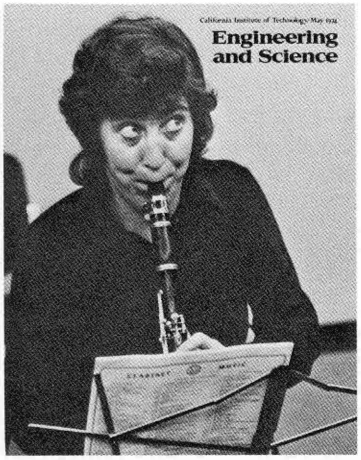
California Institute of Technology/May 1974

Engineering and Science

In This Issue



Music in the Air
On the cover—with some trepidation, Cynthia Friedman, graduate student in biology, tries for a high note in Caltech's new chamber music class, conducted by the famous concert artists, Alice and Eleonore Schoenfeld. Cindy and the Schoenfeld class are just a small part of the music explosion that has hit the campus this spring. A pictorial record starts on page 9.

Choosing One's Ancestors
"Choosing One's Ancestors" (page 2) was first given—in radically different form—as a Watson Lecture in Beckman Auditorium on November 12, 1973. Lord Zuckerman was introduced to the audience at that time by his old friend Harold Brown, president of Caltech, who said, in part:

"This evening we will be hearing from an eminent anatomist and zoologist, Solly Zuckerman, on a subject which 100 years ago, give or take 10 years, was being debated between theologians and scientists. In fact, it must have been just about 100 years ago that the famous debate between T. H. Huxley and Bishop Wilberforce took place.

"The creationist vs. evolutionist argument is not quite over, I suppose—even, or especially, in California; but the more interesting conflict now is among scientists who seek a clearer picture of the line of human evolution during the last 5 million years. To this problem this evening's speaker brings a broad and productive background of knowledge and of experience, and a record of achievement in academic life as a medical educator and zoological researcher, and in government and other public service.

"Born in South Africa, Lord Zuckerman received his university and medical education there and in London, where he was a demonstrator in anatomy from 1928 to 1932. He came to Yale University in the United States as a Rockefeller Research Fellow in 1933 and stayed as a demonstrator and lecturer in human anatomy until World War II. During the war he worked in operations analysis with the United Kingdom War Cabinet and with the U.S. Air Force. Following the war he returned to academic life as professor of anatomy at Birmingham University while continuing to advise the British Government on research, on science policy, and on military policy.

"In 1958 he became Chief Science Advisor to the UK Minister of Defense. It was then that I met the then Sir Solly Zuckerman. We have since enjoyed many years of collaboration on nonanatomical subjects, and close friendship.

"From the early 1960's he was Chief Science Advisor to the government and to the cabinet in the UK—the equivalent of the President's Science Advisor in the U.S. And, while doing both that and his Ministry of Defense job, he also ran the London Zoo.

"In 1970 he formally retired as Chief Science Advisor and was elevated by the Queen to a Life Peerage. Now Professor at Large at the University of East Anglia, he continues his active interest in scientific subjects and continues also to advise the British Government on science policy matters.

"In his research on primates, in his military operations analysis, in his work as science advisor to the government in civil and military affairs, Lord Zuckerman has had, in effect, several outstanding careers. And in each of these he has displayed both achievement and skepticism.

"In military operations, his association with the military gave him a healthy skepticism about the capabilities of military weapons and a feeling for the limitations of the knowledge on which military plans purport to be based. This he has described in many public statements and papers.

"In his advice to the government he has gained a healthy skepticism about the value and accuracy of scientific value, and on the ability and will of governments to use that advice when they have it—even when it's good advice. The full story of that probably awaits publication of his memoirs.

"We shall see this evening that this healthy, skeptical approach to the evidence he also applies to the human species and its descent."

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Engineering and Science

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Choosing One's Ancestors

by SOLLY ZUCKERMAN

My theme has two beginnings. One relates to the remote ancestry of the Mammalian Order of which man is a member. The other concerns my own ancestral relationship to the subject about which I write.

First, a very brief outline of the Order of Primates. Like all other Mammalian Orders, the one to which we belong emerged some 70 million years ago at the start of the geological epoch known as the Eocene. It soon differentiated into two sub-orders, of which the first and more ancestral was the Prosimii. This is represented today by the lemuriform lemurs of Madagascar, the lorisiform lemurs of Africa and parts of the Orient, together with the spectral tarsier of the East Indies and the Philippines and perhaps the tree shrews. The second sub-order was the Anthropoidea, which today consists of the gibbons and the three great apes, as well as the monkeys of the Old and New Worlds. Man is also one of its members. However isolated the geographical area or areas in which our species *Homo sapiens* may have evolved, we are the only primate which today lives in all habitable parts of our planet—and sometimes even in parts which are not habitable.

The particular division of the Anthropoidea to which we, and the apes, belong, and in which all related fossil forms are classified, is called the Hominoidea. A fragmentary series of fossils allows us to track the history of this whole group back some 30 million years into the Oligocene, and that of indisputably human forms back about half a million years.

In theory animals are classified by evaluating resemblances and differences in the largest possible number of their structural and functional characters, and not by reference to some preconceived view of their evolutionary relationships. There is, however, a vast difference between theory and practice; for while the number of characters which could be evaluated in living animals is theoretically almost unlimited, in practice the number that is considered is relatively few. Taxonomists and systematists proceed on the assumption that characters are correlated, and that relatively few are needed to establish the "character complex" of a species.

The gulf between theory and practice does, however, lead to a confusion of the two needs of describing and diagnosing a species. Describing still implies the consideration of as many characters as possible. Diagnosis can often be adequately based on a very small number, whose value as diagnostic features would be simply that they are adequate for the purpose of diagnosis. But this does not endow the features concerned with any particular merit in the determination of evolutionary relations.

So much for one of my beginnings. The other relates to my own professional interest in the subject of human evolution. This goes back almost 50 years to a time when the human fossil record was mainly limited to a few specimens of Neanderthal man, those beetle-browed, allegedly stooping cavemen who lived in various parts of Europe in its last glacial phases—and who have since been found in other parts of the world. There were also the ape-man *Pithecanthropus* of Java, and a fossil type known as Piltdown man, after the name of the Sussex village near which it had been unearthed. At the time about which I am speaking, fossil apes were essentially limited to a few remnants, clearly ape-like, most of which had been assigned to the genus *Dryopithecus*—the ape of the forests.

But living as I was in South Africa, there were also some black races which were regarded as primitive, and of which at least one, the Bushman, was referred to as a living fossil. Bushmen still exist, but in much fewer numbers than when I was a boy. And then, just before I left South Africa, the picture was abruptly transformed by the recovery of the skull of a young ape-like creature in the Taungs limestone quarry in what is now Botswana. This fossil was immediately proclaimed to be the so-called missing link between apes and man for which people had been searching. It was christened *Australopithecus africanus*, an unauthorized confusion of Latin and Greek.

All this was more than enough to stimulate the imagination, and I must confess that but for an accident I might have become a professional speculator in the wares of physical anthropology, as opposed to having become, over the years, a professional skeptic and critic or, let me say, an anthropological agnostic.

I had already embarked on some research into the comparative anatomy of apes and monkeys and I must have been one of the first of those who had ever studied the skulls of monkeys and apes who was allowed to handle the original Australopithecine skull. That was toward the end of 1925, when the newly appointed professor of anatomy in South Africa's second medical school, Raymond Dart, visited Cape Town and took the opportunity of discussing the Taungs skull with his opposite number, Professor M. R. Drennan, who was the first professional anatomist to work in the country. I can still recall my astonishment when, as I was sitting at my bench chipping bits of limestone from a fossil baboon skull. I

An anthropological agnostic reflects on the subjective and arbitrary nature of the recognition of an ancestor

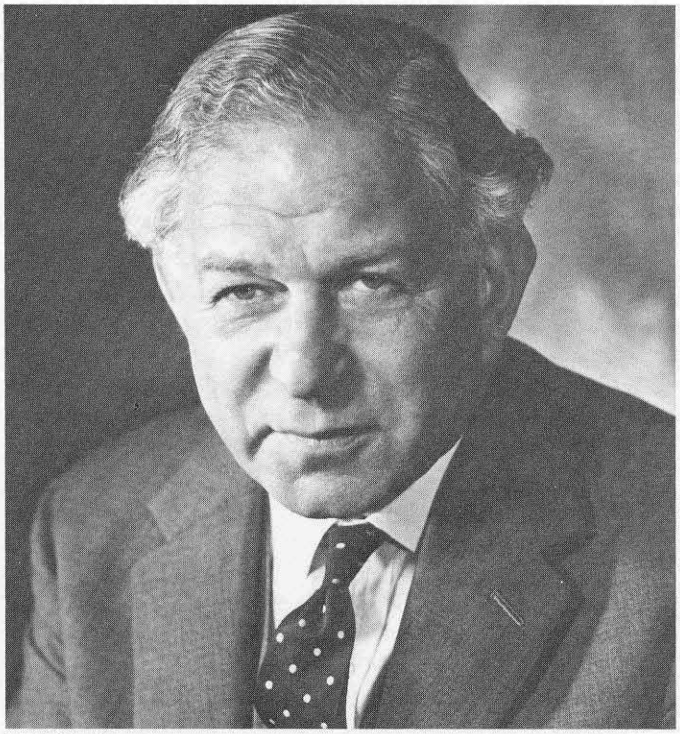
was shown the skull of a young gorilla—at the time one of of at most two or three skulls of young apes available for study in Cape Town—and asked whether it was the skull of a chimpanzee or gorilla. I found it remarkable that while Professor Dart had no difficulty in diagnosing that the ape-like young *Australopithecus* was a unique creature totally different from the family of existing apes, and that it was in the direct line of human descent, he could be uncertain about what to call the skull of one of the extant apes.

That was my first real introduction to the presumed techniques—or methods of revelation—used in the study of fossil primates. It was something of a shock.

Soon after I had moved to London as a medical student, I came under the influence of both Grafton Elliot Smith and Arthur Keith, then the two leading British authorities on the subject of the ancestry of man. Neither was much impressed by the extravagant claims which Dart was making about his missing link. Thus in 1931, Keith, after a careful analysis, dismissed Dart's claims; but in 1949 he qualified this judgment and wrote, in deference to the views of his old friend Dr. Robert Broom, who had entered the fray on Dart's side, that whereas he, Keith, differed from Broom about the matter of the evolutionary separation of the Australopithecines from the living apes, he was ready to concede that "of all the fossil forms known to us, the Australopithecines are the nearest kin to man." Broom's view, however, was that the common ancestor of both had already separated in the Pliocene from the line which led both to the great apes of today and to related extinct fossil forms. To him *Australopithecus* was one of man's lineal ancestors.

Elliott Smith, who was a far more distinguished and critical scientist than Keith, was Dart's mentor; but although he probably wished to lend him his support, he knew that at that time Dart was no expert in the comparative osteology of the primates, so that in spite of all Dart's efforts at persuasion, he refused to give his support to this one of his disciples.

On the other hand, he had already lent all the weight of his authority to the claims of Davidson Black, also a former member of his school, who had become responsible for the first official description of the newly discovered Peking man, and who had declared that the Chinese fossil differed sufficiently from *Pithecanthropus*, the Java ape-man, as well as from Neanderthal man, to be placed in a genus, *Sinanthropus*, of its own.



Lord Zuckerman

Elliot Smith, like almost all other anthropologists, accepted and proclaimed this view, but one day, in 1931, he asked me to make an independent review of the subject for a lecture to be given at the Royal Anthropological Institute. I accepted the invitation, but soon found that in their metrical characters—the fossils all looked fairly similar to the naked eye—there were fewer differences between those *Sinanthropus* remains which were then available and certain Neanderthal skulls, than there were in the whole group of skulls that were classified as *Homo neanderthalensis*. Nor were there any significant dimensional differences between *Sinanthropus* and *Pithecanthropus*.

Since it was known that the Neanderthal specimens, although widely separated from each other both geogaphically and in time, were no more variable than a sample drawn from a homogeneous population of contemporary man, it was clear that something was wrong. Unequal significance had obviously been attached to the peculiarities of all these fossil men, and I therefore felt that one either had to include Peking and Java man in the same genus, or revise the classification of all extinct human fossils. That seemed preferable, so I suggested that we should subdivide the Family Hominidae, to which both fossil and living men belong, into the two subfamilies, Palaeanthropidae and Neanthropidae—into the latter of which modern man would fit. Elliot Smith immediately abandoned his previous view and accepted this conclusion and, in due course, so far as I am aware, so did other students of the subject. The terms I proposed are no longer used, but to some extent they are implicit in the two that are—*Homo erectus* and *Homo sapiens*.

Once again the subjective and arbitrary nature of the recognition of an ancestor had been driven home to me—but I must confess, this time with hardly any surprise. The more I became aware of the personalities engaged in the subject, the less the subject seemed to be a science than a free-for-all in speculation. When, therefore, the Piltdown story blew up as it did, I was again not surprised—although I still may be when its final chapter comes to be written, if ever it is.

Let me recall the bare outline of the story. Over a period of a few years beginning in 1908, some pieces of a brain case, a piece of jaw, and a lower canine tooth were found by three different sets of men in a gravel pit near Piltdown in the County of Sussex in England. When fitted together, and reconstructed, the cranial fragments were clearly human—whichever of nine opposing reconstructions one considered; to some, however, the jaw and tooth seemed to be those of an ape and not a man. Arguments became shrill. Most of the world's authorities, including men like Alex Hrdlîcka, then the leading physical anthropologist in the USA, and Elliot Smith and Arthur Keith in England, were convinced that the skull and jaw came from the same individual, and that the ape-like appearances of the jaw and tooth were, in fact, illusory. Only a few took the contrary view.

Then in 1935 a dental surgeon named Marston discovered another and indubitably human fossil skull, now known as the Swanscombe skull, after the name of the village in Kent near which it was found, and his suspicions became aroused about the Piltdown specimen. He was convinced that the jaw was that of a chimpanzee. Other people's interest was then stirred, and it was decided to use the fluorine test to see if the jaw and skull were of the same age—the fluorine content of bone increases with geological age. It soon became apparent from this and other tests carried out later that the Piltdown jaw, tooth, and skull did not belong to the same individual. All had been planted, and the whole thing was a deliberate hoax.

I seem to have been the only person who in public comment at the time was far less interested in the fraud itself than in the fact that it showed that the most distinguished anatomists and primate palaeontologists were unable to diagnose, by reference to so-called anatomical criteria, what was human and what was ape-like. This brings me back to *Australopithecus*.

The key persons in this story after Dart were Broom and Le Gros Clark. I say key persons because, as I think I have shown, the assessment of fossil remains which might bear on human ancestry has depended up to now far less on science than on advocacy and authority and on *ex cathedra* statement. One reads time and time again in the literature of the subject that this or that must be so because more people believe that it is than do not. Scientific truth has, however, never depended on a counting of heads!

Broom was the only South African scientist of any renown at the time *Australopithecus africanus* was unearthed. He was a Scot by birth and, after graduating in medicine from Glasgow, had emigrated, first to Australia, and then to South Africa, where he built up a considerable reputation as a palaentologist. He was a man of colossal energy, and soon became the leading authority in the world on Triassic mammal-like reptiles, for the collection and study of which he was prepared to sacrifice anything and everything. Sometimes he practiced as a country doctor, sometimes he served as a professor, sometimes he was a dealer in fossils; but whatever he did, his main concern was palaeontology. He was a man of firm belief and considerable character. At one point in his intermittent career as a general practitioner he had taken up the job of medical officer for two gold mines. That was in 1909. Within three months of making his home in what was then the township of Springs on the East Rand, he was mayor of the town and president of its chess and revolver clubs!

Scientific truth has never depended on a counting of heads

Fossil reptiles were the central interest of the main part of Broom's scientific life, but there was no fossil about which he was not prepared to offer an opinion. He did a certain amount of not very profound work on South African fossil mammals. He had also entered the debate about the significance of a fossil human skull found in a place called Boskop about 150 miles from Taungs. This skull clearly falls into the range of modern man, but to Broom (presumably because it was a fossil) it was a primitive species of man for whom a separate species —*Homo capensis*—had to be created.

Broom believed himself to be capable of recognizing immediately whether a fossil which came into his hands was something he had seen before, or something he wished to declare unique. He was what is known in the world of taxonomy as a "splitter," the opposite of a "lumper," and he loved inventing new species. He had a considerable visual memory, and an ability to remember the detailed characters of a particular specimen. But he was little concerned with the degree of variation to which all species are subject. There are some revealing passages in the official biography written of him for the Royal Society of London by the late Professor D. M. S. Watson. One is worth quoting in the light of what is to come. "As soon as he had satisfied himself about the structure of a specimen," wrote Watson, "he made a drawing of it . . . . and as sutures in Karroo skulls are commonly obscure and not easy to follow, he inked them in, thus nearly ruining the specimen by preventing any other person from forming an unbiased opinion." No wonder Watson also writes that in his later years Broom did not move with the times!

For Broom, who believed that physical evolution had come to an end, and that modern man was the pinnacle of the Lord's design, the Taungs and other Australopithecine skulls which have been unearthed since 1924 were not only a godsend, but—and this he stated in a personal record he left—a godsend designed by Providence personally for him.

With Elliot Smith's death, and the retirement of Arthur Keith, the late Professor Le Gros Clark became the doyen of British anatomy. His research interests were modeled on those of Elliot Smith, and in addition to neuroanatomy, they included primate phylogeny and human evolution.

At first Le Gros Clark was among those who were wary of the claims that Dart and Broom were making about the Australopithecines. But soon after the end of the Second World War, he was persuaded by them—and I believe by the late Dr. Leakey as well—to abandon his earlier scepticism and to agree that the South African Australopithecine fossils were "early representatives of the hominid line of evolution and quite distinct from the pongid (i.e., simian) line." As he put it, "The Australopithecinae conform very closely to theoretical postulates for an intermediate stage of human evolution," and are "exceedingly primitive representatives of the family which includes modern and extinct types of Man."

Largely owing to Le Gros Clark's support for the views of Dart and Broom, the Australopithecine story soon became part of textbook orthodoxy. Analyses which pointed to a contrary view were either ignored or brushed aside as uninformed and inaccurate. Clark had embarked on a new mission and was unwilling to face or to answer the results of studies which showed that the anatomical facts about the Australopithecines were not always those claimed for them.

No one, not even the most ardent Australopithecine addict, has suggested that the general appearance of the Australopithecine skull is human—as opposed to apelike. No specimen yet found has a cranial capacity even as large as the largest recorded figure for one of the living great apes. Several specimens—including *Zinjanthropus*—have sagittal and nuchal cranial crests of the kind found in the great apes, and particularly in male chimpanzees and gorillas.

The great Cuvier is reported to have said: "Give me any bone of an animal, and by correlation I'll be able to reconstruct the whole skeleton." The school of Australopithecine enthusiasts seemed to be saying: "We know what one of our ancestors should look like—give us any bone, or bony fragment, and we'll be able to describe it in such a way that it fits a picture of a skeleton which we are certain was the frame of one of man's lineal ancestors. And for good measure we will also tell you about its love life—that is, if you want us to. Or how it lived, what its birth weight was, and so on, and so on." As it has turned out, however, the attempt to turn anatomical principles inside out so as to uphold a preconceived phylogenetic conclusion has proved to be as unnecessary as it was unconvincing.

The three critical sets of change which transformed a presumably simian into a human form have for generations been supposed to be the development of the brain, associated with the continued elaboration of the visual, tactile, and acoustic sensory pathways, and with it of speech and a conceptual language; the emancipation of the forelimb, with the retention by the hand of many primitive features, such as a fully opposable thumb; and the assumption of an upright stance. I shall say nothing further at this point about the brain of the Australopithecines, except to remark that their cerebral equipment was even smaller than was previously claimed—and that was small enough! Nor, because little that can be relied upon has been either released or published on the subject, shall I deal with the forelimb and the hand. Let me return to the question of upright stance.

The suggestion that the Australopithecine creatures were, in effect, small, pinhead but upright, men was first made, as I have said, because of the assumption that the head was balanced on the trunk as in man and not as in apes. It was then backed up by reference to the apparently human characteristics of the pelvic bones. Still later came vague statements to the effect that the inference that the Australopithecines walked and ran as we do was also implied by the anatomical nature of certain limb bones.

I have already spelt out in print why all these *ex cathedra* pronouncements have to be rejected. In brief, the

continued on page 22

Power from the Wind

In our search for new energy sources we might pause to consider an old one

BENGELSDORF: The world is finding out these days what scientists and engineers have known for a long time, that it's not only love that makes the world go round, it also takes matter and energy. All of our energy comes from the stars. Consider the sun—the star that is closest to our planet. The oil, natural gas, or coal we burn today are but the remnants of ancient sunbeams. The sunbeams of today supply us with the food we eat, the fresh water we drink, and the oxygen we breathe. The sun also causes ocean water to evaporate in clouds to fall as rain or snow to fill our lakes and rivers, and as the water of the river flows back to the ocean, it can be made to turn a turbine to provide electricity. We call this hydroelectricity. But the sun also provides the energy to move the air which creates the wind. Can we harness the flowing air as we have harnessed the flowing water that generates electricity? Homer J. Stewart, professor of aeronautics at Caltech, thinks we can.

Dr. Stewart, I understand that there was a project in the United States, and a technically successful project, that did harness the winds to produce electricity. Could you tell us about it?

STEWART: Well, of course windmills have been used to make power historically for over a thousand years, to turn grindstones, pump water, and similar things. Most of these were relatively small windmills—maybe one horsepower or two or three horsepower output. In the late 1930's I participated in a general project to design and build a one-megawatt windmill. This was installed in the mountains of Vermont on Grandpa's Knob, and it was operated for about four years during World War II.

BENGELSDORF: I understand this was at an elevation of about 2,000 feet where some of the wind speeds got up to about 115 mph.

STEWART: Yes. Of course, almost any site that is generally favorable from the wind standpoint is going to be one that periodically has a major storm of one kind or another. So part of the design of any such equipment has to be a protection against excessive winds. In that case we also had icing problems to worry about—which was one reason the windmill was placed at 2,000 feet rather than going higher. There were also some other problems that made the lower altitude look more favorable than the very high mountain regions.

BENGELSDORF: Could you tell us a little bit about this Grandpa's Knob machine? How many blades did it have?

STEWART: It had two blades. They were quite large; the overall disk that the windmill's blades swept out was 175 feet in diameter, and the tower on which it was mounted was 150 feet high. Each of the two blades had a cord of about 10 feet. So they looked more like middle-sized airplane wings than they did like propeller blades, as we normally think of propellers.

BENGELSDORF: So one would think of them as sort of two airplane wings turning around.

STEWART: That's about what it was like.

Homer J. Stewart

"Power from the Wind" is a transcript of a radio interview with Homer J. Stewart, professor of aeronautics at Caltech. This is one of a series of interviews with Caltech faculty members, broadcast regularly (Mondays, 7:15 p.m.) over KPCS (89.3 FM), Pasadena. The program, "Frontiers of Science," is conducted by Irving Bengelsdorf, Caltech director of science communication.

BENGELSDORF: You mentioned that it had a capability of about 1,000 kilowatts (1 megawatt). That would be about 1,000th that of Hoover Dam, at Lake Mead, which is about 1,250 megawatts.

STEWART: Yes, a megawatt is a power unit that is compatible with a small village of maybe 500 or 1,000 people, but it's not compatible with a city. It would probably take hundreds or thousands of windmills of this size to supply the power needs of any large area.

BENGELSDORF: The interesting thing about this Grandpa's Knob project was that it actually did put electricity into the grid of the Vermont electric company.

STEWART: Yes, for four years during the war it actually operated right in with their public power system. Its peak power was a megawatt. Obviously, if the winds aren't blowing you don't get any power, so the average power was about a quarter of its peaks. It averaged out about 240 kilowatts throughout the year.

BENGELSDORF: This project met with a mishap in 1945. Could you explain that?

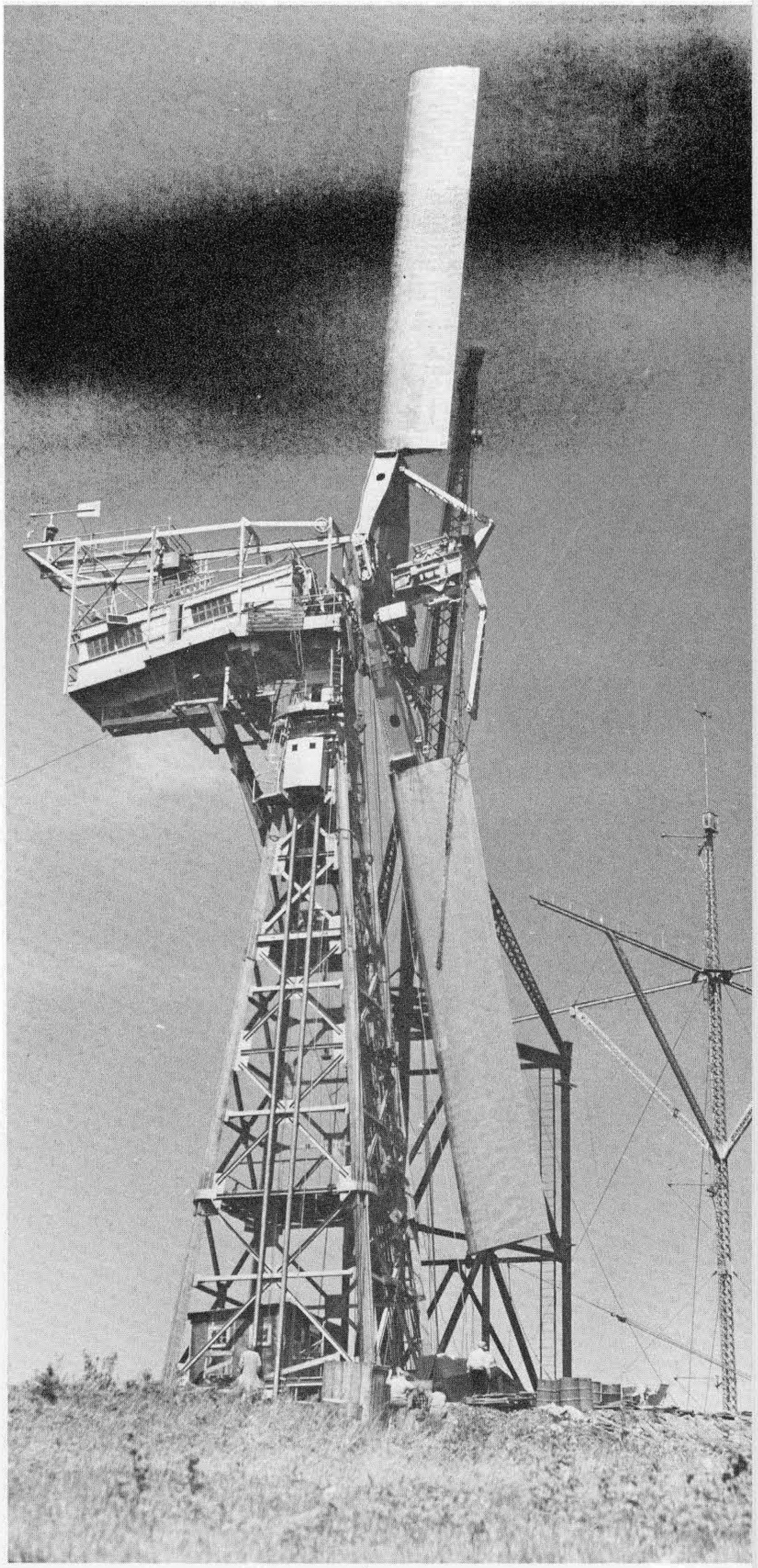
STEWART: Yes. Toward the end of the war the machine was shut down for a period because one of the bearings needed servicing and it wasn't readily available during the wartime procurement problems. Then after the war was over, the decision was made to repair it and put it back into operation. During the interval some of the blade skin had become damaged and had to be repaired. Unfortunately, in the process of repairing that, they managed to do some poor-grade welding and damaged some of the basic structure. The damage wasn't detected before it was started up again and at that time the basic structure failed and the blade broke and it was really a very major failure.

BENGELSDORF: Right. One of the two blades tore off the machine. So although the project was abandoned in November of 1945, it did demonstrate technical feasibility. What about the economics?

STEWART: That, of course, is the question. The purpose behind the project was to develop some real equipment by which the economic value could be determined more accurately than we could determine in advance by paperwork. In 1946 they concluded that by one standard the windmill missed being competitive with coal-steam by about 15 percent.

BENGELSDORF: Of course, we've come a long way since 1946 and economics have changed, so you seem to be a little more optimistic about this.

STEWART: Yes, in many ways. In the first place the coal-steam or oil-steam kind of systems are at a significant economic disadvantage now compared to what they were then. For example, the great increases in the price of oil



From 1941 to 1945, when the wind blew on Grandpa's Knob in Castleton, Vermont, the 32-ton stainless steel blades of this enormous windmill generated up to 1,000 kilowatts of electricity.

are apt to be at least partially reflected forevermore. And coal is also very much more costly than it was then. Then in addition, the environmental protection problems that we're most sensitive to these days require the installation of quite expensive technical equipment to process the sulfur in the coal either before it is burned or out of the stack gases after it's burned; and similarly with oil. Natural low-sulfur oil is quite rare and very expensive.

In addition to the factors that have served to make conventional systems more expensive, modern technological developments of the last 30 years offer promise of improving the windmill in its competitive position. For example, aerodynamic design, air foils, the things we learned during World War II and thereafter—and in particular the work on modern gliders—show how we can get maybe 50 percent reduction in blade area.

BENGELSDORF: And you can also have lighter blades.

STEWART: Much lighter blades as a result. This would result in better efficiency, better control mechanisms, electronics, and so forth. The control mechanisms in those days were very exotic pieces of machinery and of course now we've had 30 years more of experience in similar types of things, and such equipment is not only much better but it's much cheaper.

BENGELSDORF: What about where you would put them? Do you have to have a special place for these windmills? Do you have to go to the top of a mountain?

STEWART: There are places where, of course, you *wouldn't* put them. For example, the Los Angeles Basin is so well protected by the mountains surrounding it that the average winds down here are very light.

BENGELSDORF: That's part of our pollution problem.

STEWART: It certainly is. On the other hand, almost the entire Mojave Desert, in particular the Cajon Pass area of the desert as it approaches the pass, has quite favorable wind distributions. In fact, some data taken from the Goldstone Tracking Station out in the Mojave show that the wind statistics there are much the same as at the site we had in Vermont—which is quite favorable.

BENGELSDORF: You are saying that one of the windmills placed out in the Mojave Desert just outside of the Los Angeles area would behave like one placed on one of the mountains in Vermont.

STEWART: That's right. The actual statistics, when I integrate them, turn out to give exactly the same performance on an annual average basis.

BENGELSDORF: This indicates that you could tap a great deal of the available wind potential.

STEWART: Yes, and there is a lot of wind power. Wind is different from water power. We already exploit almost all of the favorable water power sites in the United States and they supply only about 4 percent of our electrical power requirement. Back in the 30's—when we were organizing this project—Hurd Willett, in the meteorology department of MIT, made an estimate that corresponds to a potential availability at good sites in the United States of about a million megawatts. That is more than the total electric power we're now using.

BENGELSDORF: So it really could supplement in large fraction the power usage of the United States.

STEWART: I think it could. Even in the long run, I think it might serve as a 10 to 20 percent supplement to the long-term nuclear systems that we may rely on for our main power.

BENGELSDORF: That's certainly a very large number. How do you envision the size of these machines? How many machines, for instance, per acre?

STEWART: Well, the Grandpa's Knob machine is about the size we have to expect. If you try to make machines much smaller, the generator costs get too large and the control costs get too large. If we try to make them much larger, the blade and windmill structure get too costly. So I think that general size is about what we can think of. This is one of the reasons I'd like to see a design like that done again, so we could reexamine these questions in modern technological terms.

BENGELSDORF: How much money would you need for funding to carry out a prototype windmill?

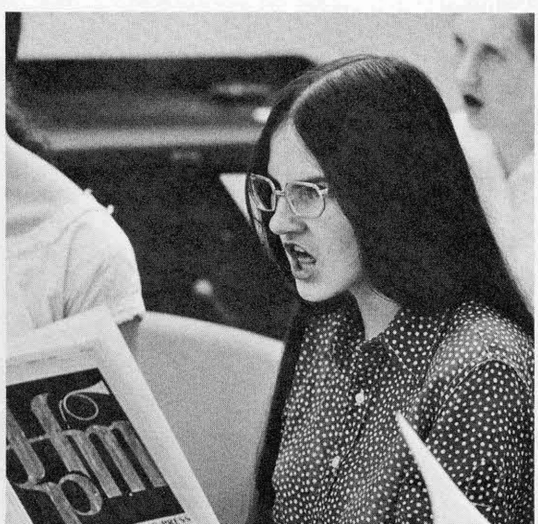
STEWART: I think that to get a device to parallel the Grandpa's Knob experiment, to get it running and get some data from it, would take something like $10 million and several years of time.

BENGELSDORF: So in a few years with $10 million, which is sort of a pittance in modern day research, you could demonstrate, perhaps with a prototype at the Goldstone area in the Mojave Desert, whether these windmills were practical or not.

STEWART: That's a proposal that I would like to have approved. □

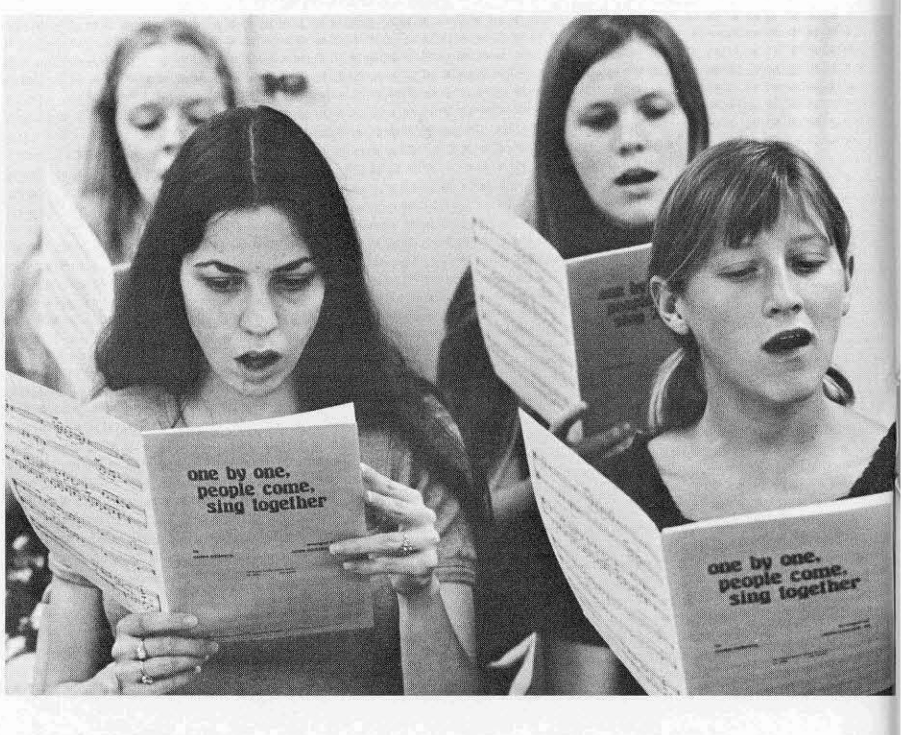
An Outburst of Music

Caltech students have always had a special affinity for music, and the Institute has always encouraged this interest by sponsoring a smorgasbord of musical activities. This year, though, it's more like a ten-course dinner. A musical explosion seems to have occurred on the campus. More students are making more music in more different groups than ever before. For instance—



Piano Man
James Boyk, a young Los Angeles concert pianist, is spending a day a week on campus this term, as Artist-in-Residence, giving private lessons and conducting a master class in piano.

Women's Glee Club
Caltech's Men's Glee Club (*E&S*—October 1973) is still one of the best in the business, but a Women's Glee Club and the Chamber Singers, under the direction of Monica Roegler, are moving in on it. The Women's Glee Club has 20 members; the Chamber Singers is a mixed chorus, with 16 singers from the larger glee clubs.



Chamber Music
Violinist Alice (left) and cellist Eleonore (above) Schoenfeld started chamber music classes at Caltech last fall. They chose almost 40 students through auditions, divided them into eight ensembles of varying size, and worked with them in weekly sessions throughout the year. The performers—who include undergraduate and grad students, faculty, staff, and their families—gave their first concert early this month.

An Outburst of Music. . . *continued*

Kiss Me Kate
After daily rehearsals for almost a month, the ASCIT musical, *Kiss Me Kate*, played to sold-out houses for two nights in April. Probably the most ambitious, and certainly the most sophisticated, musical yet attempted by the undergraduates, the show employed the talents of 40 undergraduates —including a few ringers from the Caltech staff and from nearby Occidental College.

Wind Ensemble
Under the direction of James Rotter, a concert saxophonist, the old Caltech Band has grown into a Wind Ensemble (shown here in full dress for the annual concert on May 4). On occasion, the group splinters into a clarinet quintet, a flute ensemble, a trombone quartet, a brass ensemble, or a jazz band. About 40 students, faculty, staff, and family members play in the Ensemble.

An Earthquake– On Schedule

For the first time, Caltech scientists have been able to predict the time and location of a sizable earthquake in California.

The quake occurred at 10:05 p.m. last January 30 near Yucaipa, about 16 miles east of the city of Riverside, and was felt widely throughout the east Los Angeles Basin. The event occurred at the expected site and within the expected time limit of about three months.

Only one factor of the prediction remained unfulfilled; the magnitude of the earthquake was 4.1 instead of the predicted 5.5, but the possibility of a larger event yet to come has not been ruled out.

The difference in the quake's predicted and actual magnitude may have been due to the fact that the Yucaipa event was a different kind of earthquake from the one used in developing the formula or model on which the prediction was based. If this is so, it will have significant implications on earthquake prediction research.

Research on earthquake prediction being pursued in this country, Japan, and Russia is based on the concept that strain increases in rocks prior to an earthquake, the strain causing the widening of microscopic cracks in the rocks. This reduces the speed of seismic waves moving through the rocks from distant quakes and explosions. The velocity is cut by as much as 20 percent. The widened cracks, which weaken the rocks, either gradually close or fill with water and the velocity of the seismic waves returns to normal.

When "normalcy" is reached, the rocks will rupture along a zone of weakness—a fault zone—triggering an earthquake. The longer the period of slow seismic waves, the greater the earthquake. This theory is called dilatancy from the idea of the rocks dilating as their cracks grow in size.

The dilatancy-diffusion model of quake prediction was developed by James Whitcomb, senior research fellow in geophysics, and his colleagues at Caltech. It is based on data from the 1971 San Fernando quake (of magnitude 6.4) and data reported for smaller quakes in Russia and New York. All of these quakes resulted from thrust-type faulting, in which land on one side of a fault thrusts itself under or over land on the other side.

The Yucaipa quake did not result from thrusting action but from either strike-slip fault movement or from movement on what geologists call a gravity, or normal, fault. Strike-slip faulting is where the rocks on one side of a fault slide horizontally past rocks on the other side. Gravity fault movement involves a dropping of land on one side of a fault in relation to the land on the other side.

The "unusual" behavior of the seismic velocities under Riverside was discovered in June of 1973 by Hiroo Kanamori, professor of geophysics at Caltech, by studying old seismic records of more than a dozen stations in the Caltech Seismological Laboratory network that blankets southern California. The wave velocities—averaging 20,000 to 30,000 miles an hour, with the higher speeds being at greater depths—remained constant at all stations except at Riverside.

Whitcomb determined that the wave velocity drop at Riverside began in the first part of 1972. The velocities were returning to normal last November, signifying an earthquake was imminent. The velocities had been slower for at least 13 months. That time span, based on the relationship developed at Caltech for thrust quakes of the San Fernando type, would predict a temblor of at least 5.5 magnitude. On the same basis, the time span for a magnitude 4 thrust event should be 26 days and for a magnitude 3 thrust event only 5 days.

Whitcomb observed that the velocity variations were most intense east of Riverside, implying that it was the most likely place for a quake. Portable seismic instruments were installed in the area so that when the quake did occur, a detailed record of it was obtained.

The Caltech study of the San Fernando quake demonstrated that the drop in wave velocity was due to a drop in the velocity of only one of the two earthquake waves—the P wave. That wave is a compressional wave that goes through the earth, while the S wave is a shear wave.

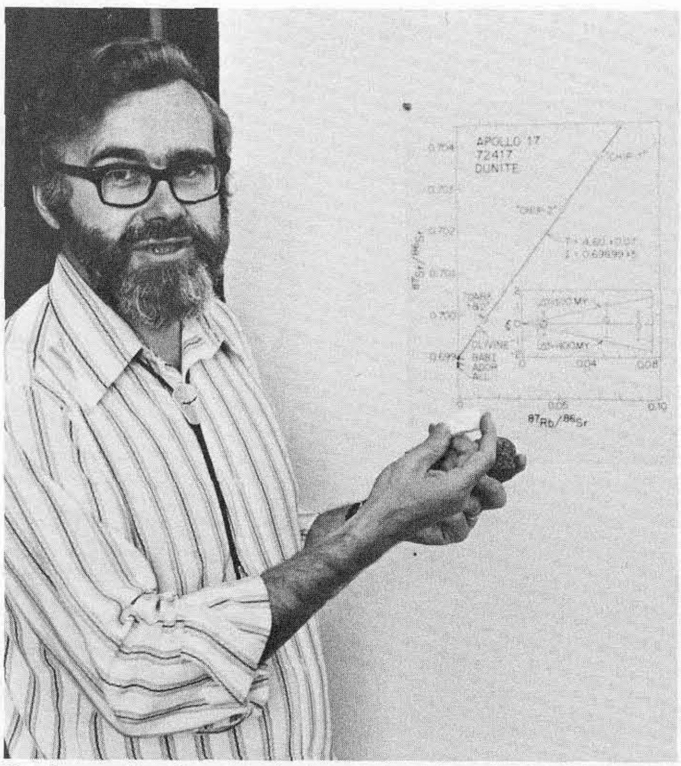
This important finding contradicted some earlier theories and supported a theory proposed by Amos Nur of Stanford University. The intensive study of the San Fernando quake led to development and confirmation of Caltech's present theoretical model for predicting earthquakes. □

Rocks of Ages

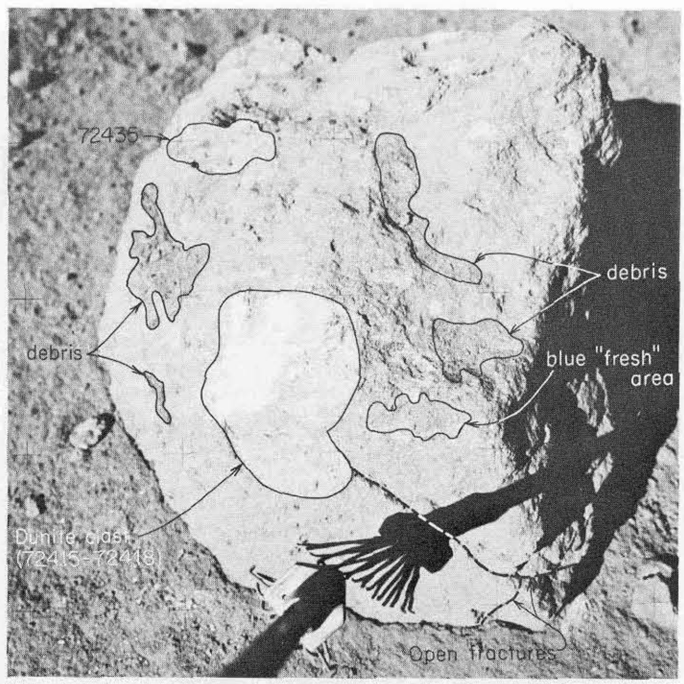
A major goal of the exploration of the moon was to find rocks as old as the solar system and thus to improve our understanding of how the earth, the moon, and the other planets were formed—about 4.6 billion years ago. The oldest terrestrial rock is only 3.7 billion years old, because erosion, mountain building, weather, and the atmosphere have erased the very early history of the earth. So, the moon, where those forces have not acted, was a logical place to look for older rock samples.

Most of the rocks returned by the early lunar missions were between 3.1 and 3.9 billion years old, with the oldest recorded at 4.2 b.y. There is a hint of older ages in feldspar crystals taken out of lunar highland breccias. But with the Apollo 17 samples, the age-dating program seems to have hit a primordial jackpot. Several of its rocks have been dated at between 4.2 and 4.5 b.y., and one tiny fragment checks out at 4.6 b.y.—which was almost immediately after the formation of the solar system.

This fragment—designated Apollo 17 No. 72417—is a greenish rock called dunite, composed largely of olivine, an iron magnesium silicate. It is so depleted in trace elements that it almost certainly must have formed during early lunar differentiation and associated gravitational settling. Apparently, at some later date, a large meteor



The fragments of lunar dunite brought back from Apollo 17's Boulder #3 are too valuable to be as casually displayed as Arden Albee seems to be doing. But, as a matter of fact, the white rock in his hand is only a scale model of the largest of the fragments.



After this boulder was photographed on the moon, astronaut Jack Schmitt chipped at it in two places with a hammer to break off samples that could be returned to earth. The largest area shown in outline is the place from which the fragments of ancient dunite were taken. Near the top of the boulder, the area labeled 72435 was sampled to secure an example of the matrix. The fringed shadow at the base of the boulder is cast by Schmitt's rake.

Scientists from the "Lunatic Asylum" report on the oldest lunar sample and the youngest meteorite

smashing into the moon gouged deeply into the interior, and propelled this bit of rock out onto the surface.

The basis for dating the origin of the solar system at 4.6 b.y. has been the study of the radioactive decay of meteorites that have crashed into the earth. Meteorites are believed to be debris left over from the formation of the planets, fated to wander aimlessly in space—unless or until they collide with another body. Most meteorites found on the earth are rated 4.6 billion years old, though one that landed in Sudan in 1942 is recorded at 3.5 b.y.

Age-dating of both moon rocks and meteorites is a project of the Caltech group working in the "Lunatic Asylum," headed by Gerald J. Wasserburg, professor of geology and geophysics. The group also includes Arden Albee, professor of geology; Dimitri Papanastassiou, senior research fellow in planetary science and physics; graduate students Robert Dymek, Alexander Gancarz, and Don Goldman; and senior spectroscopist Arthur Chodos.

Albee recently reported on the discovery of the 4.6 b.y. moon rock, and at the same time Papanastassiou announced that thanks to the generosity of Professor Clifford Frondel of Harvard University he had been able to study an unusual meteorite, called Nakhla, that fell on Egypt in 1911. Nakhla turns out to be a mere 1.3 b.y., and in addition it has a peculiar chemistry that shows it was molten sometime in the past—much later than the formation of the solar system.

Papanastassiou theorizes that Nakhla was once part of a small planet or minimoon that experienced melting about a billion years ago and then for some reason broke up and scattered pieces through the solar system. (The moon recorded its last major melting more than three billion years ago, and the earth still spews out molten lava.)

Scientists have generally assumed that the moon, with a radius of about 1,000 miles, is the smallest body in the solar system that was at one time active volcanically. But discovering the history of Nakhla suggests that there are probably numerous bodies around the solar system —perhaps with radii of only 150 to 200 miles—that have had a life of their own.

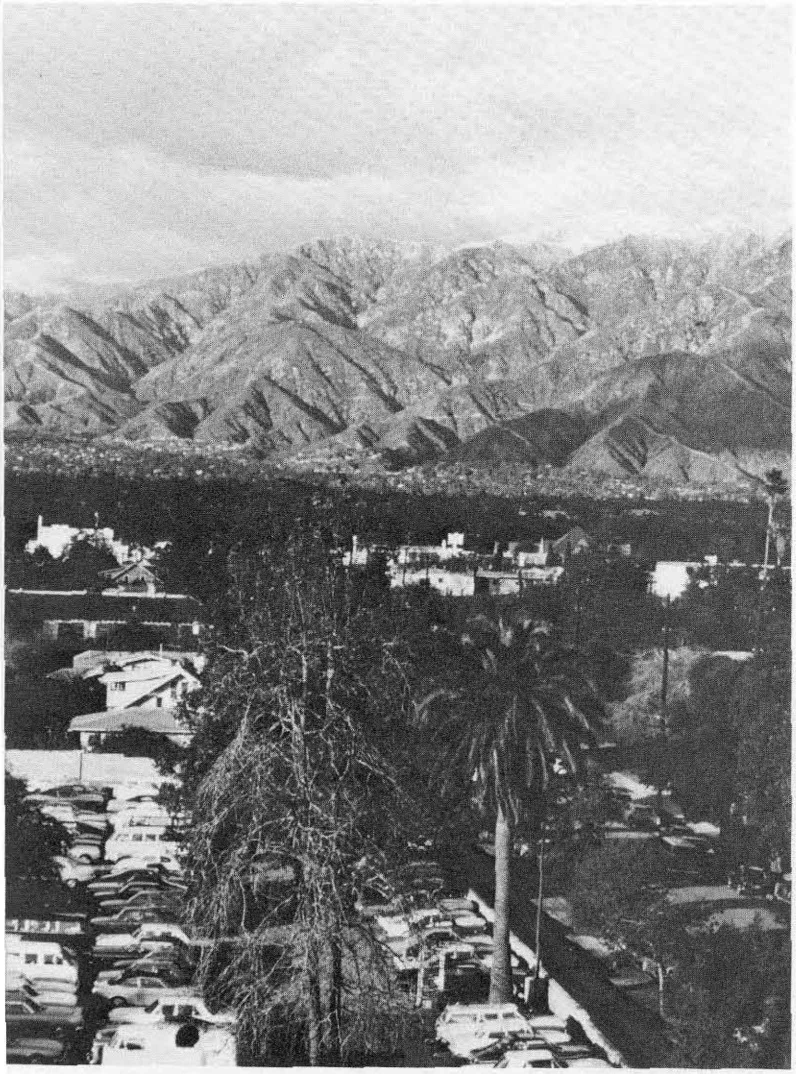
It has been so generally accepted that meteorites are close to primordial material that very few have been carefully age-dated. This preconception about the age

Dimitri Papanastassiou's cap, gown, and tweezers indicate some of the care used to prevent contamination of the rock samples analyzed in Caltech's "Lunatic Asylum." The rock is the Nakhla meteorite that Papanastassiou discovered was only 1.3 billion years old.

of meteorites and the time scale for the development of planetary objects has lasted 20 years. Nakhla's youth may change that, sending scientists back to their labs to check the age of whatever meteorite material is on hand.

This further research—and new information from it— could cause a revision of theories about the relationship between meteorites and the formation of the solar system. In fact, what the post-Apollo studies seem to show best is that we still don't understand the origin of the planets, their moons, or the meteorites. "Before Apollo," says Wasserburg, "there was one magic recipe in the solar nebula by which all planets, including the moon and earth, were formed. No one thinks that way any more. The Holy Grail is gone, and we're beginning to face the real problems of planetary formation for the first time." □

Trace-Metal Pollution–Sources, Routes, and Fates



On a clear day—
You may not be able to see forever on a clear winter day in Pasadena, but—looking across the Keck parking lot—you can see Altadena at the foot of the sharply delineated San Gabriel Mountains. The palm tree in the lower right-hand corner is almost the only identifiable feature in the photograph on the opposite page, taken on a smoggy summer day. The cause of the reduction in visibility is the high concentration of atmospheric particulate matter.

It's no secret—especially to southern Californians—that industry and automobiles discharge large amounts of pollution into the atmosphere. Of course, what happens next is not so clear, but now a group of Caltech researchers is trying to find out by studying the sources, routes, and fates of trace metals emitted into the air in the Los Angeles Basin.

Earlier research showed that 18 metric tons of lead are exhausted into the basin each day—12 tons deposited within the basin, and 6 blown out of it. Fallout measurements within a radius of about 150 kilometers (93 miles) —in the Mojave Desert, the Coachella Valley, the San Gabriel Mountains, and the coastal islands—account for less than 3 of those 6 tons. The other 3 are evidently transported more than 150 km before being removed from the atmosphere.

More lead is blown out of the Los Angeles Basin every day than is exhausted into the atmosphere in all of Riverside and San Bernardino counties and the Southeast Desert Air Basin. During daytime hours, polluted air generally moves east and north from Los Angeles, which means that Riverside and San Bernardino counties, which are downwind, cannot effectively control the amount of lead in their atmosphere by simply controlling local emissions. They must depend on the reduction of lead emissions in Los Angeles and Orange counties.

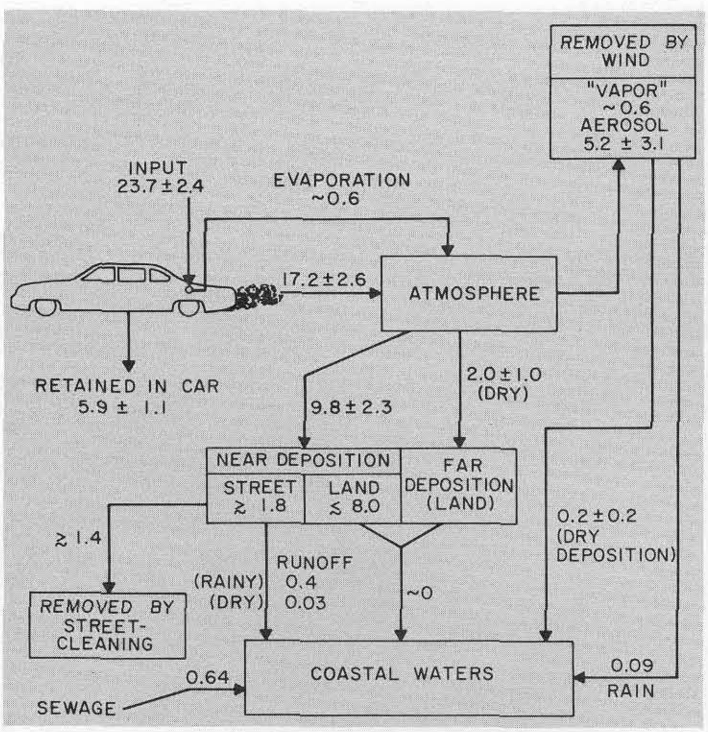
The analysis of pollutants that reach the coastal waters through rainout-washout, rainy weather runoff, dry weather runoff (from the watering of lawns, for example), treated sewage, and atmospheric fallout reveals that approximately 500 metric tons of lead enter the ocean annually. This breaks down into 30 tons from rainout-washout, 10 from dry weather runoff, 230 from treated sewage, and 70 from atmospheric fallout.

Obviously, since they play such an important role in transporting lead to the coastal waters, the atmospheric routes must be controlled as much as wastewater sources. Also entering the ocean each year—with sewage as the primary source—are 1,600 metric tons of zinc, 300 tons of nickel, and 30 of cadmium.

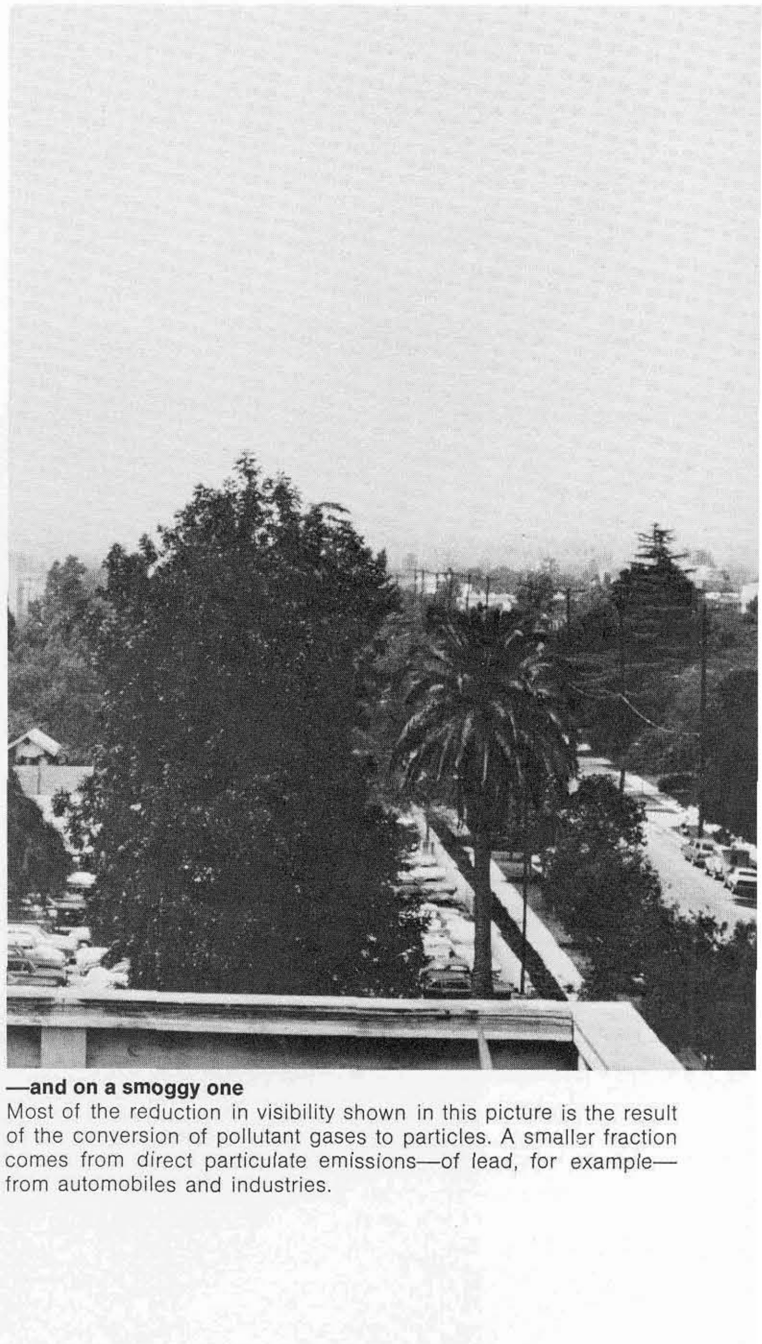
The flow of atmospheric zinc through the environment has also been studied in detail, and mass flow rates along the environmental pathways have been determined. The primary sources of atmospheric zinc are metallurgical operations, tire dust, and automotive exhaust. Most of the tire dust zinc is in large particles and deposits near roadways. Atmospheric nickel results primarily from combustion of fuel oil in power plants and other industrial operations.

These detailed studies of the flows of zinc, lead, nickel, and cadmium within the basin increase understanding of how trace metals move; and they also may make it possible to predict the behavior of other substances that might be introduced into the environment. The Caltech group hopes their research can eventually lead to effective regulation of toxic substances. It is increasingly clear to them that the environment must be considered as a unit when the effects of pollution are being assessed.

Sheldon K. Friedlander, professor of chemical engineering and environmental health engineering; J. J. Huntzicker, research fellow and instructor in environmental health engineering; and C. I. Davidson, graduate student in environmental engineering, are the Caltech scientists making these studies, which are sponsored by the Rockefeller Foundation, the California Air Resources Board, and the National Institute of Environmental Health Sciences. □



A flow diagram for lead emitted from automobiles in the Los Angeles area. Discharged into the atmosphere, primarily in particulate form, the lead is then either deposited on the land and streets, or it is blown out of the basin—depending on the size of the particles. The flows are indicated in metric tons per day.



—and on a smoggy one
Most of the reduction in visibility shown in this picture is the result of the conversion of pollutant gases to particles. A smaller fraction comes from direct particulate emissions—of lead, for example—from automobiles and industries.

Caltech's John Racs quite properly wears heavy plastic gloves for protection as he inserts a probe into the brain of an electric ray. Unfortunately, one of the gloves had a small hole, and Racs repeatedly received 200-volt shocks. The fascinated onlooker is high school teacher Gary Stellern, whose oceanography class hauled in the animal.

Neurobiological Research with the Electric Ray

On an oceanography field trip this spring a boatload of students from John Muir High School in Pasadena hauled in a hefty specimen that turned out to be a 65-pound *Torpedo californica*—an electric ray indigenous to this part of the Pacific. Their teacher promptly put in a call from the boat to Caltech, and when the ship docked, two members of the Institute's neurobiological research group —Richard Vandlen, research fellow in chemistry, and technician John Racs—were ready to remove the ray's electric organ for use in their research. As a matter of fact, Vandlen and Racs are routinely called to come and pick up torpedoes, which are netted along with the anchovies fishermen catch for bait. Rather than throw the dead rays back into the ocean, the fishermen bring them back and freeze them, and every few weeks the researchers go down and collect whatever is in storage.

What made this particular ray worth an extra trip was its size; the average ray weighs only about 40 pounds. The electric organ is a modified muscle comprising 20 to 25 percent of the ray's total body weight. It is specialized for the production of electricity—amounting to 200 volts, 50 amperes—which is equal to about 10,000 watts, or enough to light a house momentarily.

The rays are used at Caltech in studies of how nerve cells are connected with each other and with muscle cells. The objective of this research is to elucidate how nerve cells transmit signals, and eventually to understand the chemical basis of the function of the nervous system.

The point of contact between two nerve cells, or a nerve cell and a muscle, is called a synapse. When an impulse or message is passed along a nerve fiber, it reaches the synapse, where it triggers the release of a chemical compound, acetylcholine. This compound, also called a transmitter substance, elicits a response from the neighboring cell: A nerve cell will fire another impulse, a muscle cell will contract, a gland will secrete its product.

Very little is known about how the synapse actually works. This is largely due to the fact that tissues containing large quantities of synapses, like the brain or spinal cord, are very complex and contain a great number of different types of synapses. Tissues that contain a single type, like muscle, normally have very few synapses—which renders chemical investigation almost impossible.

However, nature has invented a tissue with a synapse concentration about 1,000-fold that of normal muscle tissue. These tissues are the electric organs of certain species of fish such as the electric eel, a freshwater animal from the Amazon basin; and the electric rays, which live in all of the world's oceans. One of the most highly innervated electric organs is that of the California ray, *Torpedo californica*. Large animals of these species can have electric organs totaling ten pounds and more; every gram containing 20 times more nerve-ending material than a gram of eel organ which, in turn, is much more richly innervated than ordinary muscle.

At present the main research effort in this field is directed toward a better understanding of what the transmitter substance does to the target cell. It is known from physiological investigations that the transmitter has to interact with a specific membrane component for a response—say, a muscle contraction—to occur. This membrane component, usually referred to as "transmitter receptor," obviously plays a key role in nerve cell interaction.

Two years ago British scientists succeeded in extracting the "transmitter receptor" from the electric tissue of a ray. Recently the group at Caltech worked out a procedure to purify the receptor from all the other membrane components. For the first time sufficient amounts of pure transmitter receptor have become available for detailed study. To continue these investigations, however, large quantities of torpedo organ will be required. The more electric rays are available, the more work can be done toward the elucidation of the fundamental mechanisms of nerve cell action and toward the investigation, prevention, and treatment of a variety of nerve and muscle diseases. □

The Month at Caltech

National Academy of Sciences

At its annual meeting last month the National Academy of Sciences added two more members of Caltech's faculty to its roster; they are also both alumni. Roy Gould, professor of electrical engineering and physics, and Leon Silver, professor of geology, bring the number of Institute faculty members to 49, out of a total NAS membership of approximately 1,100.

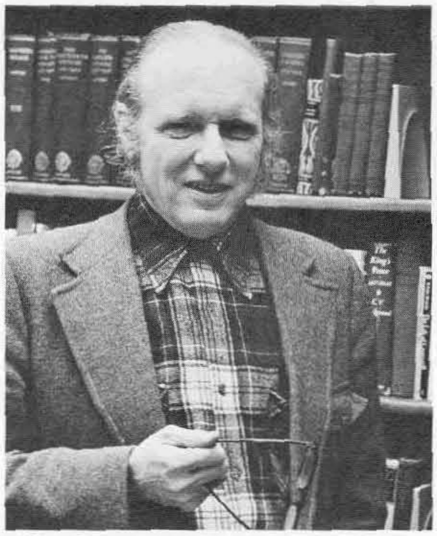
Gould (BS '49, PhD '56) is also a member of the National Academy of Engineering. He has been on the Caltech faculty since 1955 and is currently executive officer for applied physics. The NAS citation describes his fields of interest as "electron and ion dynamics, plasma oscillation and wave phenomena, physics of ionized gases, electromagnetism, microwaves, plasma physics, and controlled thermonuclear fusion." From 1970 to 1972 he was on leave from the Institute to serve as director of the Atomic Energy Commission's division of controlled thermonuclear research. During this period he supervised fusion-research efforts conducted at Lawrence Livermore, Los Alamos, and Oak Ridge Laboratories; at several private industrial laboratories; and at a number of universities. He is currently chairman of the American Physical Society's Division of Plasma Physics.


Roy Gould

Silver received his PhD in 1955, and he has been a member of the Institute faculty since then. He has done extensive research on the older basement rocks of North America with particular emphasis on geological formations in Arizona and in the San Gabriel Mountains.

He has been actively involved in the lunar exploration program, using radiometric techniques to establish the age, impact history, and evolution of lunar surface materials brought back by Apollo crews. He trained Apollo crews 13 through 17, contributing greatly thereby to the soundness of the lunar collecting program. His NAS citation points out that he "is virtually unique among geochronologists in that he employs geology, geochemistry, and geochronology to advance our understanding of the earth's crust and lunar surface."


Leon Silver

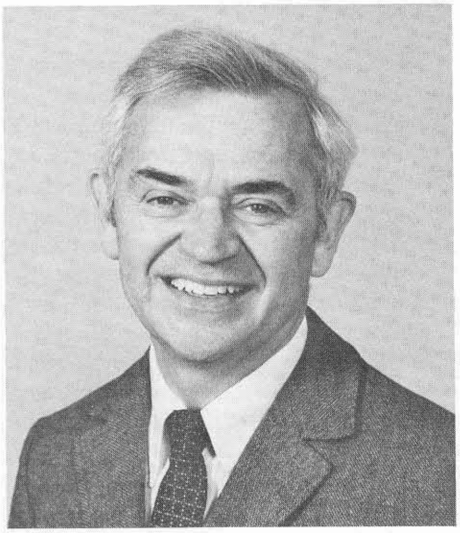

William Riker

By adoption, Caltech can also claim one other new NAS member. He is William H. Riker, chairman of the department of political science at the University of Rochester, who is currently a Sherman Fairchild Distinguished Scholar at the Institute. Dr. Riker is noted for introducing mathematical methods into the study of politics and government.

National Academy of Engineering

The National Academy of Engineering's annual election of new members in April increased Caltech's representation by five—two faculty and three alumni. One of the faculty members is also an alumnus; Frank E. Marble (AE '47, PhD '48) is professor of jet propulsion and mechanical engineering. The other faculty member is Ronald F. Scott, professor of civil engineering.

Marble's research deals with turbulent combustion processes in such diverse fields as chemical lasers, central power stations, and jet engines. He is concerned with noise created by jet engines, especially that associated with the internal aerodynamics of turbine and compressor components, and he is studying the attenuation of noise by vaporization of liquid droplets. He is a member of the National Academy of

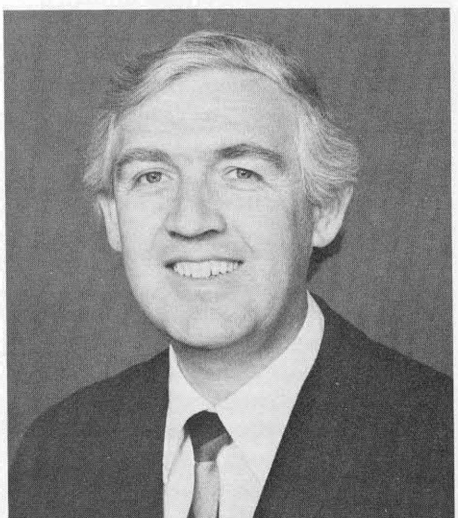


Frank Marble

Sciences committee on sonic boom and engine noise for supersonic transports and of the National Research Council's committee on motor vehicle emissions. His election to NAE recognized his work in aero-thermal chemistry and its applications to problems of gas turbines.

NAE honored Scott for his contributions to the theory and applications of soil mechanics. His current research is on the behavior of soils in earthquakes and on the ocean bottom, and on the properties of lunar soils. He has also studied the mechanical behavior of frozen ground and permafrost.

Several years ago Scott investigated the mechanical properties of the moon's surface, directing the Surveyor spacecraft's mechanical arm to dig holes and



Ronald Scott

perform other tests on the lunar surface. The information gained was later used in the design of the lunar landing module and several experiments in the Apollo program.

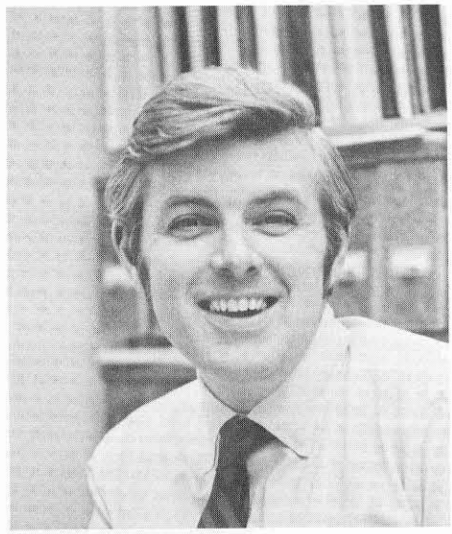
New alumni members of the NAE are Phillip Eisenberg (CE '48) of Laurel, Maryland, for contributions to naval architecture, particularly ship hydrodynamics; James C. Elms (BS '48) of Washington, D.C., for contributions to conceptual designs and management of space, electronics, armament, and transportation systems; and Thornton A. Wilson (MS '48) of Seattle, Washington, for leadership in the engineering and management of major complex aerospace systems for commercial and military use.

New Director for EQL

Norman H. Brooks, professor of environmental science and civil engineering, has been appointed director of Caltech's Environmental Quality Laboratory. He succeeds Lester Lees, professor of environmental engineering and aeronautics, who has served as director since EQL was established in 1971.

Brooks is a native of Massachusetts and received his AB in mathematics from Harvard in 1949 and his MS in civil engineering in 1950. He was awarded his PhD in civil engineering and physics from Caltech in 1954, and he has been on the faculty ever since. Interested in many phases of environmental pollution control, Brooks has directed his hydraulic research toward developing effective ways to reduce the contamination of the ocean from sewage effluents and from heated water discharges by power plants. He has been a consultant to a number of engineering firms, corporations, and governmental agencies on design of outfalls for pollution control, and is currently a member of the Environmental Studies Board of the National Academy of Sciences and the National Academy of Engineering.

Since it was established, EQL has done extensive research and published a series of significant reports on such



Norman Brooks

subjects as power plant siting, air pollution control, strategy for improving the air quality of the Los Angeles Basin, energy conservation, and solar and geothermal energy. Brooks expects that these projects will continue, and he also hopes to initiate studies related to water quality and to control of hazardous substances in the environment.

Charles Bures, 1910-1974

Charles E. Bures, professor of philosophy, died in Pasadena on April 30 after a long illness. He was 64.

A native of Cedar Rapids, Iowa, Dr. Bures received his BA from Grinnell College in 1933. His MA in 1936 and his PhD in 1938 were from the University of Iowa. He served on the faculties of the College of Idaho, USC, and the University of Oregon, and as a personnel administrator for North American Aviation before coming to Caltech in 1949. He has specialized in the philosophy of science and the concept of probability.

He was a member of several professional societies and of the Sierra Club, the National Audubon Society, and other organizations interested in the preservation of the environment.

Dr. Bures is survived by his wife, Helen, and a brother, Frank L. Bures of Des Moines, Iowa.

 ... continued from page 5

The subject seems to be less than a science and more of a free-for-all in speculation

Australopithecine enthusiasts have simply been focusing on features which they regarded as human, and have disregarded others which might be more simian, or which are neither human nor simian. The most extensive and detailed bio-mechanical and statistical analysis of the pelvic girdle of living primates that has yet been carried out, and which has only recently been published, showed that while the locomotor pattern of *Australopithecus* cannot be deduced with certainty, it must—because of the almost certain lack of any powerful source of abduction of the hip—have been significantly different from that typical of man. In features relating to weight bearing, the creature resembled man rather than the sub-human primates, whereas in features relating to muscular pull it was the other way round. The locomotor pattern of *Australopithecus* was probably therefore unique, since in the whole complex of functionally significant features, the pelvic girdle of these creatures was markedly different from all living primates—man included. Also recently published is a study which shows that many of the reconstructions of Australopithecine fragments on which students have based their work incorporate major anatomical errors, including the misidentification of the midline in the fossil pelvic material. It also now turns out that until recently we have also been misled about the mid-sagittal line of the original Taungs skull itself. A revised reconstruction of the fossil by Holloway (1970), shows that the endocranial volume of the Taungs skull is some 25 percent less than what had been claimed hitherto, and that previously published values of *all* the South African fossils were "highly over-estimated." Even when highly over-estimated, they still fell well within the ranges for existing apes!

Mistakes in the diagnosis of the mid-line of hominid fossils have been made before. One of the more celebrated concerned the Piltdown skull, and resulted in an acrimonious dispute between several distinguished anatomists of the time. Another has recently come to light in connection with the mandible of what is still described as the most ancient of all hominid remains, a specimen called *Ramapithecus wickeri*, which was found by L. S. B. Leakey in Tanzania and which he had classified with corresponding mandibular and maxillary remains found in Miocene deposits in India. The basic reason for regarding *Ramapithecus* as more human than ape-like was the belief that its dental arcade was rounded as in man. A recent and more careful reconstruction by Walker and Andrews (1973) now shows that a mistake was made about the mid-line of the specimen, and that when this is corrected, the general disposition of the jaws is less parabolic than it is U-shaped, as it is in apes.

Almost more important than all this, and indicating that the Australopithecines were probably on some kind of sideline in the story of human descent, is the fact that some fossil remains have recently been unearthed in East Rudolph in Northern Tanzania from a deposit which is estimated by radiometric and palaeomagnetic determinations to be nearly three million years old—two million years older than anyone assumes the limestone deposits were in which previous Australopithecines were found. The new finds included the fragments of a skull and a complete left femur. The cranial fragments have lent themselves to what appears to be a fairly undistorted reconstruction of a skull which, while certainly not human, is equally less simian than any other Australopithecine. The sides of the cranial vault are not sloping, as in the typical Australopithecine and ape, but more vertical, as in man. There is no sagittal, and no indication of a nuchal, crest. The endocranial capacity is estimated at 800 cc. The associated femur is said to have been predominantly human in character.

Enough has already been published about these finds to make me believe that if this skull and the associated femoral remains had been found 20 or 30 years ago, few of the writers on the Australopithecines would have wished to turn anatomical sense inside out in order to prove that these ape-like creatures were men who had disguised themselves with false cranial crests and other simian characteristics in order to test our anthropological faith. Whatever the new skull turns out to be when properly studied, it has already proved the futility of anatomists, and sometimes amateur anatomists and journalists, staking their reputations on a presumed ability to recognize marginal hominid characters in a mythical "total morphological pattern"—to use Le Gros Clark's phrase.

But, as the history of the subject has shown time and time again, there will never be any dispassionate study if it is merely based on so-called anatomical judgment. Measurement and statistical analysis have to come into the assessment of marginal differences in the shape of corresponding bones. Happily, the development of high-powered electronic computers has opened up the way to methods of analysis which were inconceivable even ten years ago. When properly used—and this means that the anatomist or physical anthropologist needs to be guided by a professional, even if he is himself mathematically minded—such techniques can help significantly in checking anatomical judgments.

I am certainly no professional, and in these matters I—and this means my students too—have always insisted on collaboration with the best help available. But judging by some of what I have been reading lately, this lesson has not been learned by the majority of anatomists now attempting to assess the significance of anatomical features which can be defined metrically.

This is not the only lesson which one should bear in mind when one approaches the problem of choosing one's ancestors. Another is that it is an incredibly difficult choice, one so difficult that the sceptic could justifiably argue that it is not one which could ever be part of a branch of science proper. The problem is difficult not only when one tries to delve into the remote past of our Mammalian Order. It is equally difficult when one tries to unfathom the relations of different races of man in a period of, say, the past ten to fifty thousand years, for which there is immeasurably more material available for study than for the longer period with which this paper deals. Cavalli-Sforza, who has attempted to make a phylogenetic study of human ethnic groups by genetic analysis associated with field studies of food-gatherers and hunters, and who postulates that the genetic divergence between two populations increases with their separation in time, holds that there are only three major ethnic groups. Carleton Coon tries to persuade us that modern man has a polyphyletic origin—in his particular variant of the theory, the belief that *Homo sapiens* consists of five distinct sub-species (whites, mongols, australoids, and two kinds of black men), all of which have descended independently from different variants of the now extinct human type *Homo erectus* which lived in Java and China some half a million years ago. Buffon, Immanuel Kant, Lamarck are among the many who count as Carleton Coon's illustrious predecessors. But most of what they wrote on the subject, like much of what modern writers have written, constitutes no more than speculation, and some provide the reader with light entertainment rather than serious science. I remember one book, which first appeared in 1924, under the title of *The Mongol in Our Midst*, from the pen of a well-known English physician of the day named Dr. F. G. Crookshank, in which it was forcefully and wittily argued that the mongol and the orang-utan share a common ancestor, the negro and gorilla another, and what he called the Mesopotamian Semite and the chimpanzee a third.

If, in spite of the help provided by genetical, biochemical, and serological analysis, we are unable to make much progress in the study of modern racial relations, why should we pay all the attention we do to speculations about our remote past? Hundreds, perhaps thousands of millions of members of the Hominoidea must have walked on earth since the Miocene. Of these, only a few hundreds at most, distributed in various parts of the globe over a time scale of ten, twenty, thirty million years, have left us fossilized fragments of their skeletons, and of these only a few not so crushed and distorted, or so fragmentary, as to allow of reasonable reconstruction. It stands to reason that only by chance could more than a handful, if indeed any, be in the direct line of human descent. Alternatively, we could assume that all were, in which case the whole concept of a single line of descent becomes spurious. As Gaylord Simpson has put it: "Much of the rectilinearity of evolution is more a product of the tendency of the minds of scientists to move in straight lines than of a tendency for Nature to do so." As an alternative hypothesis, we could therefore suppose that there was some radiating network of hominoids within which one, two, or even more strains became selected as creatures which were going to develop human characteristics, and above all the capacity to use an articulate language as a vehicle for both real and abstract concepts. For that is the critical point. An upright, inarticulate pinhead like the mythical *Australopithecus* would not climb as far on any evolutionary ladder to humanity as the beetle-browed, slouching caveman of the upper Palaeolithic of twenty to thirty thousand years ago, who left us those superb paintings in the caves of Lascaux, and in southern Europe. Even if they walked on all fours, those cavemen artists of the old stone age would have been superior to an upright *Australopithecus*, given there ever was such a thing!

If any credibility is ever to attach to the subject of human and simian palaeontology, we shall need to be assured of certain vital conditions. First, there should be no secrecy on the subject. If a fossil is found it should not be hoarded and studied in some ignorant isolation, but disseminated to serious students in the form of photographs, measurements, and accurate casts of the *separate* fragments—separate and not assembled in some total reconstruction which as often as not proves inaccurate.

Second, the dating of the deposits in which fossils are found, and of the fragments themselves, where this is possible, should be carried out by competent people who have no interest in the answers, one way or the other, but who should also be able to give an objective judgment about the contemporaneity of relatively adjacent finds.

Third, the task of studying the fossils should be entrusted to people who know both their anatomy, human and comparative, and who also know, or are ready to learn, how to use and analyze measurements, and how to apply modern methods of analysis to stress configurations. There must be an end, as it were, to people who have never been to an art gallery or who have never studied the subject, telling us that a picture that they happened to buy for $5 in a junk shop is a Rembrandt.

Fourth, the work should be done by people who know something about population genetics and about rates of evolutionary change in vertebrates as a whole.

And fifth, and last, it should be done by people who start out knowing that what has been left by chance in a fossil deposit need not necessarily have any significance whatever to the story of our human lineage.

Several years ago there was a chimpanzee in the London Zoo with a presumed artistic bent, that painted pictures—some of which were indeed sold. But after the man who was then curator of mammals left (he was later the author of a popular book called *The Naked Ape*), the zoo's chimpanzees went back to their old degenerate ways as apes, and showed no inkling of any human artistic feeling. I sometimes have a notion that when some modern students of the behaviour of apes and monkeys in the wild turn their backs on their subjects, the animals may fail to live up to what is due to be written about them! Equally I realize that the hominoid fossils that are already available will enjoy an even greater constancy than do the living apes, and that perhaps they may still provide us with an ancestor when what has so far been written about them is largely forgotten. It is salutary to think how much more confusing the whole story would be if the great apes of today were known only from fragments of fossil bone, and if we had had to speculate about their ancestral relationship to us, as well as that of the Australopithecines! ☐

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GTE SYLVANIA

Sylvania Entertainment Products Group, Batavia, N.Y.

Stan Kaufman Fights Water With Jelly...

to keep people talking. Bell Labs chemical physicist Stan Kaufman invented a material that turns waterlogged underground phone cable into a water-free "jelly roll" that can carry calls again. Pumped through football-field lengths of cable as a liquid, the material forces water out and then turns to jelly—to keep the water out.

Water sometimes seeps into cable damaged by plows, lightning, gophers, or sharp rocks. Phone calls going through the cable become noisy or don't go through at all. Until now telephone companies had to abandon waterlogged cable, or dig it up and replace it, or use acetone to flush out the water. Once the acetone was evaporated, however, there was nothing to prevent water from getting back in again.

We needed an inexpensive water-repellent liquid that would turn into a jelly inside a cable and plug up holes. The material also had to be electrically nonconductive so it wouldn't interfere with telephone signals. Such a material didn't exist, so we asked Stan Kaufman, a 1970 Ph.D. from Brown University, to tackle the problem.

Drawing on his knowledge of molecular structure and working with telephone company engineers—sometimes in muddy cable trenches—Stan came up with a new compound. A Western Electric engineer modified a pump to force the compound through long lengths of cable. And during field trials, operating telephone engineers suggested installation procedures.

Bell Telephone companies are happy because they don't have to dig up as much waterlogged cable, which often runs under highways and people's lawns, and because restoring an otherwise good cable helps hold down the cost of providing telephone service.

Sales and Service.
Is this the kind of engineering for you?

Trying to figure out the exact kind of engineering work you should go into can be pretty tough.

One minute you're studying a general area like mechanical or electrical engineering. The next you're faced with a maze of job functions you don't fully understand. And that often are called different names by different companies.

General Electric employs quite a few engineers. So we thought a series of ads explaining the work they do might come in handy. After all, it's better to understand the various job functions before a job interview than waste your interview time trying to learn about them.

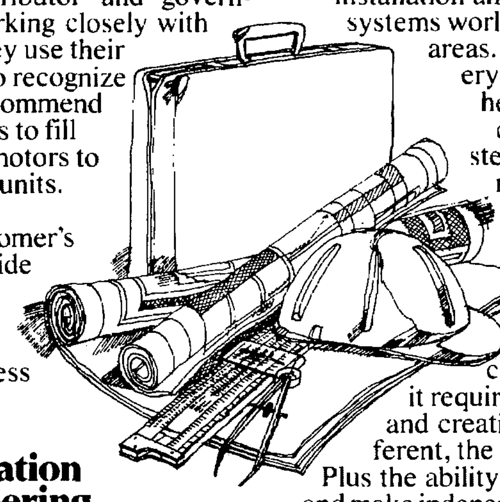
Basically, engineering at GE (and many other companies) can be divided into three areas. Developing and designing products and systems. Manufacturing products. Selling and servicing products.

This ad outlines the types of work found in the Sales and Service area of GE. Other ads in this series will cover the two remaining areas.

We also have a handy guide that explains all three areas. For a free copy, just write: General Electric, Dept. AK-3, 570 Lexington Ave., New York, New York 10022.

Sales Engineering

Sales engineering is technical marketing. Sales engineers at GE are the important liaison between GE manufacturing facilities and utility, industrial, distributor and governmental customers. Working closely with assigned customers, they use their technical background to recognize customer needs and recommend GE products or systems to fill them. From small AC motors to huge turbine-generator units. Requires a thorough understanding of a customer's business, as well as a wide range of GE products. Plus the ability to work well with people and to recognize a good business opportunity.



Application Engineering

Application engineers are technical experts who work closely with the sales engineer and the customers' engineers. Their job is to analyze special problems and equipment needs of customers, then determine the optimum GE products or systems to meet them. There are two kinds of application engineers. The first works out of a sales operation and is adept at applying a wide variety of products to create a "system" that meets the customers' needs. The second works in a product manufacturing department and is a specialist at applying the products of that one department. Both must have in-depth knowledge of the customers' technical needs. They often consult with product planners and other marketing personnel to suggest ideas for new or modified products.

Field Engineering

Field engineers at GE plan and supervise the installation and service of large equipment systems worldwide in two main customer areas. Power generation and delivery equipment for utilities. And heavy apparatus for industrial customers such as paper and steel mills, chemical plants and machine tool manufacturers. They specialize in either the mechanical/nuclear or electrical/electronic areas. Since field engineers are often called to troubleshoot and correct a customer equipment problem, it requires the technical competence and creative ability to handle the different, the difficult and the unexpected. Plus the ability to take charge, lead people, and make independent, on-the-spot decisions.

Product Planning

Product planning is a marketing function. Product planners make sure a product line offers what customers need at competitive prices. They determine the need for a new or modified product, product availability, market size, cost structure, profitability, specifications and distribution channels. To do this, they work with market researchers, application and sales engineers, finance experts, marketing management, plus design and manufacturing engineers. Their engineering background is a big plus. This work requires self-starters who can coordinate a project and sell their ideas to management.

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