Engineering & Science

California Institute of Technology | November-December 1979

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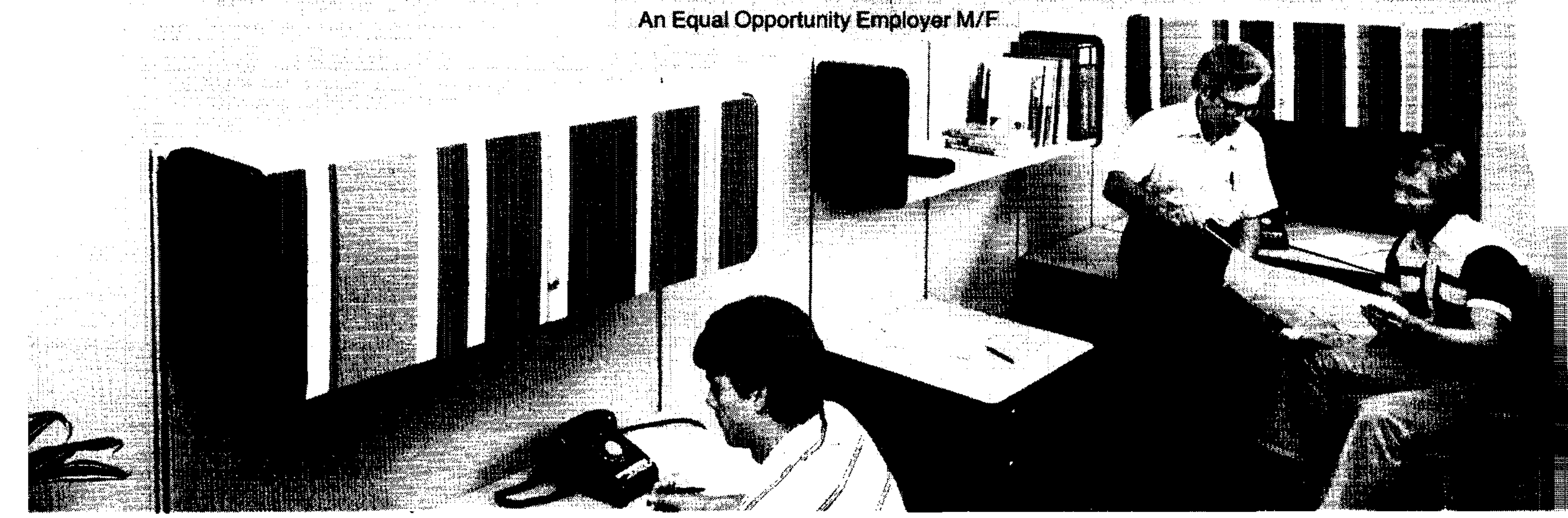

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In This Issue

Stony Silence
On the cover — a statue of George Ellery Hale? Well, not necessarily, but that's one guess, and after an inconclusive investigation, we've decided guessing is all anyone can do. "A Capital Idea" on page 13 has more pictures of great stone faces on campus and a little history of their origin.

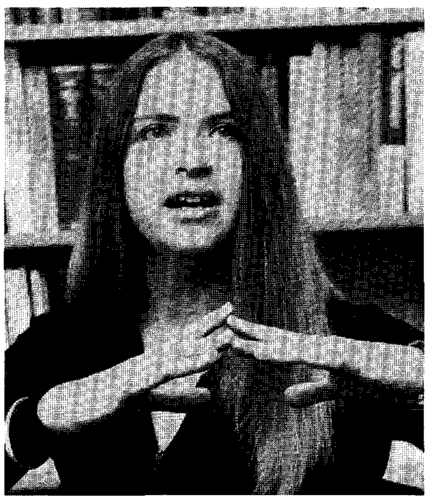
October 4, 1979 — Elizabeth and Ed Hutchings, with Lee DuBridge

Not in This Issue
For the first time in more than 30 years (and 238 separate issues) the masthead of this magazine does not list the name of Edward Hutchings Jr. as its editor and business manager. This is because Ed decided to retire as of October 1, a fact that brought some 150 of his friends and colleagues to honor him at a dinner at the Athenaeum on October 4.

The idea for the dinner came from Ed's long-time friend Charles Newton, lecturer in English emeritus; it was presided over by President Emeritus Lee DuBridge and highlighted by a brief talk by Ed himself — a talk that was illustrated with slides of some hitherto unseen photos from his private "You Can't Print That" file. James Workman (BS '57, MS '58), vice president of the Alumni Association, presented a gift from the Association, and DuBridge recalled some of the scientific progress of the last 30 years that *E&S* has reported. He also presented Ed with a 20th-anniversary volume of letters and articles, assembled by Newton, from many of the contributors to *Frontiers in Science* (a collection of some of the best articles published in *E&S* between 1949 and 1959, selected and edited by Ed Hutchings).

All this was very festive, but Ed has created such respect for both the magazine and himself that puzzlement and apprehension were also components of the evening. In fact, two questions heard frequently before, during, and since have been: How has he done it? and How can he ever be replaced?

The answer to the second question is, of course, "He can't." But one answer to the first — plus some autobiographical information — can be found in his own words on page 24. "Editing from Scientist to Informed Layman" is an illustrated adaptation of a speech Ed made some time ago giving his approach to the job of editing *Engineering and Science*. The new editor couldn't ask for a better blueprint.



Karen McNally

Shakeup
Earthquakes are a subject of such interest and concern to so many people that *E&S* frequently prints information about them and about seismological research. After all, we have a completely reliable source of material — the distinguished staff of Caltech's Seismological Laboratory. Recently they (and we) have begun seriously discussing earthquake prediction, partly because in spite of a lot of uncertainty about the subject, now and then an earthquake prediction comes true.

One of the best documented examples is that of the Oaxaca, Mexico, earthquake of November 29, 1978, the subject of a recent Watson Lecture by Karen McNally, senior research fellow in geophysics. In "Trapping an Earthquake" on page 6, which was adapted from that lecture, McNally discusses some of the problems and payoffs — human and scientific — of that prediction.

McNally knows whereof she speaks, because she was on the scene with an array of seismographs. And, in a way, she had been preparing for an opportunity like that ever since she read an article in *Scientific American* at the age of 23 and decided to become a geophysicist. A native of California, she attended UC Berkeley, becoming in 1976 the first woman in 46 years to receive a PhD in geophysics from that institution. She has been at Caltech ever since, except, of course, on those not-so-rare occasions when she's monitoring earthquakes out in the field.

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November-December 1979

Trapping an Earthquake

by Karen McNally

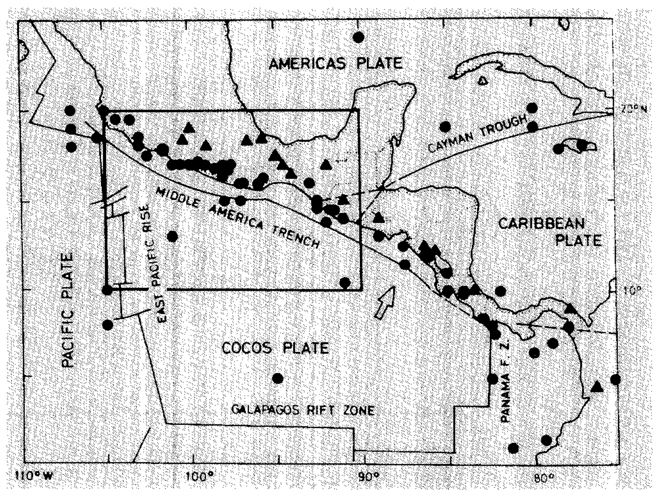
As a science, earthquake prediction is an infant — so young, in fact, that it is only just beginning to emerge from the realm of science fiction. The important way a science is distinguished from fantasies is by the application of the scientific method, that is, by the systematic collection and classification of data and the formulation and testing of hypotheses based on those data.

This is, of course, an academic definition, but it is also an accurate description of what happened that allowed us to "trap" the Oaxaca, Mexico, earthquake of November 29, 1978. In that instance, we had a case history of science in operation. There were data that led to the making of a scientific forecast that an earthquake would occur within a particular area. In response to that prediction, a group of seismologists from Caltech and the University of Mexico jointly placed seismographic instruments in the area to collect further data. Just about three weeks later, an earthquake of magnitude 7.8 occurred, precisely where it was expected. To help relate the size of this earthquake to others about which we have more knowledge, let me point out that this was close to the magnitude of the 1906 San Francisco earthquake and 120 times the size of the 1971 San Fernando earthquake.

The accurate prediction of the Oaxaca earthquake is of considerable interest to seismologists, of course, but it also has wider sociological implications. What happened to the people of that area of Mexico as a result not only of this carefully evaluated scientific prediction but also of a widely publicized non-scientific prophecy related to it could well be the script for what could happen under similar circumstances in, for example, southern California. In fact, what happened leads seismologists to urge the public and the appropriate governmental agencies to prepare for handling earthquake predictions as well as actual earthquakes.

The setting for the Oaxaca earthquake was along the west coast of Mexico. Here, deep in the Pacific Ocean, the Cocos Plate subducts, or dives, into the Middle America Trench and beneath the continent, producing many earthquakes in the process. The subduction of this plate is in conjunction with the East Pacific Rise, a spreading seafloor ridge that continues into the Gulf of California and extends northwestward to become a transform fault that we know as the San Andreas. While the motion of the San Andreas fault is different in that its two sides slide past each other, nevertheless it is an extension of the system that drives the Cocos Plate underneath the coast of Mexico and Central America.

In the period since about 1898 this area along the coast of Mexico has experienced more than 40 earthquakes of magnitude 7.0 or larger. In a similar period and over a coastal area about three-quarters as long, California has had 6 such earthquakes. If we normalize these numbers per square kilometer, we find that Mexico has five times as many earthquakes, which should make clear at least part of our motivation for going outside California to study earthquakes — that is, the availability nearby of extensive



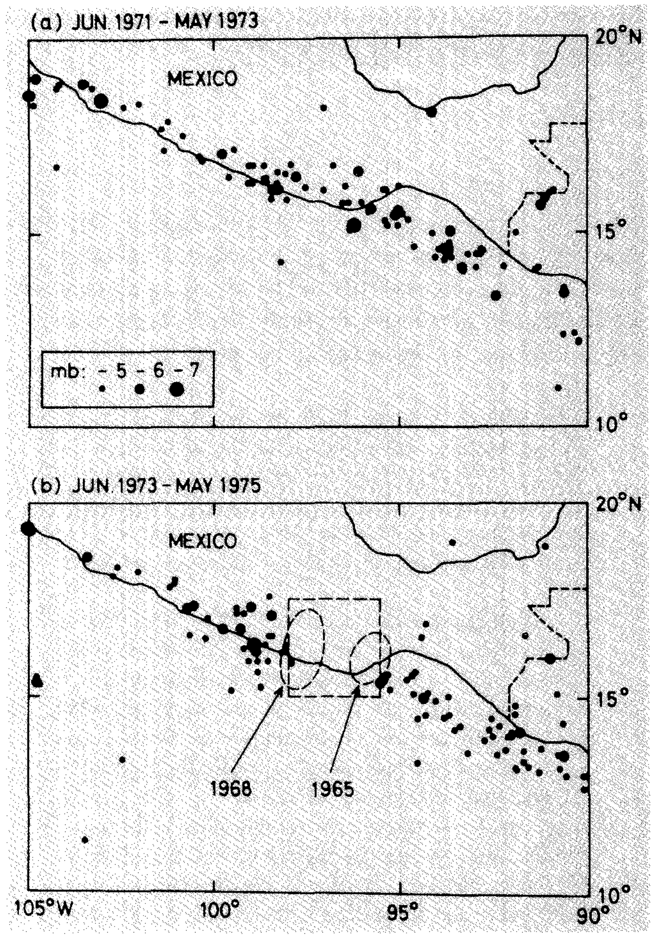
Mexico and Central America make a diagonal strip across this map, with the tectonic features that affect them on either side and the open arrow showing the direction of subduction of the Cocos Plate into the trench and beneath the continent. The black dots indicate the locations of large shallow earthquakes since 1898, and the triangles are for those originating deeper than 65 kilometers beneath the surface. The rectangle encloses the area within which the Oaxaca earthquake occurred last year. (After Ohtake, et al, 1977) material for research. We hope that when the next magnitude 7.0 or larger earthquake strikes California (and it will happen), we will have a better understanding of what to expect than we have had in the past.

Even though California has fewer large earthquakes than Mexico, we are fortunate in having an extremely dense array of seismographs, particularly in southern California where the density of the population gives rise to legitimate concern about the effects of large earthquakes on both structures and people. Instrumentation elsewhere in the world, unfortunately, is scarce, and data are basically lacking. This was certainly the situation in Mexico. But we were able to have an array of instruments in the field there by the first week of November of last year, so we have data from just over three weeks of detailed signals before the actual occurrence of the predicted earthquake, plus records of the seismic activity since.

A combination of circumstances led to the operation of a seismographic array by Caltech and the University of Mexico in the Oaxaca area at the appropriate time to trap this predicted earthquake. First, we knew about the forecast and that it was based on very thought-provoking data. Another related factor was that because an earthquake had recently occurred in the area in question, I was invited to give some lectures about Caltech's earthquake studies to a group of scientists (who also knew about the forecast) at the University of Mexico in August 1978. During one of these lectures, a second earthquake shook Oaxaca, and we decided to work together to try to discover what was occurring.

The forecast itself was made by scientists from the University of Texas in 1977, and it aroused great interest among the staff of Caltech's Seismological Laboratory. The evidence presented by the Texas group, which was so convincing to us, went something like this: Along the coast of Mexico earthquakes were fairly uniform in frequency between 1971 and 1973. In the area around Oaxaca from 1973 to 1975 they had suddenly ceased. In 1965 on one side of the area in question there had been an earthquake of magnitude 7.6, and on the other side of the area there had been one of about the same magnitude in 1968. The space along the subduction zone in between — a spatial seismic gap — had not broken.

As a matter of fact, in 1973 a group of seismologists from the Lamont Geological Laboratory had pointed out that this portion of the coastal region was a seismic gap area since no large ($M \geqslant 7$) earthquake had occurred there since a major episode of energy release in 1928 and 1931. They also pointed out that the average time periods between earthquakes of magnitude 7.0 or larger repeating at the same location was on the order of 30 years in this region. So the area seemed overdue for a seismic event.



These two earthquake patterns were significant in the development of the forecast of the Oaxaca earthquake — first, generalized earthquake activity along the coast between 1971 and 1973 (top) and second, the development of a seismic gap in a part of that area between 1973 and 1975 (bottom). The locations of the 1965 and 1968 earthquakes on either side of the gap and their aftershock areas are also shown. The size of the dots indicates the magnitude of the earthquakes. (After Ohtake, et al, 1977)

The stopping of seismic activity between 1973 and 1975 in this area was considered by the scientists from the University of Texas to be significant anomalous behavior and possibly a prelude to a large earthquake. They based this conclusion in part on data from the 1965 and 1968 earthquakes. In both of those cases, activity stopped for a period of time, then resumed for a period, and then the main shock occurred. Looking at this particular seismic gap in the Oaxaca area in terms of both location and point in time, they noted the two-year-long cessation of activity, and forecast that a large earthquake was likely to occur following a resumption of seismic activity. They did not say definitely when it would happen nor precisely how large it would be, but they estimated it would be about the same size as those of 1965 and 1968.

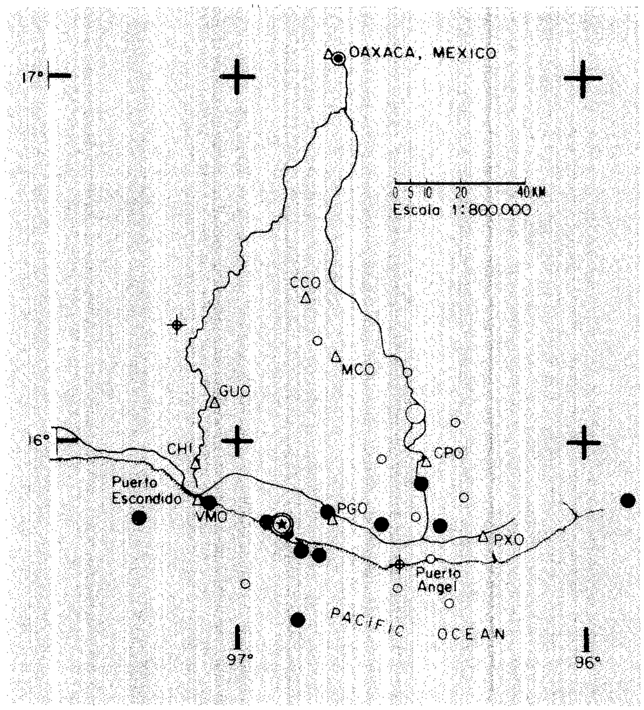
At this point I want to shift gears and describe the effect of this prediction, plus a related prophecy, on the people of the State of Oaxaca, particularly those of the town of

Trapping an Earthquake

Pinotepa. It may sound bizarre, but it happened, and who is to say it would not be similar in southern California under the same circumstances? The following account was written by two professors of geophysics from the University of Mexico: T. Garza and C. Lomnitz.

> On the 7th of February, 1978, two residents of Las Vegas, Nevada, sent a letter to the President of Mexico, which contained the following prediction, based on "demonstrated scientific facts": "Earthquake in the State of Oaxaca in the town of Pinotepa on 23 April 1978 and large quantities of water causing flooding."
>
> A copy of this letter reached the office of the Mayor of Pinotepa a few days later. Some of the effects of the "prediction" were described in the local Pinotepa newspaper as follows: "After this announcement, there has been a tremendous commotion on the Oaxaca coast, to the point where many persons are fleeing their homes to emigrate to other towns in Mexico. . . . The psychosis caused by the alarming news has induced them to sell their properties to the highest bidder, thus destroying their homes. . . . At first it was a speculative news item, but so much has been written about it that it has brought damage to all of Oaxaca as well as to the neighboring states of Guerrero, Michoacan, Puebla, and others. . . . Unfortunately, there has been panic, particularly in Pinotepa and nearby coastal towns, and this is understandable because no one wishes to endanger their families; some local people have already sold their property, and people with money are buying land. . . . One wonders: Who are these people picking up cheap real estate along the Oaxaca coast?"
>
> Meanwhile, a UPI press release from Austin, Texas, was headlined on the front page of a Mexican daily: "Texas U predicts big Mexico quake." This appeared to lend legitimacy to the prediction for April 23. The press report did not contain a specific date, but it was studded with phrases such as "A massive earthquake will occur soon in the state of Oaxaca," and "UT researchers expect the quake to be stronger than those that shook Managua and Guatemala," and so on. Verbatim quotes attributed to a reputable U.S. scientist appeared to confirm the earthquake threat to Oaxaca.
>
> The American press caused speculation to flare up from Acapulco to Salina Cruz. One Acapulco local newspaper hatched a fantastic story, complete with "geological sections," claiming that a foreign power had buried half a dozen nuclear charges in a fault located off the Oaxaca coast, to be detonated on April 23 by a remote control from a plane flying at an altitude of 15,000 feet. Unfortunately, this story was widely circulated, if not actually believed. A surprisingly large number of people thought that oil or uranium had been discovered on the Oaxaca coast and that cheap leases were being sought by foreign nationals. The questions asked by reporters reflected similar beliefs.
>
> Sunday, April 23, was a hot, sunny day in Pinotepa. The Governor of the State of Oaxaca had announced that he would be in attendance to preside over special festivities organized "to reassure the people of the Oaxaca coast that nothing was going to happen on that date with reference to the said earthquake." Folk dancing groups, musicians, and politicians had been brought in from the State Capital. The Mayor of Pinotepa told us that there had been no letup in seismic activity in recent years with reference to the seismic gap. "We feel some 50 or 60 earthquakes every year," he said. "If there had been a lull, people would have talked about it. We just had another shock four days ago." He proceeded to tell us that the two strongest earthquakes announced the beginning and end of the rainy season each year.
>
> The Mayor was indignant about the prediction, which he claimed had caused more damage to Pinotepa than the 1968 earthquake. Though he used strong and profane language, he also claimed that the reports of widespread panic were exaggerated. "Those who left were mainly out-of-towners," he stated. "Only about 15 percent of the citizens of Pinotepa are economically well off and can afford to leave."
>
> A stroll through the town revealed that perhaps 20 percent of the homes were shuttered, indicating that the residents were out of town.
>
> The Governor arrived at 5 p.m. and proceeded to the Town Hall where a special exhibit (including a tent used for emergency housing) had been prepared by the Office of Urban Emergencies of the federal government. At 17:40:02 local time, while the Governor was being shown around the exhibit, an earthquake shook Pinotepa and startled the crowd inside the Town Hall. The shock felt like a nearby local earthquake and was not even recognized as an earthquake by some people though it caused the metal doors to vibrate audibly. The Governor and his party were unperturbed, and later denied they had felt an earthquake.
>
> The festivities proceeded as planned. Around 10 p.m. a merry public dance began in the town square. Shortly after midnight, the Governor looked at his watch and decided that it was time to return to Oaxaca City as the prediction had lapsed. Some local residents still took the small shock in the afternoon as proof that the prediction had been partially successful, but everyone was relieved that no disastrous earthquake and no tsunami had occurred.

For whatever it is worth as an object lesson, then, that is a description by a Mexican seismologist of the effects of



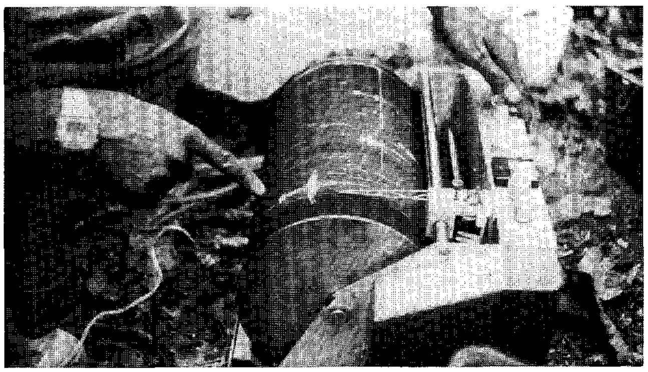
The route of the seismograph array (the stations are indicated by triangles) that trapped the Oaxaca earthquake started at Puerto Escondido, ran inland and upward 150 kilometers, and then turned back toward the coast, ending at Puerto Angel. The star is where the main shock occurred, and the black dots are the largest aftershocks within the following week. The open circles — in addition to the one around the main shock — show the locations of aftershocks as given by the standard worldwide network in the absence of local seismograph station data.

an earthquake prediction on a group of people who were not prepared to cope with it. Both Mexican and American seismologists did believe, however, that an earthquake really might occur near Oaxaca and that it was likely to be a large one. So we people at Caltech gathered our troops, our small budget, and some good equipment for working in Mexico, and joined with our colleagues from the University of Mexico to put out the array. Three weeks after it was in place, the earthquake occurred right in the middle of our net. Thus we had a complete history of what went on in those weeks before the main shock as well as after the main shock.

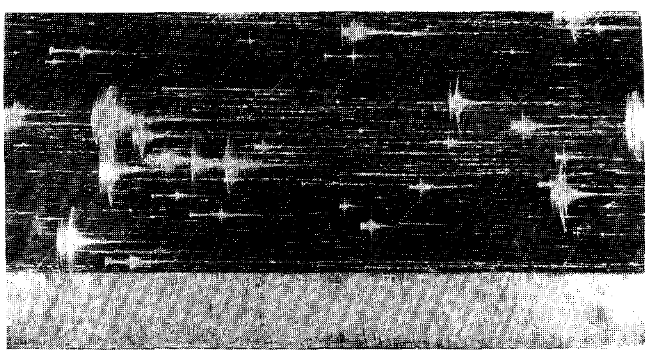
Our base camp was at Puerto Escondido on the coast of Mexico. From there, the stations of the array were strung out in a loop 150 kilometers up into the mountains above the coast and down again to Puerto Angel, some 100 kilometers east southeast of Puerto Escondido. Every station was an instrument that had to have its paper seismograms changed every one or two days, and that meant making it all the way around the loop for each change. It also meant crossing rivers where there were no bridges; it meant driving on roads that were often mere tracks through dense jungle; it meant a climb from sea level to 5,600 feet; and that meant at least eight hours of travel time — at about 11-13 miles per hour — plus the time it took to service the equipment.

The instrument we used was a battery-driven rotating drum. Around it was wrapped a piece of paper that had been smoked by holding it over a kerosene lantern. A needle, lightly in contact with the paper, scratched a record of any vibration felt by the instrument. This is one of the oldest forms of recording, recently brought into use for portable field array instruments. It is extremely simple, requiring very little except a suitcase to carry it, a source of smoke, and lacquer to coat the finished seismograms in order to fix the tracing. In this particular area there was one other requirement. Because of the intense heat in that area of Mexico at that time of the year, we had to construct foliage shelters over the instruments to keep them from malfunctioning.

After the earthquake occurred, there were — as one of the Caltech seismologists phrased it — "people ungrinding their axes." One of those people was the governor of Oaxaca. He was so pleased with our fine job of figuring out that there was going to be an earthquake that he just had to come right down to our base camp (where he had never been before), bring along his entire press group, and make a speech. Then a member of the opposition took the stand and said that there shouldn't be studies of earthquakes in this area when people are hungry. So our work became something of a political issue.



Above is a rotating drum covered with smoked paper on which a needle scratches the trace of vibrations felt by the seismometer. Below the actual tracing of the Oaxaca earthquake on a seismograph located at station PXO is so saturated that it looks like an almost solid band across the bottom of the strip of paper. The scattered tracings above that area are of the lesser shocks detected in the two days before the main shock.



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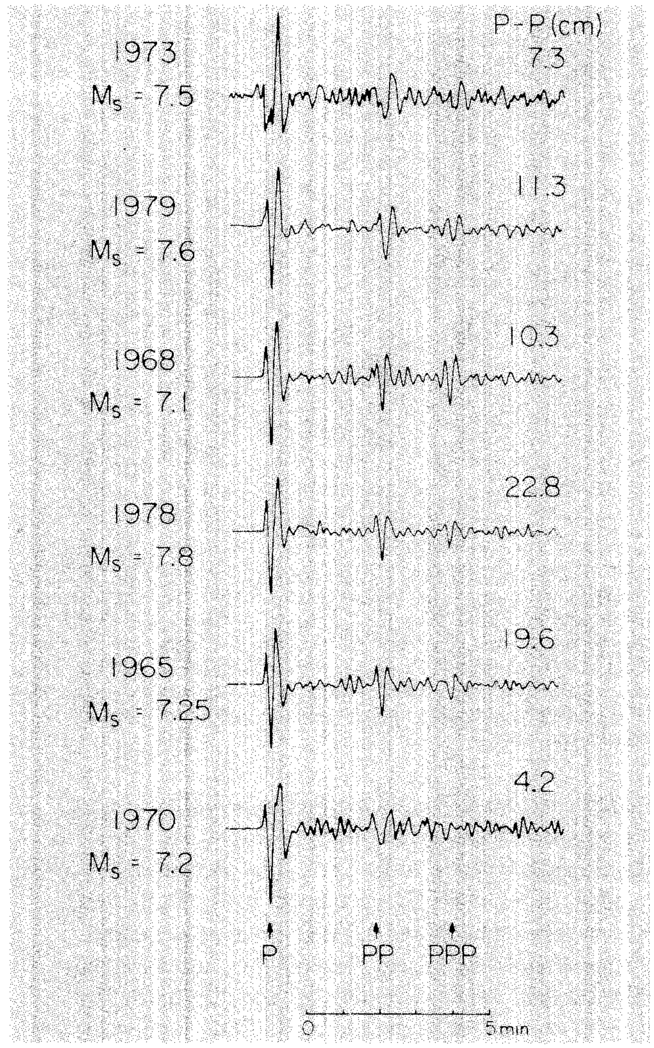
How much damage was caused by this very large earthquake? Amazingly little as far as we can tell, and much that was claimed by the local people was obviously in the hope of their getting the government to pay for repair of previously existing damage. Generally we have assumed that we have some ability to predict how damaging to structures an earthquake will be, based on its size. But this experience taught us that the matter is far more complicated than that. Are earthquakes along subduction zones typically low-damage tremors? Is there something in the way such earthquakes occur or the properties of the subduction zone or the nature of the failure itself that results in relatively small damage? We don't know the answers to those questions, but we are looking in a number of different ways at the data we have gathered to try to understand the main determining factors.

One of the first things to do was to compare the available records of the seismic wave forms obtained for other large earthquakes along the Mexican coast — in order, south to north, those of 1970, 1965, 1978, 1968, 1979, and 1973. We had records of the wave forms at teleseismic distances (that is, records obtained on instruments located at some distance from the actual event — at Caltech, for example, in the case of the Mexican earthquakes), and we could check them. All but one of these records showed extremely small, simple pulses in their first wave form. In contrast, the northernmost of these quakes showed an extremely complex pulse, indicating a ratcheting kind of effect as the earth broke and generated a high-frequency wave. Obviously, one of the first things for us to do in the light of that finding is to compare the damage reports for each of these areas in those earthquakes. We need to identify the areas of simple and complex sliding to see whether there is a correlation with the amount of damage.

We already knew that the size of an earthquake is related to the area that slipped and the amount of slippage, and we were able to obtain comprehensive data on those factors in this earthquake because the field array gave us excellent constraints on the aftershock area. We found that this earthquake was what we call a low-stress drop earthquake, and this may be another ingredient in low-damage earthquakes.

Since we had an array in place for this earthquake, we were also able to analyze the data to confirm the existence of tectonic plates, plot their depth at various distances inland, and determine the relation of the subduction failure to that depth. This is the first time this kind of information has become available in this region.

We have, of course, very well documented records of the foreshock activity in the last three weeks before this earthquake occurred. At first the seismic activity was quite low, with just a few events occurring at the edges of the area we were studying. Then a cluster of earthquakes broke the silence in the quiet region around the main shock. The quiescence then resumed within that area, but there was a much larger amount of activity around the periphery of the region. Nevertheless, within about a 30-kilometer radius of the main shock, nothing further occurred until, finally, in the last 1.8 days before the main shock, earthquakes again clustered in the quiet zone.

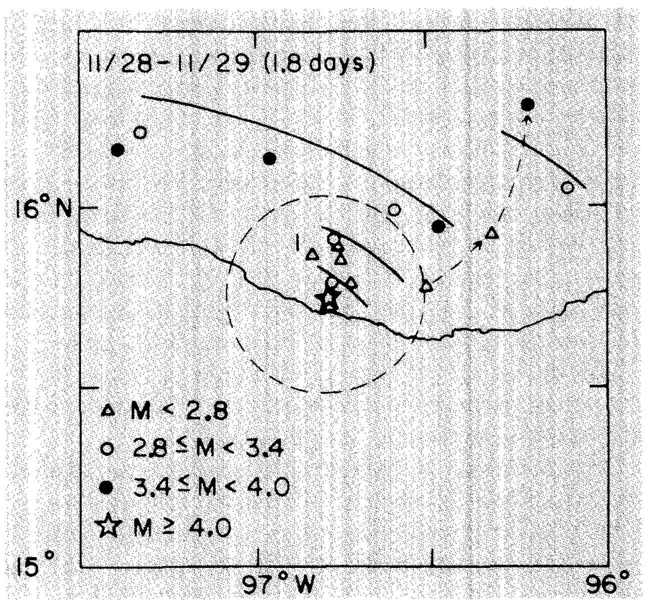


Initial wave forms of six large earthquakes along the Mexican coastal subduction zone show small simple pulses in five cases, including the Oaxaca earthquake, while a 1973 event in northern Mexico produced a complex pulse. This may be correlated with the complexity of sliding and, consequently, the amount of quake-caused damage. [The magnitudes (M_S) given here for the 1965 and 1968 earthquakes differ slightly from those quoted in the text because they are from a different source.]

To concentrate on those last 1.8 days, a small event occurred very near the main shock region, and then activity began out at the edge of the region and migrated up-dip closer and closer to the center until 17 hours before the main shock. For those 17 hours, activity mostly stopped except for three events that moved out and down-dip back down the plate. The migration that moved inward seemed to originate at deeper points along the plate and move upward toward the point of the main shock.

The fault mechanism of an earthquake can be described as something like two pieces of foam rubber sliding past each other. There is a cut down the middle where, because of the friction along the cut, the pieces of rubber first stick and then slip as you slide them along. If you imagine a sphere around the central region of the foam rubber, you can see that as you push one side past the other, the impulse of the side that is not being pushed will be away from the sphere. Our data show us where there was push and where there was pull in this zone and that in the middle of the sphere is the earthquake source. From these data we have deduced that this earthquake (i.e., the main shock) was a thrust event, with the plate thrusting under the continental crust. Analysis of the foreshocks indicates that the mechanism of the activity at some distance from the main shock was different from the main shock itself. There seemed to be a kind of predictive slipping going on in the immediate region of the main shock, however, that emulated the subsequent fault mechanism of the main shock.

Our data on the foreshocks indicate a number of other significant factors. First, the average size of the events before the main shock clearly increased with time. Second, if



The foreshocks of the Oaxaca earthquake illustrate the slipping during the last 1.8 days prior to the main shock (star). After a small event (1) near the center, shocks began on the outer edge (black and white dots) and migrated inward. For the last 17 hours all was quiet except for the three events moving outward (arrows).

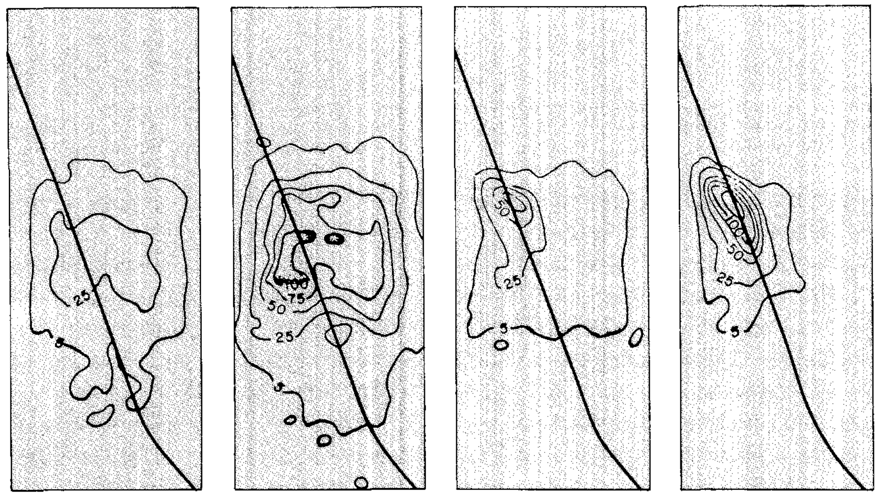
you look at numbers of the events in the 32-hour period before the main shock, you can see that there is a strong increase and then a period of quiet. We were able to see clearly that in this last 32-hour period before the main shock there was a distinct clustering of earthquake activity that was quite different from any previous activity. It is encouraging in our efforts to make reliable earthquake predictions to think that if we had had similar data in advance of other earthquakes, we almost surely would have recognized the significant changes in activity from this kind of display.

This rise in earthquake activity and then a period of quiet before a main shock seems to be very common in foreshock sequences for earthquakes all over the world, but this is not a matter for as much optimism as it would seem we might expect. The fact of the matter is that if we had not had the instrumentation in place in this earthquake, we would not have recorded this foreshock activity elsewhere. All of these short-term foreshocks were smaller than the worldwide detection threshold. Our studies of earthquakes on that basis are mainly of those of magnitude 4.0 and larger. As far as we know, about 44 percent of all earthquakes are preceded by foreshock activity of this kind. What we learned from this earthquake is that we need much more instrumentation for increased detection to find out if foreshock sequences are more common than we have supposed.

Of course, in studying earthquakes and their failure mechanisms, seismologists are under the handicap of not being able to conduct earthquakes under varying conditions in order to find out what makes them happen. It's a circular situation. We have to study what happened before an earthquake in order to predict one, but we have to be able to predict one so we will know where to go to set up our instruments to get data to study. One thing we can do is to set up models to study in the laboratory, and one class of such models — called the dilatancy model — involves studying what happens to rock samples under stress. In this model (which has been developed both in the United States and in the Soviet Union) we measure the deformation of a sample of rock as we squeeze it. At first there are a series of small failures, creating microcracks in the rock sample. At a certain point, the density of microcracks becomes large and causes an increase in the volume of the sample. A coalescence of microcracks then takes place, and the size of fracturing events increases near what becomes the main fault plane, locally reducing the stress. The actual volume increase — or uplift — decreases again, and then failure may follow.

This suggests a number of things for earthquake studies;

Trapping an Earthquake



Microfracturing of a rock sample under pressure in the laboratory does not reproduce the period of quiet observed before the actual main shock of an earthquake. It does, however, illustrate the cluster of fractures (second frame — numbers on contour lines are the number of fractures) near the fault plane, which corresponds to observed activity. In the rock sample this is followed by a decreased frequency of events (third frame) and finally increasing activity just before failure. (Data courtesy of D. Lockner)

that, for example, we might observe uplift in the earth's crust before an earthquake. There have, indeed, been many observations of such uplift, but they are not well understood because their behavior is not consistent. We have all heard of the Palmdale uplift (or bulge) and because of the dilatancy model seismologists have been interested in and concerned about that uplift and what it may mean for a future earthquake on the San Andreas fault.

If we want to carry the analogy further, we can think of the microfractures in the rock sample as representing small earthquakes. An increase in microfracturing could represent an increase in activity in the last days before an earthquake. At present, laboratory models fail to predict the quiescent period of hours immediately preceding the final shock in a real earthquake.

If you look at rock fracturing within a sample in order to try to understand the time history of microfracturing, you can see several stages. First of all, there is a cluster of fractures corresponding to earthquakes near what becomes the fault plane. There are up to hundreds of events, followed by a quiet period when the frequency of events may drop to 50. In the last stage before the sample fails, microfracturing increases to 100 close to the fault plane. The clustering of microfracturing is certainly comparable to what is seen in the field, but what isn't comparable (perhaps because it is not observable on the kind of time scale we have in the laboratory) is the 30-kilometer-wide ring of quiet near the main shock. In the laboratory we can see nothing but increased concentrations of failures near the fault line.

The time scales for earthquake prediction are on the order of days to decades, and it is expected that the time period of this particular anomaly is related to the size of the subsequent event, but we really do not have enough data to be sure of that. If we bring to bear our data from the Oaxaca earthquake, we find that things are more complicated than we suspected. The scientists from the University of Texas showed a quiescent period beginning about 1973, but we later found that it had been quiet in the local area since 1966; the quiet area just became larger in 1973.

In terms of the seismic gap area and its relation to plate tectonics, the data are also somewhat confusing. You can take all of the sizes of large earthquakes along the coast of Mexico and Central America (since the size of an earthquake is related to how much slip there should be) and locate them along the trench at the edge of the Cocos Plate — and then look for a place where there has not been any slip. You have the known average plate rates (Central America — 7 to 9 cm per year; Mexico — 5 to 8) over a long period of time to check your work against — and then you come up with problems.

At some places the amount of slip we have measured in earthquakes actually corresponds with the long-term average plate rates; but at other places — particularly in Central America — it does not correspond at all. Even more disturbing, it turns out that subduction of sea floor topographic anomalies may lead to apparent seismic gaps of a different (or non-predictive) kind. Some of the areas that are quiet seismically actually coincide rather well with areas of prominent sea-floor topography, which means that in looking at seismic gaps we have to decide whether they will ever break. It is my present opinion, based on the study of modern data and of old Mexican journals dating back as far as 1542, that some areas being subducted under the continental shelf may never break in a large earthquake. So we need to find out which gaps are likely to break and which are not and what relation they have to the topographical properties along the trench.

So this is where we stand in earthquake prediction a year after the Oaxaca earthquake. We are still collecting data, still developing hypotheses and trying them out. Someday we will be much more sure of what it all means. When that day comes, we seismologists hope there will be an enlightened public, educated and able to cope with the implications of earthquake prediction. The importance of that is one of the most significant things to have learned from our trapping of the Oaxaca earthquake. □


The distinguished astronomer George Ellery Hale?

A Capital Idea

On a campus as small as Caltech's, you'd think every aspect of every building would be familiar to everyone. But it doesn't seem to work out that way. A quick (and statistically insignificant) poll of some long-time members of the Institute community a while back produced only one gleam of recognition as to the whereabouts of the models for the photographs on these pages; yet you can see them from one of the main campus thoroughfares. Anyone who is interested should stroll down the Olive Walk and take a look at the capitals on the colonnade between Ricketts and Fleming houses.

"Where?" is not the only possible question about them. Some of them look enough like well-known figures of the 1920's for us to wonder "Who?" and also "What can we find out about them?"

The answer to the last question — after extended, though intermittent, investigation — turns out to be "Not much." Not much, at least, in the way of solid identification, but the search yielded a few factual sidelights (for which, read on) and the opportunity to speculate (a game anyone can play).

Our first step was to examine the records in the Institute archives about the building and dedication of the student houses. We looked at several scrapbooks full of newspaper clippings and at the minutes of the meetings of Caltech's Board of Trustees. As far as we can tell from those, back in 1931 when the first four undergraduate student houses were built, both the reporters and the recorders on the scene were either blind to or blasé about the decorative details of the buildings and arcades.

One solid lead was to Robert Lehman (BS '31). Bob was a member of a nine-student committee appointed in November 1930 to make recommendations to the Board of Trustees about the organization of life in the new houses, which were to be ready for occupancy in September 1931. To fulfill this assignment, three of the committee, including Bob, spent three months traveling about the United States and Europe studying various types of student housing. Their report — issued in March 1931 as a *Bulletin of the California Institute of Technology* (Volume XL, No. 131) — was a strong voice in the way the houses were organized, but Bob doesn't


A Nobel Prize-winning physicist, Robert Andrews Millikan?


The pilot of the "Spirit of St. Louis," Charles Lingbergh?

remember that anyone paid any attention to the statuary that ornaments many of the house courtyards and arcades.

It's heartening to note that someone did notice them at the time, however. Small photographs of the figures were reproduced on the pages separating the various sections of the 1932 *Big T*, which was edited by Albert Atwood (BS '32, MS '33), now of Los Angeles.

The designer of the first four undergraduate houses was the distinguished southern California architect Gordon Kaufmann. Among other structures to his credit are the Los Angeles Times Building, All Saints Episcopal Church in Pasadena, the Athenaeum at Caltech, and Santa Anita Race Track. Unfortunately for our project, Mr. Kaufmann died in 1949.

It was Arthur Klein, professor of aeronautics emeritus and Caltech alumnus of the class of 1921, who told us that his classmate John H. "Hi" Hood probably executed the figures. Our letter to Hood, who is retired from the cast stone business and living in Hawaii, produced the following reply: "Gordon Kaufmann, architect, was the designer of the ornamental column and pilaster caps. Mr. Kaufmann had the models made of clay in Los Angeles. I took these models and made glue molds, in which I cast the pieces using a concrete consisting of crushed dolomite and a blending of cements.

"After casting, my men cut the surfaces with air tools to simulate natural stone. To the best of my knowledge Mr. Kaufmann did not try to caricature anyone in particular. If they resemble someone, it is purely coincidental as my men did not have a picture to help them in their finishing."

Pictures they may not have had, but we still wonder what — or whom — Mr. Kaufmann had in mind when he made those clay models. □

Violinist — and physicist — Albert Einstein?



This one is readers' choice.

Research in Progress

Acid Rain

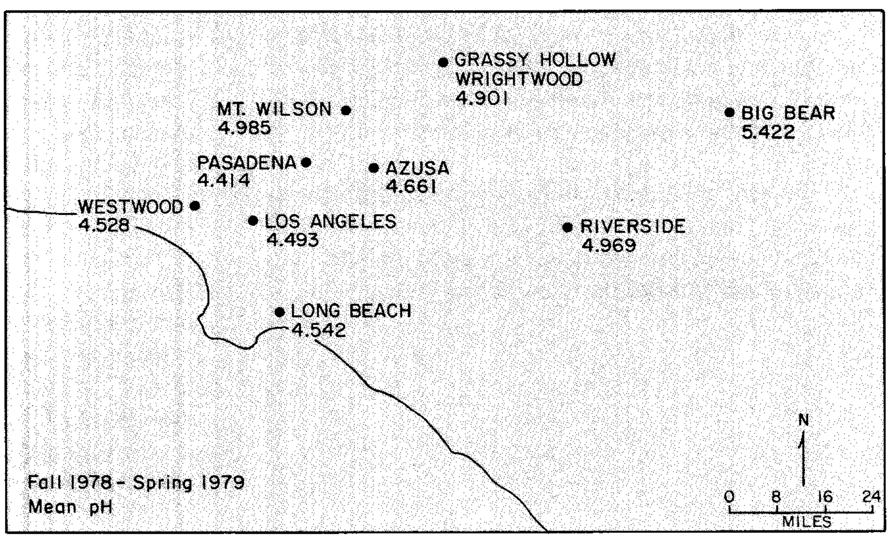
Pure as the driven snow? Despite this old saying, scientists are finding that precipitation over industrial areas is not so pure anymore but carries pollution back to the earth as acid.

This is also true of southern California, say James J. Morgan, professor of environmental engineering science, and Howard M. Liljestrand, a recent doctoral graduate working under Morgan. Under contract from the Air Resources Board, Morgan has undertaken a detailed definition of the chemical composition of rainfall in southern California, more specifically in the Los Angeles area, and its relation to that of the rest of western United States and the East.

Recent studies, with attendant alarming publicity, have shown a marked increase over the past few decades in the acidity of rain and snow in northern Europe and the northeastern United States, both highly industrialized areas. But the West, with its open spaces and less concentrated industry, has remained largely unresearched — until now.

Although there is little background historical data, acidity is probably a newcomer to the West. Using an indirect method to back-calculate probable figures for the pH of various western sites, Liljestrand found that the rain was actually alkaline as recently as the 1950s. Some sites had a pH as high as 7, while most were in the 6 range. (Although 7 is neutral in a closed system, pure water in equilibrium with the atmosphere has a pH of 5.65 due to dissolved carbonic acid; hence 5.65 is used as a reference point, and any atmospheric pH above it is considered alkaline.) This alkalinity is not surprising, since so much of the West's alkaline soil is blown into the air as dust.

Liljestrand's measurements of the Los Angeles area during the fall of 1978 through the spring of 1979 show an entirely different picture, however — a picture that is especially startling in light of the alkalinity of the recent past. Acidity ranged from a pH of 4.54 in Long Beach and 4.41 in Pasadena to 5.42 at Big Bear. There is a clear gradient from coast to mountains caused by the pollutant sources —



Even in alkaline southern California, rain can be acid — as shown by the pH measured at various sites in the Los Angeles area last year. The map also indicates a clear correlation between rain acidity and the sources of pollution.

automobiles and power plants — and the prevailing winds. Southern California is not downwind of any other major polluter, as are some unlucky places in the East, so the acidity in the rain here is all produced locally.

Liljestrand also conducted more detailed measurements and chemical analysis of precipitation in Pasadena over a four-year period, collecting in the entire study approximately 15,000 pieces of data. Measurement of rainwater composition over time showed, among other things, that the rate of rainfall caused extraordinary changes in pH. As rain intensity increases, acidity drops off.

Although a number of substances contribute to the problem, the essential ingredients of acid rain are the nitric and sulfuric acids resulting from the combustion of fossil fuels. Among Liljestrand's interesting findings is the fact that precipitation in the Los Angeles basin — compared with the East and Europe — has a higher ratio of nitrates, which come primarily from auto emissions (although from power plants as well), to sulfates, which are produced mainly by industrial sources.

In spite of the fact that what goes up must come down, Liljestrand found that most of the pollutants do not come back down in rain. Advection, or blowing away, rids the area of most of the nitrogen and sulfur oxides. The second most important mechanism for removal of acidity from the Los Angeles "airshed" is the settling of aerosol (smog) and gases back into the terrestrial ecosystem. Rain in this semi-arid climate actually accounts for comparatively little acid removal from the atmosphere. The study is giving the two scientists a feeling for the relative importance of the different mechanisms.

While raindrops serve to gather harmful gases, they also dilute them, and dry transport of gases and aerosol is potentially more damaging. For example, an aerosol with a pH of 2 can burn a hole in a leaf. Although the long-run effects can only be guessed at, pH is a master variable that controls a lot of things. Acidification may deplete trace metals, and lake populations are particularly sensitive to acid.

At the very least, rainwater has sustained a blow to its image. Far from being a carrier of pollution, it has been considered the purest water available as well as the cleanser of the atmosphere. It cannot be both.

Battery Power

Back in 1968 senior Wally E. Rippel drove the Caltech entry in the cross-country Great Electric Car Race to a half-hour victory over the MIT car (*E&S,* October 1968). Rippel drove his own 1958 Volkswagen bus converted to electric power with a lead-acid battery. MIT's losing entry in the race ran on nickel-cadmium batteries.

Rippel is back, now as a member of the technical staff of JPL, and is more than ever an advocate of electric cars and lead-acid batteries. He and his colleague at JPL, Dean B. Edwards, also a Caltech graduate (MS '73, PhD '77), claim that this traditional battery, with a little help from space-age materials, may prove to be the most efficient and economical source of electric car power over the next decade.

Although the lead-acid battery has been around for more than 100 years and probably has been developed as far as it can go for starting gasoline engines, applying new materials to the old concept offers considerable potential for improvement in application to electric cars.

The conventional lead-acid battery used in gasoline powered cars has lead grids holding electrochemically active lead materials in place. Although the grids themselves do not store energy, they add considerable weight and restrict the battery's power performance.

An alternative approach, the "bipolar construction," does away with the grids and uses thin conductive sheets (biplates) to conduct electricity from one cell to the next. Since this construction has the potential for weight reduction and higher power, numerous attempts have been made to develop such bipolar batteries. However, in the case of bipolar lead-acid batteries, virtually all such efforts have failed because of materials problems with the biplate.

Thanks to graphite fibers, a new lightweight material, Edwards and Rippel may have found a solution to the biplate problem. Their approach, using graphite fibers molded into thin sheets of polyethylene, allows a reduction in dead weight as well as increased plate surface area. This increased surface area leads in turn to improved electrochemical performance of the energy-storing materials.

One of the new battery's key features is its performance. With four times more power per pound, it will far out-perform conventional lead-acid batteries in acceleration, in climbing steep hills, and in maximum speed. According to the two engineers' calculations, their battery will enable an electric car to go from a full stop to 60 miles per hour in 12 seconds, even when the battery is 80 percent discharged.

The bipolar lead-acid battery is expected to last an estimated 50,000 to 80,000 miles, which is substantially longer than other types of batteries currently being developed. Its projected range capability of 150 miles before recharging makes a practical electric vehicle a real possibility within the next few years.

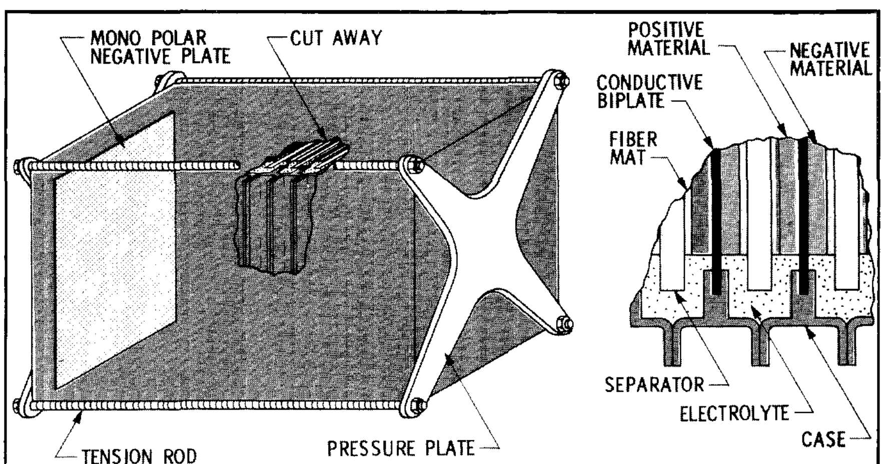
Another advantage of the new bipolar lead-acid battery is that it produces less heat than its conventional counterpart. Overheating was a problem that plagued both the MIT and Caltech cars in the cross-country race 11 years ago, a problem that was then solved by packing the battery areas full of ice. Alternate batteries, such as General Motors' new zinc-nickel oxide battery, are even more sensitive to high temperatures than lead-acid ones and could present difficulties in desert areas.

Another advantage the bipolar lead-acid battery has over nickel-type batteries is cost. Nickel is expensive and must be imported, while lead is cheap and available in adequate domestic supply. Rippel estimates that a bipolar-powered car would have an energy expense of about one cent per mile.

The first phase of basic research on the battery has been completed, and Edwards and Rippel's immediate goal is to demonstrate acceptable performance of the polyethylene-graphite fiber biplate — the key element in their invention. Portions of this work will be carried out jointly with Caltech Associate Professor of Engineering Design David F. Welch and his students. They will be involved with some of the mechanical aspects, such as fabrication of the molds for the experimental parts.

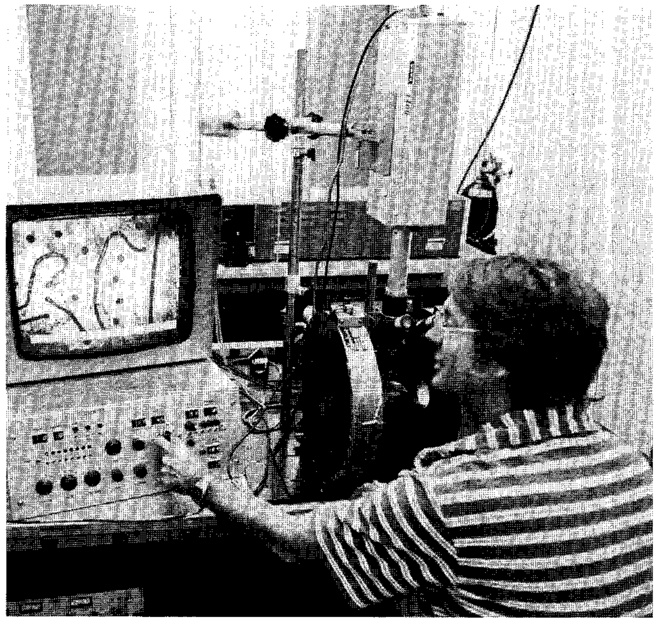
In case you're wondering, Rippel still drives an electric car and has logged nearly 50,000 miles in a Datsun converted to electric power — with a lead-acid battery, of course.

The men behind the development of the bipolar lead-acid battery — Wally Rippel and Dean Edwards, with David Welch. Edwards holds a sample of the graphite fiber material.



Thin, light biplates of polyethylene and graphite fibers connecting the bipolar lead-acid battery cells allow a greater number of cells to be stacked together for more power per pound.

Magnetic Bubbles



Floyd Humphrey adjusts video micrometer to measure one of a group of magnetic bubbles photographed through a microscope by a laser (at the rear to the right of the screen). The snake-like lines on the screen are "stripe domains" — another shape of bubble.

Over in Steele Laboratory they're watching bubbles — not soap or champagne, but magnetic bubbles, about 1-10,000th of an inch in diameter. These tiny magnetized areas are used in computer information storage, where they have the particular advantage of not "losing their memories" if power is turned off.

Development of bubble technology was begun at Bell Laboratories in 1966; bubble memories have been in use for the past five years, but a better understanding of some of their fundamental functions should greatly improve the technology. That's why Floyd B. Humphrey, professor of electrical engineering and applied physics, is watching bubbles. He is particularly interested in the physics of how they move — the dynamics of the bubble walls.

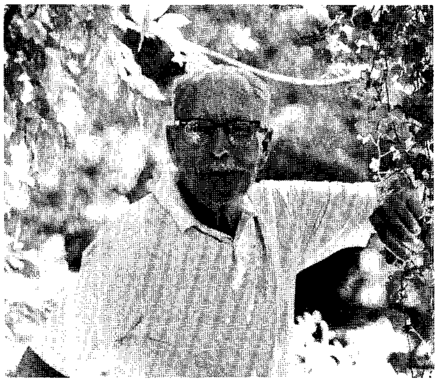
To begin creating bubbles, a thin magnetic film is grown on top of a non-magnetic single crystal. When subjected to a uniform magnetic field, the film will be magnetized in a perpendicular (upward) direction. If a particular spot is then magnetized in a downward direction, a bubble of finite size appears over that spot. Millions of them will fit on a dime-sized crystal. These bubbles are extremely stable and, since the uniform field is supplied by a permanent magnet, independent of power.

How do they move? Since a bubble will move to the spot where the magnetic field is lowest, local changes can be made in the uniform field by a pattern of small metal "magnets" put on the crystal's surface. By rotating a field that is parallel to the plane of the film, the magnetic pattern can be affected so that the point of least magnetic field at the bubble changes continually; the bubbles can be made to progress regularly from place to place through the pattern. Information is stored by the presence or absence of a bubble at a particular spot — bubble or no bubble, one or zero (in binary terms), on or off.

In order to learn what happens to the structure of these fast-moving magnetic areas as they go from one spot to another, Humphrey photographs them through a microscope with a flash 10 nanoseconds (10×10^{-9} seconds) in duration; the usual camera flash lasts about three-thousandths of a second. Humphrey's flash is a laser-driven laser, continuously taking individual pictures, which are translated onto a television screen through a silicon intensified-target videcon. This method produces what appears to be a continuous moving picture of the bubbles and simultaneously records the images as well as data on what was done to produce and manipulate the bubbles. Later these images can be studied, one picture at a time, and the bubbles observed and measured as they move, expand, contract, and deform.

Ultimately Humphrey and his students want to know how the bubbles work in a computer memory. They study bubbles in memory devices, particularly in situations where they do not act as expected. Then the researchers try to recreate the same situation with "free" bubbles (not in a device and therefore more understandable) to figure out what is happening to them and why they act as they do.

In the course of his work Humphrey has added considerably to the understanding of bubble walls and how they move. He has discovered many different kinds of wall structures and invented a theory to account for the bubbles' apparent momentum. And as long as there is more to be learned about this new technology, Humphrey's lab intends to keep on bubbling.



Winchester Jones

—How It Was

L. Winchester Jones came to Caltech in 1925 as an instructor in English, and he quickly gravitated into the freshman admissions committee. By the time he retired in 1968 as dean of admissions, emeritus, he had had considerable influence on the nature of the admissions process and its product at Caltech. He was interviewed for the oral history program of the Institute Archives by Mary Terrall, and E&S *presents here the second of two parts of an edited version of those interviews.*

As a number of readers pointed out to us after reading Part One, Winch Jones is one of the great raconteurs, and Caltech anecdotes are his specialty. Fortunately, Mary Terrall was able to record a few samples from the Jones repertoire, one of which begins on page 22.

Mary Terrall: I think we're up to World War II. Were there very many of the humanities faculty members who were involved in war work in one way or another?

Winchester Jones: Not as many as there were in science. We stayed on the job and did pretty much what we had been doing. I don't recall that any of us except Horace Gilbert really was engaged directly in war work, though we were on various boards and things that were trying to do something for the war, independent of our connection with Caltech. Bill Huse was a kind of historian for the rocket project.

MT: In terms of the teaching, were most of the same courses offered even though the enrollment was down?

WJ: The Navy V-12 program left our humanities, and practically everything else, pretty much to us. They said, "We want these boys educated the way you educate your students, so go ahead."

MT: So what was the situation — a certain percentage of the students were in the V-12?

WJ: Yes. Of course, they had to volunteer; they weren't drafted.

MT: But they didn't have to get admitted by Caltech.

WJ: No. The Navy transferred a certain number of students from other colleges that didn't have a science or engineering place, and oh boy, what a headache that was. The first day of the V-12 I didn't go to bed at all, and there were two or three others who never got to bed that night or the next day. These boys poured in from various places with their transcripts in their hot little hands, and we tried to make head or tail out of them, and decide where in the world we could fit them in. Were they partly sophomores or partly freshmen, or all freshmen? Or had they had any advanced algebra? No. What the hell are you going to do about that? Oh, it was a mess.

To make it worse, we had no commanding officer. Here these people all arrived, and there was a little lieutenant who hadn't been in the Navy more than just his basic training period, and he was scared to death. And no commanding officer. We finally got a man by the name of Mantell who came out here three days late, but he settled things in a hurry.

MT: Did you try to fit the students into the regular Caltech curriculum?

WJ: We tried to, yes. The Navy offered a very few courses — some strictly Navy stuff taught by the commanding officer. For the most part we tried to fit them into just what we had been teaching; that's what the Navy wanted.

MT: How did that work out?

WJ: It wasn't as bad as you might have thought. Some of the students got set back a year or two, but it wasn't all that bad, and the faculty wasn't quite as tough as they had been. It was wartime after all.

MT: What was the feeling among the civilian undergraduates toward the V-12?

WJ: To the best of my recollection, there weren't more than about 75 or 80 of them. And they felt out of it, of course. Except for the freshmen, for the most part they were 4F. So we had very few civilians, and it was kind of tough on them; they didn't get into the activities very much. Of course, the V-12 wanted all the athletic activities it could get — teams and everything. Boy, for three years we had the finest football team Caltech ever had. We had two-thirds of the Stanford football team in the V-12 unit, and the rest of it was made up of Cal and University of Washington football players. We won every game for two years, and one year it was 66-0 over Occidental. Those were great days for athletics.

MT: Were you involved in administering the V-12 thing?

WJ: Yes. They thought the registrar was the logical place to go, so I was the administrator, and I also made some of the feeding and housing contracts that we had to make, not only with the V-12 but also for the Air Force unit we had there, the meteorological people. In fact, more or less unknowingly, I rented Tournament Park to all three services for the same amount each. Somebody found out about it later, and said they were going to sue or something. I believe we were entitled to fifty cents a head and I got fifty cents a head from all three services. As a matter of fact, they were all out there together at the same time.

That contract-making was kind of fun. Jim Page was chairman of the trustees at that time, and when I started making contracts I said to Page, "Look, Jim, I don't know anything about business, or how to make contracts." He said, "Well, we lost our shirts on the last contract we had made by the business office, and you can't do any worse than that." And I said, "Well, all right, Jim; if I'm going to make a contract, I want a case of Scotch and a case of bourbon, and I'm not going to pay for it." And he said, "It'll be there tomorrow." So at the contract meeting we had a captain and a commander, and all sorts of flunkies around, and we sat there in a room and talked, and put things off, and looked up information and so on until about four o'clock, when I said, "It's getting kind of late, let's go have a drink back at my house." Well, finally at about eight o'clock at night, we made a darn good contract.

MT: You were also in the California State Guard at this time. What did that entail?

WJ: That took the place of the National Guard, which had gone into active duty, and so they had to have some organization in case there was a riot or other emergency. We had guard duty and riot training. As a matter of fact, on Pearl Harbor night the company that I was commanding took over Caltech. They were very much worried about the aeronautics lab and one or two of the other buildings where the beginnings of the rocket research were going on, and they wanted those guarded. They thought somebody might blow them up or that some sabotage or damage might be done. It wasn't an easy place to guard on account of those steam tunnels. Every building could be entered from underground through the steam tunnels.

MT: So what did you do?

WJ: We had guards down there, and we changed them every hour and a half or two hours. You stand there in that steam tunnel, in uniform — it's like being in a Turkish bath. I'll never forget, about six o'clock on the morning of December 8 I went over to the Greasy Spoon to have a cup of coffee and get some scrambled eggs — I'd been up all night. They kept the Greasy Spoon open all night for us. And as I was going over, I heard a guard challenge over in front of the aeronautics building. The word was passed, and the corporal of the guard came up, and I looked across to see what was going on, and there was a poor little Japanese graduate student. He'd just got up and was on his way to work; he hadn't even heard about Pearl Harbor, much less the idea that the Japanese had attacked. And this guard had him nailed right against the door. He said to his corporal, "Can I shoot him now, Corporal? Can I shoot him now, or do I have to wait?" He was really eager. I got over there in a hurry. I said to the corporal, "For gosh sake, take that gun away from him, or he *will* pull the trigger before he's through." It was a nervous moment. It was just lucky he didn't pull the trigger too, by mistake.

MT: Was this guarding of the aeronautics building just for a short period?

WJ: Just two or three days. By that time Caltech got regular professional guards.

MT: So after the war when DuBridge was brought in and Millikan retired, were there obvious changes?

WJ: Not a great deal as far as the undergraduate area was concerned. The transition was very easy on account of Earnest Watson. Earnest was really acting president — he didn't have any title, but he really was — for the last year or so between the two. DuBridge fitted in beautifully; he had the kind of mind that saw immediately what went on, and there wasn't any need for any immediate change. As far as the undergraduate work was concerned, I don't think he had any great plans. It was going very satisfactorily, and we had good students, and we were doing all right. I think he thought that we might step up our recruiting a little bit, which we did.

MT: What about more general change in the atmosphere on the campus?

WJ: I wouldn't say that the change in administration had anything to do with that at all. Things changed as they do anywhere over a period of time. One of the attitudes that had changed in that period made teaching much less interesting for me. These fellows who came out of high school were now convinced that they ought to know something about the humanities. Instead of saying, "All right, the heck with you," and then finding out what they really wanted, they sat there almost pathetically: "Here I am, educate me." It was a much more passive attitude.

MT: It was after the war that you noticed this?

WJ: That's right. Except for the veterans. The veterans were a prize, really, but after they got through, there wasn't the same feeling about the humanities. The kids had been persuaded somehow or other that taking humanities was like castor oil; it was good for you. We didn't have to work with them and convince them. For me, they were less interesting students.

MT: How did the admissions work then change after the war? Did you have a lot more applicants?

WJ: Yes, the applications picked up. We had much wider interviews and more of them. During the war, we couldn't interview at all, you see. We really didn't have to with the V-12, but we couldn't get transportation and there was no way to do it. So that was revived. In my day we never did any very heavy recruiting. In a way, the interview trips were recruiting. But I was about the only one who did any recruiting aside from that — in the fall, for example. Peter Miller did some too.

MT: You mean going around to schools and talking to them?

WJ: Yes. I had a little different system from the other colleges. The schools got fed up with the standard recruiters after a while. People were coming in all the time and wanting to see their top ten students, you know. Well, I never went at it that way at all. I wrote them a letter and asked, "Do you want a vocational guidance talk on science and engineering?" "Why, sure, we think that would be a good thing for our boys." So instead of seeing the top ten students, I talked to a whole class. Often I have talked from eight o'clock — the first class in the morning — until two in the afternoon without a break. I talked to every math class and every physics class that had met through the morning and afternoon. I never mentioned Caltech, but I was always introduced, of course, as being from there, and the students would come up afterward and ask me about Caltech; that was fine, but I never brought it up. And the schools would know that, and they figured I wasn't recruiting. Well, I certainly was; that was what I was doing it for. But they felt that those talks were valuable.

MT: Was there any discussion back in the fifties about admitting women?

WJ: Quite a bit. And as you know, the main reason against it was, particularly in graduate work, that you put all this time on the girl, and she went out, and maybe she worked at it a year or two after graduation, got married, had children, and never made any further contribution to science or engineering. That was the theory, anyway. And to some extent it's true, I guess. Caltech was a small place, and we had a limited number of graduates, so we decided it would be better to take those who had a better chance of staying in the field and going on and doing something for the next 20 or 30 years after they graduated. Well, finally it became obvious that we should admit women to graduate school. As you know, that came several years before the admission of women undergraduates. So there was a faculty meeting, and it was pretty obvious by that time that it was going to be approved.

MT: But were there people who were really vehemently against it?

WJ: Quite a number, as a matter of fact. But there wasn't any real opposition at that faculty meeting. It had all been said before. I've forgotten who it was — maybe Ralph Smythe — who got up and made the motion that we admit women to graduate school, provided they gave every promise of being unusually productive. There was a dead silence. And I rose and asked if the gentleman would kindly define his terms. Well, I wish you could have heard the next half hour. Four hundred serious faculty people tried to decide how you define productivity in women. I never had a better time.

MT: Did they then decide on that?

WJ: Oh, yes, it passed; they finally got a motion that satisfied everybody.

MT: Was there a stipulation that the female graduate students had to be especially qualified?

WJ: As I recall, the motion contained some phrase or other that they had to be people who we thought really would go on and make a life career out of it. Of course, the same thing should have been said about the men.

MT: That's right. What about the decision to admit undergraduate women?

WJ: From that time on, I said, "You will admit women to the undergraduate school when I either die or retire, not before."

MT: Why was that?

WJ: I didn't feel that any of us were capable of picking women students. I wasn't prejudiced about it; I just didn't want any more bother. So they said, "All right, we'll wait for this crazy man to get out." However, they took a minor revenge, because the last year I was there, they decided they were going to admit women the following fall. And they made me the chairman of the committee to decide what had to be done in order to admit them — where we were going to house them, where we were going to feed them, what we were going to do about this and that.

MT: Did you get a sense that there was a difference in the overall qualifications of the student body as time went on?

WJ: No, but there was perhaps a little more sophistication. Some of the early students were pretty rugged guys — rugged individualists and everything else. I would say that, on the whole, the later group was — conformist is not the right word; they never were conformists, but they were, I think, a little more housebroken. In the first place, many more of them were theoretical people, even the engineers. In the early days, engineering was a pretty practical matter. I don't mean there wasn't research in engineering; there was. But it was not expected that the engineers would go on in the same proportion and get graduate degrees, or that they would be the kind of engineer that was basically a fundamental research man.

MT: So in that sense the type of student changed.

WJ: In that sense, yes. Our admittees in the 1920s were highly motivated, but not all of them could have made it at Caltech in the 1950s. Maybe half of them or more would have fallen by the wayside. They were bright in their own way, but they were not people who could have taken the modern math and physics that are being thrown at students now.

MT: I read something that you wrote in *Engineering and Science* back in 1949 about how Caltech has one of the lowest academic failure rates in the country. And then I happened to be looking at the *Bulletin* from the early seventies and it had some figures about how 10 percent of the freshmen don't come back as sophomores, and 30 percent don't graduate. Now it's obvious that many people just can't do it.

WJ: Can't do it, or don't want to after they find out what it is really like. You see, my figures were failure rate; but the

Have you heard Winch Jones tell this one?

One of the famous people on the faculty was Fritz Zwicky. He was a wild Swiss. And a very controversial figure, a very definite figure. And very amusing. Well, I came into the faculty club one day, and I sat down at the table where I usually sat, a big round table. There were more foreigners, it seemed to me, in those days on the faculty than there were later. This must have been before the 1941 War. It was a rather dull lunch, I thought, so I threw a remark out to see what would happen. I said, "You know, all foreigners are rotten automobile drivers." Three or four mouths opened around the table, and Zwicky got his open first; he usually did. He turned on me and he said, "Jones, that is the kind of idiotic remark you have been making around here now for twenty years. Justify such a stupid statement."

I said, "Well, how about (Josef) Mattauch; he killed himself up here on the Ridge Route, coming around those curves picking wild flowers off the side as he drove along."

"Mattauch, Mattauch, he was a congenital idiot before his grandmother was born. Leave him out."

I mentioned somebody else, and he was a congenital idiot even further back. "All right," I said, "What about Epstein?"

"Oh, my God! Must you bring Eppy into the argument?"

"Yes."

"Then I am lost. Only one thing is making me believe in divine providence, and that is the conjunction of Eppy and the automobile lasting for more than 40 seconds. This cannot possibly happen by chance, only by divine interference. Am I ever telling you about the time Eppy and Mattauch — before he killed himself, before you ask a stupid question — is driving back from Azusa in the old days when the road is winding?"

And I said, "No, you didn't tell me."

So Fritz brought his fist down on the table and broke a couple of coffee cups and said, "Shut up everybody, I am talking. In the old days, when the road is winding, we are driving back in the dark, in this ancient Buick Eppy has. This Buick comes from the tomb of Tutankhamen. In about the Middle Ages is the top disintegrating so there is no more top. Comes a big wind blows across, blows the glasses off Eppy, smashing on the road. Well, you know Eppy sees about 80 feet with the glasses on, and not a damn thing with the glasses off.

So I am saying, 'Eppy, better let me drive.'

Very proud fellow, Eppy; he says, 'No it is not necessary.'

I say, 'Eppy, you can see nothing.'

Eppy says, 'That is a vast exaggeration, I can see the tail-light on the car ahead. And when this tail-light has an apparent luminosity of a star of the fourth mag-

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Winchester Jones

. . . continued

dropout, the man who shifted and decided he wanted to go to Stanford to study economics, doesn't have to go to Stanford to study economics now. We lost him, where we wouldn't now. But I wouldn't count him as a failure.

MT: Were there many people who did transfer away from Caltech?

WJ: Well, there weren't many. I've forgotten my figures now on how many actually graduated. Not nearly as many as we wanted to have graduate. We would admit 180 in those days and, as I recall, our senior graduation used to run about 125 or 130. We figured that we wasted our time on an awful lot of people. Now, not all those are failures. Some of them had just transferred to other areas. And we were concerned about it. So then there was a good deal of agitation to enlarge our transfer admission, from the junior colleges particularly, to fill up these ranks. But that was not so easy. We were beginning to outpace the junior colleges in the demands that would be made of their students as juniors. However, most of the students that we did take in did very well.

What we needed was a recruiting program in the junior colleges, and we just didn't have it. I said I was not fitted to recruit in the junior colleges because by that level they had gone way beyond any math or physics discussion that I could have with them. I couldn't hold my own there at all. So it had to be done by faculty members, and they had already given enough time on their freshman interviews. The Upperclass Admissions Committee never had the same enthusiasm for that kind of thing. Now, I understand, they even have a high school relations fellow, Lee Browne, who does a lot of recruiting. This is what they should have had a while ago.

MT: To go back to the fifties, what was the effect on Caltech of the McCarthy hearings?

WJ: It was just a horror to people, you can imagine. No scientist would have any sympathy with a thing like that.

MT: Was it discussed or was it ignored?

WJ: It was not much discussed, but if the subject ever came up, why — everybody hated McCarthy of course. It was considered a very serious matter. And it did come home to us every so often, when we would find that someone was denied access to certain kinds of information on some hearsay business, that an agency that was giving some research money — scared of the McCarthy attitude — was trembling on the verge of withdrawing it. And it created, of course, a tremendous amount of indignation. Government agencies were terrified.

I think the worst case we had was that of Hsui-Shen Tsien, the excellent aeronautical and jet propulsion engineer. Tsien was a very smart man. The whole McCarthy business was stirred up against Tsien, who was a Chinese — and this was after the Communist takeover, of course.

Fritz Zwicky

Paul Epstein

nitude, I am about 250 feet behind, too far away. When it has an apparent luminosity of a star of the second magnitude I am about 30 feet behind, too close. A star of the third magnitude I must be keeping this tail-light. Shut up, I'll do the calculations.'

Jones, do you know how to calculate luminosities? Differential equations, covering two blackboards. Eppy is doing it all in the head. Marvelous mathematician. Put the foot on the brake, on the accelerator, on the brake, he is keeping just about the right distance from behind. All of a sudden, what do you think is happening? The apparent luminosity of that tail-light disappears to a factor zero.

I am saying, 'Eppy, Eppy, what are you going to do?'

Eppy say, 'Sh-h-h, the car is going around the corner.' Eppy is counting, one-two-three-four, and then pulls the steering wheel. And we bump over a little low stone wall into an orange grove.

So we all get out and Eppy says, 'Gentlemen.' Eppy bows from the waist — but that is not strictly true, Eppy's got no waist, but he bows — and he says, 'Gentlemen, gentlemen, not since I was in the gymnasium have I made such a silly mathematical miscalculation.'

And I am saying, 'Eppy, never mind the miscalculation. How do we get out of the orange grove?'

Eppy says, 'Elementary, gentlemen, elementary. Any child knows that the square of the hypotenuse is equal to the sum of the squares of the two sides. The road is turning 90°. When I am pulling the steering wheel, I am about exactly 18 feet too far. On the square of this hypotenuse, I must now go exactly 29 feet and come out on a 35° angle on the highway and there I will be.'

And I am saying, 'Eppy, that is all very well, but there are orange trees in the way that you're going to hit.'

Eppy says, 'Oh, that complicates the situation. How many orange trees?'

I am saying, 'Four.'

'How big is an orange tree?'

I am saying, 'Well, twelve foot in diameter, six-foot radius.'

'Hmmm, off the hypotenuse I must take one-two-three-four, six-foot radius off. Elementary, gentlemen, elementary. All calculated. Get in, get in.'

So we get in. Eppy starts the engine, and we go. Around the first orange tree, the second, the third, fourth. Not touching a leaf, all by mathematics, Eppy sees nothing. And we come on the highway precisely at the right angle. Marvelous mathematician, Eppy. Only one thing he does not take into consideration in the calculation. At the same time and the same place where we come on the highway is also another car coming. And what a hell of a mess *that* was.''

He was no more Communist than I am and he didn't want to go back to China, but they deported the man. They actually kicked him into the van, by the way, with a foot, and sent him off. You can imagine what he felt like when he got back to China. Of course, he went to work for them. We lost one of our leading engineers as a result of that, and it was just stupid and outrageous.

MT: People on campus were generally outraged about that?

WJ: They certainly were — everybody, whether they knew Tsien or not. Tsien was not the most agreeable character I ever ran across, but there was certainly no reason to suspect him of being a Communist just because he was a Chinese.

MT: I guess not too many people at Caltech really felt threatened by the scare.

WJ: No, I don't think so, not directly, but every so often something would crop up about some friend of theirs who was in trouble, and there was a lot of indignation about it.

MT: What about the changes that took place in the humanities division over the years? By the sixties, it was really quite a different place.

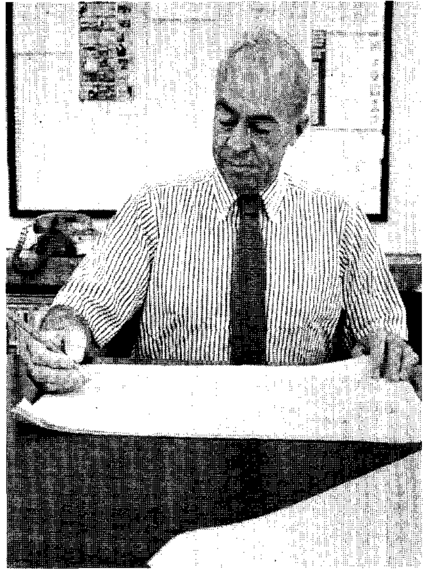
WJ: Yes, very different. It changed from a service division to more of a research and scholarly division. As I say, Rod Paul was one of the first who ever claimed to be a research man among all of us. But more and more under Hallett Smith, and later, we began to get real scholars, and we also got built up in numbers. When I stopped teaching, there certainly weren't more than a dozen of us in the humanities division. Now, there are 50 or 60 people. Something like that. That was the change. It became a major division instead of a service division.

MT: You were saying that everybody knew everybody, and it was very common to have friendships with people in different divisions. Has that changed?

WJ: I would think so. The new people came in so fast that I lost track of a lot of them. I knew all of the faculty at one time; I knew them fairly well. By the time I left, I don't suppose I knew half of them. I might know their faces and vaguely who they were, but I didn't really know them. How many does the Athenaeum hold at lunch time? In the old days, it held all the faculty that wanted to eat lunch. Now even the private dining rooms to the west are always filled at lunch, and the faculty has spread out to Chandler. We used to have about three big round faculty tables that we sat around — at one one day and one another, and you were friends with everyone. More and more, there came to be a table that you usually sat at — and at lunch was where you really had your social contact, of course. There was the physics table and a chemistry table and a geology table, and there wasn't nearly as much mixing. Then it just got too big. This happened about ten years before I retired. By the mid-fifties, it was getting beyond me, at least, to keep track of them. □

Editing from Scientist to Informed Layman

Talent in fundamental science, I have found, does not necessarily extend to communication, so . . .



. . . I have done everything I could in editing *E&S* articles to keep them as simple as possible . . .

by Edward Hutchings Jr.

Except for a few Depression jobs (bank teller, bookkeeper, and door-to-door distributor of samples of All-Bran) I have been in the magazine business since I graduated from Dartmouth in 1933. I've done a lot of free-lance article writing, edited and collaborated on several books (including, as I now recall, one entitled How to Live Without a Woman*), and written some short stories for magazines like* Colliers *and* The New Yorker. *But my career proper started at the old* Literary Digest *– where I developed great respect for good proofreading by running a department called "Slips That Pass in the Type." For a couple of years I reported on the advertising business and wrote a humor column for a magazine called* Tide, *then worked as news editor of* Business Week, *associate editor of* Look, *and executive editor of* Liberty. *I was managing editor of an experimental McGraw-Hill magazine called* Science Illustrated *when Caltech asked me to come out and run a magazine for them. In 1948, then, I decided to try California for a couple of years – and here I am, 31 years later, just retiring as editor of* Engineering and Science *– but staying on as lecturer in journalism. I must like it here.*

For more than 30 years I was editor of *Engineering and Science,* a magazine published by the California Institute of Technology and its Alumni Association. It goes to alumni, faculty, students, parents, and trustees, to individuals who contribute funds, and to industrial concerns that support research at Caltech, to high school science teachers and principals, libraries, newspapers and news magazines, and, finally, to a group of general subscribers.

This miscellaneous group of people (now numbering a little over 12,000) has one thing in common — an interest in the California Institute of Technology, and therefore an interest in, and some curiosity about, the fields of science and engineering. Naturally, these fields are the main concern of the Caltech magazine, which in years past was primarily devoted to reports of the research in progress at Caltech. Inevitably, in recent years, we have reported less on science for science's sake and more on science for society's sake. This was not simply a change in our editorial concerns; it was a reflection of the changing concerns of the people who make up this institution. But whether about basic science or about science and society, our articles have been written, whenever possible, by the men and women who are faculty members or alumni of the Institute.

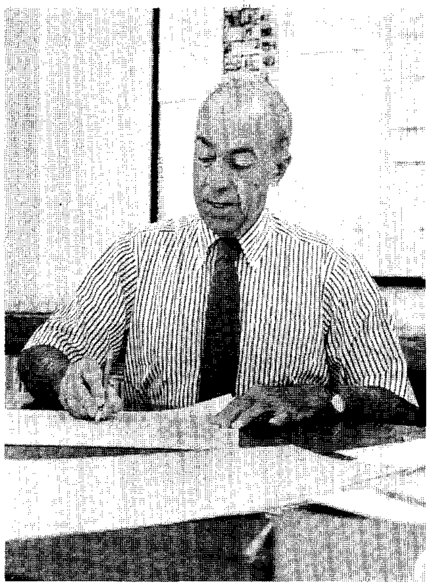
With an established editorial purpose for the magazine, we have found that we have almost limitless possibilities as to subject matter, not to mention a list of potential writers that, in another field, would be called a star-studded cast. When *Fortune* magazine did an article on Caltech several years ago, it reported that the Institute "harbors what is America's richest concentration of talents in fundamental science." These are the potential writers for a Caltech magazine. I said *potential.* The talents in fundamental science do not necessarily extend to communication.

As a matter of fact, when I first went to work on the Caltech magazine in 1948, I was prepared for (1) a mass resistance to communication, and (2) an inability to communicate in the cases of the few who might be willing. That was shortly after World War II. A lot of boundaries between science and non-scientific affairs disappeared during the war, and ever since then it has become increasingly clear that scientists would have to not only communicate intelligibly with each other, but with non-scientists as well.

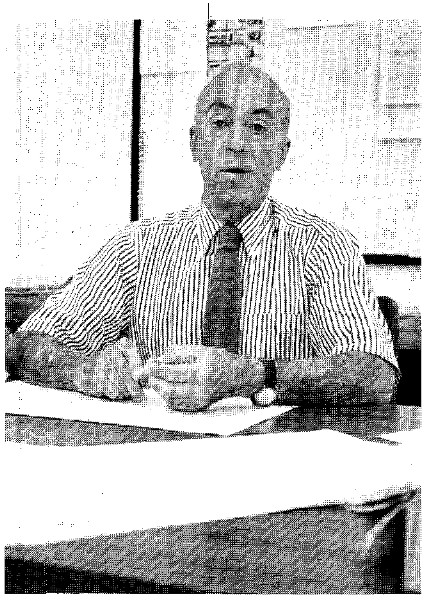
As to the ability of scientists to communicate the details of their work — I think I have seen better, and clearer, and more professional (and more interesting) writing by some of Caltech's scientists than I have in most of the articles written by science popularizers in the general magazines.

I don't know why this should be surprising. Most of the Caltech faculty are teachers, after all. And teachers are in the business of communicating. Good teaching demands many of the same talents as good writing — including skillful presentation, an awareness of the nature of the audience, clarity, color, even a little ham.

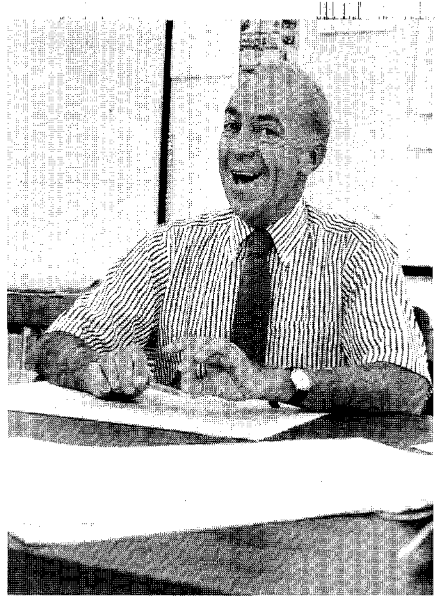
In producing a magazine at Caltech, then, I have had a collection of not-unwilling and not-untalented writers at my disposal. How have I gotten them to write for *E&S?* Well, what it ultimately has



. . . Inevitably, of course, every article has been something of a compromise between editor, author, and time . . .



. . . In fact, editing to bring scientist and layman together has been like operating a lonely hearts club . . .



. . . Those two nice kids didn't know each other, and so I have kept trying to promote a match.

come down to has been whether they had a desire or willingness to communicate. Of course, there were always plenty of people who resisted doing this. I think, though, that once a faculty member wrote an article for the magazine the results often made him a regular contributor.

E&S goes to such an assortment of people that a single article might bring a variety of responses — a letter from a trustee, an alumnus, or a high school science teacher, for example. The very fact that something a faculty member had written about his work reached, interested, penetrated, and even affected so many kinds of people was, I guess, the main incentive to writing for this kind of magazine.

An added attraction, and a most important one, was the fact that, at the same time, a generally understandable research article let a man's colleagues, and people in other disciplines, know something of the nature and progress of his work. As our world — even the confined world of Caltech — gets larger and more complex, this kind of communication becomes more and more valuable.

We have tracked down articles for *E&S* in various ways. Sometimes we persuaded a scientist or engineer to revise a technical paper already presented at a meeting of a professional society — scaling it down to more general understanding. Sometimes we were able to get a professor to write an article based on a talk he gave at a departmental seminar. Sometimes we interviewed a faculty member and then worked with him to come up with a satisfactory article.

Probably the most complicated process involved using a tape recorder for the holdout who, under no circumstances, would agree to take the time to write anything for us. Dick Feynman was the prime example. Dick never wrote anything down, but whenever he gave a talk, we'd tape it, then show him the transcript. This would be such a shock to him that, to regain his self-respect, he would work with us to put his words into printable form. And that's how *E&S* came to print the collected speeches of Richard Feynman — and although this particular editorial relationship started out with a certain amount of kicking and screaming, Feynman now refers to me as "my publisher."

I haven't mentioned our steadiest contributor of all — Lee DuBridge. We ran most of his speeches — and he made a lot of them. All good. I don't know how many times a DuBridge speech bailed out the next issue of *E&S*.

The level of understanding at which an article in *E&S* was written has fluctuated from issue to issue — even from article to article. This was inevitable, because every article has been to some extent a compromise. While I have done all I could to direct and edit the article so as to keep it as simple as possible, the author has often gone to great lengths to keep the level of understanding as high as possible. What was finally published represented the point at which any particular compromise reached its farthest limits on each side. Or sometimes, what was finally published merely represented the point at which time ran out on us.

In any case, the level of understanding we tried to maintain in *E&S* was one that could be comfortably followed by that rare creature known as the informed layman. Of course, most of the magazine's readers are Caltech alumni, and it has often been argued that whatever else they are, they are *not* laymen. But we discovered that even they are usually laymen in all fields outside their own.

In brief, in editing material for *E&S* I tried to follow the old dictum of never underestimating the intelligence of our readers while never overestimating their knowledge. Keeping in mind that our articles would be read by high school students as well as by the head of the American Association for the Advancement of Science, I was willing to take the risk of explaining too much rather than too little.

E&S has not tried to do the same job as a strictly technical publication. There, the purpose is solely to present factual information. What we were trying to do was to help fill the need that was recognized by the AAAS back in 1951, when it said:

"It is absolutely essential that science — the results of science, the nature and importance of basic research, the methods of science, the spirit of science — be better understood by government officials, by businessmen, and indeed by all the people."

Of course, I haven't often thought of my job in such lofty terms. As editor of a magazine that tried to bring scientist and layman together, I felt more like the operator of a lonely-hearts club. I happened to know these two nice kids. They were willing but shy, and they didn't know each other too well. I've tried to promote a match. □

Jack E. McKee
1914-1979

Jack E. McKee, professor of environmental engineering, died on October 22 after a long illness. He was 64. McKee had been a member of the Caltech faculty since 1949, coming to the Institute after three years as a partner in the consulting engineering firm of Camp, Dresser, and McKee of Boston, Massachusetts. For ten years he was again an active member of that firm, beginning in 1965.

A native of Pittsburgh, Pennsylvania, McKee took his BS in civil engineering at Carnegie Institute of Technology in 1936. He received an SM in 1939 and an ScD in 1941 in sanitary engineering, both from Harvard University. In 1973 he was awarded the honorary degree of DEng from Rose-Hulman Institute of Technology.

During World War II, McKee served with the U.S. Army Corps of Engineers, rising to the rank of lieutenant colonel. His principal assignment was as sanitary engineer for civil affairs and military government in the First U.S. Army headquarters during the invasion of Normandy.

McKee was widely honored for his research on water quality and waste treatment. Among those research efforts were work on water-quality criteria, disinfection of settled sewage, membrane-filter analysis, radioactive substances in sewage and sludge, marine waste disposal, and reclamation of waste waters. He was the author of 89 technical papers, including the compendium *Water Quality Criteria,* the first book of its kind to summarize and evaluate what was known of the toxicity of various elements and compounds occurring in water supplies. It has become a classic in its field, and has even been translated into Japanese.

His professional interests were also expressed in his energetic participation in technical societies. Between 1957 and 1963 he served as director-at-large, vice president, and president of the Water Pollution Control Federation. He was president of the Los Angeles Section of the American Society of Civil Engineers in 1960 and a national director of that organization from 1965 to 1968. He was a member of the National Academy of Engineering, a diplomate of the American Academy of Environmental Engineers, and served as chairman and trustee of the Environmental Engineering Intersociety Board. In 1965 he was appointed to a three-year term on the prestigious Advisory Committee on Reactor Safeguards of the Atomic Energy Commission.

McKee's energy and optimism stood him in good stead when he came to Caltech and found himself the only faculty member in his field. He developed a teaching and research laboratory in one of the "temporary" World War I bungalows, demolished in the late 1950s to make way for Page House. Long before anxiety about the environment became popular, he pushed ahead with plans for the expansion of the environmental health engineering program — with new labs and additional faculty to be housed in the Keck Laboratory, which was built in 1959. And he had a warm and continuing interest in and concern not only for his own students but for engineering education in general.

One of Jack McKee's non-professional interests was the Caltech Dixieland Jazz Band. This group of near-professional quality musicians was organized by McKee several years ago, drawing its members from the ranks of Caltech faculty and students. His instrument was the banjo, and he played it with both skill and enthusiasm. A memorial service was held for McKee on November 20, and at his request music of his choice was provided by the Dixieland group.

McKee is survived by his wife, Dorothy, and three children, Douglas, Edward, and Katherine McKee. A memorial fund has been established at Caltech in his honor. □

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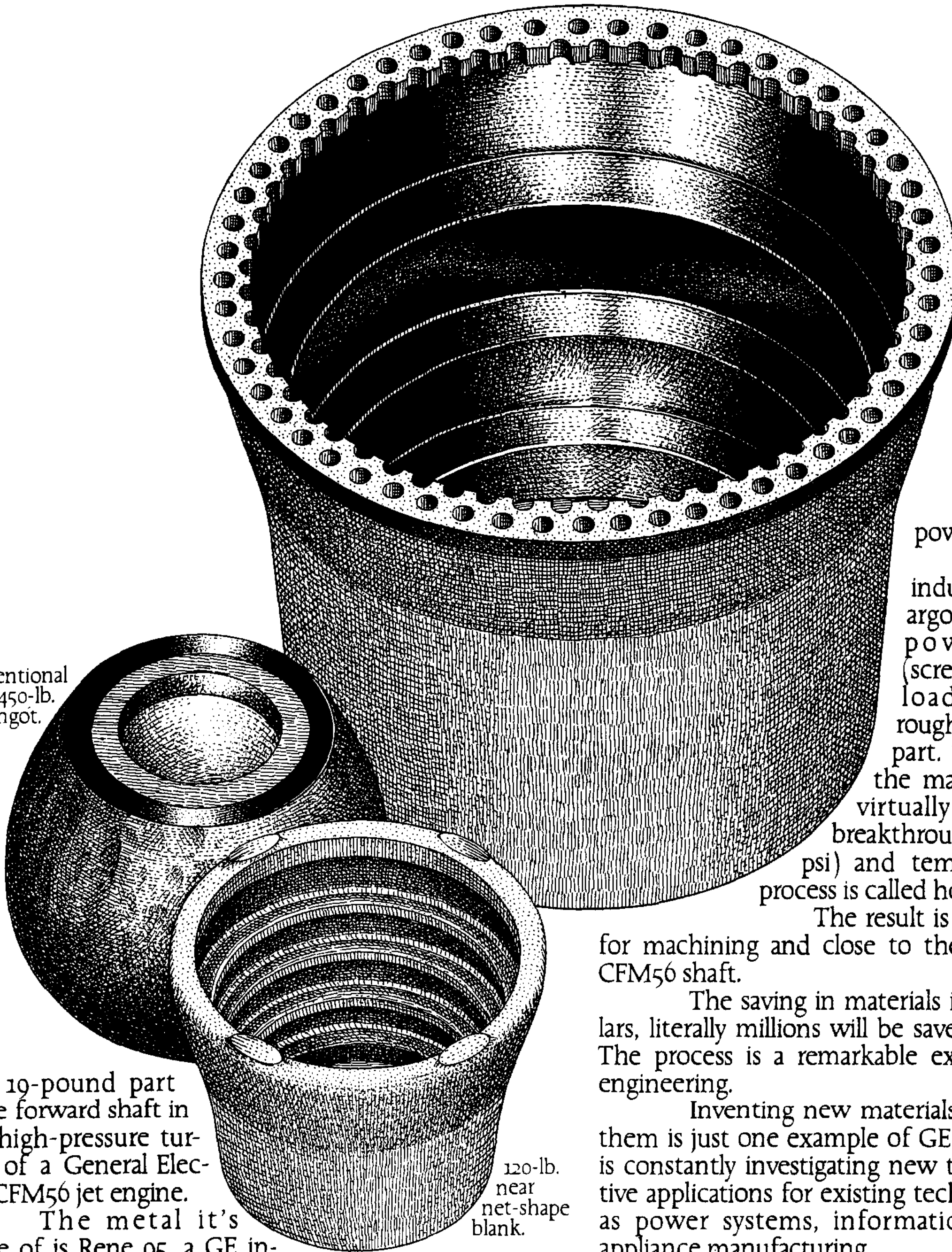
Why should it take 450 pounds of metal to make a 19-pound part?



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The saving in materials is more than 70%. In dollars, literally millions will be saved over the next decade. The process is a remarkable example of cost-effective engineering.

Inventing new materials and better ways to use them is just one example of GE research in progress. GE is constantly investigating new technologies and innovative applications for existing technologies—in such areas as power systems, information services and major appliance manufacturing.

This takes talent—engineering talent—not just in research and development, but in design and manufacturing, application and sales.

If you'd like to know more about engineering opportunities at GE, send for our careers booklet. Write to: General Electric College Communications, W1D2, Fairfield, CT 06431.

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