



**FRANK J. MALINA**  
(1912-1981)

**INTERVIEWED BY**  
**MARY TERRALL**

**December 14, 1978**

**ARCHIVES**  
**CALIFORNIA INSTITUTE OF TECHNOLOGY**  
**Pasadena, California**



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### **Subject area**

Aeronautics, Jet Propulsion Laboratory, art

### **Abstract**

Interview in one session with Mary Terrell, December 14, 1978. Frank J. Malina was research fellow (1940-1942) and assistant professor of aeronautics (1942-1946) at the California Institute of Technology. The interview begins with his arrival at Caltech as a graduate student in 1934 to begin a master's degree in mechanical engineering (MS, 1935). He then undertook a second master's in the Guggenheim Aeronautical Laboratory, known as GALCIT and then under the direction of Theodore von Kármán. Writes PhD dissertation ("Characteristics of the rocket motor and flight analyses of the sounding rocket," 1940) on rocket propulsion under Von Kármán, marking the beginning of a long-term relationship with Kármán, who became "a second father." Formation of rocket propulsion research group with William Bollay of Caltech and two men outside Caltech, Jack (John) Parsons and Edward Forman; later involvement of others at Caltech. Early rocket experiments on campus in Guggenheim create hazard, resulting in move to Pasadena's Arroyo Seco; group named "the suicide squad." Early funding provided by Army Air Corps, 1939. Malina recalls open, permissive atmosphere

in GALCIT. Support of Robert A. Millikan and Irving P. Krick for rocket development for meteorological research; skepticism of Clark B. Millikan, who later becomes more supportive. Rocket group becomes known as Jet Propulsion Laboratory, GALCIT. Malina comments on relationship of the group, later known as JPL, to Caltech; administrative changes and tensions upon Von Kármán's departure and promotion of Clark Millikan to GALCIT leadership; Malina makes day-to-day decisions at the lab. Establishment in 1942 of Aerojet Engineering Corporation to engage with aerospace industry; resistance of both Caltech and Army Air Corps to this venture. Success of Aerojet; Malina's financial gain.

Christmas, 1946, Malina departs for initial leave of absence to work for UNESCO in Paris; never returns to Caltech. Discusses reasons for change; eventual decision to pursue artistic career; interest in kinetic art. Founding of art journal *Leonardo*. After launch of *Sputnik*, founding with Von Kármán of International Academy of Astronautics. Discusses international cooperation in science. Comments on early skepticism about intercontinental missiles and satellites; key paper 1946 with Martin Summerfield on rocket escape from Earth's atmosphere. Failure of Americans to think creatively about space at that time, despite technology at hand; Russian thinking different, leading to 1957 launch of first satellite. Concluding comments on living in Paris, travel, the relationship of art and science.

## **Administrative information**

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GALCIT's rocket team, alias The Suicide Squad, gathered at the test site in Pasadena's Arroyo Seco, 1936. From left to right: Rudolph Schott, Apollo M. O. Smith, Frank J. Malina, Edward S. Forman, John W. Parsons. Photo Caltech Archives.



In the run-up to America's entry into World War II, the use of rocket engines in military aviation began to be tested. The first JATO (Jet-Assisted Takeoff) experiments were conducted by Caltech's GALCIT team with the U. S. Army Air Forces in August, 1941, at March Field, near Riverside, California, using the small Ercoupe airplane. In the photo are (left to right): Clark B. Millikan, Martin Summerfield, Theodore von Kármán, Frank J. Malina, and test pilot Lt. Homer A. Boushey. A week earlier, a test on the ground in the Arroyo Seco had sent rocket parts flying in all directions. Fortunately the pilot had been a student of Kármán's and agreed to go ahead with the airfield test. In his memoir, *The Wind and Beyond*, Kármán recounts that story and the following: "As Boushey climbed into the Ercoupe that day [August 12], I am sure we all shared a certain tingle of mounting excitement. We knew that this test was vital to convince the Air Corps that our rocket worked. But were there any other unknowns that we hadn't considered? Would there be trouble with the airplane?" The first jet-assisted takeoff was completely successful, exceeding even Kármán's expectations. It proved that big bombers, thus equipped, could operate on much smaller runways. Photo Caltech Archives. Quotation from T. von Kármán, *The Wind and Beyond* (Boston: Little Brown, 1967), p. 250.



Frank J. Malina and Theodore von Kármán at the International Academy of Astronautics in Paris, 1961. Photo by Espinat, Paris, from the T. von Kármán Papers, Caltech Archives.

**CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES**

**ORAL HISTORY PROJECT**

**INTERVIEW WITH FRANK J. MALINA**

**BY MARY TERRALL**

**PASADENA, CALIFORNIA**

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**Interview with Frank J. Malina**  
**Pasadena, California**

**by Mary Terrall**

December 14, 1978

**Begin Tape 1, Side 1**

TERRALL: So you came to GALCIT as a graduate student in '34, right?

MALINA: That is right.

TERRALL: And very soon got into work on rocket engines, from what I understand.

MALINA: Yes, well, actually I had a scholarship in mechanical engineering, but registered in aeronautics and took my master's degree in aeronautics finally. Then in '36 I took stock of aeronautical research, and decided to go into rocket research and got approval from von Kármán to write my PhD thesis on rocket propulsion and rocket flight—that was about 1937. Well, then there's a long tale of how actually the rocket research got underway.

TERRALL: I wanted to ask you about von Kármán: What was your impression of him when you first met and how did your working relationship develop with him?

MALINA: I think my first real encounter with him must have been when I took a class of his in 1935-36. At that time he was writing a book called *Mathematical Methods in Engineering*, with Tony [Maurice A.] Biot. They were looking for someone to make the illustrations for their book. I had done that kind of work before at Texas A&M; and I was working on the wind tunnel at that time, and I guess it was then they learned that I had some skill at mechanical drawing. So I was asked to do the illustrations. At that time, Bill Bollay was sort of the caretaker of the manuscript, when he decided to leave—I think it probably must have been '37—to go to Harvard. By that

time I was working very closely with them, so I became caretaker of the manuscript. I began working very closely with von Kármán, not only doing the illustrations but helping to prepare the text and to work the problems in the book, et cetera. Our close relationship continued until he died. He left Caltech I think basically in 1944, and I left in 1946. I went to Europe; he was floating all around. Then we made contact again in Europe because he was somewhat interested in some of the programs of the United Nations and UNESCO in connection with fluid mechanics and wind erosion in developing countries. I was at UNESCO, and I was working at one point on a program for arid zone research. There we came together again and organized a symposium in Algeria—I think it must have been about 1950. So we would come together and go apart. In 1960, we established the International Academy of Astronautics, and I was very busy with that thing for a large number of years. Then in 1963, he came back to Europe from America in very bad health, and I took him on his last trip—from Paris to Aix-la-Chapelle, where he died a few months later. But as I've said in one of my memoirs, he became in a way my second father. It was really a remarkable relationship, as far as I'm concerned, certainly. I don't think we ever had any major disagreement. We didn't always see eye to eye on everything, but it was an extremely congenial type of relationship.

TERRALL: He was working on a number of different things, of course. But he was following the rocket research very closely?

MALINA: Well, it got started in a rather unusual way. Two fellows—perhaps you remember their names—Jack Parsons and Ed Forman, two non-Caltech fellows, had read about a seminar that BOLLAY gave here at Caltech, and they came here looking for someone that might help them to design a liquid propellant rocket motor. They were sent to me. I was already also very interested in the subject. So I said, “Sure, let's get together and see what we can do.” So then, together with them and BOLLAY, I prepared the first program of research—a very modest program—and then went to Kármán, and Kármán agreed that we could do it. He was of course involved in all kinds of other things; at that time I think his primary interests were in turbulence theory and in supersonic flow problems. I would talk to him now and then and he would give some opinions and advice, but he had no money in the lab to give us. So really we were carrying it on in a sort of a semi-official manner at Caltech.



TERRALL: Were you teaching or something to support yourself at the time?

MALINA: I was mainly working on the wind tunnel. But then I was also working for Kármán on the book. I can't remember which year it was, but von Kármán got a contract from the Soil Conservation Service for the study of wind erosion, and I was pulled into that. So for a while I was doing wind erosion research with him for the Soil Conservation Service, working on the book, working on the wind tunnel, and trying to keep this little rocket group alive. Parsons and Forman used to disappear to go make explosives in the Mojave Desert to earn a bit of money to keep themselves alive. Also, in 1936, Amo [Apollo M. O.] Smith and Hsue-shen Tsien joined the group. By 1938, I think Amo Smith had gone to work at Douglas, and Tsien was pulled into more and more other kinds of research with Kármán; so it was left to me and Parsons and Forman, and then they departed. So I was holding the "fort" of rocket research and getting rather discouraged in 1938, when the National Academy of Sciences set up a committee for the Army Air Corps Research. One of the problems that they were interested in was the use of rocket propulsion for the auxiliary propulsion of aircraft. I gave a talk here at the Athenaeum at a Sigma Xi lunch—I guess it must have been the autumn of 1938—and I called it "The Facts and Fancies of Rockets." After the lunch, Kármán and Millikan—Robert A. Millikan—and Max Mason came to me, and said how would I like to go to Washington to make a report to this committee. So at the end of the year, I went to Washington and gave a report. Then we received the first government funds through the National Academy—I think it was a thousand dollars—to study the literature and make a proposal for rocket propulsion research. And then we were off!

TERRALL: But before that, when there were just a few of you and the other people were off a lot of the time, what was it like to be working on a project like this? Was it something that was considered kind of crazy?

MALINA: Oh, yes. It was certainly considered dubious. Most of the so-called serious scientists and engineers were very skeptical about rocket propulsion. There was also a surprising lack of understanding among people that should have known better. I can't forget when I was working on my thesis on rocket propulsion, at that time I was taking a course from Fritz Zwicky on analytical mechanics. One day after a lecture I went to him and I said, "I'm working on this problem and I'm a bit puzzled about something." Well, he was in that kind of a mood that day,

and he shouted at me and told me I was a bloody fool, that I was trying to do something that was impossible because rockets couldn't work in space. I never reminded him of it, but I'm sure he would have found it hard to believe that he told me such a thing in 1937 or so; because in 1939, he became a consultant for our Air Corps jet propulsion research project. He used to come out to the Arroyo Seco where—I think it was 1940—we built the first Project shacks, and we used to have a weekly research conference. When we'd set up the Air Corps research project, Kármán became officially the director and I was called the chief engineer. We had a research conference every Saturday morning. At some point Zwicky was pulled in as a consultant.

On the campus, before we moved to the Arroyo, we had some misfires and explosions. We tried to make an experiment on a little motor in the Guggenheim building. We suspended a fifty foot pendulum from the top floor to the basement, and on the bob we had a little motor with some nitrogen tetroxide and alcohol, and it misfired. A big cloud of this corrosive nitrogen tetroxide floated through the building. 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. There was a platform on the east end of Guggenheim at that time—I don't know what's there now; I guess there's another building. That must have been about 1939 when that blew up. It's quite possible that I might have been done in by that explosion, but Kármán had called his secretary and asked if I would bring him a typewriter to his home. I hopped in my Model-A Ford, put the typewriter in it and drove it to his house. Then I came back; and 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. But fortunately, neither Parsons nor Forman were 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 was and buried itself in a piece of wood. So if I had been there, I don't know, maybe I wouldn't be here. It was this time that we began to be known on the campus as the "suicide squad." Well, we rebuilt the apparatus and carried on some more experiments here, and then, eventually, it was moved out to the Arroyo.

We got support, of course, from von Kármán in these early days and Robert A. Millikan. Robert A. Millikan was a source of moral support for us throughout this time—also there was Irving P. Krick in meteorology. Krick headed a meteorological research and instruction group. I don't know how it was really attached to Caltech, but it was in the Guggenheim building. At that

time meteorologists were developing what they call radiosonde equipment—meteorological instruments carried aloft by balloons. Krick, when he heard that we were trying to design a sounding rocket to go higher than balloons, became very interested too. He also became our supporter. I think it's remarkable that something like this could have been done, because it was unofficial in a sense, getting only moral support; it was very informal. I suppose things like that are still happening today. Our first program certainly was in no big proposal. When I look back at the proposals we made and compare them with these elegant things that are issued now by big organizations, it looks quite primitive. But, of course, the ideas were there.

The atmosphere at GALCIT was open and permissive. Clark Millikan at the beginning—I don't know if he was skeptical of the future of the whole affair, but in 1936 when the question came up of my staying and doing my PhD with a thesis on rocket research, he told me not to do it. He suggested that this was a good time for me to get my master's and go into the aircraft industry. Well, that was a rather bleak day, I can tell you, because I had my heart set on the research. My only hope was Kármán. So I went to von Kármán and he overruled Clark, and said, "Okay you can stay and do it." Of course, after we got the first contract, there were problems. I used to have to travel a lot, and at that time I had essentially only Parsons and Forman to do this first phase of the research. Once, I remember, I came back and, evidently, Parsons and Forman didn't like something about the way things were going, and they'd gone to Clark. So I had a bit of a personnel problem with that kind of a thing. But anyway, it all leveled out. And then we went out to the Arroyo, and the establishment became set up there. Then Clark slowly got pulled into it and became quite active. But certainly in the beginning, I think that if he had had his way, there may not have been any rocket research at Caltech; but these are ifs. Certainly at that stage he didn't see the potential of rocket propulsion. In some ways, it's perhaps understandable, because there was no research being done at GALCIT on power plants. It was structures, fluid mechanics, aircraft design, and all these things, but not engines. This of course went into new kinds of engines. He might have been thinking more in the sense of what was the pattern of research at GALCIT and that there was no engine design, and maybe he didn't think there should be such a thing. But then of course, once we moved out to the Arroyo, it became more and more an independent operation; although, until I left, at Christmastime in 1946, it was called the Jet Propulsion Laboratory, GALCIT. I think it was only two or three years after I left that the acronym GALCIT was dropped. And now I understand that there's a

problem of trying to make a closer relationship between JPL and Caltech. I can see it's difficult. In the early days, we were essentially Caltech people. But now it's an enormous organization; its objectives are quite different, and it may not be so easy to interrelate the activities.

TERRALL: Well, I wanted to ask you about just that, the relationship between JPL in the early days and campus. Some place I read that you proposed at one point having a jet propulsion section of the engineering division.

MALINA: That's right.

TERRALL: And that was not approved. Why was it considered not appropriate to have that in the engineering division?

MALINA: Well, I have to refresh my memory a bit. When the Second World War was nearing an end, it became quite evident that there were two possibilities: that the Armed Services would maintain the work at the laboratory or that they might drop it; and that would be the end of rocket research at Caltech. I was worried about that; so I then did propose—and I think Kármán also made some kind of a proposal—that within Caltech, essentially the Jet Propulsion Laboratory be fitted into the academic structure. I cannot remember why that really was not done. It could have been done. By the time I left there must have been about 350 people there. It was already getting large. And it may have been the feeling that it didn't quite fit into the instruction and research of the campus. But we had been giving courses in Guggenheim on jet propulsion, and then later on there was set up a Goddard Professorship; and there is a jet propulsion section on the campus now—with Dr. [W. Duncan] Rannie and [Frank E.] Marble and others—a small unit. I don't know if it would have been better or worse if JPL had become really a part of the structure of the campus. It's quite evident that the scale of research that was going on already was quite different from anything else that was being done on campus. Of course there was the Mount Wilson Observatory and then Palomar, which were also large operations which were somewhat independent of the actual administration. On the other hand, of course, Caltech wanted to hold on to JPL because it was a source of income. As you know, over the years this has been quite a problem. We, of course, depended on the administration of Caltech, essentially for basic contract arrangements and a certain amount of help in the

supervision of the administration. Although as time went on, we had our own comptroller, we had our own director of personnel; and all this became more and more an independent operation because it just became too big to be handled, in addition to the work that these people had in the central Caltech administration.

TERRALL: What about funding? Did the contracts go through Caltech?

MALINA: The contracts, I think, up to the end of my time, were all made through Caltech. So Caltech was the prime contractor and supposedly the administrative manager of JPL. And I suppose it's something like that still. I just don't quite understand how they do work now, because it's now a NASA laboratory; it receives a budget from NASA, and then Caltech gets some kind of percentage on the budget for its management relationship. It's a bit fantastic the amount of money that must have been taken in by Caltech from JPL. I was with Dr. Pickering the other day, and I said to him, "Bill how much money do you think went into the coffers of Caltech from JPL?" He thought a minute and he said, "Well, it must be between \$20 and \$100 million dollars since the beginning." And I said, "Well, let's see if we can't get it a little closer." And I think we finally felt that it probably must have been around \$50 to 60 million dollars, which is really rather fantastic. So out of this little group that started with nothing and were using our own pocket money, it finally ended up in an affair that brought to Caltech a vast amount of income. It's really a fabulous story. There are certainly a number of stories like this one can find throughout the past, but certainly this one was an unusual happening.

TERRALL: Well, there must have been advantages to working away from campus, though.

MALINA: Well, the advantages were of course that we were getting into the kind of experimentation we just couldn't do on campus. As a matter of fact, a lot of the kind of experimentation we did with rocket engines I don't think is being done there now. I think whatever engine research that is now being done is probably being done in the desert somewhere. So it was essential that we had to leave the campus for the experimentation. What's more, we were dealing with explosives and highly corrosive chemicals and all these kind of things. So that was necessary. From the point of view of the theoretical work, of course, that could have been done here just as well. While Kármán and I were around, we had people like

Homer Joe Stewart, Duncan Rannie, Allen Puckett, many of the GALCIT graduate students, and after graduation some of them carried on at JPL, and many of them worked on campus and would come out and do some work, too. But if they took a line position at JPL, of course, then they had to spend more and more time there.

TERRALL: But there were these people who were going back and forth?

MALINA: Oh, yes. When Kármán left in '44, there was set up a JPL executive committee. I think the reason was that I was still pretty young in 1944—I must have been thirty-two; anyway, I was left holding this whole affair completely. So I think to back me up they set up this executive board and they made Clark Millikan the chairman. There was a bit of unpleasantness at this time because Clark Millikan wanted to take over from von Kármán when he left, and I said, “That’s impossible.” I just couldn’t imagine that working. And I said, “If he wants to take over the daily operations, that’s fine with me; but then he must take it over entirely.” So von Kármán decided, again in my favor, that I would take over the direction of the laboratory and there would be this committee with Clark Millikan as chairman of the board. That also caused problems. I think two or three years after I left, when Louis Dunn became the director, he succeeded in getting that committee killed. I didn’t object to the committee myself; I thought it could be useful, and we had meetings maybe every two weeks of a general character. But it again became a problem of trying to mix broad policy with day-to-day operations. And if you’re going to do day-to-day operations, you’ve just got to be there. You can’t say, come once a week or twice a month and feel that you’ve got your finger on the pulse. Sometimes on this board there would be things coming up that I just knew from day-to-day operations would be wrong, and there would be a bit of a brawl about it. So there was tension in this committee. Part of it, perhaps, was the character of Clark. He was not always, for some of us, the easiest of people to work with.

TERRALL: Were there other people on this committee who were also not involved in the day-to-day operations?

MALINA: Oh, yes. As a matter of fact, the only one on the committee, as I remember, that was really involved with the direction of the day-to-day operation was myself. It was a committee of

about six people. I was the only one actually from the laboratory; Clark, then, from GALCIT; [Romeo Raoul] Martel; and there were two or three other people—E. C. Barrett, W. R. Stott, and E. G. Dunn. I thought it would serve a useful purpose on contract problems and policies of the institute and these things. But it was quite evident to me that it could not enter into the day-to-day decisions of the research. It, I think, became so difficult after a time that Louis Dunn, after he took over from me, said he refused to live with that board.

TERRAIL: So you ended up having to battle with them all the time.

MALINA: I wouldn't say there were battles all the time. I was quite willing to go along with this. I think there was only one time I blew my top. Clark Millikan gave some kind of a report and I thought it was such a distorted presentation that I just walked out of the meeting and I said I quit. Well, then I got telephone calls from Clark and from Martel and others saying that I couldn't do that. And so I said, "Very well." But, you know, there comes a time in life when you're faced with something, and if you think you're right you have to make a stand. If you turn out to be wrong, it's just too bad. But if you feel that something is not right, and feel strongly about it, there's no point hiding it, in my opinion; it's better to have a showdown. And so, at one point, we had a showdown; and then after that, things calmed down a bit. I think there were a lot of human problems. You see, when this thing started becoming really big and important, the whole atmosphere changed and the attitude toward it, too.

Also, in 1942 we had set up the Aerojet Engineering Corporation; that was set up by Kármán, myself, Martin Summerfield, Ed Forman, Jack Parsons and a lawyer named [Andrew G.] Haley. Clark wasn't among the founders, you see. Then, after we got the company going, about a year later, Kármán came to me and says, "You know, Clark is terribly unhappy that he hasn't got any shares in the company. Do you think it would be all right if we let him have some?" I said, "Sure, let him have some." But you can see that from the very beginning, there developed a certain—I don't know how you define it or describe it—there was just something amiss, and it just carried on for a very long time.

TERRALL: Was that between Kármán and Clark?

MALINA: I think that between Kármán and Clark, I would say it was not a very congenial

relationship. As far as I know, it never rose into any kind of real difficulty; but it was quite evident, for example, in the case of deciding whether or not I should do my thesis here, which was decided by Kármán against the advice of Clark. It's one of these things that do happen.

TERRALL: In setting up Aerojet, were there problems in setting up a company with people who'd had only academic experience?

MALINA: Oh yes, sure. It was also one of those things that was difficult to decide, and again, personal factors entered into it. I had become pretty firmly convinced that if a human being can get financial independence, then he's much freer to do what he wants, obviously. So from very young years, I dreamt of some day arriving in a position where I would be financially independent. So we were working on these things, and we were having success and they were beginning to work. And Parsons and Forman, coming from outside, of course, also felt very possessive about some of this work—quite rightly. Now, what to do? We could have just let the government take what we've done or give it to someone else. Or we could try to go and see if we couldn't get contact with the aircraft industry and have some relationship in the continuation of the application of what we had done ourselves. Then—I don't know if Kármán describes this—we made an attempt to work into the aircraft industry.

TERRALL: Kármán mentions going to a few companies.

MALINA: Northrop, I think, was contacted; Consolidated Aircraft perhaps. But I know the one where we almost did something was Air Research. They actually had made some kind of proposal. They were going to set up a little division in Air Research for JATO [Jet Assisted Take Off Units] at that time. That's when Kármán pulled in Andrew G. Haley as a legal advisor. When he came out here, he said, "Well, I don't think that's a very good offer they're giving you." They were going to essentially take us on as consultants and we would give them all our patent rights. He said, "I don't think it's a very good offer. Why don't you fellows set up your own company? I'd be quite willing to join you. All we have to do is put up about two hundred dollars apiece; we'll get incorporated in Delaware, and get going." We used to have meetings at Kármán's house. He had a little house in back where we used to have regular meetings. And I can tell you, those were quite trying times; because at that time the Caltech administration—



Robert A. Millikan must have still been the chief—didn't look on this at all with very much favor. I think things are different now, but then it was felt that academic types should not try to wear the hat of the businessman. So there was a certain amount of resistance, but evidently, not a prohibition. In other words, we were not confronted with, "Well, if you guys do this, you're going to have to leave JPL-GALCIT," et cetera, et cetera—none of that. It was merely that there was great skepticism that we would be able to do something like this. And there must have been also a bit of a feeling that here we might be trying to cash in on what we did, which of course was true. Also, the military were not very happy about it. The Army Air Corps—I think it was still called the Army Air Corps—didn't like it at all. I think the story's written up somewhere. Kármán and I once were in Washington and went to see Major General Chidlaw. Of course, we had to go through them to set up this company, or at least have their blessing. He said, "Well, what are you coming in here with—your professor's hat or your businessman's hat?" It was quite evident they also were not very happy with this idea. Perhaps they just thought we were not competent to do it; I don't know. Anyway, I know at MIT of one situation where this was not done. And I think that the people involved to some extent regret that they didn't do it. I felt—and Kármán did too—very strongly that Caltech was not an appropriate institution for production. We also were watching Willy Fowler and [C. C.] Lauritsen and [Bruce] Sage and some of these others who were doing production of armament rockets. And we thought that was a great mistake because we just didn't think that was the kind of thing that Caltech should do; I mean, it should stick to basic research and instruction and so forth. So we thought that we were on sound ground in trying to split off this production and hold on to it to our own benefit. And so the company was formed. Finally, when another crisis arose, in 1952 or so, General Tire bought out all the initial stockholders, and the only one that didn't sell was myself. Well, I was in Paris. I got a call from Dan Kimball, who was then the president of Aerojet. I had the offer from General Tire offering to buy out my shares. And I said to Dan I just didn't think the offer was good enough, that I still had a lot of confidence in the future of the company. "Oh," he says, "you're making a big mistake. I suggest that you go to see a man named Smith"—who was on the board of directors of General Tire—"who's at the Ritz Hotel in Paris. You go and see him tomorrow morning." So I called up Smith the next morning—I've forgotten his first name—and I said, "I've talked to Dan Kimball about this General Tire proposal, and he says I should come and get your advice." So I went over there in the morning and explained to him. And he said,

“Well, what do you think?” I said, “I don’t think it’s a good offer; and I don’t like to let go of the company, for sentimental and other reasons.” He says, “Well, what do you want to sell for?” I says, “I don’t.” He says, “Well, don’t sell.” And the story is that Dan Kimball couldn’t get to Smith that night because there was interference from sunspots on the radio. So he couldn’t complete a telephone conversation with Smith to tell him, “For goodness sakes, put the heat on Malina to sell.” Instead of that, Smith said, “Well, I think maybe you’re right.” At that time, Kármán was at Cornell and not very well. The other remaining original shareholders were Martin Summerfield and Andrew G. Haley. I had talked to them by telephone that same day, and they told me they were not going to sell. Kármán said he was going to sell—there were all kinds of complications there that one could go into; but he said he was going to sell but that I should do what I think is best for my tax position. But Summerfield and Haley weren’t going to sell. By the next morning, when the deadline came, they sold. And I was the only one that didn’t sell. Kármán came to Europe then, after that, say six months later, and he said, “You know, you’re now known as a financial genius. I said, “Why?” because I wasn’t paying any attention to this. He says, “Have you seen what Aerojet shares are worth?” I said, “No.” They’d gone up by something like three hundred percent. Of course, the three were absolutely sick. And that’s Kármán’s chapter in the book [his autobiography, *The Wind and Beyond* (1967)], “How I lost six million dollars.” [Actually, the chapter title reads: “How I ‘Lost’ \$12,000,000. – Ed. 2008]

TERRALL: In the early days of Aerojet, did you have a lot to do with the business end of the company?

MALINA: Well, in the first setup, I think I was the secretary. Kármán was president, Haley was treasurer; I was secretary; and Summerfield, Parsons, and Forman were vice presidents. The main thing was that we had to make a transmission of the know-how from the Air Corps jet propulsion research project to Aerojet. As soon as we got the company started, Haley was released from the Army Air Force—he was a lawyer there—and he took over the company as president. And very quickly we got contracts. The timing of the thing was really rather remarkable—inventions and all kinds of things. Anyway, some contracts started coming in. Of course, we started pulling engineers into Aerojet. So then came this period of transmission of the

know-how and information from JPL to Aerojet to get these things into production. At that time, the Forman and Parsons rocket research group of 1936 left Caltech completely and went over to Aerojet. Of the original GALCIT group by then—say 1942—I was the only one left at JPL. Amo Smith was at Douglas; Tsien had gone, I think, to MIT; Parsons and Forman went to Aerojet; and Weld Arnold—the one that gave us the first money—we'd lost complete track of. I found him much later; he was on the board of trustees of the University of Nevada. I used to go over to Aerojet only maybe once a week, just for the research conferences. And then of course I was on the board, and we had board meetings and things like that. We had the first taste of what some of these big companies do—arrange a board meeting at some very nice place, like Palm Springs or San Diego. We had a taste of the luxury of being on boards of big companies. It was an extraordinary time.

TERRALL: But it was physically in Pasadena still at that time?

MALINA: At that time it was. The first office we opened was some kind of a little orange juice building here on East Colorado. Then when the contracts started coming, we shifted—we got the automobile garage and display rooms of a company on West Colorado. It stayed there for perhaps a couple of years or so and then finally moved to Azusa. Well, we already had experimental work for the company being done at Azusa, and then the whole headquarters moved out there; and then, of course, off it went.

### **Begin Tape 1, Side 2**

TERRALL: How did you feel about moving into administration? Once you started being the director of JPL, you must have had to spend much more time on administration.

MALINA: Well, there's no doubt—let's say, in 1944, when the big contract came for developing missiles, then my life really changed a lot. I still did a little bit of personal research and theoretical work and whatnot, but more and more I was being pushed into administration. Well, I'm the type of person that likes a certain amount of work that is not just manipulating people. I think I had very good relations at JPL, and I can work with people. But I don't like to do it permanently, playing the role of the front man and these kinds of things. That was one thing that I began to grumble about when the war ended—and no doubt contributed to my desire to get off

for a while. So, as you know, I left in 1946, at Christmastime, because I had an offer of a job at UNESCO in Paris. I left on a two-year leave of absence.

TERRALL: Were you planning to come back?

MALINA: Sure, a two-year leave of absence. I left with rather great difficulties because Caltech didn't want me to go, and the military didn't want me to go. When I left here I went down to my family in Texas. And I think the day before I went to France, I got another call from a general in Washington, trying to get me to change my mind. But I was rather determined, and as I put in one of these memoirs, I talked to Kármán about it, of course—he was no longer really active here. He said, “Well, I think you're right. If I was your age, I think I would do something like that, too.” I was caught up in the wave at the end of the war of hate for war, and fear of the development of the atom bomb, and seeing the things we had been developing for space exploration being used for military purposes. And so there were all these factors. Also, I was tired and I wanted to travel and see the world a little bit. So all these things came together.

TERRALL: But getting away from the military end of it was definitely a factor for you?

MALINA: It certainly was. I found that I was getting caught up more and more in trips to Washington in meetings with the army, navy, and air force, planning the next war. As a matter of fact, I made a report on the possibilities of rocket propulsion for the future to a committee of General Stilwell. He, of course, thought it was all a lot of nonsense; he didn't grasp what the implications were at all. But I found in these meetings I was getting more and more disturbed and I would break into cold sweats. I just hated the idea of, say, planning to use all this for bombarding people. So there's no doubt that that played a heavy role. I felt that the Second World War was unavoidable and that Nazis and fascism and these crazy ideas of Hitler had to be defeated. But many of us hoped at the end of the Second World War that there was a chance, maybe through something like the United Nations, to put some kind of a control on the sovereign states to put a stop to war, at least between industrialized countries. And I must say if you look at it, so far this has happened. There have been what you might call second-hand wars of the industrialized countries—there was Korea, and of course Vietnam; and things have been bubbling all over the place. But so far, we've not had a third world war. Some of us felt that if a

third world war could be avoided for twenty years, that there never would be one. Well, I don't think any of us are quite so confident now. Anything could happen, but one has to be an optimist; otherwise life doesn't make much sense, does it? So it was a great relief for me, then, to shift from that atmosphere to go to UNESCO and work for peace.

TERRALL: How did you happen to get the job at UNESCO?

MALINA: Well, I was sent on a mission in 1946 by the War Department, by the ordnance people, to snoop around Western Europe to see what was going on in the domain of interest to JPL, with a broader opening, saying I could do anything that was in the domain of science and technology if I thought it was of interest. So I went to England and visited of course many laboratories and people, seeing what the British were thinking about for the future of these kinds of things. One day, I noticed that there was a meeting of the Preparatory Commission to UNESCO in London, and they were going to discuss natural sciences. So I went there to make a report for the ordnance department. And while I was there, I got very intrigued with the discussion. The commission at that time was headed by Julian Huxley and a man named Joseph Needham—maybe you've heard the latter's name; he has written about the history of Chinese science and civilization. So after this meeting, I went up to Needham and I said, "You know, I find this fascinating. Is there any way I might get into something like this?" He said, "Why don't you come with us?"—just like that, you know. I don't know what information he had on me. So I said that I'd really seriously like to consider it. So I came back and received a firm offer to join them, supposedly again on this understanding of two years. I left Caltech at Christmastime, thinking I was going to go down to Texas and see my family and then go quickly to Paris. I sat in Texas till April, waiting for the final instruction to go. What happened there I just don't know. I guess there was a big shake-up of people going on already. Anyway, finally a telegram said to come, so I went. And after the first two years, UNESCO said, "Please stay." And I was really interested still, so I wrote to Caltech: Would they extend my leave of absence for another two years? Well, of course then I never came back. So my leave of absence has now lasted for thirty years.

TERRALL: Did the UNESCO job, then, live up to your expectations?

MALINA: It was very interesting in the first few years because it was the beginning, and I'm always fascinated by these kinds of things. But then it was also developing into administration and repetition, and I was getting really restless. We got this arid zone program launched very well and it became a sort of a model approach to be used by UNESCO and is still being used. But then it got to be committee meetings, arranging the committee meetings, and so forth and so on. So finally in 1953 I said, "Well, I want to quit." This was just at the time of the Aerojet crisis, you see. And all of a sudden I *was* financially independent. So I said to my wife, "Well, I'm going to take up one of my suppressed desires and I'm going to become a full-time artist." And so in '53, I cut loose from everything and became an artist.

TERRALL: You were painting at that time?

MALINA: I was working in graphic arts and so forth—painting and pastels. I was doing a lot with pastels actually, during my UNESCO stay, on weekends, and getting more and more interested. Already at that time, I was very interested in the possible relationships between art, science and technology. I used to complain that when I went to the museums I kept seeing paintings of dead fish and nudes and flowers and so forth, and no one seemed to be interested in all these other things that are happening in science and technology—the products and the conceptions and all these things. I had that bee in my bonnet. So I was trying to find a way to introduce this into the visual arts. This led me, then, to start working with light and kinetic art.

TERRALL: Just to go back a little bit, was painting something that you'd always wanted to do?

MALINA: I say I'm one of these children that never stopped painting, you see. There was this phase where I went into the illustration of technical books. I remember I made a pencil drawing of Guggenheim that used to be on my office wall here. And now and then I did some other drawings. Also, I married a girl from Hollywood, who went into art school, and I helped her to go through art school. So I was learning, actually, with her at that time. So my interest was very, very strong in that direction. I really had a wonderful period from '53 to '57, peacefully working on this. And then the Sputnik went up.

TERRALL: In this peaceful period, were you still in touch with rocket research?

MALINA: No. Well, while I was at UNESCO—of course, there again I went through this phase of people thinking this is all foolishness. As a matter of fact, there was an article in the *Saturday Evening Post* on UNESCO. It was sort of an attack on Julian Huxley, partly from the religious standpoint because he said the concept of God is dead and these kinds of things; and that made a lot of people angry. Also in that article it said, “They even have a fellow there at UNESCO named Malina, who’s a rocket engineer.” You see, as though it was a joke. Now, of course, communications satellites are a different story; they have a whole section at UNESCO on it. Again, you see how people can’t visualize the future. Also, at that time I wrote a manuscript for an English book that never got published—it was called *Unmanned Rockets Towards Space*. And I wrote a history of rocket propulsion for Princeton University, where Kármán and Summerfield and some others had started a large series of books on aeronautics and jet propulsion. So on the side, while I was at UNESCO, I still kept my finger in the pie. But then in ’53, I really cut loose from astronautics. And then the Sputnik went up.

TERRALL: Were you in touch with Kármán during this period?

MALINA: Oh, yes.

TERRALL: Did he encourage you to go into art?

MALINA: Well, he was not very responsive to visual art. He preferred poetry and fiction. He was not too musical, although I used to go to the opera with him sometimes. But he was hard of hearing; I don’t know how much he really got out of it. He watched me rather with astonishment I think—that I would take up visual art. I remember I had my first exhibition in Paris in 1954, and that was before I started working with light. He came to the opening with his secretary and lots of his friends—you know, he was always surrounded by a lot of people. So he brought quite a lot of people to the exhibition. I remember one of the pictures—I’d made a reclining nude, but instead of using lines done with a brush or with a pencil, I’d made the lines out of black electric cable, just by forming the cable. The frame was about so big [three feet by four feet]. So this was in the exhibition. And he came to me and he says, “You know”—I can’t remember his secretary’s name at that time—“she says that’s abstract. I’ll have to explain it to her.” Well, you see, when people expect the traditional media, when something is put into a somewhat different medium, they lose sometimes recognition of what is quite obvious, actually. I was making both

figurative and non-figurative or abstract pictures at that time; but I was also working in some of the ideas from science and technology—like shock waves and fluid flow and paintings of airplanes and rockets. And then, within the next few months, I started working with light, and then this became very exciting. In 1957-58, there was a large amount of interest in kinetic art developing, even on the popular level.

Then the Sputnik went up. So Kármán and Haley came to me and they said, “Art’s all very well, but about twenty-five percent of your time devoted to art should be enough.” And I had to come and help them set up the International Academy of Astronautics. Oh, that was a tough decision! I had a talk with my wife, I remember, one night till late in the morning, trying to make up my mind if I would agree. Because I felt that really it was the worst moment for me to cut loose from the art field. But I finally said okay. So then, in 1959, Kármán and I made the preparations for the academy. In 1960, we had our first meeting in Stockholm; and then for the next sixteen years, I must have devoted probably something like half of my time to the academy.

TERRALL: What was the aim of the academy?

MALINA: International cooperation in astronautics. At Stockholm I made a proposal for a study project for a manned laboratory on the moon called the Lunar International Laboratory. I’m still the chairman of the Committee on Manned Research on Celestial Bodies. Now with women’s lib, of course, it’s the wrong name—I guess it should be Person’s Research on Celestial Bodies.

Now, I’m again a dropout from astronautics. In between, as I worked in the visual arts, and coming from a world of science and technology, I was surprised to find that an artist did not have a professional journal of his own. There are esoteric scholarly journals for aesthetics and history of art, and semi-popular magazines like *Art Now* and *Art News* and *Art Forum*, in which the stuff is written by art writers and art critics, and the artists are not involved. So I got the bright idea to start a journal, by artists for artists—I say it’s sort of an artist’s cookbook-type of journal—where the artists discuss their work, and they can have someone to help them prepare the articles, as most artists are not very articulate verbally; and to also make a bridge between art, science and technology, and other bridges. We just had our tenth year celebration of that journal [*Leonardo*] and it’s still going. And now that takes up all my time. I haven’t made an art object in two years.



TERRALL: That's a shame.

MALINA: No, I'm not suffering. I was worked out; and I'm sure that if I got some bright idea, that I would do it. But I think in that period from '53 to '76 or so, I made I think something like 3,000 bits of art—all the way from little miniatures to rather large things. Well, I can make more; and in the kinetic art, of course, I can make more. But I've gotten so interested now in the more theoretical aspects of art that I find it very exciting. I don't know if I'm going to make any contribution, but I think the journal has turned out to be a real contribution. I just had a meeting yesterday at the University of Southern California where they're going to set up a group called TACT—Technology and Culture. They're thinking along the terms that I thought about this journal, twelve, thirteen years ago. But they want to go into the performing arts—verbal arts I guess—although I haven't heard much about that and the visual arts—but to try to bring together somehow technology and the arts. Also, they're interested in the future of developing countries, where advanced technology's being put in and the old cultures are giving way. It's quite a challenging problem. But it could be that now, all of a sudden, we are going to find *Leonardo* fitting into a program of the UNESCO National Commission, which of course I couldn't anticipate. We shall see; this should be quite an interesting year in that respect. [Note of 10 January 1980: I have been invited to participate in a Conference on Art in a Technological Society to be held at the University of Southern California between 20-26 January 1980.]

TERRALL: So in the time that you were setting up and working on the academy half-time, you were also continuing with your art?

MALINA: Oh, yes. I worked very strenuously on the art until—well, it's about two years now that I haven't done anything, so it's about 1976. The academy, of course, took a lot of my time and a lot of my money. And this journal, to some extent, has been in a way a sharing of my financial good luck, too, because I get no salary as editor. I have an editorial budget from the publisher, with which I pay an assistant editor and secretarial help and postage and so forth. But until I think maybe this next year, the budget was less than the outgo, so I've been sort of subsidizing it a bit on the side.

TERRALL: Do you still have your Aerojet?

MALINA: Still have the Aerojet—well, it's now General Tire, but I still have it. And that's what permitted me to work on the academy, which is nongovernmental—it was also without any compensation. The scientific types who work with the academy do this as voluntary contributions. We have a small secretariat of two people, and of course they are paid. But otherwise, all the other work is done by voluntary effort.

TERRALL: Were you involved in the IGY [International Geophysical Year] at all?

MALINA: No, we were not really involved in IGY; that was before the academy. That was a program of the International Council of Scientific Unions. There again you open up such a can of worms in a way about international cooperation, and especially in the domain of space research and astronautics; because there's the International Council of Scientific Unions which has a committee called COSPAR. They are supposedly into the basic sciences. There's IAF—the International Astronautical Federation—which is more toward technology. Then the academy was actually set up after the Sputnik because the federation tended to attract enthusiasts and not the so-called serious scientific types. And so the idea was to set up an academy where people would be elected, and this would provide to the federation a more serious scientific base. But there is no clear cut understanding between the COSPAR and the academy and the federation. We started out, for example, in the academy on manned research in space and started organizing international symposia on man in the space environment, or something like that. I was at Florence at a meeting of COSPAR in about 1961 or so, and said to the COSPAR people, “Do you have any intention of going into man in space?” And the reply was, “Oh, certainly not; that is not science.” I said, “Well, that makes Kármán and me and other people very happy; we will pick up that ball.” Well, of course, now they can't keep out of it either because you get into physiology and all kinds of biological problems, so it's not something where you can just say, “Well, that's not science.” That's sort of silly. So now it's not very clear exactly how these two should complement each other.

TERRALL: Well, you had actually done that report on orbiting satellites—not manned satellites but orbiting satellites—back in '46.

MALINA: That was the paper on the problem of escape from the Earth by a rocket that I wrote

with Martin Summerfield.

TERRALL: That was purely a nonmilitary application of rockets. How was that received?

MALINA: You know, I gave that paper in Paris at the International Congress of the International Union of Theoretical and Applied Mechanics. Actually that paper had quite an impact, but I used that paper also in my report to the Stilwell Committee. And there it really was not understood.

TERRALL: Where you were talking about the long-term possibilities?

MALINA: About the possibility of escape from the Earth, sending out instruments and so forth, and then of course into the satellite direction. And of course, it also meant that you could have intercontinental missiles—that was not generally understood. And perhaps you've heard that there were people in key positions in Washington in the late forties who were convinced that intercontinental missiles were a lot of nonsense, for various reasons. Of course, they were wrong. I think in the Soviet Union they've had a clearer direction on this than we have in our country—perhaps for some kind of philosophical reasons. I think there they started thinking in terms of satellites before we did here. I think that if there had been an understanding and a reaction, say, to that paper and those in the next two or three years here, that we would have probably gotten a satellite up four or five years earlier than the Soviets. But the mind was not there. And you know Eisenhower made fun, saying that the Sputnik was just a grapefruit or something—and that was '57, you see. So in the upper echelons there was still a complete lack of grasp of the implications of all these things. But that, I think, was probably the first paper after the Second World War that really gave an estimate, which was of course very conservative. We were extremely conservative. But even being very conservative it showed that it was going to come; and actually, it's turned out to be to some extent easier. But I think it's always better to be conservative than overly optimistic. If you're overly optimistic, you have disappointment. Certainly for the kind of rocket we visualized to send up, twenty-five pounds like the Explorer sent up, the amount of propellant that was needed was much, much higher than actually turned out to be necessary. But this is all right; if you can do it better, it's all right. But that's what we showed, you could do it.

TERRALL: Did you ever consider moving back to the States, once you got established in Paris?

MALINA: Well, you know, there are all kinds of human problems. I met a British girl at UNESCO and we got married—this was 1949. In 1950, our first boy was born, who is now at Berkeley in space astronomy. In '52 we had our second boy, who's just come out of the Peace Corps—he's been two years in Zaire teaching mathematics and physics in French in a university there. So I sometimes make the remark that we stayed in Paris as a compromise—I'm American, she's British, we live in France. Of course, also, she's close to her family, and I think wives tend to hold onto their families a little bit more than husbands do, perhaps with good biological reasons. And also, from the point of view of international cooperation, Paris was a very good place. I was representative, for example, for the International Astronautical Federation to UNESCO for many years. And from the art point of view, of course, it's as good as anywhere. Paris is a nice city; the French have been nice to us and good to us. It's expensive now, but all big cities tend to be expensive. I say I'm a space man in a way—I'm not very geographically conscious. I think what's important to me is if I can do what I feel I should do. Take this journal—I could do the editing of this journal here in Pasadena, I'm pretty sure. It's rather far off from my publisher who is at Oxford—I couldn't get any other publisher—so that holds me there also a bit; the printer is in England. And with the mail as it is today, it poses problems. So from the point of view of the production of the journal, it's still a good place. But if something should come along and my interests should shift, well, I don't care where it might be.

TERRALL: Well, you probably travel a lot anyway, don't you?

MALINA: Oh, now we travel a lot. I imagine we're on the road about four months a year. I usually carry work with me. But we are trying to see a lot of the world. We were in Indonesia, Bali, Burma, and Thailand last February. I'd met someone in one of these astronautical congresses—as a matter of fact, in Anaheim here—from Indonesia. I said, "You know, we've always dreamt of going to Bali." He said, "Well, my wife has a house in Bali." So, when we knew we were going to make this trip, I wrote to him and said we were going to Bali, and I wondered if his wife still had the house. I got a letter back saying, "Would you come to Djakarta and give us a talk on the past and future of astronautics?" So I said okay. So we went to Djakarta. Unfortunately, he was on a trip to America, so we didn't get to see him there at all.

We got quite interested in the country and whatnot. Anyway, we've just given to the Indonesian NASA—we were overwhelmed by journals of all kinds—so I've given them about six hundred pounds of aeronautical scientific journals for their library, which has been quite an interesting experience. I think they're sending these journals from Paris in the diplomatic pouch, about five pounds at a time. So Paris is a base. My mother and my sister are in Texas—we were just down there. My mother comes usually and spends the summers with us. She's now eighty-nine, so it won't be much longer. But we still hope she'll come next summer. So there you are.

TERRALL: How has your background in science and rocket research affected your relationships with other artists, for example? How do artists think of you as an artist and as a scientist?

MALINA: Well, at times, one finds oneself in a situation—you keep hearing that you shouldn't be too narrow in your outlook on life and in your work. But basically, most people like to have you in a clear-cut cubbyhole. So sometimes I find that my former colleagues in scientific and technical research regard me no longer as such, and that now I'm an artist. Among the artists, they find it very difficult to accept me as an artist and they say I'm an engineer. Well, okay, it doesn't really much matter. But I suppose sometimes it causes some prejudice, among art writers, art critics, and the commercial art world, since they are rather protective too. I suppose when they see someone coming from a different domain and becoming active, that there is a bit of suspicion. On the other hand, certainly there was a phase of this interest in the impact of science and technology on the visual arts and other arts. And at that time, of course, this was taken into account. But I don't suffer from that really. Again, you see, if I didn't have financial independence, it would be a different story; because then you have to start playing a different game. Even with this journal, since something like this didn't exist before, I'm getting not very much support from the commercial art world, or from museum director types, or from art critic types; but a lot of support from the direction of the aestheticians, and to some extent art historians and these people. Because in the commercial art world these people make their living by writing articles for these kinds of things, and as an interplay between advertising and galleries. And, of course, artists sometimes are not very happy. We have no advertising, for example, of living artists because we feel that's unethical in a professional journal. In the art journals I get, if I see a big advertisement in the issue, I can almost be sure there'll be an article

in the issue by somebody about this artist. It means the gallery essentially is supporting the art magazine by paying advertising to get that article printed. And that can sometimes lead to a little bit of doubtful value, but that's the way it is.