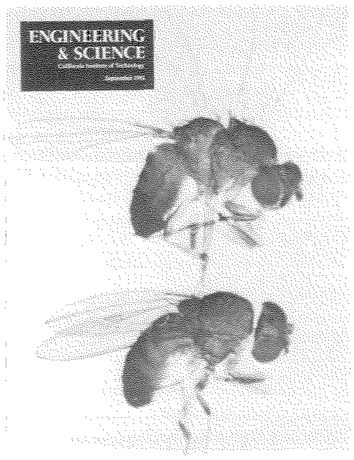
ENGINEERING & SCIENCE

California Institute of Technology

September 1981

In This Issue



Fly Paper
On the cover — two specimens from the extensive Caltech stocks of *Drosophila melanogaster* that are used all over the world for the study of genetics. The fly at the bottom is a normal, two-winged insect, but the one at the top is a mutant with four wings. It is just one example of the many mutants developed, studied, and preserved in the Institute's fly repository over the last 50 years. In "The Second Golden Age of Drosophila Research" on page 13, the director of Caltech's News Bureau, Dennis Meredith, describes some of the current research — and reassures us that there is no relationship between Drosophila and the infamous Mediterranean fruit fly.

Brownian Motion
Last April, members of the Caltech community received a note from Provost John D. Roberts announcing an opportunity to hear a lecture by Caltech's former President Harold Brown. They turned out in force to hear him discuss "National Security and International Crises: The World in the 1980s" — one of his first public addresses since leaving office as Secretary of Defense in the cabinet of President Jimmy Carter. An adaptation of that talk appears on page 22.

Introducing Brown, Roberts said in part, "Harold's Caltech colleagues were hardly surprised when Jimmy Carter began to consult with him before the presidential election in 1976. And almost all of us were pleased for him, if not for Caltech, when he was selected to be Secretary of Defense. We were relieved to know that a person of his knowledge, intelligence, and experience was going to be in that position. Four years have passed, during which he has been expected to wrestle with SALT II, the Iranian hostage crisis, the MX missile proposal, the invasion of Afghanistan, and a host of other problems. Now he is what the stage folk call 'at liberty,' and it will be interesting to see where he goes from here."

"At liberty" is, of course, a relative term; in addition to being Distinguished Visiting Professor of National Security Affairs at the Johns Hopkins University School of Advanced International Studies, Brown keeps very busy with his own consulting firm in Washington, and he serves as a consultant and/or board member for a number of corporations.

Blast Off!
Fortunately, Sue Kieffer was not right on the scene when Mount St. Helens erupted in May 1980. But she had been there just before, to study the volcano as it began to awaken, and returned the day after the main blast to interpret the flow dynamics from the patterns of destruction. In "The Blast at Mount St. Helens: What Happened?" on page 6, she describes some of the experiences of "being there" and her theoretical model of the massive blast. The article is adapted and updated from her talk at Seminar Day last May.

Kieffer is currently a geologist with the Branch of Experimental Geochemistry and Mineralogy of the U.S. Geological Survey in Flagstaff, Arizona. She received her MS in geological sciences from Caltech in 1967 and her PhD in planetary sciences in 1971. She was named an Alfred P. Sloan Fellow in 1977-79, received the Mineralogical Society of America Award in 1980, and was the first W. H. Mendenhall Lecturer of the U.S.G.S., a lectureship to emphasize the importance of basic research in applied science.

Drop Us a Line
With this issue, *E&S* offers its readers a few changes. Thanks to designer Doyald Young, the magazine has had a facelift that we hope will make what we print more attractive to look at and easier to read. We also have a new last-page-of-the-magazine feature called "Random Walk," in which you will find interesting items about faculty, alumni, and campus events. We'll continue our coverage of the research and ideas of the people who teach and study at Caltech, told mostly in their own words. We'd like to add a regular "Letters" page, and for that we need your help. We can't guarantee to print everything we receive, but we'd like to hear from you.

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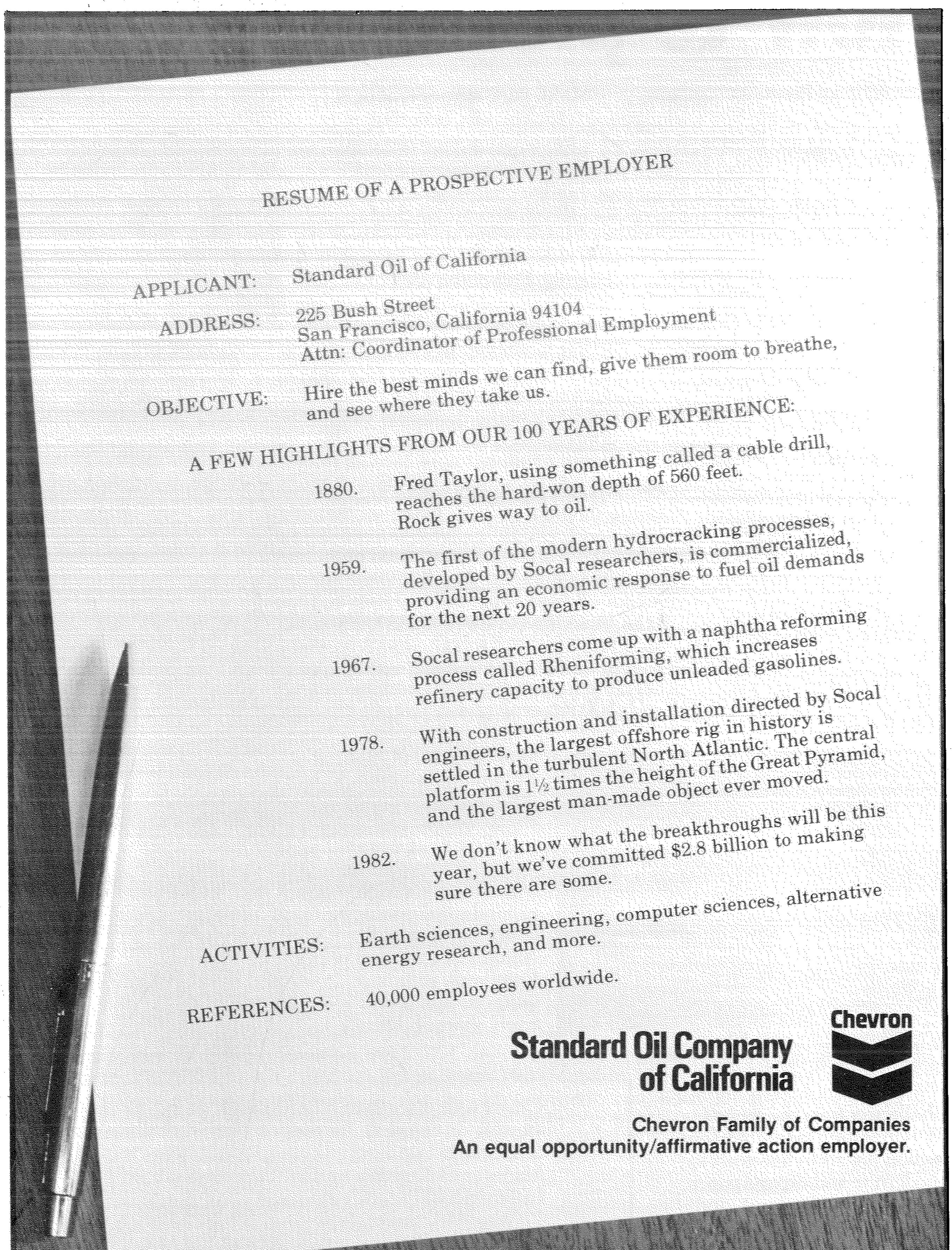
PICTURE CREDITS: Cover, 13 — Edward Lewis/14, 29, 30, 31, 38 — Chris Tschoegl/40 — Paul MacCready.

Engineering & Science (ISSN 0013-7812) is published five times a year, September, November, January, March, and May, at the California Institute of Technology, 1201 East California Boulevard, Pasadena, California 91125. Annual subscription $7.50 domestic, $15.00 foreign, $20.00 foreign air mail, single copies $1.50. Controlled Circulation postage paid at Pasadena, California. Published by the California Institute of Technology and the Alumni Association. Postmaster: Send change of address to Caltech, 1-71, Pasadena, CA 91125.

ENGINEERING & SCIENCE

CALIFORNIA INSTITUTE OF TECHNOLOGY | SEPTEMBER 1981 — VOLUME XLV, NUMBER 1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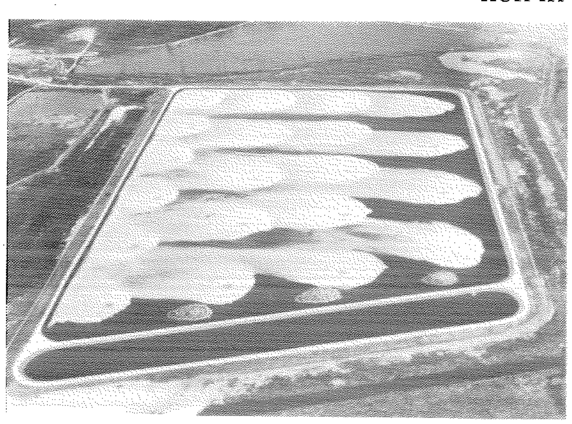

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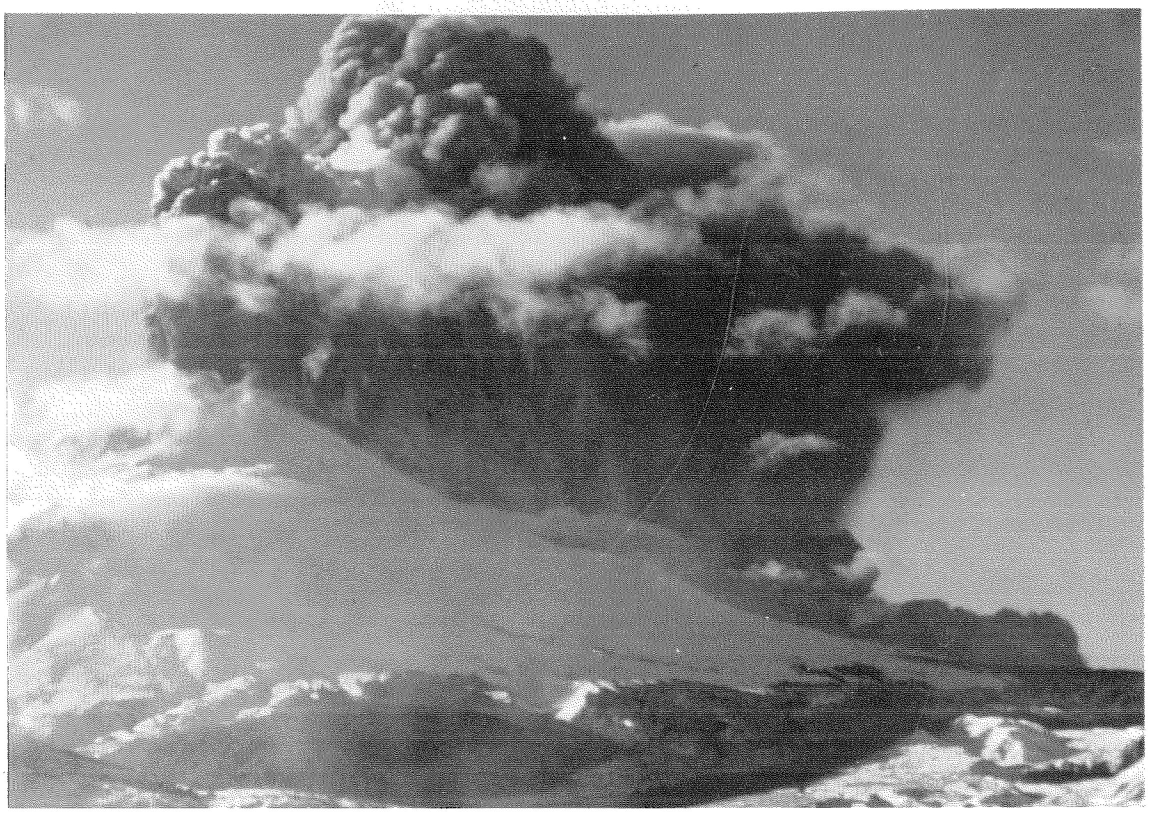
Many of the eruptions of Mount St. Helens in the spring of 1980, though not as large as the May 18 catastrophe, were nevertheless events of considerable magnitude. In this eruption in late March, a column of steam and ash emanates from the summit crater, ash curtains rain out of the plume, and dense flows of ash roll down the west flank of the mountain. The view is from Sue Kieffer's first camp at Coldwater 0 looking across the Toutle Valley in unusually fine weather. This and all photographs on pages 7, 8, and 9 were taken by Sue or Hugh Kieffer, both of whom are staff members of the United States Geological Survey.



The Blast at Mount St. Helens: What Happened?

by *Susan Werner Kieffer*

The memories of working at Mount St. Helens during March, April, and May of 1980 should be unscrambled by a psychologist; and only a poet or philosopher could describe the experiences as they deserve. In the chaos of those months, new and complicated logistical, political, and sociological experiences were superimposed on the challenge of geologically diagnosing a volcano that had awakened from a century of repose. Because a mere "what-happened" theory doesn't really convey the spirit of Mount St. Helens, I'm going to reminisce about some of the personal experiences there, as well as summarize my ideas about the May 18 lateral blast that initiated the cataclysmic eruption.

My involvement with Mount St. Helens began on March 27 when I received a phone call that the volcano had erupted after a week of seismic activity. Hoping to get to the mountain as quickly as possible to film and analyze the progression of eruption styles as the mountain awakened, I grabbed trusty camera equipment that had served me well in field work in Yellowstone and Iceland, packed film and camping gear, and arranged a temporary home for my son with trusted neighbors and friends. Although I have always worked alone in the field, when the difficulty of getting into the Cascades to a strange volcano in midwinter dawned on me as I packed, I asked my husband, Hugh, a planetary scientist specializing in infrared remote sensing, to accompany me; I could think of no better field partner than one with whom I've hiked, climbed, and camped for 15 years. My goal was to document the visual characteristics of the eruptions in order to model, at least qualitatively, the thermal evolution of the volcano.

Hugh and I were on a plane from Flagstaff to Phoenix to Portland (What state *is* that volcano in?) within two hours of hearing of the eruptions and, after dealing with numerous logistical difficulties, worked our way on lumbering roads to a high vantage point about 13 kilometers north of the mountain, a spot Hugh and I call "Coldwater 0." This point was outside any danger zone known from the geologic record — i.e., outside the zones of recorded ash flows, mud slides or floods, or heavy ash fall.

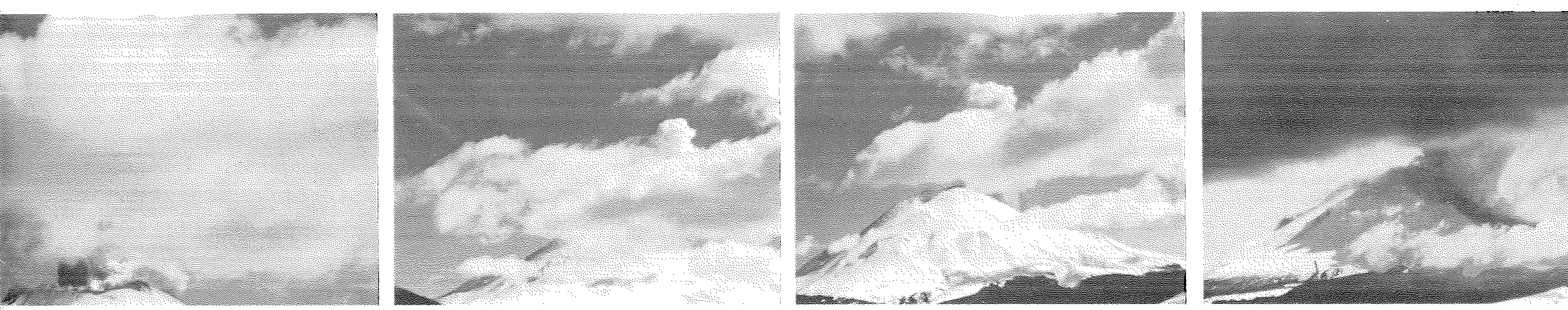
The weather was abominable. The mountain was invisible when we arrived, and, for all we knew, its eruptions were a media myth. We had no radio or other communications, and amused ourselves by making Dictaphone recordings of hailstones pounding on the tin roof of the Scout we were driving, and by learning a new skill — waddling on snowshoes without tripping over rain ponchos. (It was to become a handy skill when I later had to snowshoe out of our first U.S. Geological Survey camp in a light ashfall.) Map-reading and compass use became more than academic skills as we calculated which way to orient our tent for a good view in case the mountain should ever become visible.

We were rewarded by a beautiful sunrise view of Mount St. Helens from our sleeping bags the next morning. Three minutes after we first glimpsed the mountain, a wisp of steam appeared at the summit. Grabbing the cameras out of the foot of the sleeping bags, we jumped into the snow — in time to film a small, geyser-like eruption. An hour later, in beautifully clear weather, one of the biggest eruptions of the March-April sequence occurred: Ash-laden steam rose thousands of feet above the summit, ash-curtains rained out of the plume, and dense flows of ash rolled down the west slope of the mountain. This was indeed too good to be true — I had once spent six consecutive days at Yellowstone trying unsuccessfully to film a single ornery geyser eruption, and here, less than three days since we left Flagstaff, I had "trapped" a volcanic eruption beyond my greatest hopes.

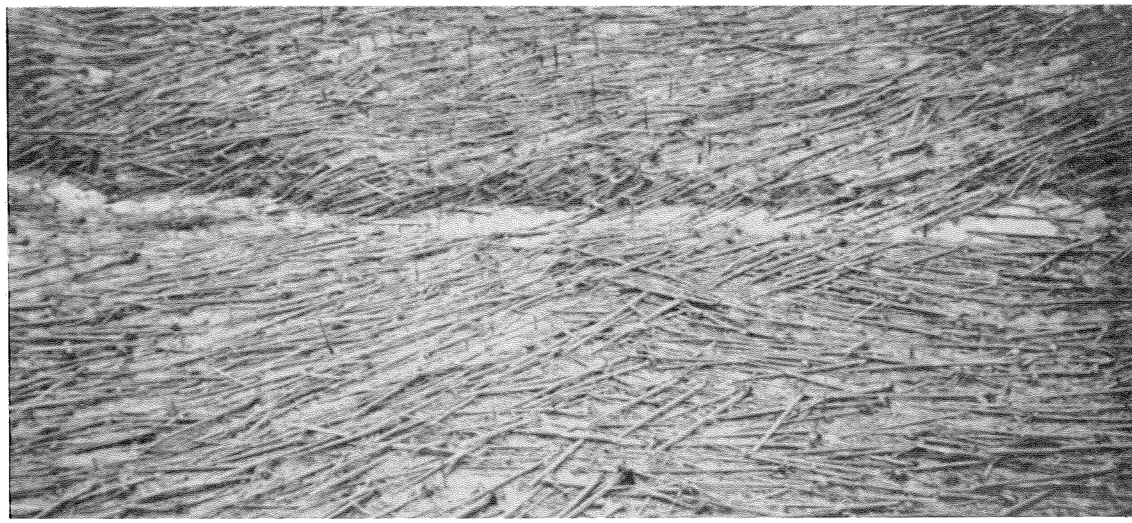
Two hours later easy times ended for good, as Pacific Northwest fog and clouds rolled in for spring residence. Fortunately for Hugh, the U.S.G.S. asked him to take charge of coordination of infrared and remote sensing activities of the mountain, and he departed for Survey Headquarters in Vancouver (Washington). I moved to Coldwater 1, a tent station that had been established for the U.S.G.S. while Hugh and I were at our first location. The station was put in for Survey observers by the U.S. Forest Service. There was a remarkable, instantaneous, and continuing cooperation between the Forest Service and the Geological Survey during very stressful times, and many new interagency friendships were formed from the long hours of hard work together. There was, however, initially at least one weak link: The Forest Service took me to Coldwater 1 in a snow-cat about 9:00 one snowy night, dropped me off, and promised to return each day with food and water. In the chaos that ensued, they not only forgot to return with the food and water, but forgot where the campsite was. After a few too many days on K-rations and no success in getting either the U.S.G.S. or Forest Service to find me, bad weather and blowing ash forced me to snowshoe out.

While living alone at Coldwaters 0 and 1, I worked out a schematic cross-section for the edifice of the volcano and its new crater, and a scenario for its thermal evolution, in blissful ignorance of any geology of the mountain, unconstrained by any data that conflicted with my own, and without any colleagues or professors to hassle me about my ideas — indeed a privileged time in life. The March-April eruptions were shallow-seated water — or phreatic — eruptions emanating from a breccia-filled conduit that had been created during the initial eruption of March 27. Most eruptions were indeed very geyser-like: They had a rough periodicity, at least for a day or two at a time; some were even comparable in scale to Yellowstone geysers, and the fluid was water, not new magma. The eruptions differed in energy from real geyser eruptions because the steam was transporting ash from rocks that were being ground down in the conduit during eruptions. By mid-April, when these eruptions stopped, measurements of many observers suggested that the mountain was continuing to heat up, while simultaneously drying out.

When the eruptions stopped, Hugh and I went home to recover pieces of professional and family life that had been hastily dropped. During late April and May, the deformation network installed by the U.S.G.S. showed the north slope to be moving outward at a rate of several meters a day. It became a certainty that the north side of the



More typical filming conditions yielded view of the mountain through (left to right) "cut-off-the-top-of-the-plume" clouds, "cut-off-the-bottom-of-the-plume" clouds, "cut-off-the-middle-of-the-plume" clouds, and "sandwich-the-plume" clouds.



Approximately 500 km² of forest were destroyed by the blast of May 18. In this area, trees were uprooted (rootballs can be seen on the ends of the trunks), tops were snapped off, and all small limbs and needles were stripped and carried away. The deposit left by the blast is rich in organic material derived from the trees.

mountain was dangerous, and very likely to fail catastrophically.

Early on the morning of May 18, a magnitude-5 earthquake triggered the failure of the north slope. About one cubic kilometer of rock, forest, and glacial ice detached from the north face and slid downhill into the drainage of the North Fork of the Toutle River. A lateral "blast" of vapor and rock blew out through the scarp formed by the landslide. This lateral blast was the initial stage of magmatic activity at the mountain. Within minutes of the triggering, an eruption column soared nearly 100,000 feet into the atmosphere, and over the next two days the volcano erupted about one cubic kilometer of fresh magmatic ash.

Hugh and I returned to Vancouver on May 19, saddened by the loss of a valued Survey colleague and friend who had been on duty at Coldwater 2, and stunned by the magnitude of the lateral blast that had triggered the eruption. There was no indication of such an event in the existing geologic record. Perhaps they are one-time events in the life of a volcano; we now know, however, that even if they are recurrent, the deposits left are too thin and fragile to survive long or obviously.

The lateral blast sent an atmospheric pressure wave throughout the Pacific Northwest. Peak-to-peak amplitudes up to nearly 1000 pascals had been recorded 54 kilometers from the mountain. These barograph records, reports of audible sounds hundreds of miles to the north, and a large seismic signal from the eruption, fueled speculation that a bomb-like event had occurred, a notion that has plagued volcanology for too long. One of my most time-consuming jobs that first week was dealing with the many military agencies and contractors who were interested in the possibility of a large bomb-like event, including postulated radiation fields. I made some initial energy estimates, based on scaling tree-blowdown dimensions, mass excavation and air-pressure wave propagation; Jack Reed of Sandia Corporation has subsequently refined all available barograph data and infers an effective energy of 1 to 10 megatons (Mt) for the source of the air pressure wave. It is not surprising that there was a military interest in this event.

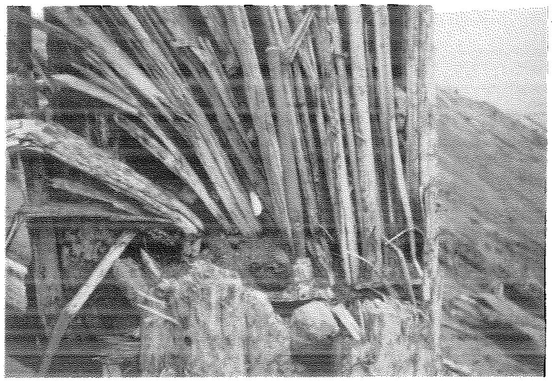
It was immediately clear from field observations that the damage had not been produced by a bomb-like air shock. Close inspection of trees revealed a record of extended pummeling and abrasion by large and small shrapnel. The blast was a dense multiphase wind, a flowing vapor containing ash, rocks, soil, magma, and tree fragments.

Except in the immediate vicinity of the summit of the mountain, the blast deposit was nowhere very thick. At my former campsite, 13 kilometers from the mountain, it was about a meter deep. Rain and wind quickly erode this fine-grained material, and in many places, particularly on slopes and hilltops, it is already gone. When the downed trees decay, the geologic record of the lateral blast will largely be obliterated. Thus, even if such events were to occur more than once in the life of a volcano, we cannot rely on the geologic record to define a hazard zone for lateral blasts.

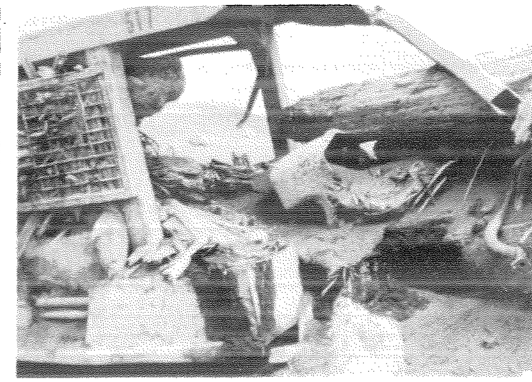
I decided that much of the story of the flow dynamics during the blast was preserved in the pattern of tree blowdown and did reconnaissance sampling of tree damage and blast deposit characteristics at about 20 sites within the devastated area in order to describe the material in the blast and the force that it exerted. From field work and then later work with air photos, I have constructed a detailed map that shows the limits of the devastated area and patterns of downed trees within it.

The forest was devastated through a sector of more than 180° close to the vent (which was

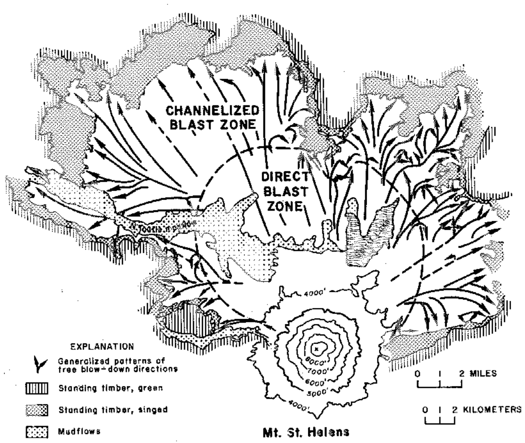
A close look at details of tree damage from the May 18 event shows sandblasting of the trunk on the upwind (left) side and deformation of the splinters on the downwind side.



Pumice and sand were driven deep into trees that had been broken, stripped of bark, and sandblasted – evidence that the devastation was not produced by an atmospheric shock but by a particulate-laden wind of considerable duration.



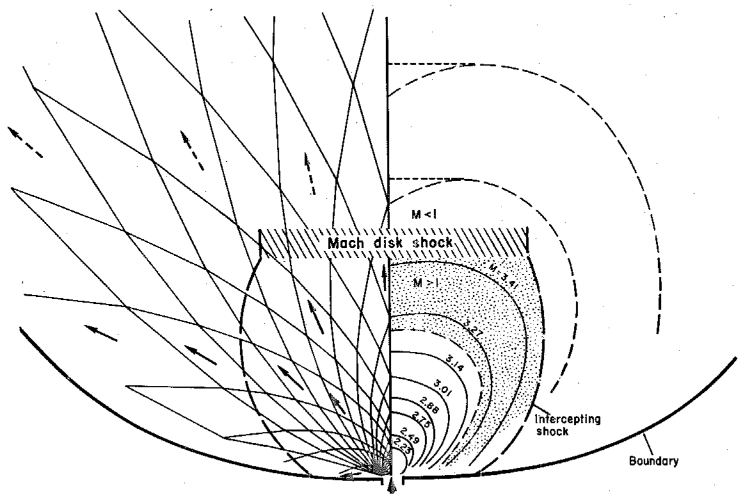
Sample collectors were unexpectedly provided by the Weyerhauser Lumbering Company. Here, about 10 km from the volcano, the blast was still carrying material a meter in dimension.



A map of the devastated area and flow streamlines (arrowed vectors) as they were indicated by alignment of fallen trees. The boundary between the direct and channelized blast zones is shown by the short broken lines.

about one kilometer north of the old summit of the mountain). The southern boundary of the area is approximately east-west near the vent, and sweeps to the northeast and northwest as it diverges from the crater. In a broad sense, the pattern of downed trees shows three major irregularly shaped zones: (1) an inner zone that I have termed the "direct blast zone," in which the flow was approximately radial from the volcano and was relatively undeflected by large topographic features, such as the Coldwater Creek drainage; (2) an outer zone, which I have termed the "channelized blast zone," in which the flow followed or was deflected by the local topography; and (3) a border zone of standing brown trees called the "singed zone." It is a notable point that this singed zone is widest where the terrain that the blast was approaching slopes uphill, and thinnest where the terrain slopes downhill. Transitions from totally devastated trees into the singed zone, and from the singed zone into green forest are remarkably sharp.

The model that I've developed attempts to account for many of the eyewitness observations of the flow and for features of the devastated area. Without regard to the classic volcanological classifications of eruptions as phreatic, magmatic, or phreatomagmatic, I simply describe the fluid in my model reservoir in terms of its initial pressure, temperature, and average mass ratio of solid to vapor phases. The question appropriate to St. Helens, simply stated, then, is "What happens when a complex multiphase fluid, at rest in a reservoir under pressure, is suddenly exposed to a world at much lower pressure?" Although the model can easily be scaled, for the sake of definiteness I have taken the initial pressure in the reservoir as 125 bars (the pressure appropriate to 650 meters of rock overlying the reservoir) and the initial temperature as 600 K or 327°C. This temperature probably seems surprisingly cool to anyone who is thinking of red-hot, incandescent magma. It happens to be the saturation temperature of pure water at 125 bars and is a reasonable number to assume *a priori*. It could also be thought of as an average temperature for a complex mixture which, after it had traveled only a short distance, contained material ranging from the temperature at which dacite (the volcanic rock present in the May 18 eruption) begins to melt (perhaps 1000 K) to the freezing temperature of glacier ice entrained (carried along) in the flow. I assume that the mass ratio of rock to steam in the part of the mountain that blew away was 25:1, and model the fluid as a "pseudogas" in which heat was continually transferred from fine-grained solids to the expanding vapor. Reasonable changes in assumed initial pressure, temperature, and solid-to-vapor mass ratio would not qualita-



Kieffer constructed this map view (a horizontal section through the flow) of the flow field according to the model of blast dynamics. All length dimensions, x and y, are normalized to the vent diameter. To ease numerical computation problems, the exit Mach number of the flow is assumed to have been 1.02 instead of the sonic Mach Number, 1.00. The model is symmetric about the axis of vent, so it is split into two halves here for conciseness. On the left, the mathematical characteristics (the expansion waves) of the solution are shown as thin lines radiating from the corner of the vent. The boundary of the flow is assumed to have been at constant pressure, 0.87 bars. The peripheral intercepting shock formed by the reflection of the expansion waves from this boundary is shown as the dashed line. Note how the reflection of the expansion waves deflects the boundary of the flow away from its original expansion angle of 96°. Flow directions are shown by representative arrows, solid within the zone where the model is strictly valid, dashed in the zone where the model is extrapolated across the shock waves. On the right half of the figure, contours of constant Mach Number M and, therefore, constant pressure (P/P_o), temperature (T/T_o), and density (ρ/ρ_o) are shown. Velocities are given implicitly by the Mach numbers. Each contour is labeled by the value of the Mach number, M. The approximate location of the Mach disk shock is shown; it separates the inner region of supersonic flow, for which the model is valid, from the outer subsonic zone in which no solution has been obtained.

tively alter the conclusions. The sound speed (an important parameter in fluid dynamics calculations) of the postulated reservoir mixture was 105 meters per second (m/s), about ⅓ the speed of sound in air.

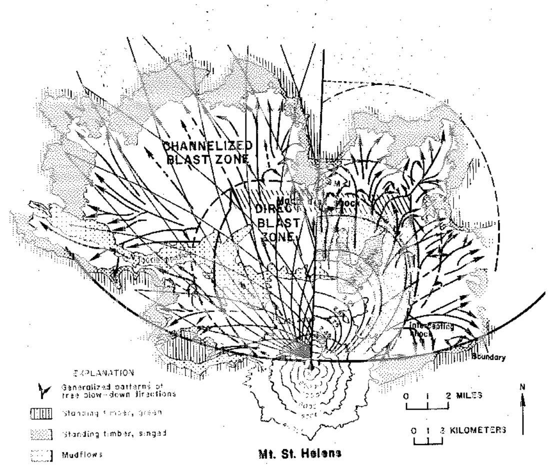
According to fluid dynamics theory, as material in a high-pressure reservoir flows from the reservoir through a vent into a much lower pressure atmosphere, it expands supersonically. My model attempts to define the supersonic flow characteristics, the transition from supersonic to subsonic conditions, and the thermodynamic properties of the flow as it expanded into the northern sector of the area surrounding the volcano. Let us try to keep the predictions of the model separate from inferences about the blast by first considering just the mathematical features of the model shown above.

According to the model, the fluid would have expanded around the corners of the vent through the two expansion waves, called "rarefaction waves." (See left half of figure.) Very close to the vent, where the rarefaction waves do not intersect, the initial turning angle of the flow would have been 96°. However, this turning angle would have been altered by processes occurring within the flow as the expansion waves crossed the flow and reflected off the boundary between the flow and the atmosphere. (This boundary is assumed to be an isobar at atmospheric pressure.) The expansion waves would have reflected as compression waves that piled up into weak compressive shock waves, called "intercepting shocks" in the aeronautics literature, because they intercept the initial expansion waves. The intercepting shocks would have coalesced across the flow into a stronger shock, called a "Mach disk shock." The boundary reflection of the initial expansion waves into compressive waves would have deflected the flow away from the initial 96° divergence angle into a more circular arc.

Pressure, temperature, velocity, and density in the flow can be obtained from the mathematical solution by contouring across the expansion and compression waves. Values are shown on the right half of the figure. Consider first the pressure distribution normalized to the reservoir pressure of 125 bars. According to the model, as material flowed from the vent, the pressure would have initially decreased from the reservoir pressure. Material that flowed out into lateral parts of the blast field would have been kept from dropping to extremely low pressure by compression waves reflected back into the flow from the atmospheric boundary, but material that traveled more directly in line with the vent would have overexpanded because of the extreme divergence of the flow; as a result, the pressure would have dropped *below* atmospheric near the axis of the flow (the gray zone of the figure). Although the surrounding atmosphere would have tried to converge into this low pressure region, it could not have eliminated the sub-atmospheric pressure zone because of the finite width of the rarefaction waves. However, at the Mach disk shock the reflected compression waves would have driven the pressure back up toward the ambient atmospheric pressure, while simultaneously the flow velocities would have dropped from supersonic to subsonic.

According to the model, temperatures throughout the flow would have remained remarkably high because of the buffering effect of the large fraction of solids contained in the flowing vapor. Density would have decreased rapidly, not only because of areal expansion, but also because of high temperatures maintained by entrainment of solid phases.

The model can be applied to Mount St. Helens



Here the blast model is superimposed on the map of the devastated area. For this superposition, the vent diameter Kieffer used was 1 kilometer; the vent was placed at the 8,000 ft. contour north of the old summit of the mountain and was oriented 5° east of north.

by the simple choice of a vent size, position, and orientation. The figure above shows the theoretical model superimposed on the map of the devastated area. For application of the model, I have taken the vent diameter as 1 km, the position as the 7000′ contour on the old map of the mountain (about 1 km north of the old summit), and have oriented the vent about 5° east of north. These are the only variables in the model — there are no arbitrary fitting parameters. What does the model explain about the blast?

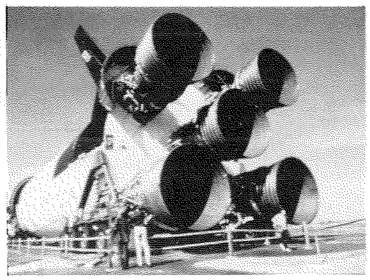
According to the model, the angle of divergence of the flow near the vent was more than 90°, in agreement with the shape inferred from geologic mapping. This angle was determined mainly by the initial reservoir overpressure relative to the ambient atmospheric pressure, and secondarily, by the thermodynamic characteristics of the multiphase fluid, namely, the solid-to-vapor mass ratio. The angle of divergence changed with distance from the vent due to the restraint of the atmosphere; at the boundary of the flow, the expansion waves reflected back into the flow as compression waves. These reflections produced the "swept-wing" shape so obvious on maps of the devastated area. The model shape on the west, south and east is in good agreement with the blast flow boundary as marked by the singed zone. There were a number of people who survived the blast (for example, in the area between Goat Mountain and the Toutle drainages, where the flow was initially headed straight toward them) who are alive today only because of these internal reflections of the waves.

According to the model, temperatures in the flow ranged from about 600 K (327°C) at the vent to approximately 480 K (207°C) at the fringe of the devastated area. These model temperatures are in remarkable agreement with temperatures inferred by a group of Sandia scientists working on degradation of plastics from vehicles in the area. Temperatures were within 20 percent of the initial reservoir temperature throughout the devastated area because of the buffering content of the entrained solid phases.

The model predicts that there was a stable low-pressure core within the flow, extending roughly from the North Toutle drainage 6 kilometers north of the vent, to the high country near the Dome 11 kilometers to the north, and extending west and east from Castle Creek to Independence Pass, a distance of about 15 kilometers. It is very difficult to test this hypothesis of the low-pressure core. One eyewitness told me that when a group went to search for a friend lost in the blast, they found that windows in his car had been blown *out,* rather than *in,* an observation at least consistent with the postulate that the pressure outside of the car was lower than inside. With thermal experiments, Sandia workers have been able to duplicate general damage to plastics, in particular the formation of bubbles, but they have not been able to reproduce the large size of bubbles found in the plastics in the blast area. Pressure reduction may account for this observation.

The model proposes that the flow was initially sonic at the vent, with a flow velocity equal to the sonic velocity of the multiphase fluid, about 100 m/s. It was supersonic beyond the vent, with the velocity increasing with distance, probably to several hundred meters per second. The model does not account for changes in flow velocity due to the changing topography or to viscous dissipation, and I think that in general my velocities are upper limits on actual velocities. However, even in the fastest parts of the flow, in front of the Mach disk shock, flow velocities were still less than the speed of sound in air (340 m/s) so that, in general, atmospheric shock waves were not generated, a conclusion that agrees with observations.

According to the model, there was a Mach disk shock about 11 km north of the vent; flow velocities would have dropped from supersonic to subsonic across this shock, causing the pressure to rise. The calculated positions of the Mach disk shock and the lateral intercepting shocks coincide with the transition from the direct blast zone to the channelized blast zone inferred from the tree data, and I propose that this was the boundary between supersonic and subsonic flow regimes. The generally radial nature of the streamlines in the direct blast zone, in spite of large topographic features, and the more topographically controlled shape of the streamlines within the channelized blast zone are consistent with supersonic and subsonic flow, respectively. The abundant occurrence of tree blowdown reversals in the direct blast zone and their rarity in the channelized blast zone is also consistent with the two different flow regimes.



This NASA photograph gives an unusual view of the bottom of a Saturn 5 rocket, showing its five F-1 motors and how they dwarf the two people in the foreground.

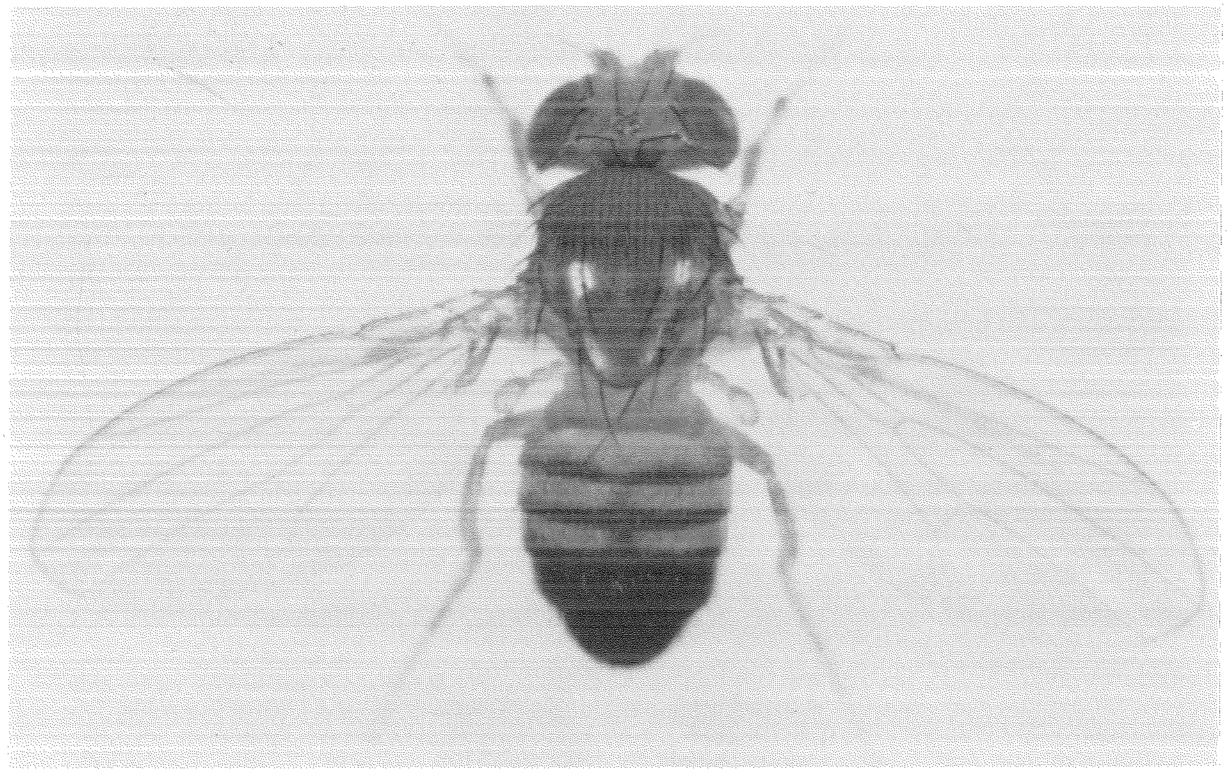
The model also suggests the reason for the peculiar, sudden transitions between the devastated area, the singed zone, and the undamaged forest and, I believe, may allow prediction of the extent of such blast zones based on the following reasoning. The density of the flow decreased continuously with distance from the vent — I have ignored the effect of internal shocks for this calculation. About 24 km from the volcano, the density of the flow was about 0.002 grams per cubic centimeter (g/cm^3), twice the normal atmospheric density. Remembering that this is a steady-state flow model and applies to the flow after passage of the initial material out of the vent and across the land, it is reasonable to suppose that the density of the atmosphere into which the flow was moving was two, three, or even five times normal because of dust-ladening. Thus, I propose that the blast expanded until it was less dense than the atmosphere, and that it simply ramped up into the wild blue yonder, still going at a fairly high velocity and probably still capable of knocking over trees, but, because of its buoyancy, no longer able to stay on the ground and cause the devastation. In the vivid lumberjack language of the Northwest, several eyewitnesses described the flow rushing toward them and then stopping: "It just stood up." The singed zone then represents the zone across which the blast was rising, yet was still close enough to the ground to scorch the trees. Where the flow had to climb uphill parallel to the trees, the singed zone is thicker than where the slope dropped away below it. The model contours give a good fit to the blast zone size on the north, if one assumes that atmospheric density was twice normal density.

The limits of the devastated area were determined on the north by the flow buoyancy, and on the west, south, and east by the internal dynamics of expansion. Calculation of the position of internal waves and of the density of the flow with distance from the vent will ultimately give us the way to predict hazard zones for such blasts, a tool necessary because the deposits of such blasts are not left for long in the geologic record. Limiting factors for predictive accuracy will be our ability to guess reservoir conditions and our knowledge of processes occurring at the boundary of the flow with the atmosphere.

The model also suggests why the flow advanced soundlessly, in spite of the fact that it was destroying a forest and hurling boulders across the land. Even at its distant boundaries, the flow was relatively dense because many small solid particles were still entrained in the vapor. Calculation of the attenuation of sound in a particulate cloud with plausible densities and particle sizes for the blast cloud shows that the attenuation must have been three or four orders of magnitude greater than the attenuation of sound in air. Thus, whereas the sounds of falling trees, or of flying, impacting rocks would be carried for kilometers in clear air, such sounds would have been attenuated within a few meters or tens of meters within the blast cloud. Some survivors reported that they could not even hear each other shouting as the cloud overtook them; others reported an eerie silence as the trees fell down around them.

Finally, the mass flux and thermal flux during the blast can be calculated from this model. The flow rate was controlled at the vent, where the flow was choked. For the initial conditions of the model, the maximum mass flux was 10^4 grams per second per square centimeter (g/s/cm^2). The thermal flux or power per unit area was 2.5 megawatts per square centimeter (Mwatts/cm^2). The total energy of the blast was 24 Mt, of which 7 Mt was dissipated during the blast itself, and the remaining 17 Mt was dissipated during the almost simultaneous condensation of the steam in the blast and subsequent cooling of steam and rock to ambient temperature in the weeks following May 18.

It is difficult to comprehend the magnitude of the lateral blast, so let me conclude with a comparison appropriate for Caltech readers. One of the most impressive displays of power created by humans was the launching of a Saturn 5 rocket that carried Apollo astronauts to the moon. The first stage of the Advanced Saturn 5 consisted of five F-1 liquid-oxygen/kerosene motors. The mass/flux area at the exit of an F-1 motor was about 25 g/s/cm^2; that of the lateral blast at the Mount St. Helens vent was 240 times as great. The power per unit area of the F-1 motors was approximately 0.8 Mwatt/cm^2; that of the lateral blast was three times greater. The Saturn 5 power was delivered over five rockets covering roughly 50 m^2; the power at Mount St. Helens flowed out of a vent more than 2,000 times this area. The total power of the five Saturn 5 motors was about 4×10^5 megawatts; that of the blast was nearly 16,000 times as great. The thrust of the Saturn 5 was 7.5 million pounds (3.3×10^7 N); that of the blast was 10^5 greater. The lateral blast of May 18 was indeed an awesome event by both human and geologic standards. □



A Second Golden Age in Drosophila Research

by *Dennis Meredith*

How genes cause a single fertilized egg to almost magically erupt into a functioning, behaving creature is one of the central mysteries of biology. If we humans ever come to understand this stunning transformation and how it can sometimes go tragically awry, we will owe an enormous debt to a modest creature known as *Drosophila melanogaster*. Drosophila, better known as the fruit fly, is a delicate little insect about the size of a BB, usually found flitting about garbage cans. With its prismatic red eyes, its slim, translucent tan body, and its delicate many-veined wings, Drosophila is an aesthetically pleasing animal as insects go. And it is a benign creature. For instance, it is not really a "fruit" fly, for it does not feed on fruit like its fellow insect (but non-relative), the voracious Mediterranean fruit fly. Rather, Drosophila feeds on the yeast growing on rotting fruit. It is amiable, does not bite, and adapts well to captivity.

All these traits, plus the fact that it zips through its life cycle in a matter of ten days have made Drosophila ideal for the study of the genetics of development. Unlike bacteria, it is a full-fledged animal, with an embryonic stage of development, a full range of senses, and a repertoire of behaviors. However, it is a simpler animal than mammals such as the rat, and its genetics are easy for scientists to tinker with.

And tinker they have, using X rays and chemicals to produce over the decades tens of thousands of mutants — a weird, fascinating menagerie of flies possessing a huge array of precisely defined colors, deformities, or quirky behavior patterns. More than just oddities, these mutations represent experimental probes into the genetic machinery that yield valuable clues about how that machinery functions.

An indication of the value to science of little Drosophila is the number of research papers pub-

Fly Repository

Edward B. Lewis

One of the world's most important repositories for Drosophila mutants is little more than a 20-foot-long rack of stoppered half-pint milk bottles kept in a cool room in Caltech's Kerckhoff Laboratories of the Biological Sciences. From this room, 1,500 stocks of flies from the lab's collection of more than 1,500 strains are shipped annually throughout the world. They range from strange color mutants like ebony, to exotic malformed flies with curly wings, to odd behavioral mutants like "Drop-dead," which true to its name drops dead at the sound of a hand clap.

Business has been booming for this National Science Foundation-sponsored repository, with the number of shipments rising about 10 percent a year. The steady rise in demand is just another example of the boom in research using the flies during what the Drosophilists call "The Second Golden Age of Drosophila." The First Golden Age began in the early 1900s with Thomas Hunt Morgan's use of Drosophila to develop the basic laws of genetics. In the 1950s, however, interest in the flies as research subjects waned, as biologists turned to cheaper, rapidly multiplying bacteria to discover the basics of DNA structure and function. But the 1960s saw a resurgence of Drosophila work, as scientists became interested in the molecular genetics of development and behavior in higher organisms.

Presiding over Caltech's collection for the last three decades has been Professor Edward Lewis, dubbed "Lord of the Flies" by waggish colleagues. But the responsibility is a serious business, for the mutants in this repository can offer scientists studying them valuable clues to genetic processes. And the Caltech center is one of only two in this country; the other is at Bowling Green, Ohio.

Maintaining the stock of mutants is a tedious, exacting task, for even in a cool room, the little flies breed like . . . well . . . *flies*. Technicians must periodically extract the insects from the bottles, anesthetize them with ether, and use a microscope and a fine brush to sort out the best to restock another new colony. In the course of a year, keeping the stocks fresh means preparing more than 100,000 half-pint milk bottles, which were chosen originally for their convenient geometry and sturdiness. They are sterilized; partially filled with Lewis's fly-food recipe of agar, cornmeal, mold inhibitor, and sugar; and seeded with a puff of yeast. And so it has been for over half a century for billions and billions of Drosophila.

It's no fly-by-night business.

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lished worldwide on the creature — currently about a thousand per year and increasing. Caltech scientists have been pioneers in Drosophila research since 1928. That was when Nobel Prize-winning biologist Thomas Hunt Morgan first used the fly to develop some basic theories of genetics. Caltech remains a major center of Drosophila research, for besides studies of the fly by Caltech scientists, the Institute continues to operate the oldest repository for Drosophila mutants — one of two in this country. (See box above.)

The current studies at Caltech using Drosophila are a fascinating carnival of experiments and ideas, and a look at them offers an excellent insight into the fly's value to science. Caltech scientists produce mutants with exotic names like "Dunce" and "Shaker." They insert microscopic electrodes into fly nerve cells, perform fly brain transplants, monitor the faint mating call of the lovesick Drosophila, waft odors past instrumented flies, plunge fly wings into solutions of radioactive tracer, heat-shock fly larvae, and even set a computer to watch over the flies' scurryings. All these activities provide basic insight into how genes create organisms, but they also help us understand an enormous range of disease — including cancer, learning disabilities, birth defects, neuromuscular diseases, and sleep disorders.

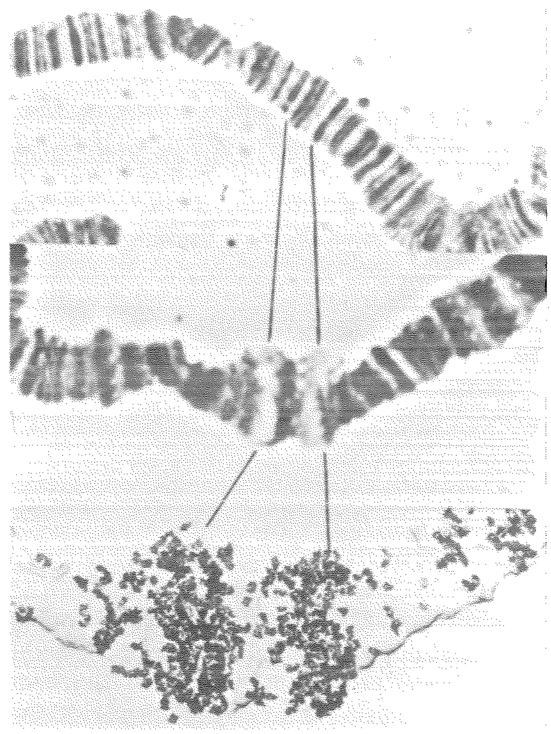
One veteran Drosophila researcher at Caltech is the Thomas Hunt Morgan Professor of Biology, Edward Lewis. Besides operating Caltech's Drosophila stock center for the last 30 years, Lewis has been studying the set of genes that control the fly's body segmentation. To trace these genes, Lewis has produced a wide range of mutant flies with altered body segments. Ensconced in bottles in his lab are wingless flies, four-winged flies, eight-legged flies, and four-legged flies. Some flies are too abnormal to even make it to adulthood. One mutant that consists of a head and a chain of thoracic segments dies as an embryo. By studying such mutants, Lewis has found a set of about ten genes, called the bithorax complex, that seems to code for body substances that somehow regulate body segmentation. If the bithorax complex is thought of as a series of switches, Lewis's strange mutants, about 500 so far, are animals in which various of these switches have been clicked on or off.

Lewis thinks of these genes as regulating other genes that actually produce the segments. What's exciting about this system, he says, is that it may provide a way of solving for the first time just what it is that regulatory genes make that affects other genes — whether it is a protein or an RNA. Nobody knows exactly what genes do to cause undifferentiated human cells to produce arms or legs or teeth or hair, so this system could offer an extremely valuable insight.

But what controls the "switches" of the bithorax complex? Lewis thinks he and his colleagues may have found an answer. By examining various mutants, his co-worker and wife, Pamela, discovered another gene, called Polycomb, that seems to control the bithorax region. Polycomb could be a "master regulator" — a gene which codes for a product that finds its way to the bithorax region and throws the correct switches to produce a normal fly. One clue, for instance, that Polycomb is a master regulatory gene, is that when the gene is removed by mutation, the bithorax genes are all unregulated, and all of the mutant's segments look like abdomens.

According to Lewis, the discovery of Polycomb is exciting because, although there are known cases of such genes controlling enzymes, there are no other examples of such a gene that controls development. Formerly, it was believed it would be years before such regulation would be understood, because nobody could trace its biochemistry. But in the Polycomb gene, scientists may have a tool that will aid them in coming to such understanding.

When genetic machinery is hard at work churning out proteins to create an organism, it is like a player piano, running through its roll of punched paper to spew out a song. To understand that machinery, biologists seek ways of freezing that genetic "paper roll" at certain points to "read the holes." Veteran Drosophila researcher Herschel Mitchell, professor of biology, has found that the development of wing hairs in Drosophila pupae offer a good model system for monitoring development. The 31,000 cells that make the hairs of a Drosophila wing all work conveniently in synchrony, and in a process that takes a mere 20 hours or so, all the hairs form and erupt at once, like a field of super-wheat springing up. To understand how this process operates, Mitchell and his colleagues carefully slit the pupal cases of flies at various points in hair development, clip off one wing, and immediately immerse it in a solution of radioactive amino acid, to label the proteins being produced. By separating these hundreds of proteins on an electrophoretic plate, the scientists produce a sort of snapshot of activity that tells what genes are turned off or on at any instant.



From the laboratory of Herschel Mitchell, a photo micrograph of a normal Drosophila chromosome (top). In the center, the same region on a chromosome from a fly that was given a heat shock and a pulse of tritiated uridine. Note the two puffs. The same chromosome is shown at the bottom after exposure to a photographic emulsion, and the silver grains appear as irregular black bodies in the scanning electron micrograph. There is rapid synthesis of RNA (messages for heat-shock proteins) at the puff sites.

Mitchell has used this technique to study various wing-hair mutants to understand the machinery of hair development, and has also embarked on studies of wing-hair abnormalities produced when pupae are subjected to heat shock. Such heat shock has been found to switch certain genes off and turn others on, producing abnormalities. In Drosophila, such abnormalities due to stress on an embryo are known as phenocopies. In humans, they are known as birth defects, and the two phenomena are thought to arise from the same basic mechanism of damage.

In numerous biochemical "snapshots" he has taken of the proteins of heat-shocked flies, Mitchell has found certain genes that are especially sensitive to heat shock during critical periods of development. He is continuing to study the machinery in hopes of gaining fundamental insights into the process that causes birth defects.

Basic research often yields surprising applications, and this has been the case with Mitchell's work. Cancer researchers have long known that heat may be used to kill cancer cells selectively. Recently, Mitchell and collaborators at other institutions have found that the mechanism of heat shock is quite similar in both flies and cancer cells. In both cases, the shock appears to wreck the coordination of the genetic machinery by switching genes on or off prematurely.

But there are weird complications. In Drosophila, a mild heat shock before the main shock appears to protect the larva, perhaps by turning on still other proteins that protect the embryo. Mitchell and his colleagues are currently working to understand the complexities of the heat-shock phenomenon.

Mitchell believes there may be a better chance of understanding heat shock in Drosophila than in cancer cells because researchers have better control over the Drosophila system. It is much more uniform, homogenous, and synchronous than are cancer cells.

The new techniques of genetic engineering are also being applied to studies of Drosophila by such researchers as Professor of Chemistry Norman Davidson and Assistant Professor of Biology Elliot Meyerowitz. Basically, these techniques consist of using enzymes and other biochemical tools to isolate genes and to insert them into bacterial factories, so that the genes can be produced in large amounts for structural analysis.

For example, Davidson and his colleagues are in the early stages of a long-term project to understand the control mechanisms for the genes producing the several kinds of muscle proteins in Drosophila — including actin, myosin, and tropomyosin. The genes for these various proteins are turned on and off in precisely regulated ways in different cells to produce the different muscles of Drosophila, such as crawling muscles and flight muscles. By using genetic engineering techniques to fish these genes out of Drosophila cells and figure out their structures, these researchers hope to learn how muscle formation — a basic process in all animals — operates. Similarly, Davidson and his colleagues have begun isolating and characterizing the various genes governing the formation of the proteins of the insect's tough outer covering, called the cuticle.

Assistant Professor of Biology Elliot Meyerowitz could be said to be studying the "genetics of glue." Using mutants and recombinant DNA techniques, he has set out to understand a gene in Drosophila that codes for a protein glue called SGS-3, which is spit out by the fruit fly larva to hold it in place while it metamorphoses into the adult fly. Normally, spying on an operating gene might be difficult, because the strands are almost too small to be seen, even by a microscope. However, obliging Drosophila possesses a set of chromosomes in its salivary gland that are gigantic by biological standards. Unlike the usual chromosomes, which consist of a couple of DNA strands lined up, the salivary gland chromosomes are 1,000 or so DNA strands thick. Easily visible on these giant chromosomes are dark bands, which act as landmarks in searching out genes. Also visible are active sites of genetic activity called "puffs," which, true to their name, swell up like popovers in a hot oven when the genes are actively transcribing RNA.

Meyerowitz is, in effect, "stalking" the gene for this glue protein. By experiments on the chromosomal puffs, he has found that a region known as 68C3,4 on one of the chromosomes is the site of the SGS-3 as well as other glue protein genes. Now, Meyerowitz is using X rays to produce a range of fly mutants whose DNA is altered at sites closer and closer to the glue gene. His aim is to discover just how close he can come to this gene, and still have it function properly. This game of genetic "chicken" is valuable, for it will help determine just how much of the gene is needed for the vital process of control. The mystery of how much genetic material is needed for controlling development is a major one. While scientists are now unsure, they do know that the fraction of genetic material needed to actually specify the structure of living things is small. The function of the overwhelming amount of DNA in living things is still a mystery, and a significant amount may be control DNA.

Besides narrowing down the vital areas of the glue gene, Meyerowitz is also using recombinant DNA techniques to analyze the DNA sequence of the gene to detect patterns that might be control signals.

The researchers discussed so far are puzzling out how the machinery of the genes operates to control development. Assistant Professor of Chemical Biology Carl Parker, however, is studying some of the biochemical cams and springs and levers and pulleys of the machinery itself. Specifically, he is interested in the enzyme "DNA-dependent RNA polymerase," which is the basic device that copies information from the genes for use by the cell. While many researchers have studied this enzyme in bacteria, Parker hopes to take his studies up the evolutionary scale to Drosophila, using the multitude of fly mutants that have been produced as a scientific proving ground to develop his theories. He will attempt to build in a test tube various working models of normal and mutant fly genetic machinery to discover how the components work. While many test tube studies have been done on the RNA polymerase machinery of viruses and humans, fly studies appear especially promising, says Parker. This is because the fly machinery appears to operate *in vitro* more realistically than do the human or virus test tube systems.

Parker is, incidentally, the latest addition to Caltech's Drosophila researchers, having arrived at the Institute but a few months ago. He was attracted, he says, because "the Drosophila research at Caltech is better now than any other place in the world. The huge range of work going on here makes the place most attractive."

While many researchers are concentrating on the genetics of the little fly's structure, others are taking advantage of the fact that Drosophila is a full-fledged *behaving* animal. For example, Seymour Benzer, the James G. Boswell Professor of Neuroscience, is a pioneer in Drosophila behavioral genetics. Current studies by him and his colleagues aim at tracing the genetic and physiologic basis of behavioral mutants.

Mark Tanouye in Benzer's group is studying Shaker mutants, which, as their name suggests, show uncontrollable trembling of legs and head parts. The defect in Shaker and other mutants may be analogous to various human inherited neuromuscular diseases, so the Caltech scientists believe that study of such mutants could lead to insights into these diseases.

Their studies have shown that the Shaker mutation appears to affect the electrochemical process in the nerve cell that triggers muscular contraction. By inserting electrodes into the "giant" axon of the fly — large nerve cells that are easily impaled — and recording action potentials, the Caltech researchers have narrowed the defect to a particular part of the nerve impulse mechanism known as the potassium channel. Basically, nerve impulses in both flies and humans consist of a wave of sodium inflow moving down the nerve cell, followed by a compensating wave of potassium outflow. The researchers have used drugs to selectively block different components of this process, and have implicated the potassium channel as defective. Also, using extremely delicate electrodes and recording procedures, Benzer's colleagues have managed to record the nerve signals in Shaker mutants to reveal electrical abnormalities in the cells.

From the laboratory of Seymour Benzer, a section (seen in a fluorescence microscope) of a section across a fly's head. The section was stained with one of the lab's monoclonal antibodies to reveal the fine structure of the fly's nervous system.

The fact that Shaker represents an abnormality in the potassium channel is important, because traditional biochemical techniques to study the structure of this channel are difficult, according to the scientists. While there are drugs that bind the sodium channel specifically and tightly, there are no such drugs for the potassium channel.

The other major mutant under study in Benzer's lab, called Dunce, does not learn to avoid odors associated with electric shocks, whereas normal flies can learn such avoidance. In their work with Dunce, the Caltech scientists recently experienced an example of the kind of serendipity that occurs when many scientists study the same organism. In discussions with researchers from the University of California at Davis, they discovered that the behaviorally defined Dunce mutant was affected in the same gene as another mutant with a biochemical abnormality in the enzyme cyclic AMP phosphodiesterase. This enzyme controls the breakdown of the substance cyclic AMP, a fundamental regulatory chemical in the body, which also seems vital to memory formation. Such breakdown is necessary to prevent undue buildup of cyclic AMP and thus, malfunction. Lawrence Kauvar of UC Davis and Caltech graduate student Sandra Shotwell are now tracing both Dunce's enzyme abnormality and its genetic abnormality.

Visiting Fairchild Scholar Obaid Siddiqi in Benzer's laboratory is studying the sense of smell, a poorly understood phenomenon at present. So far, mutant flies have been isolated that are behaviorally deficient in response to specific odors. These could be used to perform a "genetic dissection" of the spectrum of basic odor specificities. Siddiqi is testing normal and smell-insensitive mutant flies by inserting electrodes into the odor-sensing antennae, and wafting odors past the flies, while measuring the currents produced by the smell receptors.

In another highly promising project, Benzer and his colleague Shinobu Fujita are developing extremely specific stains for various parts of the fly's nervous system, using a new technique of producing monoclonal antibodies. This technique involves injecting mice with fly brains and then fusing sensitized spleen cells with myeloma cells to produce hybridoma clones, each producing a single antibody type. The many different antibodies thus obtained can be used to trace the development of the nervous system, perhaps ultimately detailing not only how the nerves connect, but how they got that way.

Besides having the ability to learn, Drosophila are like humans and other higher animals in that they possess a built-in daily rhythm of activity. For about 12 hours a day, they actively go about the business of feeding and mating, and for another 12 hours they "power down," sitting quietly. (Whether flies actually sleep, nobody knows.) Assistant Professor of Biology Ronald Konopka has been searching for the genetic and neurological basis of this cycle by studying "clock mutants" that have shorter or longer cycles, or are arrhythmic. For instance, a 19-hour mutant tends to settle into a natural cycle of 7 hours of activity and 12 hours of inactivity; a 29-hour mutant is active for 17 hours, inactive for 12. These mutations occur on a gene known as the *per* locus, and in collaboration with Meyerowitz, Konopka is currently trying to isolate the gene and clone it, producing huge numbers of copies to deduce its structure.

A fly-brain transplant recently led to one important discovery about the fruit fly clock. After the brain from a short-period mutant fly had been carefully removed and inserted into the abdomen of an arrhythmic mutant, the two-brained fly assumed a short-period cycle. This was the first proof that a humoral substance produced by the brain governs periodicity.

Konopka and collaborators at Brandeis University are now puzzling over the surprising discovery that the fly's clock also governs the rhythm of the animal's courtship song. This modest ditty, sung by the male to a likely female, consists of a rapid series of low-frequency clicks made by vibrating the wings. By recording this song in normal flies, the Brandeis University researchers discovered that the interval between clicks naturally lengthens and shortens in an oscillation that takes about a minute. However, in the short-period mutant this oscillation takes about 40 seconds, and in the long-period mutant, approximately 76 seconds.

Still other long-period mutants are slow learners in the game of Drosophila mating. When normal amorous male flies are rejected by a female, they quickly learn to seek companionship elsewhere. However, certain long-period mutants are persistent-but-dumb suitors — unable to learn to "buzz off" when rejected.

The production of "mosaic" flies is one technique Konopka and his colleagues use to find out which brain cells are responsible for such behavior. Mosaics are insects possessing genetically different cells in the same animal. These cells can show the scientists which fly-brain cells are the key clock cells. Thus, if a mosaic fly with short-period cells and arrhythmic cells assumes a short-period rhythm, the scientists know that the brain clock cells are of the short-period type. The scientists program special marker genes into these mosaic cells that enable them to use a little detective work to narrow down the exact brain cells responsible for the clock.

Incidentally, the onerous duty of figuring out the day-night cycles of hundreds of flies is not heaped upon some poor student; it has been taken over by a fly-watching computer. A fly to be monitored is first placed inside a small tube with a supply of food. The tube is then placed in a rack, along with hundreds of others within a special temperature- and light-controlled chamber. Beneath each tube are photoelectric cells, and each time the fly moves over a cell, the computer counts the pass. By statistically analyzing the number of passes, the computer can trace the activity cycle of the fly.

From these studies, Konopka has advanced a tentative theory to explain the phenomenon of circadian rhythm, found throughout the animal kingdom, including man. According to this theory, the clock genes may code for an ion pump found in the cell membrane. This pump may operate during the day, moving the ion out of the cell like the sand running through an hourglass. By nightfall, the ion is exhausted, activity slows, and the pump begins a recharge cycle. Slow cycles like the circadian rhythm may employ slow pump systems; fast cycles such as the courtship song may employ fast pumps.

These of course, have been only brief descriptions of the sophisticated studies being carried out using Drosophila. However, they do show clearly what this little fly means to the science of biology.

So, while that tiny fly flitting about an overripe peach or a past-its-prime banana may be a nuisance in your kitchen, in the laboratory it's a treasure. □

ALUMNI FLIGHTS ABROAD 1980–1981

This program of tours, originally planned for alumni of Harvard, Yale, Princeton, and M.I.T., is now open to alumni of California Institute of Technology as well as certain other distinguished colleges and universities. Begun in 1965 and now in its sixteenth year, it is designed for educated and intelligent travelers and planned for persons who might normally prefer to travel independently, visiting distant lands and regions where it is advantageous to travel as a group.

The program offers a wide choice of journeys to some of the most interesting and unusual parts of the world, including Japan and the Far East; Central Asia, from the Khyber Pass to the Taj Mahal and the Himalayas of Nepal; the surprising world of South India; the islands of the East, from Java and Sumatra to Borneo and Ceylon; the treasures of ancient Egypt, the world of antiquity in Greece and Asia Minor; East Africa and Islands of the Seychelles; New Guinea; the South Pacific; the Galapagos and South America; and more.

REALMS OF ANTIQUITY: A newly- expanded program of itineraries, ranging from 15 to 35 days, offers an even wider range of the archaeological treasures of classical antiquity in Greece, Asia Minor and the Aegean, as well as the ancient Greek cities on the island of Sicily, the ruins of Carthage and Roman cities of North Africa, and a comprehensive and authoritative survey of the civilization of ancient Egypt, along the Nile Valley from Cairo and Meidum as far as Abu Simbel near the border of the Sudan. This is one of the most complete and far-ranging programs ever offered to the civilizations and cities of the ancient world, including sites such as Aphrodisias, Didyma, Aspendos, Miletus and the Hittite citadel of Hattusas, as well as Athens, Troy, Mycenae, Pergamum, Crete and a host of other cities and islands of classical antiquity. The programs in Egypt offer an unusually comprehensive and perceptive view of the civilization of ancient Egypt and the antiquities of the Nile Valley, and include as well a visit to the collection of Egyptian antiquities in the British Museum in London, with the Rosetta Stone.

SOUTH AMERICA and THE GALAPAGOS: A choice of itineraries of from 12 to 29 days, including a cruise among the islands of the Galapagos, the jungle of the Amazon, the Nazca Lines and the desert of southern Peru, the ancient civilizations of the Andes from Machu Picchu to Tiahuanaco near Lake Titicaca, the great colonial cities of the conquistadores, the futuristic city of Brasilia, Iguassu Falls, the snow-capped peaks of the Andes and other sights of unusual interest.

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THE SOUTH PACIFIC and NEW GUINEA: A primitive and beautiful land unfolds in the 22-day EXPEDITION TO NEW GUINEA, a rare glimpse into a vanishing world of Stone Age tribes and customs. Includes the famous Highlands of New Guinea, with Sing Sings and tribal cultures and customs, and an exploration of the remote tribal villages of the Sepik and Karawari Rivers and the vast Sepik Plain, as well as the North Coast at Madang and Wewak and the beautiful volcanic island of New Britain with the Baining Fire Dancers. To the south, the island continent of Australia and the islands of New Zealand are covered by the SOUTH PACIFIC, 28 days, unfolding a world of Maori villages, boiling geysers, fiords and snow-capped mountains, ski plane flights over glacier snows, jet boat rides, sheep ranches, penguins, the Australian "outback," historic convict settlements from the days of Charles Dickens, and the Great Barrier Reef. Optional visits can also be made to other islands of the southern Pacific, such as Fiji and Tahiti.

CENTRAL ASIA and THE HIMALAYAS: An expanded program of three itineraries, from 24 to 29 days, explores north and central India and the romantic world of the Moghul Empire, the interesting and surprising world of south India, the remote mountain kingdom of Nepal, and the untamed Northwest Frontier at Peshawar and the Punjab in Pakistan. Includes the Khyber Pass, towering Moghul forts, intricately sculptured temples, lavish palaces, historic gardens, the teeming banks of the Ganges, holy cities and picturesque villages, and the splendor of the Taj Mahal, as well as tropical lagoons and canals, ancient Portuguese churches, the snow-capped peaks of the Himalayas along the roof of the world, and hotels which once were palaces of maharajas.

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Prices range from $2,350 to $4,500 from U.S. points of departure. Air travel is on regularly scheduled flights of major airlines, utilizing reduced fares which save up to $600.00 and more over normal fares. Fully descriptive brochures are available, giving itineraries in detail and listing departure dates, hotels, individual tour rates and other information. For full details contact:

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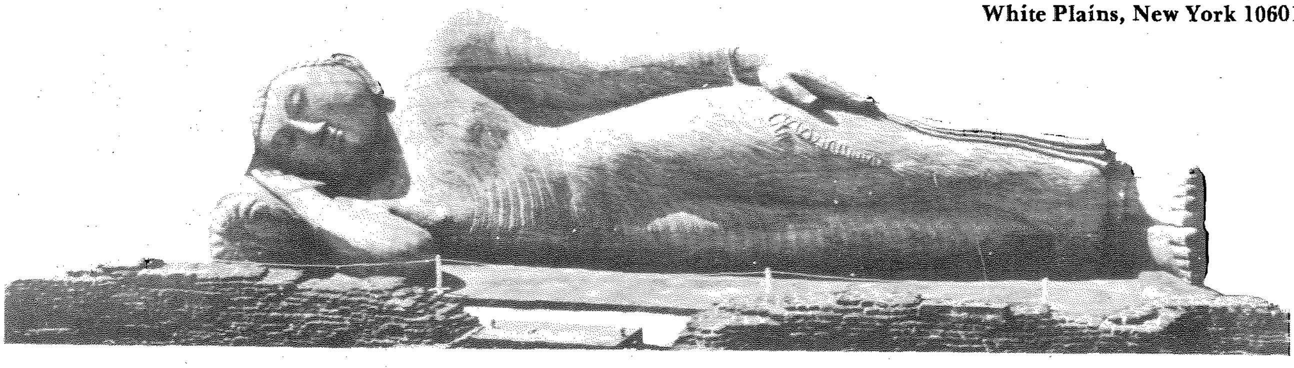

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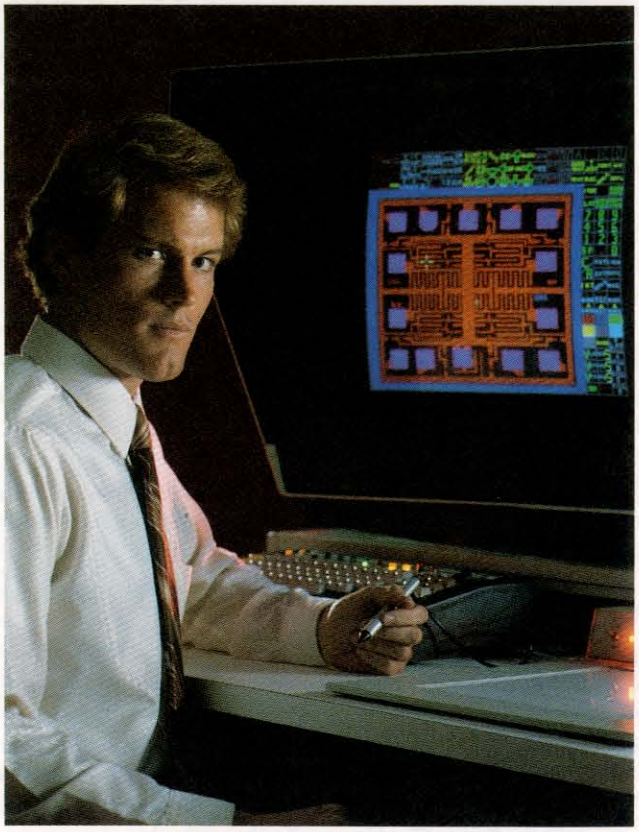

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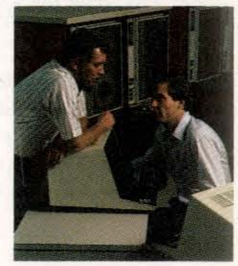

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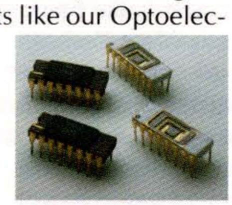

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National Security and International Crises:

The World in the 1980s

by *Harold Brown*

We have only recently entered a new decade — that of the 1980s. That makes this a natural time to evaluate the present international situation and what we may expect during that decade. To understand the implications of this situation for the United States, however, it is useful first to look back on how the international situation has changed over the past few decades, how those changes have affected us, and how we need to react to changed circumstances in the future.

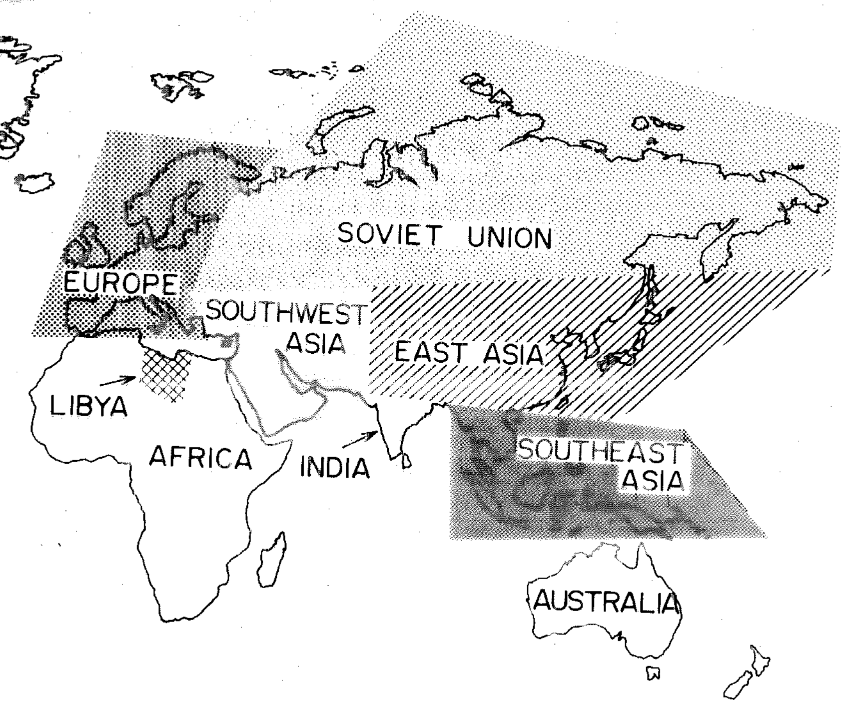
The international interests of the United States since World War II have actually been rather constant. They include the need to avoid international chaos, to contain Soviet expansionism, and to advance human rights and political independence throughout the world. Each of those particular interests fits into a more general goal of preserving a world in which all nations are free to develop politically and economically along lines that they themselves determine and to exchange both goods and ideas on mutually acceptable terms.

The principle of staving off chaos was turned into practice even before the end of World War II through various relief efforts that were funded almost entirely by the United States, and shortly after World War II by the Marshall Plan and other economic reconstruction programs. The advancement of human rights and political independence were strongly expressed as U.S. goals immediately after the war when we brought pressure on those of our allies that had colonial empires to grant independence to their component parts. The United States itself gave independence to the Philippines, and whatever reservations one may hold about political developments there since, I have no doubt it was the correct thing to do. The British, the French, and the Dutch remember well — and not always kindly — our urgings at that time, but there is little doubt that those nations now are, and consider themselves, better off for having dissolved the bonds of their respective empires.

The diffusion of political power to a large number of countries was probably inevitable, whatever complexities and difficulties it may have created. The Marshall Plan itself, though the Soviets were invited to participate in it and refused, was designed in substantial part to contain Soviet expansionism by strengthening Europe economically. Subsequently, the Truman Doctrine, the formation of NATO (the North American Treaty Organization), and the encouragement of Japan and the Federal Republic of Germany to participate in their own defense were all elements in a strategy of resisting Soviet expansionism. By the end of World War II that phenomenon had already expressed itself in the annexation of territories from every country that shared a pre-war European border with the Soviet Union. It also expressed itself in the establishment of puppet regimes in Europe that constituted a Soviet empire that continues to exist to this day.

We may have perceived Soviet actions incorrectly in those days, but I think not. Clearly the Soviets themselves in the late 1940s and through the 1950s saw the prospect, or imagined the presence, of a U.S. encirclement of the Soviet Union. In any event, the U.S. goals that I have listed were not bad ones, and we continue to hold them as national policy.

How do the trends of the 1970s extrapolate into the 1980s? One such trend is the continuing diffusion of power. Not only are there now more than 150 countries, some of them very small and practically all politically independent if not necessari-



ly economically viable, but also the U.S. military and economic predominance that prevailed at the end of World War II is long gone. Measured in economic terms, the Gross National Product (GNP) of Western Europe exceeds that of the United States. The Soviet Union and Eastern Europe together probably have more than half the GNP that the U.S. has, and Japan all by itself is perhaps at a level of a third of the U.S. GNP.

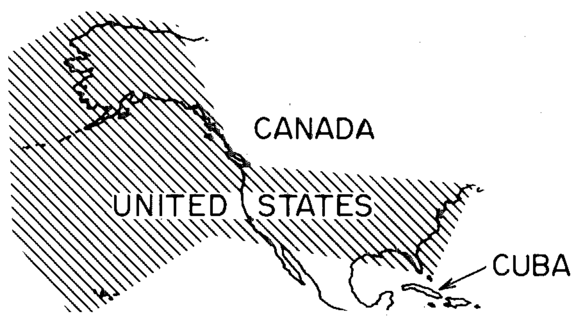
Furthermore, the world has become considerably more interdependent, economically and culturally. The U.S. has a smaller ratio of international trade to its GNP than any other major industrialized democracy, but even that ratio has more than doubled in the last decade. Unfortunately, the import side of the ledger has risen mostly because of the increase in quantity and price of imported oil. By comparison, many of the Western European countries — and even more so, Japan — have imports and exports each of which run anywhere from about a quarter to a half of their GNP. Imports from the oil-producing third-world countries, and exports to them and to the truly poor countries of the fourth world, and exports to and imports from Eastern Europe have become major factors in the economies of practically all of our allies. And the trade in ideas is equally widespread, though the ideas are often distorted or debased. The ideas of freedom and independence, for example, are applauded even by those who hypocritically distort them.

This economic and cultural interdependence has been a contributing element in a prosperity that has been growing — at least until the mid-1970s — in the industrialized world. But it has also made that material prosperity more precarious because not only can the U.S. and the Soviet Union effectively destroy each other and everyone else militarily, but a shutoff of oil from the Persian Gulf could bring on a worldwide depression that would make that of the 1930s look like a minor economic ripple. Another example of a problem growing out of today's interdependence comes from the export of nuclear technology that began in the 1950s. That technology now threatens to produce a proliferation of nuclear weapons in many newer, smaller, in some cases poorer, and often politically less stable nations.

As this discussion suggests, not all of the world's or America's political, economic, or security problems arise from the Soviet Union. (In most cases, however, the Soviet Union will be found fishing in troubled waters and occasionally stirring up some of the trouble to its own anticipated advantage or the detriment of the industrialized democracies.) Libya, for example, has lately embarked on an expansionist course in Africa, threatening military adventurism against Egypt, the Sudan, Tunisia, and even Algeria, as well as occupying much of the nation of Chad. Those activities, plus supporting and exporting terrorism worldwide, are no mean feats for a country of a couple of million people, even an oil-rich country. The Soviets didn't invent that situation at all, though they do take advantage of it and egg it on. Nor is nuclear proliferation a Soviet policy; quite the reverse, as the government of any Eastern European country could assure us.

As a matter of fact, the economic strengthening and political independence of many of our friends and allies has itself created some of the current problems of the United States. Japanese and European exports of manufactured goods where the U.S. once reigned supreme — as in automobiles, steel, and consumer electronic goods — have had a considerable and negative effect on our economy. It's true that this wouldn't have happened had not some segments of American industry failed in management or in foresight, and had not labor costs in the United States stayed or gone much higher than they have in some of these other countries.

Furthermore, in international forums our allies (let alone our unallied friends) do not always support U.S. positions politically, but that is the price we pay for being the leader of a voluntary coalition of free nations. I submit that it is a price



worth paying. The alternatives are to retreat into isolation and give up trying to influence events outside the United States, or to adopt Soviet methods. The first alternative is infeasible in a situation where we are importing 50 percent of our oil. The choice of isolation also creates a prospect of facing at some point unfriendly governments on our own borders — an outcome that is a logical consequence of a retreat from participation in international affairs. If you doubt that such a situation can be a real problem for us in view of our great size and strength, consider the effect of having an unfriendly government in Cuba. Last year's Cuban boatlift resulted in the U.S. losing control at least temporarily over the decisions as to whom we would admit to our own country. Our sovereignty was curtailed, and that is not a comfortable situation.

The second, Soviet-style, alternative in dealing with foreign countries has problems of its own, as the Soviets are now finding out in Poland. There is no doubt as to which governmental and ideological style is more popular in the world among people who are given a free choice. No country seeks to emulate the Soviet state these days, not even those revolutionary governments in emerging countries who by force of circumstances become Soviet clients. Some aspiring revolutionary movements look to the People's Republic of China as a model; some, I guess, still look to Castro's Cuba; and some, I think, try to devise one of their own based on some idealized concept. But none of them says, "We want to be governed the way the Soviet Union is governed."

It is no accident that the refugee flows have been in the direction that they have. More than a million refugees have fled from Afghanistan into Pakistan; another million or more from Cambodia and Vietnam into Thailand and into the sea; nearly a million into Somalia from Ethiopia; another million from Cuba to the United States. Until the Berlin Wall sealed it off, there were a million from East Germany into West Germany. Whether in Southeast Asia, Southwest Asia, Africa, Europe, or North America, the flight is always *from* Soviet-dominated territories, from persecution and from slaughter. And the Soviets' heavy hand with those who accept their economic or military assistance has, in cases where they have not been able to occupy the country militarily, driven a whole series of countries out of the orbit of Soviet influence and into a more friendly relationship with the United States. Three very diverse examples, beginning in the 1960s and running into the 1970s, include Ghana, China, and Egypt.

If we are to maintain and expand the circle of nations with whom we have friendly relations — and we need to do this in an economically interdependent world in order to gain access to resources as well as to build political barriers against Soviet aggression — we need to continue to support the cause of freedom and independence. We need greatly to expand our economic and developmental assistance programs, which have an effect all out of proportion to their costs. Those costs are, admittedly, substantial in absolute terms, but they are only a few tenths of a percent of our own GNP as compared to almost a percent for some of the other developed countries. One of the most difficult things to get the Congress to approve has always been developmental assistance to the developing countries. Individual congressmen and senators will almost always admit in private that such money is well spent or that spending more would improve the U.S. position in the world and thus our ability to conduct a successful foreign policy. But its unpopularity with the public as a whole — partly as a result of lack of understanding and partly, I fear, as a result of selfishness — has not been countered by adequate leadership and education on the part of our political leaders. Spending more on developmental and economic assistance is probably the best investment, dollar for dollar, that we can make to advance our foreign policy.

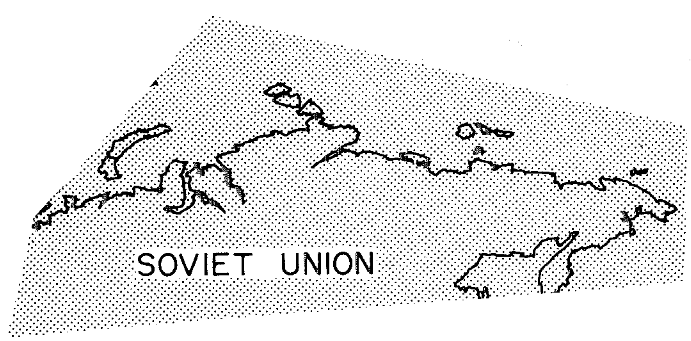
In the area of human rights, we need to push authoritarian governments toward greater liberalization, whether they are friends of ours or not. At the same time we need to keep in mind that it is not in our interests that a friendly authoritarian government be replaced by a totalitarian government opposed to us. (I think that although the new administration overstates this point, they are quite right to bring it to our attention.) We need to look at each case carefully to see whether our human rights program encourages liberalization or is going to lead to an even worse situation.

Let me now turn to the military situation, which in many ways is the one measure in which we don't have a clear advantage over the Soviets. It is the area in which they have made the greatest strides in recent years and the only one in which they have been able to compete with us on roughly equal terms. During the past 25 years we in the United States have seen defense against Soviet military power and expansionism as centering in three main areas. The first is the defense of the United States itself against a strategic nuclear attack by the Soviets. This is the only way in which we could be physically destroyed as a nation, and it remains true that we are not able to prevent such action by any active means. There is no physical defense against nuclear war. So long as large numbers of nuclear weapons remain in military arsenals and in light of the ability of the offense to concentrate and overwhelm the defense (assuming there is no poor military planning on either side), both the Soviets and the U.S. will be able to destroy each other for the foreseeable future. But only at the cost of themselves being destroyed in return. That's what deterrence means. It's a poor substitute for safety, but it is one we have learned to live with. A combination of adequate military-force building, careful planning, and equitable and verifiable arms limitation agreements to minimize the costs and help assure stability — these together comprise the proper program to maintain deterrence of strategic war.

The second role of U.S. military policy has been to defend, in alliance with its inhabitants, Western Europe, which constitutes the largest concentration of industrialized population and production in the world. The NATO alliance has served as an effective instrument of this policy for ourselves and our NATO allies since 1949. Originally the U.S. provided not only the strategic forces but a considerable fraction of the conventional forces in place in Europe. Those forces were designed to act as a trip wire if the Soviets should invade Western Europe. Over the last 10 or 12 years, the defense expenditures of European members of NATO have risen from being less than 50 percent of those of the U.S. to more than 60 percent. During the early 1970s, when our defense expenditures were falling in real terms, the expenditures of our NATO allies were rising. Since the early 1960s the Soviet military effort has been increasing at the annual rate of 4 percent or more in real terms, and that means that it would double every 18 years, and it has doubled. In 1978 it was agreed that to meet such continued expansion, the NATO nations would each commit to 3 percent annual real growth for military expenditures. They and we have come close to and in some cases exceeded that number since then.

One question, of course, is how do you match a 4 percent annual growth with a 3 percent annual growth, and the answer is that you don't. Two other things need to be done to balance the growth of Soviet military capability. One is to be more efficient, which isn't always easy I can assure you. The second is to cooperate more closely with our European allies so as to make use of economies of scale and not to duplicate development and production. And that's not easy either.

There continues to be, however, less willingness in many of the European countries than in the U.S. to see the Soviet military growth and



threat as real. My own judgment is that the American people and the Congress will be unwilling to follow through on present plans to increase the capability of U.S. forces to reinforce Europe in a crisis unless the Europeans become more willing to tilt the balance between their defense expenditures and their social welfare expenditures more in favor of the former. I can recall a recent argument in the parliament of a European country over whether they could possibly increase their defense budget by 3 percent from the previous year. Strong arguments were made that in view of the economic exigencies the best that could be done would be to keep it constant. At the same time in that same parliament, a debate was being carried on about increasing some of their social program expenditures. The argument in that case was about increasing it by an amount in excess of the entire defense expenditures of that country. It seems to me unlikely that the percentage of the total U.S. GNP assigned to defense can rise to 6 percent or even 7 percent while the levels of most of our NATO allies (including some of the most affluent) remain under 4 percent.

We are committed by agreement with our allies to provide modernized, long-range nuclear forces in Europe to offset the Soviet growth in numbers and quality of intermediate-range ballistic missiles and medium-range bombers aimed at Western Europe from the Soviet Union. But in my judgment we are unlikely to carry out those plans unless the Europeans can show the political cohesion and courage necessary to provide bases and facilities — and funding — for such new U.S. deployments in addition to those necessary for U.S. conventional reinforcements. It remains to be seen whether the political leadership of various European countries will take the lead in their own countries for forming such a consensus and in turn join as partners in the kinds of initiatives proposed by the U.S.

There are other areas of the world that are important to us. Our own backyard is certainly important — the Caribbean and Central America. Sub-Saharan Africa is also important in terms of resources that we depend on. But the third area in which our defense has hitherto been centered and which has been seen as critical to the U.S. is East Asia, including Japan, Korea, and the other countries along the Pacific rim. These have become the great success story of U.S. policies since World War II. Europe was industrialized and prosperous before World War II; these areas have become so since. It is true that during the past decade the Soviets have greatly increased their military capability in the Far East; but the cooling of relations with Japan, the progressive alienation of the People's Republic of China from the Soviets, and the normalization of relations between the P.R.C. and the U.S. have more than counterbalanced that factor in overall political-military terms. Political instability in the Republic of Korea, however, has compounded the effects of the North Korean military buildup that took place during the 1970s, and has thus created a trouble spot. The aggressive behavior of Communist Vietnam, which has failed to solve the internal problems consequent on its conquest of the south, has provided the Soviet Union with new bases from which to spread its reach into the western Pacific and Southeast Asia, and at the same time Vietnam threatens its neighbors in that area.

Aside from these three traditional areas — the strategic, the European, and the East Asian — I believe there is now a fourth. During the past few years Southwest Asia, which has always been of importance to us, has become even more so. One reason is that the industrialized democracies have become even more dependent on oil from that area. While the U.S. gets only about 11 percent of its oil from the Persian Gulf, a cutoff of exports from that area would disrupt world markets in terms of availability and prices. This disruption would be severely damaging to the U.S. through its effects on the rest of the 40 to 50 percent of its oil that the U.S. imports. Since our allies in East Asia import as much as 50 percent or even 75 percent of their oil from that region, a cutoff would be even more catastrophic to them than to us. Soviet domination of the region would make our allies of the industrialized world, as well as many third-world nations, economic vassals of the Soviets.

Moreover, the region itself is beset by ethnic, political, and religious conflicts both between nations and within them. For many years it has been influenced in a destabilizing way by the conflict between the Arab countries, many of them our friends, and Israel, to which we have a special security commitment. And there is an internal threat in many countries of this region from political or religious extremists. They could well act to overthrow existing regimes whether conservative or less so.

The recent Iranian revolution made more explicit and obvious the weakness of what had been regarded both as a bastion of stability in that region and as a shield against the Soviets of the Persian Gulf. In addition, the illegal actions of the Iranian government in holding prisoner and abusing U.S. citizens has made a difficult situation for U.S.-Iranian relations into one in which those relations are characterized by deep animosity. It will take a long time to change that.

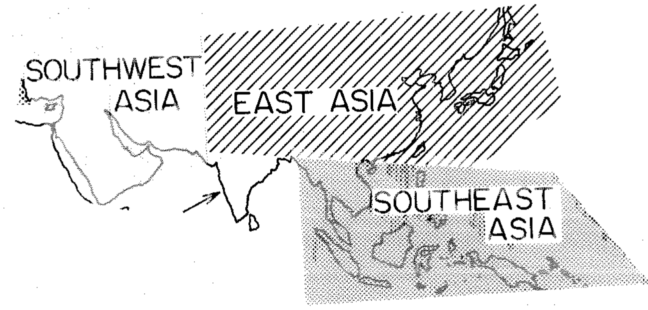
The war between Iran and Iraq has added to the instability in the region, threatening at times to spread to other Persian Gulf states and splitting the radicals from the moderate Arab states (with the radicals tending to side with the Iranians). Let me remind you, however, that military threats are not the only ones in the region, nor are the Soviets the only possible military threat. Attacks by one country on another — with the attacker usually equipped with Soviet-supplied arms — have happened before and they're happening now; examples include Libya in Chad and Vietnam in Cambodia. And similar attacks are likely to happen again in the future.

The Soviet invasion of Afghanistan strongly suggests Soviet willingness to use its own military forces in the general area of Southwest Asia. Though the Soviets may think of this action only as assuring the continued Marxist orientation of a neighbor, the non-Communist countries of the region correctly see it as a possible harbinger of Soviet attitudes toward themselves. That has tended to bring the Saudis and the Iraqis, who formerly had been rather friendly to the USSR, into a great appreciation of the value of U.S. military capability as the only possible counterbalance to a blatant Soviet military push toward the Persian Gulf, or as an offset to a Soviet attempt at political domination based on military power in that area. That, of course, is a more likely use of Soviet military power.

The reluctance of most of the countries in the region to align themselves explicitly with either the U.S. or the Soviet Union has advantages and disadvantages for us. In one sense, it's simply an Islamic wish to avoid either Western or Soviet domination. One facet of this attitude is a strong anti-Communism, which is a principal stumbling block for the Soviets in the area. At the same time, the governments of these countries can't afford to be seen as providing bases for U.S. forces. Because the region is so far from us and so near the Soviet Union, this reluctance poses difficulties for our ability to help defend them. Fortunately there are some natural as well as human obstacles between the Soviet Union and the Persian Gulf.

During the last couple of years the U.S. has made a start in improving the capability to move forces into the region rapidly by, among other things, deploying two aircraft carrier battle groups and other naval forces in the Arabian Sea. This constitutes the most powerful fleet that has ever been in these waters, regardless of country of origin. And our French, British, and Australian allies have deployed naval contingents as well. We have also moved into the region seven newly acquired ships containing pre-positioned military equipment and supplies that could marry up within seven to ten days with an augmented marine brigade and supporting tactical aircraft squadrons. We've re-oriented U.S.-based ground and air force bases for rapid deployment into the region if necessary. And we've organized a rapid-deployment joint task force that has begun planning for such a contingency. It would command the forces of various military services in case such operations were required. We've begun to develop and procure new aircraft and rapid sealift capabilities, in order to augment our ability to move anywhere from two or three to more than six divisions into the area quickly.

These actions are going to take several years to come to full fruition, of course. Moreover, they're going to cost money. I estimate that over the next five years they'll cost anywhere from



$20 to $25 billion. Only the U.S. can assume the bulk of the responsibility for organizing a security framework that includes the nations of the area and for providing the great majority of the outside forces that would act to deter or contain Soviet military expansion in the Persian Gulf region. But since our European and Far Eastern allies depend even more than we do on access to the resources of the region, they're going to have to participate as well. Only a few of them — notably France, Great Britain, and perhaps a couple of others — can be expected to supply additional forces (albeit much smaller than our own) that can be moved to or stationed near the region itself. But all can contribute by providing transit facilities for U.S. forces if needed, and all must also contribute by increasing their own defense efforts and capabilities in their own areas. These will substitute for some of the additional capabilities the U.S. has been planning to introduce into Europe and East Asia as reinforcements in case of a crisis.

To conclude, I'm compelled to draw a twofold lesson from all I've described about the world of the 1980s. It's a simple, perhaps a self-evident, lesson, but it's the one I took with me from four years of service as Secretary of Defense. First of all, military power — no matter how strong — has important limitations in assuring that U.S. interests are preserved in a complex world of intertwined relationships. All the other instruments of national policy — economic, political, and diplomatic, for example — must also be skillfully used if we are to navigate the dangerous waters of the 1980s.

The second part of this lesson is equally important. It is that without adequate military capability on the part of the United States, plus joint military planning and programs with and sufficient efforts by our allies, we are headed for trouble. Soviet military power, the dependence of the industrialized world on Middle Eastern oil, and the growing instability of the developing world will combine to make the world of the 1980s more dangerous than any we have yet seen. With optimism engendered perhaps by remoteness from the scene, I tend to believe, however, that we can and will solve these problems. □

SURFing

The SURFers who hang around Caltech all summer are not engaged in a fruitless search for the perfect wave in Pasadena. The 46 young men and women who are participating in the Summer Undergraduate Research Fellowship program this year are doing independent research — work that is expected to make a genuine contribution to the field and result in a scholarly publication. And they are receiving support to do it.

This is the third year of SURF, which is funded by donations from Samuel P. Krown, a life member of The Associates, and from the Caltech Prize Scholarship Fund, established by an anonymous donor in 1976. The initial idea of using some of these funds to encourage creative research, promote interaction between undergraduates and faculty, and improve the undergraduate program came from Fred Shair, professor of chemical engineering, and Harold Zirin, professor of astrophysics and director of Big Bear Solar Observatory. President Marvin Goldberger has been an enthusiastic supporter of SURF since its inception.

During the program's first three years William Schaefer in chemistry, Bernard Minster in geology, and Shair, respectively, have volunteered to administer it. Carmen Longo of the Office of Student Affairs has helped keep the program running smoothly.

Teaching in the framework of research has always been a cornerstone of Caltech's philosophy of undergraduate education. But the SURF program goes several steps farther than the more usual situations where a student is hired as a small piece of a professor's grant or to work around the lab as part of a financial aid package. SURF students are chosen primarily on perceived research potential, and they, working with their faculty sponsors, must develop project proposals themselves. These are reviewed by faculty familiar with the field, and the final decision is made by a special committee of the Faculty Committee on Scholarships and Financial Aid.

SURF students are expected to carry their projects to completion during the ten-week summer period, and at the end of that time present written and oral reports. The oral reports are given at a daylong conference run along the lines of a technical meeting. Each participant receives a copy of the conference proceedings. SURF participants are also invited to attend weekly luncheon seminars during the summer at which faculty present overviews of their own research fields; attendance at these seminars has been practically 100 percent.

Few students, nationwide, have such an opportunity for involvement in research from start to finish — from proposal to publication — until graduate school.

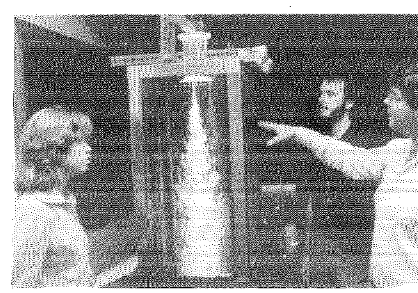
During the summer of 1981 students each received $2300 for what is considered full-time employment. While this amount may not be competitive with that from some industrial summer jobs, it is sufficient to live on and to enable students to save for their expected contributions to academic-year expenses. Shair hopes that the student stipends can increase with increases in the cost of living. Essentially all of the money for SURF goes to the students; such expenses as laboratory supplies and computing are covered separately through faculty research grants. This structure serves to place the student research closer to the mainstream of faculty interest.

Shair hopes SURF can be expanded to include as many Caltech undergraduates as are capable and eager for such an experience. Based on insights gained over the past three years, he believes that the optimal SURF program would involve about 70 students and 60 faculty members each year. Enthusiasm for SURF among faculty and students and the desire to expand it have stimulated an attempt to have the program endowed. In the meantime, it is still growing, and the 46 SURFers in 1981 represent the largest SURF program to date. In the following pages six of these students discuss their research at midpoint and what the program has meant.

KELLEY SCOTT
Sophomore, Engineering and Applied Science

The aim of my SURF project is to obtain a three-dimensional view of the structure of turbulent jet flow. Knowledge of this structure can be applied to understanding such problems as jet noise and the mechanisms of mixing and chemical reactions. The completed "picture" of the flow can be assembled from a series of slices in much the same way that a few cross sections of a boat can convey the idea of an entire vessel.

Working with Paul Dimotakis, associate professor of aeronautics and applied physics, I planned to photograph successive slices of jet flow using laser-induced fluorescent dyes, a laser sheet, and a stepping motor/mirror combination to place the sheet in the desired positions in sequence throughout the flow. Sounds simple? I had problems just saying it at the beginning of the summer. As I learned more about what this plan was actually going to entail, I started to realize that those photographs were a long way down the line in terms of what had to be done to get there. Poor me — I was so naive.



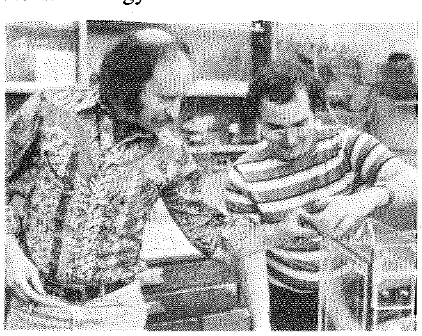
When the time came that I needed my first piece of equipment, I asked, "Where do I get one of those?" I discovered that *I* was supposed to wire the thing together. Since I had never done any electrical work at all, this came as a bit of a shock. So I learned some basics in that area. Soon I had to find another part of the system. I was a little wiser by this time; "What do I have to put together now?" I wondered. It turned out that I didn't just need to put something together this time; I had to design it too. Things have been progressing along this line to the point that I've tried my hand at wire wrapping, designing and building control panels with the appropriate switches and connections, studying different computer systems, photography, optics, and learning basic business practices, along with other odds and ends associated with a research project.

GARY MOCKLI
Junior, Biology

My SURF project has allowed me to work in the developmental biology laboratory of Professor of Biology Eric Davidson this summer, studying specific RNA transcripts found in the mature sea urchin oocyte. Such transcripts are stored in the egg before fertilization and are believed to play a major role in the early development of eukaryotic (all but the most primitive) organisms.

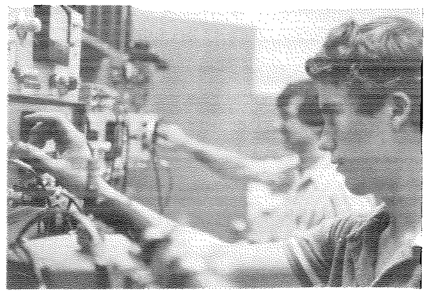
Several factors make the sea urchin an excellent choice for the study of early eukaryotic development. For example, large quantities of eggs can be easily obtained, and also sea urchin embryos have very distinct developmental stages. Fertilization of many eggs at one time makes it possible to collect large numbers of embryos at the same stage simultaneously. Thus it is simple to make comparisons between the varieties and concentrations of the transcripts found in the mature oocyte and in the early stages of development.

I am using recombinant DNA technology to examine these transcripts. Recombinant DNA is an area of biology that I had not previously encountered, and I have been able to learn a great deal about it this summer. In fact, because of the SURF experience I am seriously considering this field of research as a career. I intend to continue working in Professor Davidson's lab, both term-time and summer during my remaining two undergraduate years, to gain still more experience in both recombinant technology and developmental biology.



RICHARD POGGE
Junior, Physics

As a physics major with a strong interest in astrophysics, I would find it very difficult (or rather, impossible) to find summer employment in an astrophysically related field that would actually get me looking through a telescope. The SURF program has given me a chance to do real astrophysical research for not just one, but two summers.



The value of the program lies in the experience it gives. Suddenly, science is stripped of the comforting veils of elegant theory and "plug-and-crank" formulas, and the student can see what research is really like — more questions than answers, error sneaking in from practically everywhere, noisy data, cloudy nights, and equipment that won't work the way it does in the Phys 1 demonstrations. But it also lets the student see the beauty as well — the data that conform well to theory, the clear, moonless night when everything works, and the sense of pride that comes from seeing your name among the authors of a paper to be published. This is something no number of hours in the classroom can give.

I am working this summer in the field of infrared astrophysics with Gerry Neugebauer, professor of physics and director of Palomar Observatory, Tom Soifer, senior research associate in physics, and other members of the infrared group. The Carnegie Institution of Washington has granted me 38 nights of observing time on the 61-centimeter (24-inch) telescope on Mount Wilson. The time is being used to make a long-term monitoring of the unusual quasar, BL Lacertæ.

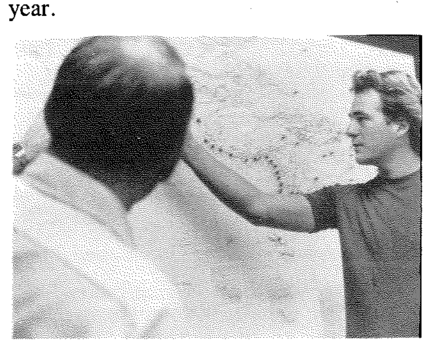
BL Lacertæ is a strong radio source that is also a strong optical and infrared source. It is unusual in that its spectrum exhibits neither emission nor absorption lines — only a continuum. In addition, the continuum is highly polarized, indicating perhaps some sort of synchrotron emission mechanism. The object is known to vary in its emitted flux on time scales of about one day, but lately it has been very dim and quiet. Since it is a bright source at both infrared and visual wavebands, it is possible to study it from the 61-cm telescope.

My observing program consists of watching BL Lacertæ with an infrared detector with filters for 2.2 micron and 1.6 micron wavelengths, and simultaneously monitoring the visual flux with a photomultiplier tube sampling around 5500 Å wavelengths. The three wavelengths will allow me to determine whether the changes in intensity (if any) are accompanied by broad spectrum color changes, giving some insight into the emission process.

This sort of "hands on" experience is beyond value for me. Sitting at the back of this big metal and glass "light bucket," counting photons all summer, and then "dancing with the data" (as Professor of Theoretical Astrophysics Roger Blandford says) to make some sense out of it all will perhaps educate me more in practical science than anything else I'll do during my remaining undergraduate years at Caltech.

JOHN KING
Senior, Chemical Engineering

I think one of the best things about undergraduate education at Caltech is the opportunity for us to get involved in doing research. The SURF program is the perfect chance to spend a substantial amount of time working on our own projects. I was able to begin working in chemical engineering during the summer after my freshman year and continued during subsequent summers as well as the school year.



My time was mostly spent working on studies of atmospheric flow and pollutant transport using a tracer gas technique developed by Professor of Chemical Engineering Fred Shair. In one such study we are now working on the final versions of a paper on the characterization of up-slope flows. After a release made from a chemistry building at Caltech, we followed the tracer into the mountains and saw it move through Altadena, Henniger Flats, and on up to Mount Wilson. The data from this study provides an accurate characterization of flow velocities and dispersion associated with this important ventilating mechanism. My SURF project this summer involves an investigation into the safety of a liquid natural gas facility proposed for Point Conception. This will be based on some new data from a study we conducted in the Santa Barbara Channel region last summer.

Because of the SURF program I have been able to spend a lot of time working on my own projects like this. I'm sure that the experience provided by working independently on a research project will be very valuable when I enter graduate school.

STUART GOODNICK
Junior, Physics

Caltech prides itself on the large number of research opportunities available to undergraduates, and the SURF program is certainly the best example of the Institute's encouragement of student scientific work. The greatest benefit of this program is not so much the quality of the research but the chance it gives students to find out what a career in science is actually like; professional science bears little resemblance to undergraduate academics.

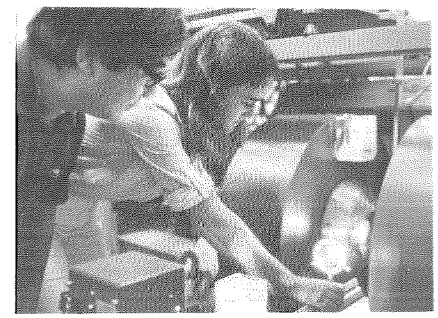
I am working on a project in theoretical nuclear physics; in particular I'm attempting to improve a simple kinetic model of a proton-nucleus scattering process. The original first approximation of this scattering, published in 1980, describes the proton collisions with the individual nucleons in terms of Boltzmann two-body collisions. Even though this description relies upon gross approximations, the theoretical energy cross sections calculated from it closely match the experimental data within the intermediate energy range. In other words, when a 90 MeV proton collides with a large nucleus, the kinetic model can roughly predict the likelihood that the energy of the emergent particles is in the range of 30-60 MeV. My current research involves refining this model by considering Pauli blocking for nucleons being scattered below the Fermi energy of the nucleus and by considering the effects of the coulombic interaction on each individual collision within the nucleus.

This project has been very different from regular class work where a certain amount of information is set forth for you to learn. Here I must decide what specific things I need to learn at every step, and then I have to go dig them out. My adviser, Associate Professor of Physics Steve Koonin, helps by pointing me in the right direction and suggesting ideas for further research. Working as a physicist with professional physicists has given a realistic perspective to my desire to embark on a scientific career. To me this is the greatest value of the SURF program.

LYNMARIE THOMPSON
Junior, Chemistry

When I was deciding where to go to college, I remember thinking that one of Caltech's important assets was that undergraduates could get involved in research. At the time I thought that I'd like to make a career of scientific research, but it worried me that I couldn't know ahead of time what it was really like. With the SURF program I can really immerse myself in a project, which is in many ways better than trying to fit research in during the year (with at least five other classes). I can learn some science as well as something no lecture or textbook could teach me: the kind of thinking and learning that research involves — and the frustrations and rewards.

My specific interests are in biophysical chemistry, and I have joined the research group of Sunney Chan, professor of chemical physics and biophysical chemistry; they work on determining the structure of cytochrome *c* oxidase, a mitochondrial enzyme that catalyzes the last step of cellular respiration. My project's goal is to study heme-sensitized photo oxidation so that this process might be used to map the structure of the oxygen-binding site of the enzyme. To get a better understanding of how this reaction might work I am trying to determine the relative rates of photo oxidation with different sensitizers, specifically porphyrin rings with different metal centers in different oxidation and spin states. (Heme is a porphyrin ring with a Fe II center.) This is done by monitoring production of the excited singlet state of oxygen ($^1O_2$), which is an indication that photo oxidation is taking place. $^1O_2$ reacts with an ammine (TMP) to form a nitroxide radical (TEMPO), which because of its lone electron gives an electron spin resonance signal. The intensity of the signal is proportional to the concentration of TEMPO, which depends on the amount of $^1O_2$ produced. Thus, plotting signal intensity versus irradiation time gives the rate of photo oxidation for a particular sensitizer.



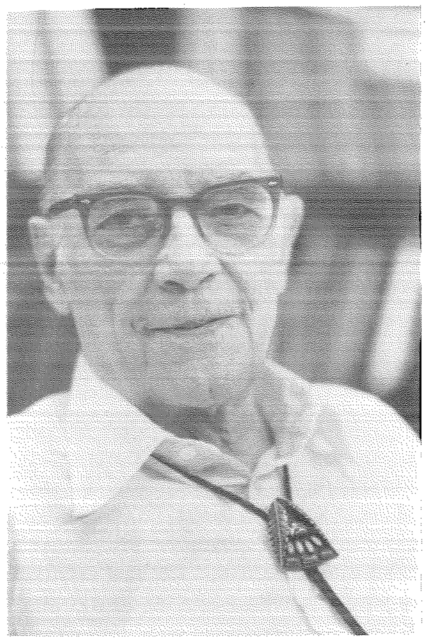
So far I've learned some very specific, detailed science that complements the broader learning I've done in classes. I've also gained some familiarity with the lab, techniques, and instruments that will be useful in later research. One of the most important things I've discovered about research is that everything takes twice as long as expected because lots of things go wrong.

Besides the expected slowness and mistakes of inexperience, things take longer than I'd expect because all the details involved are not apparent until I start doing the experiment. New problems and questions arise even as I try to just set up and standardize the experiment — the lamp setup varies, requiring a daily control: room light also affects the reaction; solutions are contaminated by hard-to-clean syringe tubes; variance in diameter of capillary tubes can affect the signal intensity by as much as 30 percent.

It's easy to feel sometimes that I'm not getting anywhere, since until now I've done nothing but prepare for the "real" experiment. However, through these preparations I am improving the data and learning how to think about the problems so that later I'll be able to iron them out more quickly.

I'm glad that the SURF program has given me this opportunity to learn about scientific research.

Carl D. Anderson – How It Was



Carl Anderson enrolled as a freshman at Caltech in 1923; in 1976 he retired as Board of Trustees Professor of Physics, Emeritus. In the 53 years between he was active in all the standard academic pursuits, plus a few that were not so standard – winning a Nobel Prize in 1936 at the age of 31, for example, and receiving membership in the National Academy of Sciences two years later. He also served for eight years as chairman of the Division of Physics, Mathematics and Astronomy. Obviously he was a prime candidate to be interviewed for the oral history program of the Institute Archives, and this was done by Harriet Lyle in 1979. E&S *presents here an excerpt of his memories of the years 1926 to 1937, the period during which he discovered the positron and the first mesons.*

Harriet Lyle: I know you were born in New York City and that you moved to Los Angeles when you were seven. Would you tell me a little bit more about your family?

Carl Anderson: My parents both emigrated from Sweden when they were eighteen or nineteen years old, and my father spent most of his life in the restaurant management business. My grandfather was a farmer in Taby, which is a suburb of Stockholm, Sweden. I visited there in 1926 and saw my paternal grandfather. This was on my Caltech Junior Travel Prize trip.

HL: You had a six-months trip on your travel prize?

CA: It was supposed to be, but we were homesick for California, so we spent probably five months or so actually in Europe. We bought bicycles in Munich with the idea of bicycling up through Germany and into Holland and Belgium and so on, but we never got outside the Munich city limits on them. The trouble was that it rained all the time, and we decided that it was not a very practical way to travel.

HL: Was the Institute paying for all of your food and lodging?

CA: Yes. I think the prize was $900, which was just enough to make the trip if you were economical. And then Dr. Noyes slipped us each $50 just before we left, to spend going up to Gornergrat and the Jungfrau. He loved the mountains in Switzerland. To get to Gornergrat you take a cog-wheel railroad car from Zermatt up to about 10,000 feet. And then you have a 360-degree view of the Alps. We decided to climb Monte Rosa, the second highest peak in Europe.

HL: Were you supposed to meet certain people, or did you just go from city to city on your own?

CA: No, we had no appointments to meet any certain people. But in Munich we did attend a class given by Sommerfeld, who was, as you know, a very famous physicist at that time. We read in the paper about American Students' Week in Leiden, Holland, and we made only a very slight revision in our itinerary so we could be there during that week. That's where I first met Robert Oppenheimer. He was in Germany at the time and decided to attend American Students' Week.

HL: Did you think of yourself as a physicist yet?

CA: I was majoring in physics when we left to go to Europe, so I was a budding young physicist. All my life, from as early a time as I can remember, I wanted to study electrical engineering. The thing that changed my mind was the third term of the sophomore year at Caltech. There was what they called Section A, which was supposed to be a special section for some of the better students to do the three-term regular physics course in two terms. And then Ira Bowen took the class for the third term and talked about modern physics. It was great, interesting, wonderful, and I learned from him that you could even make a living doing it. So I changed my course to physics, but I got a degree in both engineering and physics because the courses were quite similar.

I might mention that Millikan, as far as I know, always taught a class when he was, in effect but not in name, president of Caltech. He was much more than president, and if there hadn't been a Millikan, there wouldn't have been a Caltech. I'm sure of that. He gave a course called "Electron Theory" to first-year graduate students. In the first three or four minutes, he'd write an equation on the board that had something to do with electron theory. But then he would often begin to reminisce. He wore these pincer glasses that he put on one finger, and would then tell about what happened in 1906, for example, in connection with his working on the oil drop experiment and the day he happened to think of using oil instead of water. It was much more valuable than if he had talked in a formal way about electron theory. You could learn that by reading in a book or hearing somebody else.

HL: What did you do about tests and things like that in a class like this? It seems that students would be a little worried about what they were expected to learn.

CA: Yes, he did give examinations; his reminiscences, although wonderful, did not occupy the whole time in his class.

Several years later I was in Millikan's office talking to him one day about cosmic rays, and the registrar came in and said, "Dr. Millikan, you gave A's, B's, and C's to your students in your class." And Dr. Millikan said, "Yes. Now take this first man, for example. He was a good student; he wasn't top-notch, though, so I gave him a B." And the registrar said, "Oh, I wasn't questioning your assignment of grades to the students. I was really pointing out that Caltech has the 4-3-2-1 system and not the A-B-C-D system." So then Millikan said, "Well, I could change these letter grades to numbers — or we could change the system at Caltech." I thought it was interesting that Millikan saw two solutions to this problem; he could change the sheet of paper, or the system could be changed.

HL: Of the distinguished people who visited Caltech, are there any that you remember particularly?

CA: Oppenheimer was on the faculty at Caltech and at Berkeley at the same time. So he used to commute and spend one term at Caltech. And Oppenheimer, who later became an extremely eloquent lecturer, was not in those days a good one. He didn't speak loudly enough, and he didn't really face the audience. I took a course in quantum mechanics from him when I was a graduate student, and I had no idea what he was talking about. He paced back and forth, and wherever he happened to be at that instant, he would write some squiggles on the blackboard — part of an equation. The parts were scattered at random all over the blackboard.

I didn't have the background to understand theoretical physics at the level that he was speaking. So I went to his office one day and said I would have to drop his course. He sort of pleaded with me not to, and then he admitted that everyone else in the course had already asked to drop it. I was the last. He really pleaded with me to stay, because he wanted to have an official course at Caltech, and he assured me that everything would be all right at the end of the term. So I stayed registered as a student, and he had an official course. At the end of the term, he asked me what the highest grade and the lowest grades at Caltech were. For an instant I thought of reversing them, but I didn't. So I told him the highest and lowest grades, which he should have known and probably did. Anyhow, I got an A in the course.

HL: Did he get better? Could you understand it more as you went along?

CA: No, I didn't. It was over my head, all the way through. And I'm not the only one. This was in the days when the first papers on the Dirac theory were being published. Richard Tolman got Oppenheimer to agree to give a series of evening lectures two hours long — three a week, I think — on the Dirac theory, for anybody who wanted to attend. So I attended the first meeting of that series, and Oppenheimer talked for two hours. And at the end, Tolman got up and said, "Robert, I didn't understand a damn thing you said tonight, except . . ." Then he went to the blackboard and wrote an equation. And Oppenheimer replied, "That equation is wrong." And there was never a second meeting of this attempt on Oppenheimer's part to tell people what the Dirac theory was all about.

A very young Professor J. Robert Oppenheimer

HL: I want to talk a little bit about your graduate work at Caltech, which started in 1927.

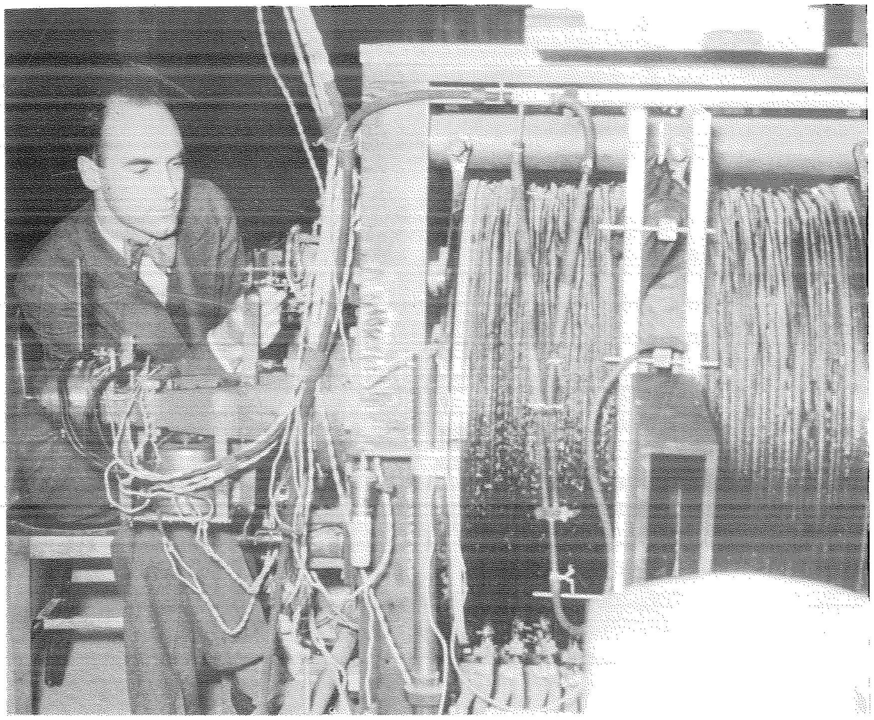
CA: Actually it started in 1926, when I was still a senior. Millikan was away on a trip, so I talked to Earnest Watson. I told him that I would like to get started on some research because I didn't have enough to do. So he assigned me to work with Lee DuBridge, who had just come to Caltech as a National Research Council fellow, to work on the photoelectric effect. I guess I worked for him for about three weeks, building a monochromator for his photoelectric experiment. And then Millikan called me into his office and said that I should be doing something else. I should be working with D. H. Loughridge and not DuBridge. Millikan really assigned research projects, I guess, at least in my experience. So I looked up Loughridge, who was working on the photoelectric effect of X rays with a cloud chamber. He was just finishing up his work for his degree, and he left something like a week or two after I started working for him.

HL: Millikan must have known he was going to leave, right?

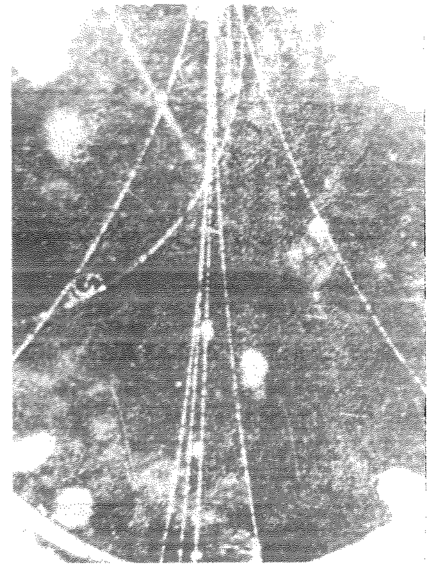
CA: Oh, yes, Millikan knew he was going to leave. I guess that's why he felt he needed somebody to carry on that work. I did that for four years, and greatly modified the equipment and did quite a bit more than Loughridge. That was my thesis work. Many months after I started working there, I happened to bump into Millikan and tell him that I didn't have any research adviser, as you're supposed to have. And he said, "Oh, that easy; I'll be your research adviser." So I was his student — although not once during the time that I was a graduate student did I discuss my work with him or was he in my laboratory. So I had a free hand to do things as I wanted.

HL: Millikan didn't come back to check up on you?

CA: No, no. I probably talked with him during those years as a graduate student about my research, but I have no memory of doing so. I do remember my final oral examination was scheduled for nine o'clock in the little seminar room in East Bridge. I reported there on time, and no one else was present. Then E. T. Bell, the mathematician, came in. (I had a minor in math.) He said, "Well, I'll start it off." He asked me about Bessel's equation, and I guess for about 20 minutes or so he questioned me on that. I just happened to know Bessel's equation pretty well, and I wrote it on the board, and he asked me various things about it. Then he said, "Well, that's enough; I'm through." We sat there, and nobody else came in until ten o'clock. I guess they all had classes or something. Bell was interested in the history of mathematics, so I had a delightful 40 minutes or so listening to him tell me all about Bessel's childhood. That part of my PhD exam was very simple.



Carl Anderson in 1933 with the magnet cloud chamber he designed and built to measure the energy of electrons produced by cosmic rays. The instrument was installed in the aeronautics building to take advantage of the powerful (400-kilowatt) generator that provided electricity to operate the wind tunnel.



Anderson's cloud chamber unexpectedly produced this photo, which shows the tracks of electrons *and* positrons*. The particles come down from the top and as they move into a magnetic field, the electrons move to the left (there are five electron tracks) and the positrons to the right (there are two positron tracks). Anderson received the Nobel Prize for the discovery of the positron.*

At ten o'clock several people came in. I can't remember who they all were — I think it was Bowen and Millikan and Paul Epstein. And I made one horrible blunder. Millikan asked me to give a review of the history of the photoelectric effect. Of course, the photoelectric effect is involved with visible light, and Millikan became famous for showing that the Einstein equation applied, as well as for measuring the charge on the electron. Those two things were what he got the Nobel Prize for. Well, I forgot all about light and the photoelectric effect of light. Because it was my thesis topic, I gave a history only of the photoelectric effect of X rays, the experiments and theories and so on, and I ignored or forgot about visible light. I don't know if he ever held it against me or not, but the next morning I met him and he said that that was a corking good examination. It wasn't until later that I realized what a horrible blunder I had made.

HL: Did Millikan have any other students?

CA: Yes. He was active, and he had a knack of sensing very early what were the important fields of research in physics. He was the first man in the United States, I'm quite sure, who worked with cosmic rays. It turned out in later years that a lot of physics came out of the study of cosmic rays. And this isn't the only instance. There's the far ultraviolet work that Millikan started. He put Bowen on that when Bowen was his graduate student, and that brought forth all kinds of important new things. He started Charlie Lauritsen on the cold emission — we all know a hot wire will emit electrons, but for cold emission you have a cold wire on which you put a strong electric field to pull out the electrons. That was very new at the time, and led directly to Lauritsen's building the world's first one-million-volt X-ray tube.

One day I asked Millikan directly: "How were you able to sense the importance of fields of physics when they were hardly known to people and nobody was thinking about them? How come you got interested in them?" Millikan's answer was, and he said it as though he was completely serious about it, "I read science abstracts." Well, I told him, "I read science abstracts, too, but I don't get these ideas."

HL: Why did you decide to stay on at Caltech after you had your PhD?

CA: About a year before I was to get my PhD, I went to Millikan and asked him if there was any way I could spend one more year at Caltech. I had two things I wanted to do: One was to learn something about quantum mechanics. I was having a difficult time, and every physicist had to know something about quantum mechanics. And then I had an idea (which grew out of the work I did for my thesis) of working with gamma rays of higher energy than X rays, but with a cloud-chamber technique. In other words, I wanted to study the interaction of gamma rays with matter at as high an energy as I could. And the highest energy gamma rays then available were those from ThC″, which were 2.6 million electron volts. I was going to shoot those through the cloud chamber in a magnetic field.

HL: You already had this magnetic field?

CA: No, I didn't have a magnetic field for the photoelectric effect. It would mean building new apparatus. Another reason I wanted to do that was that C. Y. Chao, who was a postdoctoral fellow, was working with ThC″. He was finding anomalous effects of scattering and absorption of gamma rays, but he had no way of observing the details of what he was doing. He was using electroscopes, which sort of integrate things, and measuring intensities at various angles in relation to pieces of lead absorber, and so on. It wasn't known at the time, but he was actually observing the annihilation radiation of positive electrons.

I went to Millikan to ask if I could spend another year a Caltech doing the same type of work that Chao was doing, but using a cloud chamber, where you could see the details of what's going on. And I'm quite sure that if I had done that, the positive electron would have been discovered before it actually was. That was the direct way, in hindsight, to attack the problem.

Millikan's answer was a very definite no. He said, "You have done your undergraduate work here, you've done your graduate work here. You're getting very provincial. You've got to go somewhere else." The only way you could go somewhere else in those days was to apply for a National Research Council fellowship. So I did. I wrote to Arthur Compton at Chicago and described the proposed experiment to him and said that I had applied for a National Research Council fellowship. He wrote back a very nice letter and said that he would be glad to have me there, and he would do his best in providing facilities, equipment, and some money to build this equipment.

But it never happened. Millikan called me into his office one day and said he wanted me to stay on at Caltech for another year. By that time I had sold myself on the idea of going to Chicago to do this experiment. So I used all of the arguments with Dr. Millikan that he had used on me, and he said, "Yes, that's all true; but your chances of getting a National Research Council fellowship would be very much greater if you had another year at Caltech." It turns out that he was a member of the National Research Council selection committee at the time. So I stayed on at Caltech and worked on this experiment that he wanted me to do, which was quite similar to the one that I wanted to do, except that I wanted to use gamma rays and he wanted me to use cosmic rays.

HL: Did you talk to him any more about the experiment that you wanted to do? Why was he so against it?

CA: Well, he knew that I had used and was familiar with cloud-chamber techniques. As a graduate student, I was measuring mostly the space distribution of X-ray photoelectrons, but to some extent the energy distribution. It was generally believed at that time that the primary cosmic rays from space were like gamma rays, were photons. There was no proof of that; but Millikan had a theory of the creation of cosmic rays, namely, the atom-building hypothesis — that atoms were being built in free space. A bunch of electrons and protons would, in some very mysterious way, arrange themselves in a certain pattern and then coalesce into an atom, and that would give off a calculable amount of energy, presumably in the form of photons. I didn't believe the theory, and I think most people did not believe it.

What Millikan wanted me to do was to measure the energy of the electrons that were produced by the cosmic rays. He had measured the penetrating power of cosmic rays, and they were much more penetrating than any other radiation known. His hypothesis was that they were gamma-ray-like in character, but of much higher energy. One can measure the energy of the gamma rays by measuring the energy of the electrons — the Compton electrons, in those days. So my job was to build an apparatus to measure the energy of the Compton electrons that were produced by the primary cosmic ray photons.

So I started to build a piece of apparatus. Of course, it took almost a year to build it, and in the very first experiments, it became clear that the picture was much more complicated than what was then thought to be the absorption mechanism of the primary cosmic rays — namely, by Compton electron collisions. Immediately, as many positive particles appeared as negative particles, which said something new was happening. The mere presence of the positively charged particles showed that something different was going on than the Klein-Nishina absorption of gamma rays, which was the process by which gamma rays were absorbed, so far as anybody knew at that time.

HL: Do you think that the experiment that you had designed originally would have been better?

CA: No, I'm not saying that. I think it would have found the positive electron sooner than it was found, because in the experiment I was going to do you knew what the incoming radiation was and you knew its energy. In working with cosmic rays, you didn't know what the radiation was that was coming in; in fact, you knew nothing. You didn't know how the particles interacted with matter or what the particles were that you were observing in the cloud chamber. This was chiefly because the energies were so high that it was impossible in a cloud chamber to learn much more than the momentum of the particle and its electric charge. Many of the cosmic ray particles have energies of billions of electron volts.

My apparatus was in the aeronautics building, because the magnetic field had to be as strong as one could possibly get — to deflect the cosmic ray particles to a measurable degree. So I designed a magnet to take the full power of the aeronautics department's generator that provided electricity to run the wind tunnel. That was, as I remember, a 400-kilowatt generator, which could be overloaded for periods like an hour or so at 600 kilowatts. So I designed the equipment to handle 600 kilowatts.

HL: How did you do it?

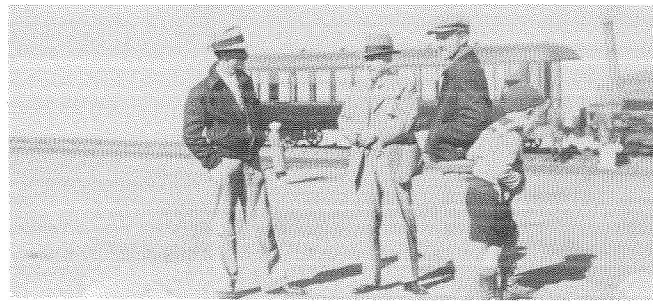
CA: To design a magnet is a very complicated thing. But I knew I had to have magnetic fields that were stronger than you can get by using a magnet of orthodox design because of the saturation effect of iron, which is something like 12,000 gauss. (I guess now you say "webers," or something, but in those days it was gauss.) So what I built was essentially air-core coils, with iron wherever there was any room for it. At the center of the magnet, where the field was the strongest, there had to be a cloud chamber, and you had to be able to see it, which put limitations on the use of iron.

That magnet was used by other people later on, and they thought it was very poorly designed, but they didn't know the purpose it was designed for. As an orthodox magnet, it would have been very poor. But we did get 25,000 gauss over a volume with a diameter of six inches and several inches in depth. It was water-cooled, and we put 40 gallons of water through it a minute. The water came out, not quite but nearly, boiling hot. We were in the aeronautics building, because there's where the generator was, and we were on the third floor because that's where the space was available. And the discharge water used to run out of the magnet into Throop Alley; then it would cross California Boulevard and run down Arden Road. Under certain climatic conditions, there were clouds of steam half a block down Arden. Some of the neighbors objected to that.

HL: Tell me how you found the positron.

CA: That's sort of a long and complicated story. The first thing that came immediately out of the cloud-chamber pictures was a set of high-energy particles of unit electric charge — roughly half posi-

En route to the summit of Pikes Peak in 1935, Anderson and Neddermeyer's 1932 Chevy truck and trailer (rear) are given a tow by a Pikes Peak Company truck.



Anderson (left) and Neddermeyer (right) had a distinguished visitor to their summer outpost at Pikes Peak in 1935 – Robert A. Millikan. The cog-wheel railway car was used to bring tourists up the mountain.

tive and half negative. The fact was that there was no way of knowing anything about the positive particles except that they were positive and had a very high energy. One didn't know what their mass was, for example. But the only known particles of positive charge were protons. So the assumption was that atoms were being broken up by this very high energy radiation into the fundamental building blocks — protons and electrons — the only particles known at that time.

In a cloud chamber, you can, in a magnetic field, make measurements of mass only on slow-moving particles. By slow, I mean moving with a speed of appreciably less than the speed of light. Now, these energies were so high that most of the particles were moving at 90 or 95 percent or more of the speed of light. All you could tell was their charge and momentum. You measured the momentum from the magnetic field and the charge from the density of droplets along the cloud-chamber track. Some of these particles, the positive ones, were moving slowly enough so they should have (if they were protons) exhibited an increase in ionization, which they did not do. Another not very good explanation was that they were electrons going the wrong way, that is, going up. I said to Dr. Millikan, "You wouldn't expect it, but there must be electrons that are going up." These tracks weren't heavy enough to be interpreted as protons.

Millikan said that was ridiculous. They couldn't be moving up — any number of them anyhow — they must be protons. So I decided to put a plate of lead in the cloud chamber, which would prove whether they were moving up or down. Then one day a particle of low energy, so it was very clear that it was moving at much less than the speed of light, went through the lead plate. In fact, it *was* moving upward. It was a clear-cut case, and that's when it became clear to me that these positive things were mostly positive electrons and not particles as heavy as protons.

HL: You said Millikan didn't think it was that. Were there other people who agreed with him?

CA: Millikan told me to publish. I think he felt there was enough evidence for that. I was going to write a letter to the editor of the *Physical Review,* but he said, "Send it to *Science,* because you can get it in print quicker than in the *Physical Review.*" So I sent it to *Science*. But it turns out that all physicists read the *Physical Review,* and only a fraction of them read *Science*. The positive electron was met with disbelief on the whole. Ed McMillan, who was a good friend of mine (he was an undergraduate at Caltech in the class behind me), asked me, "What sort of nonsense is this that you're writing about?" And I read in Dan Kevles's book, *The Physicists,* that Bohr didn't believe it and just passed it off. I heard, too, that Joliot was very angry with me for publishing in *Science,* which he didn't read, instead of the *Physical Review,* because my paper might have helped him with his work.

HL: How was research financed in the 1930's?

CA: My feeling is that Millikan really ran the Institute. Essentially all faculty members in all divisions, if they needed funds for research, went to Millikan and explained their woes and asked for money. He was the one who made the decisions.

The main thing in those days, I think, was that the research that was done did not need the large sums of money that present-day research does. And people were accustomed to making do with very little money and a lot of individual effort. We used to make regular trips to the Southern California Edison Company junkyard in Alhambra to buy for a song — or sometimes have given to us — a transformer or a switch or something else that we needed for our research. I asked Frank Capra, who was at the height of his career as a film director at Columbia Studios, for a motor generator. And I got one that had been used by the movies to run their klieg lights at various locations. It was mounted on a 1911 Pierce Arrow truck that must have been parked in the desert for many years, because the sun had turned the headlight lenses a beautiful purple. They used acetylene headlights in those days. So the truck was towed to Caltech, and we used the motor generator set to provide power for our magnet.

Another example of a different way to finance research happened in the summer of 1935, which Seth Neddermeyer, my first graduate student, and I spent at the summit of Pikes Peak. We bought a used 1932 1½-ton Chevy truck for $300 or $400 and a flatbed trailer from a used trailer lot. A classmate of mine was then an officer of Bekins Moving and Storage Company, and I went to him and said we needed housing for our trailer. He gave us a whole bunch of great big packing cases, and with our own hands and a hammer and a saw we built the trailer housing to protect our apparatus from the elements at Pikes Peak. Our total load, counting the trailer, was over five tons. In a test run, when it was loaded, we ran from California Boulevard up Lake Avenue to Colorado in Pasadena. Now, normally you don't think of that as much of a hill, but it was a stiff second-gear operation for our truck. We made Hope, Arizona, the first day, by

Anderson . . . *continued*

driving all night mostly in first and second gear. But eventually we did get to the foot of Pikes Peak.

HL: Were you planning to be towed up the mountain?

CA: Yes. First we stopped at the Chevrolet agency in Colorado Springs to have the valves ground and change the oil and have a new clutch put in, and then we started out for the mountain itself. We knew very well that we had no chance whatever of getting up the peak under our own power, but we went as far as we could and then managed to get stuck in the middle of the road, where we were blocking traffic. The Pikes Peak Company had quite a bit of equipment to keep the road in repair and keep the snow under control and so on, and they came down with a big company truck, tied that on the front end of our truck, and towed us. With both trucks working as hard as they could, we did get up to the summit.

The Pikes Peak project was a very good thing to do scientifically. It was a great success because the cosmic rays are more intense at higher altitudes, and intensity was one of our major problems. Also cosmic rays have different components, and the components that do the most interesting things increase very rapidly with altitude. The problems leading up to the discovery of the positive electron were resolved in 1932, but there were other particles that didn't behave like electrons, positive or negative, or like protons. They had peculiar properties. I wrote a letter to Dr. Millikan from Pikes Peak, in which I said I thought we had strong evidence for the existence of new particles intermediate in mass between electrons and protons. There were paradoxes in our data, and the existence of new particles would resolve them. But that was a very radical assumption to have to make. We got more cases of tracks of that kind up on Pikes Peak — oh, a hundred times as many as we'd gotten previously in Pasadena. It was an interesting situation, but we did not feel we had enough evidence to publish at that time. But there was enough to write a letter to Dr. Millikan about — and the particles later turned out to be the first mesons, now called mu-mesons or muons. About two years later, in May 1937, Seth Neddermeyer and I, after many more experiments, published our first formal paper announcing their discovery — this time in the *Physical Review.* □

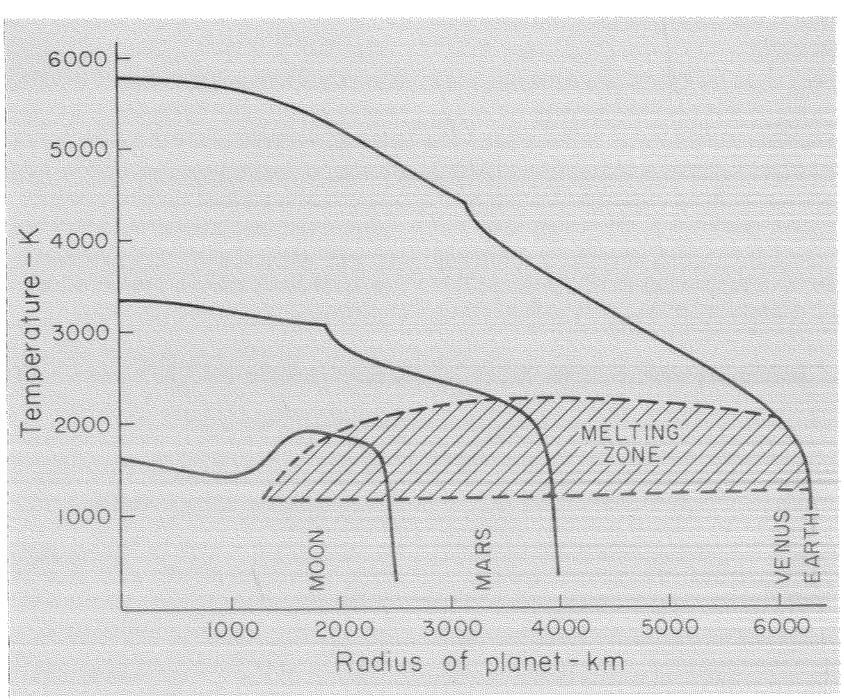
Research in Progress

As the World Churns

Rocks as old as 3.8 billion years have been found on the earth. The earth, however, is estimated to be 4.5 billion years old; where are the 4.5-billion-year-old rocks? Sunk to the bottom of a magma ocean that covered the ancient earth, says Don L. Anderson, professor of geophysics and director of the seismological laboratory.

The idea of a gigantic ocean of molten rock overlying the earth represents a radical departure from recent theories of the earth's early history. These theories generally depict a primordial earth that melted very little and remained basically unchanged from its beginnings until the present. But the chemical differences in the magmas emerging today from oceanic islands and mid-ocean ridges, along with the discontinuities that seismological data have revealed in the mantle, led to Anderson's model.

The earth's mantle, surrounding the superdense nickel-iron core, is a semi-solid region about 3000 kilometers thick on which float the tectonic plates and the thin crust. The mantle hasn't made itself very open to direct observation, but studies of



Temperatures (estimated by Associate Professor of Planetary Science David Stevenson) as a function of radius are shown for planets of various sizes. Temperatures are above the melting point in the region labeled "melting zone." The curves represent the central-to-surface temperatures of successive states of an accreting planet, the bottom one an early state (slightly larger than the moon) and the top curve the later accreted state of an earth-size planet. When a planet grows beyond about moon size, the kinetic energy of accretion is sufficient to melt material in the upper mantle. At great depth the effect of pressure increases the melting point so that the lower mantle is solid. The core is also molten because iron alloys have a low melting point.

seismic waves after large earthquakes have provided a variety of ways of mapping it. Seismic waves behave differently in two regions of the upper mantle. The layer from below the crust down to about 220 kilometers is characterized by low velocity and high attenuation of seismic waves. (This low-velocity zone was discovered in the 1940s by Caltech geophysicist Beno Gutenberg.) The waves have a harder time getting through this region than through the layer between 220 and 670 kilometers, called the transition zone, where seismic waves propagate faster and are less attenuated. At 670 kilometers there is a sharp discontinuity that is an efficient reflector of seismic energy. This is also the depth where earthquakes cease.

Anderson connects this evidence of layering in the upper mantle with geochemical findings that the concentrations of a dozen or so trace elements — including potassium, rubidium, strontium, barium, thorium, and uranium — are enriched in the mantle material emerging from ''hot spots'' where the magma punches up through continents (volcanoes and geothermal areas such as Yellowstone National Park) and ocean islands (Iceland, Hawaii). On the other hand, the same trace elements are depleted in the material coming out of the mid-ocean ridges forming new crust. These two types of magma are complementary; that is, if they were combined, the resulting mixture would have a pattern similar to estimates of the average composition of the earth. They might be complementary because a melt or fluid that extracted trace elements migrated from one source into the other. Anderson's analysis of how migrating materials would extract trace elements suggests that most of these elements are distributed between the two upper mantle reservoirs, which correspond to the layers between the seismic discontinuities.

The magma ocean postulated by Anderson's model would have melted from the heat of accretion as the initial nucleus of the planet, which had condensed out of the solar nebula, was bombarded by other pieces of matter. The heat generated by this accretion process (when the planet had grown beyond moon size) would have been sufficient to keep most of the outer layer continually melted. As the earth kept growing through accretion, these outer layers became inner ones and solidified because the increased pressure at greater depths also increased the melting point. All the mantle material, however, at some time would have gone through the melting process.

When it reached its present size and began to cool, the earth would have still been covered by a molten ocean 300 to 500 kilometers deep. This ocean would be capped by a thin, transient crustal layer. As it cooled, the first rocks to crystallize out of the magma would have been denser than the remaining molten rock and would have sunk. They would have continued sinking even below the floor of the molten ocean until they met the even denser material of the lower mantle. The 670-kilometer seismic discontinuity marks the bottom of this descent and the 220-kilometer demarcation its eventual top, according to Anderson's theory.

At the same time, a thin crust of less dense material would have risen to the top of the melt — like ice on water. But because this first crust would have been so fragile and the convection beneath it so violent, it would have been dragged back down and mixed back into the upper mantle. In between this thin crust and the top of the rock layer that had crystallized out earlier, would lie the last concentrated dregs of the melt, stripped out during the fractionation process. This layer would contain heavy concentrations of all those large atoms that resist crystallization, including those of heat-producing elements such as potassium, thorium, and uranium.

Anderson's model provides an explanation for the appearance of high concentrations of these elements where mantle melt oozes out from continental and oceanic hot spots. This enriched magma is coming from the shallow mantle layer above 220 kilometers characterized by seismic-wave measurements as the low-velocity zone. New crust being formed from the mantle at spreading mid-ocean ridges comes from the transition zone — the region between 220 and 670 kilometers, which would be depleted of those elements not concentrated in the early-forming crystals such as garnet. In this zone would lie the ''lost'' 4.5-billion-year-old rocks, fractionated as the earth formed. The oldest rocks found on the surface today would be remnants of the first crust formed after the outer layer had become strong enough and thick enough to resist being dragged back down into the hot mantle. □

Talking Back

Overcoming language barriers is not a problem solely of tourists, exchange students, diplomats, and missionaries; the barrier also exists between man and machine. Computers have heretofore been able to understand only their own languages, and while a computer language like Basic or Pascal may be easier for us to learn than French, the ability to communicate in English (or any other ''human'' language) would make the vast potential of computer technology much more widely accessible.

The task of teaching English to computers has brought the fields of linguistics and computer science into close alliance. At Caltech, one of a handful of institutions in this country where the field of computational linguistics is being explored, Bozena Henisz Thompson, senior research associate in linguistics, and Frederick B. Thompson, professor of applied philosophy and computer science, have been working in this area for nearly a decade. The Thompsons' research interest is in how humans — for example research teams and management staffs — communicate in problem-solving situations. Their research strategy has been to provide a similar capability between man and computer and then to use this capability as an experimental tool to investigate the nature of the communication process.

Over this last decade they have developed two English language systems — REL (Rapidly Extensible Language) and its even more articulate successor, POL (Problem Oriented Language). REL is designed as a data-base management system, intended for use by a group in a relatively narrow field maintaining large banks of data. The system itself is very broad and flexible and is not limited in vocabulary or syntax to any particular discipline; it ''knows'' more than raw data and can figure out fairly complex definitions and relationships. The goal of a natural language system like REL is to come as close as possible to normal human communication. The Thompsons' REL comes pretty close. Its rapid response time also makes it extremely useful, and it has already leaped beyond its original status as a research tool. For instance, IBM has already im-

Fred and Bozena Thompson converse with a Hewlett-Packard 9845.

plemented sister systems in Germany and Japan.

Bozena Thompson has applied REL in an extensive series of experiments using a real-life task — loading Navy cargo ships. She analyzed how this problem was solved in three different ways: two persons talking face to face; two persons typing out their discussions on separate but connected terminals; and a person "conversing" with a computer in REL. These controlled experiments (involving more than 100 subjects and 80,000 words) confirmed some expected differences in these modes of communication. But some quite surprising similarities also showed up, especially in sentence length and the frequency of sentence fragments. This substantiated the Thompsons' thesis that ordinary conversation exhibits definite, identifiable structural patterns, particularly when that conversation concerns a serious task.

Teaching English to computers has some features in common with translation by computer from one language to another, an undertaking that has largely been abandoned in the United States (although successful applications are being made in Europe and Canada). Bozena Thompson does not agree with the negative assessment of machine translation in this country; even though such translations may not sound natural or be stylistically elegant, they are quite adequate — and helpful — to users familiar with the subject matter. Indeed, in a study of actual users of machine translation (in Euratom and our own atomic energy communities), she found that scientists preferred computer to human translation because the machine translation was "more honest," that is, it had fewer of the distortions that the non-scientist translators tend to make for stylistic purposes.

Natural language systems store vocabularies and rules that enable the computer to analyze a query or command and then generate a program to retrieve the answer or carry out the instruction. Currently, communication between human and computer is via typewriter keyboard, but one long-range goal for natural language systems is to come as close as possible to understanding speech. In general, research in computational linguistics aims at gaining a deeper understanding of how the vast domain of knowledge that underlies human communication can be effectively stored and utilized. What distinguishes the research at Caltech is the emphasis on the experimental method, using real tasks as a primary source of insights into the underlying linguistic aspects of communication.

Linguistic problems abound. Pronouns and conjunctions are only slowly yielding to linguistic analysis. For example, in the sentence, "The lawyer approached the parking-lot attendant and demanded his car," we would assume the lawyer wanted his own car; but in "The thief approached the parking-lot attendant and demanded his car," quite another interpretation is possible. Worse yet is a sentence such as "John came up to Peter and took five dollars out of his pocket." Researchers in computational linguistics are scrutinizing how we ourselves bring to bear our moral judgment about the intentions of thieves and lawyers to determine the more likely antecedent of the pronoun. Similar problems with other forms of ambiguity resolution appear to depend on identifying the participants' common frame of reference and keeping track of it over the development of a dialogue. Surgical reports, for instance, usually end with the statement that "the patient left the operating room in good condition." While doctors would understand that the phrase refers to the person's condition, some of us might imagine the poor patient wielding a broom to clean up the mess.

What the Thompsons learned from their experiments with REL influenced the design of POL. This successor language system, for example, can identify words not in its vocabulary and cope with poor or ambiguous punctuation, spelling, or grammar from its human partner. The commercial version of POL also will not require a room full of machinery. The Thompsons now have it operating on a desk-top Hewlett-Packard computer — an enormous step in reducing the cost of such a system and making it available to the many new users that a natural language system will attract. Fred Thompson predicts that within a decade a typical professional will carry a pocket computer capable of communication in natural language.

One component of human speech that the Thompsons are still working on is phatics — utterances such as "well," "wait," or "you turkey" (all found in the actual person-to-computer protocols) that don't serve the direct purpose of communication but that keep the conversation running. They're natural, all right, but some perhaps are not all that necessary. The optimum degree of human-like behavior on the computer's part is still uncertain, but the POL-speaking computer is not likely to learn to respond in kind to a frustrated user's swearing. ☐

Putnam Problem Solutions

A story in the June issue of *E&S* about the Putnam Mathematical Competition and Caltech's star competitor Peter Shor (BS '81) left some problems for readers to tackle. Here are Shor's answers.

Problem B-3, 1980

For which real numbers a does the sequence defined by the initial condition $u_0 = a$ and the recursion $u_{n+1} = 2u_n - n^2$ have $u_n > 0$ for all $n \geq 0$?

(Express the answer in the simplest form.)

You can make the recursion easier by defining v_n's by

$$v_n = u_n \cdot 2^{-n}$$

u_n will be positive whenever v_n is positive.

Then $u_n = v_n 2^n$, and $v_0 = u_0 = a$.

The formula $u_{n+1} = 2u_n - n^2$

becomes $2^{n+1} v_{n+1} = 2 \cdot 2^n v_n - n^2$,

or $v_{n+1} = v_n - n^2 \cdot 2^{-n-1}$.

This gives

$$v_{n+1} = a - \frac{1}{2^2} - \frac{2^2}{2^3} - \frac{3^2}{2^4} - \ldots - \frac{n^2}{2^{n+1}}$$

v_n will be positive for all n if

$$\frac{1}{2^2} + \frac{2^2}{2^3} + \frac{3^2}{2^4} + \ldots \leq a$$

Now, if $x = \frac{1}{2}$ this is

$$x^2 + 2^2x^3 + 3^2x^4 + \ldots$$

To sum this series, consider

$$f(x) = 1 + x + x^2 + \ldots = \frac{1}{1-x}$$

$$\frac{d}{dx} f(x)$$

$$= 1 + 2x + 3x^2 + \ldots = \frac{1}{(1-x)^2}$$

$$x\left(\frac{d}{dx} f(x)\right)$$

$$= x + 2x^2 + 3x^3 + \ldots = \frac{x}{(1-x)^2}$$

$$\frac{d}{dx}\left(x\frac{d}{dx} f(x)\right)$$

$$= 1 + 4x + 9x^2 + \ldots = \frac{1+x}{(1-x)^3}$$

$$x^2\frac{d}{dx}\left(x\frac{d}{dx} f(x)\right)$$

$$= x^2 + 4x^3 + 9x^4 + \ldots = \frac{x^2(1+x)}{(1-x)^3}$$

and this series is what we want with $x = \frac{1}{2}$. So, the series converges to 3, and the answer is $a \geq 3$.

Problem B-4, 1980

Let $A_1, A_2, \ldots, A_{1066}$ be subsets of a finite set X such that $|A_i| > ½|X|$ for $1 \leq i \leq 1066$. Prove there exist ten elements $x_1, \ldots, x_{10}$ of X such that every A_i contains at least one of $x_1, \ldots, x_{10}$.

(Here |S| means the number of elements in the set S.)

We can find an element x_1 that is contained in more than half the sets A_i. If there were no such x_1, then $|A_1| + |A_2| + \ldots + |A_{1066}|$ would be less than or equal to $1066 \cdot \frac{1}{2}|X|$ because each x is in at most half the A_i's. But for each A_i, $|A_i| > \frac{1}{2}|X|$.

Consider only the sets A_i with x_1 not contained in A_i. There are at most 532 of these, and each of them contains more than half of the elements of X. Repeating the argument above, there is an x_2 contained in more than half of these 532 remaining sets, leaving at most 265 sets without x_1 or x_2. Repeating this ten times, you get $x_1, x_2, \ldots, x_{10}$, with each A_i containing one of them.

Problem A-6, 1978

Let n distinct points in the plane be given. Prove that fewer than $2n^{3/2}$ pairs of them are unit distance apart.

Draw a circle of radius 1 around each of the points p_i. Define e_i as the number of points p on the circle around p_i. $\frac{1}{2}\Sigma e_i$ is the number of pairs of points at distance 1, since each pair gives two points on the circles. We can assume $e_i \geq 1$ for all i, since otherwise we have a point unit distance from no points.

Each pair of circles has at most two intersections, so there are at most n(n-1) intersections. The point p_i occurs as an intersection $\frac{1}{2}e_i(e_i-1)$ times, so we have

$$n(n-1) \geq \Sigma \frac{e_i(e_i-1)}{2} \geq \frac{1}{2}\Sigma (e_i-1)^2.$$

By the Cauchy-Schwarz inequality,

$$[\Sigma (e_i-1)]^2 \leq [\Sigma 1] [\Sigma (e_i-1)^2] \quad n \Sigma (e_i-1)^2.$$

So, combining the above equations,

$$[\Sigma (e_i-1)]^2 \leq n2n(n-1) < 2n^3, \text{ so}$$

$$\Sigma (e_i-1) \leq \sqrt{2}n^{3/2}$$

$$\frac{1}{2}(\Sigma e_i) \leq \frac{n + \sqrt{2}n^{3/2}}{2} < 2n^{3/2}$$

And finally, there's Problem A-4 from 1980, the first part of which was solved in the June issue, leaving (b), the harder part, for readers.

(a) Prove that there exist integers a, b, c, not all zero and each of absolute value less than one million, such that

$$|a + b\sqrt{2} + c\sqrt{3}| < 10^{-11}$$

(b) Let a, b, c be integers, not all zero and each of absolute value less than one million. Prove that

$$|a + b\sqrt{2} + c\sqrt{3}| > 10^{-21}$$

In (b) you can multiply by conjugates to get rid of the square roots:

$$(a + b\sqrt{2} + c\sqrt{3})\,x\,(a + b\sqrt{2} - c\sqrt{3})$$
$$x\,(a - b\sqrt{2} - c\sqrt{3})\,x\,(a - b\sqrt{2} + c\sqrt{3})$$
$$= a^4 + 4b^4 + 9c^4 - 4a^2b^2$$
$$- 6a^2c^2 - 12b^2c^2.$$

If a, b, and c are all integers, then this will be an integer, and if it's not equal to 0, it has to be of magnitude at least 1.

In the first part of the problem we showed that a number of this form had to be less than 10^7, so if the product of the conjugates is not 0, then we have $a + b\sqrt{2} + c\sqrt{3}$ times three factors that are less than 10^7 yielding something bigger than or equal to 1, and therefore

$$|(a + b\sqrt{2} + c\sqrt{3})|\,(10^7)^3 > 1$$

$$|a + b\sqrt{2} + c\sqrt{3}| > 1/10^{21}$$

and we're done.

Now all we have to do is show that the product of the conjugates cannot be equal to 0, and we can do this by proving that it's impossible for any of the four conjugate factors to be 0. If

$$a + b\sqrt{2} + c\sqrt{3} = 0, \text{ say,}$$

then

$$a + b\sqrt{2} = -c\sqrt{3}.$$

Now, squaring both sides, we get

$$a^2 + 2ab\sqrt{2} + 2b^2 = 3c^2$$

$$a^2 + 2b^2 - 3c^2 = -2ab\sqrt{2}.$$

If a and b are not 0, then this last equation could be used to express $\sqrt{2}$ as a quotient of integers, which is impossible since $\sqrt{2}$ isn't a rational number. So it only remains to consider the possibility that a = 0 or b = 0. If b = 0, then

$$a + c\sqrt{3} = 0,$$

which would make $\sqrt{3}$ a rational number unless c = 0. But if c is 0, then obviously a is 0, so a, b, and c are all 0, contrary to assumption. So the only remaining possibilities are in the case $a = 0, b \neq 0$.

In this case

$$b\sqrt{2} = -c\sqrt{3}$$

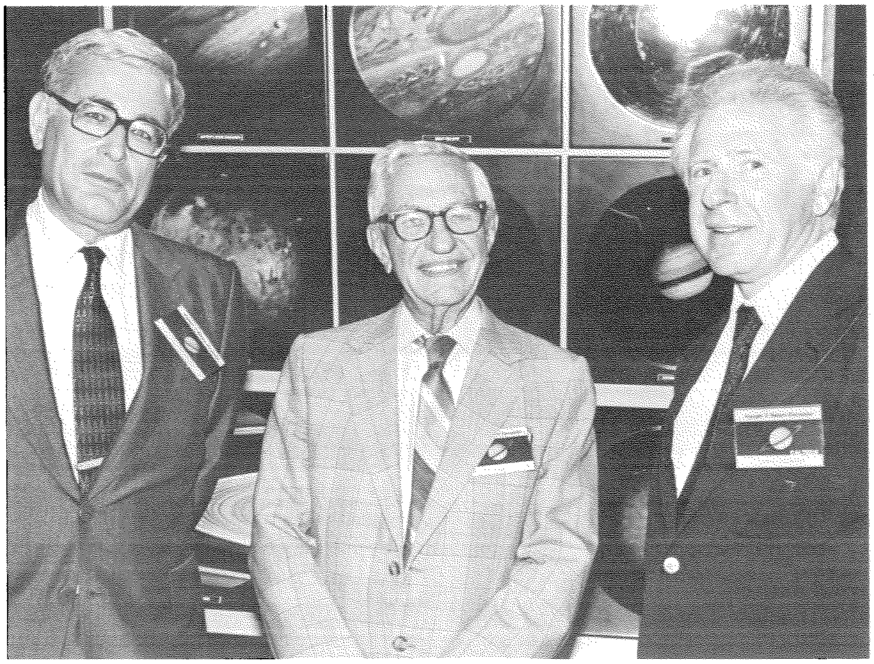
so we get

$$\sqrt{6} = -3c/b,$$

which is impossible because $\sqrt{6}$ is not rational, and this completes the argument.

Random Walk

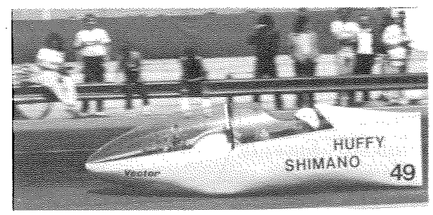
Summit Meeting



The Voyager 2 Saturn encounter in late August attracted a crowd of scientists, journalists and public officials to Caltech's Jet Propulsion Laboratory. And it provided the occasion for an unprecedented meeting of three Caltech presidents, past and current. From left to right, Harold Brown (1969-1977), Lee A. Dubridge (1946-1968), and Marvin L. Goldberger, president since 1978.

Energy Saver

Almost everybody has heard about the achievements of Caltech's distinguished alumnus Paul MacCready (MS '48, PhD '52) in developing two man-powered flying machines — the *Gossamer Condor* and the *Gossamer Albatross* — and about his latest success, the sun-powered *Solar Challenger*. None of these, says MacCready, has any direct practical applications, but they may yield indirect benefits. These peculiar aircraft stimulate thinking about what is achievable in doing many

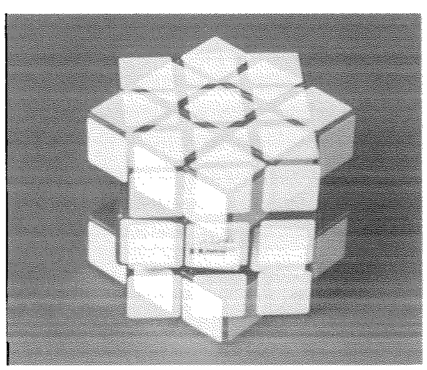


This human-powered vehicle was developed in the U.S. MacCready is working on something similar but not necessarily so racy.

kinds of jobs with less energy and less material — and thus less cost in dollars, pollution, and consumption of resources. Now, in addition to his work on environment and alternative energy techniques at his Pasadena company, AeroVironment, MacCready is exploring the development of safe, low-cost surface transportation devices. There have been for some years streamlined human-powered vehicles in which people have propelled themselves at 60 mph. MacCready's goal is vehicles of more modest performance, which, for safety, would be limited to "commuter speeds" of under 35 mph, and which might even augment human power with battery or liquid fuel. Right now he is trying to coax others into carrying on similar efforts, but he is also simultaneously working on his own version.

Cube Root

Manipulating the nine small cubes on each of the six sides of the Rubik's Cube until the big cube has a different color solidly covering each face is a "game" that has had millions of people going around in circles for months. Since there are more than 43 quintillion potential color combinations on the cube, a certain amount of mass vertigo isn't surprising. But Caltech alumnus James Nourse (PhD '74), who is now a research associate at Stanford, has come up with a solution — and written a how-to book about it. Nourse, a chemist, visualized the cube as a molecule and the moveable cubelets as atoms, and then applied mathematical methods of research in his field to figure out a procedure. It took 4½ hours for his first — and successful — try and approximately a minute for each go-around since. Impressive as this accomplishment is, he is also to be commended for his skill as a modern-day alchemist. His book, *The Simple Solution to Rubik's Cube* (Bantam Books) has sold nearly 1¾ million copies in its first three months, making it, he says, "a sideline that has turned into a gold mine."



Going around with Rubik's Cube?

Coming Events

The Watson Lecture series for the first half of the 1981-82 academic year has a stellar cast. Leading off on October 28 will be William Bettes, a member of the professional staff in aeronautics, whose topic is "The Aerodynamic Drag of Road Vehicles — Past, Present, and Future." On November 18, Jeremy Brockes, associate professor of biology, will talk on "Nerve, Myelin, and Multiple Sclerosis." Thomas Tombrello, professor of physics, will discuss "Particle Tracks in Solids" in his lecture on December 9. After time out for the Christmas holidays and getting second term started, the series picks up again on January 6 with Leon Silver, professor of geology, speaking on "The California Crustal Shuffle." And the series winds up on January 20 with Bruce Murray, professor of planetary science and director of JPL, asking "Where Do We Go Next in Space?"

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