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The Rocket Pioneers

Memoirs of the infant days of rocketry at Caltech

by Frank J. Malina

My interest in space exploration was first aroused when I read Jules Verne's *De la Terre à 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 1933 I wrote the following paragraph for a technical English course at Texas A. & M. College:

> *Can man do what he can imagine?*—Now that man has conquered travel through the air his imagination has turned to interplanetary travel. Many prominent scientists of today say that travel through space to the Moon or to Mars is impossible. Others say, "What man can imagine, he can do." Many difficulties present themselves to interplanetary travel. The great distance separating the heavenly bodies would require machines of tremendous speeds, if the distances are to be traversed during the lifetime of one man. Upon arrival at one of these planets the traveler would require breathing apparatus, for the astronomers do not believe the atmosphere on these planets will support human life as our atmosphere does. If a machine left the earth, its return would be practically impossible, and those on the earth would never know if the machine reached its destination.

In 1934 I received a scholarship to study mechanical engineering at the California Institute of Technology. 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) ten-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, 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.

After discussion with Bollay, Parsons, and Forman, I prepared in February 1936 a program to design a high-altitude sounding rocket to be propelled by either a solid- or liquid-propellant rocket engine.

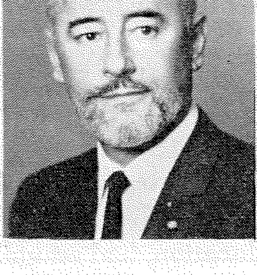
We reviewed the literature published by the first generation of space flight pioneers—Ziolkowsky, Goddard, Esnault-Pelterie, and Oberth. In scientific circles this literature was generally regarded as science fiction, primarily because the gap between the experimental demonstration of rocket-engine capabilities 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, including the results of the experiments of the American Rocket Society, that it was not possible to design an engine to meet specified performance requirements for a sounding rocket to surpass the altitudes attainable with 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 static 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 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 lab-



Frank Malina's supporting role in the drama of early rocket research at Caltech, as well as his part in the founding of JPL and the Aerojet-General Corp., is fairly well known. Less well known, however, is his enthusiasm for art and music. In the early fifties, with success in the field of aeronautics assured, Malina proceeded to achieve international recognition in the field of kinetic art—a unique combination of art and science in which paintings are luminous and mobile and controlled by electricity. He has lived in Paris since 1953, and his paintings have been exhibited throughout Europe and the United States. He is currently editor of *Leonardo*, an international journal of the contemporary artist, which he founded.

"The Rocket Pioneers" has been adapted from a paper prepared for the First International Symposium on the History of Astronautics held in Belgrade, Yugoslavia, in September 1967.

oratory before we could begin. In March I proposed to Clark Millikan that I continue my studies leading to a doctorate and 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 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. Only much later did I learn that back in the 1920's in Germany he had given a sympathetic hearing to discussions of the possibilities of rocket propulsion and that in 1927 he had included in his lectures in Japan a reference to the problems needing solution before space flight became possible. 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 to our group 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 work of our group had the benefit of advice from von Kármán, Clark Millikan, and other GALCIT staff members. We realized from the start that rocket research would require the ideas of many brains in many fields of applied science.

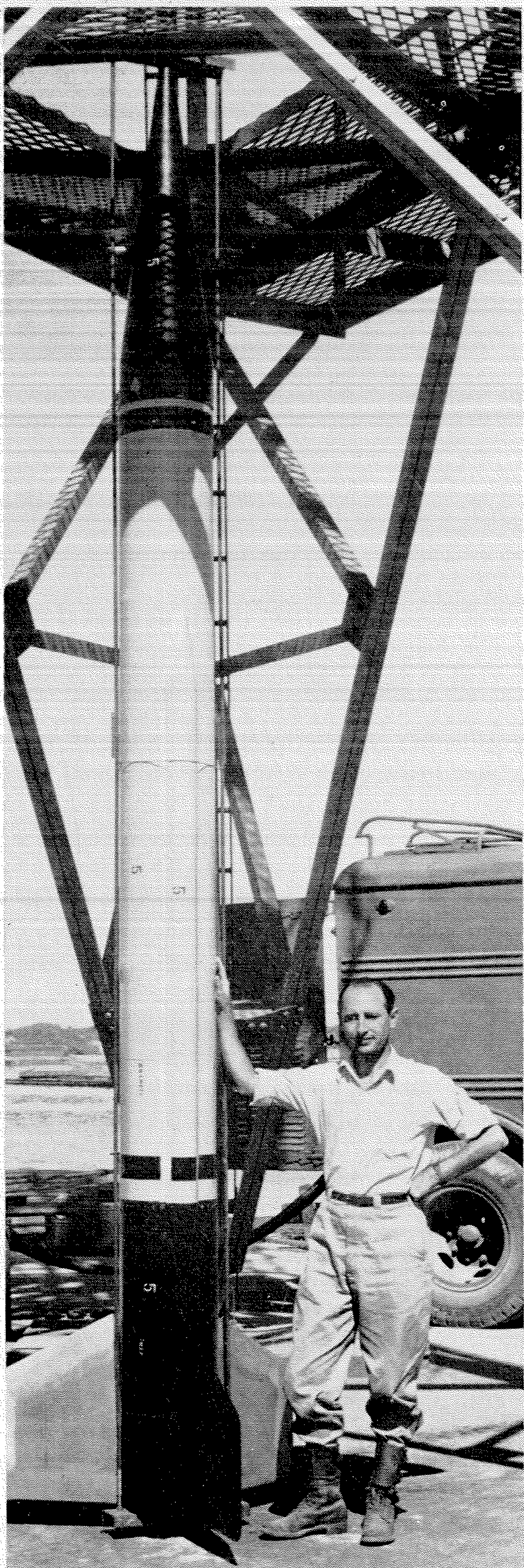
I was very fortunate at this time to enter von Kármán's inner circle of associates because he needed someone to prepare illustrations for the textbook *Mathematical Methods in Engineering* which he was writing with Maurice A. Biot. Bollay had been assisting von Kármán with the manuscript of the book, and when he left for Harvard University, I inherited his job as "caretaker" of the manuscript.

Thereafter I worked with von Kármán on many projects until his death in 1963. In a way he became my second father. We worked so closely during the formative years of the Jet Propulsion Laboratory that it is not always possible to separate the contribution either of us made to technical and organizational developments during the period 1939 to 1944.

It is necessary to point out, however, that during the period of the GALCIT Rocket Research Project the initiative rested with our group, and it fell to me to hold it together.

The group heard with excitement in 1936 that Robert H. Goddard would come to Caltech in August to visit Robert Millikan. Millikan was a member of a committee appointed by the Daniel and Florence Guggenheim Foundation to advise on the support given by the foundation to Goddard for the development of a sounding rocket. 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



Frank Malina with the Wac Corporal at White Sands, New Mexico, in November 1945.

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.

With this background to the relations between Goddard and the project, a summary of his effect on our work can be made. This appears needed, for erroneous impressions exist as to his influence on rocket research at Caltech.

As I pointed out earlier, the stimulus leading to the formation of the GALCIT Rocket Research Project was Sänger's work in Vienna. Like Goddard, our group at first believed that the most promising practical application of rocket propulsion would be a sounding rocket for research of the upper atmosphere, which was of interest at Caltech in connection with cosmic ray studies and with meteorology requirements. Actually it did not turn out this way, for the first application of rocket power we successfully made was in assisting the take-off of aircraft.

Our group studied and repeated some of Goddard's work with smokeless powder, impulse-type motors, which he reported on in his Smithsonian report of 1919. Work on this type of solid-propellant rocket motor was, however, dropped by the group in 1939 in favor of developing one of constant-pressure, constant-thrust type. Goddard's smokeless powder rocket engine did, however, find application in armament rockets during World War II.

There is no doubt that had Goddard been willing to cooperate with our Caltech group his many years of experience would have had a strong influence on our work. As it happened, our group independently initiated the development of different liquid and solid propellants from those that Goddard studied. Finally in 1944 when I initiated the construction of the Wac Corporal sounding rocket at the Jet Propulsion Laboratory, it bore little technical relation to Goddard's sounding rocket of 1936, about which we still did not have any detailed information.

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.

In November, I wrote home as follows:

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.

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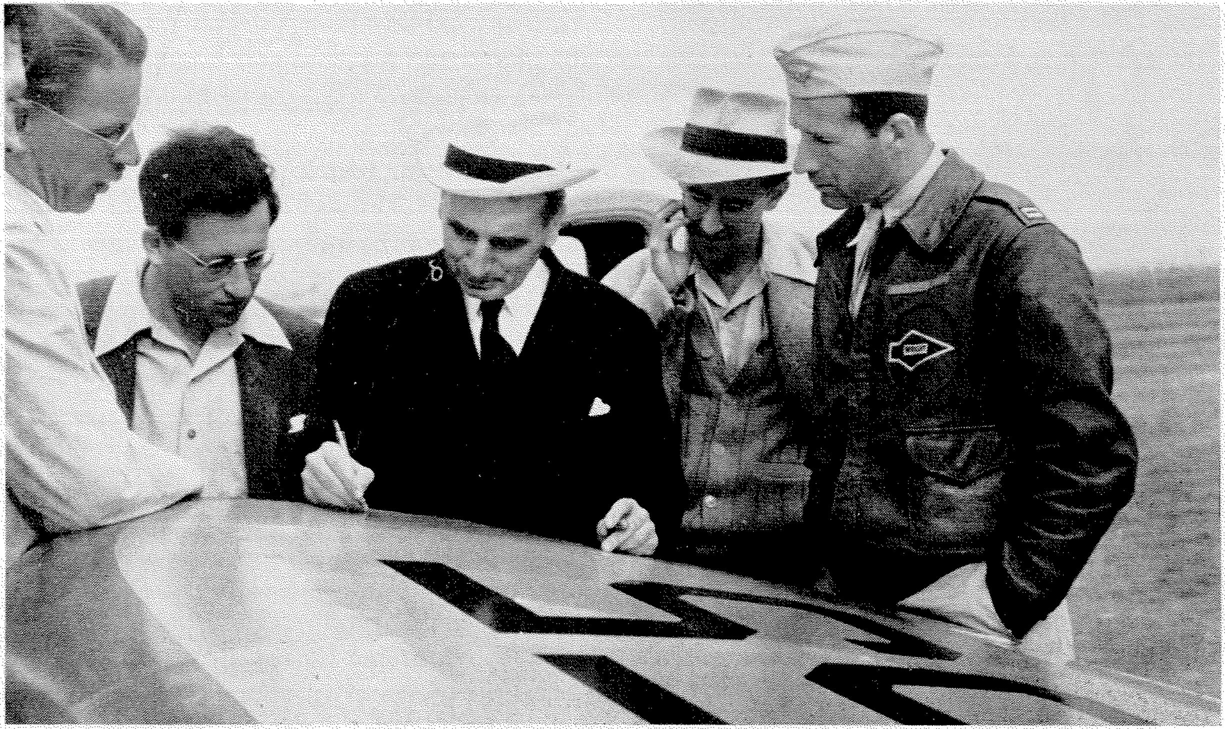
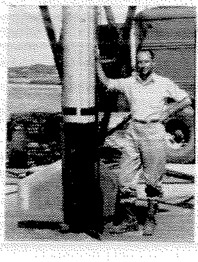


The GALCIT rocket research group makes final plans for the first jet-assisted takeoff test flight in 1941. From left: Clark Millikan, Martin Summerfield, Theodore von Kármán, Frank Malina and pilot Homer Boushey.



A number of tests were made with this transportable experimental 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 his 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.

This enabled Parsons and me to give up our effort to write an antiwar novel with a plot, of course, revolving around the work of a group of rocket engineers. We had hoped to sell it for a large sum to a Hollywood studio as a basis for a movie script to support the work of the project! This was of some relief to me, for I could then spend less time in Parson's house, where he was accumulating tetranitromethane in his kitchen.

Arnold, who commuted the five miles between 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.

What has been called the original GALCIT rocket research group was now complete. It consisted of Parsons, Forman, Smith, Tsien, Arnold, and myself. In June 1937 studies made by the group up to that time, including Bollay's paper of 1935, were collected together into what our group called its "bible."

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. It was 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 that we had placed on the lawn in front of Caltech's 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 NO_2-alcohol mixture permeated most of GALCIT, leaving behind a thin layer of rust on much of the permanent equipment of the laboratory. We were told to move our apparatus outside the building at once. We also were thereafter known at Caltech as the "Suicide Squad."

We remounted the pendulum in the open from the roof of the building and obtained a limited amount of useful information. We made the first, or one of the first, experiments in America with a rocket motor using a storable liquid oxidizer. On the basis of this experience with nitrogen dioxide, Parsons later developed red-fuming nitric acid as a storable oxidizer which is still being used today.

Although rocket research unavoidably involves experimentation of a dangerous nature, to my knowledge no one has suffered a fatal injury up to the present day at JPL. Unfortunately, Parson's familiarity with explosives led to contempt, and in 1952, when moving his Pasadena home laboratory to Mexico, he dropped a fulminate of mercury cap which exploded and killed him. It was a great loss. His work was of great significance in the history of the development of American rocket technology, both as regards storable liquid propellants and composite solid propellants.

During this period Tsien and I continued our theoretical studies of the thermodynamic characteristics of a rocket motor. To check our results, steps were taken to design and construct a test stand for a small rocket motor burning gaseous oxygen and ethylene gas. Von Kármán reviewed our plans and agreed that we could build the apparatus on a platform on the eastern side of GALCIT. In 1939 this apparatus exploded. I escaped serious injury only because von Kármán had called me to bring him a typewriter at his home. Parsons and Forman were shaken up but unhurt.

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Smith made simple experiments to determine the material from which we should make the exhaust nozzle of the motor. He describes these experiments as follows in a recent letter to me:

> Sometime, perhaps in the 1937-38 school year, perhaps before [it was in the spring of 1938], we began investigation of materials—ceramics, metals, carborundum, etc. I developed a standard simple test. I would use the largest tip (No. 10, I believe) on an oxy-acetylene torch and play it over a specimen for one minute. Some super refractories spalled and popped like a pan of popcorn and some just melted. You obtained a ½" cube of molybdenum and I tested that. It did not melt, but when I removed the neutral protecting atmosphere of the torch, before my very eyes I watched it literally go up in smoke. While cooling, it

> dwindled from about a ½" cube to a ¼" cube giving off a dense white smoke. As part of this phase you and I visited the Vitrefax Corporation in Huntington Park to get help from them about super refractories. One important refractory was forcefully brought to our attention. We watched them make mullite and saw large graphite electrodes working unscathed in large pots of boiling super refractory. This opened our eyes to the possibilities of graphite. It tested well under the torch. Later, shortly before I left Caltech in June 1938, I happened to try the torch on a ½" x 2" x 12" long piece of copper bar stock. The torch could not hurt this piece at all and this test opened our eyes to the possibilities of massive copper for resisting heat.

The first combustion chamber liner and exhaust nozzle of the motor were made of electrode graphite. Later the exhaust nozzle was made of copper. An experiment made in May 1938 at 300 lbs. per square inch chamber pressure for a period of one minute showed that the graphite had withstood the temperature, and the exhaust nozzle throat, which was 0.138 inches in diameter, suffered only an enlargement of 0.015 inch. The motor delivered a thrust of the order of five pounds.

"The word 'rocket' was still in such bad repute in serious scientific circles that it was felt advisable to drop the use of the word."

In the winter of 1938, Tsien and I also extended Smith's and my study of the performance of a sounding rocket to the case of propulsion by successive impulses from a constant-volume solid-propellant rocket engine. We had reviewed Goddard's 1919 paper on "A Method of Reaching Extreme Altitudes" and decided to find a mathematical solution for the flight calculation problem, which Goddard had not carried out. We did this in spite of the difficult practical problem of devising a reloading mechanism for such a rocket engine, for at that time no propulsion method could be discounted.

Parsons and Forman built a smokeless powder constant-volume combustion rocket motor similar to the one tested by Goddard. With it they extended Goddard's results. To my knowledge, no practical solution has ever been found for a long-duration solid-propellant rocket engine using the impulse technique.

The negative conclusions we reached as regards the practicability of devising an impulse-system rocket engine for long-duration propulsion made us turn to the study of the possibility of developing a composite solid propellant which would burn in a combustion chamber in cigarette fashion. Parsons decided first to try extending the burning time of the black powder pyrotechnic sky rocket. He finally constructed a modified black powder 12-second, 28-lb.-thrust rocket unit in 1941.

During the summer of 1938, Smith began working in the engineering department of the Douglas Aircraft Co., where he is still employed. Arnold left Caltech for New York and completely vanished as far as we were concerned. It was not until 1959 that I learned that he was a member of the board of trustees of the University of Nevada. We then corresponded until his death in 1962. Tsien was able to devote less time to the work of the project, as he was completing his doctorate under von Kármán. I struggled on with Parsons and Forman, little suspecting that in the next few months the project would become a full-fledged GALCIT activity supported financially by the federal government.

We also had less time to devote to rocket research, for we had to support ourselves. Parsons and Forman took part-time jobs with the Halifax Powder Co. in the California Mojave Desert, and I began to do some work on problems of wind erosion of soil with von Kármán for the Soil Conservation Service of the U.S. Department of Agriculture.

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.

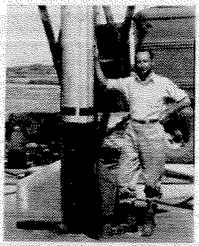
"In 1944 I proposed a jet propulsion section at Caltech. It was decided that it would be premature. Instead, von Kármán and I founded JPL."

Then in August 1938 Ruben Fleet, the president of the Consolidated Aircraft Co. of San Diego, California, 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. It was not, however, until 1943 that liquid-propellant rocket engines, constructed by the Aerojet-General Corporation, were tested in a Consolidated Aircraft flying boat on San Diego Bay.

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!

I had learned during the year of the REP-Hirsch International Astronautical Prize, which was administered by the Astronautics Committee of the Société Astronomique de France. The prize was named for the French astronautical pioneer Robert Esnault-Pelterie (REP) and the banker rocket-enthusiast of Paris, André-Louis Hirsch.

The money contributed by Arnold was rapidly being used up. In the hope of augmenting the funds of the project, I decided to enter the competition by sending a paper on some of my work. I did not learn until 1946 that the prize had been



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awarded to me in 1939. The outbreak of World War II in Europe had prevented the Astronautics Committee from notifying me. In 1958 Andrew G. Haley, then president of the International Astronautical Federation, arranged for the medal to be presented to me by André-Louis Hirsch at the IXth International Astronautical Congress at Amsterdam. The prize was then worth a fraction of its former value.

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 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 being given 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.