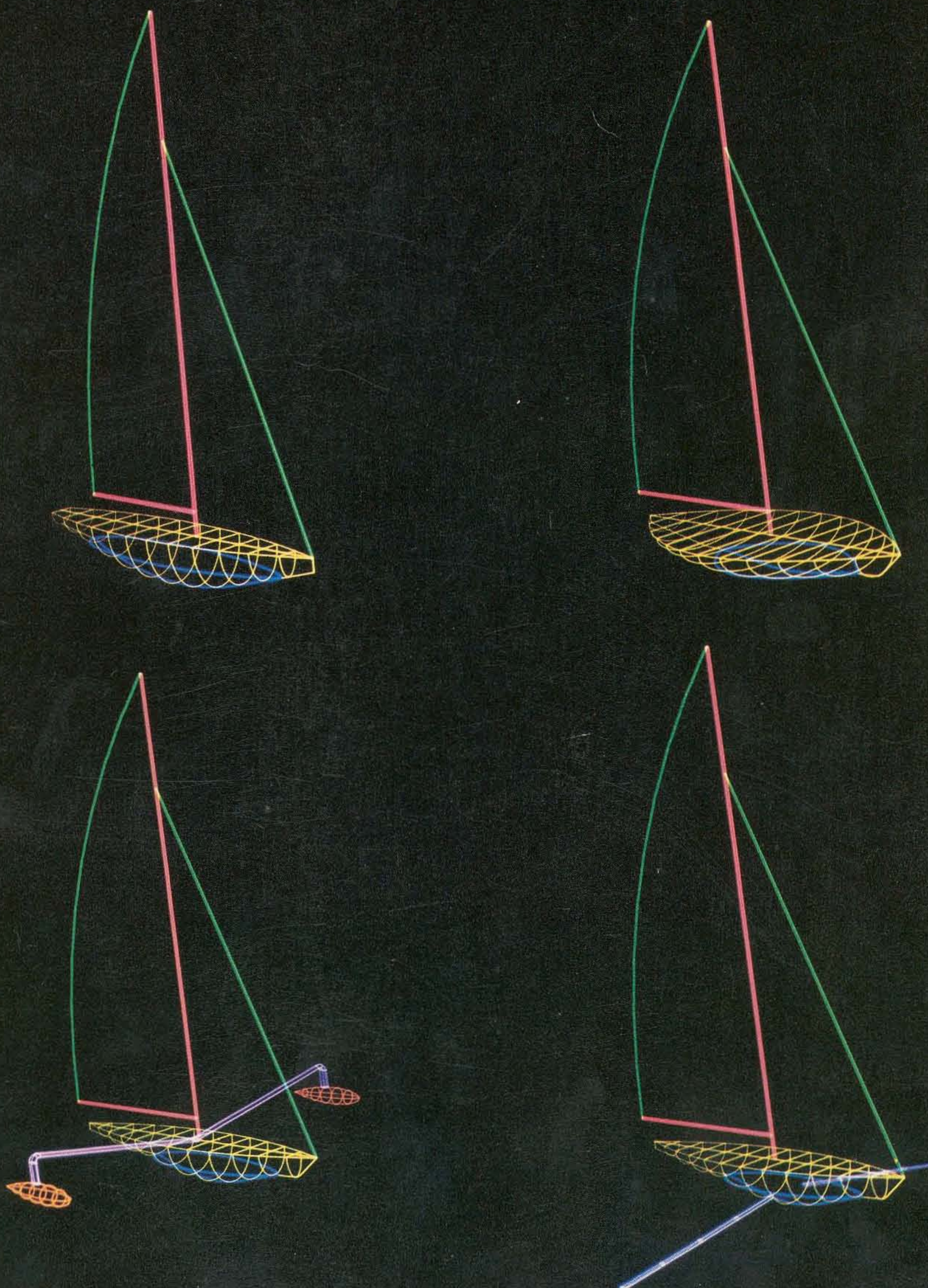
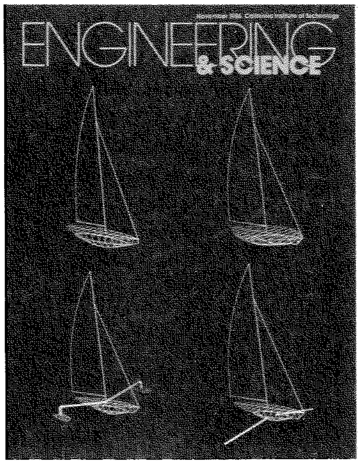
November 1986 California Institute of Technology

ENGINEERING & SCIENCE

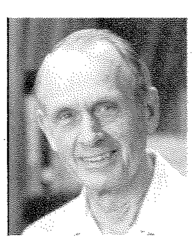


In This Issue



Hot Yachts
On the cover — Computer graphics compare a conventional 12-meter yacht (top left), such as those currently competing for the America's Cup in Australia, with three innovative alternatives to the yacht's heavy keel, which normally provides upright stability. In his article, "The Boat That Almost Was," which begins on page 2, Francis Clauser explains why solutions such as a broad-beamed hull (top right), pontoons on outriggers (lower left), or underwater ailerons (lower right) could make an America's Cup yacht go faster.

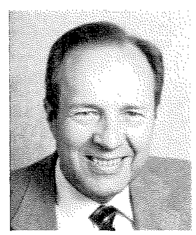
Clauser, the Clark Blanchard Millikan Professor of Engineering, Emeritus, readily admits that he's not a naval architect. But as a participant in the aeronautics revolution in the 1930s, he has retained an interest in applying scientific solutions to problems previously left to intuition. His reputation reached the sailing world, and he was recruited by the *Eagle* syndicate to be chief scientist in the Newport Harbor Yacht Club's effort to win the America's Cup.



Clauser earned all his degrees from Caltech, his PhD in 1937 under Theodore von Kármán. After spending the war years at Douglas Aircraft Company, he established the aeronautics department at the Johns Hopkins University in 1946. He remained there until 1965, when he took the post of academic vice chancellor at UC Santa Cruz. He finally returned to Caltech in 1969 as chairman of the Division of Engineering and Applied Science. Clauser became professor emeritus in 1980.

The computer graphics of Clauser's ideas on the cover and in the article were created by Bob Bolender, a first-year grad student in mechanical engineering. He programmed the designs in Caltech's Engineering Design Research Laboratory, which is under the direction of Erik Antonsson, assistant professor of mechanical engineering, who took the pictures.

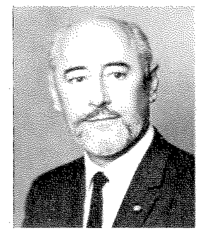
Capital Ideas
At last year's Research Directors Conference, sponsored by Caltech's Industrial Associates, William J. Perry delivered an enthusiastically received keynote address on "Entrepreneurship and Advanced Technology."



Perry, who earned his MS from Stanford and PhD from Penn State (1957), is a former Undersecretary of Defense for Research and Engineering and is currently president of H&Q Technology Partners, Inc., in Menlo Park, California. Before entering government service he helped found ESL, Inc., and worked with Sylvania/GTE.

An article adapted from his address begins on page 14. Information about the 1987 Research Directors Conference can be found on page 32.

Jet Start
On October 31, 1936, near the future site of the Jet Propulsion Laboratory, the jet age was born. On that date, just 50 years ago, Caltech graduate student Frank J. Malina and his cohorts conducted the first test-firing of a liquid-fueled rocket motor.



Malina went on to co-found JPL with Theodore von Kármán. In 1946 he left Caltech for a job with UNESCO in Europe. In his later years he settled in Paris and devoted most of his time to art — he was a pioneer in the kinetic art movement and the founder of *Leonardo*, an influential art journal. Malina died in 1981.

"The Rocket Pioneers," which begins on page 8, is adapted from an article Malina wrote for *E&S* in 1968, with the addition of some material from the oral history he gave to the Caltech Archives. (A separate article on the Archives can be found on page 19.)

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The Boat That Almost Was

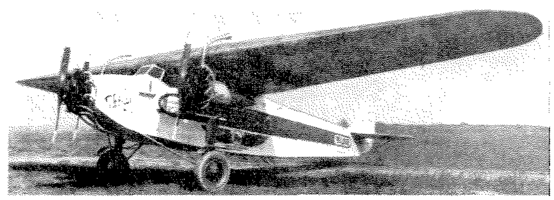
by Francis H. Clauser

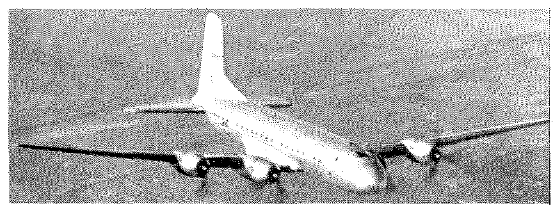
Above: Eagle, *currently competing among the America's Cup challengers in the waters off Perth, was built by the Newport Harbor Yacht Club as their conventional 12-meter yacht.*

In the late summer of 1984 Chuck Newton approached me at lunch in the Athenaeum and asked if I would be willing to meet a man who wanted to talk to me about the America's Cup. That man turned out to be George Tooby, BS '35 — almost a classmate of mine — who proposed that I sign on with the Newport Harbor Yacht Club to bring aeronautical science into the design of its America's Cup challenger.

I had never designed boats before, but my interest had already been piqued by the 1983 America's Cup race of 12-meter yachts. Before that race a number of my sailor friends had assured me that the United States was sure to win the cup again, as it had consistently for the past 132 years. These friends said that we had the finest crews in the world and that furthermore, the art of 12-meter yacht design had reached a pinnacle of perfection in this country. When the Australians won with their winged keel, the question naturally arose in my mind: Is the design of these yachts as advanced as I had been led to believe? The Australian keel wasn't that radical. If this was really supposed to be the state of high technology in yacht design, I thought it might be interesting to see how it looked from an insider's point of view.

So I began to study the various America's Cup contenders over the years, particularly the 12-meter yachts that had been racing since the competition was resumed in 1958 following the hiatus of World War II. I was immediately struck by a parallel with my early experience in aeronautics. In the 1920s airplanes were designed mostly by intuition. When scientific knowledge was applied to airplane design in the 1930s, earlier intuition frequently turned out to have been grossly wrong. For example, pilots often complained that the early airplanes had problems with stability and control. Science in the 1930s showed that the tail surfaces (elevators and rudders) had been much too small.





The Fokker Trimotor 10-passenger transport (top) was typical of the intuitive designs of the 1920s. The Douglas Globemaster (bottom) was designed during World War II. It typifies the changes brought about by the revolution in aeronautical science during the 30s and 40s. The author designed the wing and tail sections on this airplane, and he holds the patents on the ailerons and flaps.

As I looked at the America's Cup yachts, my immediate reaction was that they must have had stability and control problems with their very small rudders located in the wake of both the hull and the keel. And it turned out that indeed a previous America's Cup skipper, Bill Ficker of *Intrepid* in 1970, claimed that "the present breed of 12 meters is very difficult to steer and keep 'in the groove.' *Intrepid's* biggest difficulties were experienced when tacking in light weather. It was not easy at all to get her moving again on the wind and to regain the speed of the previous tack." Other helmsmen said the yachts needed constant control to keep from yawing in moderate weather. Comments from the designers of these yachts make it clear that they didn't know how to solve this serious problem. It was at this point that I concluded that the design of 12-meter yachts probably had not reached the peak of scientific perfection that everyone seemed to think.

Ever since a group of New York millionaires first challenged the British yacht clubs in 1851, the America's Cup has been a race not between nations but between yacht clubs. It was set up as a series of match races every three years between a single defender and a single challenger. The defenders and the challengers would hold their own series of eliminations to choose one boat to represent them. But there are different rules for each side. The winner of the challengers' elimination races would represent its home yacht club, while the defender must sail under the colors of the yacht club that holds the cup — until 1983 the New York Yacht Club. This had been a galling experience for all the other American yacht clubs, particularly since the New York Yacht Club had grown almost insolent about its superior position in the race. Australia's winning of the cup made it possible for other American yacht clubs to dream of winning the cup. At present a total of 13 yachts are vying for the opportunity to challenge Australia in the waters off Perth — one each from England, Canada, and New Zealand, two each from France and Italy, and six from the United States.

Figure 1: The 12-meter yacht Intrepid *was typical of the America's Cup contenders of the 1960s and 70s. These yachts experienced the same type of stability and control problems that airplanes of the 1920s did.*

When George Tooby decided that it would be appropriate for the Newport Harbor Yacht Club to enter a challenger in the next America's Cup race, he formed the money-raising *Eagle* syndicate and hired Johan Valentijn as Newport Harbor's chief designer. Valentijn, a young, Dutch naval architect, had designed *Liberty*, the unsuccessful American defender that had lost to the Australians in the 1983 cup race, as well as an earlier contender, *Magic*, for the New York Yacht Club. In the fall of 1984 I (as chief scientist) joined forces with Valentijn to build one-third size models of *Liberty* and *Magic* (the latter having been purchased by the *Eagle* syndicate) — to be tested in the large towing tank in Escondido. This was to establish a database of known characteristics of these two yachts so that we could compare future models with these two well-known quantities.

The *Eagle* syndicate had laid out a program to build two yachts, for which their fund raising goal was \$8-10 million. The first of these was to be a conventional yacht, incorporating whatever improvements could be made on *Magic* and *Liberty*. The second was to be as radical and innovative as we were capable of making it. Valentijn and I agreed that he would spend almost all of his time on the conventional yacht, and I would devote most of my effort to the more radical second yacht.

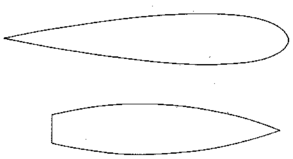


Figure 2: The airfoil or airship (top) is the aerodynamicist's concept of a well-streamlined shape. In contrast, the typical boat shape (bottom) is designed to travel the other way around.

Early in my work with Valentijn I was struck by the great difference in our perceptions of what constitutes a good streamlined shape. Both theory and experiment long ago led aerodynamicists to conclude that the typical airfoil or airship shape shown in Figure 2 has the least resistance. Also shown in the figure is the typical shape associated with boats. Any tendency to see a similarity between them is dispelled when one realizes that in one case the pointed end is forward and in the other it is backward. When we tested the model of *Magic*, I persuaded Valentijn to tow it backward as well as forward. The results, shown in Figure 3, were a shocking revelation to Valentijn. I kidded him by saying the reason we lost the cup was that we were sailing the wrong way around.

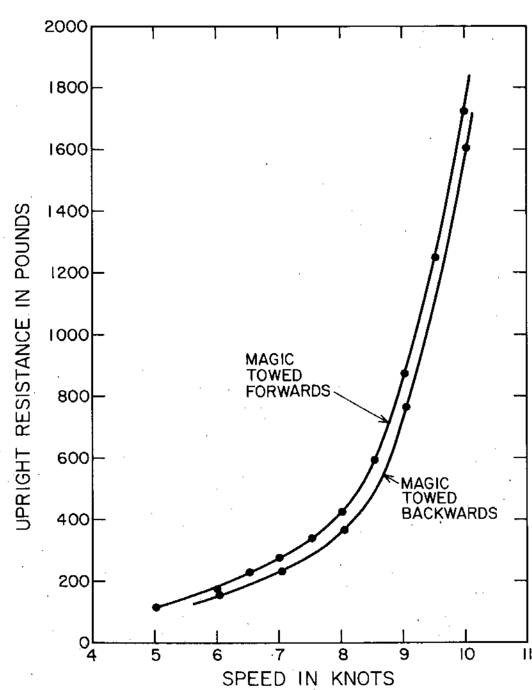


Figure 3: Towing tank tests on Magic's *hull (without keel and rudder) showed that it had less resistance when towed backwards rather than forwards.*

Unfortunately, his conventional boat ended up still sailing the wrong way around in my opinion.

What *is* a 12-meter yacht? For an America's Cup designer, this always looms as a key question, since it defines the limits to his innovation and creativity. As originally laid down, the rules were intended to be sharply and clearly defined. As the rules are written, two quite different lengths are involved. One of these lengths, LWL, is the length on the waterline from the bow to the stern (or to the rudder post if this is farther aft), when the yacht is afloat without a crew. This length has nothing to do with the 12-meter rating but serves to limit the yacht's displacement and draft. The displacement must not be less than the volume given by

$$\left(\frac{\mathrm{LWL} + 0.75}{5}\right)^3$$

all in meters. Otherwise the yacht is penalized in its rating by double the deficiency. The draft must not exceed 16 percent of the LWL plus 0.5 meters. Here the penalty is three times the excess. The second length, L, is directly involved in the 12-meter rating. It enters the all-important formula

$$\text{Rated length} = 12 \text{ meters} = \frac{L + 2d - F + \sqrt{S}}{2.37}$$

Here d is the girth difference, F is the freeboard, and S is the sail area, all in meters. (I do not know the origin of the factor 2.37; without it, these would be 28.44-meter yachts.)

The definition of L is a study in complexity. Imagine a plane 0.18 meters (7 in.) above the waterplane. This defines new bow and stern points, and the first part of L is the distance between these points. The second is a girth measurement from a point on one side of the bow 0.6 meters above the original waterline, around and under the bow, and up to the corresponding point on the other side. From this is subtracted 1.2 meters, and the result is multiplied by 1.5 to give the second part of L. The third is a corresponding measurement at the stern, except that here the multiplying factor is 0.33. The forward girth measurement (when multiplied by 1.5) has a minimum value of 0.54 meters, and if the actual value is less than this, the minimum value must be used. The aft girth measurement similarly has a minimum value of 0.4 meters. In practice most yachts have measurements close to these minimums.

The girth difference measurement, d in the formula, is taken at the keel. It's the difference in length between a chain following along the hull from a point "a" above the waterline down along the keel to a point 1.5 meters below the waterline, and the length of a chain stretched tight between these same two points (Figure 4). The freeboard, F, is an average distance from the deck to the water plane. Since it is subtracted in the formula, it encourages the designer to build high decks. There is a maximum value of 1.21 meters permitted for F, however, and most designers use this maximum value. When L, d, and F have been determined, the sail area must then be chosen so as to give the yacht a 12-meter rating, that is, to fit the formula. If we add the requirements that the beam must be at least 3.6 meters, the height of the mast must not exceed 25 meters, and the height of the jib must not exceed 18.75 meters, we have the basic answer to the question: What is a 12-meter yacht?

There are other subtle but important restrictions, however. One is that there be no hollows in the hull between the waterline and the deckline except in the region near the rudder — a rule that excludes catamarans and other multi-hull craft. A second restriction states that below 1.7 meters under the water plane, no width can exceed 3.6 meters. This was adopted to limit the span of the wings on Australian-type keels.

A third restriction requires construction in accordance with the "scantlings" established by Lloyd's Register of Shipping. These are an archaic set of construction specifications that almost completely rule out significant structural innovations. In effect they require that, regardless of a 12-meter yacht's total displacement, the hull minus its keel must weigh approximately 17,000 lbs.

And finally, for 12 meters competing in the America's Cup races, there is an additional rule that requires the length of the yacht at a plane 50 mm (2 in.) above the water plane to be at least 44 ft. long.

Given all the variations that are possible, why is it that almost all 12-meter yachts weigh close to 57,000 lbs. and have an overall length of 66 ft. and a waterline length of 46 ft.? To figure out why, we must first explore some of the fundamental facts of hydrodynamics so that we can understand why a good yacht goes fast. When a ship travels at low speeds, the principal resistance it encounters is from the skin friction of the water as it moves along the wetted surface and from the eddies off the stern that are created when the streamlines fail to close in behind the ship. These resistances increase roughly as the square of the ship's speed.

At higher speeds wave resistance also comes strongly into play. At first many small waves form; as the speed increases, the waves increase in both length and height, and the pattern becomes less complex. Finally there comes a speed for which there is simply a wave crest at the bow, a trough along the midship and a final crest at the stern. Spreading out from this wave pattern at the ship itself is a great train of waves extending out in a chevron to the rear. At higher speeds this wave train carries off large amounts of energy, resulting in a large increase in resistance of the ship. This wave resistance can become so great that it overshadows the frictional resistance (Figure 5).

It was an Englishman, William Froude, who in the last century showed that the wave pattern of a ship is governed by the dimensionless ratio $V/\sqrt{gL}$, that is, velocity divided by the square root of the acceleration of gravity times length. This important result enables us to predict the wave resistance of large ships from tests on smaller scale models. Another Englishman, Osborne Reynolds, also working in the last century, showed that fluid frictional resistance is governed by the dimensionless ratio VL/ν, that is, velocity times length divided by kinematic viscosity. Using

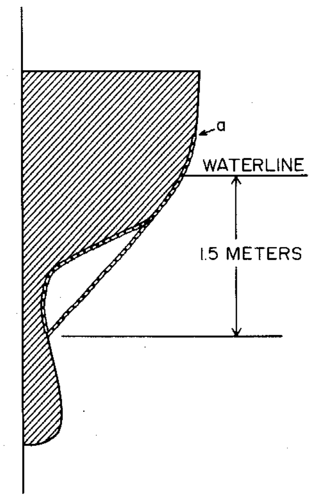


Figure 4: The chain midgirth measurement is the difference in length between a chain running from point "a" along the hull to a point 1.5 meters below the waterline and a chain stretched tight between those points.

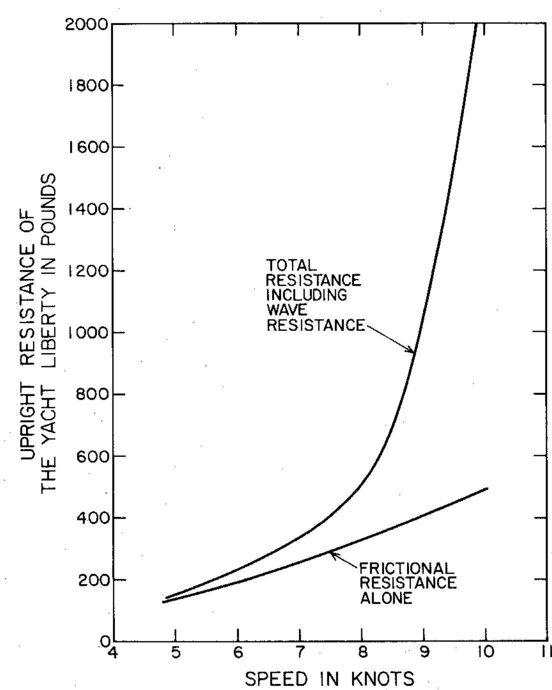


Figure 5 shows results from towing tank tests on a one-third-scale model of the 12-meter yacht Liberty. *Note how wave formation causes the total resistance to rise rapidly at higher speeds.*

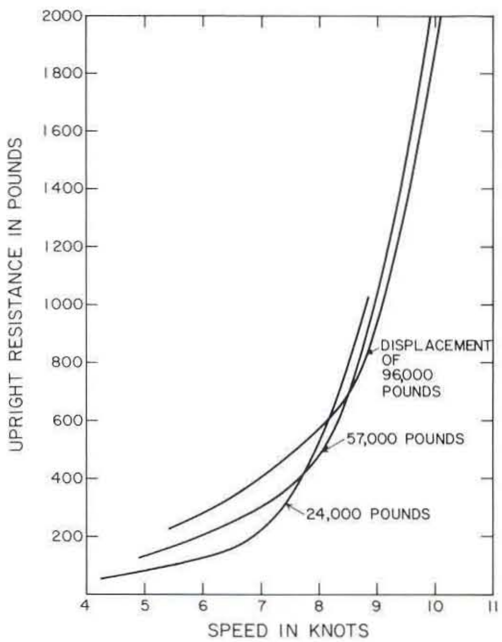


Figure 6: Towing tank data have been scaled up to show the effect of size on the resistance of typical 12-meter yachts.

this result we can scale up the fluid friction of a model to that of the full-sized ship.

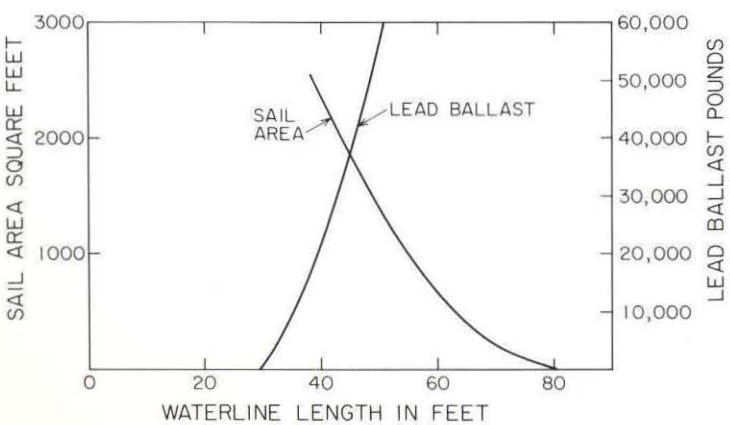
The first question that faces the 12-meter designer is: How heavy should the boat be? If *Liberty* were to be built in three sizes, 24,000, 57,000, and 96,000 lbs., with waterline lengths of 33.6, 45.6 and 54.8 ft. respectively, how would their resistances compare? Using our towing tank data and the scaling laws described above, we can calculate the resistance curves for these three yachts (Figure 6). At low speeds the lightest boat, being smaller, naturally has the least resistance. But at higher speeds the picture changes. For the longer boats the rapid rise in wave resistance does not begin until proportionally higher speeds are reached. Consequently their total resistance is lower at high speeds, where they enjoy a superiority that progressively gets better with length.

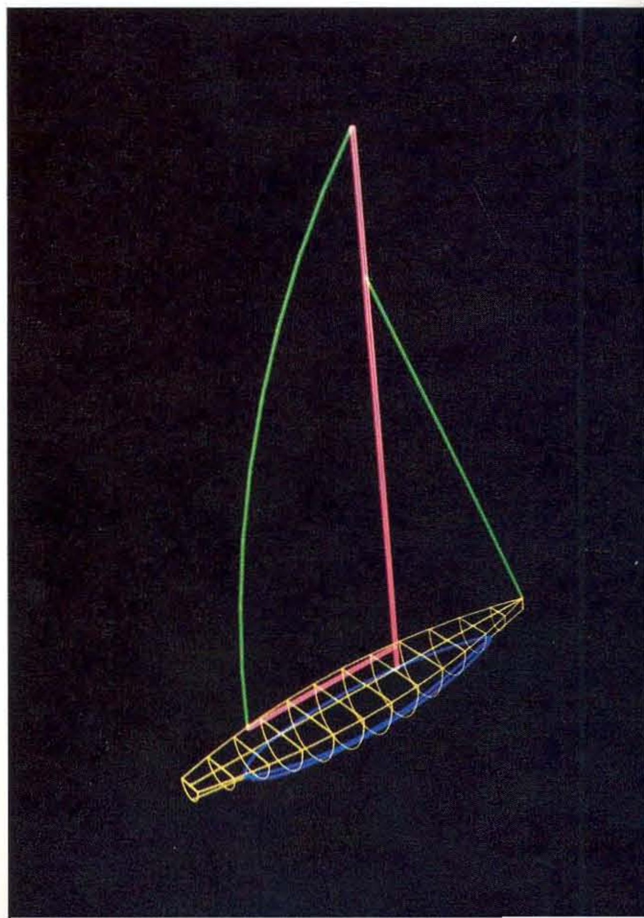
In Perth the winds are expected to be strong, and in most of the races the yachts will be pushed right up into this high speed range. So it would seem that designers of 1986-87 America's Cup yachts should strive for the greatest length they can get. And indeed, the first models that Valentijn and I built after our benchmark tests were larger, heavier versions of *Liberty*.

But if we look back at the 12-meter rules, we see that as the size of the hull (length, girth, and freeboard) goes up, the area of the sail must come down. For a typically proportioned hull, this leads to the relationship of sail area to length shown in Figure 7. Clearly, under the 12-meter rule the 96,000-lb. yacht with its 54.8 ft. of length would be required to have a much smaller sail. This result seems to indicate that, instead of a longer, larger hull to gain the advantage of high speed, the designer should be aiming for the smallest boat possible to have the greatest amount of sail area.

The hull must, however, be constructed in accordance with Lloyd's scantlings, which specify that the hull structure (minus keel) shall weigh no less than approximately 17,000 lbs. regardless of the displacement of the yacht. The 57,000-lb. yacht will thus be able to have 40,000 lbs. of lead in her keel, while the 24,000-lb. boat can have only 7,000 lbs. In a 12 meter the principal factor that enables the yacht to stand up in heavy winds is the enormous amount of lead ballast down in the

Figure 7: Under 12-meter rules, as the hull of a conventional yacht increases in size, the sail area must be decreased. The lead ballast permitted for upright stability goes up with length, leading to a design dilemma.

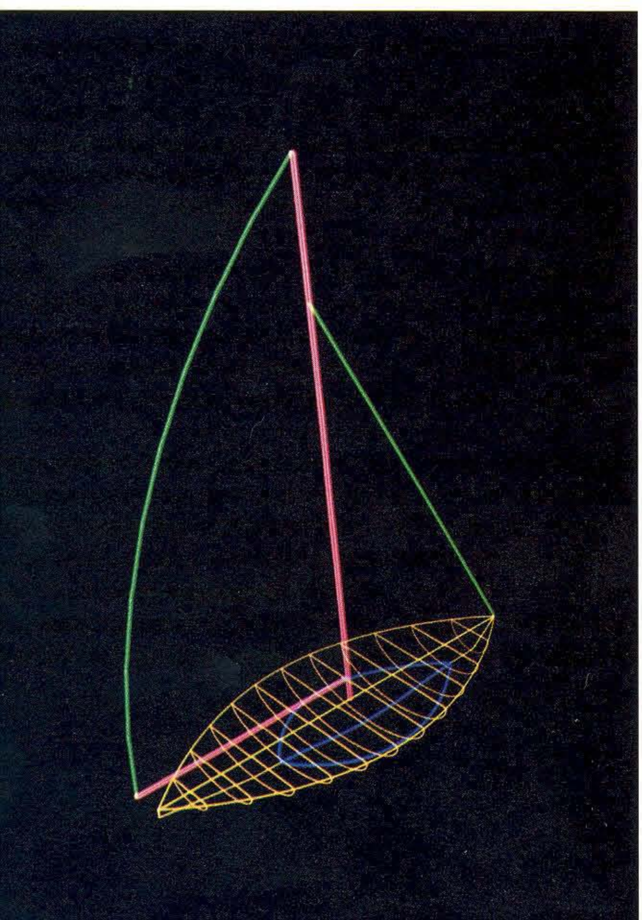




keel. In fact, all 12 meters have keels much larger than hydrodynamics alone would dictate, simply to house the lead. Our tank tests showed that the great advantage of the Australian winged keel lay not in the hydrodynamic properties of the wings themselves, but in the fact that the shape of the keel and the wings permitted the center of gravity of the lead to be significantly lower.

Figure 7 also shows the amount of lead ballast permitted in 12-meter yachts of various sizes. This now puts the designer's task in sharp focus. If he designs a small hull, it can have large sails, but even light winds will blow the boat over because of its small righting moment. In contrast, a large hull will have a very large upright stability, but it will be permitted to use only small sails. These opposing constraints dictate that most 12-meter yachts end up with waterline lengths of 43 to 48 ft. and with gross weights of 50,000 to 65,000 lbs.

It was in the fall of 1985 that the design of Valentijn's conventional yacht *Eagle*, was completed, and my ideas about a radically new boat began to crystallize. In international offshore racing a new class of fast ultra-light boats has been sweeping the field. How do they do it? There are several ways of gaining upright stability other than by placing lead in the keel, and ultra-lights do it by using broad beams with shallow-draft hulls. In spite of the success of this idea, it doesn't seem to have occurred to 12-meter designers to explore this avenue.



What would a 12 meter look like as an ultra-light? The conventional 12 meter with a gross weight of 57,000 lbs. has a righting moment of about 160,000 ft.-lbs. when it is heeled 30°. The keel with its 40,000 lbs. of lead provides about 145,000 ft.-lbs. of this. The remainder is made up of an unstable moment of about 38,000 ft.-lbs. from the hull, mast, rigging, and sails, and a stable hydrostatic moment of about 53,000 ft.-lbs. provided by a waterline beam of approximately 11 ft.

An ultra-light with a gross weight of 24,000 lbs. (less than half that of the conventional 12 meter) would have a length of 33.6 ft. compared to the 45.6 ft. of the conventional hull. To comply with the America's Cup rule, it would have to have a length of 44 ft. at a height 2 inches above the waterline. So it would have long, nearly horizontal overhangs both fore and aft.

One purpose of designing an ultra-light is to be able to use much greater sail area. My calculations indicated that to be able to stand up in the wind, it would need 40 percent more righting moment, that is 160,000 × 1.40, or 224,000 ft.-lbs. when it heels 30°. Here the righting moment is to come from a broader beam, rather than from a heavy keel. This will require a waterline beam of 20 ft. rather than the conventional 11 ft. Being much lighter and broader of beam, the draft of the lightweight hull at 1.5 ft. will be much shallower than the usual 4.5 ft. Entering the dimensions of such an ultra-light hull into the 12-meter formula gives a sail area of approximately 2,450 sq. ft., which is much greater than the typical 1,750 sq. ft. for 12 meters. Referring back to Figure 6, we see that the lightweight hull has the advantage of significantly less resistance at low speeds, and the larger sail area would much more than make up for its greater resistance at higher speeds.

It has been found that lightweight, broad-beamed boats can readily get up and plane like a surfboard on the forward face of a wave. They frequently reach speeds of 14 to 18 knots. The reason for this is simple. For a planing hull the resistance, instead of rising sharply at high speeds as shown in earlier

continued on page 26

Figure 8 (far left) shows a conventional 12 meter in computer graphics created by Bob Bolender. Figure 9 (to its right) depicts a broad-beamed, lightweight hull, which is permitted to carry a much larger sail. It can plane like a surfboard.

The Rocket Pioneers

by Frank J. Malina

The late Frank J. Malina inaugurated the jet age 50 years ago with the construction and test-firing of the first liquid-fueled rocket motor in the Arroyo Seco. In this article, adapted from one he wrote for E&S *in 1968, he describes those exciting early days of rocketry at Caltech.*

My interest in space exploration was first aroused when I read Jules Verne's *De la Terre a la Lune* in the Czech language as a boy of 12 in Czechoslovakia, where my family lived from 1920 to 1925. On our return to Texas I followed reports on rocket work which appeared from time to time in popular magazines.

In 1934 I received a scholarship to study mechanical engineering at Caltech. Before the end of my first year there I began part-time work as a member of the crew of the GALCIT (Guggenheim Aeronautical Laboratory, California Institute of Technology) 10-foot wind tunnel. This led to my appointment in 1935 as a graduate assistant in GALCIT.

The Guggenheim laboratory at this time, a few years after its founding, was recognized as one of the world centers of aeronautical instruction and research. Under the leadership of Theodore von Kármán, GALCIT specialized in aerodynamics, fluid mechanics, and structures. Von Kármán's senior staff included Clark B. Millikan, Ernest E. Sechler, and Arthur L. Klein. The laboratory was already carrying out studies on the problems of high-speed flight, and the limits of the propeller-engine propulsion system for aircraft were beginning to be clearly recognized.

In 1935-36 William W. Jenney and I conducted experiments with model propellers in the wind tunnel for our master's theses. My mind turned more and more to the possibilities of rocket propulsion while we analyzed the characteristics of propellers.

In March 1935 at one of the weekly GALCIT seminars, William Bollay, then a graduate assistant under von Kármán, reviewed the possibilities of a rocket-powered aircraft based upon a paper published in December 1934 by Eugen Sänger, who was then working in Vienna. The following October Bollay gave a lecture on the subject before the Institute of the Aeronautical Sciences in Los Angeles.

Local newspapers reported on Bollay's lecture, which resulted in attracting to GALCIT two rocket enthusiasts — John W. Parsons and Edward S. Forman. Parsons was a self-trained chemist who, although he lacked the discipline of a formal higher education, had an uninhibited and fruitful imagination. He loved poetry and the exotic aspects of life. Forman, a skilled mechanic, had been working for some time with Parsons on powder rockets. They wanted to build a liquid-propellant rocket motor but found that they lacked adequate technical and financial resources for the task. They hoped to find help at Caltech. They were sent to me, and that was the beginning of the story which led to the establishment of the Jet Propulsion Laboratory.

We reviewed the literature published by the first generation of space flight pioneers — Ziolkowsky, Goddard, Esnault-Pelterie, and

Oberth. In scientific circles this literature was generally regarded as science fiction, primarily because the gap between the experimental demonstration of rocket-engine capabilities and the actual requirements of rocket propulsion for space flight was so fantastically great. This negative attitude extended to rocket propulsion itself, in spite of the fact that Goddard realistically faced the situation by deciding to apply this type of propulsion to a vehicle for carrying instruments to altitudes in excess of those that can be reached by balloons — an application for an engine of much more modest performance.

We concluded from our review of the existing information on rocket-engine design that it was not possible to design an engine to meet specified performance requirements for a sounding rocket to surpass the altitudes attainable by balloons. After much argument, we decided that until someone could design a workable engine with a reasonable specific impulse there was no point in devoting effort to the design of the rocket shell, propellant supply, stabilizer, launching method, and payload parachute.

We therefore set as our initial program the following: (a) theoretical studies of the thermodynamical problems of the reaction principle and of the flight performance requirements of a sounding rocket; and (b) elementary experiments of liquid- and solid-propellant rocket engines to determine the problems to be met in making accurate statistical tests. This approach was in the spirit of von Kármán's teaching. He always stressed the importance of getting as clear an understanding as possible of the fundamental physical principles of a problem before initiating experiments in a purely empirical manner, which can be very expensive in both time and money.

Parsons and Forman were not too pleased with an austere program that did not include at least the launching of model rockets. They could not resist the temptation of firing some models with black powder motors during the next three years. Their attitude is symptomatic of the anxiety of pioneers of new technological developments. In order to obtain support for their dreams, they are under pressure to demonstrate them before

This photo, taken in the Arroyo Seco in 1936, shows the first liquid-fueled rocket motor and the men who built it. The man at the left is unidentified. The others are, left to right, Apollo M.O. Smith, Frank J. Malina, Edward S. Forman, and John W. Parsons.

they can be technically accomplished. Thus there were during this period attempts to make rocket flights which were doomed to be disappointing and which made support even more difficult to obtain.

The undertaking we had set for ourselves required, at a minimum, informal permission from Caltech and from the Guggenheim laboratory before we could begin. In March I proposed to Clark Millikan that my thesis be devoted to studies of the problems of rocket propulsion and of sounding rocket flight performance. He was, however, dubious about the future of rocket propulsion and suggested I should, instead, take one of the many engineering positions available in the aircraft industry at that time. His advice was no doubt also influenced by the fact that GALCIT was not then carrying out any research on aircraft power plants. Later he supported our work.

I knew that my hopes rested finally with von Kármán. He was at this time studying the aerodynamics of aircraft at high speeds and was well aware of the need for a propulsion system which would surmount the limitations of the engine-propeller combination. After considering my proposals for a few days, von Kármán agreed to them and gave permission for Parsons and Forman to work with me, even though they were neither students nor on the staff at Caltech. This decision was typical of his unorthodox attitude within the academic world. He pointed out, however, that he could not find funds.

During the next three years we received no pay for our work, and during the first year we bought equipment — some secondhand — with whatever money we could pool together. Most of our work was done on weekends or at night.

We began our experiments with the construction of an uncooled rocket motor similar in design to one that had been previously tried by the American Rocket Society. For propellants we chose gaseous oxygen and methyl alcohol.

Our work in the spring of 1936 attracted two GALCIT graduate students, A.M.O. Smith and Hsue Shen Tsien. Smith was working on his master's degree in aeronautics; Tsien, who became one of the outstanding pupils of von Kármán, was working on his doctorate. Smith and I began a theoretical analysis of flight performance of a sounding rocket, while Tsien and I began studies of the thermodynamic problems of the rocket motor.

The group heard with excitement that Robert H. Goddard would come to Caltech in August to visit Robert Millikan. Millikan arranged for me to have a short discussion with Goddard on August 28, during which I told him of our hopes and research plans. I also arranged to visit him at Roswell, New Mexico, the next month, when I was going for a holiday to my parents' home in Brenham, Texas.

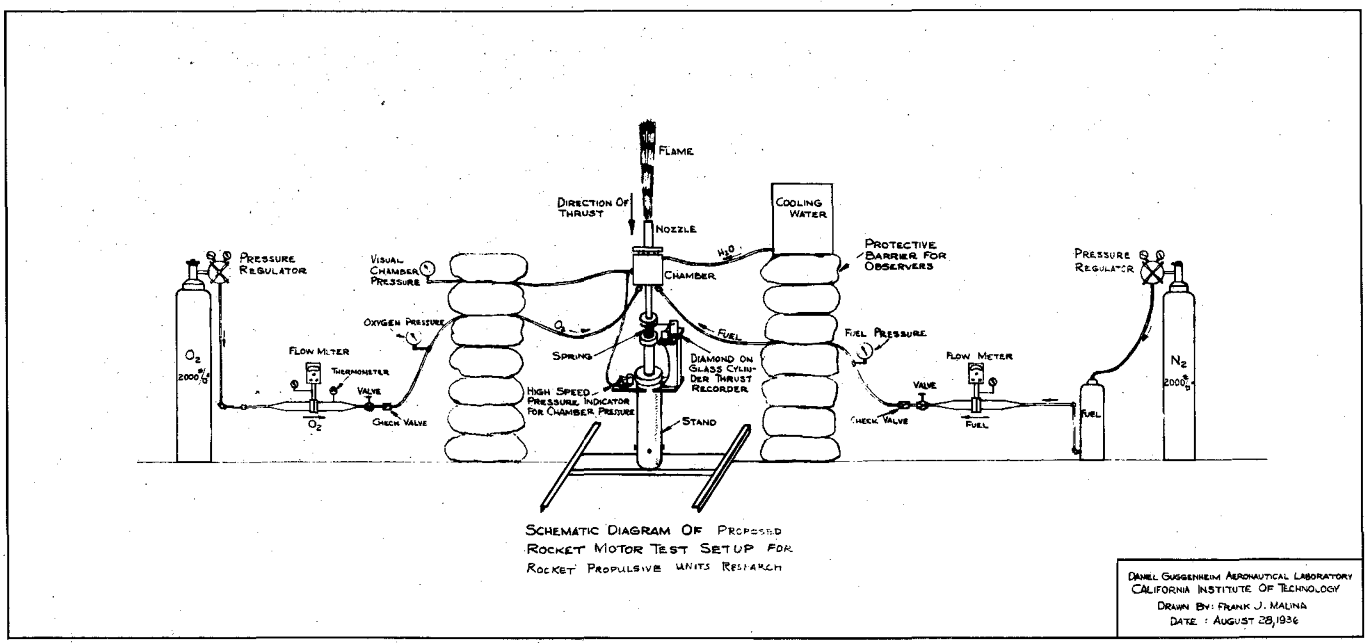
Both Dr. and Mrs. Goddard received me cordially. My day with him consisted of a tour of his shop (where I was *not* shown any components of his sounding rocket), a drive to his launching range to see his launching tower and 2,000-lb.-thrust static test stand, and a general discussion during and after lunch. He did not wish to give any technical details of his current work beyond that which he had published in his 1936 Smithsonian Institution report, with which I was already familiar. This report was of a very general nature and of limited usefulness to serious students.

The impression I obtained was that Goddard felt that rockets were his private preserve, so that any others working on them took on the aspect of intruders. He did not appear to realize that in other countries there were men who had arrived, independently of him, at the same basic ideas for rocket propulsion, as so frequently happens in technology.

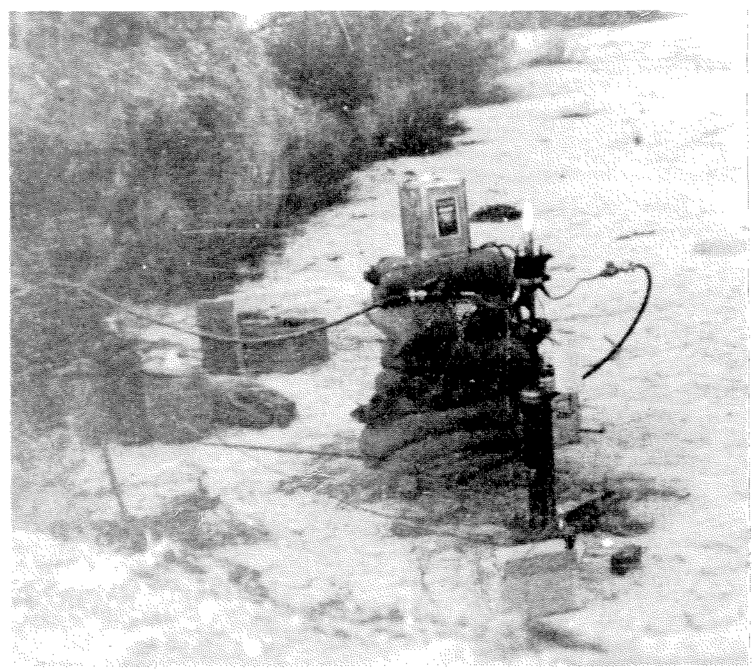
Von Kármán in his autobiography, *The Wind and Beyond*, writes:

> I believe Goddard became bitter in his later years because he had no real success with rockets, while Aerojet-General Corporation and other organizations were making an industry out of them. There is no direct line from Goddard to present-day rocketry. He is on a branch that died. He was an inventive man and had a good scientific foundation, but he was not a creator of science, and he took himself too seriously. If he had taken others into his confidence, I think he would have developed workable high-altitude rockets and his achievements would have been greater than they were. But not listening to, or communicating with, other qualified people hindered his accomplishments.

On October 29, 1936, the first try of the portable test equipment was made for the gaseous oxygen-methyl alcohol rocket motor



in the area of the Arroyo Seco back of Devil's Gate Dam on the western edge of Pasadena — a stone's throw from the present-day Jet Propulsion Laboratory. I learned several years later from Clarence N. Hickman that he and Goddard had conducted smokeless-powder armament rocket experiments at this same location during World War I. On October 31 we tested the rocket motor itself. The next day I wrote home as follows:



The rocket motor fires.

> This has been a very busy week. We made our first test on the rocket motor yesterday. It is almost inconceivable how much there is to be done and thought of to make as simple a test as we made. We have been thinking about it for about six months now, although we had to get all the equipment together in two days, not by choice, but because there are classes, and hours in the wind tunnel to be spent. Friday we drove back and forth to Los Angeles picking up pressure tanks, fittings, and instruments. Saturday morning at 3:30 a.m. we felt the setup was along far enough to go home and snatch three hours of sleep. At 9 a.m. an Institute truck took our heaviest parts to the Arroyo, about three miles above the Rose Bowl, where we found an ideal location. Besides Parsons and me, there were two students working in the N.Y.A. working for us. It was 1 p.m. before all our holes were dug, sandbags filled, and equipment checked. By then Carlos Wood and Rockefeller had arrived with two of the box type movie cameras for recording the action of the motor. Bill Bollay and his wife also came to watch from behind the dump.
>
> Very many things happened that will teach us what to do next time. The most excitement took place on the last "shot" when the oxygen hose for some reason ignited and swung around on the ground, 40 feet from us. We all tore out across the country wondering if our check valves would work. Unfortunately, Carlos and Rocky had to leave just before this "shot" so that we have no record on

film of what happened. As a whole the test was successful.

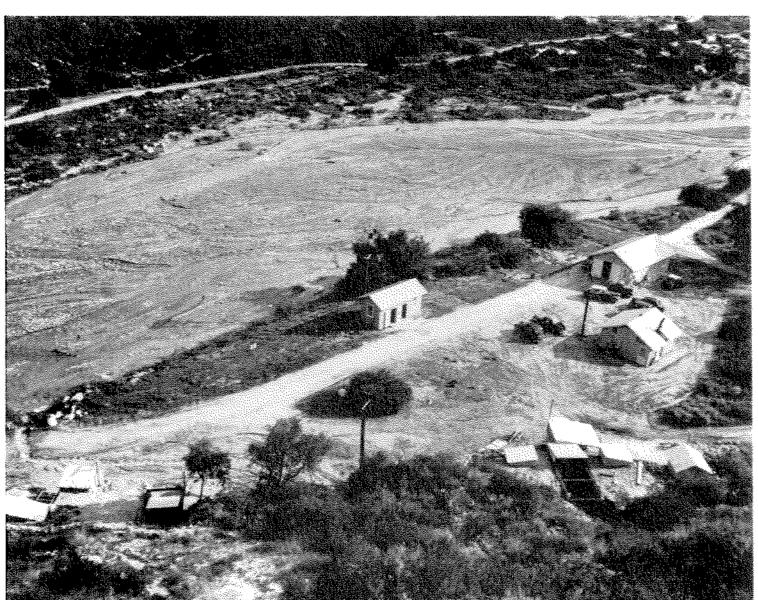
We made a number of tests with this transportable setup, the last one on January 16, 1937, when the motor ran for 44 seconds at a chamber pressure of 75 lbs. per square inch.

In March 1937 Smith and I completed our analysis of the flight performance of a constant-thrust sounding rocket. The results were so encouraging that our project obtained from von Kármán the continued moral support of GALCIT. We were authorized to conduct small-scale rocket motor tests in the laboratory. This permitted us to reduce the time we wasted putting up and down the transportable equipment we had used in the Arroyo Seco. Von Kármán also asked me to give a report on the results of our first year's work at the GALCIT seminar at the end of April.

The unexpected result of the seminar was the offer of the first financial support for our project. Weld Arnold, then an assistant in the astrophysical laboratory at Caltech, came to me and said that in return for being permitted to work with our group as a photographer he would make a contribution of $1,000. His offer was accepted with alacrity, for our project was destitute.

Arnold, who commuted the five miles betweeen Glendale and Caltech by bicycle, brought the first $100 for our project in one-dollar and five-dollar bills in a bundle wrapped in newspaper. We never learned how he had accumulated them. When I placed the bundle on Clark Millikan's desk with the question, "How do we open a fund at Caltech for our project?" he was flabbergasted.

In 1942 the Jet Propulsion laboratory consisted of just a few buildings in the Arroyo Seco, close to the site of the original rocket-motor firing. This location is at the eastern edge of the grounds of today's JPL.



When von Kármán gave the group permission to make small-scale experiments of rocket motors at GALCIT, we decided to mount a motor and propellant supply on a bob of a 50-foot ballistic pendulum, using the deflection of the pendulum to measure thrust. The pendulum was suspended from the third floor of the laboratory with the bob in the basement. We planned to make tests with various oxidizer-fuel combinations.

We selected the combination of methyl alcohol and nitrogen dioxide for our initial try. Our first mishap occurred when Smith and I were trying to get a quantity of the nitrogen dioxide from a cylinder we had placed on the lawn in front of Gates Chemistry Laboratory. The valve on the cylinder jammed, causing a fountain of the corrosive liquid to erupt all over the lawn. This left a brown patch there for several weeks, to the irritation of the gardener.

When we finally tried an experiment with the motor on the pendulum, there was a misfire. The result was that a cloud of nitrogen dioxide-alcohol mixture permeated most of GALCIT, leaving behind a thin layer of rust on much of the permanent equipment of the laboratory. We were thrown out of the building the next day, of course.

Then we built what we called the gas apparatus on the outside of Guggenheim. It must have been about 1939 when that blew up. It's quite possible that I might have been done in by that explosion, but von Kármán had called his secretary and asked if I would bring him a typewriter at home. I hopped in my Model A Ford, put the typewriter in it and drove to his house. When I came back I saw many people standing around. As I came closer and closer to the end of the building, I began to see pieces of the apparatus on the ground, and I realized something terrible had happened. Fortunately, neither Parsons nor Forman was hurt; they were shaken up a bit. But where I had been sitting before I left, a piece of a pressure gauge had blown right across where my head would have been and buried itself in a piece of wood. It was at this time that we began to be known on the campus as the "suicide squad."

From the beginning the work of the group on rocket research at GALCIT attracted the attention of newspapers and popular scientific journals. Since our work was not then classified as "secret," we were not averse to discussing with journalists our plans and results. There were times that we were abashed by the sensational interpretations given of our work, for we tended to be, if anything, too conservative in our estimates of its implications.

The fact that our work was having a real impact in America came from two sources. In May 1938 von Kármán had received an inkling that the U.S. Army Air Corps was getting interested in rocket propulsion.

Then, in August 1938, Ruben Fleet, the president of the Consolidated Aircraft Co. of San Diego, approached GALCIT for information on the possibility of using rockets for assisting the takeoff of large aircraft, especially flying boats. I went to San Diego to discuss the matter and prepared a report entitled "The Rocket Motor and its Application as an Auxiliary to the Power Plants of Conventional Aircraft." I concluded that the rocket engine was particularly adaptable for assisting the takeoff of aircraft, ascending to operating altitude, and reaching high speeds. The Consolidated Aircraft Co. appears to have been the first American commercial organization to recognize the potential importance of rocket-assisted aircraft takeoff. But in October 1938 a senior officer of the U.S. Army Ordnance Division paid a visit to Caltech and informed our group that on the basis of the Army's experience with rockets he thought there was little possibility of using them for military purposes! It was not until 1943 that liquid-propellant rocket engines, constructed at Aerojet-General Corporation, were tested in a Consolidated Aircraft flying boat on San Diego Bay.

In December 1938 I was informed by von Kármán, Robert Millikan, and Max Mason that I was to go to Washington, D.C., to give expert information to the National Academy of Sciences Committee on Army Air Corps Research.

One of the subjects on which General H.A. Arnold, then Commanding General of the Army Air Corps, asked the Academy to give advice was the possible use of rockets for the assisted takeoff of heavily loaded aircraft. I prepared a report which contained the following parts: (1) Fundamental concepts, (2) Classification of types of jet propulsors, (3) Possible applications of jet propulsion in connection with heavier-than-air craft, (4) Present state of development of jet propulsion, and (5) Research program for developing jet propulsion.

The word "rocket" was still in such bad repute in "serious" scientific circles at this time that it was felt advisable by von Kármán and myself to follow the precedent of the Army Air Corps of dropping the use of the word. It did not return to our vocabulary until several years later, by which time the word "jet" had become part of the name of our Laboratory (JPL) and of the Aerojet-General Corporation.

I presented my report to the committee on December 28, 1938, and shortly thereafter the Academy accepted von Kármán's offer to study with our GALCIT rocket research group the problem of the assisted takeoff of aircraft on the basis of available information, and to prepare a proposal for a research program. A sum of $1,000 was provided for this work.

Parsons and Forman were delighted when I returned from Washington with the news that the work we had done during the past three years was to be rewarded by government financial support and that von Kármán would join us as director of the program. We could even expect to be paid for doing our rocket research!

Thus in 1939 the GALCIT Rocket Research Project became the Air Corps Jet Propulsion Research Project. In 1944 I prepared a proposal for the creation of a section of jet propulsion within the division of engineering at Caltech. It was decided that it would be premature to do so. Instead, von Kármán and I founded JPL. □

The first practical application of Malina's rocket work came sometime between August 6 and 23, 1941 with the first jet-assisted takeoff of an airplane. Malina is the man furthest to the right.

Entrepreneurship In Advanced Technology

by William J. Perry

WE LIVE IN AN AGE that I believe historians will call the Age of the Computer. The most profound technological revolution mankind has ever experienced is occurring right now, but because we are immersed in this age we take it for granted. The computer has transformed the way we work; it has transformed our companies, our industries, our economy, and our defense posture. Indeed, it is in the process of transforming society more dramatically and more rapidly that the Industrial Revolution did in the 19th century. This technological revolution is causing the same sort of turmoil and confusion in our ability to project the future as was the case with social and political revolutions of an earlier day.

Charles Babbage invented the "analytical engine" in England more than 150 years ago, but the computer was never realized in Babbage's lifetime. The enabling technologies had to be invented before the real power of the computer could be unleashed — first, the invention of electricity, then the invention of the transistor, and finally the invention of the integrated circuit 130 years later.

If the computer is the engine that drives this new technological revolution, the integrated circuits provide the fuel for that engine. That fuel has led to price/performance improvements in the computer of more than 20 percent per year for the last few decades. Such rapid and steady improvement is unprecedented and has led to the development not only of new products and new companies but of entire new industries. In fact, this technological revolution has provided the underpinnings for an economic revolution. In the 1960s and 1970s, at least, the United States not only provided leadership of this economic revolution but was also the primary beneficiary. If you look at the economic consequences, you might say that in the 19th century the new wealth in the United States came from the gold in our mountains; for the last few decades our wealth has come from the silicon in our valleys.

We would like, of course, to extrapolate this exciting past into the future, but some prophets have said that's not going to happen. Some postulate that the rapid pace of technological innovation, which has driven these economic changes forward, has flattened out and that the innovative phase of this revolution is over. Others believe that the technological revolution will continue, but the leadership of it will pass to other countries — to Japan or even to the Soviet Union — and that the U.S. will end up as a second-class technological power.

Will this technological and economic revolution continue? I believe that, not only will this revolution continue, it will accelerate. To characterize this by a number, I would say that in the next decade we can expect an improvement of about a hundred times in price-performance of computers. In the transportation field a hundred-fold improvement in performance (speed) represented a change from the horse and buggy to jet aircraft. In the same field, a hundred-fold improvement in price would require reducing the price of a $10,000 automobile to $100.

I think that the increase in density in integrated circuits, which has characterized this price/performance improvement in the last few decades, will continue for at least another 10 years. That is, we'll be going from geometries in integrated circuits of a few microns to a few tenths of a micron; this tenfold compression in linear dimension will result in about a hundred-fold increase in density. To the extent that the history of this industry is a valid predictor of the future, this increase in density will allow for approximately a hundred-fold decrease in price per bit or per transistor.

This will require a whole new set of enabling technologies, not the least of which will be a whole new class of lithographic and etching equipment. Those technologies are well in hand, and it will be a matter of a relatively few years before they are commercially introduced. This continuing compression of the density of integrated circuits, however, will lead to what has been commonly called "Moore's dilemma." Moore's dilemma states that the more transistors you put on a chip, the harder it becomes to design it.

The solution to Moore's dilemma is the development of very sophisticated design tools that can be used not only by professional integrated circuit designers but by systems engineers as well. This technology was pioneered at Caltech by Carver Mead, the Gordon and Betty Moore Professor of Computer Science. But it leads us to a new dilemma: As the number and specialization of computers proliferates, the next choke point comes in software.

The solution to that problem — the development of design tools for writing software — is also under way but is not as far advanced as design tools for integrated circuits. A whole new industry is forming for companies that are building software design tools. In certain classes of problems three- to five-fold improvement in productivity has already been demonstrated, and I think that by the end of this decade we will see software design tools that will allow an order of magnitude improvement in productivity in writing code.

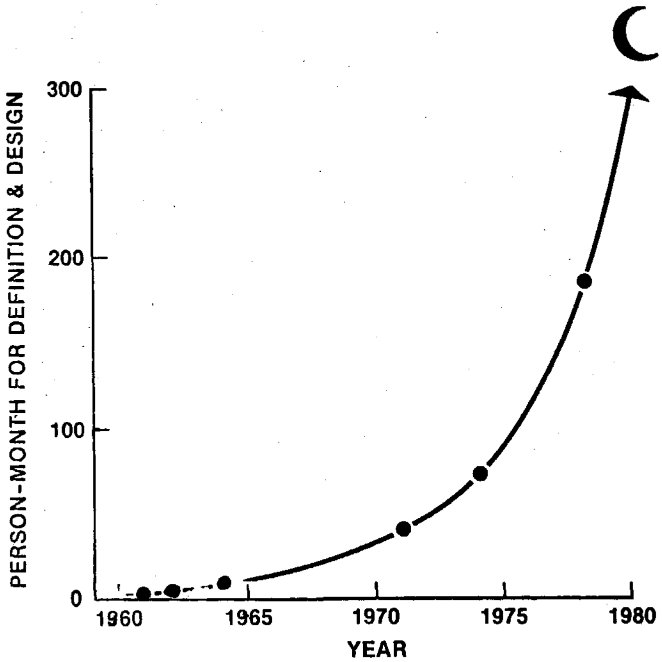
New architectures are also being designed for computers. After several decades of computers based on von Neumann architecture, we are now seeing a veritable explosion in concurrent computers, or parallel processors, an area in which Caltech has been a pioneer.

The economic revolution — the application of these new advantages to the development of products — follows the technological revolution. The products include not only new computers themselves but also a wide range of other goods that can be made more efficient and effective by embedding computers in them. We will be riding this price/-performance curve in two directions. Many applications will ride the price curve *downward.* Falling costs will lead to a proliferation of small, embedded computers in the home, the office, the factory, and the automobile.

Some pundits have criticized earlier forecasts of the increase of computers in the home and office as overblown. They have already been proven wrong. In my own house recently I went around room by room and counted computers. I have 17 computers, and that number will probably double in a few years. General Motors has contributed five computers to my garage — two in one car and three in the other. I never thought I was buying a computer when I bought those cars, but there they are. General Electric has contributed three computers to my kitchen. There again I didn't know I was buying them. My sprinkler system has two computers, and my hi-fi system (TVs, VCRs, and compact disk) has five. In my office I have two "real" computers, one sitting on a desk and another that I carry around with me in my suitcase.

The other half of this revolution consists of products that ride *up* the performance curve. Among the applications that will be possible with 100-times improvement in performance are image processing, expert systems, and — perhaps most dramatic — simulation. We are already at the state in the design of integrated circuits, for example, where simulation plays a crucial role in the design process. No one would think today of designing a very large scale integrated circuit without the benefit of a computer to do the simulation. That same process is going to be applied to the design of missiles, automobiles, tanks, and airplanes. Where are we going to find the leadership for all of this technological innovation?

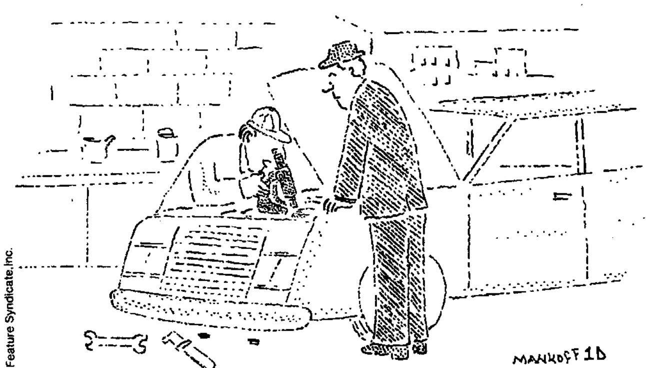
To answer that question I want to go back in history and ask where we found the leadership for the last phase of this revolution — where the leadership for the development of integrated circuits came from and why. My authority on the subject is a British engineer, G. W. Dummer. Dummer is the man who



Gordon Moore expressed the dilemma posed by the increasing complexity of integrated circuits: as it became possible to put more transistors on a chip, the design time increased at an exponential rate.

almost invented the integrated circuit. At a technical conference in 1952, several years before the invention of the integrated circuit, Dummer said: "With the advent of the transistor and the work in semiconductors generally, it seems now possible to envisage electronic equipment in a solid block with no layers of insulating, conducting, rectifying, and amplifying materials, the electrical functions being connected directly by cutting out areas of the various layers." You don't have to be an IC designer to understand that Dummer was describing the integrated circuit. He not only described it, but he set out vigorously to try to develop it. He did this with the full support and cooperation of the British government.

DOLLAR$ AND NONENE™



"Yep, she's gonna cost ya—your microprocessor's shot."

But it was not Dummer who invented the integrated circuit, nor was England its primary beneficiary. The IC was invented by Jack Kilby of Texas Instruments and Bob Noyce of Fairchild, and the consequence of that development occurring in the United States has been profound.

Dummer, years later, looking back wistfully at this missed opportunity, said: "It is worth remembering that American electronic companies were formed since the war by a relatively few enterprising electronics engineers, setting up with either their own capital or risk capital from the bank. Often a government contract would start them off. Hard work was necessary and the large home market was a great asset, but the climate of innovation was such that any advanced technical product could be sold. The American system of encouraging employees to hold shares in the company is one which should be emulated, as a part share in the company's prosperity gives an increased sense of responsibility. Successful businesses are almost always dependent on a few people who are innovative and enthusiastic."

Dummer is pointing out the critical importance of the entrepreneurial spirit and the availability of risk capital in the U.S. As a case in point: If a chief engineer in a major company in Europe or Japan left his job to start up a new company to follow up an innovative idea, his friends and co-workers would think that there was something wrong with him. In the U.S. if a chief engineer did *not* leave his job to follow up an innovative idea, his friends and co-workers would think something was wrong with him. A difference in culture leads people to take action in one country that they would not take in another. These cultural traits do not change quickly. As far as I can tell from sitting near the fringes of the venture capital industry, that innovative spirit is as vigorous today as it was five years ago. I think that there are even *more* bright, enthusiastic people bringing forward interesting ideas than was true five or ten years ago.

As for the availability of risk capital, anyone who tries to form a company soon finds out that banks and public stock are not useful or available sources for such capital. Neither of these institutions is created for the purpose of providing risk capital for inventors. There are, however, other sources. A funding technique that was popular five years ago, and I think is still an appropriate form, is the R&D limited partnership. But it's currently out of favor and may not return to popularity for a number of years, because a few very large companies (Trilogy and STC Computer in particular) bombed out and took with them about $50-100 million of R&D partnership investment. Not surprisingly, that had a chilling effect on investors, and it's not likely that we'll see many more R&D partnerships start technology companies.

"Bootstrapping" — that is, doing it with your own funds — is a time-honored way of financing new companies in this country. I would like to suggest that it is a greatly underrated technique. It's the way that I used in 1964 for my own company, ESL. It never had outside investment, never had venture capital, and is now a company with over $200 million annual revenue. It was also the technique used by two engineers named Bill Hewlett and Dave Packard when they started their company, which is now a multibillion-dollar enterprise. So it can be done, and it has been done successfully. The company founder has to accept a much slower growth than he could achieve if someone else were pouring money into the enterprise, but that's not all bad. It forces the entrepreneur to avoid the pitfalls that he would inevitably face with rapid growth of a new company.

The most popular form of risk capital in companies today is venture capital. This has undergone a remarkable transformation in the last seven to eight years. During the 1970s there was about $100-200 million a year of venture capital flowing into the venture pool, most of what was available for new company starts. This underwent a dramatic change in 1979. From then on there has been $2-4 billion a year of new money coming into the field — in other words, an order of magnitude increase in the investment in this area.

It would require a separate article to explain what really precipitated that rapid change, but certainly the change in the capital gains tax— an effective rate of 20 percent, down from 50 percent — was a primary contributor to that growth. More important, and less easy to analyze, is the fact that those venture funds that were established in the 1970s and were investing in companies during this period turned out to be fantastically successful. Many of them were showing 40 percent per year compounded growth rate return on those investments, and that attracted the attention of institutional investors. So it was a combination of several factors (primarily these two) that pulled out the throttle and caused this surge of new money to come into the venture capital business.

Another related and very important factor that is often neglected in discussions of this situation is the existence of the public over-the-counter market. Although I have already said that the public market was not a useful vehicle for a start-up company, its existence is critical, because it provides the mechanism by which venture investors and entrepreneurs eventually realize liquidity in their investments. The vigorous over-the-counter market that exists in this country has often provided the liquidity for these investors in five to seven years at 10 times their original investment, thereby making these investments very attractive. In Germany and Japan, for example, no such healthy over-the-counter market has existed, which is one reason that risk capital did not flow into start-up companies in those countries. Both of those countries are trying to change that now.

In the last year or so there has been much gloomy talk about how venture funds have fallen off and how people with bright ideas can't get venture money to start new companies anymore. This talk represents a gross misunderstanding of what the real situation is. First, today there is well over $10 billion in venture funds available for investment. That is plenty of money to invest relative to any standard, and particularly relative to the standards of the 1970s. Two factors have caused this atmosphere of reluctance on the part of venture capitalists to invest. First, the over-the-counter market was depressed for high technology stocks, and therefore the initial public offerings almost completely dried up for a year and a half. With the absence of initial public offerings, venture companies didn't have any way to graduate from a private to a public company, so their investors didn't have any way to achieve liquidity. When their companies needed a second or third round of financing, and there was no public market out there to provide it, they had to turn back to the original investors for it. So what has happened is that while the amount of money flowing into venture companies is actually greater in the last few years than previously, most of it is going into second- and third-round financing rather than into start-ups. So it indeed has been true that there has been less money for start-ups but not because of diminishing money in the venture capital pool.

The situation obviously will change rapidly as soon as the initial public offerings start up again. And there's every indication that this new wave of public offerings is already under way. To the extent that happens, there will be a dramatic transformation again in the use of funds that are now in this venture capital pool, and a much higher proportion of them will be eager to fund start-up companies.

There's another, relatively new, source of risk capital funds for start-up companies — funds from large corporations. So far I've been talking about innovation through start-up or emerging companies. But the amount of money that IBM invests in its R&D program in a given year is about the same order of magnitude as all of the venture money going into start-up companies in a year. So why don't these innovative developments all come out of a company like IBM? Why do we depend on start-ups at all for this innovation?

The large companies are effective in using their R&D funds to develop products that evolve from predecessor products, but they are not very effective in using their funds to take an innovative technology that leads to a new product — something that really breaks ground with predecessors. The size and the bureaucratic nature of a large company is simply incompatible with seizing a new idea and taking it rapidly to the marketplace. There is also another much more subtle problem — what I call the "liability of leadership."

I know this from personal experience. At the time that the transistor was being commercialized, I worked for Sylvania Electric Products, one of the three world leaders in the production of vacuum tubes. The research director clearly saw that the transistor was the successor product to the vacuum tube, so he launched an energetic R&D program. Then the president assigned responsibility for the commercialization of the transistor to the manager of the vacuum tube division. The rest is history.

The psychological problems, not to mention the very real financial problems, of a manager vigorously introducing a new product that kills off the product that is his bread and butter are not to be underestimated. A similar situation happened when IBM stood by and watched Digital Equipment invent the minicomputer and capture that market. This was not because IBM didn't know how to build a minicomputer. But had they successfully produced a minicomputer, it would have cut the legs off the low end of their mainframe (a very profitable part of their line), and they just couldn't quite bring themselves to do that. Somewhat later Digital Equipment did the same thing with the personal computer market. Ironically, by that time IBM saw the opportunity developing for the personal computer, and, since it didn't compete with their mainframes, was able to go in and make a major impact on that market.

More and more of the large companies facing this problem are asking themselves the question: What can we do to participate in these venture or start-up activities? They are concluding that if they can't beat them they'll join them. There have been a number of attempts to participate by acquiring small companies. I believe that when the statistics on this are available they will demonstrate a high ratio of failure. The problem with the acquisition of small companies is that the large company snuffs out the very flame it is trying to capture. Realizing this, some companies have switched over to the venture capital business. Exxon and General Electric are cases in point. My opinion is that these ventures will turn out to be unsuccessful as well. The companies may be successful as venture capitalists, depending on whom they have running the activity, but what they're really trying to achieve — a transfer of technology and a head start on new products — will not come this easily.

In the past few years a number of companies have tried an innovative approach known as corporate partnering, or strategic partnering, as an alternative to acquisition or venturing. In this arrangement the large company forms a business relationship with the small company that usually involves some technology transfer, and it also makes a minority investment in the company. A number of companies have been experimenting in one way or another with the technique in the last few years — IBM, General Motors, Eastman Kodak, Lockheed, Rockwell, TRW. In each case what they are trying to do is to get access to innovative applications of technology and rapid seizure of new product opportunities. The benefits to the small company are an access to risk capital, to a broad base of technology, and to markets and credibility. Basically this marriage combines the mass of a large company with the velocity of the small company to provide momentum for both companies to move forward.

In summary, I believe that the technological revolution that has taken place these last few decades not only will continue through this decade but will in all probability accelerate in both its technical and economic manifestations. Second, the culture — the entrepreneurial spirit — that underlies this revolution in the United States is alive and well and will continue to support leadership in technological innovation in this country. Third, the large pool of risk capital that has been formed during the last seven to eight years will maintain itself and will be available in ever increasing quantities to fund these start-ups; the risk capital will come both from the conventional venture funds and from a transfer of funds from large corporations to small corporations. □

The Past Recaptured

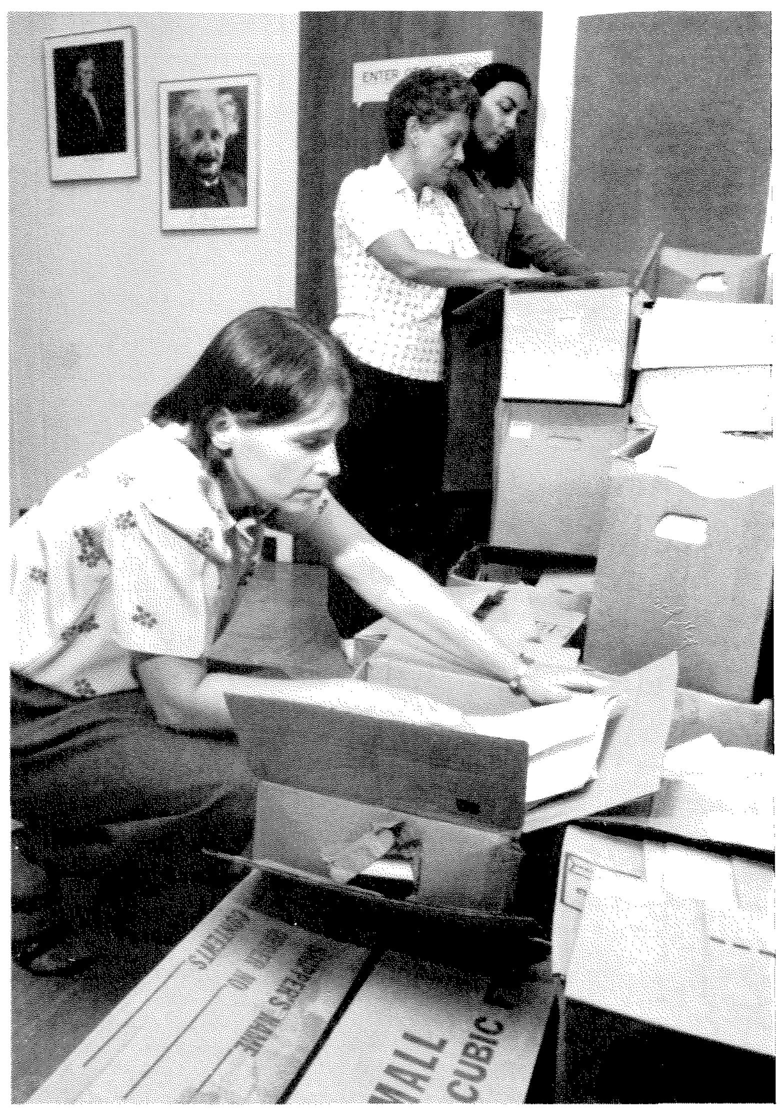
WHEN CHARLES RICHTER died last year, he left his papers — correspondence, unpublished writings, course notes, and technical reports — to Caltech. The collection from the inventor of the well-known earthquake scale was packed into dozens of cartons and delivered to the Caltech Archives, and then left in the hallway of the Millikan Library basement. There was no other place to put it.

The next morning Archivist Judith Goodstein and her assistant Carol Bugé discovered to their horror that the janitor had tossed out six or seven boxes, thinking they were trash. Bugé tracked down the garbage truck driver, who held up unloading his cargo until the Archives rescue team could reach the city dump. Fortunately the driver knew his route across campus and was able to determine approximately where in his truck the library trash might be located. "Then he dumped it inch by inch in front of us," remembers Goodstein. They managed to recover some but not all of the Richter cartons. "It broke my heart," says the archivist, even though she had figured that something like this was bound to happen sooner or later. The Archives had long since outgrown its space.

Although Goodstein has managed to seize some adjacent space over the years, the center of the Archives is still the same room that she walked into in 1968 as Caltech's first archivist. The room contained a map case, a filing cabinet full of medals, a bunch of boxes wrapped in brown paper — and the papers of Robert A. Millikan and George Ellery Hale. Theodore von Kármán's papers arrived a week later.

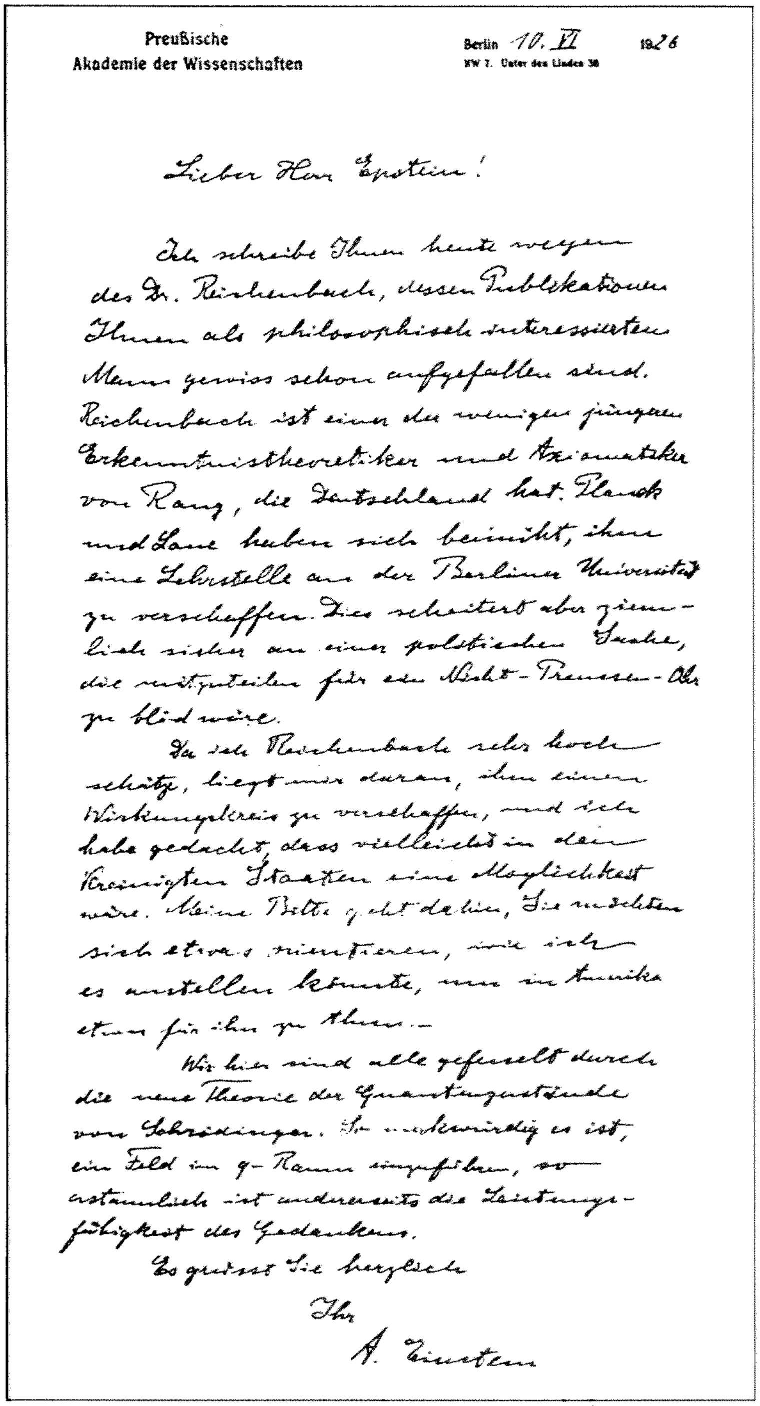
Daniel J. Kevles, now professor of history, had already begun to organize the Millikan and Hale collections, also getting the first crack at their contents. (This resulted in Kevles's prizewinning book, *The Physicists*,

Judith Goodstein (foreground), Paula Hurwitz (center) and Carol Bugé inspect the recently delivered boxes of the papers of former Caltech President Harold Brown — stacked on the floor in the Archives' basement hallway.



published in 1978. Scholars since have tended to avoid the Hale papers, presuming, falsely, that it has all been done, says Goodstein.) Kevles played an important role in getting Caltech's Archives established. In a memo to then-provost Robert Christy, he wrote: "The principal reason [for maintaining an archive] is that the study of history rests heavily upon the records of institutions and of the activities of its members. . . . Given the central importance of science in the 20th century, it would seem that, as one of the leading institutions in the world and as a community of scholars, Caltech has an obligation . . . to preserve the record of its life."

The record of Caltech's life had already been pretty well preserved, chiefly through the efforts of Roger Stanton, director of the Institute libraries from 1948 to 1963. Stanton was careful to save all the papers and files given to him over the years. The creation of the Archives provided a place to put these records, as well as a person to organize and make them available to the scholarly world. The boxes that Goodstein found when she arrived had been brought from various storerooms around campus to this "dumping ground" in the basement of the new Millikan Library when it was built in 1967. "What I inherited was everybody's little attic, plus Hale and Millikan," comments Goodstein.



Preußische
Akademie der Wissenschaften

Berlin 10. VI 1926
NW 7. Unter den Linden 38

Lieber Herr Epstein!

Ich schreibe Ihnen heute wegen des Dr. Reichenbach, dessen Publikationen Ihnen als philosophisch interessiertem Mann gewiss schon aufgefallen sind. Reichenbach ist einer der wenigen jüngeren Erkenntnistheoretiker und Axiomatiker von Rang, die Deutschland hat. Planck und Laue haben sich bemüht, ihm eine Lehrstelle an der Berliner Universität zu verschaffen. Dies scheitert aber ziemlich sicher an einer politischen Sache, die mitzuteilen für ein Nicht-Preussen-Ohr zu blöd wäre.

Da ich Reichenbach sehr hoch schätze, liegt mir daran, ihm einen Wirkungskreis zu verschaffen, und ich habe gedacht, dass vielleicht in den Vereinigten Staaten eine Möglichkeit wäre. Meine Bitte geht dahin, Sie möchten sich etwas orientieren, wie ich es anstellen könnte, um in Amerika etwas für ihn zu thun.–

Wir hier sind alle gefesselt durch die neue Theorie der Quantenzustände von Schrödinger. So merkwürdig es ist, ein Feld im q-Raum einzuführen, so erstaunlich ist andererseits die Leistungsfähigkeit des Gedankens.

Es grüsst Sie herzlich

Ihr

A. Einstein

A 1926 letter from Einstein to Paul Epstein, then professor of theoretical physics at Caltech, urges him to try to arrange an academic post in America for a young Jewish colleague, who was denied a teaching post in Berlin for "political" reasons. He concludes with the comment that everyone there was caught up in Schrödinger's new theory of quantum states.

Opening up the boxes and sifting through the papers was a heady experience. From Amos Throop's death mask to letters from Einstein, from long-lost photographs to the personal notes of Thomas Hunt Morgan — Goodstein found herself spellbound. The Caltech cache and its documentation of the rise of science and technology as a dominant influence in the 20th-century exerted a considerable influence on her own studies. Although her PhD work at the University of Washington dealt with the history of science in the 1800s, after arriving at Caltech she "never again looked back to the 19th century." Goodstein is currently writing a book on Caltech's history.

The immediate task at hand was cataloging 96 manuscript boxes (containing 76,000 documents and covering 144 linear feet) of Theodore von Kármán's papers, left to Caltech in his will. But Goodstein lost no time in laying the groundwork for future donations. She sent a memo to all faculty members and administrators raising their historical consciousness and urging them not to be hasty in throwing things out. "Students of the history of 20th-century science turn to unpublished primary sources . . . when they want to unravel the genesis of a particular idea, or how a particular research field grew, or how knowledge of it was communicated within and outside of the scientific community." Personal records that she considered worth preserving included "preliminary drafts of published papers, privately circulated memoranda, data books, records of experiments (the unsuccessful ones, too), original drawings or photographs of equipment . . . ."

Her plea was heeded only too well. There are more than 100 manuscript collections (70 of which have guides prepared by Goodstein and her staff) splitting the Archives' seams, and now Goodstein reluctantly has to turn material away. The one missing collection she coveted was Linus Pauling's; he gave it to Oregon State, his alma mater — including the Caltech chemistry division papers from the years Pauling was chairman. Such division records provide a wealth of information. "You can get a whole new perspective on the history of biology," says Goodstein, "in the Division of Biology papers between 1928 and 1955, the years of Thomas Hunt Morgan and George Beadle. And it's not just budgets . . ."

Sometimes the art of the archivist has demanded more than just sitting in the library basement waiting for the stuff to roll in. Frank Malina was an avid collector of primary source material relating to Caltech's rocket research project and the founding of the Jet Propulsion Laboratory (see article on page 8). When he left his collection to the Library of Congress on his death in 1981, Goodstein flew to Paris to survey the papers and subsequently shipped 302 kilos of paper back to Caltech for microfilming before relinquishing it to Washington.

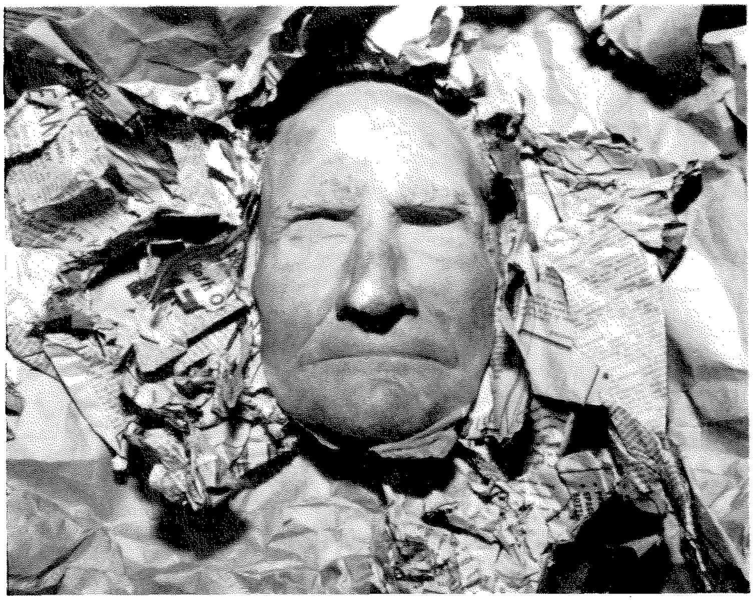
Not content with collections of papers, which might not tap the details and nuances of important events, Goodstein also began to mine people's memories. She started the oral history project in 1978, and it now consists of nearly 70 transcribed interviews with people who were on the scene during Caltech's formative years. (Portions of a number of the oral histories have been published in *E&S*.) Since many of these world-famous scientists led other lives before (and after) Caltech, their stories encompass a much broader scientific and political arena than the Pasadena campus.

This is what makes the Caltech Archives such a great resource of intellectual history — its collection does not just concern Caltech. Most other places are far more parochial and hence far less interesting, but here "these guys sat at the center of the storm," says Goodstein. Caltech people corresponded with just about everyone who was anyone in 20th-century science. And science wasn't the only thing they wrote about. For example, the horrors of the rise of Nazi Germany and fascist Italy can be read in some of the scientists' letters. Paul Epstein's papers contain unopened letters from relatives in Europe plead-

Collections in the Caltech Archives

bold face — guides available
* available in microform

Walter Adams
American Society of Civil Engineers, Caltech student chapter
John A. Anderson
Architectural Drawings
ASCIT Research Project-4
Audio Cassettes including
  Alumni Day Seminars
  The Feynman Lectures on Physics
  Frontiers of Science
  Watson Lecture Series
Richard Badger
Edward C. Barrett
Harry Bateman
George Beadle
Frederick Bedell
Eric T. Bell
Biology Division Records
Ira S. Bowen
Norman Bridge Medical Library
Harold Brown
John P. Buwalda
California Graduate School of Design
Caltech Carnegie Program, Science and Government
Caltech Clean Air Car Project
Caltech Committee Files including
  Ad Hoc Study Group on JPL/Campus Interactions
  Papers of Faculty Advisory Committee on Presidential Candidates
Caltech Faculty Board Minutes
Caltech Faculty Minutes
Caltech Historical Files
Caltech Physics "Bone Books"
Caltech Population Program
Dan H. Campbell
Marshall H. Cohen
Robert S. Corey
Norman Davidson
Max Delbrück
Charles H. Dix
Lee A. DuBridge
Jesse W.M. DuMond
Paul S. Epstein
Richard P. Feynman
Arthur Fleming
William A. Fowler
Horace N. Gilbert
Alexander Goetz
Jesse L. Greenstein
Beno Gutenburg
Herbert Hahn
George E. Hale*
Marshall Hall
Institute Libraries
Institute Publications
Jet Propulsion Laboratory
Kanpur Project Files
Theodore von Kármán*
Arthur L. Klein
Charles C. Lauritsen
Thomas Lauritsen
Tullio Levi-Civita
Frank J. Malina*
Jon Mathews
Jack E. McKee
Medals
Aristotle Michal
Clark B. Millikan
Glenn A. Millikan
Robert A. Millikan*
Edward W. Morley
Thomas H. Morgan
(Henry) Victor Neher
Arthur A. Noyes
Observatory Council
Office of Sponsored Research
Oral Histories
Palomar Site Survey
Pi Kappa Delta
Physics 3 Project: World War II Rocket Project
Russell W. Porter
Grennady Potapenko
Press Releases
Prints, Photographs, and Slides
Arthur Raymond
Charles F. Richter
Howard P. Robertson
Henry Robinson
W.C. Rockefeller
Bruce H. Rule
Bruce H. Sage
James A.B. Scherer
Scientific Apparatus
Seismology Microfiche Publications*
Sigma Alpha Pi
Robert L. Sinsheimer
William R. Smythe
Royal W. Sorensen
Homer J. Stewart
Chester Stock
James H. Sturdivant
Alfred H. Sturtevant
Synchrotron Program
Charles Taylor
  Aviation Memorabilia
Amos G. Throop*
Throop Polytechnic Ledgers
Richard C. Tolman
Albert Tyler
Anthonie Van Harreveld
Video Cassettes including
  Ditch Day activities
  The Mechanical Universe
  Mirror-Casting Construction, 200-inch reflector
Jerome Vinograd
Morgan Ward
Earnest C. Watson
Leon Watters
Jean Weigle
Cornelis A.G. Wiersma
Harry O. Wood
Don M. Yost
Laszlo Zechmeister



Among the more peculiar items in the Archives collection is the death mask of Amos Throop, Caltech's founder, here staring out of the yellowed newspaper in which it was wrapped.

ing for help in getting out; Epstein felt so helpless and depressed that he couldn't read them. "I opened some of the letters," admits Goodstein. "I didn't feel so good about that."

The intermingling of science and politics can also be read in the Archives. For example, Hale's attempt to create the International Research Council after World War I, which foundered on the enmity of the scientists of the Allied nations as well as on Woodrow Wilson's political commitment to the League of Nations, is well documented in Hale's letters.

Other scientists' personal papers reveal somewhat off-beat personality traits. (Trivia question: Which famous Caltech professor frequented nudist camps?) A donor can place access restrictions on any material of a private or controversial nature, and many do. But "whether your interest is prurient, or more academic, in science or politics, it's all here," says Goodstein.

Grants have enabled the Archives staff (which includes, besides Bugé, Assistant Archivist Paula Hurwitz) to make the Caltech collection more available to scholars. A grant from the National Endowment for the Humanities helped process the von Kármán and DuBridge papers, and the Smithsonian Air and Space Museum financed publication of von Kármán's papers in microfiche. Hale's documents had earlier been published in microfilm under a grant from the National Historical Publications Commission.

Another large microfilming project made the earthquake records of the Kresge Seismological Laboratory (including single-copy seismograms on 276,000 individual photographic sheets) available to the scientific community as part of an international earthquake data bank. It took the Archives staff 3½ years to film and catalog the records between 1923 and 1962, which provide a continuous long-term chronicle of local and distant earthquakes over much of the period for which instrumental data exist. Earthquake data over long periods of time are important in understanding seismicity rates and changes — in order to assess current risks, for one thing. Financial support for this project came from the U.S. Geological Survey and the National Oceanic and Atmospheric Administration.

It's always nice to have grants, as every scientist knows, but they don't solve all budget problems. So Goodstein thought up a more enterprising form of financing to counter budget cuts in the early 1970s — marketing the now well-known photograph of Einstein on a bicycle. The poster, distributed by the Smithsonian Institution and several other outlets, has netted about $40,000, which has helped fund many of the oral history projects. Is the original photograph itself in the Caltech Archives? "Actually, I think I first saw it in *E&S*," admits Goodstein.

About 500 researchers, a steadily increasing number, now come to use the Archives every year and there are numerous other requests to the staff for information and photographs. In the early 1970s most Archives users were Caltech faculty; now almost all come from outside, most of them scholars (with an occasional "respectable" journalist) and most of them in history of science. "By and large we're pretty open," says Goodstein, "but you have to have a legitimate reason." Goodstein has lately noticed a significant switch in users' interests. Last year for the first time more historians consulted the papers of biologists, biophysicists, and geneticists than those of physicists, mathematicians, and chemists. Most popular were the collections of Max Delbrück, Thomas Hunt Morgan, and George Beadle, in that order.

Researchers using the Archives are consigned to two desks in the hallway — the same catch-all hallway from which the Richter cartons disappeared. Goodstein has been trying to get the work area improved for the past 16 years. The day may come, however, when this space too is completely eaten up by more cartons of documents awaiting either cataloging — or an errant garbage truck.□ — *JD*

Research in Progress

One Genome, Fully Deciphered

Nineteen years of concentrated labor have had their final payoff: this year Giuseppe Attardi, the Grace C. Steele Professor of Molecular Biology, and his research group have completed the functional identification of all the genes in human mitochondrial DNA (mtDNA). In the course of this work they've uncovered quite a few surprises, including the highly efficient mode in which information is packed into the genome, a mode that Attardi calls "a lesson in economy."

In most genetic systems, including the chromosomes in the nucleus of human cells, the great majority of DNA does not actually code for any gene product. There are great stretches of "nonsense" DNA in the spaces between genes, and even within most eukaryotic genes there are long noncoding regions called introns. Not so in mtDNA. Except for one short region, which anchors the mtDNA to the inner mitochondrial membrane and is important in initiating transcription, every part of the mitochondrial genome codes for some product, either a protein, or a ribosomal RNA (rRNA), or a transfer RNA (tRNA).

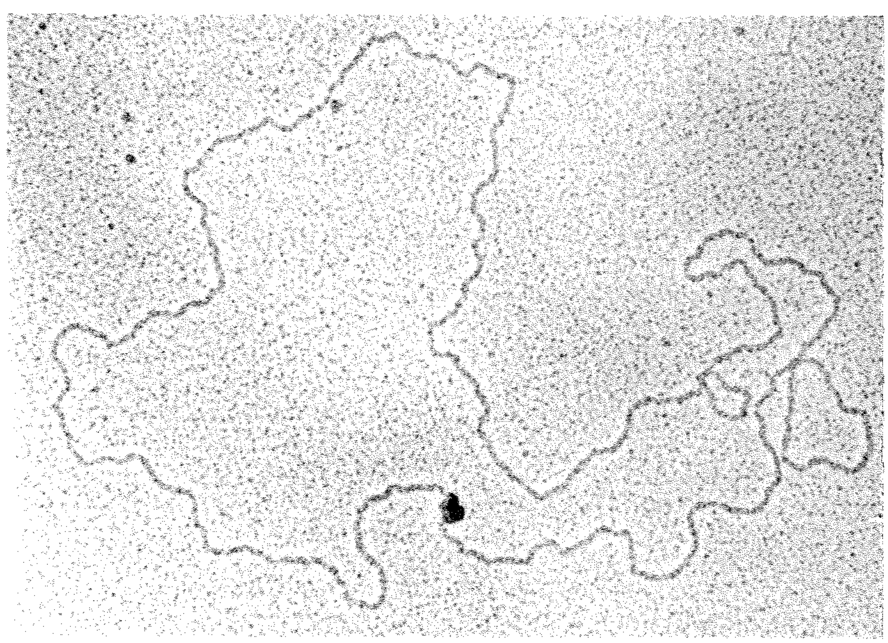
Also, in most genetic systems only one strand of the double-stranded DNA molecule in a given segment codes for gene products. In some cases genes can be found on both strands, but normally when this happens one strand is transcribed in one segment and the other strand is transcribed in the next segment. Human mtDNA is unique in that both strands are completely transcribed.

But even with this extreme degree of information compression, mitochondria (the cell's power plants) are far from autonomous. Attardi thinks that this may not always have been the case. Like many biologists, he believes that the mitochondrion was once an independent cell that long ago mounted an invasion of another cell. In a process known as endosymbiosis, the proto-mitochondrion became its host's partner, providing quantities of energy in the form of adenosine triphosphate (ATP) in return for a protected environment. Eventually, after hundreds of millions of years of evolution, most mitochondrial genes became incorporated into the host's genetic machinery. In mammalian cells this continued until the mitochondrial genome was a ghost of its former self — it contains just 16,500 base pairs in a circle 5 micrometers in circumference. Because of this limited coding capacity, the mitochondrion must import at least 95 percent of the proteins it needs to function.

Much of the content of mtDNA is devoted to coding for the mitochondrion's own protein synthesizing machinery. With some help from the nucleo-cytoplasmic compartment of the cell, mitochondria produce their own ribosomes — the protein-synthesizing factories that translate messenger RNA (mRNA). These mitochondrial ribosomes are scaled-down versions of the ribosomes used in the cytoplasm for the same purpose. And mtDNA codes for 22 different tRNAs. These are the molecules that bring the 20 different amino acids to the lengthening polypeptide chain as a protein is actually being assembled on the mRNA template.

At first, this number of tRNAs was something of a puzzle — the cytoplasm requires 32 tRNAs because in the universal genetic code a single amino acid is often coded for by two or more nucleotide triplets, and each tRNA can recognize only one or two triplets. Although it initially appeared possible that mtDNA-coded proteins might simply lack several amino acids or that the missing tRNAs were imported, Attardi and others were able to discard these two proposed solutions to the discrepancy. The actual explanation is that mitochondria use a simplified version of the genetic code, one in which (among other differences) each of several amino acids is coded for by four triplets that can be recognized by a single tRNA.

Apart from the different RNA species coded for by mtDNA, it became obvious, when the DNA sequence was determined in F. San-



This high-resolution electron micrograph shows one molecule of human mitochondrial DNA. The molecule forms a circle 5 micrometers in circumference, and it contains about 16,500 base pairs. The dark knob at bottom center represents a residue of the inner mitochondrial membrane, to which the DNA molecule is attached.

ger's laboratory in England, that the mitochondrial genome codes for 13 different proteins. It proved relatively easy to identify six of these proteins because of their amino-acid sequence homology to known proteins. Four turned out to be proteins important in the respiratory chain — a series of electron carriers that transfer to oxygen the electrons released during the final stages of the oxidation of food molecules. (These four proteins are cytochrome b and three subunits of cytochrome *c* oxidase.) The fifth and sixth proteins were found to be two subunits of ATPase, the enzyme that uses the energy derived from the oxidation of food molecules to synthesize ATP.

This left seven "unidentified reading frames" (URFs) in mtDNA — 60 percent of its protein coding capacity. Although it was possible that the URFs were nonsense sequences, Attardi and his colleagues were able to reject that hypothesis for two reasons. For one thing, these sequences were highly conserved in different mammalian species, something that never happens with nonsense DNA. For another, the researchers had clear evidence that the URFs were expressed — that is, transcribed into mRNA, which in turn is translated into protein. But, says Attardi, determining the functions of these proteins "appeared to be a terrible job. I thought that a possible shortcut would be to show that these proteins were somehow related to each other, that they belonged to the same complex."

This indeed turned out to be the case. Two of Attardi's postdocs, Anne Chomyn and Paolo Mariottini, collaborating with Russell Doolittle's group at UC San Diego, and using antibodies raised to the proteins encoded by individual URFs, were able to show that each of these antibodies under certain conditions precipitated six of the proteins produced by the URFs, indicating that they were all part of the same enzyme complex. Further investigation showed that this enzyme complex is another important component of the respiratory chain — NADH dehydrogenase. Finally, this year the remaining URF was shown to encode another subunit of this same enzyme, a finding that will appear soon in the journal *Science*.

Even though the last URF has finally been identified, Attardi claims that he has not put himself out of business. "Our effort now goes in the direction of studying how the expression of this genome is regulated. These are very intriguing problems."

And Attardi also hopes to study a number of genetic diseases that seem to result from damage to the mitochondrial genome. "There is a whole group of diseases called mitochondrial myopathies, which mostly affect the muscular system but sometimes also affect the nervous system and the heart. All these myopathies are characterized by alterations in mitochondria — both structural alterations and functional alterations. And the enzyme complex that is most frequently altered is NADH dehydrogenase."

This brings Attardi back to his original motivation for working with human cells rather than with yeast or *Neurospora*, both of which are more amenable to genetic analysis. "I'm an M.D. I'm interested in man not only as an example of a mammalian species or a higher eukaryotic cell, but also as an organism for which there are medical problems, practical problems to be solved." □ — *RF*

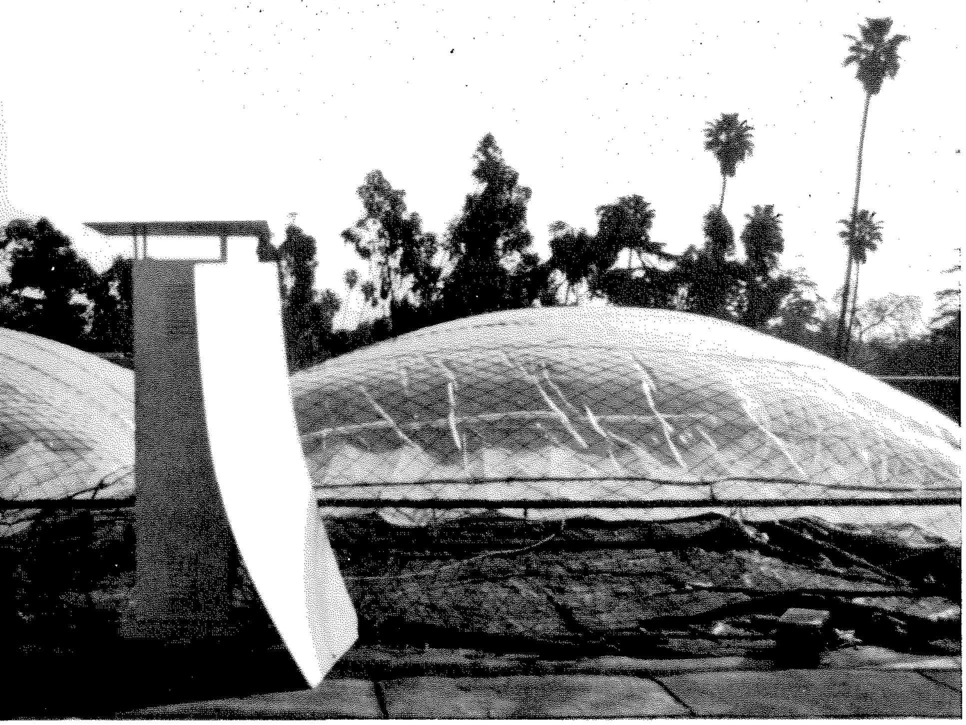
Smog — In Bags and Balances

DURING AN AVERAGE Los Angeles morning rush hour, thousands of tons of pollutants from hundreds of thousands of cars join thousands of tons of pollutants from industrial smokestacks. As the prevailing winds carry these compounds in a generally westerly direction, the sun causes them to begin reacting with each other. These chemical reactions produce ozone, nitric and sulfuric acid, and many other things as well. Some of these condense to form suspended aerosol particles, which cause the haze that we call smog.

Understanding this process is a job every bit as complex as predicting tomorrow's weather, but John Seinfeld and his colleagues are very close to achieving this goal. Seinfeld, the Louis E. Nohl Professor and professor of chemical engineering, has developed a detailed computer model of Los Angeles smog.

"You could, in principle, use this model to predict tomorrow's smog," says Seinfeld. "The only problem is the expense. You would need a supercomputer more or less dedicated to this use. The chemical mechanisms of smog involve 100 to 150 reactions. Then on top of that you've got the particle formation process, which is of comparable complexity. Our long-term goal is to have such a model on a computer that the Air Quality Management District could use."

However, "a computer model like this is only as good as the fundamental chemistry and physics in it," notes Seinfeld. In particular, the formation and composition of aerosol particles



On the roof of the Keck Laboratory is a large Teflon bag that Caltech researchers fill with hydrocarbons and nitrogen oxides to simulate early morning air. Sunlight drives the reactions that produce smog, which is analyzed by instruments housed in the structure in the center of this photograph.

are still only poorly understood. In an effort to learn more about atmospheric aerosol, Seinfeld and Richard Flagan, professor of environmental engineering science and mechanical engineering, together with graduate students Mark Cohen, Carol Jones, Gideon Sageev and Jennifer Stern, are conducting both large-scale and small-scale experiments.

To study large-scale smog phenomena, the researchers have installed a smog chamber — a room-sized Teflon bag — on the roof of the Keck Laboratory. They fill the bag at night with precisely controlled concentrations of hydrocarbons and nitrogen oxides to simulate early morning air, and then they cover the bag with a tarpaulin. The next morning they remove the tarp, and the sunlight begins driving the chemical reactions. Periodically they withdraw air through ports in the bag and measure gas concentrations and particle numbers and sizes.

One important question that remains to be answered is the extent to which aerosol particles are formed by nucleation or by condensation on pre-existing particles. Nucleation occurs when the partial pressure of a gas gets so high that aerosol particles precipitate spontaneously from the saturated air. It had been thought that nucleation played an insignificant role in atmospheric aerosol formation, since condensation will continually occur when the atmosphere is full of particles on which gases can condense. But Jennifer Stern has found that nucleation can and does occur at a significant rate. This happens when the concentration of a gas increases at such a rate that it exceeds the capacity of the condensation process.

The small-scale studies of aerosols are conducted at a very small scale indeed — that of individual aerosol particles. In a variant of the Millikan oil-drop experiment, the researchers use a device called an electrodynamic balance to levitate a single charged droplet, just five micrometers or so in diameter, between hyperbolic electrodes. Once the particle is in place, they can determine its precise size and, in a most remarkable recent accomplishment, Gideon Sageev has measured the particle's infrared spectrum and thus its composition.

Determining the particle's size is fairly straightforward — they simply turn off the charge on the electrodes and let gravity take over briefly. By measuring how long the particle takes to fall a predetermined distance, a computer can automatically calculate its size.

Sageev has recently developed an ingenious way to determine the infrared spectrum of an aerosol particle. Normally, to determine an infrared spectrum, light of various infrared wavelengths is shined through a sample of liquid or gas. When the wavelength of the light matches a particular molecular resonance, the molecules absorb the light and this absorption is inferred by detectors that see a decrease in light transmission through the sample. But with such a small particle the amount of light absorbed is infinitesimal, so measuring it is impossible.

Instead, the researchers take advantage of the fact that when the droplet absorbs some infrared energy, it heats up and a tiny bit of the liquid evaporates. Less than a single layer of atoms on the surface of the particle evaporates, but the laser light scattered by the particle changes enough to signal even this minuscule change in size. The device slowly steps through the infrared wavelengths, measuring size changes with each step, a process that takes about one hour.

The researchers plan to use the device to look at the composition of particles containing sulfates. Such particles are a common by-product of industries that burn coal. This is an important question since Carol Jones has shown that the composition of these particles changes with time, especially if they contain small quantities of catalysts such as manganese.

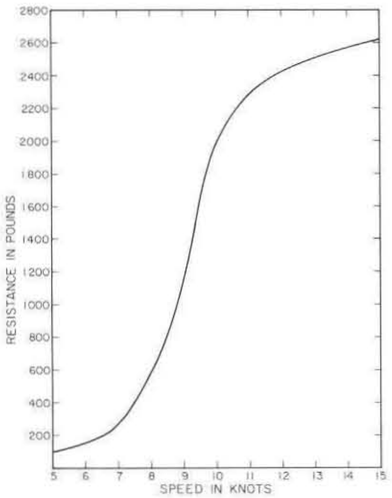
As the results of these experiments come in, they will add detail to Seinfeld's numerical model of smog. It's particularly appropriate that these developments have happened at Caltech, since the late Arie Haagen-Smit virtually invented smog research here. Notes Seinfeld, "Around 1950 Arie Haagen-Smit really figured out the relationship between oxides of nitrogen and ozone — a critical step in understanding the system. In a sense my research is a natural progression from what Haagen-Smit started." □ — *RF*

America's Cup contenders in 1983, Liberty *(top) and* Australia II, *illustrate the running room that they require when both have mainsails and spinnakers out.*

The Boat That Almost Was

continued from page 7

Figure 10 indicates that the upright resistance of a planing hull (with a gross weight of 24,000 lbs.) levels off at high speeds. This will permit it to surf on larger waves.



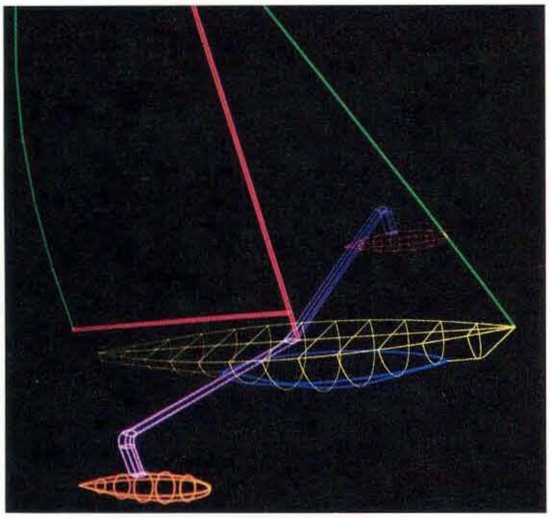
figures, levels off as indicated in Figure 10. Although sails cannot provide the several thousand pounds of thrust to cause a hull to plane, ocean waves frequently have slopes of 20 percent or more, and for a 24,000-lb. yacht, this gives a forward thrust of 4,800 lbs. or more — quite enough to cause it to plane.

Besides a wide beam, there are other ways to provide upright stability for an ultra-light hull. When a sailboat is running before the wind, it has the mainsail out on one side and the spinnaker out on the other. On a 12 meter each of these sweeps out to a distance of about 35 ft. on each side of the boat. This establishes the "running room" that must be permitted under the rules.

This suggests the possibility of deploying pontoons on outriggers within this running room in order to obtain upright stability instead of using lead ballast. If the pontoons are out of the water when the boat is in the upright position (all 12-meter measurements are taken in the upright position), they would not be classed as extra "hulls." In fact, they can be lying on the deck when the yacht is measured. And if the outriggers are deployed above the deck level, they would be classed as "booms" and thus not in violation of the "hollows in the hull" rule.

If the pontoons take the place of lead ballast, the wetted area of the keel can be reduced by about 140 sq. ft. If one of the pontoons has this much wetted area, then using an airship shape, such a pontoon could have a buoyancy of 6,000 lbs. When deployed on a boom 35 ft. long, such a pontoon would give a righting moment of 230,000 ft.-lbs., an even greater moment than provided by the wide beam of our earlier lightweight hull (and far greater than provided by the conventional lead keel). Perhaps even more important, the pontoons would prevent the yacht from heeling more than about 15°. This would be a significant advantage, since a great deal of sail power of a conventional yacht is lost when it's heeled as much as 30°. Pontoons could also be changed just as sails are changed. When light winds are anticipated, small pontoons with less resistance could be used, and extra-large pontoons could be substituted in heavy winds.

The required upright stability for ultra-lights can also be provided by "ailerons" extending out 35 ft. on each side of the boat. Each of these long thin wings could be rotated about its axis so as to provide hydrodynamic heeling moments when the yacht is under way, acting in the same way as ailerons on an airplane. Since almost all of the 12-meter measurements are



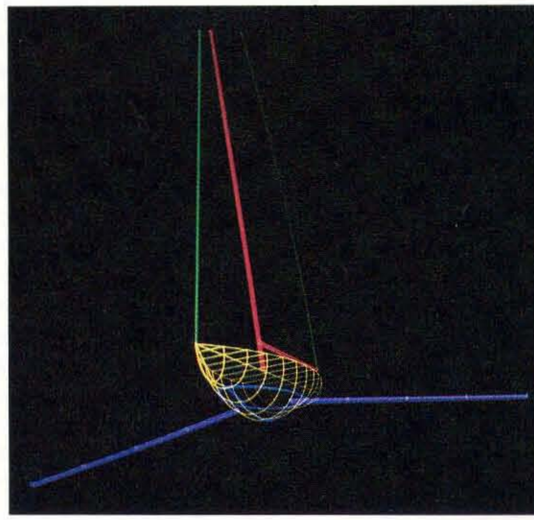


Figure 11: Pontoons on outriggers (far left) can provide upright stability and also keep the yacht from heeling more than 15°. The underwater "ailerons" in Figure 12 (to the immediate left) can also provide upright stability. Such solutions as broad beams, pontoons, and ailerons render a heavy keel unnecessary and make an ultra-light hull feasible.

***Below:** Later this last summer the author had this 40-percent-scale model built and tested to explore further the potentialities of the underwater proboscis.*

taken at or above the waterline, there is nothing to prevent this. In fact, an official interpretation of the rule says that the hull may be of any shape below 150 mm (6 in.) below the waterline as long as it does not exceed the length between the fore and aft points 180 mm above the waterline.

If these wings, made of carbon fiber composites, have chords of 12 in. and thicknesses of 2.5 in., they can support the loads that are required to keep the yacht from heeling under the force of the sail. Such wings would have about 140 sq. ft. of wetted area, about the same amount that would be saved if the lead were removed from the keel. So the fluid friction would be approximately the same as that of the larger keel, but with the very great savings in weight that would make the ultra-light hull possible.

Ailerons would have a significant advantage in heavy winds. As wind strength increases, the forces go up as the square of the speed. With a conventional keel, the heeling of the yacht increases rapidly with increasing winds, and once it has reached 30°, the sail area must be decreased (reefed) to prevent the yacht from heeling farther. As a result of the decreased sail area, the yacht's speed no longer increases with increasing wind speed. In contrast, the power of the ailerons goes up with the square of the boat's speed, enabling the yacht to stand up straight even in the strongest of winds. So the yacht could take full advantage of higher winds without sacrificing sail area and thus speed, as shown in Figure 13 on the next page.

The fact that the 12-meter rules give the designer great freedom in shaping the portion of the hull that is 6 in. or more below the waterline suggests still another avenue to explore: Is there a way of making a boat with a small waterline length have a much larger wavemaking length?

We often see supertankers and other commercial vessels with bulbous bows that are designed to reduce their wave resistance at a specific speed. The reasoning that leads to the bulbous bow concept is relatively simple. The bow of a ship cutting through the water creates a wave crest, which initiates the train of waves that spread out behind. It is known that if a sphere is towed just under the surface of the water, there is a slight upwelling ahead of the sphere, but a large trough is created immediately behind it. This also initiates a train of waves that spread out behind it. But where the ship's waves start off with a large crest, the waves from the sphere start off with a large trough. If this trough could be positioned to counteract the crest, they would cancel each other, and the wave resistance would be nearly eliminated. It turned out indeed that by placing a bulb underwater out ahead of the ship's bow and joining it smoothly into the hull lines, the bow waves could be dramatically reduced.

One problem was that the interaction of the two wave systems was altered by a change in the ship's speed. A bulb that would produce a favorable interaction at one speed could produce unfavorable interactions at other speeds. For commercial vessels that spend their time traveling at one speed, this was an acceptable restriction. But for sailboats, which travel at a wide variety of speeds, bulbous bows have appeared in the past to have little chance of success.

Earlier we have seen that it is the Froude number, $V/\sqrt{gL}$, that governs the wave patterns of ships. Commercial vessels typically travel at Froude numbers between 0.25 and 0.30. In contrast, 12-meter yachts in moderate to heavy winds travel at Froude numbers of 0.37 to 0.45. At these higher values the wave pattern is simpler. This should make the design of a 12-meter bulbous bow simpler and should permit it, if properly designed, to be effective over a wider range of speeds.

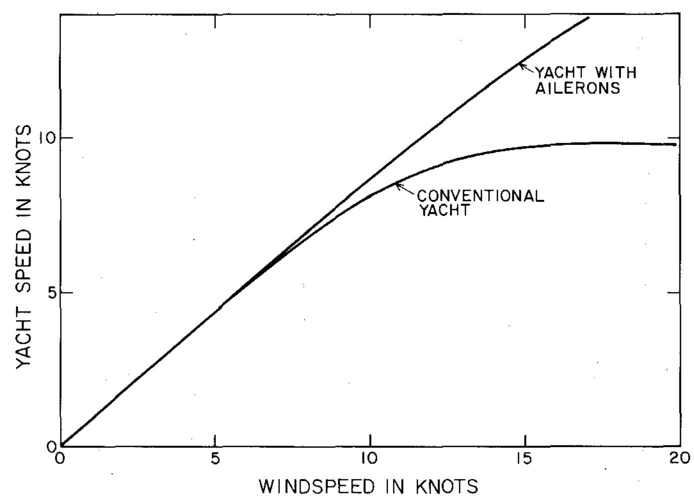
The concept of a bulb producing a beneficial interaction with the bow is a useful one, but it misses an important point. The wave resistance of a hull is proportional to the fourth power of the slenderness ratio of the hull. The bulbous bows on tankers are merely small add-ons. However, the 12-meter rule gives the designer great freedom to make the bulbous bow almost any shape he desires as long as it is at least 6 in. under the waterline. Such an underwater proboscis can thus extend far out ahead of the bow without increasing the measured length. And it can provide a significant addition to the displacement so that the hull itself can be greatly slenderized.

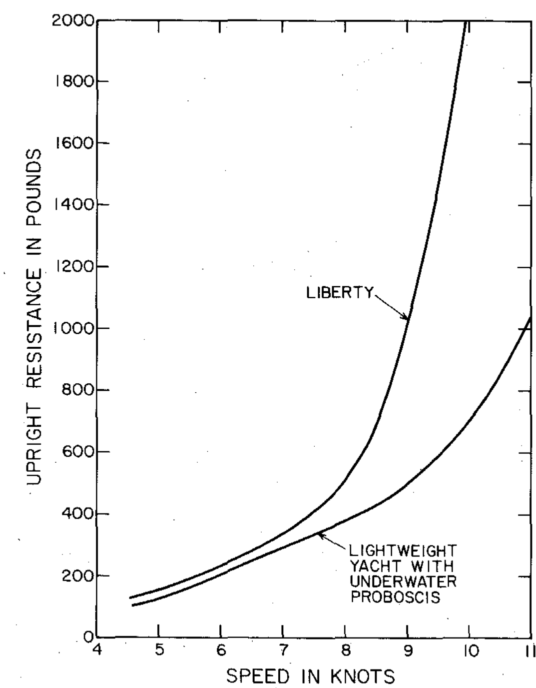
Let us consider again an ultra-light with a gross weight of 24,000 lbs. and a waterline length of 33.6 ft. Again, using nearly horizontal bow and stern overhangs, we can meet the America's Cup rule for a minimum length of 44 ft. at a plane 2 in. above the waterplane. And with proper shaping of the bow and stern we can have a measured length which will again allow a sail area of 2,450 sq. ft. Further, the 12-meter rule will permit a total underwater length of 44 ft. Hence the proboscis can extend 44 minus 33.6 ft., or 10.4 ft. out ahead of the bow.

All of this accomplishes two important things. First, the wavemaking length of 44 ft. is now nearly equal to the 45.6 ft. of conventional 12 meters. But more importantly, the light weight (24,000 lbs. vs. 57,000 lbs.) and the long underwater proboscis lead to a hull whose slenderness ratio is about half that of the conventional hull. This implies that the wave resistance will be reduced to a small fraction of that of the conventional hull. So much for theory and computer calculations. How will such a hull really perform?

When all of this was coming into focus in my mind in November 1985, construction was just beginning on Valentijn's conventional yacht *Eagle.* Unfortunately, the *Eagle* syndicate was a half-million dollars in debt. The construction costs for *Eagle* began to mount rapidly, and ahead still lay the need for sails, masts, booms, and so on.

Under these circumstances the board of directors of the *Eagle* syndicate became deeply divided on the question of planning for the second, more radical boat. Tooby, chairman of the board, and Gary Thomson, syn-





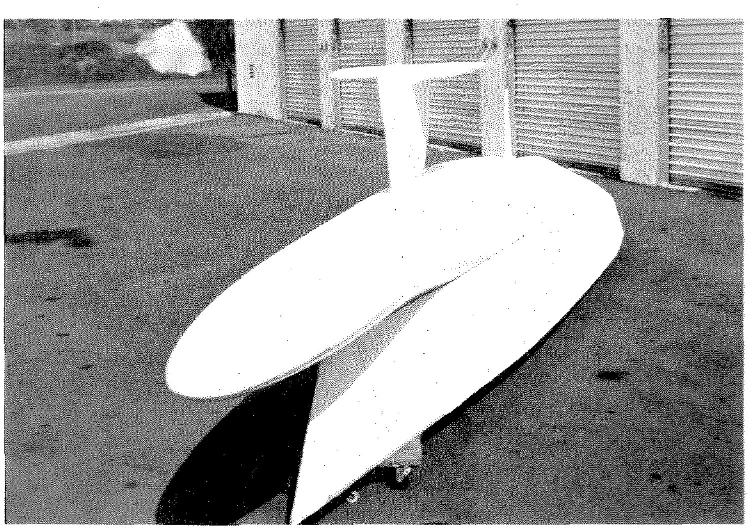


Figure 13 (above left): Since the power of the ailerons goes up with the square of the boat's speed, a yacht with ailerons could take full advantage of high winds. A conventional yacht would have to reef its sail, thus leveling off its speed, to keep from heeling too far.

Figure 14 (above): Towing tank tests showed the slender, lightweight model with an underwater proboscis with the same resistance at 11 knots as Liberty *had at 9 knots. The* Eagle *syndicate insisted that these tests on the model at the left be conducted with* Eagle's *keel rather than with pontoons or ailerons.*

dicate president, were enthusiastic about building and testing a model of my proposed hull with the underwater proboscis. But Valentijn and other members of the syndicate were opposed on the grounds that this would take away funds needed for *Eagle*.

It was not until March 1986 that a decision was made to make funds available to build a 40-percent scale model of the lightweight boat with the underwater proboscis. Even though this boat was designed to be used with either pontoons or ailerons, members of the syndicate insisted that it be tested with the keel and rudder used on *Eagle*, ostensibly to have a direct comparison.

Early in April we were able to test this model, and the results were little short of spectacular. They are shown in Figure 14 in comparison with *Liberty*. At low speeds the resistance is lower because of the smaller size of the lightweight hull. At higher speeds, instead of the usual disadvantage that a smaller boat has, the underwater proboscis, combined with the slenderness that it makes possible, gives the ultra-light the truly remarkable advantage in decreased resistance that theory had predicted. The ultra-light has the same resistance at 11 knots that the conventional hull has at 9 knots. And on top of this, the ultra-light can carry 40 percent more sail area. Advantages such as this usually occur only in a yachtsman's wildest dreams.

These results created quite a commotion within the *Eagle* syndicate. Tooby and Thomson (who had witnessed the towing tank tests) were highly enthusiastic. Others were less so. Because of an ever-increasing dearth of funds, there was now the danger that if a new hull were to be built, work on the original hull might have to be stopped. But perhaps the most critical factor facing us all was the question of time. To compete in the America's Cup, all yachts had to be measured and certificated by September 1, 1986. In order to meet this date, construction on the new hull would have to begin by May 1, and it would have to be flown to Perth by air cargo.

During the last two weeks in April, Thomson and Tooby made a heroic effort to raise an extra million dollars to build the new boat. But by May 1 it was clear that there was no chance of meeting this goal, and the syndicate decided to focus its efforts on the conventional boat.

At this point Tooby and I decided to resign from the *Eagle* syndicate and approach the New York Yacht Club (of which Tooby was a member) to see if at this late date they would have any interest in undertaking the design and construction of such a radical hull. The members of the New York Yacht Club syndicate expressed enthusiastic interest, but the boat-building firm that had constructed all of the NYYC's 12-meter yachts (as well as *Eagle*) told them that it was too late to design and build a new hull, particularly such a radical one.

By this time the New York Yacht Club had already built two versions of its yacht *America II* and was in the process of launching a third, since the first of these had proven to be a poor contender. Although it was too late to design and build a new boat, the boat builders said that it might be possible to take the first boat, cut the entire bottom out of it, and put in a new bottom. They thought they could do that by September 1.

Tooby and I went back East for a lengthy meeting with representatives of the New York Yacht Club and their designer. We laid all our cards on the table, describing in detail the design of the hull and the towing tank tests on it. The NYYC members were at first skeptical but then admitted that in all their computer studies they had never uncovered the great potential that our tests had demonstrated. They said they would have to confer with their various committees. Tooby and I came away buoyed up.

A week later we received a letter from the chief operating officer of the *America II* syndicate full of praise for what we had done but then saying that they had decided not to go ahead with such a radical project. It thus became clear that the current America's Cup races would be run without any of the ideas that I have described here.

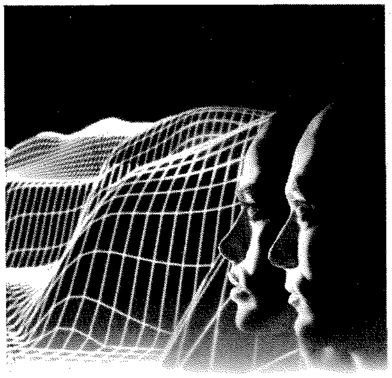
As I look back on my two years' initiation into the design of America's Cup yachts, I am amazed at the great difference between the hyperbolic claims of "high tech" innovation that appear in the press and the design of the actual yachts now contending in the waters off Perth. The designers of most of these yachts are outspoken proponents of the "2 percent school," that is, that what it takes to win the America's Cup race is a 2 percent advantage. This is being borne out in Perth. In a four-hour race, the difference between the winner and the loser is usually less than five minutes.

Still, I am convinced by our tests that improvements of tens of percent are possible. What lies behind this difference in point of view? Almost certainly it can be traced to the secrecy that surrounds the design of 12-meter yachts. There is almost no scientific literature on the subject. Each designer jealously guards his store of information. The usual intellectual discipline that exists when scientific ideas are published and subjected to the examination of others is almost entirely lacking. I have become aware of a long list of intuitive ideas that sailors and designers alike cling to that I am convinced are nonsense. Perhaps with this article I can convince others to publish their ideas and, in the process, to see how well these ideas stand up under scientific scrutiny.

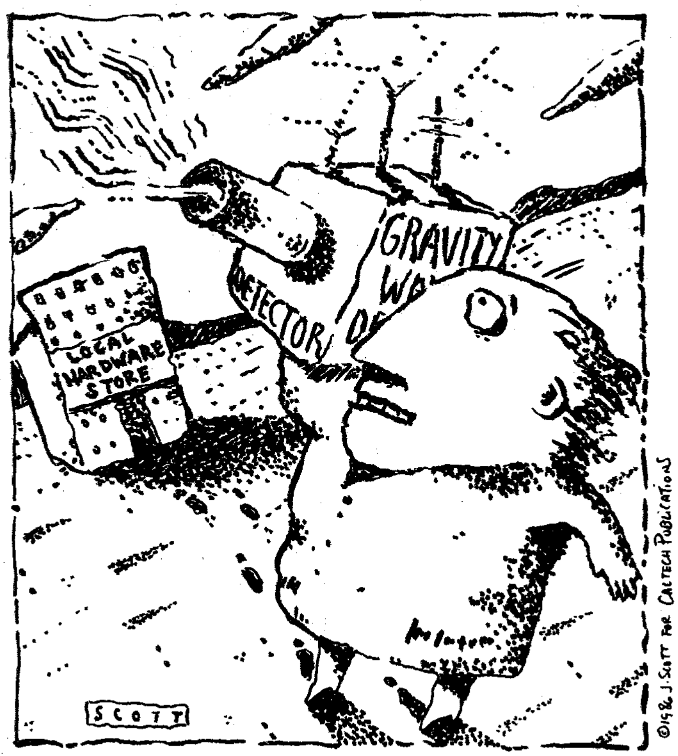
One risk I run in doing this is that publishing this article may cause the rules to be changed. Although the rules as originally written were intended to be well defined, the keepers of the rule book, being human, couldn't quite let go of the strings of control. Not in the rules themselves but in the instructions to the measurers, they inserted the following paragraph:

"If from any peculiarity . . . the National Yacht Racing Union . . . is in doubt as to the application of the rules or instructions . . . it shall report the case to the International Yacht Racing Union, who . . . shall award such certificate of rating as it may deem equitable; and the measurement shall be deemed incomplete until this has been done."

As a result, almost every innovation is challenged on the basis of this "peculiarity" instruction. The IYRU has maneuvered so that it can make quite subjective judgments as to what it will permit. The New York Yacht Club people told me that a number of decisions have been made solely on the basis of what the IYRU deemed "good for the sport." It remains to be seen whether broad beams, pontoons, ailerons, or underwater proboscises will be good for the sport. □



The Case of the Unconfirmed Phenomenon



It was cold, dark, and cloudy day—gravity wave weather. Gravity waves. Hmmm. Were they for real or was Einstein just pulling my leg? I had to find out for myself.

I stopped in at my local hardware store and asked Harvey, the high school kid behind the counter, where they kept the gravity wave detectors. He gave me a look that told me I should have gone straight to Ed, the owner. "Maybe you should talk to Ed, the owner," the kid said. Ed could be found somewhere in aisle three by the sixteen-penny nails.

"What are you looking for buddy?" Ed said as he hoisted a case of claw hammers to the upper shelf.

"Gravity waves," I replied.

"S'pose you'd be fixin' to find a gravity wave detector then?"

"That's right."

"Well I can't help you... try Caltech."

He'd been right about the weather stripping I'd asked about last winter, so I trusted him about this, too. And since Caltech was just a hop, skip, and a three hour plane flight from home I dropped in at the Institute.

"$500,000! That's a little more than I was hoping to spend!" I protested when told the tab just to house the prototype. Actually I knew that the cost was quite reasonable; it takes only chalk and a blackboard to conceive of the universe's fundamental natural phenomena, but a few million dollars to confirm them. I just figured that if I acted a little indignant I might be in a better position to haggle.

"It's not for sale anyway," the wizened professor told me, "and don't call me wizened! Caltech alumni—thousands of them—got together through the Alumni Fund and chipped in to support this project and a whole lot of other things too. You know, 'cutting edge,' 'state of the art,' 'tomorrow's discoveries today,' that kind of stuff. Alumni are our silent partners in a lot of what goes on here."

"How can I get in on this?" I said. "How can I find the Alumni Fund?"

"Don't worry," he said with a mischievous grin, "the Alumni Fund will find you. You just be ready to make out your check when they do. Now get out of here; you're blocking my laser!" And so I left.

I don't worry about gravity waves anymore. I just wait for the Alumni Fund solicitation to arrive. And when it does, I'm going to send them a check, a big one.

Because if a gravity wave turns up, I want a part of the credit for detecting it.

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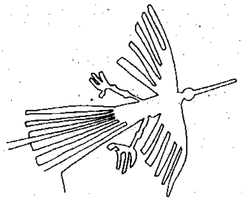
NOTE: The kind folks in Gravity Physics want you to know that none of them are wizened. They also point out that the prototype detector they are working on has two arms, each 130 feet long, and incorporates vacuum tanks, suspended masses, and several lasers. The proposed upscaling of this model will have arms over one mile long. Unrestricted funds provided the seed money for this project which, in turn, is helping Caltech obtain additional funding from the National Science Foundation.



The Travel Program Of

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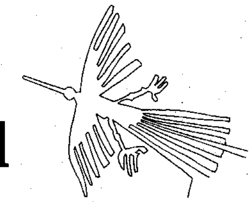

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