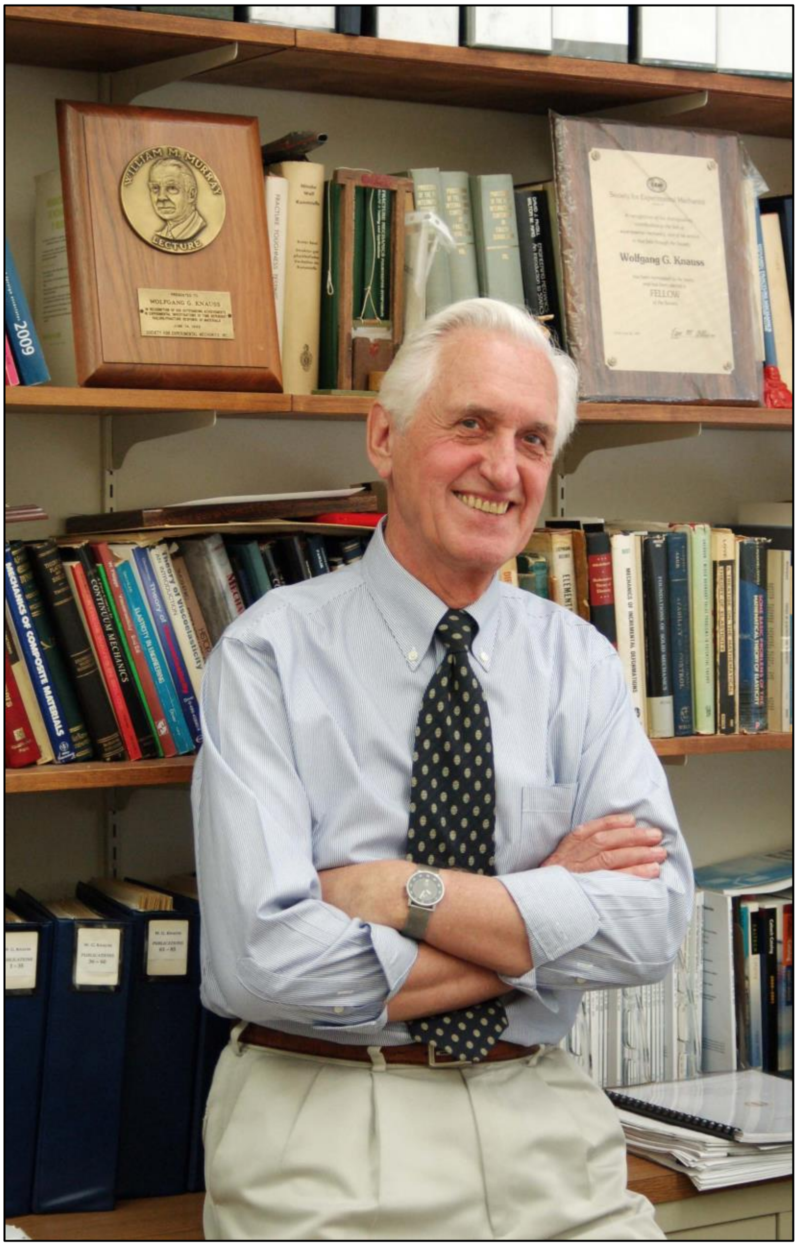
CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH WOLFGANG KNAUSS

BY HEIDI ASPATURIAN

PASADENA, CALIFORNIA



Wolfgang Knauss in his office in Firestone, 2012. Photo by Vicky Chiu.

NOTE TO READERS

Oral history interviews are historical documents that provide valuable first-hand testimony of the past. The views and opinions expressed in them are those of the interviewees, who describe events based on their own recollections and from their own perspective. They do not necessarily reflect the views or official history of the Caltech Archives and Special Collections or of the California Institute of Technology.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with Wolfgang Knauss **by Heidi Aspaturian**

Pasadena, California

Session 1	**September 23, 2019**
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SESSION 1

September 23, 2019

ASPATURIAN: This is September 23rd, 2019, interview session No. 1, with Professor Wolfgang Knauss. I usually start by asking about your family background. So, tell me a little about your ancestry; what you know of it.

KNAUSS: Start with the birthdate maybe: December 12th, 1933. Which was in the year the Third Reich began.

ASPATURIAN: Of course.

KNAUSS: My father and mother are from the area of Heilbronn in Swabia. My father was a Methodist minister, which put me in a kind of a special class in German youth, which we'll come to a little bit later.

ASPATURIAN: Was that a family occupation?

KNAUSS: No, my grandfathers on both sides were farmers. My father's father was not terribly well-to-do, so my father's early life, until he went to school for the ministry, was kind of tough; he was a laborer. But being a Methodist minister in Germany was a standout profession, and not in a positive sense. There were only about 80,000 Methodists in Germany.

ASPATURIAN: The chief Protestant denomination being Lutheran, I imagine.

KNAUSS: Right. When I was an older teenager, my father struck up an acquaintance with a Lutheran minister in Heidelberg, and that pastor was amazed that the religious content of his beliefs and the Methodist beliefs were indivisible. “They” didn’t know what Methodists were. Well, we’ll come to that in a bit more detail later. So that was my father’s profession. My mother was continually a housewife. I had two older brothers, and when I was born, my father was a minister in a little village not too terribly far from Frankfurt.

Wolfgang with his parents, and two older brothers in 1935, in front of his father’s congregation hall.

It was in the Nahe district, which is a good wine country—wine connoisseurs will know the Nahe district—near a larger town called Kreuznach [*Bad Kreuznach*], and within a year after I was born, my father was moved to Düsseldorf. We were in Düsseldorf for three years, and then we moved to Siegen, a more industrial town in the southern tip of North Rhine-Westphalia. You know Siegen, or you've heard of it?

ASPATURIAN: No, but I've seen it in connection with your biography. So, I'm now familiar with it.

KNAUSS: Okay. We lived there for thirteen years.

ASPATURIAN: How large was his congregation, if you recall?

KNAUSS: I would estimate that on Sundays, we had typically fifty, sixty people in church. And the Siegen district is an area where there were many smaller Protestant denominations, and so there, the Methodists were not really out-of-line, so to speak. There's one denomination that the Methodists joined with maybe ten years ago; I forget the name—it was something like United Brethren. [The American branch of the Evangelical United Brethren merged with the Methodists in 1968 to form the United Methodist Church. *–Ed.*] At any rate, I grew up in this industrial town. The population at the time was maybe 50,000 or 60,000, I would estimate—not very large. What was most memorable for me growing up was how to live under Hitler in a family that didn't support Hitler. I may have to say something about that.

ASPATURIAN: Yes, please.

KNAUSS: Because I often sensed after I came to the United States that the mere fact of being German indicated something about my attitude towards Jews. It might be important for some people to be reminded that you cannot generalize about that. When I was a boy, my father talked—very guardedly, I must say—about the elections that went on in our local village where I was born, and how the village council announced that there was 100 percent voting support for Hitler. I remember my father later laughing about that a few times and saying, “That can’t be true.”

ASPATURIAN: This is when you were a young boy.

KNAUSS: Yes. The elections must have been just about the time of my birth. I think by the time I got that message, I must have been older than six.

ASPATURIAN: But still very young.

KNAUSS: In our home when my parents talked about something that later I could associate with being precarious, we continually had the admonition, “Don’t talk to anybody of this outside.” Because we knew of very minor missteps—in my consideration—that landed people in jail. This is something I’ve always felt throughout my whole life here in the United States—that Americans don’t appreciate what it’s like to live in an authoritarian state. You simply cannot act in that environment like you do in the United States. So, this is the way we grew up.

When I was six, I was somewhat proud that I could join the Hitler Youth because you got a belt and you got a tie with a special knot. That, for a six-year-old, is really something, but I soon learned it wasn’t really for us because we were not part of the Brownshirt [Nazi Storm Troopers] crowd, and that was well known in the neighborhood. Even though I believe I kind of stood out in some capabilities, for example, when it came to what you called *Geländespiel* in German, which translates loosely into “Scouting Game.” [The American equivalent would be “Capture the Flag.” *–Ed.*] This was a competitive group game for which you went out into the forest, and you mimicked soldiers and garnered information about the “other side’s camp,” or something like this. I remember I was at the top of the list of winning for our team by garnering their flag, but I never got the prize for it. It was simply not considered kosher for a Methodist to get such a prize. That’s how I interpreted it.

So, those were some of the experiences that made my next older brother and me not appreciate that system very well, and so we began to avoid the weekly meetings of the Hitler Youth. My mother told us that one day a Hitler Youth leader, who must have been only fourteen or fifteen, threatened her with what they would do if we didn't show up; and she just hauled out her hand and said, "If you don't disappear, I'll slap you." He never came back. Because my brother and I had taken to hiding in a place in the house that not even our parents knew about. We just didn't like marching and doing this kind of thing without being truly a part of the group.

My oldest brother was by this time older than ten, which classified him for an older group of Hitler Youth, and they had their meetings on Sunday mornings. And so, one of *his* troops showed up at my father's doorstep and said, "Your son isn't here." And my father said, "He can go anytime during the week; Sunday morning is Church service." We never heard from them again, either. So that is essentially our experience in terms of interacting with the political system of the time.

ASPATURIAN: After Germany started the war in Europe, and then it became a world war involving the United States as well, do you recall what the atmosphere in your community was like?

KNAUSS: What I do remember strangely enough—this was '39, so I was just close to six years old—is that it was a Sunday morning , and the parishioners came to the church and as they walked out after the service and shook my father's hand, they talked about the fresh war, and generally it was with a kind of dismay. In contrast to the first world war, where I remember hearing that it was a big thing for the young people to say, "Ah-ha, ah-ha, Germany, we'll go!" As a matter of fact, I had an uncle who was legally not old enough to join the army but volunteered. His parents couldn't deter him, and after a year he was dead in France. So, that was something that was always with us. Then for about three or four years, the war was basically the only news, and as a youngster you're not really on top of it, but you got the sense that at first there was kind of a positive attitude amongst people: "Yes, see what the Germans can do!" But then when the time came that the troops had to retreat, nobody talked about it.

ASPATURIAN: From the Soviet Union, that area.

KNAUSS: Yes. And then especially when the Americans landed—

ASPATURIAN: D-Day.

KNAUSS: There was not much of a telling to the German public what was going on. And you still feel that today because people about my age don't really know what went on in the battles in the Ardennes ["Battle of the Bulge"] in France, and they can't appreciate the manpower loss that America put into the war to save Germany. I've always regretted that. But that's part of the politics of the Nazi regime. According to them, the German side was always winning. The little flurries that they won toward the end of the war—they played those up big on the scale, but the bigger picture wasn't there. And you could not discuss it. If you said we're losing—

ASPATURIAN: Someone would report you.

KNAUSS: Yes, and you might end up somewhere.

ASPATURIAN: Did your father as a clergyman come under pressure to conform to precepts of Nazism? I know there were major efforts to co-opt the churches.

KNAUSS: I was too young to recognize all the faces in the congregation, but my oldest brother, who is four years older than I am, knew that there were people from the Gestapo who appeared periodically in the congregation on a Sunday morning. The Salvation Army, as I remember, was closed, but the minister who was its captain became close with my father, because when my father had to go and hold service in another part of the district, he would come and preach at our church. That was the situation there.

ASPATURIAN: Of course, you were very young, but do you recall whether there was any understanding or awareness of what was happening on the eastern front or in the concentration camps or in the occupied countries?

KNAUSS: The topic of the concentration camps was off limits. You didn't talk about it. We learned later that people knew more about it than we heard as children. But as children, we did not hear about it because it was dangerous simply to talk about it and acknowledge it because it would be taken as offensive to the government. [*WK subsequently added:* I learned as I grew up, that the people in the close neighborhood of concentration camps knew, of course, of their existence and more of what was happening there, but certainly more than we did. There was no concentration camp within 200 or 300 miles of us.]

ASPATURIAN: So, there was an awareness that Jews and Slavs were being transported to these places?

KNAUSS: Yes. [*WK subsequently added:* That is something I did not know about until after the war when the American Forces fostered the relevant education. There was a fairly scary control over what people said or let out that they knew.] Each block of houses had a block leader who had the order to listen to what was going on. It was like the East German spy system [*Stasi*]. And so, we got wind of it as children, but we were careful. So, those were basically the important impressions I had about the political system.

ASPATURIAN: You may be too young to remember this, but I'm curious. What did your father, as a practicing Christian and a leader of his religious community, find to say to his congregation on Sundays in this environment?

KNAUSS: I don't think he was penalized in any way by the congregation. There were some people who had to join the Nazi party because of their jobs, and for the time being kept their distance from going to services Sunday mornings. But I also know of someone in particular who was a very intellectually ripe person who very much regretted how that went—but you didn't have any opportunity to avoid playing along. He was high up in the City administration, and if you did not join the Party in such a situation, you simply lost your job—it was a very heavy penalty to pay. I always admired people in the eastern zone [i.e., Communist East Germany] who would not join the equivalent of the Hitler Youth.

ASPATURIAN: You mean the Communist youth groups?

KNAUSS: Yes. I forget now what they were called.

ASPATURIAN: I can't remember what were called either, but of course yes. [Free German Youth—*Freie Deutsche Jugend, FDJ*]

KNAUSS: If they did not join the youth group, they couldn't study at the university. I thought that was a very difficult situation to be in. Growing up, I was not old enough to be aware of that happening in our surroundings.

Left: Young Wolfgang in his first year of grade school. Right: The Knauss family home attached to his father's Methodist Church in Siegen. The living quarters are on the right; the assembly hall for Sunday Services on the left.

ASPATURIAN: Did you recall whether there was any talk when the plot against Hitler failed? Did any of that percolate down?

KNAUSS: Yes, you heard about that. But I have a poor recollection because somewhere around 1941, you got this *Volksempfänger*, "people's receiver" [a radio that was wired into the German propaganda ministry under Joseph Goebbels], and if you wished to listen to anything else such as underground or Allied broadcasts, you had to have an old radio. I don't know whether my father ever listened to English channels—he had a little bit of

English from his seminary days. But I don't know if he could speak or really understand. But I remember him laughing at the so-called "people's receiver." [*WK subsequently added:* Yet that was all we had because, as I remember, everyone was supposed to turn in their "old" radios for "recycling" in the war effort. I do not think my father did.]

ASPATURIAN: As the war ground on, rationing and so forth must have started to affect your family. What do you remember from that?

KNAUSS: Yes, yes. Oh, I remember that you got stamps for each food item, and they got torn off when you went to the store, along with the payment. Milk got scarce. Butter and all these kinds of special items also. Until after the war was over, I think bread was usually to be had. I do remember that we had to walk a long way to get to vegetables. But it was worse after the war was over, because then things really became tough. I remember, as one example, that the Americans brought in an unfamiliar flour—maize flour, basically corn meal—so the bakers tried to bake their bread just using this instead of regular flour, and it was simply a solid mass.

We were fortunate that my mother came from a well-to-do farm. She rented her inherited land share out to her brother, who was working it, and so we could get apples. My father and my oldest brother would go like 200 miles away by bicycle to take apples off the tree, crate them, and put them on the train. What is amazing is that during that time they all made it to us. So, we had maybe 300 pounds of apples one year. But that's kind of the effort you had to put forth. We got oil the same way and a sack of flour, so it helped. Whereas other people would have to go to the farmers in the neighborhood and basically beg. I tried that once because friends wanted to do it, and I thought the experience was horrible. We didn't get a single potato the whole day. That's the way that life went.

ASPATURIAN: What would you say the general mood was like during the war years?

KNAUSS: I don't know that I—

ASPATURIAN: Children don't necessarily—

KNAUSS: People followed the war, but I think the everyday life took precedence over all the worries. We were safe until December ’44.

ASPATURIAN: In other words, you didn’t experience much of the war directly.

KNAUSS: Yes, not directly. Except that maybe the year after the war started, there was one Sunday morning when all the so-called free denominations like the United Brethren, Methodist, and Baptist all came together. So, we had to go across town from our homes in Siegen, and as we went through the center of town there was a prominent church where the soldiers would go for service if they wanted to. So, they could do that.

ASPATURIAN: This was a Lutheran church, I assume?

KNAUSS: A Catholic church. There was a concordat between the Nazis and the Catholic Church, so they were a little bit freer. At any rate, on this particular Sunday, the soldiers came through in their controlled lines, and just then three airplanes came over, turned around, and dropped bombs not very far from the center of town, not far from where we happened to be on the way home. That was the only experience for the next three years that I can remember. But then, because Siegen, where we lived, was basically an iron manufacturing town, the Allies thought it was important to knock out the iron and steel for the machinery facilities, and we got bombed out on two occasions. In the first one, we simply lost the roof on the house and because of the local iron industry, my father could get corrugated roofing sheet metal. By the time he had finished the roof over our house, it was in December and icy cold. Sheet metal in the winter is not very good to handle, but he got the job done—and a night or two nights after that, a big bomb was dropped behind us, where the congregational hall was. That was a terrible experience.

Fortunately, after that bombing raid, which happened during afternoon school time on a Saturday we could hustle home along the river where there were only burning houses on one side and get to a bunker [bomb shelter] that we had within about 300 yards of our house, and we and our parents were safe there. From then on, we couldn’t live in our house anymore.

But from then on, living was kind of tough. It was my first year in the

Gymnasium [academic secondary school in the German educational system], and because the local schools had been knocked out to some extent by bombing, the boys had to share the school building with the girls, and so we had to go across town to school. We were fortunate that one member of our congregation had moved out of town. The husband who wasn't home from the war yet had been a really strong supporter of the Nazi regime; the wife was maybe more flexible in that regard; but in any case, she went to her parents in the countryside and that left her apartment free. And so, she offered it to us, and we could live there for about two years while my father rebuilt the church-owned building, where we then lived. The children, of course, had to help.

It was a three-story building, and he converted the apartments on the ground floor into a service hall for Sunday services. And we lived in the second story. I was only eleven years old, and I wasn't terribly concerned what things were happening.

ASPATURIAN: Was Siegen bombed frequently? I know you were not far from Cologne, which came under heavy RAF bombardment in the last part of the war.

KNAUSS: It was basically those two times. After that, every night a bomber flew over, and we didn't know where he would drop his bomb.

ASPATURIAN: I see.

KNAUSS: So, there was this disturbing fear. I remember as a ten, eleven-year-old, I was terribly frightened. Especially when the alarm sounded. We were, by then, living up on a hill, and we had to go down the hill to the nearest bunker, maybe a half a kilometer away. I remember certain nights when it was raining hard, and at night you were not allowed to have a light—you didn't have batteries anyway. The streets were littered with bomb craters, and so you walked on these little, slick paths between bomb craters in the dark. So, to make it to the bomb shelter, that was frightening.

ASPATURIAN: This is when you were living in this apartment?

KNAUSS: Yes. It was on the top of a hill.

ASPATURIAN: The war ended in ’45. Do you recall that event?

KNAUSS: As a matter of fact, yes. We were by that time mostly living in that bunker hewn into that hill, because we were constantly afraid. The Americans had moved their artillery not far from Siegen and were shooting above us into the center of town, and I do remember that the American troops came up from the area where the guns were stationed. Let me interject something here while I think of it.

ASPATURIAN: That’s quite all right.

KNAUSS: By that time my oldest brother was fifteen or sixteen. The Brownshirts went through the bunker and looked for all the male teenagers in that age group, and my father said to my brother, “You do not go. Sit here, under this bench.” And a man went through the bunker—I remember that—with a pistol in his hand to drive these youngsters out, and my oldest brother was spared because once this Brownshirt was gone, he didn’t come back anymore. But then when the American troops were close, another group of people, along with my father—because they knew he spoke a little English—went with a white bed sheet up the hill to meet the Americans. That’s all I remember; I don’t know what they talked about. They wanted to impress on the Americans that there was nobody on the hill who was going to oppose them.

ASPATURIAN: No one was going to open sniper fire.

KNAUSS: Right.

ASPATURIAN: Was there much apprehension about being occupied by these Western troops?

KNAUSS: Nobody knew what to expect—well, the propaganda was that the Americans are terrible. They’re doing all these horrible things. The Americans *were* very sensitive to and afraid of sabotage. They had all these telephone lines, bundles and bundles of cables. If you grabbed them in a round packet, they were something like maybe six or seven inches across. For them, that was a lifeline. And the attitude was, “If we find anybody playing with those, we’ll shoot them.”

I do remember that when they came down the hill, there was one soldier in front with a gun in his arm, and the next ones were maybe fifty yards behind on foot. The road was torn up, so they couldn’t drive in. It took another few days before we could go from our current location down to where our destroyed house originally was. There was still fighting going on there. My father went down there, and he came back and said, “I don’t like these bullets flying back and forth.” But then after a week or so, I would say, things were quiet. And then the hard times started with eating, clothing.

ASPATURIAN: Were you in the American zone or the British zone?

KNAUSS: We were first in the American zone; then it was turned over to the Belgians.

ASPATURIAN: Oh, interesting.

KNAUSS: It’s not fair today to say, but as children we laughed about the Belgians, saying that when they heard a shot, they ran behind a building. What’s more natural than that? But as children we didn’t understand, because German soldiers were supposed to be macho. After the surrender, it was basically survival mode. For example, where do you get your clothing? I do remember that in Siegen, which lies on seven mountains, there were at least three barracks for soldiers.

ASPATURIAN: For the occupying troops?

KNAUSS: No, first for the German soldiers and then of course the Americans didn’t go into these, but once the war ended, they were plundered by the population because there were all sorts of supplies in them. I remember my father bringing home two, maybe even three, green soldiers’ uniforms. My mother took them apart piece by piece and stitch by stitch and cut clothing for us boys. She was a good seamstress.

ASPATURIAN: It sounds like she was a very accomplished person.

KNAUSS: I never minded to have those clothes on me. They were orderly. But that was kind of how things were for at least three, four years; it was tough.

ASPATURIAN: The late 1940s.

KNAUSS: It was in 1948 that the financial system changed over to the new currency, because the old currency wasn't worth anything anymore. People would get on the city tram and pay with stamps because they had monetary values printed on them. Paper money wasn't worth anything, because no one took it as payment for goods.

ASPATURIAN: During this period, how did your education proceed? Were you and your brothers able to go to school in what we would consider a fairly normal way?

KNAUSS: Yes. I think I had entered *Gymnasium* in May or June of '44. We lived right next to a Catholic grade school—our fence bordered the school—but we couldn't go there before entering the *Gymnasium*.

ASPATURIAN: This was your home?

KNAUSS: Yes, our original home. So, we had to go about three blocks away into a Protestant school, which ceased to be Protestant after the war was over, with the new constitution, I guess. But I was only in *Gymnasium* six months or so, by the time we were bombed out, now that I think of it. The school building was not destroyed, but instruction was suspended for about a year because so many other things were happening. I felt the impact of that all the way through my high school education. Because of all this, I graduated a year older than most youngsters at that time. I was twenty, and typically high school was over at nineteen or something like that.

ASPATURIAN: How did your academic interests develop as a child and a teenager?

KNAUSS: I've never forgotten what my thoughts were and how hard it is for a youngster to make a choice—a correct choice—for a life profession. I was somewhat artistically oriented—inclined; I was good in drawing and painting. And for a while I thought I might become a fashion designer. Mentioning it to people they would laugh—so that wasn't very good support. [Laughter]

ASPATURIAN: This was when you were, like, an adolescent?

KNAUSS: Yes, right, fourteen, fifteen. But then I began to think I would like to do something like designing cars, but then it seemed to me that there were already too many designers for cars, so—"If I stake my goal a little bit higher, maybe I can design airplanes. They're sleek things; they look good; they're something." Little did I know that airplanes are governed by a different control center than sight. [Laughter] That idea was still there when I was in the last years of high school, and I thought. "Well, maybe if you want to design something like airplanes, you've got to know something about engineering." That's how my thinking drifted over to engineering, with the idea of going into aeronautics.

ASPATURIAN: You must have had a natural aptitude for math to be comfortable thinking about this.

KNAUSS: I think I was good in math. I think that's what got me into Caltech. I remember that when I took the exam to transfer from Pasadena City College to Caltech, I did not do very well in physics. We had a teacher at PCC who had taught physics at Caltech as an assistant professor. He was very strict, so I thought maybe it's a very good class. I didn't take a course from him directly, but from one of his subordinates. But I know I didn't do well because I had to take freshman physics over again when I came to Caltech—but I didn't mind. I think I did do well on the math test. I remember that it had ten problems on it, of which we were required to do seven. And I remember I did all of them. I felt very good about it—that I had done them right. My guess is that made a—

ASPATURIAN: A difference.

KNAUSS: I was also something like a good two years older than the average student seeking admission at that time. So maybe I was a little bit more mature. There was also an essay that we had to write. As a youngster about the time when the war started, I acquired a friend who developed into a very good pianist. So, I was always exposed to classical music, and I became very interested in it. My brothers had music lessons, but my parents thought I wasn’t really inclined that way—but I was the one who carried it on ’til today! [*See Appendix A:* “WGK’s Musical Life”] So, Caltech had a teacher here—Hunter Mead [professor of philosophy and psychology, d. 1961].

ASPATURIAN: Of course.

KNAUSS: Hunter was a dedicated organist.

ASPATURIAN: I did not know that.

KNAUSS: As a matter of fact, during summers he had gone to European churches and looked for parts and pieces of organs. He dug out the basement of his house in Altadena or Pasadena and built a whole organ into it. I’ve looked it up on the internet, and I’ve been at the house, but I didn’t knock on the door to find out whether it was still there. Probably not. At any rate I think he put an essay question on the transfer exam that had something to do with what science may have to do with music. I picked that question because I’d often wondered about structure, organizing, frequencies—the logic of the musical scales. So, I talked about that. Maybe I was the only one who chose that topic. I think that helped me.

ASPATURIAN: We should talk about how you got from Heidelberg to Caltech. Your family moved to Heidelberg for your brothers’ education, I believe.

KNAUSS: Yes, my father was transferred to Heidelberg in 1950. I never heard this from my father, but my mother, I think, let it slip out. See, we were three brothers, and at that time it was still necessary for citizens to pay high school costs to the city or whoever ran the school. We were exempt from this expense, both in Siegen and in Heidelberg, because we were very good scholars. So, before my oldest brother finished high school in ’49, my father ostensibly went to the bishop and said, “Look, bishop, I have been in these small towns. I have three sons; they’re gifted children; they need to be closer to universities. My oldest son is ready to go to university. What can you do?” So, the bishop moved us to Heidelberg, and I still remember that the minister who had been there was very unhappy about that. He had only been there for three or four years. We were in Düsseldorf only for three years or four years, so it’s not an unusual thing to make a move after that, but, you know, people liked Heidelberg.

ASPATURIAN: I’ve been there. It’s a beautiful city.

KNAUSS: You’ve been there? It wasn’t destroyed in the war. I think it had only one building destroyed because the Americans already had it in mind to make it their European center of command. So anyway, that’s how we came to Heidelberg in 1950.

ASPATURIAN: And your brother started university there?

KNAUSS: Yes, the oldest brother started university. He became a high school teacher [*Oberstudienrat*]. Amazingly enough, he became a teacher of Latin and Greek and ancient history.

ASPATURIAN: Oh, he was a Classicist.

KNAUSS: He had had no Greek in high school, so he had to learn it all at university. I always admired that.

ASPATURIAN: You have to have a gift for languages to do that at that point in your life.

KNAUSS: Yes. So that went well. But then another thing happened to us, and that’s how I came here, but let me think for a moment about whether there’s something else we should talk about first.

ASPATURIAN: I guess I have one question. I don't know if this would have affected you much as a youngster, but after the war ended, and the Allies made the German population aware of what their government had been doing under the Nazis, were you particularly aware of that as a child?

KNAUSS: No.

ASPATURIAN: Well, not while it was happening, but after the war?

KNAUSS: After the war, yes. There was an effort under way to educate youngsters.

ASPATURIAN: That's the word I was looking for.

KNAUSS: It was not to the point; I cannot remember that we were ever shown pictures of what happened in the concentration camps. And I cannot remember that this happened to any extent in the local newspaper. But the effort was there, yes, and the effort was also there to instill something of the American spirit in the Germans. I do remember very vividly that we were shown movies, cowboy movies basically—that this was the free lifestyle. We had no relation to that. We went; it was nice; you didn't have school. But I often puzzled later over what the basic thought behind that was. You can't transmit culture that way.

ASPATURIAN: No, but they tried.

KNAUSS: Okay, that's probably all I can say in that direction to your question.

ASPATURIAN: Okay. Cowboy movies.

KNAUSS: Maybe we can start with what that first year in Heidelberg had to do with my coming to the United States.

ASPATURIAN: Sure.

KNAUSS: All my life, I have looked upon that—not as the foresight—but as the consequence of my father's talk with the bishop. Because Heidelberg at this time was a tourist town for Americans in Germany. You may have gotten this from my writeup. [*See Appendix B:* "Celebrating Fifty Years of Wolfgang," The Caltech Athenaeum, 1994] Frank Williams, the minister up here at the Holliston Avenue Methodist Church had put together a group of youngsters from the Southern California conference to go and work in a children's home not terribly far from Heidelberg. They would spend six weeks or so there, working during the week and visiting communities on weekends. The first town they went to was, of course, Heidelberg, the closest. They wrote to us that they were coming, and my father went out of his way to respond. I don't know how he managed it, but he got a big ham, and we had a ham Sunday meal for the whole group. My mother was always a good baker, so she had two or three fruitcakes on the table. My middle brother and I took them around town, showed them the castle and all of the sights.

ASPATURIAN: Did any of you speak any English?

KNAUSS: I was the best student in my English class. And of course, I took that opportunity to practice my English whenever somebody came who spoke English.

So that went very well. There was only one individual—an American—who insisted on speaking German. I still remember that I was disappointed, but there were some Americans who spoke German.

ASPATURIAN: They wanted to practice their German.

KNAUSS: He wanted his German. I understood that, even at age sixteen.

ASPATURIAN: Sure, sure.

KNAUSS: I think there were seven in this Pasadena group—boys and girls. Before they left Pasadena, they thought maybe they'll bring a student back to the United States or make it possible for one to come. They talked amongst themselves about this, apparently. My brother Tim had just finished high school, and they said, "Why don't we take Tim? He doesn't have any high school problems. Why don't we bring him to Pasadena?" The amazing thing in this was that this minister, Frank Williams, was able to go to Stuttgart and get a visa for Tim in three weeks. Unheard of. But somehow, he finagled that. And when they said they didn't have room on the ship, he took my brother with him anyway and said, "We'll find a place." Today that would be unbelievable, but they did it.

He came over with them and he lived in the Williams' home, which was just north of the Holliston Church here. The house doesn't exist anymore; there is now a social hall there. And then Dr. Williams was called into the cabinet of the bishop, so he gave up his church position there and bought a house in Altadena.

ASPATURIAN: Did the Williams have children of their own also?

KNAUSS: Yes, as a matter of fact, they also had his father living with them: Grandpa Charlie. Also, Frank's oldest daughter, who had just gotten divorced from a soldier and had two children. I think the next oldest daughter and the son were in college. So, only the oldest daughter with her two children and Grandpa Charlie were home. It was a little bit crowded, but they made out all right. My brother stayed there, went to PCC [Pasadena City College] for a semester, and then got a job with a member of the congregation to earn money. Those times were different than they are today. He had only a student visa, but he earned money and could save all of it, because Frank Williams supported him in his home. Then my father was able to get that money transferred to a Swiss bank account, and my brother paid for his whole education on that money.

That's how he became an engineer, working for General Motors in Germany for his whole life. The consequence of that was that when I got to the end of my high school, Frank Williams said, "Why don't we have Wolfgang come over? Tim was such a nice boy; Wolfgang can't be much worse." He didn't say it that way, but I always thought he must have thought that.

ASPATURIAN: "How bad could Wolfgang be?"

KNAUSS: So, we started thinking that maybe I would come over here. He suggested I try to enter Caltech.

ASPATURIAN: What made him suggest that?

KNAUSS: Because—well, I don't know. He thought it was the best school. Maybe Tim had talked about how good I was in school, and so he thought, "let's try that." I took the entrance exam, the SAT test, in Heidelberg. It was a horrible experience. I'd never seen or done anything like that. This big room with kids; you know what the routine is here. I was just flabbergasted. By the time I figured out what the first physics problem was, the others had solved it. And my English was insane, by comparison. So, I remember, I failed that. But Dean [Foster] Strong [associate professor of physics; dean of students, emeritus; d. 1984], who was Caltech's dean of students, wrote a very nice letter that said, "I'm sorry it didn't work out, but why don't you come to the United States, learn more English, and then try again?"

ASPATURIAN: I assume you did very well on the mathematics section, because that didn't require much English.

KNAUSS: There are parts like that, but then there were also question problems, as I remember.

ASPATURIAN: Yeah, that's true, and they tend to be tricky.

KNAUSS: And then I had to figure out what they wanted. Even though I thought I didn't have any problem with English, the logic and the sentence structure was just more than I was used to. So, anyway, that failed. I still was invited to come for a year, but then my father died. We three children didn't know it, but he had colon cancer. Nobody in those days ever talked about what he died from. Though my mother never said so, I'm sure that's what it was. He didn't die from the cancer; he died after an operation through a blood clot in the lung. With that happening, I had to write to Frank Williams that I didn't know whether one year in the United States was going to help me much and that I didn't


Margaret and Frank Williams

know what to do because my family didn't have money to send anybody to college. It was still expensive. It wasn't free like it is today in Germany. And so, Frank wrote back and said, "Why don't you come and stay with us for four years, and we'll see where we can get you in school here?"

ASPATURIAN: That was a very generous offer.

KNAUSS: Oh. It still boggles my mind today how anybody did that. They were so wonderful, such wonderful people, both he and his wife. So. Let's see—how are we doing on time?

ASPATURIAN: We're doing fine. If you would like to continue.

KNAUSS: Let's get at least to the point where I made it to Caltech.

ASPATURIAN: That sounds about right to me.

KNAUSS: So, then, I did come over. By this time all the children had left the Williams household, so only Grandpa Charlie was still there, and that left one whole bedroom free for me. I came, I believe, in August, having spent something like a week or two weeks on the way visiting relatives. We did have some relatives in America. In the hard times after the war, we repeatedly got packages from them.

ASPATURIAN: And where were they living?

KNAUSS: They were living in North Dakota, South Dakota, and Minnesota. After my father died, a direct cousin of his who lived in Brainerd, Minnesota, visited us in Heidelberg. So, I stopped in Minnesota. Her youngest daughter and I stayed in communication and used to write each other at Christmas time. I don't know if she's still alive. [*WK subsequently added:* She was still alive at Christmas time in 2020; she is 100 years old now.] I have to try to make contact with her again. I also visited another cousin of my father's in North Dakota, and from there I flew to Burbank.

Frank Williams and his wife were not there, because they had taken their son to the east coast, where he was just starting a seminary in Boston. So, when I arrived, I had to live with another Holliston congregation family, the Prices. They tried to induce me to stay with them instead of with the Williamses, but I lived with them for only two weeks before Frank Williams and his wife came back. The husband was a bridge contractor, and they were well-to-do, with a lovely home up in Altadena by Eaton Canyon. Wonderful people. Starting in 1957 they had my future wife from Germany stay with them for a year, so she and I could get reacquainted.

ASPATURIAN: What were your initial impressions of Southern California and America in general?

KNAUSS: Wonderful! It's still my feeling today. I can't express how grateful I am that I made it to Southern California, and that I didn't end up somewhere else in the United States. This is the best environment. Let me describe a first impression when I first went to PCC to register before classes started. I took the bus there and back, and when I came back up to my stop, I walked up to Eaton Canyon—and I go along this curved street with palm trees. Totally different from what I was used to.

ASPATURIAN: Of course.

KNAUSS: That first winter I never wore a jacket. I went to school in a long-sleeved shirt. What a wonderful life. And people were so friendly. We get this impression from our current visitors when they come over and visit with us, family members. They say, "People in the stores are so nice here."

ASPATURIAN: Yes, it's true.

KNAUSS: The other place this friendliness came out: Frank Williams had put three people who had gone through school here together to help me get started at PCC and make the right choices for what I might want to do. That was marvelous because they really got me on the right track and helped me. We all became friends. Some moved away, but one was a lifelong friend until they moved up north in California, and we saw them less; but we went to his funeral near Fresno. So that was a wonderful experience. It couldn't have been better.

Something else Frank Williams did for me very early: His wife had a car, and of course he had a car, which he had to drive to Los Angeles every day, but his wife didn't need her car every day, so after half a year or a year, they let me use their second car. I did some damage to the car. I'm always amazed about their generosity in that regard; it's amazing. I don't have a single bad experience in coming to the United States and to Pasadena.

ASPATURIAN: As a German national, not that long after World War II had ended, did you encounter any hostility?

KNAUSS: I personally did not; this is why it surprised me. My wife did.

ASPATURIAN: What is your wife's name?

KNAUSS: Lydia [Dautel]. In Germany, she had gone to *Berufsschule* [vocational/technical school]—that's a school you enter after you leave grade school—and she trained as a bookkeeper. She did the office work for her father's business. There had been some irregularities by a man who ran the office before. He tried to take over the whole business and compromised it terribly by incurring a large debt—a terrible family problem. So, my wife had to take over. She was only eighteen or so. When she was twenty-four, we got engaged by telephone. We hadn't seen each other for four years, only written correspondence. Her father didn't want to let her go to America— "I can't let her go." My future mother-in-law reminded him that when they got married, she was twenty-four years old, and it's time.

Wolfgang, Lydia, and the Chevy outside the Williams home in Altadena in 1957.

So, Lydia came over under the provision that she will see how it might work, and she went to PCC for half a year, to freshen up her English. For somebody who had not learned it in school, her English became very good. Then another member of the Holliston Church helped her get a job in a bank—I forgot which bank it was. [*WK subsequently added:* The building was at the southeast corner of Colorado and Marengo—where today BJ's Restaurant and Brewhouse is.] So, she worked at that bank as a bookkeeper. There she had bad experiences. They would call her "Nazi," and they would ostracize her, and she often came home crying.

ASPATURIAN: I see.

KNAUSS: She stood that for a year, then she moved and became the bookkeeper at the First Methodist Church in Pasadena. From then on, she was very happy. Those were basically the only negative issues. I think I've sensed some hostility here because we have very many Jewish colleagues; it's a professional proclivity for Jews to be in academia—I don't know whether we'll talk about that later. But on the whole, I cannot remember any bad experiences in that regard.

ASPATURIAN: What did you study at PCC? Obviously, you were working on your English.

KNAUSS: I had math, I had English. I did well. I remember the teacher always calling on me.

ASPATURIAN: In English class?

KNAUSS: [Laughter] In English class. Then next semester I had another English teacher, and he said, "I'm not giving you any credit for not being English-speaking. This is an English class; you get the grade that you deserve here. You get the same treatment as the others." Fine. I had some American history. I took physics. I don't think I took chemistry, but of course, math. The math class was interesting because I had already had calculus in the German high school, so I was somewhat ahead compared to the other students. But we had a teacher who gave us problems that only two or three of us could solve, and which he couldn't solve. Two from his class entered Caltech with me via the transfer exam. And so, I always thought at that time, if you wanted to study, PCC was a good place. It was also still part high school at the time. They were in the transition.

ASPATURIAN: I did not know this.

KNAUSS: PCC used to be Pasadena High School. Then the high school moved to the new location over in the Victory Park area. But there were still senior high school students on the PCC campus while I was there, so there was a different atmosphere.

But if you wanted to apply yourself to do academic work, the opportunity was there. Years later when Steve [Steven E.] Koonin [Caltech provost, 1995-2004] was one of our professors, he thought, "Why don't we get good students from PCC anymore?" So, a number of us got together and we visited with the PCC administration, and they said, "Yes, yes," but nothing ever happened that I could see. I think it's not Caltech's fault; it's the people over there.

ASPATURIAN: I think they're also geared to training students to transfer to one of the UC or Cal State campuses. That they do very well, I think, but they're not really set up for anything else.

KNAUSS: I had my eyes on Caltech, so I didn't need any pushing.

ASPATURIAN: Exactly.

KNAUSS: I don't know how it works today; you have these counselors. I never found counselors any use; I made my own decisions about what I needed. I think I only needed a counselor when I needed a sign-off on taking a class at PCC.

ASPATURIAN: How did you get from PCC to Caltech?

KNAUSS: Well, it was very simple. There was a date for the transfer exam, and you showed up here at eight o'clock, and you were assigned to a room. You got pieces of paper, and it lasted all day. I remember one other student who was taking the exam with me. At lunchtime he said, "Oh, I'm doing well; I'll be . . ."—I never saw him again. That's what you get. There were two others from PCC who transferred in, whom I later saw in classes. I can remember two faces, but I can't remember their names.

ASPATURIAN: What year was this?

KNAUSS: In '55. I entered as a sophomore.

ASPATURIAN: So, you had completed the freshman year requirements at PCC?

KNAUSS: Right. Except for physics. I never had much affinity for chemistry, although in high school you could take extra lab or classes in the afternoon if you were dedicated, and I did this for chemistry. I still remember the chemistry teacher. They called him Mambo. [Laughter] I don't remember his real name. But he was very kind and a very good chemistry teacher. So, I had a little bit of extra training, and I say that now because I didn't want to take chemistry here. I wasn't forced to, but I needed to have that exemption signed off by a chemistry teacher. I went to one who was busy at the moment over in—I guess it was Crellin [Crellin Laboratory of Chemistry]. There was a big apparatus, and a graduate student was working on it, so I struck up a conversation. I asked him "what are you doing?" and he explained it, and eventually the professor got free and listened for a while and called me in. I told him I had already done the kind of chemistry I'd seen in the textbook, and I'd also taken this extra class, and I'd like to be excused from it, and he said, "Okay, fine."

Little did I know I made a mistake, because in all my later rocket work, it would have been very important for all my polymer work for me to have had more chemistry. I've always suffered under that a little bit. I made use of that knowledge later when I advised students from Germany who came in saying "Oh, I've had all this." I had figured out by then what the difference was between physics here and physics in high school. I had been taught in high school all the physics that we did here, but my high school background was mainly a matter of being informed. The equations were all worked out for you. You didn't have to master the physics, the concepts—all the physical processes were kind of displayed to you as they were being taught—whereas here, you learn the fundamentals and then you had to see how to formulate those fundamentals into solving problems. That was a big difference. I remember one student in particular coming from Germany for a master's program here, and he absolutely refused to take mechanics issues—

ASPATURIAN: Classical mechanics?

KNAUSS: Well, he came for our graduate program, and he had had mechanics over there. I knew enough about it from my brother to realize that he didn't have the understanding that we had here. Nobody gave a test for that. I let him off. But in the classes, the material was too hard for him. He didn't have command of the fundamentals. So, after a year, he had to leave. He didn't get readmitted.

Whereas another student—Hans-Karl Müller, my first PhD student here—took all the classes that were offered normally—and he said, "Oh, but it's different here." He was eternally grateful that he had that opportunity. He was a very good student, and he made it very up high in DuPont based on his exposure to polymer work here.

ASPATURIAN: It worked out for him.

KNAUSS: So that was basically my transfer to Caltech. I've never regretted it. Caltech has been a good place for me.

ASPATURIAN: Let's stop there.

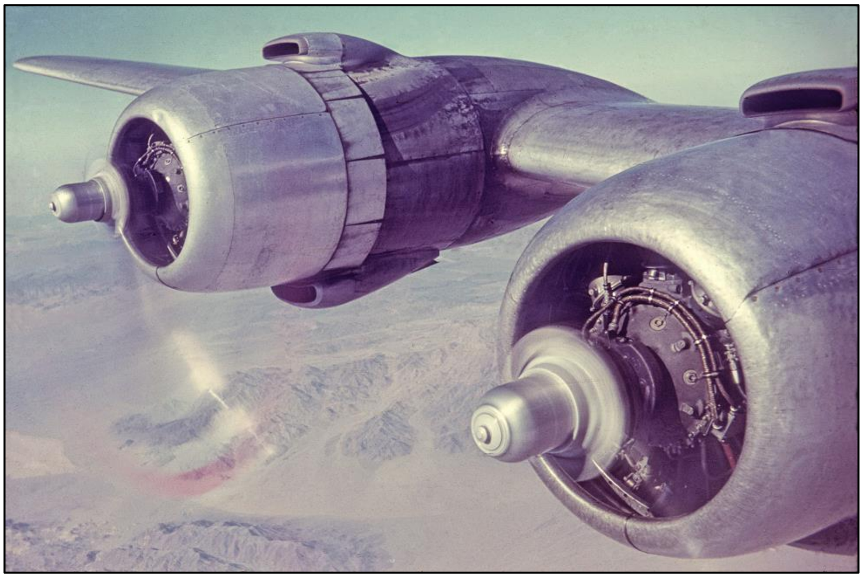
KNAUSS: Okay.

WOLFGANG KNAUSS

SESSION 2

December 9, 2019

ASPATURIAN: When we left off last time, you had entered Caltech as an undergraduate. Did you know you were going to focus on aeronautics when you came in? Was that an interest that had already developed?



Knauss's early studies at Caltech included work on the postwar passenger plane Lockheed *Constellation*, a military version of which became the first *Air Force One* in the Eisenhower administration.

KNAUSS: Yes, as a matter of fact, I came from Germany with the idea that I wanted to go into aeronautics. The reason was that postwar Germany was not allowed to have any aero-related activities because we were too close to World War II, and I thought, "At some time, this policy is going to change, and if I get an education in aeronautics, I'll have a jump on all the competition in getting into the top jobs."

That was my motivation. I did not have any firm idea of what I wanted to do within aeronautics, beyond the simple idea—and this may be admitting naivety—that being artistically oriented, I thought aeronautics would be a wonderful place to do design of shapes, etc. Little did I know that all the shapes are determined by the engineering fundamentals—that is one of the first things I learned when I came here. But originally, I was more oriented in the direction of fluid mechanics and later—jumping ahead to the end of my undergraduate program—jet propulsion. I think it was also mentioned in the Athenaeum talk that I talked with Frank Marble [Hayman Professor of Mechanical Engineering and Professor of Jet Propulsion, Emeritus; d. 2014], and he said, "Let's hold off; we'll see what the end of the year looks like." [See *Appendix B*]

ASPATURIAN: Yes, you talk about that when you talked to Trity, also. [*See Appendix C:* "Reflections on GALCIT History"]

KNAUSS: So that's how I ended up where I am now.

ASPATURIAN: Did you have any interest in flying?

KNAUSS: Not particularly. I thought I would have to learn that to be an aeronautical engineer, but when I got here, I don't think there was even a flying club. I did not have any way really to learn about that, because I lived off campus in Altadena, as I couldn't pay for my housing here. In a way, that living arrangement was maybe not particularly helpful academically, but I never felt that I was short-changed in my experiences at Caltech. So where does that leave me? That basically tells you how my thinking developed along the lines of why I ended up in aeronautics.

ASPATURIAN: What kind of environment did you find when you started here? This was the mid-1950s, I believe?

KNAUSS: The school environment?

ASPATURIAN: Yes.

KNAUSS: I came to the U.S. in '54, and that fall I enrolled in PCC and then went through my transfer exam at the end of that year and transferred into Caltech. There were three of us from PCC, as I remember, who transferred. It was a surprisingly large number according to the PCC people; in the math class, there were two other good students, and the three of us often solved problems the teacher couldn't solve. [*See also Session* One] And so, they made it in here. I don't know what happened to them. But the environment here was of course quite different from what I was used to in German high school. German high school was much more structured—pretty much like what PCC was, actually. You were told what you had to take. You didn't have a lot of freedom. And that's fine.

The interaction with fellow students was new, because in a German high school, unlike American high school, you are always together with the same students, and the teachers come to you in the classroom. So, your interaction with your fellow students was very close. The teachers were at a disadvantage in that system, because they often got ridiculed, sometimes very badly, which still troubles me today. That is not possible in the American high school environment nor in college.

And so, in that sense, there was quite a difference. The interest in the academics at PCC was not at the level that I had anticipated; but I learned that if you wanted to learn, you could do that at PCC. If you applied yourself, and you knew what you wanted to do and didn't necessarily rely on the advisors, you got what you needed, and you could do all right. When I came to the campus here, there was a Throop Club, and there was still the Old Greasy—

ASPATURIAN: Oh, yes, the cafeteria.

KNAUSS: The cafeteria, yes. I was put off by the Throop Club, which was an organization for off campus students to mix with on campus students. I'm sorry to say I found it a little bit too raunchy. And so, I never participated.

Other than that, the interaction with students was limited, but I had a number of close friends. In particular, I developed one close friendship with a student who was also in mechanical engineering, Dalip Saund. His father [Dalip Singh Saund] was Indian by birth, but he was a member of the U.S. Congress from the Riverside area.

ASPATURIAN: The father?

KNAUSS: The father, yes. And the way I got closer to Dalip was that one day after I had suggested we go bowling, he told me that he couldn't do that at certain places. I said, "Well, why can't you?" "Oh, they look upon me as black." That kind of stunned me—I had not been exposed to that. That was one of the awakenings that I had during the undergraduate years.

Dalip was a wonderful person. He was a little bit older even than I was—remember, I had lost a total year in school in Germany. I turned twenty-one in my first year here, so I was somewhat older than the average student, and he was about my age. In his senior year, he didn't study hard, and I remember he said to me, "Once you're in Caltech, you can't get thrown out." He wanted to change his field, and after graduating he went into anthropology, and then he learned flying. He got killed flying.

ASPATURIAN: At a young age?

KNAUSS: I think he must have been in his thirties or early forties.

ASPATURIAN: That's still pretty young.

KNAUSS: Pretty young, yes. While he and his wife were going to graduate school in anthropology, we met repeatedly and got together to compare notes, but then it kind of petered out, and I only heard from him occasionally. Other than that, there was not a lot of interaction with other students, except in the study groups before exams.

ASPATURIAN: Of course.

KNAUSS: That's kind of the normal thing. There was also a period at that time, especially in my senior year, when aeronautics had an agreement with, I believe, the Navy and the Air Force to educate some of their academically inclined students with courses here and maybe have some of them earn an Engineer's degree. And so, there were always a number of these people around, with whom I interacted in the senior year. But before that it was basically a grind. I think you saw in my writeups that I did swimming and tennis. [*See Appendix B*] I quit the swim team, even though during that time I organized my work carefully so I would have enough time for swimming, but I simply could not stand the coach. How he ridiculed you if something didn't work out. He thought that would spur you on; it turned me off. So, the swimming had its end there.

ASPATURIAN: I want to go back to the military group.

KNAUSS: Yes.

ASPATURIAN: When you talked about this with Trity you mentioned that future astronauts were among—

KNAUSS: [Frank] Borman. [*See Appendix C*]

ASPATURIAN: Borman. Did you have any interaction with these guys?

KNAUSS: Not outside of class. We had a particular class taught by Maj [Arthur] Klein [professor of aeronautics, emeritus; d. 1983]. I can't even remember now what the course name was, but he would basically talk about his experience as a fulltime consultant at Douglas [Douglas Aircraft]—the stupid things they did and the good things they did. And, of course he had had all this interaction with the more mature, aircraft-wise military people, and so that was kind of educational in that you learned what real life was like.

Even though I may not have understood the terms they used, Maj did. But that was pretty much it. They were typically about four years older than we were, because they'd already gone through their military training. They were married. They had outside lives. But it gave a certain flavor to the classes that we had.

ASPATURIAN: Who stands out in your mind as professors at that time? Not necessarily in aeronautics, but in any fields.

KNAUSS: Let me do it in the order in which I liked them.

ASPATURIAN: [Laughter] Okay.

KNAUSS: Anatol Roshko [von Kármán Professor of Aeronautics, Emeritus; d. 2017] for my whole encounter at Caltech was an absolute gentleman and a caring person. I was introduced to him in, I think, my junior year when I took his course in fluid mechanics. Maybe it was even my senior year, because it was '57, the year of Sputnik, and we were in class on the morning that Sputnik went up. But one time I had been really ill just before an exam, and I came to take the exam. Maybe I even took it a day late and still wasn't feeling well. He said, "Why don't you wait a few days; you'll feel better." But I thought it wasn't fair to the other students to get extra days. When I turned it in, he said, "How'd it go?" Well, not so good. He said, "Take another half hour or hour and feel good." I thought, "He's singling me out because I'm sick, and the others don't have that advantage," so I didn't take him up on it, but that attitude stuck with me all my life, and also influenced how I treated my students.

Of course, we also had Ernie [Ernest Edwin] Sechler [professor of aeronautics, d. 1973], who was my master's program advisor, who was always very gentlemanlike and concerned. I did talk to you about how I almost missed being admitted to graduate school?

ASPATURIAN: You talked to Trity about it. [*See Appendix C*]

KNAUSS: Oh, yeah, so it's in there. Let's see, there was an undergraduate-year history teacher, Jim Davis; he was an associate professor, nontenured, who was a very interesting person, and we got to know each other. He knew I was German and had a different history and background from most of the other students. [*WK subsequently added:* He taught me something I still believe to be very important about democracy. My understanding was, basically: Majority rules absolutely. Instead, he pointed out to me that even in an absolute majority, there may yet be a sizeable group who has other ideas, and that they need to be heard and understood. I still try to adhere to that principle today].

So, we would talk, and he was still around when I went into graduate school. We had always kept up kind of a loose relationship; and then he introduced both my wife and me to hiking, for a week, in the Sierras because he had done it, and we had not done it before. Unfortunately, he did not get tenure, even though he thought he had made a very good contribution about the causes and sources of revolutions and why that happens in history; but I know the advancement process at the Institute is not always the very best. It was rather a hit and miss kind of thing.

ASPATURIAN: It's like that in academia everywhere, I think.

KNAUSS: Yes. I don't know what you do about that. So, let's see—what other teachers?

ASPATURIAN: Who taught you physics?

KNAUSS: I was just coming to that.

ASPATURIAN: Okay.

KNAUSS: Even though I had the so-called best teacher at PCC in physics, I did not pass my Caltech entrance exam in physics, so I had to take Physics 1 again. I didn't mind. And I found, yes, there were differences.

ASPATURIAN: Of course, sure.

KNAUSS: And there was Gerry [Gerhart] Neugebauer [Millikan Professor of Physics, Emeritus; d. 2014], who was a long-standing professor here in physics and in charge of Mount Palomar [director of Palomar Observatory]. But he was a fresh graduate student at the time, and he was a teaching assistant. He was basically running—I'm not sure whether it was a class or a lab—and then once a week you had a physics professor come give a lecture. I don't know whether I ever heard [Richard P.] Feynman [Tolman Professor of Theoretical Physics; 1965 Nobel laureate in physics; d. 1988]—I seem to remember that he came in once or twice—but Gerry Neugebauer I remember all my career. He was fair. Not that other professors were not fair; but he kind of stuck out a bit. One thing I have to tell you about is—let's see there was in the humanities—is it Hallett Smith [professor of English, emeritus; d. 1996]?

ASPATURIAN: Yes, the humanities division chair; a Shakespeare scholar.

KNAUSS: Yes. I wanted to get a foreign language credit for German—maybe this was already at the graduate level—and he wouldn't give me credit. I said, "Well, you know—two languages; I had to learn English," and he said, "Well that's just the requirement. And if I allow this for German, what do I do with the Indians? We don't have an Indian language as a program option. I can't give you a foreign language credit because you happen to know German." So, I had to take a course in German literature, which satisfied him. But I thought that was kind of strange. I understood what he was saying, but still. Though, of course I didn't have any problem with my foreign language requirement when it came to graduate school. We had to learn two, so I took German and French. In French when I got the written exam, I simply went to the supervisor and said, "Can I translate that quickly for you?" He said, "Yes, you can," and I was done. But then in mechanical engineering there was, of course, George Housner [Braun Professor of Engineering, Emeritus; d. 2008].

ASPATURIAN: Oh, yes.

KNAUSS: George Housner was a lovely person.

ASPATURIAN: I met him. I wrote about him.

KNAUSS: Did you do his oral history?

ASPATURIAN: I did not do his oral history. I interviewed him after the [1989] Loma Prieta quake. Where, as you probably recall, many major structures in the Bay Area remained intact, due in large part to his work. I interviewed him, and I wrote about that.

KNAUSS: I remember him; he taught me one course in structural mechanics during my undergraduate years, and it was quite different from what I learned out of books and from what other schools had. It was well advanced. Then Don [Donald] Hudson [professor of mechanical engineering and applied mechanics, emeritus; d. 1999]. He happened to live across the street from me, up in Altadena where I lived with the Williams family. We didn't have any neighborhood connections at that time, but later I did inherit the woodshop tools that he had inherited when his father died. But I got to know Don here very well as a colleague, and when he got married, and when he got older, up to virtually his death we were close. I remember those colleagues mostly through later in-depth interactions at the lunch table at the Athenaeum.

ASPATURIAN: Ah-ha, the round tables.

KNAUSS: Yes, Don and George were very reticent. George would sit there silently for maybe an hour until somebody made a comment about something, and then he, with a little bit of a quiet, smirky face, would make a comment that really put a new light on the whole discussion. This sticks so well in my mind. He didn't have to be first to talk. He listened. When he had something to contribute, it was there, and it was to the point. Similarly with Don Hudson. I did not have much personal interaction with Rolf Sabersky [professor of mechanical engineering; d. 2016], though he taught me thermodynamics. Allan Acosta [Hayman Professor of Mechanical Engineering, Emeritus; d. 2020] taught me the basics of fluid mechanics.

ASPATURIAN: You mentioned being in a class with Dr. Roshko the day Sputnik launched.

KNAUSS: Oh, yes.

ASPATURIAN: Do you recall what the reaction was? I mean, particularly among aeronautics people, this must have been a very interesting event.

KNAUSS: The topic of the class was of course initially suspended; instead, it was all about Sputnik. I cannot recall the details and what the points of discussion were, but there were a lot of contributions and questions from the students. It was fifteen or twenty minutes before the end of the class that Anatol said, "Hey, we ought to get something class-related going here." [Laughter] But I clearly remember the seating arrangement in the room—now no longer a classroom—to this day.

I remember Don [Donald] Coles [professor of aeronautics, emeritus; d. 2013] very well, because I had him in a class of experimental mechanics. Don was a very quiet individual. We had something in common in that he was the son of a minister, and I was the son of a minister. So, I understood his demeanor maybe better than other people would. I do remember that when I turned in my report for an experiment, I had made use of some handbook data, which he did not like. He said, "You don't believe books; you do it yourself." Little did he think of the fact that a classroom experiment doesn't leave much time to do that kind of research. There was another occasion later, which had to do with him and a student of mine, where I thought he was being a bit too demanding, but I thoroughly understood that he was a very demanding experimentalist, and whatever he did was unquestionably correct. In that regard, I had a lot of respect for him. [*WK subsequently added:* It turned out that the work which he did not consider first rate was awarded in 2019 a prize by the International Society of Digital Image Correlation, recognizing that it was twenty years ahead of its time in that field.]

ASPATURIAN: You mentioned "Kleinisms" a little while ago. You did talk to Trity about them but not to me. [*See Appendix C*] These were Maj Klein's sayings from his years of experience with industry, I gather; something like that?

KNAUSS: Where do I have those? Oh, here.

ASPATURIAN: I'm going to put this on pause for one second. [Tape pauses, then resumes.] Okay. So, we were just talking briefly about the "Kleinisms," and that you're going to make a copy of them for me to append to the oral history. [*See Appendix D*] I wondered if you would talk briefly about your relationship with your graduate advisor, Max Williams. You talked with Trity about this quite a bit, but I'd like to have kind of your overview of what GALCIT was like at the time you were there as a graduate student. [Originally an acronym for Guggenheim Aeronautical Laboratory, California

Institute of Technology, in 1961 GALCIT was renamed the Graduate Aeronautical Laboratories etc., and in 2006, the Graduate Aerospace Laboratories etc. –*Ed*]

KNAUSS: I may have to think about that for a little bit.

ASPATURIAN: Okay. One of the things you mentioned was that [Theodore] von Kármán, [professor of aeronautics, emeritus and founding director of GALCIT, 1930-1949; d. 1963] was not really present anymore.

KNAUSS: I think I mentioned that I met him only twice.

ASPATURIAN: Two or three times, you said.

KNAUSS: He was already mostly back in Europe by that time. Others— I begin to forget the names, and I may have to go up and look at the rouges' gallery [of photographs] up in the foyer. Oh—Ira Bowen [professor of physics, emeritus; d. 1973] was still here. In either my senior year or my first graduate year—I think it may have been my senior year already—he gave us a class in instrumentation, which Maj Klein was also supposed to teach. Then, of course, Hans Liepmann [von Kármán Professor of Aeronautics, Emeritus; d. 2009]

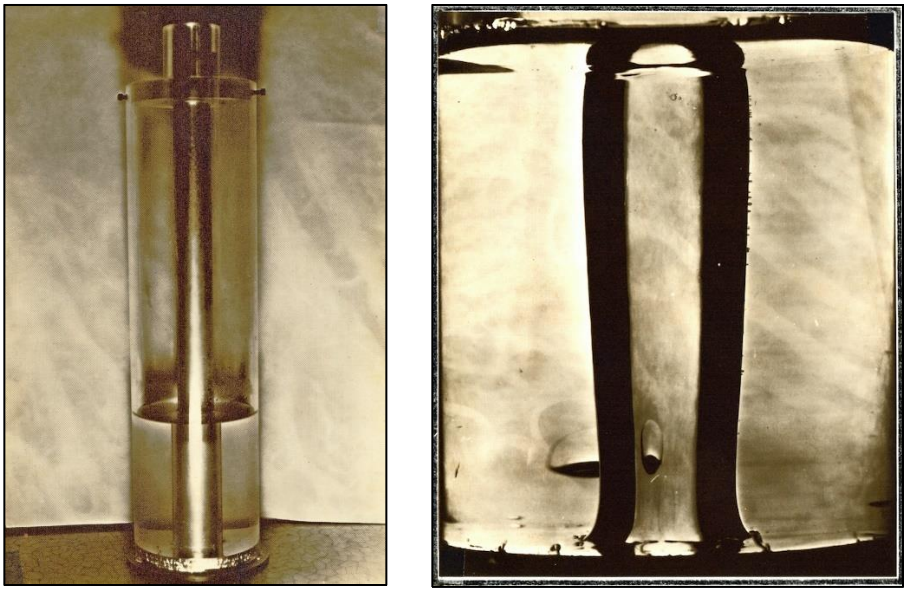
In Jet Propulsion, Duncan Rannie [Goddard Professor of Jet Propulsion, Emeritus; d.1988] was part of the aero group, and Frank Marble. In solid mechanics, there was basically Ernie Sechler, Max Williams, Bert [Yuan-Cheng] Fung, and I think that was it—plus a number of postdoctoral or visiting fellows. It was always a relatively smaller group compared to the fluid mechanics group because fluid mechanics had dominated, even though von Kármán was active both in solid mechanics and fluid mechanics—the von Kármán equations for shell [membrane shell equations] are still quoted today. Bert Fung worked on flutter [a phenomenon related to airplane wing or panel vibration] in the interaction of the solids with fluids. That's all with both fluids and solids under von Kármán, and then when von Kármán left it kind of shifted more strongly to fluids.

ASPATURIAN: Do you want to very briefly describe the distinction between fluid mechanics and solid mechanics.

KNAUSS: Fluid mechanics is the flow of air—any gas—and liquids over bodies, and you calculate the forces on a structure or solid body. In addition, it deals with explaining certain flow patterns and the influence they have on the motion or behavior of their neighborhood. Solid mechanics deals with the forces—the deformations and stresses—on and in structures or solid bodies. Structural failure in aircraft is of course very important, and that's where Ernie Sechler really started, working with von Kármán on the buckling of shells. When metal aircraft came into being, bending forces on the fuselage would sometimes cause the thin shell to buckle, and so you had to understand buckling. It was primarily due to Ernie Sechler that GALCIT for many years was the leading institution in the United States dealing with buckling issues.

ASPATURIAN: And solid mechanics was the area you gravitated to.

KNAUSS: That's right, yes. I gravitated to that because Frank Marble in the Jet Propulsion option didn't act quickly enough, and Max Williams took an interest in me, which came out in one or two situations. Max had me in a course where he gave us analysis problems to do as term papers, and I had done a fairly thorough job of analyzing deformation in a solid propellant rocket. It wasn't a problem that he had suggested, but he had talked about these things in class, and I found it kind of interesting. I remember making a model out of gelatin: I made a cylinder, built a shell around it with a bottom like a coffee can lid—except it was all made of clear plastic—Plexiglass—and then I put a round brass rod down the middle and sealed the tube on the bottom of the plastic can, so to speak. I could pour Jell-O in there—lemon yellow Jell-O—and that would settle down. When I heated the brass rod, I could remove it, and now, if I took the bottom lid out, I had the equivalent of a model of a solid propellant rocket motor that was perforated by a cylindrical bore on the inside. When I lifted it up, the Jell-O deformed under gravity. You could see with your own eyes what it was doing.



The gelatin deformation experiment: With lemon flavored Jell-O in a plexiglass cylinder (left) serving as a proxy for a solid rocket fuel propellant, Knauss investigated how the material deformed (right) under the impact of gravity.

So that became part, ultimately, of the analysis that I presented. He thought that was inventive, and he said, "Hey, you want to have a summer job?" Marble didn't have a summer job; he wanted to see what was coming down the pike and then maybe take me on at the end of the year, and I couldn't wait. I was already working twenty hours a week at the F. M. Moseley company in Pasadena, but that job ended when I graduated with my bachelor's degree, and that's how Max Williams could latch onto me, because I needed financial support, even though my wife-to-be was working.

ASPATURIAN: What did you do your PhD thesis on?

KNAUSS: It was on the fracture of viscoelastic materials, and maybe I need to explain a little bit here what viscoelastic materials are: When you deal with most metals—steel or something similar—and you deform them a little bit, they spring back, and that spring is almost immediate, very quick. If you have a rubbery material, it's also elastic and can undergo relatively large deformations, as rubbers typically do. And when you deform them and then let go, making them stress- or load-free, they also snap back very quickly. However, something different happens when you cool rubber down; and this comes back to the Feynman thing. [Reference is to physicist Richard Feynman's famous rubber in ice-water demonstration as a member of the commission to determine the cause of the explosion of the *Space Shuttle Challenger*. *See Appendix E*: "GALCIT and the Challenger Disaster." –*Ed.*]

You can try this yourself when you go home tonight: Put a piece of rubber, stretched around something, in the freezer for a while, and you'll find that if you take it out of the freezer and take the load off and quickly put it on a table, it doesn't snap back. It kind of creeps back to its original shape as it heats up. A material like rubber at cold temperatures does not just snap back. It will go back, but it needs time. So, it has the characteristics of being viscous-like; and there are also fluids—grease and oils, for example—that if you deform them very quickly, have a higher resistance to the motion. It's the way honey runs off the spoon if you wait long enough. It's very viscous at room temperature, and if you make it cooler, it flows more slowly.

ASPATURIAN: So, the familiar behavior of metals did not apply to any of these materials. And you needed new analytical tools to figure out what was going on?

KNAUSS: Yes, right. And so, in solid propellant rocket materials, the fuel is a combination of rubber and a crystalline material—ammonium pro-chloride. The ammonium pro-chloride carries the oxygen, and the rubber has the carbon in it, so these are the two sources of the burning gas that is ejected through the nozzle at the end of the rocket to provide the propulsion. These compound materials are time dependent—viscous—and, just for an example, if you have a rocket propellant grain sitting on end, and you let it sit for a sufficiently long time, it will slump. You have cast it in its form—the containing shell for the fuel—but if it's sitting there for a long time, ultimately it will want to move—

ASPATURIAN: It sort of settles.

KNAUSS: It settles, yes, towards the bottom. And that is the viscous aspect of this material. Early on, they had a problem with this at JPL, because the earliest solid propellant fuels were asphalt based, and the asphalt component would literally flow very slowly under gravitational forces.

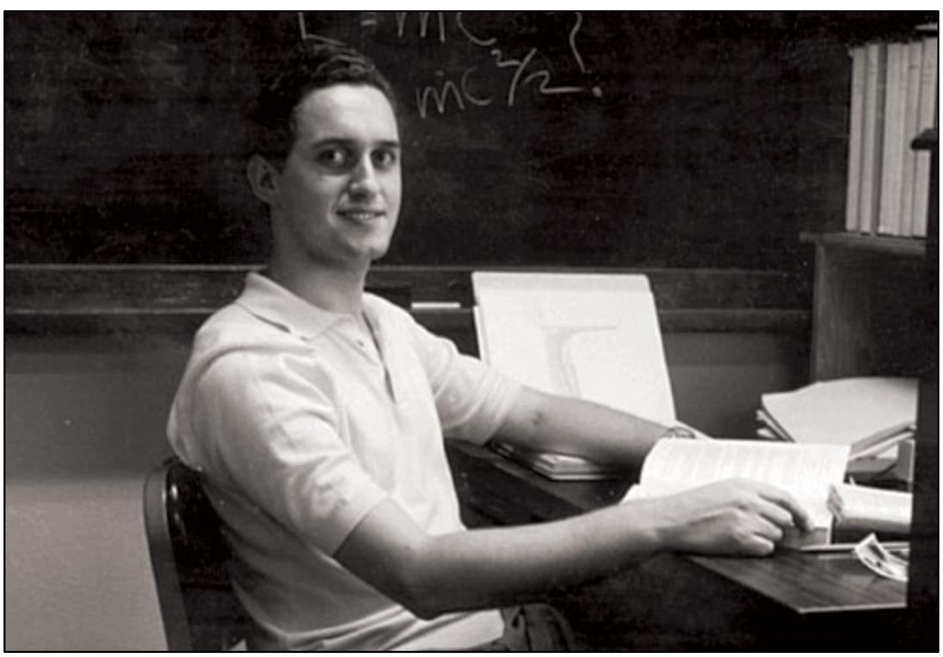
ASPATURIAN: What years are we talking about now? Late 1950s, early 1960s?

KNAUSS: It must have been the end of World War II.

ASPATURIAN: Oh, that long ago.

KNAUSS: Because these early asphalt-based fuels came into existence through the JATO [Jet Assisted Take-Off] rocket program. [JATO helps overloaded aircraft into the air by providing additional thrust in the form of small rockets that drop off after launch. *–Ed.*] Asphalt is really like a very, very stiff liquid. Given enough time, it will flow. You know how when you drive on an asphalt road in a hot summer, it's kind of sticky, and the car can move the stuff aside. In the winter, it's no problem.

So, through Max, I got introduced to the problems of long term, time-dependent behavior for these materials. But then came another problem, which was that these materials were not very strong under high stresses. And nobody understood how the time-dependence of these materials influenced the propagation of a crack.



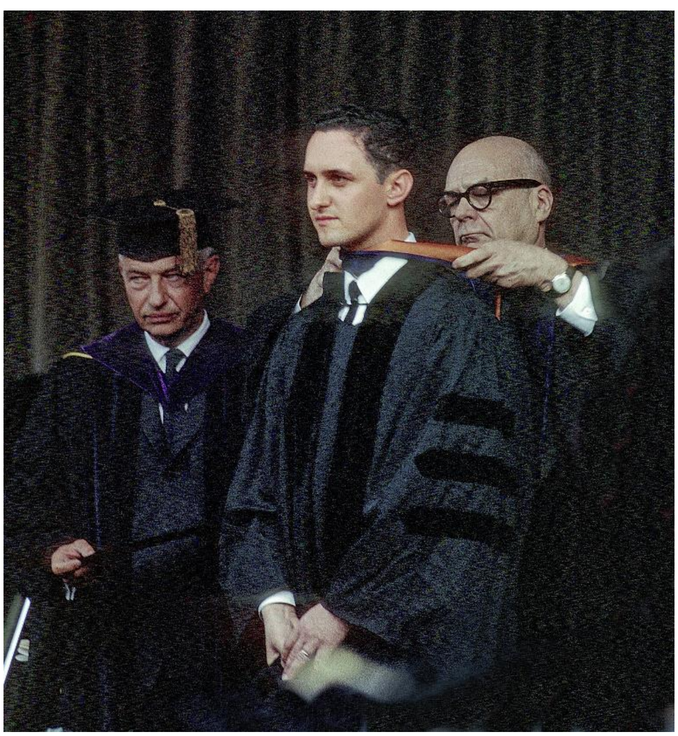
Knauss as a graduate student in Guggenheim Lab, c. 1961

Now Max Williams was a leader—initially not a very well recognized leader—in fracture mechanics, and he understood and dealt with cracks and crack propagation. As a matter of fact, he introduced the problem of dynamic fracture here at Caltech by getting the money from the Air Force to deal with crack propagation under very high loads and very high rates of loading. He built one of the very early high-speed cameras to image crack propagation for Boeing. [*See Appendix C*] The remnants of that are still in the basement somewhere.

ASPATURIAN: So, you wound up working at what you might call the interface of solid mechanics and fracture mechanics, it sounds like—dynamic fractures.

KNAUSS: Fracture mechanics is a normal part of mechanical engineering today. But it wasn't at that time. It was in the process of becoming a part. But in a way, I kind of sat at the interface of solid mechanics and fluid mechanics, though not really feeling I was part of fluid mechanics. The solid part was, initially, very, very slow, and didn't have the dynamic issues that fluid mechanics has.

ASPATURIAN: Yes, I see.



KNAUSS: But basically, I always considered myself a solid mechanics engineer.

ASPATURIAN: You came onto the faculty almost immediately after receiving your PhD. That's unusual nowadays, you know.

Knauss receives his PhD in 1963 from Caltech president Lee DuBridge (left) and commencement chief marshal Harvey Eagleson.

KNAUSS: These days it probably wouldn't happen. I was a postdoc for one year.

ASPATURIAN: Also, here, with GALCIT.

KNAUSS: I took off for half a year because we hadn't been back to Germany—I had not been back in nine years, my wife not for six years, so we took off for half a year. When I came back, Max was still here, but he was on the verge of leaving. I was a postdoc for a year, and then Max through Fred [Frederick C.] Lindvall [then division chair for engineering and applied science; professor of engineering, emeritus; d. 1989] got me appointed as assistant professor. I have to admit that the mode of advancement to higher level positions is not very good when you were originally a student here. It's something that didn't strike me strongly enough at the time. I might have done better elsewhere in terms of professional advancement, but I have never regretted not going away, because in the long run things have always worked out for me. I recognize that it is a difficult situation for a student to have your former teachers as colleagues. And it's not easy for former teachers to not look down on a student appointee. Although when it's their own former student, then it's quite different.

That's one of my complaints here. I saw that people whose advisors stayed around for a long time did a lot better. I did not have my advisor—my "doctor father"—stay here.

ASPATURIAN: He went to Utah, I believe.

KNAUSS: He went to Utah from here. And so, I was kind of left dangling. That was tough at times.

ASPATURIAN: Who was the head of GALCIT at that time?

KNAUSS: In the beginning it was Clark Millikan [professor of aeronautics; d. 1966], but he had a heart attack and died fairly early. I remember him very clearly; I was assistant professor for two years before he died.

ASPATURIAN: What was he like, do you recall?

KNAUSS: I didn't have much interaction, because he was totally in fluid mechanics and by that time, I was in the solid mechanics group. But he had an annual Christmas party at his house, and so you got a little bit of a feeling for him there. He was always dressed in a smoking jacket, which for me as a poor German was something quite different. [Laughter] And he did smoke. I don't know how much that contributed to his early death.

ASPATURIAN: It may have, yes.

KNAUSS: As I recall, he had a heart attack, and afterward he didn't take the doctor's advice to rest longer. He couldn't stand not being in Washington and took a trip and this wasn't good for him. After Clark died, Hans Liepmann and Ernie Sechler didn't see eye to eye well enough, as I understand, for Ernie to become the new director. The position of GALCIT director disappeared while Hans Liepmann's student Brad Sturtevant [Liepmann Professor of Aeronautics, d. 2000] was running things, though I forgot the official name he carried. Hans Liepmann didn't think Ernie had enough brain power to be director of GALCIT.

ASPATURIAN: I see.

KNAUSS: I was kind of on the fringe of all of this, but you sensed what the issues were. Then, finally, Hans Liepmann took over, officially again as GALCIT director. I forget when now; I have to look up which year.

ASPATURIAN: I can do that. [It was 1972]

What were your interactions with him? Did you have many?

KNAUSS: Not very happily. I had some issues with promotion, and as a result of that I took off for a year to go to Germany, with the idea of possibly looking for a position there. Ultimately, I did decide I would stay here.

ASPATURIAN: Do you remember when this was?

KNAUSS: That was in the 1970s—'73. I was an associate professor for a long time. Nobody had an interest in advancing me. That's what I meant earlier.

ASPATURIAN: Because your mentor had left.

KNAUSS: For example, George Housner's student, Paul Jennings [professor of civil engineering and applied mechanics, emeritus; Caltech provost, 1989-95; 2004-07]—he was moved right up, and he was in the [National] Academy of Engineering [NAE] very early as a young man [1977]. You could tell if somebody looked out for you.

ASPATURIAN: Yes.

KNAUSS: And ultimately that had financial repercussions, too. Because the salary increases by percentage, and when you start at low percentage, you see—

ASPATURIAN: That's right.

KNAUSS: And the only one who ever discovered and worried about it [i.e., the salary discrepancy] was when Paul Jennings became division chief [1985-89]. He said, "Hey, how can this be?" And Chuck [Charles] Babcock [professor of aeronautics and applied mechanics; d. 1987] was still alive, and they talked about that, and something major happened at that point. Whereas any administrator with whom I talked about it before had just ignored me and done nothing.

So anyway, you're asking about Hans Liepmann. I had heard from Ed [Edward] Zukoski [professor of jet propulsion and mechanical engineering; d. 1997], who was in jet propulsion, that Hans would have liked to shut down solid mechanics because only Chuck Babcock and I were here. Hans didn't think much of me, but he needed Chuck Babcock. Chuck Babcock was a very levelheaded individual who understood people and how to deal with people—something Hans could not do. He needed Chuck Babcock for advice, and Chuck Babcock then became—ah, what do you call that?

ASPATURIAN: He became vice provost, I think, eventually. [1984-1987]

KNAUSS: Yes, that's what I was looking for, vice provost. And I think Robbie [Rochus E. Vogt, Avery Distinguished Service Professor and Professor of Physics, Emeritus; Caltech provost, 1983-1987] really appreciated how Chuck did the same service for him that he had done for Hans.

I had taken off for a year after '73, '74, and when I came back, all my administrator contacts in the government programs had changed. And it's very important that I had polished apples with these people for ten years, so they all knew me; and now they were gone. I had one program with NASA left and nothing else, and it became very difficult to get proposals through. People have to know you before they'll trust you with $100,000 or more of their own responsibilities, at that time, for a proposal.

ASPATURIAN: Sure.

KNAUSS: So, I needed to make phone calls because I couldn't go to Washington; I didn't have the money. My travel funds came basically through the Industrial Associates here, who at that time had a program where they would pay for a trip if you spent a day at one or another of their member companies. So, I did that quite a bit. Dick [Richard] Schuster was in charge of that. Did you know Dick Schuster?

ASPATURIAN: I do not know that name.

KNAUSS: Dick Schuster died in the Chicago air crash.

ASPATURIAN: Oh, yes, the one outside O'Hare.

KNAUSS: Yes.

ASPATURIAN: 1979, or something like that.

KNAUSS: Something like that. [May 1979]

ASPATURIAN: That was a horrible disaster.

KNAUSS: So, I needed to make phone calls, and Hans [Liepmann] got on my case at one time, saying I spent too much money telephoning. And I thought, “Mister, things like this are necessary,” and he kept telling me—he tried to tell me—what I had to do for research. What does a fluid mechanician know about what’s important in solid mechanics? Of course, Hans thought he knew everything. The other thing was that Hans was always a jokester, but he always made jokes at other people’s expense. If I tried the reverse, he didn’t like that. So, as I said, we didn’t get along well with each other.

ASPATURIAN: This was not a good relationship.

KNAUSS: It was not a one-to-one relationship. That’s what I meant earlier. That’s one of the drawbacks if you are a student and then a professor here.

ASPATURIAN: In some people’s eyes, you always remain a student, no matter what happens.

KNAUSS: That’s right. I started to talk about Ed Zukoski, saying that this was the time when apparently Hans thought, “Well maybe we ought to eliminate solid mechanics.” But see, he had appointed all his students to positions here: Anatol Roshko, Don Coles, Brad Sturtevant, Paul Dimotakis [Northrop Professor of Aeronautics and Professor of Applied Physics]. So, there was no problem when he needed or wanted somebody to get promoted, but when it was somebody else— It was unusual that Ernie got promotions for Chuck Babcock, who had only two papers to his credit when he was made a full professor. But they were important papers, and that was the main difference. That’s changing a little bit, too. But those were some of the issues that made life a little bit hard here.

ASPATURIAN: In your early years.

KNAUSS: And I don't know how important it is—some of that ought to be left over for the rest of the world to see, but not too much.

ASPATURIAN: I understand. How did you come to work with NASA?

KNAUSS: First through Max Williams, and later through one of our former graduate students, Jim [James] Starnes. [*See Session Three*]

ASPATURIAN: And this must have been in the 1960s then—early 1960s.

KNAUSS: Yes. Max Williams had a large grant on fracture, and I got my degree on that research. There was a man by the name of Paul Wetzel at NASA, and Max was on good terms with him. Max was on good terms with many people because he helped many people. He told them what they should and should not do. He had consulting positions with companies and with the military services, and he helped the higher ups make technology-based decisions on where to spend their money. Not necessarily to give it to him. I think he had a good core of people who appreciated his help. But at any rate, because he had an interest in fracture, he had me working on fracture and crack propagation. He was reasonably happy, I guess, with me, and when he left to go to Utah, he left me his grant.

I think it was $125,000 a year, which in those days was a lot of money. [*WK subsequently added:* $1.04 million in 2021 dollars.] And I did all my crack propagation theory work with that support. I had students for many years on that program. I still hold it highly to Max's credit that he left that whole amount of money for me here. And we always had a very good relationship afterward. It turns out that ultimately, I was the only one left of his students whom he seemed to trust to pursue things with respect to his personal estate, and whom he could trust to fulfill his wishes.

ASPATURIAN: So, he left you—

KNAUSS: In 2003 he had earlier left me some money out of this Vanguard Fund. He gave me some money through this fund to be spent here. He said, "Wolfgang, when you retire, you always need money, and nobody gives you money." So, I think he gave me about 15,000 dollars. There's still about half of Max's money gift left at this time.

ASPATURIAN: This is funding for research?

KNAUSS: These were discretionary funds for my work. That's what I live on professionally, so to speak. I also put some money in myself. At any rate, Max was very helpful in that regard, and we had a very good, trusting relation till the very end.

ASPATURIAN: That's what it sounds like.

KNAUSS: He did not tell me how close he was to dying. Before he died, because the sizable donation he had made to the Institute had originated from the *International Journal of Fracture*, he wanted to have a plaque mounted in GALCIT saying that the journal was established here in 1963, which was the year that I graduated. He asked me, "Wolfgang, will you do that?" I arranged to have a plaque put up in the Firestone building; I was going on vacation to Germany, and it was all arranged before I went to Europe. But Dimity Nelson, the GALCIT administrator at that time, must have thought that it was not so important to get this done. It could have been all done by the time I came back, but when I came back nothing had happened. So, I saw to it that it was going to be done quickly, but he died while that was in progress. I wanted to be able to tell him, "Hey, Max, it's done!" Missed out that way and am very sorry that Max did not get that message. So anyway, I had a good relationship with Max, and I always had a good relationship with Bert Fung.

[*WK subsequently added*: Max also had a lot of respect for Jerry [Jerold J.] Swedlow, who received his degree under Max a year or two after I did. One of Max's long-term wishes was to leave a substantial amount of money to Caltech. I visited him on his 90th birthday [February 22, 2012] in Austin Texas, for the day. We discussed that, and that he had finalized his estate documents and that he wanted me as the contact at Caltech to oversee his wishes, as I knew him best and he felt I was close to his philosophy on how universities did or did not follow directives—after all, he had been a dean of engineering at two universities, Utah and Pittsburgh, after he left Caltech. He also did not trust lawyers and had done all his estate work by himself, whereupon I encouraged him to at least pass all his work past the review of a lawyer, just to guard against any oversight, which he did.

Two actions followed these discussions, the first one leading to a very substantial gift to the Institute in 2013, which arose from Max starting the *International Journal of Fracture*. The Journal had started publication in 1965 under the name *International Journal of Fracture Mechanics*. Max renamed it, still in the 1960s, as he did not want people to think that this was a publication for "mechanics" that performed plumbing jobs," the more official reason being that it was not to be limited only to mechanics issues. The publishing house changed from the initial, Dutch, publisher Sijthoff & Noordhoff several times. On one of these publisher changes, possibly as Kluwer [Wolters Kluwer] took over in the 1990s, the question arose as to who really owned the journal. As I understood from Max, he had always thought it was his and not the property of a publisher.

Well, out of that dispute came the agreement that a trust fund would be established. I do not know whether that was something that would be funded once at that time or was geared to the turnover earning by the journal. This fund was to assist in supporting research into the subject of the journal—mechanics of fracture. The extent of those topics was never clear to me, though I suspect that Max's and Mel's—his second wife, Melba—trip to England and extended academic stays there along with *Journal* business were part of it. Mel had been a steady coeditor, mostly responsible for the "Blue Pages," which were shorter research communications that could be published more speedily than the fully refereed papers.

At any rate, a trust was established, as I recall, on the Isle of Guernsey off the English coast, and I know that establishment only in connection with the name "Vanguard Foundation," which "was run" by Max and Melba with a local administrator. Though there was some kind of a board for that foundation, I never knew who the members were; I only knew that everything went through Max, with the executor being the local Guernsey administrator.

Max had approached me well before my retirement to help him identify personalities and/or activities worthy of support, without spelling out what form that support might take. However, we never could jointly discover a worthy cause. I believe that gradually he convinced himself that a sizeable donation to Caltech was the way to go, as he was going on ninety and his heart was giving him trouble. The result was a donation of $1.335 million from the Vanguard Foundation to Caltech to benefit GALCIT, particularly solid mechanics activities and support for international students.

I am sure that this is the connection of the 2013 gift, because I had received substantial funds on Max's initiative via this Vanguard path in 2003. Though I must say that Max always made sure I knew that he had to do this through a distinct request to the administrator. He suggested that I deliver an annual report of how money was spent. For quite a number of years, I reported no spending to the foundation, as I had still other resources available, but gradually ceased reporting as the administrator became seriously ill. I also know that Max wanted to terminate the trust; he once remarked that it was "only eating up substantial money in administrative costs [fees]."

Max always tried to stay out of the limelight in matters like this. I believe he was mortally afraid of being identified as a "do-gooder" who might be pressured by others for donations. But Caltech was very high on his list for support; he was *very* appreciative of what Caltech gave to him, and his drive to excellence was concomitant with Caltech's goals. I am also very sure that the gift to Caltech was by far the lion's share of the trust: He had mentioned at least one other institution that had received rather limited (trial?) funds but did not deserve more.

Bert Fung was always very, very helpful. He was a gentleman. He was helpful to his students. He had an excellent mind and a good way of looking at life: "Don't take something too hard; other things will make up for it." I called on him when I was working in the biomechanics field, because at that time you heard these horror stories about how students who had graduated in biomechanics couldn't get jobs. There was one PhD graduate who was pumping gas. I said, "I can't put my students through that," and so I called him and said, "Bert, what do you do?" He said, "I don't know; they find jobs, but it's a problem." I said, "I can't do that to a student; I have enough problems." So, I gave up on biomechanics. [*See Appendix C for a detailed account of WK's biomechanics research.*]

ASPATURIAN: And now look; thirty years later, it's a completely different story.

KNAUSS: Different story. Well, Bert stuck it out, and I did not.

ASPATURIAN: We've been talking for an hour. Next time, I'd like to maybe talk about some of your work for NASA and the Air Force, and then the culture of GALCIT as it evolved.

KNAUSS: Okay.

WOLFGANG KNAUSS

SESSION 3

December 18, 2019

ASPATURIAN: Today we're going to discuss some of the research highlights of your career, and I think we wanted to talk at least some about NASA and the Air Force, and then also about your work on fracture- and micro-mechanics, which seems to have been a very vital part of your research.

KNAUSS: Okay, maybe we'll start with NASA. The motivation for that research came from the need to understand the fracture and failure of solid propellant rocket fuels. You may recall that this was the period of the high competition between the Soviet Union and America.

ASPATURIAN: Sure, the space race.

KNAUSS: The space race. And the solid propellant rockets were the version that you could store in the silo and then fire after a relatively short preparation time. But they did have a problem in that the solid propellant fuels are what we now today call time-dependent materials.

ASPATURIAN: Which you talked about a little last time. [*Session Two*]

KNAUSS: Okay, so we don't need to go into that. If you do, we can fill that in.

ASPATURIAN: I did want to ask were these fuels the ones being used to send satellites and the astronauts and their capsules into space?

KNAUSS: Yes, that's right, but typically as an assist with a liquid-based rocket for the heavy payloads.

ASPATURIAN: And is this related to the problem of why they initially kept blowing up on the platform?

KNAUSS: No, I think the ones that we mostly saw on the platform blowing up were liquids, except for the Challenger launch

ASPATURIAN: Ah, okay, so the solids were developed to resolve that problem?

KNAUSS: The solids also had their own problems. A major weapons system was the Polaris missile, which is stored in nuclear submarines, and you didn't want to have a misfunction in one of those.

ASPATURIAN: No, you certainly didn't, no.

KNAUSS: But in the early days there were—I won't say frequent, but not infrequent—blowups of tests for solid propellant rockets until one began to understand the time-dependence issue somewhat better.

ASPATURIAN: I see, I see.

KNAUSS: Because these materials are not of the type like steel—I won't even talk about plastic—because they have also time-dependence but on a different time scale. When the motor, as we call the interior of the rocket, was manufactured—cast—it was all fine. But then the propellants were stored at an elevated temperature in order for the chemical process to solidify the charge—which we call the cure process. There is a shrinkage of the propellant during that process, and since the propellant is bonded to a relatively rigid or stiff outer case, you begin to set up stresses. To understand the magnitude of the stresses, and the response of the material to these stresses as a function of time was a very important issue. And that did get worked out. Let me just jump ahead a little bit.

ASPATURIAN: Sure, sure.

KNAUSS: Today we have missile systems for solid propellant rockets that have been in storage for over thirty years, forty years, and they are still functional. But because one does not totally believe or trust the computations in all the tests in the past, there are continual test firings where one takes a motor, selected via random counting, out of the arsenal and fires it. If that motor is fine, that gives you assurance that probably the others are also likely to be okay. Every once in a while today, you hear in the news that there was a test firing out at Vandenberg Air Force Base. Most likely it's one of these test firings. So, the science, the engineering of that aspect, has had success.

ASPATURIAN: When you were first asked to address this problem back in the 1960s, how did the difficulties come to light? What led people to realize that these solid fuel propellants had these problems?

KNAUSS: Casting the motor and testing it to demonstrate that it worked. And they had too many misfirings.

ASPATURIAN: I see. So, in the manufacturing process things didn't go right.

KNAUSS: Not quite; actually, there's another issue. The problem with failure and fracture is basically a mechanics issue. But since propellants were concocted by chemists, the chemists who were involved in doing the engineering didn't necessarily have the right background for fracture or other failure analysis—including chemical engineers, in spite of the name. [*WK subsequently added:* Thus, a motor might look fine all the way through the manufacturing phase, but when, after a while in storage, the first pressurization occurred after the onset of ignition, a failure could result.]

ASPATURIAN: These weren't issues they were prepared for, in other words.

KNAUSS: That's right, yes. It wasn't looked at from the proper perspective until mechanical engineers, i.e., mechanics people, became involved in the issue, and that was during my graduate years. Basically, in the later 1950s is when that started, and my doctor father Max Williams was heavily involved in that.

ASPATURIAN: Which we talked about last time [*Session Two*]. So, you first became involved in this problem in connection with NASA and then the Air Force. Was that the chronology?

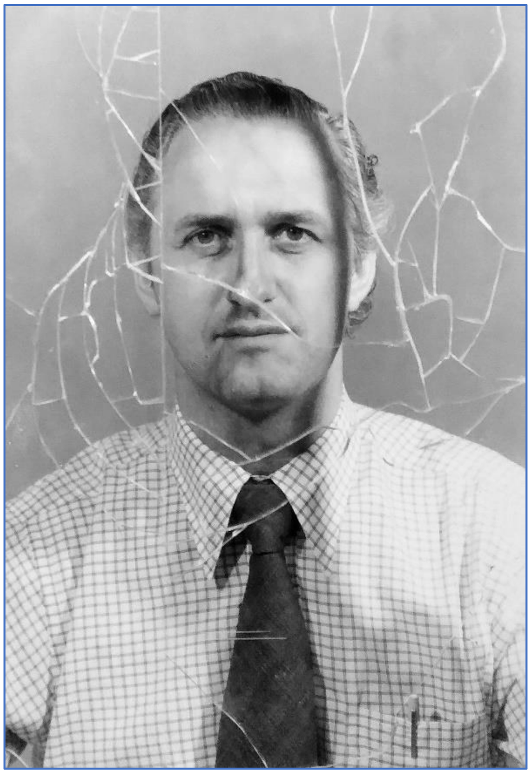
KNAUSS: I think that is correct. But the reason that NASA crops up is because during my graduate years, Max Williams received a major grant from NASA, about which we talked already [*See Session Two*], to deal specifically with the crack propagation issue in these types of materials. I think maybe that was my first success in this business, in that I was able to generate the first fracture mechanics model.

ASPATURIAN: Was this the Jell-O model you were talking about? [*See Session Two*]

KNAUSS: This fracture work was past that, after graduate school. Jell-O was a classroom experiment that qualified me for graduate school with Max Williams. But this later work involved an extended study, through my PhD thesis, and I followed this up with my first graduate student, who was a leftover from Max Williams. When Max went to the University of Utah, the student, Hans Müller, stayed with me.

ASPATURIAN: And you inherited him.

KNAUSS: I inherited him, yes. He was also German by happenstance, and he was a very good student, and I am still in contact with him. We worked out a first fracture mechanics model for viscoelastic materials on which I then improved the analysis, and that became the first real extension of fracture mechanics beyond the then typical engineering materials like metals and glass. It was the first model for a material that had a strongly time-dependent viscoelastic characteristic, and we not only calculated but measured the crack propagation speed in a sheet of viscoelastic material, depending on how much the sheet was stressed or strained. And that correlated very well with the analysis which I had performed. First with my student, but then there was an improvement in the model about the details of what happens at the tip of the crack and that gave a solution to the problem without any approximations, other than when you're talking, for the scientific audience, about small strain analysis applied to materials that



withstand large strains. I mention this, because my earlier office mate at Caltech, Dick [Richard Allan] Schapery, who was also a Max Williams student, but not in fracture, published an approximate theory after I had my general theory printed in a conference proceeding. There has been, as far as I know, no large strain viscoelastic analysis—none that I have seen other than numerical computational treatments, but then the material models are not likely to be proven. Maybe it has happened since I have retired, but I kind of doubt it, or else somebody would have probably told me.

Studying crack propagation in a viscoelastic material, c. 1965

ASPATURIAN: How long did it take for your experimental and analytical work to be incorporated into the manufacture of these fuels?

KNAUSS: I'm not sure. I've never seen that particular aspect applied, because the only way I would see that is if I was working with a company.

ASPATURIAN: I see, I see.

KNAUSS: I had started to work with a company, United Technologies, in Sunnyvale [California], and during that time I went on a sabbatical.

ASPATURIAN: That's when you were having the issues with Dr. Liepmann and went to Germany?

KNAUSS: Slightly different. We may come to that yet. This was before that. And so, I was gone for half a year and during that time, that connection to United Technologies stopped. I did not see how and to what degree they applied my research, but I had started them in that direction. When Dick Schapery developed his approximate model somewhat later, he started to take over my lead with United Technologies, and he worked for them. So, I never saw a direct application of the theory, but it has wider applications than just solid propellants. It has applications in plastics. When a plastic gets heated, it can fracture something like plexiglass: It can break like a brittle material, depending on how rapidly one loads it, but if one stresses it to lower stresses—lower strains—for a very long period of time, a propagation can also occur.

ASPATURIAN: In the plastic.

KNAUSS: In the plastic.

ASPATURIAN: Did you work directly with NASA and the Air Force people on these projects, or was it more a matter of submitting reports?

KNAUSS: This was research. I don't think this viscoelastic fracture domain ever interacted anywhere with NASA at one of their centers. It was through a contract for research funds.

ASPATURIAN: And the Air Force, same thing?

KNAUSS: Yes, yes. The only extended and directly industrial interaction I had at that time—well, there was also some work where I helped the Navy directly on projects for them—but how I came to this other one may be of interest. We're in the Firestone building [Firestone Flight Sciences Laboratory] on campus, which was donated by Harvey Firestone, who was an acquaintance of Lee DuBridge [president of Caltech, 1946-68; d. 1994].

ASPATURIAN: He was a tire guy, wasn't he?

KNAUSS: Yes, right. His family owned the Firestone Tire Company. And when our Firestone building was finished, the Firestone people sent a group of executives to Caltech to see what the Firestone money had done; and in the basement I had my laboratory setup to study crack propagation in a viscoelastic rubber. One of the first mechanical engineers hired by Firestone in the research department, Joe [Joseph D.] Walters, was there with his boss, the director of the Firestone Research Laboratory; and I remember him nudging the research director, saying, “Hey, we should talk to this guy.” It turned out that out of this contact evolved consulting with Joe Walters for many years.

ASPATURIAN: For Firestone.

KNAUSS: For Firestone, yes. And then I became involved in some of the legal issues because they had major tire failures—alleged tire failures. You remember the Radial 500?

ASPATURIAN: I know that there were accidents as a result of some catastrophic tire failures.

KNAUSS: They were claimed to be accidents. Later there were others, including the Ford Explorer rollovers, and I was involved in those.

ASPATURIAN: Doing experiments to investigate?

KNAUSS: Consulting, analyzing what happened and telling them what to do, what to measure or what would be important. I don’t know whether you want to cover that right here, or we can talk about that later.

ASPATURIAN: Oh, let’s talk about this now. It’s fine. What time frame are we speaking of?

KNAUSS: I think the Radial 500 must have been somewhere in the late 1970s or early 1980s, around that timeframe. The Ford Explorer issue was around 2000.

ASPATURIAN: OK.

KNAUSS: In both cases, my perspective is that Firestone did not have the problem that was alleged. As a matter of fact, I recall distinctly that the NHTSC, the government agency that forced Firestone—

ASPATURIAN: National Transportation Safety—

KNAUSS: I have to look it up; it's too long ago.

ASPATURIAN: I'll find it. [National Highway Traffic Safety Commission]

KNAUSS: At any rate, they held a televised press conference related to the Radial 500 tire, at which they showed a Firestone tire that they claimed had failed by the alleged mechanism. Somebody from Firestone pointed out to them on TV that this tire had a hole in it, and that's why it had failed. And they were so hurt by this, they effectively said, "We're going to get you." That's the story I heard from the people I talked with. They socked it to Firestone.

Let's see—at that time, I think Firestone was not yet owned by Bridgestone. But I had to do a lot with the fracture questions. There were issues between the steel belt and the rubber and how cracks formed and how they propagate and under which conditions. Usually in a majority of cases, the failures occurred because people didn't inflate their tires properly.

This was a recurrence also in the Ford Explorer issue. There were two issues. The failures had a preponderance in summers in the southern states, when it was hot. Then failures occur faster, and there were many times—I can't say always—but many times with heavily loaded vehicles. Ford at that time had decided that vehicles that were involved in those accidents had a fairly high CG—center of gravity—so that if something went wrong, there was a large propensity for a rollover.

ASPATURIAN: Yes, I do remember that.

KNAUSS: Ford eventually corrected that. They didn't publicize it, but they corrected that gradually. It had been my opinion, from the testing that Firestone did, and from what I saw, that if Firestone were to recall its tires—take them all off the market—nothing would change, or else it would get worse. I've kept track for about ten years, and that's precisely what happened.

ASPATURIAN: So, Firestone did a recall?

KNAUSS: Firestone had to do a recall; they were forced, yes. The government was involved; the Senate was involved. I remember sitting in the airport in Phoenix, Arizona, where we had been to a birthday of my American "sister" and were waiting to fly home, and there was John McCain, the senator.

ASPATURIAN: Oh, yes, Arizona.

KNAUSS: I thought I ought to go over to him because he was on the oversight committee, and he had pounded the table that Firestone needed to recall. I was going to try to educate him, but I thought, "What does Wolfgang Knauss have to do with a politician like that? He'll talk his way out of it, so it's no use." But it turned out that Ford had to ultimately "eat a lot of cases" that were brought against them because somebody in Texas found an email in the Ford files that talked about this issue and said, "Throw Firestone under the bus." That said a lot about them. And my finding as I was involved in this was that Ford had done a lot of directing to Firestone about how to design their tires. They had people there repeatedly telling them how to change the tire, and that never came out.

ASPATURIAN: So, was Firestone sued? Did they have to pay a lot of money?

KNAUSS: Oh, yes.

ASPATURIAN: Even though it was your conclusion that they were not culpable.

KNAUSS: Once you make a recall, that's like a public admission. But Firestone was Bridgestone now, and you see the Japanese—

ASPATURIAN: It was Bridgestone that did the recall, not Firestone? [Firestone was purchased by the Japanese company, Bridgestone, in 1988. –*Ed.*]

KNAUSS: Yes. The name of the company is Bridgestone Firestone, but it was a Firestone tire—that was the name on it—and the Japanese are not confrontational. They give in, and they try to get around it.

ASPATURIAN: It's an interesting episode in your career.

KNAUSS: So, there was quite a bit of this work that came out of this. So now we're back to the NASA work.

ASPATURIAN: And the Air Force.

KNAUSS: And the Air Force. With the Air Force, I had similar programs, not directly connected with a propagation of an existing crack, but rather—

ASPATURIAN: This is again on solid fuels?

KNAUSS: Yes, right, we're back to solid fuels, but the work had more to do with the initiation of fracture: If you have a propellant, how does it come about that the propellant begins to fail.

ASPATURIAN: I see, I see.

KNAUSS: There is a process that's called voiding. You may recall that propellants are made of little granules of the oxidizer grain in a rubbery binder. When you pull on this strongly, the binder tends to pull away from the rubber grains. You can't bind it well enough to prevent that. Once that happens, how does this separation between the oxidizer grains and the rubber propagate to the next set of grains? And so, we studied that with various means. These are typically three-, four-year programs, and after that you start on another variant of the same problem that you have done before. So that's mainly what I did with the Air Force. Like some of the biomechanics that we talked about [*Session Two*], which was a short program. [*See also Appendix C*]

ASPATURIAN: Was it hard to give up on the biomechanics?

KNAUSS: I would have liked to continue, yes.

ASPATURIAN: I got that impression from your talk with Trity.

KNAUSS: Working with human issues, I would have liked to do that. But like we said last time, it's even harder to see somebody with whom you have worked for three or four years have trouble with respect to employment. I just couldn't stomach that. So, well—where we are today is fine.

ASPATURIAN: Yes, but you were ahead of your time. That must have been frustrating.

KNAUSS: There's another story of being ahead of the time with micromechanics.

ASPATURIAN: Which we're about to talk about.

KNAUSS: Let's see. There were a number of programs I had with Edwards Air Force base. The Air Force has, or had, a major laboratory—

ASPATURIAN: Just up here in the high desert?

KNAUSS: Yes. One of the technical contacts up there, Dr. Jimmy [James] Liu, became a good friend. We have kept up contact. He's a very honest scientist, and he kept prodding away at solving some of these issues that we had started with; we're still very good friends. He couldn't be here for my birthday, so he came from the San Francisco area about a month ago expressly to wish me a happy birthday.

ASPATURIAN: Very nice.

KNAUSS: Very nice. So that was more or less a quick overview of the Air Force program. The Navy—there were basically two programs.

ASPATURIAN: That was more recent, I believe.

KNAUSS: Yes, one was probably 1990s. And the last was the beginning of this century. Until about 2014; that's when I decided to get out of all research. The first program was similar to an issue that confronted Firestone. When a ship has sonic gear for measuring distances to various underwater objects—submarines or in some instances, smaller devices like mines—they need to have a special structure underwater because the sonar signal doesn't go through steel. They attach a very large bulb of steel reinforced rubber to the front of the ship. And they had one occurrence where a destroyer lost its bulb, so to speak, while they were in the Indian Ocean or somewhere that far away, and the ship had to be basically towed back home. They can't routinely afford that kind of investment, so they realized they had an issue with reinforced rubber breaking under varying loads.

ASPATURIAN: I see.

KNAUSS: I think we helped them for about three, four years in steering them how to do their testing and what they needed to worry about in fixing the relatively flexible steel rubber structure to a rigid ship, since that's where you get high stresses, generally.

ASPATURIAN: Who would have designed this for them in the first place, naval engineers?

KNAUSS: Yes. I think actually it was Firestone, but they had a division that I was not even aware of, because it dealt in that special area—so nobody wanted to talk about it.

ASPATURIAN: National security.

KNAUSS: I'm not 100 percent sure whether it [i.e., the designer] was Firestone or B. F. Goodrich, for whom I've also consulted, but I never heard about that division. As I understand it, it started with Firestone and then went over to B. F. Goodrich. I was in direct contact with the naval laboratories outside of Washington—I dealt with them. Which was a very pleasant experience. Then the next, heavier naval involvement was through bonding research. The Navy had a high interest in bonding rubber and similar materials to steel. When a fracture occurs at or near the interface—

ASPATURIAN: Of the bond, where the two are bound together—

KNAUSS: Where the two are stuck together, then the failure occurs primarily through the rubber, and here you deal again with a crack running through rubber. So, we dealt with issues having to do with extending fracture mechanics to this bonding problem. Bonding is a very general engineering application for polymers. Most glues that we deal with today are elastomers or are polymeric bonding agents. So presumably the crack propagation we had developed for NASA had something to say there. We had an extensive program on this, and there were several graduate students who got their teeth cleaned on it.

ASPATURIAN: Cut their teeth.

KNAUSS: On these types of problems. That was a very successful and satisfying program.

ASPATURIAN: What was the concern for the Navy in this bonding interface issue? What problems were manifesting themselves?

KNAUSS: One had to do with the solid propellant rockets; they're bonded to the case. That issue pervaded all the agencies. There were some very major Navy rocket motors that blew up. There was a case where one stage of a Polaris rocket blew up in Utah and damaged houses half a mile, a mile away.

ASPATURIAN: And this was because of a fracture issue involving this bonding interface?

KNAUSS: Yes, right, just that. But then out of that, at that time, I had started to worry about micromechanics—what happens with failure when you talk about domains that are only microns large rather than an inch type of size. So, I wanted to get instrumentation with which we could make measurements at the submicroscopic level.

ASPATURIAN: If I can interrupt for a minute, what was the root of your interest in it? Is it because these micro-fractures can lead to major catastrophic results?

KNAUSS: It was more a new area, new questions. I think this basically all came out of people becoming aware of how little we knew about what happens to solid mechanics at the microscale. The laws and the theories that we have developed—where do they stop with macroscopic versus microscopic size scales.

ASPATURIAN: Right, and the tools were just starting to emerge to look at this experimentally.

KNAUSS: You're aware of my little tiff or misunderstanding with Dick Feynman; but Dick Feynman had— I forget now which year it was.

ASPATURIAN: No, I don't think I've run into this.

KNAUSS: He had given a talk, I believe it was in France, stating that we know basically nothing about what happens at the microscale—

ASPATURIAN: Oh, "There's Plenty of Room at the Bottom." That was 1959.

KNAUSS: Oh, you remember that.

ASPATURIAN: I'm aware of it. Famous talk.

KNAUSS: The first result of that was he put out this challenge to pay a thousand dollars for the micromotor, which is now still on campus, but nonfunctional after all these years. [https://archives.caltech.edu/photogallery/feynman-nanotech.html]

That idea gradually grew to where the mechanics people said, "We know nothing about what happens at that scale," and so I thought it is time in mechanics that somebody develop a method to look at these microscales. And something else happened, for which I just got a prize this October after twenty-five years, and that was the new method of measuring deformations and strain through digital computer methods. It's called digital image correlation.

ASPATURIAN: Would this have been in the 1970s, 1980s?

KNAUSS: Let's see, 1980s, 1990s. The other thing that happened at that time was that the scanning electron microscope was invented—the scanning force microscope had not been invented yet. And it occurred to me that here is a new method to measure stress and strain using digital information—point by point information—and here is a new submicroscopic device that can gather digital deformation information. Let's go and marry those two. Nobody wanted to buy it.

ASPATURIAN: You wrote up a proposal and couldn't get funding.

KNAUSS: You couldn't get funding, yes. Or you went to talk to somebody—"Oh, that's a nice idea"—then never heard from them. But I finally got $30,000 from the National Science Foundation, and that was only because Lalit Anand, who was a professor of mechanical engineering at MIT took a stint for two, three years as a program monitor at NSF. I submitted this proposal, and it struck a note with him. He got me the money for $30,000 just to construct the tunneling microscope. There were tunneling microscopes you could buy, but they were not adapted to what you need in mechanics. We had some on campus, but I couldn't work with them.

So, I had a French graduate student who was very smart, but he didn't want to tackle that problem; he said it was too hard. He was not really an experimentalist, initially. I said, "I'll get you help from another good student, Carl Schultheisz." He started on it, but he didn't need much help from Carl Schultheisz. He did an excellent job in building this device.

ASPATURIAN: What was his name?

KNAUSS: It was Guillaume Vendroux. "Guillaume" is "William" in French. You probably know that. But he built a tunneling microscope. I still remember the joyous day when we "saw first light." He put something in there, and he could measure things.

ASPATURIAN: How large was the sample you were looking at? I imagine it was in microns.

KNAUSS: The regions where we looked at the samples are bigger, maybe tens of microns with submicron resolution. He also had to build a stage that took a two-, maybe three-millimeter specimen. You could grab on it mechanically on a larger scale, but then you looked at a smaller region and put in a defect. The good thing about this was you didn't really have to have an electrically conductive specimen for looking at things; even a polymer would work, if you put a very thin film of a metal on there. We initially used that extensively to see what happens to a composite material which is made out of very thin fibers. Here's something else I've forgotten in connection with NASA. There was another long-lasting program that has to do with the composites.

ASPATURIAN: Okay, we'll go back.

KNAUSS: At any rate, this was very successful.

ASPATURIAN: Were you the only lab in the world doing this at that time?

KNAUSS: At that time, yes. There's now a society for digital image correlation [International Digital Image Correlation Society], and the past president for that was one of the first inventors of the digital image correlation method. He has made his whole career on that, including forming a company that sells equipment and software for it. So, he's flush in money, and he supports Society for Experimental Mechanics meetings. He was responsible for Vendroux and me to get the founders prize from the society in October [Founders Award of the Digital Image Correlation Society]. I went to Portland, where they had their meeting, and he pointed out—which is true—that we got this prize because we were so early.

ASPATURIAN: You were the first.

KNAUSS: Nobody else did this until the next attempts started about ten years later. So here is again a case where you simply have to try to do something that doesn't bring fruition until much later.

ASPATURIAN: When this field took off a decade to two decades later, was it independently rediscovered or was it based on your original work?

KNAUSS: I don't think that anybody invented anything newer in that direction. People find other ways of doing it, but in order to measure strains directly at that scale, I haven't seen anything else. So that was kind of satisfying.

ASPATURIAN: I can imagine.

KNAUSS: And Guillaume Vendroux will get his part of the prize when the society meets in France next year. [The scheduled 2020 annual meeting was postponed due to the Covid-19 pandemic. *–Ed.*]

ASPATURIAN: Did he focus his career on this or on something different?

KNAUSS: No, he did not. In fact, it's very interesting. At the time when he graduated in the 1990s it was very difficult in the United States to get a professorial appointment for young people. I can look it up in his thesis when he graduated, I don't remember the precise year.

ASPATURIAN: I can look it up, too. [1993]

KNAUSS: The University of Texas, where we have a number of students on the professorial staff, held a position open for a quarter year for him to make up his mind about whether he wanted to stay in the U.S. or go to France. And he decided he would go home.

I thought it was remarkable that another school would hold up an appointment because at the time these jobs were gone in a week. He went to work out of Paris for an automotive supply company's engineering department. The next thing I heard he was working for a company for the highspeed train TGV. He got higher up in the management to the point where, when California started to develop the highspeed train, he was over here talking with them about contracting, etc. Unfortunately, I was in Germany at the time, and so we didn't meet. I wrote him not long ago that I was happy that he didn't get involved with California because of the mess we have. Then there was a hiatus when we didn't communicate; he is now at Airbus. He is the director of their whole digital program, as I understand it. Something like a vice president. So, I thought he has done very well.

ASPATURIAN: He didn't want a career in academia, it sounds like.

KNAUSS: But this is the problem with most of the French students we have had. They just want to go home.

ASPATURIAN: Well.

KNAUSS: All right. So, this maybe is what I can think of about the digital imaging stuff. We then had a program with the Navy that was not so successful. We tried to measure with the digital imaging process—this gets hard to explain—but we wanted to form sheets of material that were only a few atom-layers thick. That's where we ran into problems because we're now into the nano—

ASPATURIAN: Yes, that's the nano realm.

KNAUSS: And we thought we might be able by making several layers to get into the submicron range. We were at about a tenth or a hundredth of a micron. So that's within a factor of ten or a hundred of where we wanted to be. But that was just a little bit too far. So that program stopped, and we didn't go farther than that.

ASPATURIAN: You wanted to talk about the NASA composites research.

KNAUSS: Yes. We had another graduate student, Jim [James] Starnes, who got his degree here with Ernie Sechler and Chuck [Charles D.] Babcock.

ASPATURIAN: What year are we talking roughly?

KNAUSS: I think that would have been in the 1970s.

ASPATURIAN: Okay, I can double check that, too, not a problem. [It was 1970. *–Ed.*]

KNAUSS: I don't have his thesis, so I don't remember. After he graduated, he went to NASA, Langley Air Force Base. And after a few years, he was head of the composites branch, and I remember going to a meeting where Jim was; we'd always been friendly, and he said, "Wolfgang, let's go to dinner." And we went to dinner, and we talked, and he said, "Wolfgang, I've been thinking about you; we have this problem in composites called delamination." When you hit a composite with a bullet or so—a composite that is made up of layers of reinforced glass or fibrous materials layers—they split between the layers.

ASPATURIAN: These were composites they were working with at NASA? NASA-specific composites?

KNAUSS: Yes, that's right. Well, they wanted to build their spacecraft out of that, or airplanes, and he was responsible for major largescale tests. One test cost a million dollars or more, and so he was very concerned about what could happen as a result of a technician dropping a wrench on the airplane and not being able to detect possible damage. And so, he said, "We're worried about these possible de-laminations spreading, and that's a crack, Wolfgang. Can't you do something?" This also happens when there is a delamination crack inside a composite panel, and you compress the panel in a direction that is axially parallel to the plane of the crack—you can get local buckling. You understand what buckling is?

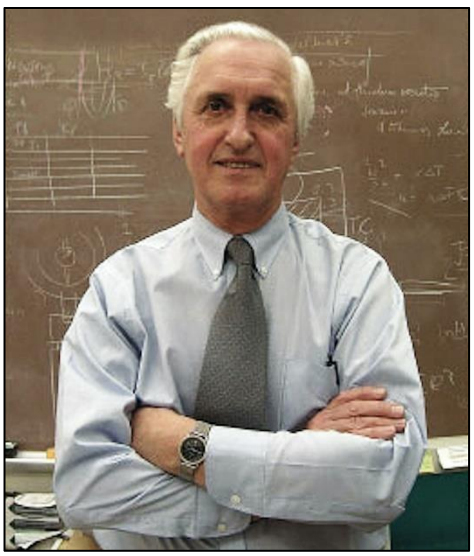
ASPATURIAN: I do.

KNAUSS: Okay. When something buckles, there are forces that want to spread the crack. I said, "Sure, Jim, we can look at that." And so out of that dinner meeting came maybe twenty years of support from NASA for the composites field. My last graduate student, Luis González, worked in the close of that program. Jim Starnes had a brain tumor and died prematurely [2003]—I don't think he was older than 60 or 65, somewhere in that range. Jim had transitioned to a newer, more responsible job and had given his earlier position over to somebody else, but when Jim died, I lost all support. Except I had a proposal in, and this manager had probably already signed off on all the proposals. So when we got toward the end of the program, when my last student was within a year of finishing, and NASA said, "Hey, we made a mistake, we didn't mean to, please give us the money back." I was fortunate. [Laughter] Our proposal acceptance was a contract: "What can we say; we have these obligations to this student—and whatever's left over, we'll give back to you."

ASPATURIAN: Were you able to diagnose and solve the problem that he had initially presented to you?

KNAUSS: That was solved, yes; Chuck Babcock and I worked on those problems together. I did the fracture research and combined that with Chuck's buckling research, and we had a student who had first worked for me, Herzl Chai, but he and I didn't get along too well. But he then worked with Chuck Babcock, and we got along well. We turned into friends; and he has come to some of my birthdays from Israel, where he is now a professor in Haifa. This was the first delamination study that was performed in this field. I thought that research was very useful. Anything that came after that was simply a computational refinement of what we had done. From there we went on to deal with failures in composites between fibers at the microscale and various issues like that. It was never a very large program, but it was steady and supported a student and sometimes a postdoc. That was a very satisfying program.

ASPATURIAN: Of these many varied areas in which you worked did you have a favorite?



Knauss requested that campus custodians "never clean my 'black' board because it had happened too often that things I left on the board got lost overnight."

KNAUSS: Yes, we'll come to the dynamic fracture. Well, the viscoelastic fracture is a very long range, very satisfying issue. These little building blocks I talk about—those are the ones that stick in my mind now that I'm now grown into my current age. And when you look back, they leave the best impressions. But I just didn't want to forget about the NASA involvement with the composites.

ASPATURIAN: That's an interesting story

KNAUSS: The bonding issue overall was a very long lasting, fruitful interaction. I remember how we started. As a matter of fact, there's another contribution that I think I was involved in. I can't say I was responsible for it, but when I first latched onto the bonding issue, there had been some people involved from the mechanics committee. My professor Max Williams had contacts with lots of people, so I don't think I can claim that I invented knowing about fracture and bonding first; I probably heard it from him. But it was a very natural thing to follow up on. He had contacts at the Navy, and there were annual conferences on bonding issues through the Gordon Research Conferences. I had a student at the time by the name of Ken [Kenneth] Liechti, who took hold of the problem of bonding issues. We built a huge facility—in our terms—to very precisely controlled loading conditions

ASPATURIAN: Here in Firestone?

KNAUSS: Here in Firestone, yes. When you go out of my office, I'll show you a picture that is still on the wall from that time. But the issue was that we needed to have a thin bond line between two materials, and we took two plexiglass sheets and bonded them together with a very thin polyurethane rubber. But since the layer of rubber is so thin, when you pull on it you don't get very much of a relative displacement between the bonded plexiglass components—and how do you control a displacement that's a few microns large? The way we decided to do that was to have heating coils on aluminum rods. When heated, they would expand and if that bonded assembly is held in a rigid frame—a stiff frame—you can control through the temperature how much you pull on this arrangement.

ASPATURIAN: Very ingenious.

KNAUSS: So, he did that very well, and we could follow how cracks propagate along the interface. It was not the way we thought it was, and that was very interesting—that the crack would not propagate like a nice straight crack but undulated sometimes from little bubbles ahead of the crack. So, there were kind of new visions.

ASPATURIAN: There had been no theoretical predictions.

KNAUSS: Not on that.

ASPATURIAN: This was a surprise. Okay, yes.

KNAUSS: Yes, a bit of a surprise, yet I'm not sure that when people see it—nobody is likely to say, "Yes, I got this idea from you, Wolfgang." Very few people in our business tell where they get their ideas. They're all "their own." That's the way life is.

So that also came out of the Navy program, and Ken became heavily involved in the organization for the Adhesion Society. I went to many of their meetings in the beginning, but then I lost interest in the adhesion, but I think that through me, Ken had a lot of influence in introducing mechanics into the Adhesion Society. Because again, like the solid propellants, that was dominated by—

ASPATURIAN: Chemists?

KNAUSS: Chemists. Because they made the adhesives. Then they had simple tests. One of the continual arguments was about a certain test they used—the shear lap test—that I always thought was kind of useless. It's kind of like a screening test but not a scientifically meaningful test. That idea basically got me involved with the Navy; the program manager at the time for the Navy was Larry Peebles. He was a chemist but very receptive to mechanics, nevertheless. But I think through Caltech we had a good influence on the Adhesion Society; then other mechanics-type people also joined from as far as England. So, I think that field is in fairly good shape.

ASPATURIAN: It became more interdisciplinary, it sounds like.

KNAUSS: Yes, that's the right term. Correct. So, I think that's okay. Let me finish with the last Navy project. I may have mentioned this already before, but let's start again. One of the issues in viscoelastic material behavior is temperature behavior. When you heat this material up, deformations—all processes—occur faster, and there's a rule that has been widely accepted as basically being right. It was established by various researchers in Europe in the 1920s and earlier, but then it resurfaced here without reference to these people through a 1957 paper by Williams—not our [Max] Williams, but a graduate student [Malcolm L. Williams]—and Bob [Robert F.] Landel and [John D.] Ferry. Ferry was the professor; the other two were students.

ASPATURIAN: Here at Caltech?

KNAUSS: No, University of Wisconsin. And Bob Landel came to JPL, so he was a colleague, and he took over one Air Force program when Max Williams left and handled that on campus here with Nick [Nicholas W.] Tschoegl [professor of chemical engineering, emeritus; d. 2011]. So, this WLF principle—as it's called, because of Williams, Landel, Ferry—governs many, many polymers in how they behave under different temperatures.

ASPATURIAN: Under changing temperature conditions.

KNAUSS: Right. So that was fundamentally and thoroughly investigated by a standard test procedure in the laboratory, where you have test machines that run, well, slowly—let's leave it at slowly—but not explosively. Then—I forgot under which program this was; I think it was NSF—I had a postdoc, Sairam Sundaram, who came from Brown University, and he wanted to do computations rather than experiments.

ASPATURIAN: To do computational modeling of this theory.

KNAUSS: Yes. So, we modeled—this had to do with the Navy program.

ASPATURIAN: What decade again are we talking about? The 1990s?

KNAUSS: I think it was also the early 2000s.

ASPATURIAN: Computations, I would think so.

KNAUSS: Yes. This goes back to explosives and propellants. Explosives had the same physical consistency of solid propellants, but they have different ingredients that react more rapidly. As a matter of fact, I think solid propellants were developed from explosives by people saying, "Hey, we'll put in something else—not so much accelerator, or something like that—in there." But there had been a phenomenon where, under very rapid highspeed impacts, you could ignite an explosive and you'd get an extremely high-rate reaction, and the question was where does the source of the temperature rise—the heating—come from? They call them hotspots. Nobody had any idea how hotspots arose.

And so, with this postdoc, we tried to work out what happens between two grains of rigid solids when they get sheared under pressure. Because what we found—and this is another outcome of the NASA work—is that if viscoelastic material is subjected to high pressure—many atmospheres—this will also change the viscoelasticity, because now the atoms are forced more closely together, so they don't move past each other so easily. If you do that under high pressure and then deform it, it generates more energy dissipation in rubbers than in the absence of pressure.

And so, we had the idea that if we pressurize the viscoelastic material—computationally—and we enforce shear deformations that would then generate these local hotspots; and we calculated that one could get up to temperatures and energy expenditures that could ignite the viscoelastic explosive material**.** People didn't make much of it; they didn't like it because it wasn't their own idea, I guess, but I think if I had it to do over again today, and I was forty years younger, I would do more with the computational effort than we could at the time.

But when that paper was published, the reviewer—he lived in Washington—was somebody who knew something about polymers and something about explosion, but his argument, like anybody else we heard from, was, "Yeah, but how do you know that this WLF principle that you dealt with applies under these high rates of loading? It's only been demonstrated to hold for these slow tests."

Even before that, I had already talked to various agencies, saying, "You know, this is an issue that's going to come up somewhere. We ought to investigate to what extent the WLF principle can be extended to these high rates of deformation." And nobody wanted to spend money on it because they didn't have the immediate relevant problem before them.

We talked about the Yemen issue—the destroyer *USS Cole* being blown up.

ASPATURIAN: I don't think we talked about this.

KNAUSS: We didn't?

ASPATURIAN: I don't think so.

KNAUSS: This was an attack on the ship, and the Navy wanted to find mitigating measures to protect ships in the future from experiencing such devastating effects from an explosives attack. [*USS Cole,* a U.S. Navy guided missile destroyer, was attacked by two suicide bombers in October 2000 while refueling at a port in Yemen. *–Ed.*] Terrorists had taken an inflatable boat, I think, and shot a projectile at this destroyer. It killed seventeen sailors and wounded thirty-nine. So, the Navy labs had the idea to coat the inside of the ship's steel plate with a rubbery polymer so that they could limit the damage. They ran big tests that involved shooting at materials of different types and thickness. And so somewhere early in the 2000s, maybe 2001 to 2003—in that time range—we had a big meeting where all the mechanics people who had to do with high-rate deformations got together in Washington and talked about what to do. They had a presentation of having a rubbery polymer—polyurea—on the inside of ships that could mitigate the damage. I was the only viscoelastician there, and I still take it to the credit of the ONR [Office of Naval Research] program manager, Roshdy Barsoum, who ran this meeting, that he listened to me. I said, "You've got a viscoelastic problem if you deal with rubber because it doesn't behave like you think it does under these high impact speeds."

ASPATURIAN: Which you knew from your earlier experiments.

KNAUSS: Right, and from the principles of viscoelastic theory.

And so, he listened to that, and he supported us without further question through continuation proposals with $150,000 to $180,000 dollars a year for about ten years. After I had officially retired, I was very fortunate to have a Chinese postdoc—an older postdoc, but he wanted to have some experience in the U.S.—who was able to do the calculations and the experiment, and we did the highspeed impact experiment downstairs in Dr. [Guruswami "Ravi"] Ravichandran's [Goode, Jr., Professor of Aeronautics and Mechanical Engineering] laboratory. We shot bullets and things to see how we could measure the response and found we could fit these responses to this viscoelastic material model and for me, this was kind of like a breakthrough. So, this has put that original question largely to rest.

ASPATURIAN: The WLF principle held up.

KNAUSS: That's right. And it's amazing. I was surprised myself how well that worked.

ASPATURIAN: That is amazing. What was the name of this postdoc, do you happen to recall?

KNAUSS: I have to look that up, too. [*WK subsequently added:* Jianheng Zhao]

ASPATURIAN: That's fine. You will get the transcript back and you will have the chance to do this.

KNAUSS: I'm embarrassed.

ASPATURIAN: No; everybody I talk to blanks on some of this.

KNAUSS: I'm a number of years away from all of this, and so.

ASPATURIAN: It's very, very common.

KNAUSS: He was good. At that time, I was also heavily involved in some nature photography, and I wanted a relatively highspeed desk computer. I was going to order a system that I saw proposed in a photo magazine, and he said—and this had nothing to do with his job—"Oh, I can do better for you with a Dell computer." And he modified the Dell computer, which I still have.

ASPATURIAN: Where is he now?

KNAUSS: He's back. He now runs a national laboratory.

ASPATURIAN: He went back to China?

KNAUSS: He went back to China. The FBI wanted to get one of his collaborators over here for some time because there was a time when they wanted to know about his government lab's science in highspeed failures. But that never came to fruition, so maybe that's okay. But he was excellent.

ASPATURIAN: It certainly sounds like it.

KNAUSS: Then I think unless something comes up, that leaves the Navy stuff done, and we need to talk about the NSF [National Science Foundation] because I consider that one of my major accomplishments, but I don't know if you want to do that today; we've been at it for an hour and a half.

ASPATURIAN: An hour and fifteen minutes. We have another quarter of an hour; what do you think?

KNAUSS: Okay, let's see, we can probably do it. This is now straight fracture mechanics—no viscoelasticity. After I got my degree, there was a question for a while: What do I do with all my fracture viscoelasticity research, because the solid propellant issues slowly went away; they kind of died, and there was less research money in that area. That's how I got into the bonding. Then there are time-dependent effects—what we call dynamic fracture. For example, if you have a steel plate and you run a crack through it, it runs at the rate of several kilometers per second, very rapidly, but nobody knows how that goes. There was a linearly elastic analysis for a mathematically sharp crack running at very high speeds, but the maximum speed with which you could run a crack was about two or three times higher or more than what one measures in the laboratory.

ASPATURIAN: I see, I see. So, it was beyond observation.

KNAUSS: Right. That was a big question. The man behind that theory was Bertram Broberg, a professor in Göteborg—Gothenburg University [Sweden]—and I had him here also as a visitor. He had an international reputation for the work he had done, but during the time he was here, half a year, nothing new came to him to help with my particular problem. I started this program, and it was strange, one of the only two or three times it happened to me—I was basically approached by the Navy twice for help. This one was also at a meeting where Cliff Astill, who was then the engineering mechanics program manager at NSF.

ASPATURIAN: And what year, roughly, would we be in?

KNAUSS: It must have been very early in the 1970s. He said, "Wolfgang, you work in fracture, and we have a shortage of experimental programs in fracture here; they all come in with theory. Can't you do something?" "Yeah, I can do something; let me go and think about it."

We had, with Chuck Babcock, done buckling problems under explosive loading with an electro-mechanical device, which is basically two sheets of metal separated by a narrow space. Chuck didn't do that here; it came out of his consulting for McDonnell Douglas Aircraft. I think they did it with wires, such that if you run a current through two wires in opposite directions, each wire establishes its transient circular magnetic field, and since the current runs in opposite directions and magnetic fields also run in opposite directions, they want to separate. There's a force to separate them. I thought, we can do dynamic fracture under tremendously fast loading if we take a big plate, cut a crack with a small thickness in it and put in a copper strip that is double bent back like two wires with a loop, and separate it with an insulator so the current has to run through it without shorting out. Then we can generate, with a sufficiently fast and high voltage, a current in there that drives these cracked surfaces apart.

What I considered as the most important thing was that if we made this big enough, then we do not have to worry about stress waves from the vicinity of the crack tip running to the boundary and coming back to influence what happens at the crack tip. There is an analysis for this problem— "closed form" analysis—about what should happen to the crack running in an infinite sheet—or two dimensionally in an infinite body. It's a quasistatic analysis, so with our experiment, we're closest to what anybody can do with the problem of an infinite domain with a crack that is pressurized.

ASPATURIAN: How large a scale were you working on?

KNAUSS: Our specimens were typically something like 1x1 meters.

This was the year I went on sabbatical for six months, 1976. I had a graduate student, [Krishnaswamy] Ravi-Chandar, who had just come to do his graduate studies; not our Caltech Ravi, but Ravi-Chandar, who is now a professor at the University of Texas, Austin—Austin Ravi. I left him here, basically, for half a year alone. And he said later, "Wolfgang, that time was a wonderful introduction to Caltech; I always enjoyed that. I could go to the library; I could study what I wanted to." He knew roughly what area he was going to work in because I had already worked in the 1970s with a PhD student, Gordon Smith, on the feasibility of the experimental approach. As it had turned out—and this is the only good thing I ever got from Hans Liepmann—Liepmann had worked on the influence of magnetic fields in fluid mechanics, and he had a battery of ten high-powered capacitors, which he "graciously" loaned us after his student spilled a tankful of mercury in the lab, and he stopped those experiments.

So, these capacitors had been sitting around. Gordon Smith had worked with them to build a facility that he used for the first of these fast crack propagation experiments. So, I knew that this experimental approach was workable. Ravi-Chandar then refined that facility, and he did all the desired experiments in a perfect way with the advanced design. He has an observation power and deduction power that's really strong. So that was a great success. We could demonstrate why a crack doesn't run at the analytically predicted speeds.

I had had in my mind the idea that the reason the analysis didn't match the experiment was that the analysis was based on small deformation-linear elastic behavior, and when you get to the crack tip, the material behavior is no longer linearly elastic. In fact, it begins to microfracture, and the crack propagation process is more complicated than the simple analytical model assumed and used. But then the question was: "Can we make the point that this is the reason why the crack doesn't run so fast"? And I had another student, Peter Washabaugh—PhD in 1990—continue on with that. I said, "Can we reason that if you have a material that has virtually no strength and you propagate a crack through it, then its behavior should be nearly linearly elastic because you don't put high stresses at the crack tip on it?"

ASPATURIAN: I see.

KNAUSS: If we can make the experiment such that we have a material with a very weak interface, and if we propagate a crack, and make that interface weaker and weaker, we should be able to increase the speed and, in the limit, maybe get to the speed that this linear analysis predicts. He did that very well. We stuck, I think it was plexiglass pieces, together and depending on what solvent we used, we got bonds of various strength, and we decreased that strength one by one to the point where there was great difficulty to handle a big sheet, and then we used the same electrodynamic magnetic device to propagate a crack, and, sure enough, as we made the bond weaker, the crack speed increased. So, in the limit, the theory was vindicated but only for materials that don't have any strength. But now people know, "Hey, now we understand why the discrepancy is there."

ASPATURIAN: This is partially the work that got you elected to the National Academy of Engineering, as I understand it.

KNAUSS: I'm sure that it helped.

ASPATURIAN: Yes, it's cited.

KNAUSS: Yes. The dynamic fracture. That had a lot of ramifications. Ravi—Austin Ravi—is a very good man. He has continued many, many things in that field. Excellent work. One gratification I have is that there is a very well-known fracture mechanician Jim [James R.] Rice. He got his degrees from Allentown [Lehigh University], then went to Brown University and then to Harvard. He's kind of an international, very well-known individual; he and I didn't get along together too well. He did something non-kosher one time that displeased me greatly, but I heard then he went to a seminar where Peter Washabaugh gave a talk. And apparently Jim Rice was impressed and said, "Hey, that is really something." That's the first time I heard—second hand—Rice say something positive about any of my work. So anyway, that dynamic fracture work, I think, was a major achievement.

ASPATURIAN: A major capstone in your career.

KNAUSS: Yes. Again, it came through again basically a contact at a conference.

ASPATURIAN: What are some of the real-world applications of that research?

KNAUSS: Of this highspeed fracture?

ASPATURIAN: Yes, I'll ask you that, and then I think we'll wrap up this session.

KNAUSS: Well, it relates to anything where cracks run fast. I think the major influence today is in the modeling of crack propagation—computer modeling—people build the models—

ASPATURIAN: I see, I see.

KNAUSS: I just recently received another publication where they showed how microfractures form ahead of the crack. These are the pictures that Austin Ravi had already shown years ago about how when the crack branches and tries to go from one crack to two cracks, it doesn't quite make it, so you get a very rough surface. So, he illustrated all these processes, the results of which were well known, but that nobody understood why they happened. And there was a laboratory in Freiburg, Germany, that had dealt with highspeed fracture, and they had never thought of doing anything like this. I think the way highspeed fracture is dealt with today is heavily influenced by work done here.

ASPATURIAN: So, its value is in fundamental understanding.

KNAUSS: Yes, right. I couldn't think of an application myself. Maybe Ravi or another student could, but I think I'll have to leave it there.

WOLFGANG KNAUSS

SESSION 4

January 9, 2020

ASPATURIAN: One of the things you had talked about earlier and said you might want to go into in more detail later was the dominance of fluid mechanics in GALCIT for many decades until 2004 when Ares Rosakis [von Kármán Professor of Aeronautics and Mechanical Engineering] became the director. I wanted to ask what impact you thought that dominance had on the program. Were there tensions; were collaborations impeded? How did that strike you?

KNAUSS: There was generally not much cooperation between fluids and solids. Since the fluid mechanics people had the larger number of faculty members, they essentially dominated what was going on in GALCIT.

ASPATURIAN: In terms of resources? Grants? Decision making?

KNAUSS: Decision making, and where appointments would be made. This one I can talk about—which isn't very pleasant—because I got this story from a deceased faculty member. He told me that the director at the time, when things were a little bit more dicey really would have liked to get rid of solid mechanics.

ASPATURIAN: This was Dr. Liepmann, I assume.

KNAUSS: Yes. And there were only two faculty members [in solid mechanics] here, namely Chuck Babcock, who died early, unfortunately, and I. My assessment of why not more happened in that direction is that Hans Liepmann was not very astute in dealing with people on a personal basis. He was the old German professor who wanted to dominate, and very little else played much of a role. But he needed help in dealing with the personal issues, and Chuck Babcock was very good with that. So, he became to a large degree dependent on Chuck in that context. You might say, how do I know that? Chuck never talked about that directly, but he and Hans were together quite often, compared to anybody else from solid mechanics with an earlier director. I believe at the time, all the professors in aeronautics and fluid mechanics were Hans Liepmann's students. [*See also Session Three*]

ASPATURIAN: Everyone who became a professor had been a former student of his?

KNAUSS: I'm trying now as I sit here—my age may play a game on me—but as I count them off on my fingers, each one that I remember was a student of Hans Liepmann.

ASPATURIAN: Had been his PhD student?

KNAUSS: Right. And so, there was a different bond. Of course, Chuck Babcock was also a student here. He was Ernie Sechler's student. I was Max Williams' student; however, Max had left, so I was dangling. And I believe that one of my issues from the time that Max left was that I didn't have a spokesperson for me.

ASPATURIAN: You talked about that earlier; how your mentor had gone. [*Session Two*]

KNAUSS: I think we talked about that and how others who had support made it into the Academy of Engineering very much earlier, in their assistant professor times.

ASPATURIAN: Was hiring former PhD students to that degree unusual? Because Caltech doesn't do that so much these days.

KNAUSS: Not at the time.

ASPATURIAN: Not at the time.

KNAUSS: But in other schools it is looked down on. For example, in Germany you cannot get a professorial appointment if you got your degree at that university.

ASPATURIAN: Ever or just not immediately?

KNAUSS: It's just not done.

ASPATURIAN: I see, I see.

KNAUSS: Here it has become difficult in the last twenty, thirty years that you get directly hired [out of the PhD stage], except in certain circumstances. That we don't need to talk about, but there were issues in how to handle staffing at universities, and so the administration— I don't think it's a bad idea to hire former PhD students onto the faculty. It's a bad idea from the point of view that you're never quite— It takes a long time before you become a colleague in the true sense when you were originally a student.

ASPATURIAN: Do you think then that the reason Liepmann tended to appoint his own students to tenure track positions had something to do with the fact that he felt they wouldn't challenge him?

KNAUSS: I don't think that so much. He didn't worry about being challenged.

ASPATURIAN: Okay, so that wasn't his issue.

KNAUSS: I think he just thought they were great.

ASPATURIAN: The best.

KNAUSS: Yes, the best. That's why I have issues with the policy of not appointing your own students. Caltech is at the peak of the academic, scientific circles, and when you see a good person here, you know them. So, you have to deal with that situation differently. We still see that. We do get our own students appointed. Usually from what I have seen, it really isn't a drawback for the institution. We do have good people. I look at the ones especially from the last twenty years when this unofficial policy has been in effect. To the extent that I know the individuals, I think they were good choices.

ASPATURIAN: I was just struck by the extent to which [under Liepmann] this seemed to be going on.

KNAUSS: Right. And I think that is kind of unusual.

ASPATURIAN: That's what I wondered about.

KNAUSS: It has caused friction. Friction is never good in a group, and my feeling is, that is why, in part, fluid mechanics kind of lost impetus, both within GALCIT and on the outside.

ASPATURIAN: When do you think this happened, after Liepmann's directorship ended, or did it start before then?

KNAUSS: I think it was in progress under Hans Liepmann. These are things that you cannot put a date on.

ASPATURIAN: You can't quantify, no.

KNAUSS: But the net result was that.

ASPATURIAN: How about when Dr. [Hans G.] Hornung [Johnson Professor of Aeronautics, Emeritus] took over? You and he both had Germanic roots, German background. Was the relationship better between solid and fluid mechanics under his leadership? You know, we can take out or close portions of the history.

KNAUSS: No, I make one point: There have been chairs appointed within GALCIT that were for a number of years reserved for fluid mechanics. Not because anybody said it.

ASPATURIAN: It was just understood.

KNAUSS: It simply was not considered due course for solids. That's something I can and want to talk about, because after I'd gotten into the National Academy [of Engineering], that was a good reason to make an appointment [i.e., named chair appointment] for somebody of that stature. I wasn't going to fight that if it didn't happen, but then I heard from my colleagues in solid mechanics—by this time we had grown to a bigger group again—that this didn't seem fair that all the chairs go to fluid mechanics. I took it upon myself to go to the provost and say, "I'm not here for myself, but this is what I heard."

ASPATURIAN: This would have been Steve Koonin at that time; or was it still Paul Jennings?

KNAUSS: It was Steve Koonin, yes. And it did change.

ASPATURIAN: So Koonin listened to what you said.

KNAUSS: Yes, and now everybody in solid mechanics has a chair. And I can't think of who among the older colleagues doesn't have a chair in fluid mechanics. But, anyway, there has been a change in that direction. And it was things like that—where the fluids people simply made a decision. If a decision is made in the director's office, who's going to question it when you go to the administration. "They" think that's agreement by the faculty.

ASPATURIAN: You became an NAE member in 1998. I don't have this in my head; I'm looking at my notes.

KNAUSS: I think that's right.

ASPATURIAN: You became von Kármán Professor in 2001. Was this part of the result of your talk with Steve Koonin?

KNAUSS: That's right, yes. Well, but there were numerous significant honors before that that could have precipitated this.

ASPATURIAN: And they did not.

KNAUSS: And that is why I heard the rumors from my colleagues in solid mechanics.

ASPATURIAN: So, it sounds like there was kind of an inner circle with the fluid mechanics people.

KNAUSS: Yes.

ASPATURIAN: And certain assumptions and decisions were just made on that basis?

KNAUSS: Yes. Right. And there were some very strong individuals who spoke up authoritatively because they felt that this was the background, and they could do that. But it also led to issues. For example, after Clark Millikan died, Ernie Sechler became—not the GALCIT director; the term director was eliminated—the executive secretary [executive officer for aeronautics] or something on that order.

ASPATURIAN: He was sort of like the de facto director but did not have the title?

KNAUSS: Yes, he was in charge. I believe Hans Liepmann did not, I don't think, really respect Ernie very much. [*See also Session Two*] Ernie had a lot of experience; he was the first or second—I believe it was the first—PhD that von Kármán had here, so he knew GALCIT from the ground up. Hans Liepmann came later, but I don't think they ever saw eye to eye; they were not close associates. I do not know the workings behind closed doors, but Brad Sturtevant was then appointed as executive—

ASPATURIAN: Something or other. I can look it up [executive officer for aeronautics].

KNAUSS: It comes back to me. It's one of these things—you go home, and say, oh, my gosh, why didn't I think of that? Then Brad did this for a number of years, maybe two, not terribly long; and then came the time when Hans Liepmann wrote a large proposal with other faculty members and left Brad Sturtevant off the proposal list. And within a week, that office was empty. Brad said, "I'm not going to do all this work for nothing, and you cut me out." Then came the time of, "What do we do with this position?" And

Hans Liepmann would take the job only if it was made director again. That's where we are. So, there's a mixed bag.

GALCIT has been a very good institution and has a high reputation, as far as I can tell, but it has had its internal personality issues. Maybe you have to expect that in organizations like this. Especially at Caltech; maybe we'll talk more about that—some on this side, and some on that side. It's a mixed bag, but from my experience at other schools, this is still a good place.

ASPATURIAN: You mentioned that both Rosakis and Ravichandran were people you recruited to Caltech.

KNAUSS: Yes.

ASPATURIAN: Each of them subsequently became head of GALCIT. And then head of EAS [Division of Engineering and Applied Sciences]. So, I'd like to have you talk about your role in bringing them here; and whether that was difficult, given the fact that fluid mechanics dominated the scene?

KNAUSS: I think with Ares Rosakis—there always had to be a job opening. I don't think I would have had the clout to get an additional faculty member appointed.

ASPATURIAN: This was in '82 that he came, so somebody must have retired or gone to another school.

KNAUSS: Yes, right. At that time, it was Chuck Babcock and I who were here; and I was working on the highspeed fracture work.

ASPATURIAN: Which we talked about last time.

KNAUSS: And Ares Rosakis, who came from Brown University, was working on highspeed fracture. I visited him there; I was impressed with what he was doing, and obviously we made the successful point that after Ernie Sechler had retired, nobody had been here in that area and that Ares Rosakis was a good choice.

ASPATURIAN: Did you have a hard time convincing GALCIT to hire him or not?

KNAUSS: I forgot who the chairman of the committee was. I was not. I don't think I was even a member of the committee. That's a little bit hazy. I do remember the Ravi committee because I was chair—

ASPATURIAN: We'll get to that in a minute.

KNAUSS: But with Ares, after he came, okay, I could make enough of a point that he had good capability, and that we would have some initial equipment that he could use—my equipment— so there wouldn't be an immediate large outlay; it was at a time when you didn't get a million or two million dollars for startup, which you have today. So that may have been a consideration, but I think Ares does cast a very good image when he presents something.

ASPATURIAN: I see, so that was in his favor, too.

KNAUSS: But that ran well, and then when Chuck Babcock died, there was a year, two years, of a kind of hiatus when we were low on manpower, and then it was decided we ought to look for somebody. Ravi, of course, had been here as a postdoc; he also came from Brown, and he wanted to work in fracture, but he had done experimental work and said, "Can I maybe work a little bit more on analysis now rather than experiments?" I said, "Fine, okay; here's a problem," and it was a difficult problem, but he wanted to do something numerical. When you go out in the hall here, there's a picture on the wall about this big with a feature like a parabola on it, and that's the boundary of a crack in a rubber sheet.

ASPATURIAN: I see, which is what he was working on?

KNAUSS: I was working on it; you know, I was interested in a large deformation, crack tips. And so I said, "Can we handle this numerically?" And he said, "Sure, let me try." And he did this very nicely. He was very good. But that didn't hit me so much as the following: Max Williams became famous for his analysis of the crack-tip stresses. That was a very unique, very important, contribution. He showed there was a certain universality to that stress field that everybody had to draw on, and did, in small deformations. We had Eli Sternberg [professor of mechanical engineering, emeritus; d. 1988] and Jim [James K.] Knowles [Keenan Jr. Professor of Mechanical Engineering, Emeritus; d. 2009] over in Thomas, and they cooperated on tackling that problem—that extension of very local behavior surrounding the crack tip, but when large deformations are present. They published that work, and Ravi was familiar with that. So, he combined that analysis—incorporated it and married it to his numerical scheme.

ASPATURIAN: Ah, very clever.

KNAUSS: Because numerical, finite element methods cannot deal with a local, very, very small domain. So, he put that continuum analysis in and tied it to his numerical system. And they were impressed with that, they said. There may have been a little bit of a "me too" story there, but Jim Knowles brought this up in a meeting.

ASPATURIAN: So, this was before Ravi came here, or after?

KNAUSS: I'm sorry, I went a little bit too fast; you're not familiar. He was here for a good year.

ASPATURIAN: As a postdoc.

KNAUSS: As a postdoc, and then became an assistant professor at the University of California, San Diego, and he was there for a number of years; and then when a position [in solid mechanics] became available, we thought of Ravi, amongst others.

ASPATURIAN: On the basis of the work he had done.

KNAUSS: Well, simply because he was very good. And then we had numerous meetings about whom to hire; Jim Knowles had never brought Ravi's research up, but I think it worked on him. When he made this point at what turned out to be the final meeting—we had a number of people who had visited and were under consideration—that concluded the meeting.

ASPATURIAN: So, Ravi had done this work [as a postdoc] before he was hired here.

KNAUSS: Yes.

ASPATURIAN: It was a factor in the decision to hire him.

KNAUSS: That's right, yes. He was known here, and there was the fact that he had this close working relation with Caltech colleagues. Ravi has a very nice personality. He doesn't offend anybody. He doesn't get offended. He sees both sides or all three sides. That's what makes him so effective also, I think, as division chair.

ASPATURIAN: Let me see, Rosakis succeeded [Hans G.] Hornung as head of GALCIT.

KNAUSS: Yes.

ASPATURIAN: This was the first time a solid mechanics guy became GALCIT head; how did that come about?

KNAUSS: I was not part of that deliberation. Nobody asked me.

ASPATURIAN: Really.

KNAUSS: Yes. I believe—it's only a belief—that there was really no personality available in the fluid mechanics program who would have fitted into that. And Brad Sturtevant was gone. So, it came down to the division—let's see who was the chairman?

ASPATURIAN: John Seinfeld [Nohl Professor and professor of chemical engineering], was he still chair at that time? No, no.

KNAUSS: After. What's his—

ASPATURIAN: Murray?

KNAUSS: Murray, yes, I think it was Richard Murray.

ASPATURIAN: Richard Murray [Everhart Professor of Control & Dynamical Systems and Bioengineering].

KNAUSS: He was also instrumental in seeing through the [named] chair appointment for me because he came and said, "Don't worry about Hans; it'll go." Anyway, I remember it was about that time, and I think he had had interactions with Ares in some form or another. Whenever Ares presents something, he speaks with authority. I always think he was born with a silver spoon in his mouth, not for his talking, but he grew up in a family that paid much attention to proper behavior.

ASPATURIAN: I see.

KNAUSS: And considering other people and how you play that game. So, I think that when Ares talks with somebody, it leaves a good impression.

ASPATURIAN: So, the thinking was that he was the right person for this job.

KNAUSS: And then Ravi became his assistant, so to speak, so there was already that bond, and frankly I was a little bit surprised because Ravi never pushes himself. But I'm very pleased with how well it worked out.

ASPATURIAN: I was going to say, how did you feel about this? Your two personal recruits taking over the future of GALCIT.

KNAUSS: Well, before that, I think I had two secretaries who became secretaries for the division chairs, so I'm not so bad at picking good people.

ASPATURIAN: Apparently not, although you lost your assistants to the division, it sounds like.

KNAUSS: Yes, but it's for their good.

ASPATURIAN: That's true, that's true.

KNAUSS: I brag a little bit since we're on this topic of seeing what people can do. I was on the undergraduate admissions committee for a number of years, and then I was off for a while and then I got back on again. And I remember that one time—there's always a fight over students—

ASPATURIAN: So, I've heard.

KNAUSS: Because the interviewers at that time were still traveling, going to various places, although I was confined, because the travel occurred rather late in the admission process and there were not enough professors who wanted to be available. I worked in the Pasadena area, so I went to all the schools where they had applicants, and you know how the interviewers develop a personal feel for their interviewees, and they think they're the greatest and there's this personal side. I simply separate myself and let the facts speak. Somebody told me later that in all the history of the admissions committee, I had the highest number of admittees. I didn't fight very much; I simply stated their qualifications; so maybe I do have a little bit of a feeling about what people can do. Anyway, those are some of the things that happened.

ASPATURIAN: How do you think the shift in leadership affected the character of GALCIT?

KNAUSS: Ares did one thing that in retrospect I feel did a lot for GALCIT, and I say, "in retrospect," because a number of years ago—and this was about the time when Max had left—there was a discussion about whether GALCIT should be called aeronautics and space—

ASPATURIAN: Right, you talk about this in your interview with Trity. [*See Appendix C*].

KNAUSS: So, there was this issue at the time that we rename GALCIT, and we had a special effort in that direction. Ares wasn't part of it yet, so he didn't know; his effort was all new.

ASPATURIAN: The first one was before him.

KNAUSS: That's right. He established it; he brought in a faculty member, Sergio Pellegrino [Kresa Professor of Aeronautics and professor of civil engineering], who was devoted to space related things—

ASPATURIAN: His wife, Mariella [Soprano], works in the Archives with me.

KNAUSS: Okay. Oh, she works with you?

ASPATURIAN: She's also in the Archives, yes. We're in the same program.

KNAUSS: That brought in a lot of new students. There was a lot of interest in that.

ASPATURIAN: I can imagine, sure.

KNAUSS: And so, I think that's something that was very good for aeronautics, for GALCIT. I think that's because fluid dynamics now requires different fields than just airplanes. The aeronautics aspect is rather mature. I don't think you find in solid mechanics anyone worrying about airplanes, primarily. It's a sideline. But it's basically a solid mechanics issue.

ASPATURIAN: Yes.

KNAUSS: So, in that sense there's a lot of cooperation with the Thomas people, for example. Ravi and Kaushik Bhattacharya [Tyson, Sr., Professor of Mechanics and Materials Science; Caltech vice provost, 2016–], they're like hand in glove.

ASPATURIAN: They work together a lot.

KNAUSS: That's what Chuck Babcock started with Paul Jennings. They reconfigured courses that ought to be taught for both aero and civil as well as mechanics students. I think that's been very good for the division.

ASPATURIAN: So Rosakis introduced elements that led to more interdisciplinary collaborations as well as the focus on aerospace.

KNAUSS: Yes. I don't know that I'm aware of anything of the same sort that Ravi had done while he was director. But he basically carried the office through from Ares, I thought very effectively. I think what is good about Ravi is, as I said before, he has a very good understanding of people. He's sensitive, and he doesn't easily blow up.

ASPATURIAN: And you think this was a factor in his being named division chair?

KNAUSS: Could very well be. Maybe Rosakis recognized this also and suggested him—

ASPATURIAN: As his successor.

KNAUSS: That's right. I also remember another division chair who had a lot of his own ideas that he wanted to push, and with the—let's see, who was our provost before the current one.

ASPATURIAN: Ed [Edward M.] Stolper [Leonhard Professor of Geology; Caltech provost, 2007-2017].

KNAUSS: Ed Stolper. Early in Ed Stolper's position he told the division chair—

ASPATURIAN: Oh, that would have been David Rutledge [Tomiyasu Professor of Engineering, Emeritus], I think.

KNAUSS: I didn't want to name names. [Laughter] Yes. David had some very good ideas, but they didn't necessarily go over very well with the provost, because they maybe would have curtailed the provost's prerogatives too much. So, we have people who have very strong opinions, and sometimes that's good and sometimes it isn't, and this is how the world runs.

ASPATURIAN: That's true. Paul Jennings was division chair for quite a while.

KNAUSS: Yes.

ASPATURIAN: What did you think of his leadership?

KNAUSS: I owe Paul Jennings a great lot.

ASPATURIAN: Everybody says that.

KNAUSS: Is that right? I'm glad to hear that. But I'll tell you, in my case, being a graduate student here, it was largely a financial salvation; and for Chuck Babcock also, because he went for one or two years to the National Science Foundation, looking for a job elsewhere, really.

ASPATURIAN: Because of the difficulties you had as solid mechanics people.

KNAUSS: That's right, and the financial rewards here were very low. And I already may have mentioned this—if you get on the payroll as basically a postdoc, you get a little bit of improvement as an assistant professor, and then it's percentages.

ASPATURIAN: Your baseline—

KNAUSS: You start on a low basis, and so we had financial difficulties, and when Paul Jennings got into the office, he looked at that, and I understand his reaction was something to the effect that it can't be true. [*See also Session Two*] And so, I saw in two subsequent years, very significant improvements financially. That made me decide I would stay. Because for me, it was a matter of—I was looking for positions. I had several offers; I had one from Germany, a very respected technical institute in Darmstadt [Technical University of Darmstadt]. I also went for interviews in Munich and in Karlsruhe—I had a good friend on the faculty at Karlsruhe. Ultimately, I had a firm offer from Darmstadt, but I did not want to go to Darmstadt. First of all, it was the state of Hesse, which does not conform too well with my more conservative viewpoint of life. And at that time, there was the so-called tripartition of university's staff. That is, the students counted as one third; the employees, including janitors, counted as one third; and the rest was the faculty. When things needed to be decided academically, they all spoke their piece. I just could not think how a janitor could talk about academically important topics.

And when I went there for a visit, the walls were filthy with graffiti. What ultimately did it: I speak German, and so I presume they assumed that when I got contractual material, I understood this fully. But the contractual language goes beyond what I learned up to age eighteen, nineteen. And they simply wouldn't understand that, and when I asked for clarifications, I never got an answer. So, I thought, "I cannot work with these administrators—and I really would be responsible to them, reporting to the ministry in the state capital, Mainz. I cannot go there." So, I terminated that. That was forty-two years ago.

ASPATURIAN: So,'78?

KNAUSS: Yes, something like that. Because we bought our house where we live now after that. We had put off buying a house because I was negotiating with Darmstadt. And I have never regretted the decision not to go there. They accused me at Darmstadt of just going there and talking to increase my stature here.

ASPATURIAN: Well, even if that was true, people do it all the time.

KNAUSS: And I simply had to write back that I did not benefit a single penny from this. Whether they believed me or not is another matter. I generally liked the people; the staff, the colleagues were very nice, probably competent, and I communicated with one for a number of years—but it simply was not the environment that you had here. And I've always felt better that I stayed here.

ASPATURIAN: It seems that things did work out for you here as well. The situation improved.

KNAUSS: Well, there was later another offer from the University of Houston, where one of my former students, Ravi-Chandar, was a leading professor.

ASPATURIAN: He's in Texas now, right?

KNAUSS: University of Texas at Austin, yes. And he would have liked me to come down there. My wife and I went, we interviewed, and I talked with our chairman here, and he would not see any reason to change my salary. That was before Paul Jennings.

ASPATURIAN: Who was the chairman at that time?

KNAUSS: That was Roy Gould [Ramo Professor of Engineering, Emeritus]. So, I swallowed hard, but I said maybe there are other ways. Then I found outside consulting work, which saved the situation. Sometimes I thought it was a poor administrative decision, because being made to look for more income rather than devoting yourself 100 percent to the efforts of the university is a drawback. I got all my honors basically afterward, so I don't think it hurt me terribly much, but that is ultimately what helped me over the financial deficiency. When I looked at what other professors made—you get statements of what the average salary is; you divide by the number of faculty; and you see. I didn't get anywhere near the average, and you know there is something wrong.

ASPATURIAN: Of course. Of course.

KNAUSS: I had seen some other faculty members who had never achieved what I would consider international stature, but they were very important on the campus, and I could feel that they were being treated in a second-class way, also. There are a number of these in the engineering division. I've now lost track whether there are similar situations today.

ASPATURIAN: Well, it sounds like to some extent that you were a victim of the internal politics at GALCIT.

KNAUSS: Yes, actually division wide. A chairman has to think of what the capability of a faculty member is, and how it represents to the outside of the institution. That has to be honored and acknowledged in some way, and the financial aspects are part of it. I understood all this. I often felt I have always worked in an area that was separate from what was going on here traditionally. Why should I duplicate at Caltech an area that is well covered?

ASPATURIAN: That's true.

KNAUSS: But this leads to problems. You get into areas that are not known by others. The need for the area is not understood. They don't delve into that, and so you suffer in a way if you have new ideas that are out of line with what is going on.

ASPATURIAN: Which you experienced with the biomechanics, for example.

KNAUSS: Yes, right, except that was not under the influence from any Caltech colleague. I'll give you one example. We had a very capable computational man, Tom [Thomas J.] Hughes. Tom came from Berkeley; he was a solid mechanics computational specialist; what Michael Ortiz [Marble Professor of Aeronautics and Mechanical Engineering] did in a sense. Large computer codes. And as a solid mechanics faculty he developed an interest in dealing with fluid mechanics issues. So, he worked for a number of years numerically on fluid mechanics problems.

ASPATURIAN: This was here?

KNAUSS: Here. He was over in Thomas [Gates-Thomas Laboratory of Mechanical and Civil Engineering]. He made the "mistake" that he did not "seek the permission" or the consultation of a fluid mechanician over here. It came to—I forgot in what context, but I think it was a full professorial appointment—

ASPATURIAN: He had tenure already?

KNAUSS: I don't think he had tenure when he came, if this was in connection with a full professorial appointment. He got tenure.

ASPATURIAN: Okay, he was tenured.

KNAUSS: But it left a mark on him apparently. I don't know how one finds out about "secret" promotion processes, but you usually find out when there are issues; I understand that very well. And he basically said, "I don't have to take this." He went to Stanford. So, there are those issues where you have to toe the line in a certain way with the existing hierarchy. And that's something I have never done. I felt no obligation to go and talk with Hans Liepmann, especially; we talked once about how I believe that he loved to crack jokes about people, and he wouldn't mind making a joke about me to my face. If I retaliated in a certain way, that was forbidden.

ASPATURIAN: Not appreciated.

KNAUSS: If you're a colleague, you're a colleague. If you're not a colleague, don't act like it. So anyway, those are some of the issues that occur, I believe, normally in almost any institution, and GALCIT wasn't free of that.

ASPATURIAN: Obviously not.

KNAUSS: But outside of that, toward the rest of the world, GALCIT looked very strong. GALCIT did have one advantage that I did not experience personally, but which was told to me basically by Max Williams and Bert Fung—that when there was a division-wide issue, it would be discussed in a GALCIT faculty meeting, and a decision or a course of action would be agreed upon, and then Clark Millikan would go with that agreement and present it at a division meeting as "Well, GALCIT says." And here are all these other individual professors who had no special clout as a group; so that had a lot of sway. This was not really liked universally. So, whenever they could get around GALCIT, they would try to do that.

ASPATURIAN: There was some maneuvering.

KNAUSS: It's understandable, it's a natural way of people living.

ASPATURIAN: Is GALCIT the one major programmatic area within the division?

KNAUSS: It was. Right now, I'm not sure anymore since I've been retired officially since 2004. That's sixteen, seventeen years. If you sit in your office and don't—are not invited to—participate, you really don't know what goes on.

ASPATURIAN: You kind of miss out on some of this stuff.

KNAUSS: Not that I really miss that, but most of my feeling about GALCIT was from the time I was still active. I was still active in research for about ten years after I retired, and then I thought, "That's enough." So that's the way life is.

ASPATURIAN: There was a funny comment that caught my eye in the Athenaeum tribute [*Appendix B*]: "Wolfgang only gets mad at you when you tell him you are going to marry his secretary." Is this based on an actual incident?

KNAUSS: [Laughter] I don't recall getting mad, but I remember coming home from Europe at the end of a summer. I had a graduate student—very capable guy, Giancarlo Losi, Italian. I wanted to get him a position in the United States, couldn't get it, took him on as a research fellow for a year, and then went off for the summer. When I came back, I was confronted with the fact that he was going to marry my secretary.

ASPATURIAN: Who was—?

KNAUSS: Theresa Thalken

ASPATURIAN: So, you had no idea they were having a relationship.

KNAUSS: No, and I was surprised. Giancarlo did not seem to be the type. And she is from out here, Arcadia. And she has an Italian background. As a matter of fact, I have at home an email from Giancarlo that I have to answer, because I haven't communicated with him in about two, three years; I want to see how they're doing. That was the comment [in the Athenaeum tribute]; I was surprised by that. Apparently, I was a bit upset because getting a good secretary is not easy.

ASPATURIAN: Not necessarily. I have a couple more questions.

KNAUSS: Yes.

ASPATURIAN: The write up that you did on your *Challenger* encounter with Richard Feynman [*Appendix E*] was very interesting and very thorough. I was particularly interested in your statement at the end about General [Donald J.] Kutyna and his account of how Feynman found out about the O-ring issue. From what I've read elsewhere, and I think this is what you're referring to, Kutyna was apparently under the impression that he clued Feynman in [on the O-ring connection] after hearing about it surreptitiously from Sally Ride. [Sally Ride, America's first female astronaut, served on the *Challenger* Commission, along with Kutyna and Feynman. –*Ed.*] And from what you were saying in your piece, that's not what happened at all.

KNAUSS: I did not hear it from there.

ASPATURIAN: No.

KNAUSS: These were discussions that were in the open here.

ASPATURIAN: You went and spoke to Feynman on the phone very early in the process.

KNAUSS: Just when the committees started. So, he would have known about that issue, but obviously he didn't want to refer to this "junior" faculty member here, and the fact that he never mentioned me, I find that really peculiar.

ASPATURIAN: I wondered if he just kept it to himself and just listened to what Kutyna had to say, and didn't bother—

KNAUSS: That's right, yes. I make no bones about the fact that I have a certain distain for physicists. Maybe that's too strong a word. Because they are so hierarchical. They're at the top of the heap, especially at Caltech. Now all our provosts and presidents until Tom [Thomas E.] Everhart [Caltech president, 1987-1997].

ASPATURIAN: Well, Paul Jennings was not a physicist.

KNAUSS: Well, provost, but I think Paul was the first engineer.

ASPATURIAN: And until Tom Everhart, the presidents were all physicists.

KNAUSS: Tom Everhart, yes, was the first engineer. Right. It's always physics. There's another little story that illustrates it. I get the *Physics Today* magazine and have for many years. In the back where they list new developments, there was a news article about a physicist who had finally figured out why a shower curtain always wants to cling to you.

ASPATURIAN: This is a physicist; it took a physicist to figure this out?

KNAUSS: A physicist, right. I thought, "You must be nuts." That is something that any fluid mechanician knows. I mentioned this to Hans Hornung once as a joke, and he said, "Sure, that's just the flow over the shower curtain"—

ASPATURIAN: It's the word "physics"—

KNAUSS: For them, this was totally new, a revelation. They never ask what engineers know. I have another situation like this with—I forgot his name now—a professor of physics at Austin, Texas, who was interested in dynamic fracture; and early in the game he wanted to come and visit me and talk to me about dynamic fracture. This was after Ravi had gone to Houston. I don't think they had at that time any relations, but I may have mentioned to him that my student was there.

ASPATURIAN: This is Ravi-Chandar?

KNAUSS: Ravi-Chandar. He then basically totally ignored everything I said in our discussions. I never saw it mentioned in any of his publications. I may be wrong; I admit that, because I may not have read everything that he wrote, but I never saw a reference to our engineering work. It simply wasn't "clean" enough. They wanted to look at the molecules. And therefore, anything that describes the physics at a macroscale isn't acceptable. So as an engineer, you don't count. And our work was published in a different journal; *Fracture Mechanics Journal* is not a physics journal. [*WK subsequently added:* There is a more recent example of how little attention physicists pay to engineers: A year after I retired, an article by a physicist appeared in a European physics journal describing a model of crack propagation in viscoelastic materials that was basically the same as what I had published thirty-two years earlier. I commented on that in a review paper, and the author(s) of the "new" article did not become aware of this review until five years later. At a time when a few words typed into a computer can generate virtually any publication on any topic, this "lack of awareness" is just astounding.]

ASPATURIAN: Do you think this has changed a bit?

KNAUSS: A bit, yes. But I still see a lot of the same. Today there is more interaction generally between disciplines, and so as part of that, I think things have happened at Caltech. I forgot now which one, but one of our physicists tied up with the biology people, because there are many biology problems at the molecular level that are physics.

ASPATURIAN: Yes.

KNAUSS: So, there are these cross feeds, and gradually people hear about them, and then they start working together.

ASPATURIAN: Which is greatly to the benefit of science in general.

KNAUSS: And that's the advantage of a small institution like Caltech, where this is very easy. I could go over to anybody in another division and say, "Hey, listen, I got this problem," and either you get help or you find— "Oh, yeah, maybe I ought to bring up one of these things." When I was done with my PhD on time dependence in fracture, I wondered, "Where else is that important?" I thought, "Well, earthquakes are really time-dependent problems," so I started to go to the earthquake physics seminars. They were still in—what is the earthquake center in the hills across the arroyo?

ASPATURIAN: The San Rafael seismology lab—that doesn't exist anymore.

KNAUSS: Yes. They had weekly seminars, and I simply went for about two years to the seminars, and I could never get much traction with my questions. I had—and this becomes a little bit dicey again—I had one student from geophysics who took a class from me; the course was in fracture mechanics.

ASPATURIAN: This was a graduate student?

KNAUSS: A graduate student. And at the end of the class—this is after nine weeks—he said, "How does this relate to this model in geophysics?" I just fell into myself; "Have I been that bad a teacher? I have taught *anything but that,* because I had previously written to Frank Press, who was then a very well-known geophysicist at Caltech. I had written to him once about what geophysicists call a dislocation model, which are simply dislocations, disarrangements, in a crystalline atomic structure. And they put these together and for them, this was the replacement of a crack—but it's anything but. It has the wrong singularity. It has the wrong energetics. I wrote to Press, and I said, "You should look at fracture mechanics. This is wrong for this and that reason.” I never got an answer. Then this student asked, “What about the dislocation model?” and I was crestfallen.

ASPATURIAN: How did you answer him?

KNAUSS: I think I simply gave up. I said we talked about cracks, and this basically was the end for me; it terminated my interest in geophysics. Ares Rosakis picked it up later, but for maybe different reasons. I didn’t find that a fruitful pursuit. There were other fracture mechanicists: Jim Rice, who was internationally well-known, picked it up and maybe had different clout and different people who were more careful to listen. I didn’t get anybody over here to listen except this graduate student.

ASPATURIAN: So, you win some, you lose some. I have one more question, but I thought I’d ask if there’s anything that you wanted to put into the record first.

KNAUSS: Go ahead.

ASPATURIAN: This takes us way back. You were raised under the Nazis in a totalitarian state. You experienced war, dislocation. Then it all ended, but I wondered: What impact do you think that formative period has had on your life in general? Which has been a very successful life.

KNAUSS: What comes to my mind—not that as a teenager I would think in those terms—but it is survivability. The human being can tolerate a lot of terrible things. And even to an extent forget about the terrible times; you make a new life. But it also taught me, especially when I came to the United States, that if you have not experienced behavior like this—an autocratic system—you cannot discuss how an autocratic system should behave or does behave. For example, when it came to Iraq, Americans simply did not understand that you cannot judge members of the Iraqi population by the same rules that we have in our country. And that’s so obvious when you think about it, but it simply does not occur to people who have not experienced it, and there’s nobody there to teach it. This is true in politics to a large degree.

The other thing it taught me was, never throw anything away. I suffer from that my whole life because everywhere I look, I've saved things and I finally had to draw the line on paperwork, so that anything I haven't looked at in two or three years goes into the trash; otherwise, I couldn't have saved things. But that is something where I still have my workshop at home full of things I don't think I'll ever use; yet— I might need it.

ASPATURIAN: I see. Because— you never know.

KNAUSS: It dates back to the time when you wanted to do things, and you couldn't do them. That was a big impact on me. As a result, I learned to do things, make things myself as a youngster, and that's how I became an experimentalist, because I build things that other people didn't build. I remember one thing—I'm of the same kind—Chuck Babcock always looked for a student who was a car mechanic in his teenage years because he says those guys know how to do experiments. So, I don't know if that answers your question sufficiently, but I think that it also tells you something about the longer life of history. You cannot force things into a short timeframe. Many things resolve themselves and oftentimes to the better. But not always. I don't think I have anything else I can think of. If something comes up during the time when we go over it—

ASPATURIAN: Sure.

KNAUSS: Then we can talk about it. Okay?

Wolfgang Knauss gathers with colleagues, former grad students and postdocs during a Caltech symposium and celebration held to mark his 80th birthday in 2013.

[*WK subsequently added:* In these interviews I have spoken mainly about those students who participated in a major way in my own, more or less directly recognized work. But they are only a small part of the total group on whose capabilities I relied.

I have always thought very highly of my students, and I am deeply indebted to them for the excellent work they have performed: I could not have achieved what has been recorded here without their help. It may be of interest that we—my wife Lydia and I—have continually expressed our regards to my (our) students by providing them with a sense of personal connection by way of parties at our home. These events came in two forms: the PhD parties and the more departmental parties that started out with members of the solid mechanics group in GALCIT, then expanded to include some GALCIT fluid mechanics members, and finally also the solid mechanics members from the Thomas building (Gates-Thomas Laboratory)].

Every PhD graduate experienced a dedicated dinner party to which all my students and some professors were invited, with attention paid specifically to the new graduate. These parties usually ended with a song, written by me for the graduate following a popular melody. For example, the first one was for Hans Karl Müller, which followed, "It's a small, small world…".

At later parties, songs followed maybe the same melody or others, but were always geared to the specific experiences with that particular graduate. The texts were not poetic marvels, but usually produced considerable mirth. Starting in the late 1980s, our special PhD recognition dinners decreased because of an increased workload for me.

During a sabbatical at the *Technische Hochschule Karlsruhe* [1972-73] with Professor Hans Rumpf, we found this habit also, in that Hans Rumpf emphasized, that life becomes more serious after a student leaves the academic atmosphere and that a joyous festival is a good sendoff. His parties were always at the campus, as his home could not have managed his large group. He later became *Rektor* [president] of the *Hochschule*, and in that role, later the president of the *Rektoren Konferenz* [Association of the German University Presidents].

But, apart from these special, smaller gatherings, we annually convened what came to be known as the "pool parties" to welcome the new solid mechanics students in early October. Often some of the fluid mechanics staff participated, in addition to all solid mechanics staff. The staff typically brought along their young offspring. Later these events grew in size to around 60 or 70 by including also all solid mechanics students and postdocs from the Thomas Lab. For these affairs food was arranged by potluck, except that Lydia usually provided several additional cakes and pies. Entertainment was mostly in the form of badminton, croquet and of course swimming and freewheeling water polo. During and following desert, the time was typically ripe for clever skits by the students, usually satirizing mostly the hosting professor, or a colleague, but sometimes also a postdoc.

1999 pool party followed by skits at the Knauss home.

These large parties continued for about fifteen years, and we felt they contributed much to the happiness of the student body. They ended only after the extra work in preparation and cleanup became too much of an additional load on Lydia and me. The final party at our house was, therefore, planned and arranged by the postdocs and senior students. These parties were always a lot of fun for all participants.

It seems proper in this context that all those who contributed to my "successes" be recognized in this history. [*See Appendix F*] By listing also the titles of their research and areas of contribution an enlarged view of the breadth of topics studied in my group is presented.]

APPENDIX A

My Musical Life

Wolfgang Knauss at the harpsichord he built by hand in the 1970s.

My connection to music started early in my life close to the beginning of WWII in the little town of Siegen, about 60 miles east of Cologne and at the southern tip of what is today North Rhine-Westphalia. My best friend then, Hans Becker, who had lost his piano-playing father early, was encouraged by his mother in piano lessons. My own parents provided piano lessons for my two older brothers, Erhard and Tim, saying, for whatever reason, something to the effect that the piano and I were not really compatible. Nevertheless, I was sufficiently intrigued by what went on around me and heard what my brothers were learning. I tried to follow their piano lessons and figured out the notes and fingering from their sheet music. They rarely helped me directly, except to teach me the difference between the treble and base clefs.

My mother occasionally played simple songs from the hymnal on the piano, and I still remember a particularly simple one in G-major ("*Was mein Herz erfreut, ist das sel'ge Heut*"), which I also tried. However, one key never sounded right, whereupon she came from the kitchen to the piano and pointed out that the sharp notation on the meant that this note was to be played by the raised black key immediately to the right.

That was my basic education in music notation and logically the other keys followed from this one step, including the flats, since there must be a lowering where there was a raising, and trial and error confirmed the logic.

When my friend's mother died near the end of the war, he came to live with his childless aunt and uncle, also next door, and piano proficiency became preeminent in his life: it gave him assurance since there was no comparable prodigy in the neighborhood and earned him my full admiration. He often passed on to me what his teacher told him, but he seldom tried to teach me anything, though he was quite willing to demonstrate his proficiency. Nevertheless, the daily exposure to admirable piano playing by my best friend left its mark on me, and I persisted in playing our piano, which, during subsequent bombings in 1945, fortunately suffered only scrapes in the lacquer finish when a bomb ripped away half of the church building in which we lived.

In spite of this demonstration of persistence, my parents could not offer me piano lessons; my brothers did not continue with lessons after the bombing and subsequent months of rebuilding the church building. I presume the financial hardship of the post-war era was the major reason, and as I became aware with growing up, life was always very basic in our family on a Methodist minister's salary. I persisted in playing the piano autodidactically for about five or six years with the help of my brothers' former lesson books, which contained pieces that I later recognized as coming from Bach's music for his children, with other pieces from symphonies and operas of the famous.

Five years after the end of WWII (1950) my father was transferred to Heidelberg. where my oldest brother could attend the university. While my father's office/study in Siegen was separated from the piano by several rooms, the piano was now just on the other side of the office wall in Heidelberg, separated only by an (unused) door. This gave rise to some friction, especially when he needed to prepare his sermons, and precipitated very stern and loud admonitions to stop; but on the whole he was very understanding and accommodating to my daily practice.

Heidelberg had two great advantages and support for and because of my piano endeavors: In our building lived two daughters of a former Methodist minister who had been widely known for his musical prowess (Song-father Gebhardt), and the younger of these was a piano teacher. She heard my persistent attempts and offered me free lessons for the remainder of the time she was still in Heidelberg: Her husband had emigrated to the U.S.A., and she was waiting to join him at Oberlin in Ohio, and visas did not fall from the sky in those days. She was an excellent performer, loved Chopin (I thought her rendition of his "Le Papillon" was marvelous), and weekly took me under her wings. Thus, I received my only formal and very welcome professional instruction for about nine months.

She introduced me first to Hanon and I did quite well with Bach's Two-Part Inventions, Handel's *Chaconne con Variationes*—which I learned by heart—Mozart's Sonata in A Major, and of course a number of Chopin's waltzes (I never quite played the *Minute Waltz* in one minute, just got close without muddling it) and several Schubert Impromptus. In retrospect it was quite a repertoire she asked of me in a relatively short time. She encouraged me early to play by heart, although, regrettably, my memory is not photographic, something she seemed to value. Unfortunately, she did get her visa after she had taught me for nearly a year. Later, I was asked to perform the Mozart sonata in high school, though I am not quite sure it was overwhelming, since I do remember my very cold fingers on a wet winter morning and barely making it to school without gloves (the music class started at eight AM). I loved that short period in my life,

The second advantage of being in Heidelberg—as regards music— followed, perhaps, mostly from the first: During this time, we received a visit from a distant relative of my father's and acquaintance of my mother's, who lived in Heilbronn, about an hour by car up the Neckar River from Heidelberg. My parents came from a village and a farm close to there, and they all had known each other since their youth in the Methodist congregation in the neighboring village. Well, one of the five-head-family was a girl by the name of Lydia, whom I seemed to have impressed with my piano playing (not her words), the long upshot of which was that we celebrated our 50th wedding anniversary in 2008 in the same Methodist church where both of Lydia's and my parents were also married.

Another musical experience may be of interest here: The Methodist congregation in Heidelberg had a choir director, who also loved to have his wife sing solos during services. At some point they terminated their association with the congregation, which was now without a choir leader. Perhaps my father was desperate; however, he asked me whether I would be interested in taking directing lessons and take over conducting the choir. This I loved to do.

I was about sixteen at the time, and subsequently was admitted to the Churchmusical Institute (*Kirchenmusikalisches Institute*) of Heidelberg University. The teacher was Professor Poppen, a well-known Heidelberg musician who directed many of the public, sacred performances. The weekly lessons were interesting in themselves and just a bit short of well organized: Professor Poppen would select (in class) a piece of sheet music from a volume of choral music, which he handed to a student, who then had to get up in front of the class to direct it. The class made up the choir. So much for preparation for either, but I learned the principles of control, including pick-up (*Einsatz*) and off-beat (*Abschlag*) for voices in polyphonic sets.

The importance of strictly keeping measure of a choir was brought home to me by Professor Poppen in a painful but effective way: I had heard the *Don-Kossacs* perform in Heidelberg and was thoroughly impressed; especially the emotional leadership of their director amazed me. In my next directing lesson, I tried to emulate him, and after the "choir" did not follow, it was soon clear to Professor Poppen that he needed to "teach" me, which he did.

Since we did not have much of a radio, I was pretty much devoid of musical information, and not at all aware of what was required to enter a musical career. For a sixteen-year-old, it seemed to be exciting to become a symphony conductor. To find out about prerequisites, I dared to consult a well-known piano teacher living in Heidelberg, who received me while teaching a student at the piano. His view was that I needed to be proficient on at least two musical instruments—piano being obviously or preferentially one of these. I still remember a bit painfully his belittling tone of voice and the snicker by the somewhat mature woman at the piano. Because of my fiscal limitations, the prerequisites were out of the question, even if "age had already passed you [me] by."

A long musical hiatus ensued with the death of my father during my final high school exams in 1954, which resulted in my emigration to California to live with the wonderful Williams family, who did have a piano. However, the absence of familiar sheet music (shipping my sheet music from Germany was too expensive for my mother at that time), and studies at Caltech ruled my schedule. It was not until after Lydia joined me in the USA, and we were married and had our children that the musical drought abated somewhat, though not by way of the keyboard.

I had become interested in recorder playing and with Lydia and friends formed a small consort of five. I acquired quality recorders of the upper ranges and through gifts from Lydia's parents an f-base and a super-(c)-base. But I liked to play the alto recorder and was hoping to do so with period accompaniment. I thus asked the wife of a colleague, who owned a Sperrhake spinet, to record the keyboard portion of several of Handel's recorder sonatas, so that I could play the recorder with the accompaniment. However, she could never find the time, and after a year it became apparent that I would have to take matters into my own hands. I investigated the purchase of a harpsichord, but first learned through books and historical writings about the instrument. The Kasimoff piano store in Pasadena on North Lake Avenue at that time dealt in Sperrhake instruments. However, trying those sounded anything but period realistic and not worth the money. Thus, I investigated kits and found Frank Hubbard's offer and historical knowledge convincing and decided to purchase the kit for a French double after Taskin. I needed a kit because I lacked the tools to make some of the components, like the 1/8-inch-thick soundboard nearly 8 feet in length, nor would I relish making more than 120 identical keys to precision, and above all, it was very difficult to find sufficiently dry poplar wood from a local supplier.

"It took me nearly half a year to learn marketry (the art of inlay) during evenings in 1973-74. At that time there was no googling for information, and books were still the master-source."

The project took about two years (spread around 1975), primarily because I wanted a veneered instead of a painted finish, which meant I had to learn how to veneer, find the high-quality veneer in sufficient and matching quantity, plus learning marketry.

After a year or so of practice on the finished instrument, professional activity began to overshadow the restricted “leisure” time, and harpsichord practice virtually stopped, with only brief and intermittent attempts at resurrection until well into retirement when more time became available and learning the music by heart offered some desired maintenance of brain function. In 2003, three years before my retirement, I had the instrument re-voiced by a “professional”—which turned out to be less than professional, in my unprofessional opinion.

Starting with 2010, I concentrated on carefully re-voicing the instrument again and on reviving pieces from the two-voice inventions, studying the French Suite 2 in C-minor, going on to preludes from the *Welltempered Clavier*, and adding the *Partita* for Harpsichord, No. 1. In this effort the capability to play all pieces by heart was paramount. Needless to say, that with advancing age the “by heart” aspects required particular endurance.

Also, during this period, I reworked the action (Celcon and leather plectra). In addition, I spent much time analyzing the “abnormal” frequency content of select notes. In this connection it must be realized that a plucked string has a much richer overtone content than the hammer piano would produce. It appeared that these notes (e.g., G4 on the back 8) had unusually strong overtones so that I inquired of the Frank Hubbard successor what the reason might be. There was no answer to that.

I asked Curtis Berak, the harpsichord regulator whom all better-known musical organizations in the Los Angeles area call for repairs, voicing and tuning; and he came to our home. His message was essentially that I was too particular, and that I could try a heavier wire. To no avail. Thinking that the reason might be a missing overtone rather than an overly strong one, I measured the ratio of the plucking length (length of the string segment from the front support to the location where the plectrum plucks) to the string length. That did not turn out to be close enough to an integer to qualify as a reason.

I finally emailed Claudio di Veroli in England (he appeared to be of the thorough Frank Hubbard type) about that; he assured me that every instrument has these notes and that they result from the integer ratio of the plucking distance to the string length. I carefully re-measured the string ratio, and indeed that ratio was then very close to ¼ (2-3%). Also, analyzing the frequency content after recording the note confirmed that the fourth overtone was missing. I found that the other “odd” notes corresponded to the ratios 1:3 and 1:5, with the 1:6 ratio being not seriously objectionable since it was on the lower, heavier strings, and the higher overtones are typically not very strong compared to the fundamental.

The only correction to this acoustical dilemma is, in my opinion, to introduce structurally a new string end support, which changes the 1:n ratio for the appropriate string, enough to make the different sound less noticeable. This string support has to be very firm to accommodate the (vibration) force in the string, and I have not yet had the heart to irreversibly screw up the optical cleanliness/beauty of the instrument this way. Thus, I have followed di Veroli’s advice and accepted to live with it. (He built a Frank Hubbard kit, and his instrument has A4 as the “odd” note.)

A Note on the Speed of Playing

I love Johann Sebastian Bach. I am also in awe of Georg Friedrich Händel, Joseph Haydn, Wolfgang Amadeus Mozart, Ludwig van Beethoven, as well as the romanticists like Johannes Brahms, Franz Schubert, Felix Mendelsohn-Bartholdy, Frederic Chopin. To possess the ability to develop multileveled melodic interaction is, if not incomprehensible, the marvel of a genius. But Bach has such a clear melodic and logical development that is fairly readily recognizable yet amazing at the same time. Though I admire the manual dexterity of some harpsichordists (e.g., Ralph Kirkpatrick) I do not cherish the excessively fast rendition of Bach pieces because my mind cannot follow the interactive notes fast enough, when a cloud of notes results, which is more appropriate for a Ravel composition, rather than for the logic of a Bach.

At least that is how I rationalize my playing more slowly. Of course, I recognize that I am not a virtuoso, and besides that, I no longer have the dexterity to even try approaching a virtuoso performance. For this reason, I appreciate and love the slower but clear playing pace of a Helmut Walcha (blind organist in Frankfurt with an enormous Bach repertoire), which displays the clear melodic intimacy of interweaving multiple voices (*melodische Linienfűhrung*) without having to impress by speed, or even of a Gustav Leonhardt (died January 2012).

Besides, it is understood that these works were written for Bach's young children and for Anna Magdalena, for whom he also wrote the *Notenbüchlein*, many with simpler pieces, indicating that she was probably not the virtuoso paralleling current concerto pianists, though she flourished through her voice. This observation speaks for Bach having conceived of a slower tempo for these pieces.

Also, Bach was obviously heavily committed to the organ. Contemporary instruments of that time connected the keyboard mechanically to the valves that activated the pipes and, especially when multiple registers were involved simultaneously, the mechanisms did not react with the speed to which we have may have become accustomed in the post-romantic period ("quoting" Marie-Claire Alain). It would seem illogical that Bach completely divorced his thinking and feeling from the mechanically dictated capabilities of the organ when writing for stringed keyboards. His second son, Carl-Philipp-Emanual, writes—the only extant speed reference by his father—that he liked to "take his music cheerful (*frisch*)."

Appendix B

Celebrating 60 Years of Wolfgang

The Athenaeum, California Institute of Technology

February 1, 1994

All of you gathered here know that our friend and colleague, Wolfgang, recently turned sixty. What we are seeing in these two days' worth of seminars are a few of the technical aspects of his contributions to our lives. However, many of you may still be wondering where this man came from and how he got here. Wolfgang Gustav Knauss was born December 12, 1933, in Mandel, Germany. Wolfgang was the youngest of three children (sons) of Gustav and Maria Knauss. Shortly after his birth, Wolfgang's father, a Methodist minister, was transferred to Düsseldorf and three years later to Siegen, where Wolfgang spent his boyhood years.

The unsettling times of Hitler's regime, his parents' anti-Hitler convictions, the terror of the air raids and hardships of its destructions, interfered seriously with Wolfgang's ability to concentrate academically. Wolfgang's early school years were marked by a special affinity for drawing, painting, and classical music. The latter was a result of a close relationship with a boyhood friend who excelled at the piano. Wolfgang's parents misjudged both his interest and his ability in music at the time, and Wolfgang never received piano lessons as his two older brothers did. Even so, Wolfgang is the only one of the three who actively pursues the keyboard instrument still today. During the war, like many others, the family's home and the church where Wolfgang's father served were lost to bombs. Everything needed to be rebuilt from scratch. Although his older brothers claim the twelve-year-old Wolfgang was very clever at avoiding work, the present Knauss home in Altadena shows the unique combinations of artistic skill and manual labor which Wolfgang possesses.

When Wolfgang was sixteen, his father was transferred to Heidelberg. Although he soon met Lydia there, his academic achievements suddenly became important to him. As it turns out, his father had told him that if he wanted to have a girlfriend, his grades would have to improve. Needless to say, Wolfgang graduated at the top of his class! During these years the Knauss family became involved with American church groups who helped rebuild in Germany, and they also received packages with American clothes. Wolfgang liked how he looked in these clothes and pretty soon his classmates called him "Jimmy, the all-around-man." The latter part was probably because he also excelled in tennis and swimming. Wolfgang became fascinated with Americana.

To the name Jimmy, Wolfgang soon added the last name "Baker." Wolfgang and Lydia knew, that at the age of seventeen, their parents considered them much too young to date. Since they lived 60 miles apart and saw each other only a few times a year, their letter correspondence flourished but had to be kept secret. Wolfgang picked up his mail at the post office under the name Jimmy Baker. One of their rare meetings happened while Wolfgang was with his class at a school retreat. Between lunch and dinner, he raced with his bicycle on a 4-hour round trip to surprise Lydia. She was ecstatically happy to see him, but it never occurred to her that he might also want a kiss goodbye. Only later did she hear of him being disappointed about that. The highlight of their second year of dating was that they could arrange to be at a church camp together for two weeks. Since their love letters had accumulated and were hard to hide at home, they had brought them along to ceremoniously "hide" them in the middle of the lake.

In Heidelberg, Wolfgang's joy of music took a more practical turn. At age seventeen his father encouraged him to attend the Institute for Church Music in Heidelberg. He was then the choir director for three years at the Methodist Church in Heidelberg. In 1951 one of the American work teams visited Heidelberg and the Knauss family. The team was led by Rev. Dr. Frank Williams from the Holliston Avenue Methodist Church of Pasadena. Wolfgang was invited to spend a year with the Williams after graduating from high school. The goal was to study aeronautics in America so that when the German aeronautics industry picked up again, Wolfgang would have a professional advantage relative to other young Germans. As it turned out, things happened differently. Wolfgang's father died during Wolfgang's final oral high school examinations (*Abitur*), only three days before he was to take the SAT exam required for his Caltech application. Although he failed this first attempt, he was encouraged by the dean of students to learn more English and try again. Wolfgang entered Pasadena City College in 1954 with that goal in mind. Today, Wolfgang's meticulous attention to grammar and spelling shows that he never again intends to let English get the better of him.

One year later, Wolfgang transferred to Caltech. Old habits die hard, and Wolfgang remained active on the swim team, and in his junior year he was the Caltech Novice Tennis Champion. In addition to classwork Wolfgang also worked at the F. L. Mosely Co. in Pasadena. This was most likely the beginning of his (often tortuous) relationship with computers. However, Wolfgang obviously had other things on his mind besides school and work. In 1957, following a transatlantic telephone engagement, Lydia came to Pasadena. The Price family, also from the Holliston Avenue Methodist Church, had visited Lydia’s family in Germany and invited her to stay with them for a year to get reacquainted with her fiancé after a three-year separation. In 1958 after Wolfgang received his BS in engineering, the couple was married. None of their extensive families came for the wedding, expecting to celebrate after their permanent return to Germany. Little did they know that Caltech had snared Wolfgang and that he would remain here permanently!

Professor Frank Marble had his chance to take Wolfgang on as a graduate student in Jet Propulsion, but he lost, the “opportunity” when he decided to wait until the beginning of the fall term. So, instead of being hot and wet (fluids), luckily for us (?), Wolfgang “rocketed” into solid mechanics. Wolfgang took on a summer project with Professor Max L. Williams to “clean up” a former class project on the slump deformation of solid rocket grain. Wolfgang went on to earn his PhD under Max's guidance.

Wolfgang never lets the incoming graduate students think it was easier to get a PhD in the early sixties. As all graduate students can attest, Wolfgang's “apology” when assigning more work is “it’s all part of the learning process.” His work habits in those days were such that Lydia needed to go to campus at midnight and throw pebbles at the window, announcing that it was time to go home for the day. Even at home he was constantly working. After the wedding he purchased $100 dollars’ worth of walnut plywood and announced that he would be making the necessary furniture for the apartment. Lydia envisioned “orange crates” for furniture. However, after seeing the end result, she wholeheartedly agreed to let him make more. Today these, and many more homemade, heirloom quality furniture pieces, can be seen in the Knauss's house.

Wolfgang expected to finish his studies in 1962. However, he first had to discover that students and (even more often) professors tend to think that the work is done sooner than reality allows.

Shortly before Wolfgang's PhD was conferred, Friedrich, the first of Wolfgang and Lydia's two sons was born (1962). After Wolfgang's graduation, Lydia and Frieder went directly back to Germany to visit family while Wolfgang congratulated himself on a job well done by flying around the world before returning to Germany. Wolfgang had not seen his family for nine years. Even so, he did not stay long; he loved Caltech. Returning to Pasadena, Wolfgang worked his way through the ranks from research fellow to professor. His life continued at a fast pace both on campus and at home. In 1965 Wolfgang and Lydia's second son, Stefan, was born. With two sons and Wolfgang's various projects, the house started to get rather crowded. In 1975, showing his love for music and his skilled craftsmanship, Wolfgang completed a three-year project of building a two-manual harpsichord. To make room for this instrument, the family had to move into their present house. Although the house was dreadfully rundown at the time, today it is quite different.

Now, as a full professor in Aeronautics and Applied Mechanics and as the Aeronautics Faculty representative, Wolfgang has been busier than ever over the last few years. As one graduate student recently commented, "If Professor Knauss doesn't want to read something you have written," he says, "Let me take a look at this in my office. I'll get back to you tomorrow." As we have seen, long work hours are nothing new for Wolfgang and even to this day, he appears to thrive on them. In addition to supervising an array of graduate students and postdocs, Wolfgang also does consulting work for a variety of organizations across the country. There are many things, however, both in and associated with Wolfgang's life which have not changed. With regard to his temperament, Giancarlo Losi, a former graduate student, recently commented, "Wolfgang only gets mad at you when you tell him you are going to marry his secretary."

Equally important is Wolfgang's family life. One sure way to get Wolfgang to jump to attention is to tell him his wife is looking for him. Although the boys have their own careers (Friedrich is a computer software developer at Megatek in San Diego, and Stefan is an orthotist and prosthetist in Arcadia), they have not strayed far from home.

As he demonstrated as a youngster, Wolfgang is always teaching himself new things. This refers to everything from practicing piano or harpsichord to the cutting edge of his graduate student's research to his newfound “point-and-click” computer skills. As one graduate student recalled during a particularly trying time during his first year at Caltech, “Professor Knauss, above all, wants to see you keep trying . . . never give up.” Most likely we all have seen Wolfgang’s perseverance present in one or many aspects of our lives. Let us all look forward to many more years with the “all-around-man,” our friend, mentor and teacher, Wolfgang Gustav Knauss.

APPENDIX C

Reflections on GALCIT History

As told to Trity Pourbahrami, Director of Communications

Caltech Division of Engineering and Applied Sciences, 2012

[Edited for the Caltech Archives Oral History Project, 2021]

I came to GALCIT after Theodore von Kármán had left. He was here probably three times while I was a graduate student, so I met him only twice briefly. But I know basically all the professors who have been here since the middle 1950s. I received my bachelor's degree in 1958 with an option in aeronautics. My advisor was Ernie Sechler, who, for many years—as far as I can remember—was the option representative until he retired. And so, he advised and guided all the students in their first graduate year. At that time, besides Ernie Sechler, the structures faculty (today, the solid mechanics faculty) were Maj (Major) Klein [Arthur l. Klein], Bert [Yuan Cheng] Fung, and Maxwell Williams.

By this time Maj was at the Institute only on Thursdays, teaching Aero 109, which was a class in instrumentation design. I remember Maj sitting on one of these individual chairs in a classroom on the second west floor in Guggenheim in what's now a big conference room. As I recall, the class was basically a rather informal sequence of experiences and stories from Maj's varied career in building aircraft. These stories were mostly spun in connection with discussions with Air Force and Navy pilots who at that time were students here and who had a special arrangement with the Institute to get advanced degrees. Every year, we had five or six of them. These were people who flew airplanes, so there was always this discussion on current airplanes. And they would ask questions that we as poor seniors and first-year graduate students could not understand, but they were discussing things back and forth. The astronaut Frank Borman was one of these. Maj spent the rest of his working time as a full-time consultant for Douglas Aircraft.

Maj had been here from the beginning of the planning for the second 10-ft wind tunnel, for which he designed the balances. He was particularly astute at doing this. His "Kleinisms" [*See Appendix* D] give you a feel for what a character he was. I don't agree with him on the first one. It's true, plastics make good toothbrushes, but they're also very good for many other things if properly designed.

Maj Klein was a very entertaining lecturer. He had lots of stories out of the practical life about his experience at McDonald Douglas. My memory isn't so good that I retain any stories, but he would talk about how and why people devised certain designs that didn't work out; what people did wrong, and what you should not do. And it instilled in us kind of a feeling of "Hey you know, academics doesn't tell you everything." And I think that was an important asset.

At that time fluid mechanics dominated GALCIT—then the acronym for the Guggenheim Aeronautical Laboratories. After the Kármán and Firestone buildings were added to the aero complex [in 1961 and 1962, respectively], the name was morphed into Graduate Aeronautical Laboratories. I remember the lengthy, several-months-long discussion as to how to change "Guggenheim" into something else that preserved the worldwide-recognized GALCIT acronym. And then, according to Max Williams, Clark Millikan (the son of Robert Millikan) had the idea for the new name. I still remember Max, who by then was my graduate advisor, coming out of the meeting where all the faculty got together for a final decision and indicating that all were very happy with the choice.

During Ares Rosakis's directorship, the name was changed into Graduate Aerospace Laboratories. This is of interest because in the mid 1960's, when the US space effort was in its early prime, and many universities wanted to develop an academic, competitive advantage by renaming departments, there was also discussion of changing the laboratory's name at Caltech while preserving the GALCIT acronym, and "Graduate Aerospace Laboratories" was the top potential choice. I was part of that discussion as a freshman faculty member (1965); what settled the issue was a comment by Paco Lagerstrom, who was then still a member of the aero (fluids) group (after the mid-1960s he became part of the newly formed applied mathematics option). He opined that what identified a faculty was its work and capability, not a name: "This naming will change again in a few years or disappear without any lasting effect on the quality of the work or the group's reputation. And so, the name "Graduate Aeronautical Laboratories" persisted until it was changed under Ares Rosakis, when a distinct choice was made to add a space-oriented component to the "aero" curriculum.

The partition between solids and fluid mechanics was roughly maintained until about the turn of the millennium, primarily in proportion to the interest that Theodore von Kármán had in solid and fluid mechanics. Kármán in his younger years was also very heavily involved in solid mechanics, also known as structures; and Ernie Sechler was, I believe, his first PhD student here. From then on Ernie largely looked after the structures/solid mechanics interests, while Kármán was doing more the fluid mechanics, as far as I know. When von Kármán left after the war [World War II] to become more involved in AGARD [NATO Advisory Group for Aerospace Research and Development], Clark Millikan took over the GALCIT directorship. After Clark's death in 1966 and a brief executive directorship, first by Ernie Sechler and then Brad Sturtevant, Hans Liepmann, who had come from Switzerland in 1939 to work with von Kármán, became the new GALCIT director, followed in 1987 by Hans Hornung, who had come from the DFVLR [German Aerospace Center] in Göttingen, Germany.

Until Hornung stepped down from the directorship in 2004, the fluid mechanics perspective virtually dominated the GALCIT scene. Brad Sturtevant had a lot to do with Hans Hornung's appointment because they were both active in very high-speed flow, and he continued to have a lot of influence under Hans Hornung until his death. (For the record, the fluid mechanics people had developed considerable internal strife, and I think that kind of hurt them.)

This perspective changed with the appointment of Ares Rosakis—whom I had brought to Caltech in 1982—because his interests were decidedly in solid mechanics (dynamic fracture); and when he was appointed chair of the EAS Division at Caltech, his successor became Guruswamy (Ravi) Ravichandran, whom I had also brought to Caltech, in 1990. His interests are in the dynamic response of materials. And so solid mechanics sort of took over the directorship to make it more of a coordinated effort. I do not wish to imply that a further division resulted, but the viewpoint seems to have broadened. Ed [Edward] Zukoski, professor of jet propulsion, remarked several times to me that under Hans Liepmann's directorship, there was continuing pressure to disband the solid mechanics effort at GALCIT, since at that time Chuck Babcock—an Ernie Sechler student—and I were the only two (junior) faculty members in that group. However, I do not think that Hans Liepmann could ever carry out that process since he relied too heavily on Chuck Babcock for advice. Chuck was a very considered and personable individual, a quality Hans Liepmann needed.

Chuck later became Caltech vice provost and did his good share in keeping the then-provost Rochus (Robbie) Vogt on a balanced perspective. This was also when Paul Jennings served his first term as the EAS chair, and together he and Robbie did very well for this division, which had been "underrepresented" for some time under the physics-dominated administrations of the past. Chuck died unfortunately much too early, which I believe had something materially to do with the fact that this provost [Vogt] did not serve out the full term one would have normally expected.

When I came into GALCIT in 1957 as a senior, the solid mechanics professors were Ernie Sechler, Bert Fung, Max Williams, and Maj Klein. Maj, who was here for one day a week (Thursdays), didn't have his own office but used a table in Ernie's office. As a fulltime consultant to McDonnell Douglas, he was on the practical side of aircraft design. He knew about fluids and solid mechanics pretty well. Another instructor, Ira Bowen, taught a class in wind tunnel use, etc. I had only a brief personal interaction with him because Maj Klein had given a design problem—which dealt with the balance system for a wind tunnel—for the Aero 109 course I took from him in 1957, and Ira Bowen was the man to go to for help. He seemed to have been heavily involved in the tunnel operations and the maintenance of the wind tunnel balances.

So, for a student, it was an interesting time. Ernie Sechler dealt mostly with aircraft structures, in particular buckling type problems; Von Kármán had published a number of seminal papers on that topic. He also had another student by the name of Hsue-Shen Tsien [Qian Xuesen], who later became the father of the Chinese rocket and space program. There's a long history with his departure from Caltech and the United States during the McCarthy era. (Frank Marble was intimately familiar with his whole departure story, but I do not know whether he ever recorded that.) [Marble did speak at length about this in his oral history; see https://oralhistories.library.caltech.edu/138/1/Marble_OHO.pdf –*Ed.*] Tsien was heavily involved in jet propulsion, and he also dealt with some solid mechanics issues, not on a primary basis, but he became involved with some thermodynamic issues as I recall, related to dissipative materials, on which one of my classmates, Richard "Dick" Schapery, drew later.

So that's kind of an aside, but Ernie Sechler was heavily involved with the structural stability of monocoque structures (aircraft fuselages). Under bending forces, they're likely to buckle. And then when rockets came along in the late 1950s, he was heavily involved with NASA—I don't know to what extent with the DoD [Department of Defense]—concerning the stability problems of thin cylindrical shells (the outside skin of the rockets) when they were being compressed by the rocket thrust. Theory and experience just did not want to match very closely in those days—I won't bore you with the details of why theory didn't work out very well—and there was a considerable international effort under way to understand the underlying physics for the disagreement. Caltech solid mechanics played a very significant role in this, especially under Ernie Sechler's student, Chuck Babcock, who was a contemporary colleague of mine.

Ernie Sechler told me about an interesting experience that gave motivation and direction to this more modern effort: As part of the rocket development program, there were repeated launches for test and demonstration purposes. In one of these important tests involving a very large booster, a malfunction occurred in which only two of the four adjacent rockets fired, so that very asymmetric acceleration forces resulted, which introduced a large bending moment on the rocket shell. Under these conditions the shell should have buckled, according to all criteria used for analysis and design purposes at that time. Yet, telemetry imaging revealed no shell crinkling or buckling, so that it became clear that the existing criteria were not only inadequate, but also were much too conservative.

Charles (Chuck) D. Babcock graduated from here one year before me and joined the GALCIT teaching faculty. I think he had the distinction of being one of the few professors, or the only one, who had merely two serious publications to his name when he was appointed full professor. He died of kidney cancer in 1987, while he was the vice provost under Provost Robbie Vogt. So that's very sad. I still miss him today. As you can see, his picture is here on my desk. Chuck was a very personable individual. He thought very much about people, and Hans Liepmann relied on him very heavily for this reason.

And you may not want to print this, but I heard it firsthand from people who have now died, that Hans Liepmann would have liked to shut down the program in solid mechanics. At that time, there were only Chuck and I in solid mechanics since Ernie Sechler had retired (unfortunately he died about two years later). But Liepmann couldn't quite shut it down, because he needed Chuck Babcock. Chuck Babcock "held his hands."

At the end of World War II, Bert Fung had gotten a GALCIT fellowship while he was still in China, but he couldn't come right away. He came a year or two later and showed up in Ernie Sechler's office, saying, "Here I am. I am Bert Fung; I have a fellowship here." But after two years, the fellowship wasn't there anymore. Yet Ernie took care of him; Bert remained Sechler's student and then was appointed into the professorial track. He worked on structural dynamics problems in the form of flutter, which represents the situation of a wing structure or a panel on an airplane when one runs air over it at a high speed, and it begins to vibrate. The air excites the vibrations, and then the question is: How does one control that? How do you study such phenomena? That was Bert Fung's original domain.

He developed a very strong reputation in that field, and then somewhere in, I think, the late 1960s, early '70s, he took a sabbatical. He went to Germany for a year and later said, he didn't have an awful lot to do, but sabbaticals are for regrouping your mind. And, as I recall, his mother had some health issues that he was worried about, and so he looked into them. As he studied papers in the medical journals, it occurred to him that there was no medical doctor who talked about anything related to forces and that many of the things in the human body really involve mechanical issues. And so he started to get heavily interested in biomechanics, and I remember that he started some laboratory measurements on biomechanical tissues. And then Sol [Stanford Solomon] Penner (born in Unna, Germany), who was a professor of jet propulsion here, went to the newly formed UC San Diego campus to build the engineering department there. He enticed Bert Fung to come to San Diego, and Bert Fung is today probably still counted as one of the most outstanding biomechanics researchers. In his day, there were only four or five people who were members of all three National Academies [Academies of Engineering and Science, and the Institute of Medicine], and Bert is one of them. And so he had a very good reputation that started at GALCIT.

What I heard second-hand—and Bert never wanted to confirm this—was that in the early ’70s he went to Fred Lindvall, who was then chair of EAS—who incidentally appointed me as an assistant professor in 1965—with the idea that he needed funds to start a biomechanics effort at Caltech, but might have to go somewhere else if Caltech wasn’t interested And Lindvall did not listen to or fulfill his request. Bert claims he never asked for it outright, but Lindvall never offered him any resources for investing in a new engineering orientation.

My own involvement in bioengineering or biomechanics came in about 1977 in two forms. An engineer from JPL, Nick Panagiotacopulos, approached me with the desire to study spinal disc issues, because he had experienced severe pain as a result of a perforated disk. He had teamed up with two orthopedic surgeons from the USC medical center, the senior of whom was Dr. [Paul] Harvey, the chair of orthopedics. So we began to study the mechanical behavior of the human intervertebral disk with the idea to investigate its failure behavior. We found that the surprisingly flexible structure was really a pressure vessel containing a squishy nucleus (*n. pulposis*) with a flexible wall (*annulus fibrosus*); the latter is made up of layers of unidirectional composite construction: Collagen fibers are embedded in a matrix of mucopolysaccharide, and the layers are arranged with their fibers oriented alternately at an angle on the order of +/- 70 degrees with respect to the spinal axis. A fiber-composite arrangement by which nature had “anticipated” aircraft composites by millennia!

It turned out that this arrangement was highly viscoelastic, and our studies attracted the attention of the Air Force, because our emphasis on characterizing failure behavior was important in the agency’s desire to mitigate spinal damage resulting from ejection from fighter aircraft. We labored together for some years with an additional postdoc but shied away from taking on graduate students in that field, because it was virtually impossible at that time to find a position for a PhD in that field. I asked Bert Fung: “What do you do with your students when they’re done?” He said, “Well that’s always been a problem.” And so, I decided I simply wouldn’t follow that line of research because I couldn’t really have a student here for four to five years, and then he has to sell gas for a living. So, I shut that down. Anyway, you try things. Sometimes you're too much ahead of your time.

We started a second biomedical project a bit later, involving ophthalmologists from the USC Doheny Eye Institute. One of them, Ron Smith, was studying the durability of radial keratotomy, a means to alleviate or remove myopia by placing radial surface cuts into the cornea of the eye. They approached me in 1979 with questions about how the depth, length, and number of cuts would interplay to yield a desired adjustment.

The way in which radial keratotomy was discovered was literally through an accident in which a Russian boy experienced cuts in the eye from shattered and flying glass. His ophthalmologist, S. N. Feodorov, suggested that the cuts had "severed a collagen ligament surrounding the pupil of the eye," thereby changing/flattening the curvature of the cornea to correct vision acuity. No one was able find the ligament, however, and it was the quest of the Doheny ophthalmologists to look for it or find another explanation of why radial keratotomy worked. It therefore seemed right to perform an experimental similitude study, whereby one manufactured an oversized circular cornea of the proper shape (tenfold size scale, cast from a polyurethane rubber that I had left over from my viscoelastic fracture studies) subjected to properly scaled internal ocular pressure.

I had two students from the experimental course Aero 104, Paul Rapacz and Kevin Sene, who took this on as a third-term experimental course project. Upon cutting radial surface cracks of varying depth, length, and location with respect to the center, one could readily measure the change in curvature of the pupillary area. This process worked so well that it explained all the corrective action of typical surgeries, so that the Doheny ophthalmologists used the results for a long time in their research. No Feodorov ligament was necessary, and that is why no one could find it.

Incidentally, I presented these research findings at an Ophthalmology Congress in Florida and was floored by the lack of acceptance/understanding by the medical profession of what seemed to be an obviously mechanical issue. There was a total mismatch of concepts—for which I was simply not prepared. I decided it was futile to publish the work in a relevant journal under such circumstances; I frankly did not have the time to take away from my other research responsibilities to pursue this further.

So anyway, Bert Fung made a big contribution to GALCIT here, and then went on to UC San Diego. He must be over 90 by now, and he's currently not doing very well. [Bert Fung died at the age of 100 in December 2019. *–Ed.*] For the 90th birthday of my advisor, Max Williams, I put together a picture/letter volume of his former friends and acquaintances, but Bert's son wrote me, "I'm sorry, but my father cannot get it together anymore." Bert was here at Caltech around 2010, and he still seemed fine, but I also know that merely talking with people is quite different from organizing your mind and putting something more formal together. So anyway, that's one part of the really important GALCIT history, which spawned at least one internationally important biomechanics effort.

Another PhD student of note from here is Gil [Gilbert] Hegemeier, who went to UC San Diego after graduating. His thesis dealt with a contemporary rocket issue that concerns what happens when an explosive blast passes over the shell of a rocket: Does that screw it up or not? That was a very difficult problem, and Gil dealt with that for his thesis. And I remember that Ernie said, "Well, let's not work the real problem. Let's just see what happens when we have a string and run a load at supersonic speed over the string." So, this is one of the engineering approximations they wanted us to learn. And Gil has become a very influential national contributor to the issue of safety under blast loads. Moreover, he built a huge earthquake engineering center down in San Diego, even though at Caltech he had had no connection to earthquake engineering of any sort. So, I thought Gil Hegemeier was a very good contribution from GALCIT, equal to Max Williams.

Max was also a student of Ernie Sechler's. He came from the Air Force after World War II, where I think he worked in flight safety, flight operations, somehow. He came to get a master's degree but stayed to get a PhD and worked on large deformation problems of plates and shells. I remember him saying that after he finished his PhD, he walked into Ernie's office to talk about how he could get research support money. And at the time the US government was getting into funding university research, and Ernie suggested to Max, "Why don't you try for one of these new federal grants?" And Max did very well in that. He received numerous grants, and soon became involved primarily in issues related to solid propellant rocket fuels. Maybe I should first say how that came about.

About that time, when supersonic flight came on board, so to speak, efficient flight required swept wings (a concept in which von Kármán was leading). When one bends a swept wing up or down, stress concentrations are developed in the corners, meaning the stresses are significantly higher there than elsewhere. And one needs to understand how high these stresses are to design against break-initiation there. Now, the problem is made worse by the fact that at the trailing edge, the wing/body interface forms a sharp corner, smaller than 90 degrees, which is the worst imaginable situation.

So, Max Williams investigated what happens simply to the stresses in sharp corners as one changes the corner angle: How does that angular change alter the stresses? And he got a very beautiful, very important solution through a nice mathematical scheme (eigenvalue solution) showing that when you make that angle zero, one is confronted with a crack. And this crack solution (within the linearly elastic domain) is the most important analytical tool used today in fracture mechanics. There are a number of individuals who said (after the publication of the Williams eigenvalue solution): “Oh yeah, we knew all about that.” However, nobody had shown the most important feature before, namely, that all crack solutions have this same mathematical structure—what we today call a square root singularity. And no matter what the rest of the geometry is in the vicinity of the crack tip, it’s the square root singularity—and the factor that multiplies the singularity—that determines basically how big the overall stresses are in the neighborhood of the crack tip even if they are infinite at the tip itself. This multiplying factor is determined by the general loading condition and the geometry of the solid. Thus, one can separate the influence of the geometry from that of the loading, and today this feature is the basis of the engineering field called linear fracture mechanics. Through various augmentations, it has been taken over into plasticity and other properties governing the behavior of materials. Ultimately this work and its influence was the basis for Max Williams’s induction into the National Academy of Engineering [NAE].

As a result of that finding, he became involved in fracture mechanics, rather than being limited to special aircraft structural problems, but people on the East Coast considered him a bit of an interloper and never quite wanted to accept him. George Irwin, who is sometimes credited as being the father of fracture mechanics, didn't get along well with Max. I remember Max telling this story, where they were both at a conference. He was in the elevator, and this man (Irwin) walks in, looks at Max's conference name tag, and says, "Oh, you're Max Williams. So, you think you know something about fracture?" And they didn't talk much after that, though Max was always civil about this encounter. And as a result, there was always this dichotomy between what I call the Northeast fracture community—Harvard, Brown, MIT— and the Western contingent. It's largely gone away over the years, but it had its impact on which government agency funded whom. And therefore, until Ares Rosakis came to Caltech, we didn't have much funding from the Navy (the major supporter of George Irwin). I'm working on a Navy project today, but that has nothing to do with that early fracture history, although it is connected with another area of Williams's leadership that derived from his activity at GALCIT.

He got involved in the rocket business shortly before 1958. The late 1950s was the time of large national projects involving intercontinental ballistic missiles and the start of space travel planning. In particular the quick launch readiness required by the exigencies of the Cold War put a premium on increasing the use of solid rocket fuels, which did not require the extra time that fueling a liquid rocket demanded. Early solid rocket fuels were a basic mixture of an oxygen carrier (ammonium perchlorate in granulate form) with an asphalt as the carbon carrier. These had been used already near the end of World War II through JPL for JATO (Jet assisted takeoff) applications that allowed airplanes to take off from shorter runways. These fuels felt like a rigid solid, like asphalt used in road construction, but they could creep with time, especially under elevated temperatures (like asphalt in the sun), which had some detrimental effects: Early "solid rockets" were designed as hollow cylinders encased in a metallic shell to contain the pressure; the central, cylindrical bore (the perforation) was ignited to generate the burning gas, which then pushed out through the nozzle at the rear end. If these rockets were stored for any length of time in a vertical position, the internal shape of the perforation could and would change into a slumping profile, thereby restricting the gas flow out through the nozzle. This was the situation during the latter part of the war when

JPL was involved in producing JATO rockets.

Maybe I should talk a bit more about what typical current solid propellants are. When one mixes ammonium perchlorate—which has a lot of oxygen in it—with something that carries carbon to burn—like asphalt or rubber—one generates, upon ignition, a lot of high- temperature gas. So, one mixes powder particles of ammonium perchlorate—which is a salt—together with a rubber, which bonds to the particles and holds the material assembly together; one further adds particulate components for controlling the burning rate (e.g., aluminum powder, because it transfers/conducts the heat more rapidly than the rubber). And the mixture has kind of the consistency of rubber in an automobile tire. Maybe a little bit stiffer, maybe a little bit softer. When that concoction burns, it gives rise to a very hot flame with conversion of the solid to the gas.

Some issues arose early on, which had already been experienced at JPL. When you make this kind of mixture with asphalt—before rubber was being used—and store a rocket standing on its tail, the fuel components would slowly, over a year or more, settle down, because it wasn't like rubber really but more like a highly viscous honey. That settling would generate a constriction inside the bore, which interfered with the gas flow.

Now material with that kind of characteristic is what we typically call time-dependent, which strictly speaking is a misnomer. In reality the material may not change its characteristics simply because of time change (unless one is concerned with a potential chemical ageing process); it deforms over time, even though any loading may remain constant. The technical word for this type of material is *viscoelastic*: It has rubbery characteristics (one could eventually eliminate any deformation if one were to completely unload it to regain its original shape, but that will take time), which is governed by a characteristic viscosity. So, it's a viscous and elastic material all in one. A better nomenclature for its characterization would really be a "rate-dependent material," because its stiffness depends on the speed or "rate" of deformation to which it is subjected: High rates of deformation give rise to large forces (stresses), while low rates are associated with small ones. During the early 1960s it was really the first time that this type of material posed a major engineering problem, because understanding its use had become a major national issue with regard to the missiles and rockets that had to be fire-ready without first "tanking up."

For connecting this type of material behavior to my academic effort, it is important to understand a further fact of life in the solid-rocket-propellant business: One disadvantage of having a circular bore in the "charge" of the rocket was that when the bore grew as a result of burning propellant away, the burning area increased. Thus, more gas was generated during the flight time. As a result, the thrust would change over time, which is not desirable for several reasons. Consequently, one designed the internal "perforation" as a star-shaped cross-section in such a way that the surface burned away at a constant rate. Towards burnout, it wasn't quite constant, but one could live with that. But choosing this configuration meant trading one problem for another: The tips of the stars became areas of increased stresses (stress concentrations) during both ignition and the in-flight pressurization, thus becoming potential regions of fracture. If a fracture did occur, this would generate an involuntary increase in burning surface, creating excessive gas and over-pressure that would lead to failure. Alternatively, a related problem would/could arise if such a rocket motor was stored for long periods of time. In that scenario, the propellant could undergo chemical changes with associated volumetric changes, leading to a build-up of stresses accompanied by the stress concentrations at the star-tips. It might then be only a matter of time ("time-dependent material") until a crack appeared at one, or more, star tips—even before a rocket start-up.

Other problems involving critical time-dependent failure processes were associated with holding the propellant charge (usually referred to as the "grain") against the outer casing or shell of the rocket motor. There existed typically geometric discontinuities that always gave rise to large stress concentrations that needed to be minimized; these types of issues were normally summarized under the topic of adhesion problems. This particular problem area expanded into a major research program at GALCIT area after missile and rocket problems phased out as the technology advanced.

Max Williams had entered the solid propellant rocket arena very early, when the major effort in the engineering community centered on understanding how to deal with the new viscoelastic material in an engineering environment. He had a sub-contract with Thiokol to deal with this issue. That resulted in a universally used document often referred to as the "Bible of Viscoelasticity," the content of which pervades still all the publications on viscoelastic topics. As that topic became thus better understood during my first graduate years, the need arose more strongly to understand the time-dependent processes of failure/fracture.

This is where Max Williams also made a dominant contribution in solid propellant arena, because of his close connection to the then-growing field of fracture mechanics. The viscoelastic properties raised new issues because facture did not occur in rigid solids like metals, but with a material that has time-dependent characteristics. These fractures wouldn't occur suddenly. They might arise as a result of cooling a rocket motor from its cure-temperature because of the shrinkage of the motor charge, or from the volume change associated with the chemistry of the cure process. There was concern that under cold conditions it might pop like a watermelon. But typically, it's a slow growth. Moreover, after a missile system had been installed in a national facility, the rockets would remain in silos for years. (Some of these missiles, like the *Minuteman*, could sit vertically for 10 or 20 years in the silos.) To assess whether a systematic failure developed with time, the procedure was to periodically select one and fire it to better assess whether the whole rocket system was not falling apart. But that couldn't confirm whether or not the remaining, untested ones did or did not have a problem.

Even early in the missile game, before arriving at the storage and deployment stage, it had been clear that much more knowledge was needed to assess the failure initiation and crack propagation once missiles were being used. I first became aware of this during my first graduate year at GALCIT through my contact with Max Williams, who became my advisor.

Before I get to that stage of my academic progression, I should perhaps talk briefly about how I almost missed out of being admitted to graduate school in GALCIT: I was doing well as a student (graduated with honors), and in my senior year there seemed to be no question that I should continue as a graduate student—I had always had the philosophical orientation that I would go as far as I could in academia. But I was awoken to the observation that I was rather naïve on how that worked, when Helen Burrus, Ernie Sechler's secretary (remember he was in charge of entering graduate students) asked me whether I did not want to go on to the master's program the following year. Little did I know—I had totally failed to investigate—that one had to fill out an application so a committee could decide whether to admit me or not (I later spent 14 years being responsible for the GALCIT admissions process). Well, I fortunately still had a few days until the applications deadline, and here I am. I was admitted and given a teaching assistantship with Ernie Sechler in his course Aero 102, which I would later teach for many years.

Two important things regarding my future happened during my master's year; it was a matter of being in the right place at the tight time. Because I now had a teaching assistantship for support during my first year of graduate studies, I did not need to worry about approaching a professor for immediate support to start any exploratory research activities right away. I could thus experience what was developing during that year.

My professional intention had been that I would study jet (rocket) propulsion, so during that year I approached Frank Marble about working for/with him. His response took me back a bit, as it was not very enthusiastic (we had not had any prior personal contact, and Frank typically was rather cagey and played matters quite close to his vest): "Why don't we see how things go by the end of the year?"

During that same year I had signed up for three terms of Aero 107 (elasticity applied to aeronautics), which was being taught by Max Williams, who had started to become involved in the structural failure problems of solid propellant rockets. This interest shone through his teaching: the advertised topics were the classical ones of anisotropic elasticity, thermal and large deformations of plates (related to his PhD thesis), shell theory and approximate methods of structural analysis based on minimum energy principles, elastic stability (buckling), and some wave propagation. These topics got me interested, because there was always the undertone of relevance to real-world problems.

As a final project in the third term Max Williams tasked us with picking on our own a topic that was suitable for the course material, to develop the solution, and then present it to the class, with a classmate asking (embarrassing) questions. The class grade (P/F) would be based on the class-appropriate choice of the topic, the execution of the project, the results, and finally the response to the classmate's questions (and how well one formulated the questions in the student-student interaction). I had chosen to deal with rocket-motor problem involving a circular hollow cylinder, bonded to an elastic shell while being pressurized. The solution for the infinite cylinder was trivial, but the introduction of the cylinder's finiteness and associated stress-free cross-sections made a closed-form solution virtually impossible (certainly not for a finals problem), but a decent subject for an approximate solution. It was a sizeable computational effort without having any digital computer available, yet Max told me later, that he was impressed with the logic of how I tackled and organized the work. He did not care whether there was a computational error somewhere—which I subsequently found as a result of the student-student interaction with my then-office mate Dick Schapery. Shortly after that term was over, Max asked me whether I would want a summer job working in his group. I told him: "Yes"

Not long after being appointed an assistant professor in aeronautics Max (so he told me) had asked his former PhD advisor, Ernie Sechler, about getting support for his research. But Ernie did not have institutional funds available and suggested to Max that there were new funds being made available by the federal defense departments for performing fundamental research in a variety of areas, including aeronautics and the rocket/missile domain. Max pursued this and soon found funding for dynamic fracture research through Wright–Patterson Air Force Base and through contact with a major solid propellant rocket company, Thiokol, in Utah. Someone at Thiokol had worked on finding an antifreeze fluid, but the concoctions always jelled into a rubber. It was decided that the rubber would be a great binder (carbon carrier) for a solid rocket propellant, and as of 2019 Thiokol still manufactures the strap-on boosters for space launches.

And that's the area in which I got my degree. Max dealt with fracture mechanics, starting with static problems. He also started research into dynamic fracture; and he built, together or with the help of Albert (Al) Ellis, a state-of-the art (for the time) ultrahigh speed camera to capture fast moving fractures. Ellis, an associate professor in fluid mechanics who had his laboratory in Kármán, had developed a camera for taking high-speed pictures, at 250,000 frames a second, of bubbles that burst or collapsed during the cavitation process. Max Williams took the same design and adapted it for solid mechanics imaging. So that was the beginning of dynamic fracture studies at Caltech, and Max graduated one PhD on dynamic fracture before leaving for Utah.

After 1973 I used that camera very successfully with my student Krishnaswamy

Ravi-Chandar (today a professor at the University of Texas Austin) in an extended study that is still considered an important contribution. It involved building a capacitor-cased electromechanical loading device that could generate very high stresses along the crack surfaces in microseconds and made possible the use of existing closed-form solutions for evaluating experimental results without approximations. This fact had been a major barrier to interpreting dynamic fracture results worldwide. Today that picture has changed because computational analyses have supplanted the shortcomings of analyses in the 1970s and earlier.

Ares Rosakis used the same camera when he came here. And I really believe that because of Max's insistence and our evolution from that, that Caltech has had a very significant impact on fracture mechanics as it is today. It's not at the forefront, but there is still a journal that Max started, which today is called *International Journal of Fracture*. Originally it was called *Fracture Mechanics*, and I think he changed the name because many people think of a mechanic as somebody who does the plumbing under the house. That wasn't quite the idea. But his academic grandson and my former student Ravi-Chandar now runs it out of Austin, Texas. And today, internationally, it is still the preeminent fracture journal.

So then after 1964–65—I think about the time Bert Fung left—Max Williams got an offer to become dean of engineering at the University of Utah. He went to Utah and was there for a number of years, then became dean of engineering at the University of Pittsburgh (he grew up in Pittsburgh). And with that, he kind of faded out of the directly academic research field. He was influential with the Defense Department, looking at some of their larger design and programmatic issues, and he wielded a lot of power in the Defense Department.

He had his 90th birthday on February 22 this year (2012), and, as I said, I put a little volume together with all the pictures and letters from his students, colleagues, and other associates, and took it to him personally. And he was just about as sharp then as he has ever been. Really marvelous.

While he was a graduate student at Caltech, he had taken a class from Horace Gilbert, who was what we would call today a practical economist. He taught Max how one assesses companies for their value, and what is important in forming and running them. Max really appreciated that, and in the long run it greatly influenced his interest in company startups, having already started companies while he was at GALCIT. For example, Mathematical Sciences Northwest in Seattle became well known as a consulting firm shortly after he left Caltech. And after that, he formed many companies, I would say left and right, but I don't know how many because he never counted or told me that he counted them. But he was very successful in doing those things and about 10 years ago, about the time of my official retirement (2004), he asked me whether Caltech had improved in educating our engineering students in that direction because he was interested in providing a donation to Caltech for that kind of activity. And by that time, we had somebody, but I forgot the name now. Somebody who was doing this kind of, and he taught a class in that direction. It could have been Kenneth Picard; so anyway at least I could report that back.

I should have mentioned that in the latter part of Ernie Sechler's academic life, there was a real push for energy change. He and Homer J. Stewart, who was a fluid mechanics expert, were very interested in windmills. I don't think that their work ever resulted in any hardware, but that was kind of where Ernie Sechler left off.

All right, then comes kind of the next generation, Chuck Babcock, then I, then Ares Rosakis and Ravichandran. Then Michael Ortiz and the younger ones.

But there's another thing that I believe ought to be brought out. In those early days, in Bert Fung and Max Williams's time, there was very good cooperation between applied mechanics, civil engineering, and jet propulsion. There was George Housner, Don Hudson, Eli Sternberg, Jim Knowles, Julius Miklowitz, and Ron Scott. These were all kind of cooperative, and we conducted seminars every week. But then that kind of started to thin out as people got a little older. People retired and a new generation came along. Cooperation still exists today, to a certain degree, but I view it as somewhat more fractionated. That's not a very kind word, but it's more that way because between professors, there can be issues that have to do with funding, time and pressure, young people, interaction. I still look at the children of the '70s as being not very cooperative in our school. The younger ones tend to be more narrowly focused. Today, it's also a different scene in that there are not that many visitors who want to travel here to give seminar talks, and we usually don't have the money to fully pay for seminar speakers to travel.

So that may be one reason, but it's worthwhile to note that up to roughly the millennial change, there was a very broad interaction between solid mechanics over in Thomas and here. When Ares Rosakis came, during his first year, he sat in Eli Sternberg's elasticity course, and between you and me, I'm not sure how much of that was political. But he learned a lot from the style of Eli Sternberg, because Eli would write things on the board very carefully. You would just have to use a camera and you could write his book. Very logic oriented. Oh, I forgot to mention Tom Caughey, who was part of the group of vibration dynamics. Bill Iwan, then Paul Jennings—those are the students. These were all very closely related, but then by the time the next generation came, that kind of changed for a while. But today it appears that this type of close interaction has been established again. I remember Don Hudson once saying that when he was young, there was this influx of roughly equally aged professorial appointments, and that that cycle renews itself, with the next generation following his age group

Let me start with Chuck Babcock. Chuck, being Ernie's student, continued basically working on buckling issues and in particular, he had learned through his thesis with Ernie Sechler a very important issue of why the analytical predictions for shell buckling under axial compression didn't work out very well. He set out to prove that. And I think he was one of maybe the two people—maybe even the dominant person—in the U.S. who experimentally demonstrated how to do that. Nick [Nicholas] Hoff, a colleague of Ernie Spector's age at Stanford, also performed similar experiments. But Chuck Babcock very carefully made thin-walled cylindrical shells, through a process of electrical copper deposition over a precisely machined wax mold that later melted away. One could also build in precisely determined imperfections.

The whole manufacturing process was very precise and controlled, and thus he didn't have any argument about how good the shells were. And he demonstrated that the manner in which a shell is held or fixed at its ends has a very strong effect on how the shell buckles. I always thought that that was a very neat experiment. People from McDonnell Douglas down here brought large shells in, and they compressed them axially in the lab and then measured them all around and closely followed the deformation process in detail just to make sure that the same held true for larger scaled-up shells.

In those days, we had reams and reams of IBM cards on which this data was processed on a computer, and I think those findings made a serious impact on the whole shell business. Johann Arbocz, who was a teacher at Northrop Institute in LA before coming here for his PhD, was a strong collaborator on that experiment; and after receiving his doctorate at Caltech and spending some time here as a senior research fellow, he got called as a professor to Delft University in Holland, where he retired in about 2010.

Another student who came out of Chuck's nest was Stelios Kyriakides. Chuck had worried with him about what happens if you have a long tube like a pipeline, and you pressurize it from the outside. The tube may collapse at one point and then flatten. And this collapse then propagates down the whole length of the tube. It's kind of what you call instant mechanical instability, and it was very interesting and important because all the oil pipelines on the ocean floor deal with that. And as a result, Chuck and Stelios made a big impact there. Stelios was elected to the NAE and has become and still is a professor of aerospace and mechanical engineering in Austin, Texas, along with two of former students, Ravi-Chandar and Ken [Kenneth] Liechti, who has made a significant impact on adhesion mechanics and is currently semi-retired for health reasons. They're really our competition today in experimental mechanics. But anyway, Stelios became a consultant for oil companies all over the world, with respect to this. And I think that helped him greatly to become a member of the NAE.

APPENDIX D

"Kleinisms"

Dr. Arthur Klein

AE253a: Design of Aircraft Components

- "Plastic materials make good tooth-brush handles"
- "No specialist is competnent (sic) to design anything."
- "You never see anything but the outside of a body."
- "If you try to do fancy things you'll fall flat on your face."
- "You generally know whether it is bad or just normal."
- "The big airplanes are going to be more talked about than used."
- "In an airplane you must have something to hang onto all the time."
- "50% of designers have no reason at all for design decisions."
- "Fine problems in airplane design are not subject to exact analysis."
- "The center of propellor disc is always in the center."
- "If you get two test pilots together you have three opinions."
- "Nothing but upholstery is more flexible than aircraft structures."
- "There are no straight members in an airplane."
- "A sensible engineer is not an inventor."
- "Human beings are more important than airplanes."
- "The only person in an airplane more important than a pilot is an engineer."
- "Go back to Newton!"
- "Don't ever invent anything unless you have to."
- "The length of an American engine is 19" longer than the length of an engine."
- "Benny Howard's definition of an accident: "When the worst conditions and the worst pilots get together, you have an airplane accident." [*Benjamin Odell Howard was an American aviator and widely respected aeronautical engineer.*]
- "A pilot's right arm is the only reliable power supply in the airplane."
- "A safety device is a safety device as long as people do not depend on it."
- "Design airplanes on the basis that everything fails to work."
- "Engineers tend to forget laws of Physics."
- "Big and small airplanes have the same size pilots."

- “People think that they get close tolerances on airplane parts just because they require them on the drawings.”
- “A happy executive is a useless individual.”
- “A missile is something that almost works, and an airplane is something that almost doesn’t.”
- “The length of the landing gear is governed by the fact that it must touch the ground.”
- “An organization is a method for delegating worries.”

APPENDIX E

GALCIT and the Challenger Disaster

February 2016

***Courageous**: "Richard Feynman, who was willing to state his opinion and stand his ground on the Challenger disaster. His appendix to the commission report is a must-read on safety analysis."*

This note, by an (unnamed) Caltech alumnus on the last page of the *E&S* (Caltech's *Engineering & Science*) magazine's 2015 winter issue reminded me of my interaction with Dick Feynman at the start of the Rogers Commission meetings, as did this year's January 26 program of the Caltech Alumni Association on Dick Feynman, two days before the 30-year anniversary of the Challenger disaster. This subsequent recount is not to detract from Dick's admirable contribution to the commission findings, but more to possibly illustrate the longstanding cross-fertilization and interdependence across divisional boundaries at Caltech, in this case involving work at GALCIT (then the Graduate Aeronautical Laboratories, California Institute of Technology). This interchange with Dick Feynman before the beginnings of the Rogers Commission hearings concerned the sensitivity of the mechanical properties of polymers to seemingly small temperature changes as exemplified in his ice-water–clamp demonstration during the Rogers commission hearings.

Some prehistory is in order here as to how the following story relates to the important role, which GALCIT has played in the development of solid propellant rocket engineering starting in the late 1950s. These early developments were heavily supported by Morton Thiokol, the manufacturer of the failed Challenger solid rocket booster(s) through a research program at GALCIT, to develop the methods for stress and failure analysis germane to solid propellant rocket fuels and related solid rocket design. At that time these solids were unusual engineering materials because, in contrast to other solids used in engineering applications, their properties exhibited time-dependent (viscoelastic) mechanical properties resulting in mechanical characteristics that required close attention to how loads and deformations changed with time, whether that involved small fractions of seconds (motor ignition) or months or years (storage).

With this national need for predictive analysis procedures, this multiyear Thiokol-program, envisioned and led by Prof. Max Williams, my PhD mentor was, if not the final, nevertheless an early hallmark for structural viscoelastic design procedures. The final report[1] of that project became known euphemistically in the US for many years as the "Bible of Viscoelasticity." Moreover, years of fruitful research grants at GALCIT from NASA (Paul Wetzel, monitor at NASA headquarters[2]) and from the Air Force Office of Scientific Research were devoted to the failure/fracture progression in viscoelastic (solid propellant-like) materials, in which the time-temperature connection played a highly significant role.

Although my contacts with Dick Feynman up to that time had not included technical/scientific matters apart from occasional conversations at the Athenaeum[3] roundtable(s) it was still doubtful that a theoretical physicist, prone to approaching a problem from physical feel rather than abstract mathematical viewpoint would be sufficiently familiar with the engineering aspects of the sensitivity of the mechanical behavior of viscoelastic solids under temperature changes so as to assess the response of a viscoelastic seal under the time scale of a flight scenario. I decided to offer Dick my personal understanding in the event that this topic should, indeed, come up during the commission meetings.[4]

Prior to the meetings of the Rogers Commission, stories had circulated that a blow-by failure of a rubber-based O-ring in a field joint of a solid-fuel booster could have

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[1] "Fundamental Studies Relating to Systems analysis of Solid Propellants," Caltech, February 1961, (GALCIT SM-61)

[2] Under this program, the first mechanics-based fracture theory for viscoelastic solids was developed.

[3] The faculty club at Caltech

[4] I felt comfortable to approach him on this topic because we had previously met repeatedly, also off-campus, if not in regard to technical/scientific matters, but because his daughter, Michelle, was a member of the Pasadena Young Musicians Orchestra as was my youngest son, and Dick had been at our home several times arranging chairs and tables for the annual parties we offered this youth organization in our home (my wife was the parental "supervisor" for the orchestra on its 1984 summer tour to Europe). On another occasion Dick and his wife, Gweneth, who, like my wife, was a member of the Caltech-based Arroyo Singers, had come to our house to connect a video player to our television for my wife while I was on a trip in the eastern US so that the family could see *Guys and Dolls*, because my son had just been assigned a lead part in a high school rendering.

been associated with or caused by the unusually low temperate (around 0^{o} C) at Cape Canaveral at the time of the launch. Before I spoke with Dick, I wanted to find out as much as possible about the properties of the O-seal rings to pass on and, therefore, started to call my contacts at Morton Thiokol. The first one, Gary Anderson, was my academic brother in the sense that his PhD advisor was also Max Williams when the latter was dean of engineering at the University of Utah. It was very obvious that Gary had great reluctance to talk about that issue, but neither would he deny the possibility and I was sure I would receive no further significant information from any of my other four or five contacts at Thiokol in this regard.

I called Dick's office before the Rogers Commission was to start, but his secretary informed me that he had just left for that meeting. She gave me his telephone number at a hotel —which I thought at the time to be quite below the standard for such a commission member— and encouraged me to call him there, saying he "would not mind"; but I deferred, not knowing that he was still en route at that moment, and saying that I was to leave for Washington the next day and could take up contact with him once I arrived in DC.

This I did at the end of the next day, after the first commission meeting had passed, and we spoke after I had introduced myself as a colleague from GALCIT, leaving out our previous personal encounters to save time. Thus, I was later never sure whether he really knew who was talking to him. But after informing him that my concern was with the exploration of the role of temperature of the O-rings, I spoke to him in general about what my research for the past 15 years or so had been, and about the mechanical behavior of solid propellants and similar materials, including the insulation inside the motors and the polymeric O-rings. In particular I opined that it might become important to address the issue of temperature effects on the mechanical response behavior of the seals and that the time response of these materials was governed by a time-multiplying function of temperature, such that lower temperatures elongated the effective time response—making it slower—while heating had the opposite effect.

We did speak about the fact that I could not obtain specific material data on the O-seal component in the boosters, and suddenly he asked: "Oh, you really want to help me?" It had not occurred to me beforehand that he would have had his load of crank calls in his career.

Upon the affirmative we covered several scenarios as to what happens with a viscoelastic material under tension or compression when different temperatures are involved. We covered situations when stress relaxation occurs, and that the decrease of stresses upon deformation occurs more rapidly the higher the temperature is, and that the converse holds for colder temperatures. Moreover, we discussed the (creep) situation of a viscoelastic material deformed by a force/pressure and how fast the deformed configuration could return to its unloaded shape once the forcing agent had been removed or reduced: that it happens quickly at the higher temperatures, and slowly, possibly very slowly, at lower temperatures. What I could not tell him about was the quantitative change in time scales because I had no direct information on the property of the (viscoelastic) components in the solid rocket boosters. But we did discuss the qualitative effects depending on how close the material dropped to the glass transition temperature—and that at that temperature the material would be as solid as Plexiglas at room temperature and any seal deformation would not be recoverable quickly at all. I had hoped that if this topic did come up, there would be a way to ask the appropriately relevant questions during the committee meetings. As it turned out his ice-cup experiment resolved that issue.

We conversed along these lines for nearly half an hour, at the end of which he thanked me for my concern. At no time did his interested responses imply that he was already aware of these material issues. I do not think that under the circumstances at that time he was being merely courteous to me for such an extended phone conversation if there had been no need or purpose for such interaction.

The rest is, I think, recorded history.

Postscript

Having read at least two accounts of Dick Feynman's introduction to the O-seal issues in the references below, I have not been able to find that Dick was informed earlier of the possible thermo-viscoelastic implications of the O-ring material. It also seems a bit strange that General [Donald J.] Kutyna brought up the O-ring issue in a manner different than Dick recalled, and only as very recent findings during the time of the

Commission meetings, especially since I had learned about this issue basically through the press in the weeks before these meetings started. Also, Dick's briefing at Caltech's Jet Propulsion Laboratory—on the day before he left for Washington via the red-eye—discussed erosion on O-rings, but in that context no mention seems to have arisen of the thermal implication for the slow resilience of the O-rings at low temperatures.

1) *What Do You Care What Other People Think?* 1988; R. P. Feynman as told to Ralph Leighton. W.W. Norton & Company, New York, London

2) *No Ordinary Genius: The Illustrated Richard Feynman*, 1994; Christopher Sykes, editor. W.W. Norton & Company, New York, London

APPENDIX F

Fifty Years of Wolfgang Knauss Collaborators

Doctoral Students

Students listed with an asterisk (*) were advised by Chuck Babcock, and I supervised them for completing their PhD degrees. There were several publications with Anthony Waas.

- Mueller, Hans-Karl C. A. (1968) *Stable crack propagation in a viscoelastic strip.*
- Pučik, Thomas Antone (1972; deceased) *Elastostatic interaction of cracks in the infinite plane.*
- Palaniswamy, Karuppagounder (1972) *Crack propagation under general in-plane loading.*
- Smith, Gordon Carl (1975) *An experimental investigation of the dynamic fracture of a brittle material.*
- Liechti, Kenneth M. (1980**)** *The application of optical interferometry to time dependent unbonding.*
- Ravi-Chandar, K. (1982) *An Experimental Investigation into the Mechanics of Dynamic Fracture.*
- Heymans, Luc J. (1983**)** *An Engineering Analysis of Polymer Film Adhesion to Rigid Substrates.*
- Ungsuwarungsri, Tawach (1986) *The Effect of Strain-Softening Cohesive Material on Crack Stability.*
- Giezen, Jurgen Johannes* (1988) *Plastic Buckling of Cylinders Under Biaxial Loading.*
- Madhavan, Raghu* (1988) *On the Collapse of Long Thick-Walled Circular Tubes under Biaxial Loading.*
- Waas, Anthony Marius* (1988) *Compression Failure of Fibrous Laminated Composites in the Presence of Stress Gradients: Experiment and Analysis.*
- Losi, Giancarlo Umberto Maria (1990) *Nonlinear thermoviscoelastic behavior of polymers.*
- Brinson, L. Catherine (1990) *Time-temperature response of multi-phase viscoelastic solids through numerical analysis.*
- Washabaugh, Peter D. (1990) *An experimental investigation of mode-I crack tip deformation.*
- Shimabukuro, Sy Ross (1991) *Stress Assisted Diffusion in Polymers.*
- Schultheisz, Carl R. (1991) *Comparison of experimental and computational crack-tip deformations using Moire interferometry and finite elements.*
- Pfaff, Richard D. (1991) *Three-dimensional effects in nonlinear fracture explored with interferometry.*
- Minahen, Timothy M. (1992) *Structural instabilities involving time dependent materials : theory and experiment.*
- Geubelle, Philippe H. (1993) *Nonlinear effects in interfacial fracture.*

• Pulos, Guillermo C. (1993) *Nonsteady crack propagation and craze behavior in PMMA.*
• Duong, Cong N. (1994) *A nonlinear thermoviscoelastic stress and fracture analysis of an adhesive bond.*
• Vendroux, Guillaume (1994) *Scanning tunneling microscopy in micromechanics investigations.*
• Lu, Hongbing (1997) *Nonlinear thermo-mechanical behavior of polymers under multiaxial loading.*
• Deng, Tony H. (1997) –*Measurement of the dynamic bulk compliance of polymers.*
• Lee, Sangwook (1998) *Failure of laminated composites at thickness discontinuities under complex loading and elevated temperatures.*
• Sane, Sandeep Bhalchandra (2001) *Time-dependent compressibility of poly (methylmethacrylate) (PMMA) : an experimental and molecular dynamics investigation.*
• Samudrala, Omprakash (2001) *Subsonic and intersonic crack growth along weak planes and bimaterial interfaces.* (Co-Advisor)
• Huang, Ying (2001) *Scanning tunneling microscopy and digital image correlation in nanomechanics investigations.*
• González Liñero, Luis (2002) *Global Fracture Analysis of Laminated Composite Materials for Aerospace Structures.*
• Chasiotis, Ioannis (2002) *The Strength of Polycrystalline Silicon at the Micro- and Nano-Scales with Applications to MEMS.*
• Zhu, Weidong (2002) *Nonlinearly viscoelastic response of glassy polymers.*
• González Liñero, Luis (2002) *Global Fracture Analysis of Laminated Composite Materials for Aerospace Structures.*

Engineer's degrees at GALCIT

• Mahale, Narayan Krishna (1973) . *Failure of precracked fiber reinforced composite plate.*
• Chang, Samuel Kwang Yeh (1984) *Crack Propagation in Viscoelastic Materials under Transient Loading with Application to Adhesively Bonded Structures.*
• Kubr, Thomas J. (1991) *Stresses near a change of thickness in a continuous-fiber-composite plate.*
• Bowen, John Murray (1992) *An experimental investigation of fracture at a bimaterial interface.*
• Gortsema, Steven Craig (1992) *An Experimental Investigation of the Failure of a Stepped Composite Plate.*
• Tsuyuki, Richard M. (1993) *Buckling of thermoviscoelastic structures under temporal and spatial temperature variation.* Deceased 4-4-2021.
• Sugawara, Satoshi (1994) *An experimental investigation of fracture at an interface between two epoxies.*
• Breton, Fabienne Anne (1995) *A large deformation analysis of plates or membranes for the determination of Young's modulus and Poisson's ratio.*
• Chea, Limdara O. (1997) *Finite element simulation and analysis of local stress concentration in polymers with a nonlinear viscoelastic constitutive model.*

• Gonzalez, Javier (1997) *Full field study of strain distribution near the crack tip in the fracture of solid propellants via large strain digital image correlation and optical microscopy.*
• Patel, Bibhuti Bhusan (1998) *An experimental investigation of wave propagation in fractured brittle material.*

"Exchange" Students from Overseas Universities

Students marked with asterisks wrote the research reports listed here, following the guidelines for Caltech's Engineer's Thesis, which then satisfied the requirements for their Diploma Thesis at their home institutions.

• Emri, Igor J**.,** starting 1980: Igor had a fellowship with Chuck Babcock, but came from Slovenia too early—before the new academic year when Chuck's funds were not ready. He thus came to me for support, wanting to work on plasticity-type problems. But he was willing to work on polymers and did a marvelous study on the swelling behavior of PMMA under varying moisture environments. That work and my acceptance of him as a graduate student uprooted his academic interests totally towards viscoelasticity and polymer mechanics. We have close personal and internet contact to today. Igor has become a major player in the international academic community of polymer work. He has been selected for 35 honors, including election to the U.S. National Academy of Engineering; he was also elected chairman of the Science Europe Scientific Committee on Engineering and Technology (ENGITECH), and co-chairman of the Science Europe Scientific Advisory Committee (both are offices of the European Federation in Brussels).

• von Bernstorff, Bernd***,** from TH Karlsruhe, Germany, 1979-80: He performed studies on *Crack Propagation in viscoelastic Materials under varying load histories***.** PhD: *Technische Universität Kassel*, Germany. He advanced to research director for polymers at BASF; and it was the combination of industrial problems and latest research results on polymers, promoted together with Igor Emri (as BASF consultant), whom he met at Caltech for a life-long friendship, that led to their fundamental patents governing multimodal molecular weight distributions in polymers to achieve special properties of polymers for achieving high energy absorption and high sound insulation. Their combined effort developed the most efficient sound absorption material, which, depending on the frequency range, exceeds material currently available on the world market by factors of 3 to 10. Bernd is retired from BASF and is currently Adjunct Professor at the University of Karlsruhe, Germany. We are still in close personal and technical contact today.

• Schmid, Nils*, from TH Karlsruhe, Germany, 1995: *The use of Scanning Microscopy for a Micromechanic Investigation of the Interphase in Fiber Composites.*

• Trom, Oliver*, from TH Karlsruhe, Germany, 1997: *Experimental Investigation of the Effect of relative Humidity on the Fracture Behavior of PMMA.*

• Wetzel, Bernd, from TH Karlsruhe, Germany, 1999: *Investigation of the Creep Behavior of Nonlinear Viscoelastic Simulant Materials for High Explosives.*

• Merkel. Jens*, from TH Karlsruhe, Germany, 2002: *A Contribution to Bulk Modulus Measurements of Polymers.*

• Gilliot, Anatole*, from Ecole Polytechnique Federal de Lausanne, 2003: *A method for Measuring the Bulk Modulus.*

Postdoctoral Fellows

• Cheung, Man (Max) Cheong, 1969-70. *The static and dynamic stability of clamped shallow circular arches.* Received his PhD with Chuck Babcock.
• Wnuk, Michael P., 1970. Professor at Western Michigan University; deceased 2014.
• Dietmann, Herbert, 1970. Professor at the *Technische Hochschule Stuttgart*, Germany); deceased, 2018.
• Panagiotacopulos, Nick D., 1975, JPL, *Work in Bioengineering.*
• Bloch, Ricardo, 1976. *Viscoelastic behavior of filled and unfilled elastomers in moderately large deformations.* PhD, chemical engineering. Work in bioengineering.
• Kenner, V.H., 1979-80. Professor at Ohio State; deceased 2020.
• Kim, Kyung-Suk, 1979-80. Professor, Brown University.
• Chai, Herzl, 1982. *The Growth of Impact Damage in Compressively Loaded Laminates.* Chuck Babcock's student. Professor, Tel Aviv University.
• Parvin, M., 1985–88. Student of Gordon Williams, Imperial College, London.
• Ravichandran, Guruswami,1986-87. PhD, Brown University. Currently Chair of Caltech's Division of Engineering and Applied Science.
• Vasudevan, Naresh, PhD, UCLA.
• Lee, Ouk S. Professor at Inha University, South Korea.
• Moran, Brian, 1988-90, PhD. Brown University. Professor, mechanical engineering, dean of graduate affairs at KAUST, Saudi Arabia.
• O'Dowd, Noel P.,1992, PhD, Brown University. Professor at the University of Limerick.
• Wei Tong, 1992, PhD, Brown University. Professor at Southern Methodist University.
• Zhong, Xiaoguang Allan, 1995, *Continuum dynamics of solid-solid phase transitions*. Caltech PhD. (Jim Knowles was his advisor.)
• Noe, Alfons, 1998. Professor of mechanical engineering, *Fachhochschule* Südwestfalen in Soest, Germany.
• Sundaram, Sairam, 2004, PhD, Brown University. *Pressure-stiffening of polymeric materials and implications for the detonation of plastic explosives.*
• Kinzler, Mathias, 2000–01, PhD, physics, University of Heidelberg, Germany. *Nanomechanics.*
• Jianheng Zhao**,** 2005-07, *Applicability of the time-temperature superposition principle in modeling dynamic response of a polyurea*.
• Maen Alkhader, 2011-12, *A New Shear-Compression Test for Determining the Pressure Influence on the Shear Response of Elastomers*.