Morgan have a hand in this? he asked.) Worse still, Bridges's salivary-gland work was now "making a splurge in the newspapers"—the *New York Times* had sent a reporter to Cold Spring Harbor to cover Bridges's talk on his salivary-gland research—while his, Painter's, contributions had "been belittled." Painter's public howl strikes a familiar chord. It is not uncommon for scientists to believe that their work is underappreciated.

Painter's most pressing complaint, however, concerned a Science News Service press release about Bridges's work, which the magazine's editor had sent to Painter. From reading the marked-up copy, Painter could see that Bridges had corrected his estimate of the size of the salivary-gland chromosomes and that the new figures now agreed with Painter's—which had not yet been published. He felt that Bridges had

There was, Morgan maintained, a rational, physico-chemical explanation behind all biological phenomena.

taken advantage of the situation. He wrote in his letter, "I do not intend to reflect in any way on Dr. Bridges. On the other hand, the men in . . . [our] laboratory are unwilling to allow competitors in our field to enjoy the privilege of examining our work a year prior to publication when we have no opportunity to see theirs."

"We are not interested in home runs," Morgan replied, after reading a copy of Painter's letter. As far as he, the dean of American biologists, was concerned, the two research groups were "cooperating," not competing. Far from admitting any wrongdoing, Morgan defended his group's honor, but held out a peace offering: articles submitted by Caltech would now be sent to Austin before publication. In a separate note to the editor, D. F. Jones, Morgan blamed Painter's "outburst" on Muller, noting, "[His] attitude has always been antagonistic to us . . . although he has generally managed to keep this under cover and we have consistently ignored it, treating him in the most friendly way, because we regarded his attitude as wrong and inexcusable." It is reported that Painter later mellowed in his view.

Painter's story bears telling because of Muller's experience several years later. At Caltech, Morgan had laid down the rule that getting papers published was an individual's responsibility, not the department's. But as Sterling Emerson once freely admitted, Morgan's group had no trouble getting him to submit their papers to *Science* and the *American Naturalist*, edited by J. McKeen Cattell, a personal friend. When Morgan submitted a colleague's paper, recalled Emerson, "it would come out in the next issue. It might be any time if you sent it in." Being friends of friends paid off, as Calvin Bridges discovered.

In 1936, Bridges was preparing a paper for *Drosophila* on the Bar gene, a spontaneously mutating gene that reduces the size of the fly's eye. As Bridges drew near the end, word reached the Caltech group that Muller was also working on this gene. Donald Poulson, a graduate student in the lab at the time, told an interviewer in 1978 what he remembered: "I don't know whether I should say anything about this, but I think it's current now—Dobzhansky had had a letter from Russia, from one of his friends, which said, 'Muller has solved the Bar story.'"

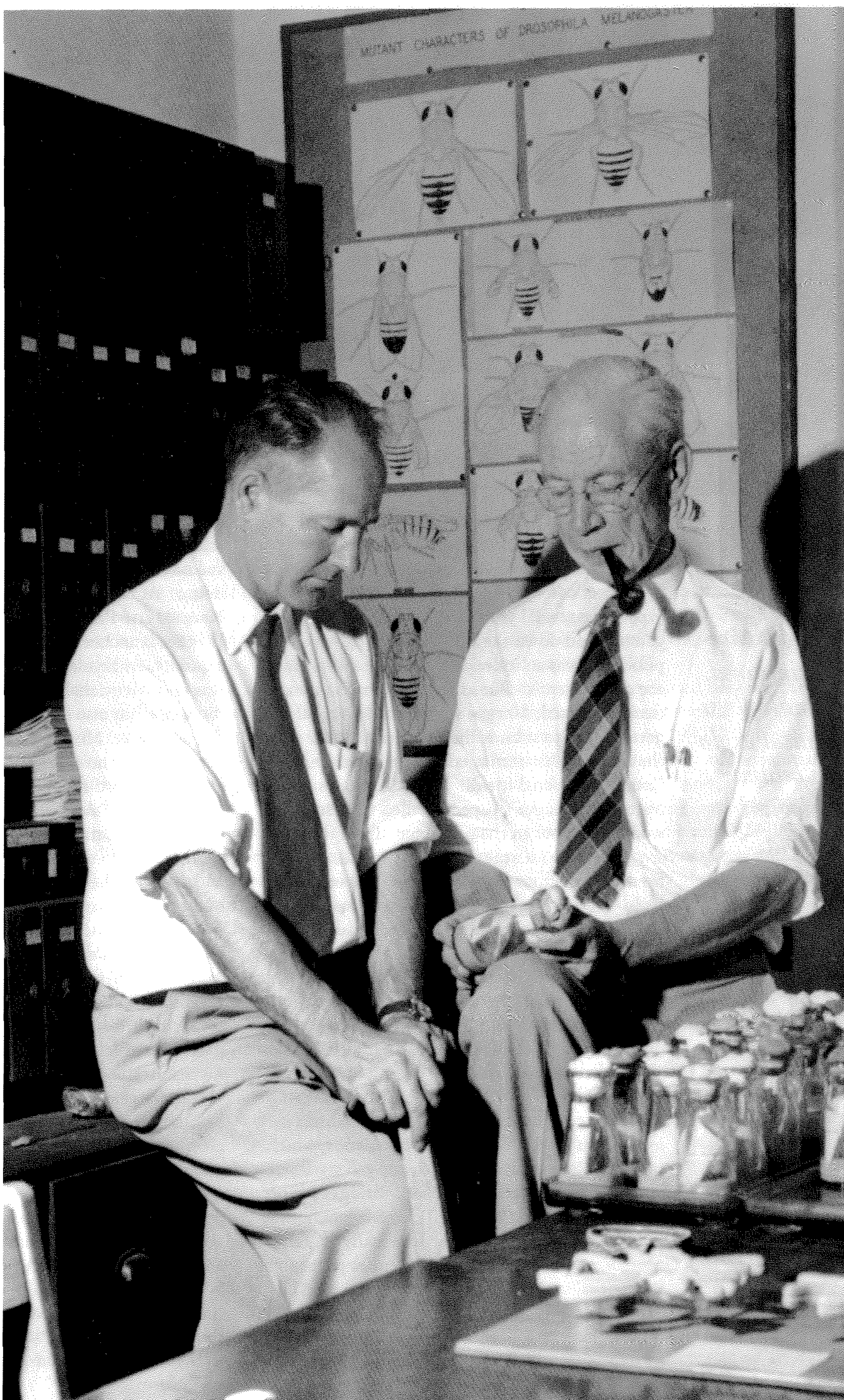
Morgan took matters in hand. Bridges's paper was submitted on February 21, to *Science,* and was published a week later. The habitually frugal Morgan, it seems, had wired the entire paper to the journal's editor. So much for Morgan's "cooperation" among *Drosophila* groups. Three months later, a special article by Muller on his own cytological analysis of the Bar gene appeared in the same journal. Muller prefaced his technical remarks by calling "the attention of American readers . . . to the fact that essentially the same findings and interpretation" (presented in Bridges's paper) had already appeared in print in the Soviet Union under Muller's name and that of his two Soviet co-workers. In truth, rivalry is the handmaiden of science, and the quest to be first is a motivating force and a powerful stimulus to creative work. Good scientists are nearly always keen competitors.

Closer to home, Morgan made a magnanimous gesture when in October 1933 he received the Nobel Prize in physiology and medicine. From the proceeds of the prize, some $40,000, he deducted his traveling expenses to Stockholm and back, and divided the rest of the cash prize three ways: one-third of the money went to his own children, one-third went to Bridges's children, and one-third to Sturtevant's.

Biology at Caltech in the thirties was special because of its emphasis on genetics, *the* essential science for the future of biology. Caltech had staked out its claim, and that was Morgan's doing. At other first-class universities, including Harvard and Princeton, genetics took a backseat to other branches of biology, such as comparative anatomy, embryology, and physiology. Caltech was unconventional not only in its choice of discipline but also in its methods of discovery. For one thing, Morgan's approach was completely experimental; no courses in descriptive biology existed. According to people in the department then, Morgan "said that as long as he had any say in this matter, there would never be a class in taxonomy or in morphology."

In setting the intellectual tone for the new division, Morgan was guided by his instincts, and by an outlook much broader than even his best pupils could muster. If he had an ideology, it "was that genetics was the root to finding out how life works," as Dobzhansky put it. Sturtevant said it in a slightly different way: "Morgan's objective in biology was the development of mechanistic interpretations. Anything teleological was sure to arouse his antagonism." There was, Morgan maintained, a rational, physicochemical explanation behind all biological phenomena.

The Caltech plant physiologist and biochemist James Bonner remembers that when he was a graduate student in biology, in the early thirties, a fellow graduate student in physics, "Willy Fowler, who will of course deny this," said to him, "'Biology? How are you ever going to make a science out of that?'" Morgan came to Caltech to answer that question. □


MUTANT CHARACTERS OF DROSOPHILA MELANOGASTER

Fifty Years Ago: The *Neurospora* Revolution

by Norman H. Horowitz

This brief paper, revolutionary in both its methods and its findings, changed the genetic landscape for all time.

This year marks the 50th anniversary of the publication of George Beadle and Edward Tatum's first *Neurospora* paper—a pivotal work of modern biology. This brief paper, revolutionary in both its methods and its findings, changed the genetic landscape for all time. Where previously there had existed only scattered observations (albeit with some acute insights) on the relation between genetics and biochemistry, this paper established biochemical genetics as an experimental science, one where progress would no longer be limited by the rarity of mutants whose aberrations could be understood biochemically, but rather where such mutants would be generated at will, and findings could be repeated and hypotheses explored as in other experimental sciences. This paper was the first in a series of fundamental advances in chemical genetics that by 1953 had bridged the gap between genetics and biochemistry and ushered in the age of molecular biology.

George Beadle (left) first came to Caltech as a National Research Council fellow in 1931 and stayed until 1936. He returned in 1946 to succeed Thomas Hunt Morgan as chairman of the Division of Biology. Here, shortly before he left to become president of the University of Chicago in 1961, he discusses *Drosophila* with Alfred H. Sturtevant, who came to Caltech with Morgan (see previous article).

I first heard of biochemical mutants in *Neurospora* at a memorable seminar given by George Beadle in the fall of 1941 at Caltech, where I was a postdoc at the time. (Beadle had come to Pasadena to recruit a couple of postdoctoral fellows to join him and Tatum at Stanford, and I ended up being one of them.) In his lecture Beadle presented their results with *Neurospora* that would shortly thereafter be published in the *Proceedings of the National Academy of Sciences.* The talk lasted only half an hour, and when it was suddenly over, the room was silent. The silence was a form of tribute. The audience was thinking: Nobody with such a discovery could stop talking about it after just 30 minutes—there must be more. Superimposed on this thought was the realization that something historic had happened. Each one of us, I suspect, was mentally surveying, as best he could, the consequences of the revolution that had just taken place. Finally, when it became clear that Beadle had actually finished speaking, Frits Went—whose father had carried out the first nutritional studies on *Neurospora* in Java at the turn of the century—got to his feet and with characteristic enthusiasm addressed the graduate students in the room. The lecture proved, said Went, that biology is not a finished subject—there are still great discoveries to be made.

The methodological innovations of the 1941 Beadle-Tatum paper were twofold. First, the authors introduced what was for most geneticists a new kind of experimental organism—a microorganism that was ideally suited for classical genetic studies, but which differed from the classical organisms in that its nutritional requirements were explicitly known—that is, it grew readily on a medium of defined chemical composition. This novel creature was the red bread mold *Neurospora crassa*. Most of the investigations that led to the development of molecular genetics employed microorganisms, but the *Neurospora* discoveries first described in the 1941 paper were crucial for making bacteria genetically useful.

Beadle had learned of *Neurospora* at a lecture by Bernard O. Dodge given at Cornell University in 1929, when Beadle was a graduate student. Dodge, a mycologist (one who studies fungi) at the New York Botanical Garden, was a strong advocate of *Neurospora* as an organism for genetic experiments. It was he who found that the mold's ascospores (which are the products of sexual fusion and recombination) require heat shock to induce germination. This made it possible to carry through the whole life cycle in the laboratory; *Neurospora* thus became domesticated. (Dodge had originally made this discovery with another fungus by accidentally setting down some plates of its ascospores in a sterilizing oven that he thought was turned off.) He worked out the basic genetics of *Neurospora*, investigating among other things the inheritance of mating type, albinism, and other single-gene characteristics. He showed that the ascospores, which come in sets of eight, each set descended from a different fertilized egg cell, display a 4:4 ratio for single-gene traits—just what Mendelian genetics predicts. By isolating and culturing the ascospores in the linear order in which they are found in the organism, he discovered the patterns of first- and second-division segregations (4:4 and 2:2:2:2, respectively). These patterns result from crossing over, or the lack of it, between the trait being studied and a point in the chromosome called the centromere; the relative frequencies of these patterns are important for gene mapping.

Genetics, which before the Neurospora *revolution had been notably isolated from the physical sciences, now found itself in the mainstream of biochemistry.*

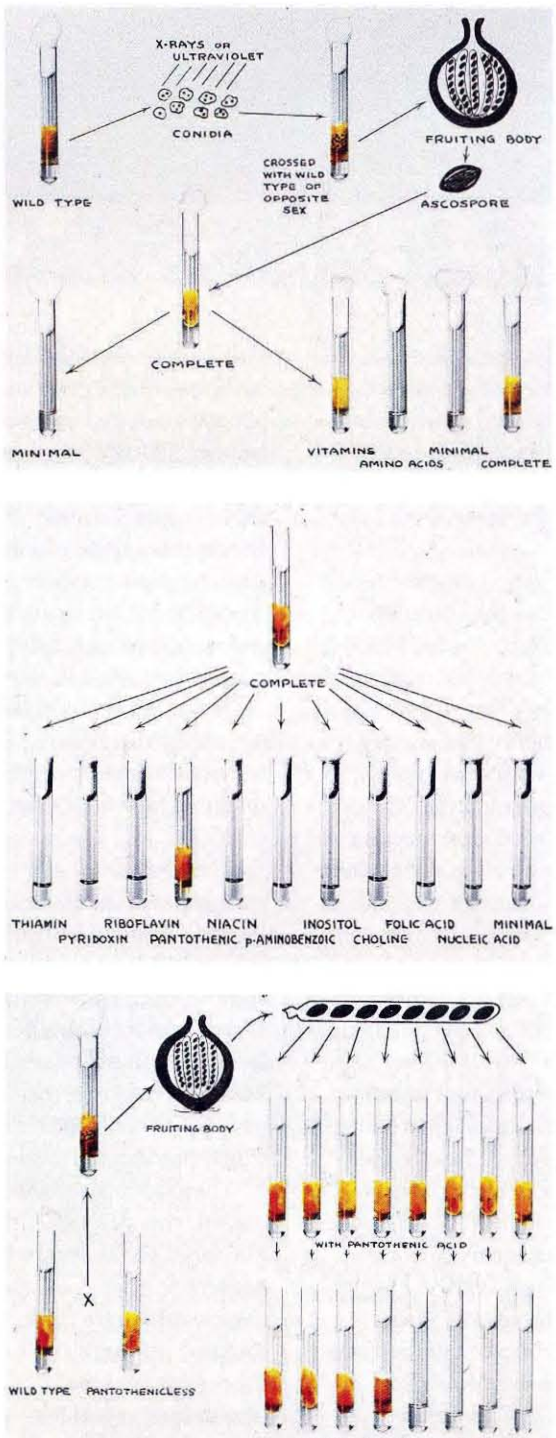
Dodge also understood the benefits that haploidy (having a single set of chromosomes, rather than two sets as in higher organisms) offered for simplifying and accelerating genetic studies. When combined with *Neurospora*'s other features, it convinced him that this fungus was the ideal genetic organism. He claimed that it was superior to *Drosophila*, as he frequently argued to his friend Thomas Hunt Morgan.

As its second methodological innovation, the Beadle-Tatum paper introduced a procedure for recovering an important class of lethal mutations—those blocking the synthesis of essential biological substances. These mutations were expressed in the organism as new nutritional requirements, and were crucial for understanding the biochemistry of gene action. They showed that each step in the biosynthesis of a vitamin, amino acid, purine, or pyrimidine is under the control of a particular gene. They displayed in a most convincing manner the central importance of genes in biochemistry and ended forever the idea that the role of the genes in metabolism was somehow a subordinate one. Genetics, which before the *Neurospora* revolution had been notably isolated from the physical sciences, now found itself in the mainstream of biochemistry. Or, more correctly, genetics and biochemistry were now seen to be different aspects of the same thing.

The fundamental character of the substances whose syntheses were affected in the *Neurospora* mutants suggested that similar mutations should occur in other microbial species. This proved to be the case. In 1944 it was shown that "biochemical mutations" could be induced in bacteria. This result solved a basic difficulty—the lack of suitable markers—that had long prevented progress toward a genetics of bacteria, and led directly to the demonstration of genetic recombination—the reshuffling of genes following mating—in *E. coli* by Tatum's student Joshua Lederberg. Biochemical mutations were induced later in yeast and other microorganisms.

Aside from its revolutionary methods, the Beadle-Tatum paper was remarkable for the results it reported. It described three x-ray induced mutants that grew on "complete medium" (a complex, undefined mixture containing yeast extract), but that failed to grow on "minimal medium" (a mixture consisting of the minimal nutrients capable of supporting the growth of wild-type, or unmutated, *Neurospora*). The presumption was that the mutations expressed in these cultures affected genes needed for the production of growth-essential compounds present in complete, but not minimal, medium. A systematic search revealed that each of the mutants required a different substance. The three substances were pyridoxine, thiamine, and p-aminobenzoic acid, and the loss of the ability to synthesize them was eventually shown in every case to be inherited as a single-gene defect.

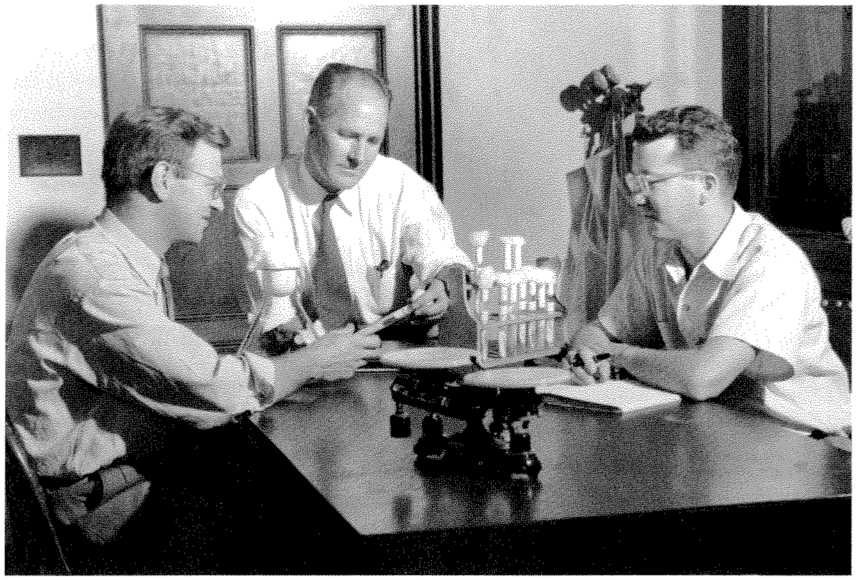
The 1941 paper reported the genetics of only the "pyridoxineless" mutant—Number 299. This was, so to speak, the breakthrough mutant, *These mutations were expressed in the organism as new nutritional requirements, and were crucial for understanding the biochemistry of gene action.*



Beadle drew and lettered these diagrams himself; he used them as lantern slides to illustrate his talks during the 1940s. The top one shows the conidia (asexual spores) of wild-type (unmutated) *Neurospora* exposed to radiation, crossed with the opposite mating type, and producing ascospores in sets of eight. These then germinate in the complete medium (the reddish color indicates the presence of the mold), which has everthing they need to grow. But when a bit of the culture is transferred to the minimal medium, they don't grow, indicating that a mutation has affected genes needed to produce an essential growth compound—in this case vitamins (or nucleic acids). Further subcultures (center) show that pantothenic acid is the substance the mutant has lost the capacity to make for itself. Crossing this mutant with wild type and dissecting out the eight ascospores in order (bottom) shows that all grow on pantothenic acid, but without it four grow and four do not—a perfect Mendelian ratio, indicating a single-gene mutation.

the one that vindicated Beadle and Tatum's ideas about a new kind of genetics. But its importance did not end there. Soon after the 1941 paper was published, Beadle received a letter from an acquaintance at the Merck Research Laboratory requesting a culture of Number 299 for the purpose of developing an assay method for pyridoxine. Beadle sent a transfer, as he invariably did once a mutant had been referred to in print. Beadle firmly believed that this policy was in the best interest of science, a belief that was certainly confirmed in this case because, in the course of their investigation, the Merck group discovered that Number 299 would grow without pyridoxine if the acidity of minimal medium was brought to a pH of 6 from its normal value of 5.

I recall first hearing of this unexpected result at an afternoon tea break in Beadle's Stanford lab. In the ensuing discussion we decided to learn whether other environmental variables—temperature in particular—might affect the characteristics of mutants in a specific way. The mutant hunt that ran more or less continuously in the lab was modified accordingly to include an incubation step at 35° C in addition to the usual one at 25°. Soon the first temperature-sensitive mutants were found—that is, ones whose nutritional deficiency was expressed only above (or occasionally below) some temperature in the normal temperature range of the organism. By modifying the gene in such a way that its activity was abolished only at certain temperatures, these mutations made it possible to identify genes that otherwise would be lost because their end



Beadle published the one gene-one enzyme theory in 1945, developed from the cumulative results of the new approach to the study of biosynthetic pathways that the Neurospora *mutants had opened, and for this he and Tatum won the Nobel Prize in 1958.*

product is, for example, too large to penetrate the cell (a nucleic acid, for instance); this product therefore cannot be restored to the organism by adding it to the medium. This attribute greatly extended the range of recoverable genes and made possible an early test of the "one gene-one enzyme" hypothesis.

Beadle published the one gene-one enzyme theory in 1945, developed from the cumulative results of the new approach to the study of biosynthetic pathways that the *Neurospora* mutants had opened, and for this he and Tatum won the Nobel Prize in 1958. This theory had already been foreshadowed in the first paragraph of the 1941 paper, where the authors suggested the possibility that genes may act "by determining the specificities of enzymes" as well as the further possibility of "simple one-to-one relations" between genes and chemical reactions. These ideas doubtless grew out of the authors' earlier work on *Drosophila* eye colors. In his Nobel lecture Beadle, in an oft-quoted passage referring to one gene-one enzyme, said: "In this long, roundabout way, first in *Drosophila* and then in *Neurospora*, we had rediscovered what Garrod had seen so clearly many years before." Beadle was without doubt sincere in this characteristically generous remark, but was he right? Was the one gene-one enzyme concept that forms one of the foundations of molecular biology really formulated decades earlier? I think the answer is no.

A. E. Garrod wrote his great work on human hereditary disease, *Inborn Errors of Metabolism,* in 1909, the same year that W. L. Johannsen introduced the word *gene* into the language. And although Garrod lived until 1936, recent writing on his work suggests that his understanding of genetics stopped around 1910 and concludes that he could hardly have had Beadle's one gene-one enzyme idea in mind at that time. The chromosome theory of inheritance was still in the future. Biochemistry was also in an embryonic state. In a monograph published in 1914, W. M. Bayliss considered it necessary to defend the idea that enzymes could be assumed to be definite chemical compounds, "at all events until stronger evidence has been brought to the contrary." The one thing that seemed clear in 1914 was that enzymes were not proteins, a belief that was not disproved until Sumner crystallized urease in 1926.

The most prescient of all writing about genes and enzymes are those of the French geneticist Lucien Cuénot. In 1903 Cuénot discussed his celebrated experiments on the inheritance of coat color in mice in terms of *mnémons* (genes), enzymes, and a chromogen, but he too at the time lacked the knowledge essential to putting the whole picture together. Unfortunately, Cuénot gave up genetics and discouraged his students from entering the field.

There were later antecedents of the one gene-one enzyme principle in the writings of Wright, Haldane, and others, where unfamiliarity with modern science does not enter in. But while these works were correct in deducing that genes must act through their effects on enzymes (and other proteins), none of them succeeded in persuading geneticists of the classical era that a direct relation between genes and proteins was

Left: Beadle (center), postdoc Harold Garner (left), and Norman Horowitz examine some *Neurospora* cultures in the early fifties. Right: Beadle bows to the Nobel assembly in 1958. Tatum is behind him to the left (with glasses).

real and important and was, in fact, the key to understanding the organization of living matter. Alfred N. Sturtevant (who came to Caltech with Morgan in 1928, eventually becoming the T. H. Morgan Professor of Biology) wrote in his *A History of Genetics* in 1965 that geneticists were disinclined to accept simple ideas of gene action because they were convinced that development was too complex a process to be explained by any simple theory. Not long before he died in 1970, Sturtevant told me that in particular E. B. Wilson's position on gene action had carried much weight. Wilson was one of the most influential figures in American biology. Although he died in 1939, the third edition of his monumental book, *The Cell in Development and Heredity*, published in 1925, is still in print. Usually very clearheaded, Wilson took what can only be described as an exceedingly murky view when, regarding the role of the genes, he wrote:

> In what sense can the chromosomes be considered as agents of determination? By many writers they have been treated as the actual and even as the exclusive "bearers of heredity"; numerous citations from the literature of the subject might be offered to show how often they have been treated as central, governing factors of heredity and development, to which all else is subsidiary. . . . Many writers, while avoiding this particular usage, have referred to the chromosomes or their components [Wilson rarely used the word *gene*] as "determiners" of corresponding characters; but this term, too, is becoming obsolete save as a convenient descriptive device. The whole tendency of modern investigation has been towards a different and more rational conception which recognizes the fact that the egg is a reaction-system . . . and that (to cite an earlier statement) "the whole germinal complex is directly or indirectly involved in the production of every character."

In an obvious and not very interesting sense, the foregoing statement is correct; but in another and much more important one, it is altogether wrong. With the *Neurospora* revolution, musings of this sort on the nature of gene action faded away. The evidence for a one-to-one relation between genes and enzymes (actually proteins, later modified to polypeptides) now became clear, abundant, and undeniable. The individual gene in some way determined the specific enzyme, although it was not yet seen how. The efforts of the pre-*Neurospora* workers to understand gene action had been made with systems often not suited for both biochemical and genetic studies. Beadle and Tatum changed this by founding a new science based on an organism and an experimental protocol designed to be maximally useful for the purposes of biochemical genetics. In doing so, they transformed biology, and that is the reason we remember this 50th anniversary. □

Norman Horowitz, professor of biology, emeritus, first arrived at Caltech as a graduate student after earning his BS from the University of Pittsburgh in 1936. Caltech's Division of Biology, under Thomas Hunt Morgan, was not even a decade old, and George Beadle was just leaving for 10 years at Harvard and Stanford. Horowitz worked with embryologist Albert Tyler, earning his PhD in 1939. After a National Research Council fellowship for a year at Stanford, he was back at Caltech as a research fellow from 1940 to 1942, when he witnessed Beadle's historic presentation, recounted above. This began a long collaboration on Neurospora, *first at Stanford and then back at Caltech, where Beadle returned in 1946, bringing Horowitz as associate professor. Horowitz was full professor of biology from 1953 until he reached emeritus status in 1982. He was acting chairman of the biology division in 1973 and chairman from 1977 to 1980.*

Besides his work on Neurospora, *Horowitz has long been interested in the biochemical aspects of the origin of life and the possibility of life on other planets. As chief of the bioscience section at JPL from 1965 to 1970, he sent biological experiments to Mars on Mariners 6 and 7 and the Viking landers; his book,* To Utopia and Back: The Search for Life in the Solar System, *was published in 1982. This article was adapted from one that first appeared in the April 1991 issue of* Genetics.


MONGOL MOTORS
CALIFORNIA
TOUVA
KYZYL

Forty-five Snowy I

"A place that's spelled K-Y-Z-Y-L has just got to be interesting."

by Ralph Leighton

The author furnished his car with a personalized license plate hoping to attract the attention of Tuvan immigrants, or at least friendly stamp collectors. Here Feynman pretends to push the car a few feet along the road to Tuva.

At a dinner-table conversation in 1977 about Tannu Tuva (known to all serious stamp collectors for its wonderful triangular and diamond-shaped stamps) Ralph Leighton and Richard Feynman impetuously resolved to go there because of its capital's name ("A place that's spelled K-Y-Z-Y-L has just got to be interesting," said Feynman.) Over the next decade, until Feynman's death in 1988, the two pursued their goal, sometimes desultorily but always with enthusiasm. Leighton, who also collaborated with the Nobel prizewinning physicist on two earlier books, "Surely You're Joking, Mr. Feynman" *and* What Do You Care What Other People Think?, *has now written* Tuva or Bust! Richard Feynman's Last Journey, *describing their attempts to penetrate the Cold War bureaucracy and make their way to the center of Asia. A chapter of the book (© Ralph Leighton) is excerpted here with permission of the publisher, W. W. Norton & Company. The book is available in bookstores or from the publisher via the coupon on the inside back cover.*

Leighton grew up around Caltech, where his father, Robert Leighton, has been a member of the physics faculty since 1947. Ralph met Feynman at the age of one, but their friendship really began when he was in high school and the two discovered a mutual passion for drumming. Out of those years of drumming came Feynman's recorded stories and some zany adventures—like this one. Just before E&S *went to press, Leighton reported that this summer he had finally reached Tuva, where he installed a plaque in Feynman's memory.*

The school year washed over me, leaving barely a moment to breathe: a typical day began with coaching the water polo team at 6 a.m., followed by five classes of remedial arithmetic and beginning algebra, and then back to coaching water polo. Most weekends included more coaching, but two welcome exceptions came in November, when Richard and I went to San Francisco to drum for a small ballet company whose home was the Elks Lodge near Union Square.

The year before, we had composed and performed the music for *Cycles of Superstition*, a ballet by the same company. Our "music" consisted entirely of drumming, which was perfectly adequate as far as Richard was concerned. He regarded conventional music and its chords and melodies as "drumming with notes"—an unnecessary complication.

Cycles of Superstition had been a great success: the audience of perhaps 30 applauded enthusiastically. This year the production was called *The Ivory Merchant*. Our job was to portray the interaction of colonial and native cultures in Africa, again entirely through drumming.

Rehearsals were Friday evening and Saturday evening, with performances the following weekend. During our free time on Saturday we walked the streets of San Francisco. Our conversation hit upon Tuva. "Let's go over to the San Francisco library," suggested Richard. "It oughta be pretty good."

Half an hour later we reached the Civic Center, a collection of European-style buildings built around a large square lined with sycamores hacked off to resemble the *marronier* trees found all over France. The library faced the City Hall, where the United Nations had convened for the first time, in 1945. As we made our way up the

wide stone stairway, Richard proposed a challenge: to find a picture of Tuva in this library.

When we looked through the card catalog, we realized we'd be lucky to find anything at all on Tuva. There was no heading for "Tannu Tuva," "Tuva," or "Tuvinskaya ASSR." There was a section on Central Asia, but it featured places like Tashkent and Samarkand.

Such a great variety in so small a country seemed impossible. Were the scenes in these postage stamps based on fact or fantasy?

Richard went off into the stacks to look at the books on "Siberia—description and travel," while I wandered around the reference section. I eventually hit upon the 1953 edition of the *Great Soviet Encyclopedia* and found an article on Kyzyl. In the middle of the page was a black-and-white photograph—a picture of Tuva!—which showed the "Dom Sovietov," Tuva's new government building. The architecture was not unlike that of the City Hall outside. A lone automobile stood conspicuously in front, casting no shadow—it seemed to have been hand-painted into the photograph.

Excited, I went looking for Richard. He was still in "Siberia—description and travel," sitting on the floor, reading a book called *Road to Oblivion*. The title looked promising. The author, Vladimir Zenzinov, had been sent into exile by the Czar—not once, not twice, but three times. The first two times he managed to escape, so the third time, the government was determined to put him in a place so isolated he would never find his way out. Even though that place turned out not to be Tuva, Richard was captivated by the story.

The following weekend we performed *The Ivory Merchant* to an audience of about 15—hardly enough to account for the relatives and friends of the cast. Depressed at the sight of so many vacant chairs, I said, "This reminds me of eating in an empty restaurant."

"If the food is good, what does it matter?" replied Richard. "Just do your best. Remember what we're doing: we're composing and performing music for a ballet, man!"

It was an unusual thing for a professor of physics and a high school math teacher to be doing, but we were doing it, and Richard loved that. But he abhorred Samuel Johnson's observation about a dog walking on its hind legs—"It is not done well, but you are surprised to find it done at all"—so it was not mentioned in the program that the drummers had other professions.

A month later, at Christmas, the predictable pattern in my family of presenting phonograph records to each other was broken by my brother Alan, who gave me some of the wonderful triangular and diamond-shaped stamps from the 1930s that Richard had talked about. They showed exciting scenes of horsemen at full gallop, kneeling archers taking aim, wrestlers interlocked in struggle, hunters shooting their quarry at close range (after all, they were postage stamps!), and a wide variety of wild and domesticated animals, from foxes and sables to yaks, camels, and reindeer. Such great variety in so small a country seemed impossible. Were the scenes in these postage stamps based on fact or fantasy? Around the border of several stamps were strange designs—festival masks of some sort—and the words "Posta Touva," spelled as if the territory had once been ruled by France.

During Christmas vacation I went to the UCLA library and discovered *Unknown Mongolia* (London, 1913) by the English explorer Douglas Carruthers (in which Tuva is referred to as "the basin of the Upper Yenisei" and its inhabitants as "the Uriankhai"), and half a dozen other books about Tuva, all of which I borrowed. Apart from *Unknown Mongolia*, all the books were in Russian, a language reputed to be twice as hard as German. But because mathematical formulas contain Greek letters, and the Greek alphabet formed the basis for the Russian alphabet, Richard was able to make out some of the captions. I bought a pocket Russian-English dictionary and looked up words one by one.

One of the UCLA library books showed the first government building—a log cabin—with a beautiful white yurt next to it. There were inevitable jokes about the president of Tuva sleeping in the "White Yurt."

Another book had several pictures of Kyzyl.

At the very least, a stamp collector might recognize the spelling and honk if he loved Tuvan postage stamps.

The new government building was already familiar to us. Other photographs showed the regional Party headquarters, a post office, and a hotel. Because the photographs were taken from different locations and included more than one building, we were able to piece together a crude map of downtown Kyzyl. In none of the photographs did we see more than one automobile.

One picture caught my interest only much later: Shkola No. 2. After deducing that there must be at least two schools in Kyzyl, I realized that here was a definite place in Tuva I could write to: I'm a teacher, so why not write to a teacher in Tuva and ask how I can visit? As much fun as it was to find out more about Tuva, our real goal was to get to Kyzyl, and so far we hadn't done anything about that.

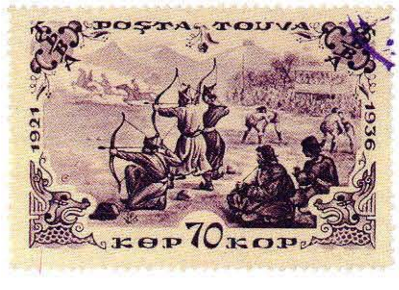
I contacted Mary Fleming Zirin (the wife of Caltech Professor of Astrophysics Hal Zirin), whom I had bummed rides off of when I was a student at UCLA, where she was working on her PhD in Russian. Mary remembered me and agreed to translate a short letter to "Teacher" at Shkola No. 2 in Kyzyl. For good measure I sent a similar letter to Shkola No. 1, Kyzyl, Tuva, USSR.

In the spring, after the high school swimming season and its coaching responsibilities were over, I went to the library at the University of Southern California and searched through immigration records of 1900–1950 to see if anyone had come from Tuva to America. While there was no specific category for Tuva, several Mongolians and "others" had come to the United States in any given year.

Just in case one of those "others" was from Tuva and had ended up in Los Angeles, I obtained a personalized license plate and mounted it in a do-it-yourself frame with the words "MONGOL MOTORS" and "KYZYL" flanking "TOUVA" above and below. At the very least, a stamp collector might recognize the spelling and honk if he loved Tuvan postage stamps.

An article I found at the same library at USC claimed that Kyzyl was the USSR's "Atom City"—the center of Soviet atomic weapons development—because Tuva is isolated and surrounded by mountains rich in uranium. Another article, in the *Christian Science Monitor* (September 15, 1966), said:

> According to the official version, Tannu Tuva [in 1944]. . . asked for admission into the Soviet Union. Its "petition was granted," just as four years earlier those of the three Baltic republics had been granted.
>
> In the case of Tannu Tuva the discovery of a large uranium deposit, the first to be found in the Soviet Union on the threshold of the atomic age, seems to have caused the change of status.



If Kyzyl is the USSR's Los Alamos, I thought, then the KGB will never believe that Richard Feynman wants to visit the place because of how it is spelled!

In the summer of 1978, after competing in the First Annual Southern California Clown Diving Championships in Los Angeles, I flew to Europe for a camping trip in the Balkans. Meanwhile, Richard went to the doctor complaining of abdominal pains. He soon underwent surgery. The doctor removed a 14-pound mass of cancer the size of a football that had crushed his kidney and spleen. Richard needed the remainder of the summer to recover.

When I returned from Europe, there was no reply from my fellow teachers at Shkola No. 2 or Shkola No. 1. In the fall a new school year began, this time without the coaching responsibilities. Another change: along with four math classes I was permitted to teach one class of world geography. Of course my students eventually learned about a little lost country called Tannu Tuva.

Although there was no ballet to work on in 1978, Richard and I continued drumming together. When we discussed Tuva, it was usually connected with a letter I had written, perhaps to a college or library in the United States or in England. But one time it was Richard who had something to report: he showed me a little article he had found in the *Los Angeles*

Above: A Tuvan photograph from a 1931 German book bore a startling resemblance to a postage stamp of the same period.
Below: A newspaper mention of a Scythian gold sculpture found in Tuva led Leighton and Feynman to Radio Moscow and new optimism. The sculpture shows a dog (right) biting a boar (center), which is biting a hunter (top).

Times—one of those fillers that takes up one or two inches—that said a Scythian gold sculpture depicting a hunter, his dog, and a wild boar had been found in the Tuva ASSR.

"I've been meaning to write to Radio Moscow," I said. "They have a program called 'Moscow Mailbag.' I'll ask them about the Scythian gold sculpture—maybe they have a picture of it."

During Christmas vacation I went to Washington, D.C., to visit an old high school friend. While I was there I went to the Library of Congress. The card catalog revealed a gold mine of books on Tuva. Because people off the street are not allowed into the stacks, I presented a dozen slips of paper containing call numbers to a clerk. Half an hour later, only six books were waiting for me—the other six were not to be found. Was someone else onto Tuva? A senior librarian informed me that it was common for books to be misplaced, and that finding only six out of twelve books was not unusual.

My frustration faded when I looked at the books that *had* been located. Among them were three gems. The first was a pocket-sized Tuvan-Mongolian-Russian phrase book. The second gem was much larger—a book by Otto Mänchen-Helfen called *Reise ins asiatische Tuwa*. The photographs in this book looked like the scenes in the famous Tuvan postage stamps of the 1930s. Because the book was published in 1931, this was understandable. (In fact, when I looked at my stamps later, I noticed that the picture on the diamond-shaped 3-kopek stamp of 1936 seemed to have been lifted straight out of Mänchen-Helfen's book, only it was reversed.)

When I looked at the first paragraph of *Reise ins asiatische Tuwa*, my years of suffering through high school German were rewarded at last: I was able to follow along well enough to get the point. (The translation here has been provided by my brother, Alan.)

> An eccentric Englishman of the kind Jules Verne loved as a hero traveled the world with the sole purpose of erecting a memorial stone at the midpoint of each continent: "I was here at the center of the continent on this day"—and the date. Africa and North and South America already had their stones when he set out to put a monument in the heart of Asia. According to his calculations, it lay on the banks of the upper Yenisei in the Chinese region of Urianghai. A rich sportsman, tough (as many fools are), he braved every hardship and reached his goal. I saw the stone in the summer of 1929. It stands in Saldam, in Tuva (as the former Urianghai is now called), in the herdsmen's republic, which lies between Siberia, the Altai Mountains, and the Gobi: the Asian land least accessible to Europeans.

So there *was* someone else onto Tuva—we had a soul mate from the 19th century.

The third gem at the Library of Congress was small and thin, some sort of guidebook written in Russian. From the charts and numbers I could tell there was a lot of talk about increased output of this and that—the usual "progress under socialism" stuff. There was also a map of Kyzyl, with drawings of various buildings. I immediately recognized the new government building, *I was ready to propose that Altadena and Kyzyl become sister cities, but Richard kept me on the straight by reminding me of our goal.*

the regional Party headquarters, the post office, and the hotel; there was also a drama theater. A trolley bus line ran from the airport right into the center of town. I made a photocopy of the map to show to Richard when I got back to California.

The small book also contained a crude map of the whole country, with little silhouettes of various animals: in the northeast there were foxes and reindeer; in the south, camels; and in the west there were yaks—all within 150 miles of Kyzyl. I thought, Here's this wide variety of animals again. Maybe the Tuva shown on the stamps of 1936 and in Mänchen-Helfen's book can still be found outside of Kyzyl today.

As a visitor to the Library of Congress I was not allowed to borrow the books; I would have to order them later through my local library. With trepidation I relinquished them to be "reshelved," perhaps to be lost forever.

When I got back to California and went through my mail, there was a reply from Radio Moscow: while they had no information on the Scythian gold sculpture found in Tuva, they did say that Tuva would be featured in the weekly series "Round About the Soviet Union" on January 17, only a few weeks away. I thought, How lucky we were to write just when we did—I don't listen to Radio Moscow much; we surely would have missed it! On January 3, two weeks before the program of interest to us, I tuned in "Round About the Soviet Union" to check which frequency had the best signal. The announcer said, "This week's program features Kamchatka. . . ." A week later I tuned in again. This time the announcer said, "This week we take you to the Moldavian Soviet Socialist Republic. . . ."

On January 17, after dinner, Richard came over. We drummed awhile. My timer went *ding*!—it was 9 p.m., 15 minutes before the program. I switched on the shortwave radio and tuned in Radio Moscow. The signal was strong.

As the announcer read a news bulletin, I got out the map of downtown Kyzyl I had copied at the Library of Congress. On the floor we spread out the present that Alan had given me for Christmas—a large, detailed map of Tuva (Operational Navigation Chart E-7) published by the U.S. Defense Mapping Agency. It showed elevation contours, vegetation patterns, lakes, rivers, dams, and—because the map was made primarily for pilots—deviations of magnetic north from true north, the length and direction of airport runways, and the location and height of radio towers.

It was time for the program to begin, so I switched on my tape recorder. The announcer said, "The topic for this week's program was selected by listener Ralph Leighton of Altadena, California. Today we will go to Tuva, located in the heart of Asia. . . ."

"Fantastic! They made the program just for us!" cried Richard.

Most of the broadcast was information I had already found in the *Great Soviet Encyclopedia* at the San Francisco Library—but with the names of some provinces misread, and several directions wrong. But then came a story we had never heard: in the past, shamans made coats and boots out of asbestos (called "mountain wool" in Tuvan, I later found out), which enabled them to dance on hot coals—thus demonstrating their extraordinary powers.

Then came the Party line about how Tuva had joined the USSR in 1944, and how everything is hunky-dory under socialism. Finally the narrator said, "Although Tuva was isolated from the outside world in the past, it is now easy to reach. Today, one can fly comfortably from Moscow to Kyzyl by jet."

The announcer mentioned my name again as the music faded out. We were ecstatic. "Tuva is easy to reach!" said Richard. "They said it themselves!"

We immediately began outlining a letter to Radio Moscow. I was ready to propose that Altadena and Kyzyl become sister cities, but Richard kept me on the straight by reminding me of our goal: "All we have to do is thank Radio Moscow for the program, remind them of what they said about Tuva being easy to reach, and then ask them to help us get there."

I was so excited that the next day I played the tape for my geography class without stopping to think that any student could have reported to the principal that "Mr. Leighton played Radio Moscow to his class." (It was 1979: the Cold War was still going strong, things Russian were definitely *not* chic, and teachers still had to sign loyalty oaths.) I even played the tape to my math class. Among my students were two Armenians from Yerevan who knew some Russian. Now that Tuva was "easy to reach," I asked them to translate a letter addressed to "Hotel, Kyzyl, Tuva ASSR," asking for room rates.

A few days later I finished my letter to Radio Moscow. In addition to thanking them for producing the program just for me, I played up the fact that I was a geography teacher, and that my students knew all about Tuva now. Then I reminded Radio Moscow that according to their program, Tuva is "easy to reach," and popped the question: "Might it be possible that I could visit Tuva myself?" (We figured the professor of physics could be added later, once the geography

A monument in Tuva that supposedly marks the center of Asia became the Altadenans' holy grail. Here ethnographer Sevyan Vainshtein, his wife, and a Tuvan colleague hold a card that says, in Russian, "Greetings, Richard Feynman!" Vainshtein later reported seeing a young Tuvan woman reading *The Feynman Lectures on Physics* in Russian outside a yurt.

It was a useful little book, with statements such as "I am a teacher," and questions like "Do you have a Russian-Mongolian dictionary?"

teacher got permission to go.)

I knew what we were getting into: Radio Moscow would interview us after our trip to Tuva, editing our answers so that only positive things came out, but I figured it was a price we could afford. Nobody listens to Radio Moscow, I rationalized. Otherwise, the programs about Kamchatka and Moldavia would have begun with a listener's name, as ours did.

While we were waiting for Radio Moscow's reply, Alan gave me a page photocopied from the *World Radio and Television Handbook*, the Bible of shortwave listening. Listed under 3995 kHz were two stations—Yuzhno-Sakhalinsk (on Sakhalin Island), and Kyzyl. As it was winter, the lower frequencies were carrying well in the northern hemisphere, so I set my alarm clock to 3:55 a.m. for a few nights and tuned in 3995 kHz, hoping to catch Radio Tuva's time signal and station ID at 4 a.m.

Most of the time I got one time signal—presumably Yuzhno-Sakhalinsk, since at 5,000 miles it was 1,200 miles closer to Los Angeles than Kyzyl. (I couldn't be sure, however, since shortwave signals bounce off the ionosphere in strange ways.) But one night I got two signals, one faint and one loud. The fainter one said something like "Rabeet Tivah" before it was drowned out by the louder one. I played a tape of "Rabeet Tivah" to Mary Zirin, who thought the words might be "Govorit Tuva" ("Tuva speaking"), a plausible way for radio stations to identify themselves in Russian. That prompted me to send a reception report to Kyzyl.

While I was holding my breath for a QSL (ham radio lingo for "I acknowledge receipt") card from Radio Tuva, three books arrived from the Library of Congress—the gems had not been lost. I immediately copied each book on the best machine I could find, and promptly sent them back to Washington. With the gracious help of Mary Zirin, the Tuvan-Mongolian-Russian phrase book became a Tuvan-Mongolian-Russian-English phrase book.

It was a useful little book, with statements such as "I am a teacher," and questions like "Do you have a Russian-Mongolian dictionary?" It was also revealing: "How do you deliver goods to the shepherds?" indicated that shepherds in Tuva were still rather isolated in 1972, when the book was published. There were single words for "spring camp," "summer camp," "fall camp," and "winter camp," allowing us to imagine that Tuvans were still moving with their animals from one pasture to another according to the season. There was also evidence of modernization: "How do you carry out the breeding of a cow?"—"We have adopted artificial insemination by hand insertion."

As for city life, the question, "How many rooms are there in your apartment?" was answered with, "I have a comfortable apartment." (Obviously a touchy subject: there must be a housing shortage in Kyzyl.)

In a section entitled "Government Institutions and Social Organizations" came an interesting series of phrases: "Comrades, I declare the meeting to be opened!"—"Chairman of the meeting"—"Agenda for the day"—"To vote"—"To raise one's hand"—"Who is against?"—"Who abstains?"—"It is approved unanimously."

There were single words for "national wrestling" and "freestyle wrestling," for "horse races," and for "a bow-and-arrow horse race." There were no fewer than 13 words and phrases describing the horses themselves—in terms of appearance, age, function, and behavior. The prime Tuvan delicacy was described as "fat of lamb's tail." There was also the useful phrase, "Is it possible to obtain a collection of works of folklore?"

The little phrase book had a whole section on greetings, which gave us the idea of writing a letter in Tuvan. When we got to the body of the letter—the "I would like to go to Tuva" part—we began to mix and match: in this case, we used "*I would like* to meet with Comrade S" and "They want *to go to* the theater," substituting "Tuva" for "the theater." But it was tricky. We gradually deduced that English is written backwards in relation to Tuvan: word for word, the Tuvan phrases were "I Comrade S-with meet-to-like-

would-I" and "They theater-to go-to-want they." (Tuvan seemed to have a Department of Redundancy for personal pronouns.)

The little phrase book had a whole section on greetings, which gave us the idea of writing a letter in Tuvan.

If we needed a particular word that was not in the phrase book, we used the pocket English-Russian dictionary to get us into Russian, and then a Russian-Tuvan dictionary (borrowed from UCLA) to get us into Tuvan. Then we used a Tuvan-Russian dictionary followed by the Russian-English dictionary to check our choice. We often came out with a different word, necessitating a new choice in Russian and/or Tuvan.

By the time we were finished, we had managed to put together about 10 sentences. In addition to saying, "I Tuva-to go-to-like-would I," I asked if there were any Tuvan-English or English-Tuvan dictionaries, any schoolbooks in Tuvan, or any recordings of spoken Tuvan.

At last we were ready to send off our masterpiece—but to whom? Richard noticed some small print at the back of the phrase book: It was written by the Tuvan Scientific Research Institute of Language, Literature, and History (its acronym in Russian was TNIIYaLI) on 4 Kochetova Street, 667000 Kyzyl, Tuva ASSR—a precise address, ZIP code and all!

About a month later a letter from the USSR arrived—not my coveted QSL card from Radio Tuva, but a reply from Radio Moscow. Miss Eugenia Stepanova wrote, "I called up the Intourist travel agency and was told that since they have no offices in Tuva, there are no trips for foreign tourists to that region." Tuva might be easy to reach for a Muscovite, but we Americans were still back on square one. (We should have known better than to believe everything we heard on Radio Moscow!)

I refused to be deterred. "If there's no Intourist office in Tuva," I reasoned, "then why not get them to open one?" I devised a plan:

1. I write a travel article about the fascinating postage-stamp land of Tuva, sounding as if I had already been to the place (I would write it in the form "when one goes here" and "when one goes there"), and submit it to various travel magazines.
2. A travel magazine prints the article, which tells the reader how to arrange travel to Tuva: "Contact the Soviet travel agency Intourist." (An address would be supplied.)
3. We get every friend we can think of from all over the United States to send a letter to Intourist saying they have read about Tuva in the travel magazine and want further information.
4. Responding to this "popular demand," Intourist opens an office in Kyzyl. (Never mind that only two guys actually end up going to Tuva, and the office closes one month later.)

Richard shook his head in dismay, but he couldn't talk me out of this one. I wrote an article called "Journey to the Fifth Corner of the World," and sent it off to half a dozen travel magazines. The plan never made it past step one.

Still undeterred, I thought: If we can't get Intourist to open an office in Tuva, then where is the nearest place that already has an office? Answer: Abakan, 262 miles to the northwest of Kyzyl, according to the automobile atlas of the USSR I had picked up in Bulgaria during my camping trip in the Balkans. Intourist had rental cars in Abakan. We could drive from there to Shushenskoye, a village—now sacred—where Lenin had been exiled under the czar; the turnoff is 40 miles along the road to Tuva. We would simply miss the turnoff and drive like hell for 222 more miles. Even if we got stuck behind a truck, we could easily reach Kyzyl by nightfall—especially in summer, when the sun set around 10 p.m. From Kyzyl we would telephone Abakan and say we had gotten lost.

Richard was completely opposed to that plan, because it was deceptive. Acting under false pretenses was one of the biggest sins in his book.

In the summer of 1979 Jimmy Carter and Leonid Brezhnev signed SALT II. Meanwhile, I wrote more letters in Tuvan, this time to *Bashky* (teacher) at *Shkola* (there seemed to be no Tuvan word for "school"), in remote towns with Tuvan names where (according to a map of Soviet nationalities I had found at UCLA) the majority of Tuvans live.

I also continued my research in libraries around southern California. I found an article in the *Times* of London (November 23, 1970) written by a fellow named Owen Lattimore, who had gone to Tuva on his way to Mongolia. He was apparently the first Westerner to visit Tuva since Otto Mänchen-Helfen, more than 40 years before. Lattimore's article concluded with this paragraph:

> And lastly, the Tuvinians themselves. They are the most captivating of the minority people that I have yet encountered in the Soviet Union. Mostly of middle height, they commonly have oval faces, a rather finely marked nose with delicate nostrils, often slightly tilted eyes. They are elegant, gay, assured. They love good food and drink, and wide-ranging conversation with a light touch; but their academic style, in the fields with which I am acquainted, is precise and rigorous. I lost my heart to Tuva and its people.

"It is a pleasure to welcome you to Tuvan studies—your appearance alone must double the population of the field."

Naturally, I tracked down Lattimore's address in England and asked how he had been able to get into Tuva. He replied in a handwritten letter that he had gone as a guest of the Siberian Center of the Soviet Academy of Sciences, and that his trip had been arranged in Novosibirsk. It wasn't until several years afterward that I realized the answer to my naive inquiry had come from "God" himself—the dean of Central Asian studies. (Lattimore, who died in 1989, was an American author and scholar who had had the singular distinction of being in good standing with the Soviets, the Mongols, and the Chinese.)

Soon after that I began receiving a publication called the *Central Asian Newsletter* from England. Apparently, during the course of my inquiries to colleges and universities, someone had put me on a mailing list as a specialist. My enthusiasm was further boosted by a letter from Dr. Thomas E. Ewing of the University of Leeds, which began, "It is a pleasure to welcome you to Tuvan studies—your appearance alone must double the population of the field."

In the fall of 1979 another school year began. Over Christmas vacation the Red Army invaded Afghanistan. The eminent Soviet physicist Andrei Sakharov, who had formed a committee in Moscow to monitor the USSR's compliance with the Helsinki Accords on Human Rights, publicly condemned the invasion. Leonid Brezhnev deported him to Gorky, a city closed to foreigners. Sakharov, in a letter smuggled out to the West, called on the nations of the world to boycott the Olympic Games, which were going to be held in Moscow that summer. President Carter, who had made human rights the centerpiece of his foreign policy, announced that the United States would honor the boycott. As 1980 began, Richard and I realized that we hadn't made any progress toward our goal. With U.S.-Soviet relations deteriorating by the day, we figured our chances of reaching Kyzyl had slipped from slim to none.

Nevertheless, I continued looking for books about Tuva in various local libraries. In one of them I found a photograph taken in Kyzyl that made my heart throb: a tall obelisk with a globe underneath, sitting on a base inscribed with the words TSENTR AZII, AZIANYNG TÖVÜ, and THE CENTRE OF ASIA—obviously inspired by our soul mate, that eccentric English traveler described in Mänchen-Helfen's book. I showed the photograph to Richard. The monument to the "Centre of Asia" became our Holy Grail.

At another library I also struck it rich: there was a new book out, called the *Tuvan Manual*, by John R. Krueger, a professor at Indiana University. The book was packed with information—over 75 pages on Tuvan geography, history, economics, and culture—as well as a detailed description of the Tuvan language.

In a section called "Folk Art" we encountered these intriguing words:

> A characteristic and specific feature of Tuvan music is the so-called two-voiced solo or "throat" singing commonly found among native Tuvans and hardly observed anywhere else. The singer sings in two voices. With his lower voice he sings the melody and accompanies it at the same time with a surprisingly pure and tender sound similar to that of the flute.

The only kind of throat singing I was familiar with was the bizarre imitation of animal sounds practiced by Inuit women of northern Quebec, which I had heard a few years before on Radio Canada. But a solo singer producing two notes *at the same time* sounded not just bizarre; it sounded impossible! This we had to see—and hear—for ourselves.

Another intriguing statement in the *Tuvan Manual* had to do with pronunciation: "Although adopting the term 'glottalized vowels' in this book, one remains uncertain as to exactly what the articulatory and phonetic nature of these sounds is." In other words, Tuvan was such an obscure language that the author hadn't heard it spoken.

Professor Krueger's book contained several examples of written Tuvan, a Tuvan-English

Leighton (right) and Feynman were eventually responsible for bringing the exhibit "Nomads of Eurasia" to the Los Angeles County Natural History Museum. Their museum credentials gave them their best shot yet at reaching Tuva. Here they enjoy a reception for Soviet VIPs at the museum in 1987.

"And one day, out of the blue, you get this letter written in broken Navajo from a guy in Russia using a Navajo-Spanish-English phrase book that he got translated into Russian by a friend of his."

glossary, and a 16-page bibliography, including a listing of Columbia University holdings of books written in the Tuvan language. The *Tuvan Manual* became our Bible.

At the end of January, I found a strange letter in my mailbox: it was addressed to "RALPH LEISHTOH" at roughly my correct address. I looked at the postmark: it was in Russian script; it looked like K, 61, 3, 61, upside-down U. But I knew what it was: K-Y-Z-Y-L. A letter from Kyzyl! I didn't open it. I would wait until Richard was home.

That night I went over to the Feynmans', letter in hand. Richard was surprised and excited. We opened it together. It was dated 7.1.1980, which we deduced to mean January 7, since July 1 hadn't come around yet. It was from the TNIIYaLI, the Tuvan Scientific Research Institute of Language, Literature, and History, which had written the Tuvan-Mongolian-Russian phrase book.

All I could make out was my name, which was in the first sentence. So Richard and I went over to my place and looked at the Tuvan-Mongolian-Russian phrase book. The first word of the letter, "Ekii," was the third phrase in the book: it meant "Hello." So the first sentence was "Hello, Ralph Leighton." But then the phrase book was of no use: the phrases were arranged according to subject, not in alphabetical order.

"We can't expect everything to be written just like it is in the phrase book, anyway," said Richard. "This letter is written in *real* Tuvan—not fake Tuvan, like ours was."

Richard got out our photocopy of the Tuvan-Russian dictionary, and I got out my pocket Russian-English dictionary, as well as the *Tuvan Manual*. Word by word, we deciphered the second sentence: "New Year with." So the second sentence was equivalent to "Happy New Year"!

The third sentence came out "Me Daryma Ondar called, forty-five snowy I." We couldn't make head nor tail of "forty-five snowy I."

"Imagine you were a Navajo living on a reservation in New Mexico," said Richard, beginning to laugh. "And one day, out of the blue, you get this letter written in broken Navajo from a guy in Russia using a Navajo-Spanish-English phrase book that he got translated into Russian by a friend of his. So you write back to him in *real* Navajo . . ."

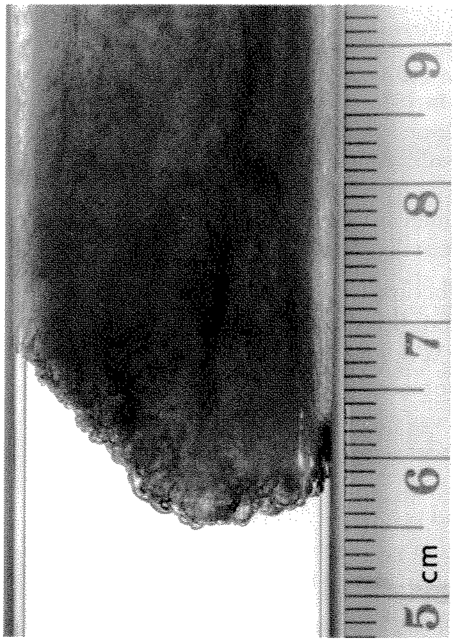
"No wonder it's hard to read real Tuvan," I said.

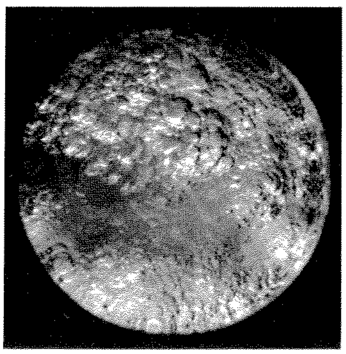
Then Richard suddenly said, "Hey! I've got it: the guy is forty-five years old." It made perfect sense. It was something like saying, "I have survived forty-five winters"—an apt phrase for Tuva, which lies between Siberia and Mongolia.

We checked the dictionaries again. There was a second definition for "snowy" that came out *letnii* in Russian—"summer" in English! "Winters, summers, what does it matter?" said Richard. "It still could mean he has lived forty-five years."

Then I looked carefully through the phrase book again. At the bottom of page 32 was the question, "How old are you?" And at the top of page 33 was the answer: *"dörten besh kharlyg men"*—"forty-five snowy I." ☐

When the cork pops—or the mountain blows—the sudden release of pressure allows the dissolved gas to vaporize.





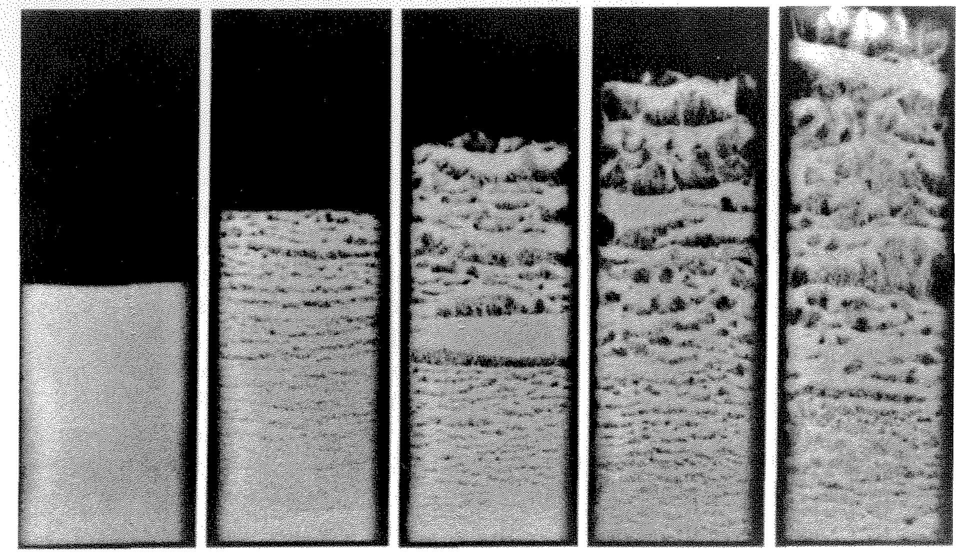
Top: This column of Freon (lower, light area) is "erupting"—boiling away at .5 meters per second. The mixture of vapor and fine droplets (upper, dark area) is being ejected upward at 35 meters per second.
Bottom: Simultaneous view of the boiling front from below.

Magma: Champagne of the Gods?

This past summer, Mount Pinatubo devastated Clark Air Force Base. On May 18, 1980, an erupting Mount Saint Helens flattened some 230 square miles of trees and killed 57 people. And on August 24, A.D. 79, Vesuvius buried the Roman city of Pompeii. In each case, the agent of destruction was a pyroclastic flow—a choking cloud of gas and volcanic dust heated to 1,000 degrees Fahrenheit. Such clouds flow down mountainsides like avalanches, knocking loose tons of boulders en route, at speeds that can be supersonic. Professor of Aeronautics Bradford Sturtevant, a specialist in shock waves and explosions, has spent seven years collaborating with volcanologists internationally on the fluid mechanics of explosive volcanoes. "We do classical geological field work—which is pretty rare for engineers—as well as laboratory flow simulations."

One way to generate a pyroclastic flow is a lot like opening a champagne bottle. The champagne—or magma—is surfeited with gas kept in solution by the pressure of the container. When the cork pops—or the mountain blows—the sudden release of pressure allows the dissolved gas to vaporize. Instantaneously, all through the liquid, microscopic bubbles form, merge, and expand, belching the liquid out of the container. The carbon dioxide in a bottle of brut hasn't got much destructive power (errant corks aside), but the superheated steam that drives a pyroclastic flow blasts magma into dust particles, called ash, as small as 10 millionths of a meter in diameter. "It's an incredible process that nobody fully understands," says Sturtevant. "We hope that learning how these flows are generated will allow us to intelligently treat their hazards."

There are certain obvious drawbacks to building a complete, working volcano in the laboratory. "We don't pretend to be able to simulate a 4,000-foot-tall volcano. We abstract certain features to make simple models that perhaps can be understood." As a result, the equipment might appear to owe more to the vintner than to the volcanologist. In 1986, graduate student Larry Hill (MS '84, PhD '91) built an apparatus consisting essentially of a thick-walled, one-inch-diameter test tube whose top could be sealed with a diaphragm of heavy-duty aluminum foil. Hill would evacuate the sealed tube, chill it, then partially fill it with Freon-12, a liquid that boils well below room temperature. Some of the Freon would evaporate as the tube warmed back up, until it finally held liquid that desperately wanted to boil, but couldn't because of the six atmosphere's worth of pressure exerted by the pent-up vapor. Then a knife blade would burst the diaphragm, venting the tube into a vacuum tank while



This series of photos of a column filled with glass beads was taken (left to right) before the "eruption," and at 2.8, 4.0, 5.2, and 6.5 thousandths of a second after it.

Hill watched the "eruption." Says Sturtevant, "We've taken movies at 6,000 frames per second, and stills at exposures of one millionth of a second—the best ever taken of this process—but we still can't write equations describing the dynamics of this behavior." They could see that the bubbles grew only on the liquid's surface—an unexpected finding. Some bubbles developed a rough surface whose texture was too fine to make out. The roughness became wrinkles, which grew into wavelets, whose crests seemed to tear themselves apart into clouds of fragments too small to see—the "ash" particles. They also saw dark clouds of vapor and fine droplets racing over the boiling surface. This rapid motion may mean that the bubbles shatter in a domino effect—shards from one bubble hit its neighbors like a shotgun blast, bursting them into more droplets that tear through *their* neighbors, and so on.

Those first experiments used one substance for both the molten rock and the water vapor dissolved in it. A real eruption is more complicated, because the gas molecules must diffuse through the involatile magma in order to meet their fellows and form bubbles. David Pyle and Youxue Zhang, then postdocs in Leonhard Professor of Geology Edward Stolper's group, heard about the work and decided to borrow Hill's apparatus for a more realistic simulation using two different components, one of which was involatile. This proved to be trickier than anticipated. The liquids had to be clear, in order to photograph what was going on within them; the involatile liquid had to be able to dissolve a lot of its volatile partner; the volatile component had to have a high enough vapor pressure to drive the "eruption," even when present only as a minor component dissolved in the involatile liquid; and finally, the liquid and the vapor had to coexist at room temperature and a pressure the apparatus could sustain. Pyle and Zhang experimented with a variety of mixtures, and finally hit on a water (involatile)-carbon dioxide (volatile) combo that, under sufficient pressure, gave a gratifyingly "volcanic" eruption.

These experiments revealed differences in the eruption styles of the one-component (Freon) and two-component (CO_2-H_2O) systems. The Freon system had a well-defined interface between the liquid and the layer of vapor above it. The eruption began at the interface, and proceeded smoothly at a constant—albeit rapid—rate into the liquid's bulk. But in the CO_2-H_2O system, bubbles grew simultaneously everywhere inside the liquid, and there was no clear interface between the liquid and the vapor. The liquid's entire volume was involved in the eruption, not just its surface.

In another set of experiments with similar apparatus, Sturtevant grad student A. V. Anilkumar (MS '83, PhD '89) used 0.25-millimeter-diameter glass beads as stand-ins to see how the ash particles, once formed, would ride the blast's pressure wave. "We had imagined that things would expand fairly uniformly, and that we'd end up with an even, high-density flow of dusty gas," says Sturtevant. They found instead that the depressurization would loft entire layers of solid-packed beads, a few beads thick, separated by regions of very nearly bead-free air. Traceries of beads would rain off the bottom of each layer, enclosing regions of the void below into "bubbles" that drifted up through the packed layers. "These kinds of buoyant instabilities are seen all the time in industrial processes at normal gravity, but we didn't expect them here, where the average acceleration on the beads is about 200 times that of gravity. All the computer models of pyroclastic flows—and of nuclear blasts—assume a uniform density. But these fluctuations from packed beads to free air mean that something in the flow, like a human being or a missile silo, isn't going to feel a steady whoooosh, but a bam! bam! bam! as these blobs hit it. The effect on the object can be quite different from what we would calculate from a nice, uniform flow. I think the flow-averaged models have to be a bit suspect now." □ —*DS*

Letters

Editor: In Timothy Ferris's letter of your Spring 1991 issue he gives the magnitudes of Regulus and Vega incorrectly. According to the most recent edition of the Yale Catalogue of Bright Stars, the visual magnitude of Regulus is 1.35, not –0.3; and the visual magnitude of Vega is 0.03, not 0.6.

A perhaps more likely target at 3:00 a.m. on November 2 is Capella, whose visual magnitude is 0.08, almost exactly the same as that of Vega. Also, its declination is more similar to that of Vega than is Regulus, and because of its high declination it would still be near the zenith even if two hours west of the meridian.

As Timothy Ferris says, many astronomers do not know the sky, but we do know enough to find the magnitudes of stars by going to the library and looking them up.

George Wallerstein (PhD '58)
Department of Astronomy

Editor: I applaud Timothy Ferris's efforts to determine which star the astronomers saw on the morning of November 3 (in place of the spurious Vega). When Bob Eklund first brought my attention to the discrepancy in Adams's account of the first light of the 100-inch, he theorized that it might have been Capella. So when I saw Ferris's idea that the culprit might have been Regulus, I did some digging of my own. After examining the sky position on two computer programs (EZ Cosmos and Skyglobe), I'm afraid that I have to agree with Bob Eklund. On the morning of the 3 (at 3:00 a.m. PST), Capella was nearly overhead (altitude of around 75°) while Regulus was low (30°) on the eastern horizon. And while the spectral types of Capella and Vega are not the same (G8 vs. A0), their apparent magnitudes are almost identical (0.08 vs. 0.03). Their declinations are also only 7° different, so that they would be in nearly the same place in the sky when at culmination (Regulus, with a declination of +11° would not come close to the zenith). In addition, I must disagree with his magnitude of Regulus. The lists I have show it to have an apparent magnitude of 1.35, very noticeably fainter than either Capella or Vega.

If we substitute Capella for Regulus in Ferris's argument, then, I would agree with his assessment of Adams's account. And certainly no one could argue with his conclusion about Noyes; he held very loose reins on his poetic license indeed.

Ronald Brashear
Assistant Curator (Science)
The Huntington Library

Editor: Capella does indeed seem to be the more likely Vega stand-in, and I'm grateful to Ronald Brashear for having looked into the matter. I was misled by having substituted the absolute for the visual magnitude of Regulus. (GeorgeWallerstein's impression to the contrary, I do know how to go to the library and consult a star catalog, although evidently I cannot be relied upon to read the numbers off the proper column.)

Absolute magnitude, I should explain, is defined as the visual magnitude a star would display if observed from a distance of 10 parsecs. Were Regulus 10 parsecs away from Earth the two magnitudes would be identical. Alas for my argument, Regulus is some *20* parsecs distant. Moreover, it's getting farther away all the time, receding at a rate of six kilometers per second, and thus slowly but steadily magnifying the dimension of my error. This I regard as fresh evidence of the stars' legendary indifference to human affairs.

Timothy Ferris
Professor of Journalism
UC Berkeley

Editor: The recent *E&S* obituary for Carl Anderson carried my memory back to the 1931–32 period. I was then a sophomore student of his in physics— and I also served part time as his lab assistant, funded under Roosevelt's NYA program. One day I was in the shop working on a piece of electronic equipment for him. He came up in a state of excitement to show me something; apparently none of his colleagues were around, and I was the only person available. He took me along to his research lab and showed me a cloud chamber photo, remarking that one of the tracks disproved current theory.

With youthful enthusiasm, I immediately expressed confidence that he could develop his own theory to explain the new results. He replied something like, "Oh no, I'm an experimental physicist, not a theoretical physicist." When I didn't seem to realize that there was a great distinction (he had always seemed pretty handy with theory at my level of sophistication), he remarked, "A theoretical physicist has to eat, sleep, and dream physics 18 hours a day; I'm not that dedicated." These remarks made a tremendous impression on me.

I was absent from Caltech for the 1932–33 academic year and somehow missed the announcement of Anderson's discovery of the positron. When he later received the Nobel Prize, I thought back to the above incident. But something about it didn't seem to fit: I'm not sure whether it was the date (a bit too early, perhaps), or that my vague recollection of the particular cloud chamber feature involved suggested it was not the reversed-curvature track of a positron. Also, there was his remark that the special track disproved current theory; finding the positron was, on the other hand, confirmation of a theoretical prediction. Thus for a long time I have had the feeling that I had been a witness to an earlier event, a precursor to the big one. But since the November 1981 article on Anderson in *Physics Today*, I have wondered if I had not indeed been there at the actual discovery.

Herbert S. Ribner (BS '35)
Professor emeritus
Institute for Aerospace Studies
University of Toronto

Editor: When I carried the Spring 1991 issue home with me and read it this summer, I was quite surprised. An article, "Caltech's *Other* Rocket Project: Personal Recollections," really struck "home." I live in sight of the China Lake Naval Weapons Center, earlier known as Inyokern NOTS. I have always wondered who decided to put a Navy facility in the barren desert of the Indian Wells Valley.

Very few of the local residents know Caltech's place in the area's history. They do not realize that if Charles Lauritsen had chosen a different site for his rocket experiments, a city of 28,000 would not exist.

I almost felt like part of the story. In 1943, those scientists from Caltech came to the Indian Wells Valley. Nearly 50 years later, I left the Indian Wells Valley to come to Caltech in order to become a scientist. It looks like a cycle.

The Caltechers set China Lake on the right track for rocketry in the forties. In the nineties, we need some Caltechers to put China Lake on the right track for chemistry. I recently read that China Lake chemists are experimenting with cold fusion!

David M. Krum
Sophomore

Editor: I enjoyed Hans Bethe's article (v. 54, no. 3) on nuclear power. Although I agree that nuclear power is relatively safe and that high-level nuclear waste repositories will probably be quite clean, the article omits some damaging information about backfill material and probably understates the overall environmental impact of nuclear power. In addition, it seems reasonable to question whether disposal of "spent" fuel is wise.

Bethe talked about the backfill material, which would be placed between the metallic waste canister and the rock. According to most designs, this material would consist largely of bentonite clay, which swells dramatically on contact with water and is supposed to form a seal around the canister. Bethe called this "the most important [component] of all," and noted that "reasonable people have estimated that this backfill may easily last 100,000 years."

In fact, bentonite is a rather unstable material, and its swelling properties are strongly dependent on the chemical and thermal environment. It was recently discovered that its ability to swell in water is severely degraded by just a few hours' exposure to steam at 150° to 250° C. The effect of steam would have been a severe design problem for the proposed repository at Hanford. This repository was to have been located several hundred meters below the groundwater table, with projected temperatures far in excess of 200° C for the backfill material.

Fortunately, there are ways around the problem, and repository designers are well aware of the effect of steam on bentonite. The repository at Yucca Mountain will not use bentonite. The Swedish repository and the Canadian repository are likely to use bentonite, but will accept waste only after it has cooled considerably.

There is one fundamental question about nuclear waste disposal that is seldom considered. That is whether it makes sense to dispose of the transuranic elements. "Spent" fuel is not spent at all, but is merely contaminated with fission products and slightly depleted of fissionable U^{235}. The U^{238} remains and can be converted to fissionable Pu. Most of the U^{235} also remains. Thus, most of the energy content of the fuel remains. On a planet that has limited resources, disposing permanently of an energy source of this magnitude seems irresponsible.

Bethe correctly pointed out the serious environmental problems caused by coal and oil, and compared them to the rather negligible accident rate and negligible environmental contamination caused by nuclear power. Unfortunately, the vast quantities of tailings and other waste that are produced by mining and processing of uranium ore were not mentioned. Although this problem may not pose as great a threat to the planet as coal and oil, it is by far the biggest source of pollution from nuclear power. Consequently, nuclear power may not be as clean as it appears from Bethe's article. Significantly, if nuclear fuel were reprocessed and reused, far less mining waste would be produced, and possibly the overall impact would be reduced.

Although nuclear energy is an excellent source of power, no source is nonpolluting, except perhaps conservation and solar power. One thing is perfectly clear: our national energy future is guided by emotional and aimless public debate rather than by rational considerations.

Rex Couture (BS '66)
Department of Earth and Planetary Sciences
Washington University

I am preparing a biography of Linus Pauling—with Dr. Pauling's cooperation—and would appreciate hearing from former students and colleagues who have anecdotes or insights about his years at Caltech. Please contact Tom Hager, 3015 Friendly Street, Eugene, OR 97405.

Random Walk

Honors and Awards

Three faculty members are recipients of MacArthur Fellowships, established by the MacArthur Foundation to recognize talented and creative individuals. Jacqueline Barton, professor of chemistry, will receive $250,000; James Blinn, associate director of MATHEMATICS!, member of the professional staff in physics, and lecturer in computer science, was awarded $265,000; and James Westphal, professor of planetary science, receives $365,000.

Jacqueline Barton was also one of nine new members elected to the American Academy of Arts and Sciences. The other new AAAS fellows are John Bercaw, professor of chemistry; Lance Davis, the Harkness Professor of Social Science; George Housner, the Braun Professor of Engineering, Emeritus; Steven Koonin, professor of theoretical physics; Carver Mead, the Moore Professor of Computer Science; Elliot Meyerowitz, professor of biology; John Seinfeld, the Nohl Professor and professor of chemical engineering (and chairman of the Division of Engineering and Applied Science); and Edward Stolper, the Leonhard Professor of Geology.

Joel Burdick, assistant professor of mechanical engineering; George Djorgovski, associate professor of astronomy; G. Ravichandran, assistant professor of aeronautics; Yu-Chong Tai, assistant professor of electrical engineering; and Stephen Taylor, assistant professor of computer science, have been named Presidential Young Investigators by the National Science Foundation. Each will receive NSF support of $25,000 annually for five years and can receive up to $100,000 through a combination of federal and matching funds.

Mark Davis, professor of chemical engineering, has received the National Science Foundation's Alan T. Waterman Award of $500,000 in research support. He's the first engineer to win the award.

Harry Gray, the Beckman Professor of Chemistry and director of the Beckman Institute, was announced as recipient of the Waterford Award, established by the Research Institute of Scripps Clinic "to recognize individuals making seminal biomedical advances."

Scientists on the Barricades

After the Armenia earthquake in 1989 the USSR Academy of Sciences and the US Academy of Sciences organized a cooperative program on earthquake engineering research and applications. A Soviet delegation visited the United States in 1990, and a return visit by the National Research Council's Committee on Earthquake Engineering (whose chairman is George Housner, the Carl F Braun Professor of Engineering, Emeritus) was scheduled for September 8–15 in Moscow. At the time of the August 19 coup, a cable was sent to the Soviet Academy of Sciences inquiring about the feasibility of the visit. The following reply was contributed to *E&S* by Housner before he left for Moscow:

From: pandora!aosussr Fri Aug 23 01:03:38 1991

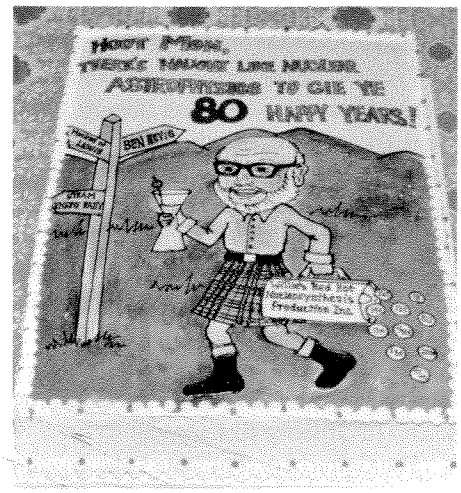
To: irex, nsf, nas

Subject: victory

SORRY FOR SILENCE DURING 19–22 AUGUST 1991. WE FOUGHT ON BARRICADES ROUND THE WHITE HOUSE OF RUSSIA, DEFENDING DEMOCRACY, GLASNOST, PERESTROIKA. WE WON, HAPPY, AND READY TO CONTINUE OUR SCIENTIFIC COOPERATION.

BEST REGARDS, THE STAFF OF THE CENTRAL ADMINISTRATION FOR FOREIGN RELATIONS OF THE USSR ACADEMY OF SCIENCES

The cake commemorating Willy Fowler's 80th birthday refers to his Scottish origins, his favorite hobbies, and his Nobel prizewinning work in nuclear astrophysics.



Centennial Willyfest

Caltech is 100 and William A. Fowler is 20 years younger. This convergence of birthdays was celebrated last month with a Centennial Year Nuclear Astrophysics Symposium, a k a Willyfest. Fowler, Institute Professor of Physics, Emeritus, has spent most of his 80 years at Caltech, arriving as a graduate student in 1933 and remaining as a faculty member since 1939. He won the Nobel Prize in 1983 for his work on the origin of chemical elements in stellar nuclear reactions.

About 70 scientists attended the symposium on August 12–14, which included sessions on the early universe, laboratory nuclear astrophysics, stellar evolution and supernovae, neutrino astrophysics, heavy element nucleosynthesis and galactic chemical evolution, and nucleosynthesis, isotopic anomalies, and gamma rays. Amidst all the heavy stuff, a festive birthday dinner was held at the Athenaeum (featuring a cake designed by Fowler's colleagues in Kellogg), and a good time was had by all.