THEODORE Y. WU
(Born 1924)

INTERVIEWED BY
SHIRLEY K. COHEN

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Subject area

Engineering and Applied Science

Abstract

An interview in three sessions, February-March 2002, with Theodore Y. Wu, professor of engineering science, emeritus, in the Division of Engineering and Applied Science. Dr. Wu was born in China and received his BSc from Chiao-Tung University (1946), his MS from Iowa State University (1948), and his PhD from Caltech (1952).

In this interview, he recalls his boyhood and tribulations during Japan's invasion of China in World War II, his emigration and matriculation at Iowa State in 1948, and his arrival at Caltech a year later. Recollections of H. S. Tsien, R. A. Millikan, Theodore von Kármán, Julian Cole. Works with Paco Lagerstrom's aeronautics group developing asymptotic perturbation method pioneered by Ludwig Prandtl. Joins faculty as a research fellow in 1952. Interest in hydrodynamics. Origins of the department of engineering science in the mid-1950s by Tsien, Milton Plesset, and Charles De Prima.

Interest in bioengineering, beginning in 1960; studies bird flight and fish locomotion. Discusses influence of G. I. Taylor and James Lighthill, and recalls his own work on flagellar and ciliary motion of microorganisms. Caltech's 1974 pioneering symposium on Swimming and Flying in Nature; new field of biofluiddynamics. Recollections of Y. C. (Burt) Fung.

Recalls his sabbatical, 1964-65, at University of Hamburg with Georg Weinblum. Joins Advisory Committee for Reactor Safeguards. Recollections of Caltech presidents Lee DuBridge and Marvin L. Goldberger. Visit to China in 1979.

Discusses his work, since 1996 retirement, on modeling of water waves; solitons and tsunamis. Concludes with comments on good relations between Chinese and Chinese American scientists and the flood of Chinese students to US for graduate work in late 1970s, after reestablishment of diplomatic relations.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH THEODORE Y. WU

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Theodore Y. Wu **by Shirley K. Cohen**

Pasadena, California

Session 1	February 21, 2002
Session 2	March 6, 2002
Session 3	March 12, 2002

Begin Tape 1, Side 1

COHEN: Good afternoon, Dr. Wu. Perhaps you could start by telling us a little bit about your parents and your boyhood and schooling in China.

WU: It is a great privilege indeed to come and tape this interview. And it gives me an excellent opportunity to look back on some of my life and work. So I appreciate this very much, and I wanted to thank you at the beginning.

COHEN: It's our pleasure, believe me.

WU: I came from a very small village on the southern bank of the Yangtze River, right in the middle between Shanghai and Nanking. My grandparents had just one house by itself, and they called it a village.

COHEN: You mean the village only had one house in it?

WU: Yes, that's about right. There were a few other houses, but somewhat scattered and at some distance away. Later on they became connected, with neighborhood development. My grandfather was a Chinese physician. His specialty was surgery, in the Chinese medicine way.

COHEN: What does that mean?

WU: Well, Huang Di, the legendary first emperor, started Chinese medicine in theory and clinical practice.[1] He wrote a book of two volumes on the fundamental principles and therapeutic medicine in the Chinese way. To be very brief, the essence of the principles is to treat the human body as a system. When a person becomes sick, if they find that the source of the pain is from some particular organ, then the idea is that you must consider the whole system; it might not be that part that is ill. This is very much a legend. But I'd be happy to bring the two volumes that he left. They are more than 3,000 years old—written and then updated by age-old followers, because the original writing and language would have been rather different. The volumes are still in print, and very comprehensive.

COHEN: Were these handed on from generation to generation?

WU: Yes. The herb medicine is also legendary, and acupuncture is only a small part of it.

COHEN: So your grandfather's training was just having these books, reading these books?

WU: Not quite sufficient. He probably learned his profession in a bigger town, although I doubt it was a big city. But he was so good and so skilled that the people would swarm to him, to our house, seeking his medical help.

COHEN: Can you remember that?

WU: Yes. I was then very young, about five or six. Sometimes I helped him spread molten pharmaceutical cream on gauze pads to make the so-called patches that would stick on inflamed skin. They were effective in drawing an abscess together under the skin for surgical removal. And I also remember him telling me, "Don't use a steel knife. Use a bamboo knife." His batting record for complete cure and recovery of patients was so high—

COHEN: That everybody came to seek him out.

[1] Wu footnote: According to legend, Huang Di (c. 2697-2597BC), elected by the ruling lords to be the imperial emperor after he conquered the barbarian invaders, called upon the sages to create characters for writing history and set up laws for ruling, in addition to his development of Chinese medicine. His queen, Luo Zu, allegedly introduced the technology of raising silkworms to produce silk threads for weaving.

WU: Yes, that's right.

COHEN: So he didn't have a hospital. He treated people right in your house?

WU: Yes, that's right. He would also go to the north side on the odd days and the south side on the even days to find people who were sick. And usually he would not take any remuneration. He'd say, "Don't pay me anything." So at the end of the year, oh, they all came. They'd say, "We didn't pay you anything," so they all brought gifts, and that was a great excitement. That was my grandfather. Then my father attended college and graduated with a degree in economics and finance.

COHEN: Where would that have been?

WU: His university was in Changsha, Hunan Province. It's very well known. After that, he served in the government, in the Department of Transportation. He managed to acquire for the government quite a few of the railroads built by foreign enterprises.

COHEN: What are you saying? That he nationalized the railroads?

WU: I recall hearing that the trade was regarded as fair and square to all involved.

COHEN: This would have been the early 1900s?

WU: Let me see—around 1928.

COHEN: So you come from a very educated family.

WU: You could say that.

COHEN: And how about your mother?

WU: My mother graduated from high school and majored, I think, in literature.

COHEN: Where would she have gone to high school? Was it a school for women?

WU: Yes, a school for women in Hangchow. My real mother passed away when I was less than a year of age, so my actual, functioning mother was my mother's younger sister.

COHEN: Your aunt.

WU: Yes.

COHEN: So your father married your aunt.

WU: That's right. And I'm the only child.

COHEN: But this was uncommon, because families were quite big in China at this time.

WU: Yes. I suppose, had my real mother been healthy…. Yes, it would have been quite different.

COHEN: You were still living in this little village, where your grandfather and your father were.

WU: Yes. We left when I was about nine years of age or so. We went north, because my father was helping to nationalize a railroad between Shijia-zhuang in Hebei Province and Taiyuan in Shanxi Province. It was built by a French enterprise.

COHEN: Was that all peaceful?

WU: Very peaceful and with excellent cooperation between the original and the new owners. We lived in a modern compound, a charming, all-walled-in town. It was built very much like a French village.

COHEN: Each compound would have one family in it?

WU: That's right. And they'd each have a large lot—oh, it was like an acre or so per residence.

The gardens were well-wooded, with trees brought over from France, or somewhere—many tall sycamore trees, as I remember.

COHEN: So you lived in a compound where the French people had lived when they were building the railroad.

WU: That's right, yes.

COHEN: Were there French people there at the time?

WU: When I was studying in that city, there were quite a few. Then later on they gradually returned to France. We made very good friendships. They'd return repeatedly, and they'd reciprocate.

COHEN: Sometimes when you hear about nationalization, it often doesn't go so smoothly. But with the French it was OK?

WU: Yes. That could be a good role model for many other things, too.

COHEN: But now we must be coming almost to the time when the Japanese came.

WU: That's right, yes. This Lugou Qiao—Lugou Bridge—incident [in 1937]. That's where they claimed that two Japanese soldiers were missing. They couldn't find them, and they said, "It must be you." So that was how they started this war, and it lasted for eight years.

COHEN: You were quite young at the time.

WU: Yes. I had just finished my first year of junior high school in Peking—now called Beijing.

COHEN: So your family had moved to Beijing by this time.

WU: That's right. It was because my father became ill, so he had to be put in a sanatorium—a German-built sanatorium, staffed with German doctors and facilities. For quite a few years my

father was off duty but was still being consulted. He headed the division of financial affairs of that railroad. So when the Japanese came, I took the train—the one before the last. My father took the very last train leaving Beijing, and we returned to our hometown.

COHEN: You went back because the Japanese were coming into Beijing?

WU: That's right. That's the invasion. I went south, to the university from which my father was graduated. He told me to see his teacher, and also the president—

COHEN: You had finished your high school in Beijing?

WU: No, I had finished just the first year in junior high. The second year, I went to Changsha.

COHEN: Which was the university?

WU: No, no. It was an educational institution with three levels—university, high school, and elementary school, with a very good, very famous educator, Dr. Yuan-Tan Hu, assuming the presidency then.

COHEN: I see. So you just went there and presented yourself?

WU: Yes—of course with a plea that I would be very happy to take an entrance examination. I said, "If you think I'm good enough, I would be most appreciative, and if I'm not good enough, then we will see what we can do." They gave me an exam, and they said, "Oh! You are so good we'll take you in."

COHEN: Did you have to pay for this education? I mean, how did you go?

WU: Not much. In Chinese education, it used to be that we paid very little. In the *suhunan*—that's the second year of high school—I stayed in the dorm. My parents, my family, were thousands of miles away. And I made friends. And their food was so gorgeous! I shouldn't take the time to tell you this. [Laughter]

COHEN: [Laughter] No, that's important. So you were not suffering, in that part of China, from the invasion of the Japanese.

WU: Well, when their bombers were coming over our heads, we had to go to the air-raid tunnels. Every time we heard the siren, we all went, and tried to conserve our resources. We'd take books, and so forth. [Laughter]

COHEN: Now, was it only boys at this school, or were there girls also?

WU: This was a very good boys' school. And then another educator said, "Well, you can't just have the boys well educated." So he established a girls-only school, just opposite ours. [Laughter]

COHEN: Across the road?

WU: Yes, that's right. A good road, and on one side was the boys' school and on the other side the girls' school. Very outstanding schools. The students would try to date—well, it was sort of…. In China, there was a custom, very conservative. We would take a very, let's say, low-key approach. You know, sometimes we would say a few words, and that was already a love story [laughter] to others. Do you see what I mean?

COHEN: Yes.

WU: Nevertheless, the two schools had a very good relationship. And later on, the students got to develop into good officers, good entrepreneurs, or good engineers—yet they still cherished this nice relationship. And this was not unique. In other universities, in other places, too—in the north, like in Beijing—

COHEN: There was a school for girls also.

WU: That's right, there were.

COHEN: In all the years you were there, did you ever go home to see your family?

WU: Yes. In the third year of my junior high my father decided—because of his prolonged sickness, he said, "No." No longer could we afford to keep running away from the battlefield. So we went back to Shanghai, where there were still the English and French so-called extraordinary territories set up, which the Japanese then were not touching. We went back to Shanghai through Hong Kong, and I continued my education there.

COHEN: It must have been very difficult to get in, because everybody would have wanted to do that.

WU: Very difficult indeed. And long after that, when I still had half a year to finish in high school, the Japanese claimed they had a new government in Nanking and it was going to take over all the schools in Shanghai. And we thought, No, we don't want to do that! So many of us students left, and we walked through all the battlefields to reach free China.

COHEN: You mean your class, your group of students, left but not your parents?

WU: Not my parents. My parents and I said goodbye one night, and then my schoolmates and I disappeared into the darkness. There were about twenty of us. We were on the road, oh, nearly half a year. We brought some things, some money, and then it was all spent.

COHEN: So you walked from Shanghai to where?

WU: To the wartime capital, Chungking, in Szechuan Province.

COHEN: That's a long walk.

WU: It was a very long walk—well over 1,000 miles, counting the detours we took for our safety. We all caught some sickness or other, especially when we got down south, to Fujian Province. That's on the coast, near Taiwan, and it was then known to have one outstanding

disease in each city—the plague in this city, malaria in that city.[2] Well, it's a long story, but we got to Chungking, having missed at least two months of that semester. We got there in late November of 1942.

COHEN: Did people on the way give you something to eat? How could you do this? I mean, nobody had money.

WU: We did everything we could. You know, we'd brought some relatively valuable items, and we tried to sell them. We knew that it would not be a very easy journey.

COHEN: How old were you?

WU: Seventeen. And when we got to Chungking, they said, "Well, you come back next year. We've already been in session for months." I said, "What do you mean, next year? You know, that could affect the rest of my life. We will subject ourselves to your most severe exam. Give us the hardest test problems and see if we can do it." So, with the Ministry of Education's blessing, we took the exam and we came out very well. They said, "OK, we'll admit you." That's how I started my university training, at Chaio-Tung University, which had moved to the wartime capital from Shanghai. All those years, the mentorship in all the different places, all different teachers, was so moving. They were so dedicated! We were very appreciative, grateful, and we pledged ourselves to continue that—to give the same to other students.

COHEN: So at this point you dedicated yourself to becoming an educator.

WU: Yes. And at that time, because we saw that all those bombers were Japanese, I thought, Why couldn't *we* have some, to protect our country, our people? So I determined to enter aeronautics.

COHEN: It was the Japanese airplanes flying overhead that inspired you.

WU: That's right. That's how it went. All our teachers poured their hearts into their work and

[2] Wu footnote: Only recently did I happen to read a newspaper report about evidence of bacterial warfare carried on by the Japanese Army, with the time and place coinciding with our personal experience.

tried to give us the best teaching. And they were good. For example, our English teacher said, "This is one of the best speeches. This was made by President Abraham Lincoln at Gettysburg. I will explain each word"—because words like "consecrated" and so forth were foreign to us. [Laughter] He would explain, and then he would say, "It's so beautiful, you'd better commit it to memory so you can pick up this way of writing." You know, "Four score and seven years ago our fathers brought forth on this continent a new nation conceived in liberty…," and so on. We did that type of recital every week.

COHEN: How big was this university? How many students were there?

WU: Again, we had a rather small class. Our class in high school was less than thirty. That's not large, compared with some of the larger high schools. And my Chaio-Tung University class had twelve.

COHEN: So they were not educating very many people.

WU: Not very many people.

COHEN: It was the top of the top of the top. So you finished university there. And was the war over by that time?

WU: No. Most of the time—actually, for almost three years—there was still heavy fighting. We were so grateful to have the so-called Flying Tiger squadron.

COHEN: Claire Chennault?

WU: General Chennault, yes. They were stationed in Chungking and started to defend our airspace from the Japanese invasion. And when your spirits are high, no matter what life is like…. Each dorm could take about thirty-six students. They were from different disciplines, like electrical engineering, aeronautics, and so forth. We all lived together. Very few students had a girlfriend, but if we heard that someone had a date, then different people would offer, "You

take my shirt. This is the best…."

COHEN: I see. So this was your family.

WU: Yes, it was like a family.

COHEN: You were there for how many years?

WU: Three and a half years in Chungking. Sometime around mid-May, 1945, the Japanese surrendered unconditionally. A classmate of mine was very resourceful. He said, "Well, I got a gunboat"—on the Yangtze River, not in the open ocean, so it wasn't too big. And he said, "It's at our disposal. How about we take it and put some of the Chinese sampans—small Chinese boats—with it, and we'll move back to Shanghai?" So that's what we did.

COHEN: All of you went on these boats?

WU: Not all, only the seniors and some juniors. We put three boats tied up on the port side, and eight boats on the starboard side, and moved it all down.

COHEN: The university from Chungking moved back to Shanghai.

WU: That's right, we moved back to Shanghai. That was in the winter of 1945. We got there just in time for New Year's Day.

COHEN: And so you saw your family again there?

WU: Yes, right. And in '46, I graduated. Then I stayed on one year, teaching. I was a lecturer. I did that for one year, and then I came to this country, arriving in San Francisco in 1948.

COHEN: I see. Now, what's interesting to me is, with all this chaos and war, the university continued. Not for many people, but for some.

WU: That's right. I would attribute that smooth continuation, even under the Japanese bombing,

first of all to our teachers. They were simply great!

COHEN: You got a good education.

WU: Yes. You know, I still have some of those class notes, and they are written on the most coarse and darkest brown paper—which just shows how poor the materials were. But it didn't matter. We still learned the things that we should, and we learned them well. So lack of some of the material resources…. I started to feel that it is not that vital, [laughter] if your spirit is there. The high spirit is needed. We all tried to study hard, and we were not as closed as here. The students could discuss so openly. We were supposed to be on our own. But then we did discuss to some extent, and that was very subtle. And usually it was the extra problems that we'd find so challenging that we started to discuss.

COHEN: But your main work you were supposed to do by yourself.

WU: That's right, yes, by oneself. I think the culture is, in a sense, such that a learned person has something to offer. He can use what he knows to help solve some of the problems that the society needs to solve. And the Chinese officials—you know, you know it so well—they all came from examinations. That was the system. The best were those who expressed their ideas the clearest, and so forth. They got to be appointed to certain positions, rather than, say, campaigning—you know, for elections. Elections have come only very recently in China. So all the officials would be looked up to with great respect by people in general.

COHEN: Because they had passed these difficult examinations.

WU: That's right. It's like in the Bible, when Solomon was looked up to for his great wisdom. He could never do wrong, and his decision was always the most intelligent way of resolving things to get the best result. I think there were some good things there that we could use here, too.

COHEN: What made you think of coming to the United States? How were you introduced to it?

WU: I wasn't too keen at first. Partly I thought China needed some of the young workers. Why should I leave? Sure, I would get more advanced knowledge, but then who would be doing this important work? So I took a very low-key approach. I said, "Well, let me work here another year."

COHEN: Many people that you were with were going to the United States?

WU: That's right, yes. I learned a lot of the ways of looking at the value of life and so forth from my father. He said, "OK. If you get to be much better prepared, you can increase your efficiency and ability manyfold and then serve your country. Otherwise it will just be so-so. Which one would you like?" So finally I came here.

COHEN: He encouraged you to go.

WU: Yes. And then, not being very eager to pick up the application forms, I missed all these important schools, like Caltech and Harvard. They offered admission once a year. So I had to wait another year, and I thought, Well, that's too long, so just forget it. One of my friends was at Iowa State University. He knew about my situation, and he wrote me. He said, "Here is the application form. We admit students four times a year, every quarter. You apply, send it to me, and I'll hand it in. That's the quickest way." And he talked me into it. [Laughter] So I went to Iowa State University—called Iowa State College at that time.

COHEN: Where did you get the money to make this trip?

WU: We must give the credit to the Chinese government. The government said, "Once you get admitted, then we will give you the special rate for foreign exchange." So we didn't have to pay the market rate.

COHEN: At that time, the government was encouraging people to go abroad and study?

WU: Yes, that's right. Once I got to Iowa State, I got to know quite a few good professors. I took mostly courses in physics and mathematics.

COHEN: Iowa is a good school.

WU: Yes, it's a good school, a very good school. I think one of Robert Oppenheimer's best students was teaching there, Professor John Carlson. I learned quantum mechanics from him. And then Professor Julian Knipp, from whom I learned the kinetic theory of gases and statistical mechanics.

COHEN: Did you feel prepared for these courses?

WU: Yes, I was about ready.

COHEN: How was your English at this time?

WU: It was all right. Conversational—I picked it up gradually. Written English was no problem. So these subjects turned out to be very helpful after I came to Caltech. Caltech was of great attraction to me, because—

COHEN: You knew about it already?

WU: Oh, yes. We heard all about it in China. Some of the leading Chinese scholars were either studying here at Caltech or had already graduated and were working, or serving the country—leading the country. Like H. S. [Hsue-Shen] Tsien. People would say, "*He* is at Caltech, so they've got to be good." And Pei-Yuan Zhou. Both Tsien (now Qian) and Zhou (formerly Chow) were accorded a Distinguished Alumnus Award.

COHEN: How long did you stay at Iowa State?

WU: Oh, just three quarters. I enrolled there in January 1948 and finished my master's that December.

COHEN: And meanwhile you applied to go to Caltech.

WU: That's right. Once I got to Iowa State, I finished the first quarter and then I told some of

my professors that I wanted to apply to Caltech for PhD study. They all said, "You ought to go there." They wrote me letters of recommendation and so on, and I was accepted by aeronautics. I was so happy! Those years are memorable not only for the education I received and the ability of self-learning that I gained but also for the people I met.

COHEN: People were friendly here. Were the people friendly to you in Iowa?

WU: Oh, yes. That Midwest—there is just no comparison; they are so friendly. And also they are true to each other. At Caltech, my first impression was that the people seemed to be so busy by themselves. And I thought, Well, that's natural, because they are doing the best work. [Laughter] We stayed in Old Dorm. Do you know Old Dorm? The Old Dorm was for graduate school students only. It was next to Thomas [Franklin Thomas Laboratory of Engineering], the engineering building. It was two stories high and roofed with thatch.

COHEN: This was 1949, so you would have been with many returning veterans.

WU: That's right, especially in aeronautics, because the navy and the air force sent their officers here for engineering degrees.

So, about three quarters of the Old Dorm was dormitory rooms and one quarter downstairs was a cafeteria called the Greasy Spoon—the name we still use.

COHEN: Yes, only it's now Chandler Dining Hall.

WU: That's right—with good food nevertheless.

COHEN: Were there very many Chinese people here then?

WU: No. We counted less than twenty—something like eighteen or nineteen. Now, you know, there are so many—it's very, very different. Tsien and Fung were the two professors—Y. C. [Yuan-Cheng] Fung and H. S. Tsien. Fung was a great loss to us; I'll tell you a little bit about that later. I didn't get to know Tsien right away, because, you know, I was in aeronautics and Tsien was in jet propulsion. He was the Goddard Professor. They appointed two Goddard

professors: one at Caltech—and Tsien was chosen and returned from MIT to Caltech, his alma mater—and the other one was given to Princeton. So the first year there were only two. Well, I just wanted to tell you that, in eating lunch—

COHEN: Wait. I'm going to turn this over. [Tape ends]

Begin Tape 1, Side 2

WU: At lunch one day, all of a sudden, there was a distinguished professor sitting opposite me.

COHEN: This was in the Greasy Spoon?

WU: Yes, in the old Greasy Spoon. Well, the conversation was ordinary—you know, "Where do you come from?" and so on. Then he introduced himself to me. He said, "I am Robert Millikan." I stood up, and he said, "No, no, sit down." [Laughter]

COHEN: So he was eating at the Greasy Spoon.

WU: Yes, that's right. So that shows—that's like a great mentor, you know. He was interested in some of the students that he didn't even know so well—to see how they were settled, to see how they were doing. And he asked all these related questions: "Where is your family? Is your family all right?" Well, I told him some, but then I told him I was greatly moved that he came here and had an interest to ask me these questions.

COHEN: And what did he say?

WU: Oh, he smiled. He said, "This is a small institute. Caltech is small, and we are privileged enough to take the time to do these things," and so forth and so on. That was really nice, really nice. It was touching. And in aeronautics I was taken in by Paco A. Lagerstrom [professor of applied mathematics, d. 1989]. He put me in an office with Julian Cole. Julian Cole was a very distinguished scholar, who finished his PhD in aeronautics in 1949, the year I got here. But to share his office—oh, nothing can be more effective than to learn from mind-to-mind interaction.

See how he thinks and see how he made mistakes and see how he spotted his mistakes. He'd immediately say, "Ha! I made this mistake. We've got to correct it." And then, in a hurry, what approach would he take? I learned a lot from that group. And that group, later on, became the center of a new theoretical development called asymptotic perturbation method. The first case of this came from a German source—a very distinguished professor, Ludwig Prandtl. ["On the motion of a fluid with very small viscosity," given as a ten-minute presentation at the Third International Congress of Mathematicians in Heidelberg in 1904.—ed.] He had two ideas. One of them was the so-called boundary layer. A plate is put parallel to a stream of air, and the viscosity will make the air stick to the plate at zero velocity. Outside the boundary layer, the air is still very fast-moving. So there are two regions: The outer region hardly experiences this viscous effect but the air in the inner region is sticky. And he said, "Well, if we have two different kinds of flow, we should do our analysis differently for the two regions. First, forget about the viscosity in the boundary layer and assume it's so thin that it has zero thickness in order to evaluate the outer flow, which is assumed to be nonviscous, or inviscid. Then we have the velocity and pressure field of the outer flow determined by using the inviscid flow model. For the flow inside the layer, we take the viscous effects fully restored. Great! Thus, by a stroke of genius, the viscous boundary layer theory is made conceptually simple and elegant. All these beautiful ideas, once they are expressed, become so obvious—but to get the idea! No one did, until Prandtl.

So our group took up further development of the general theory and became the world center of this new developmental study. The real origin was Prandtl, but we took it and put in the mathematical details in a systematic and rigorous form. Julian Cole was an active member of the Lagerstrom group, and another important name was Saul Kaplun. Oh, that group was so interesting! I was in that group, and they would bring up all sorts of exciting, challenging problems to solve. We worked late nights. We would go out for a midnight snack and then come back again and work. We learned a lot.

COHEN: So it was exciting.

WU: Yes, that's right. The method developed by applying the so-called matched perturbation principle was widely used for that general class of problems in science and engineering

afterwards. Whenever we opened up the scientific journals in that period, we would find many papers using the so-called outer solution/inner solution method, because they had to match the two solution domains asymptotically—just like Prandtl's matching of inviscid outer flow with the inner solution in the boundary layer. I'm making this analogy to illustrate how the sowing of a gem seed can blossom and bear fruit in scientific gardens. So that was a great contribution.

COHEN: You got your degree quite quickly.

WU: Yes. I stayed three years, and I got my PhD [1952] magna cum laude. The people I met meant so much to me, too. I started to know Tsien, and Tsien would bring me to visit [Theodore] von Kármán [director of the Graduate Aeronautical Laboratories of the California Institute of Technology (GALCIT) 1930-1949] during the wintertime. In those years, von Kármán would always come back to Pasadena for the winter. And he would ask me all sorts of questions. Oh, he was so delightful and inspiring! They'd discuss and I would listen on the side. Then sometimes they'd say, "On this point, we could ask you."

I was privileged to hear this story from von Kármán himself. He said, "Robert Millikan, one time came to me…." Millikan was doing all the preparation to get the aircraft companies in this area to agree that they would stay. Then Caltech would train the very best engineers and scientists for them in return.

COHEN: Millikan got the airplane companies to stay here, and he would produce the engineers to work in them?

WU: Right. He said, "If you look at all the factors, this is the best base for the aircraft companies in the whole world. So don't leave. And if you don't leave, I'll promise to train the best engineers year after year, as you like." It was a mutual agreement. Then he asked Harry Bateman, a topnotch mathematician, and Paul Epstein, a great thermophysicist on the faculty—he said, "Please give me three names of the best theoretical aerodynamicists," since he had already talked to the Daniel Guggenheim Foundation about sponsorship. Daniel Guggenheim was a flyer who had a strong interest in aeronautics, and he felt that what was especially needed was the fundamental theory.

COHEN: Whom did Bateman and Epstein recommend?

WU: They recommended just one name, and that was Theodore von Kármán. They said, "If you write him, he will come." [Laughter] And this I heard directly from von Kármán himself. He told me, "One day I received a telegram asking me, 'When is the next ship to the States?' It was signed, 'Sincerely yours, Robert A. Millikan.' Well, I knew a little bit about that, and I found myself on the next ship to the states." [Laughter]

COHEN: It's interesting to me that Tsien was so friendly with you, because I hadn't seen him—I didn't know how friendly he was.

WU: He was actually a good mentor to me, too. He and von Kármán, before I came here, for several summers held a discussion class on water waves and naval architecture—so to speak, extending the aircraft industry and aeronautics to hydrodynamics and the shipping industry. This was because of their own interest and probably the navy's, too—the navy was very nice to that group. Tsien gave me some of the notes they had. So that started me on the way—you know, it was a stimulus, and indeed I started to see the type of phenomenon and also the nature of the challenge. Because the water surface, when disturbed, is moving freely all the time, its position is very important to determine the solution. So the solution that should depend on the position of the surface has to be nonlinear for strong waves, because two solutions with different free surfaces—when you add them up, you cannot get a single surface for the resultant motion. So the solution superposed won't be giving you new solutions for strong wave motions. I started to look into that. What was very nice was that Tsien gave me very subtle directions. He wouldn't say too much, just look at it to see if I was going in the right direction or not. I usually tried to exhaust all the things I could, until I was really at the wall, before going back to see these experts.

COHEN: They must have had a great respect for you, if they gave you these problems.

WU: We all have this natural desire to share knowledge, isn't that so? So if there is some student willing and deeply interested and devoted—if you give it to him, he will do it. This is the self-learning that I learned here, listening to all the other factors.

COHEN: So you wrote your thesis.

WU: That's right.

COHEN: And you were immediately asked to stay here. Of course, that's unusual, isn't it?

WU: Yes, that is right. I was debating whether to stay. But then the offer of opportunities was so good.

COHEN: We'll stop here and start next time with your life at Caltech as a professor.

WU: Good. [Tape ends]

THEODORE Y. WU

SESSION 2

March 6, 2002

Begin Tape 2, Side 1

COHEN: Welcome, Dr. Wu. All right. We've now given you the PhD degree at Caltech. And they refused to let you go.

WU: Yes. [Laughter]

COHEN: So you take it from there.

WU: I graduated in the class of 1952 in aeronautics. And I minored, I believe, in mathematics and physics. It was an excellent class; I'm very privileged to be among some very outstanding alumni.

COHEN: Are there any in particular that you want to mention?

WU: Oh, yes. Anatol Roshko [Theodore von Kármán Professor of Aeronautics, emeritus].

COHEN: He was in your class?

WU: He was in my class. Allan Acosta [Richard L. and Dorothy M. Hayman Professor of Mechanical Engineering, emeritus] was in my class. And I believe there were a few others who left Caltech but made very successful careers. We three continued here.

COHEN: Roshko, Acosta, and yourself. But how come? Were they building up the department and felt that you were too important a part of it? Because this is not the usual custom. Why do you think that happened?

WU: I think we have to make exceptions sometimes to our excellent principle—that we encourage our students to go elsewhere to develop and then perhaps return sometime in the

future. In some fields—for example, in aeronautics—it's hard to find really outstanding candidates who have expertise in both theory and experiment. Well, we looked all over, and I think Anatol was the truly outstanding one.

COHEN: I see. So it was such a new development, in some ways, that there weren't other places to get people from.

WU: That's right, yes. And in my case—I probably should fill in a little bit of the background. During my graduate student days, I was fortunate to know Professor H. S. Tsien. And he told me that during the winter months Professor von Kármán would be back in Pasadena. And that gave us the opportunity to visit him at his house. I learned that in the previous years von Kármán and Tsien had held a few rounds of study sessions on water waves and hydrodynamics—a general subject somewhat beyond aerodynamics, since aeronautics is GALCIT'S primary concern. So I was stimulated to look into that new field of hydrodynamics, and I became quite interested. I read some of the notes, and then I changed my field. My PhD thesis, actually, was on viscous heat-conducting flow of compressible fluids in the regime of subsonic and supersonic speeds ["On problems of heat conduction in a compressible fluid" (1952)], and this water wave theory was quite different. So I took on an interesting but then somewhat challenging problem, and that was hydrofoils of finite span—that is, you take the wing of an airplane and submerge it in the water to some depth, to lift the vehicle above the water's surface. That, I think, was one of the prevailing interests, and it was known to be quite difficult. But I took it on and made a very concentrated study, which somehow turned out to be a very interesting work. I published it, with a note of gratitude to Tsien and von Kármán for their encouragement and stimulation and for pointing out that new direction to me.

COHEN: Was Tsien still here then?

WU: He was still here for another three years.

COHEN: Were you affected at all by his political problems?

WU: Well, we didn't get into that. He was still very much concentrating on his work and on

being the Goddard chaired professor. He did his best leading the group, and the group was very inspired by his enthusiasm—like Frank Marble [Richard L. and Dorothy M. Hayman Professor of Mechanical Engineering and professor of jet propulsion, emeritus] and Duncan Rannie [Robert H. Goddard Professor of Jet Propulsion and professor of mechanical engineering, emeritus, d. 1988]. I was like an adopted member of that group, although I joined the hydrodynamics group. So for that early period of my postgraduate days, this free surface flow was the main field, and it had a few important branches, water waves being one. Then there were the so-called jet, cavity, and wake flows. Those flows are known to be strongly affected by viscous effects, as to make flow separations from body surface—for example, to make the jet flow. To appreciate why the viscous effect is so important in its role in manifesting the resulting phenomenon, let me tell a cute anecdote. I recall that Dick [Richard P.] Feynman [Richard Chace Tolman Professor of Theoretical Physics, d. 1988] used to ask his class a question. He'd say, "OK. You have a lawn sprinkler, and you turn on the water, and the sprinkler rotates while shooting out oblique jets of water. Now, suppose you put this same sprinkler in a swimming pool and start to suck the water out through it. Would the sprinkler still be rotating?" It would make a great difference—like day and night. Things like that made the category of separated flows very interesting. And then the wake, like that behind a moving automobile or an airplane. I was working in that particular area, and my first work seemed to attract some interest. I got to know an outstanding agency called ONR, Office of Naval Research.

COHEN: Yes, they were very good about funding science after the war.

WU: Right. Those were the good old days, the golden era.

COHEN: Let me go back a minute. This course you were taking with Feynman. This was the graduate physics course?

WU: I think it was a graduate course, but I cannot remember exactly which class. I was there when he asked us this question, and I thought, "Ah, that's a great teacher." A lawn sprinkler—everybody sees those, daily. But he just changed the direction of the water flow in the hose and we had to say what would happen.

COHEN: Did he solve the problem or just pose the problem?

WU: He was posing the problem to stimulate students to think. That was good teaching! Should I say something about ONR?

COHEN: Were they already supporting much of the work here?

WU: Yes, Caltech had excellent support from ONR. ONR was established in 1946, just after the war. It pioneered as the primary channel for federal support of science in this country.

COHEN: I know they supported many things.

WU: Yes. They knew the field, they knew who could do what. And they used to leave us quite alone. They wanted to know something of the subject—but otherwise it was not as it is now. At that time, there was a famous tale that ONR invited quite a few very famous and outstanding scientists from Europe right after the war to come to the US and continue their professional work—[John] von Neumann being one, and Richard Courant another. They said, "OK. Here is a building. If we called it the Richard Courant Institute of Mathematical Sciences, would you come?" And with von Neumann it was similar. So von Neumann came and went to work at the RAND Corporation.

COHEN: So the ONR didn't just support but really stimulated the science.

WU: That's right, they stimulated the science. It was under the leadership at that time—I think the chief scientist was Dr. Joachim Weyl. He is the son of the famous mathematician Hermann Weyl. He was outstanding. Under him, the mechanical branch was led by Phil Eisenberg and later by Ralph Cooper—a remarkably brilliant team.

COHEN: These were the people who oversaw the grants?

WU: That's right. I was a research fellow at Caltech for three years, and I worked in the group led by Professor Milton Plesset [professor emeritus of engineering science, d. 1991]—with other

people, like Frank Marble and H. S. Tsien, being my guiding stars, my mentors. So I enjoyed it. It was a great fortune for me to graduate from Caltech and know people like them and von Kármán and so forth. Von Kármán was always very keenly interested in who was doing what and what I was doing. After I told him my PhD thesis topic, he raised a few simple, exploring questions, and immediately I felt the force of that thinking power casting light on some course that I had not seen before. I saw a lot of promising new directions. That is the type of great educator, pointing out questions. And they are good listeners, too. They leave you alone—but not without inspiration. They really inspire you.

COHEN: Those people weren't here very long during your early years—they were soon gone.

WU: Yes.

COHEN: And then you became an assistant professor?

WU: After three years I became an assistant professor of applied mechanics [1955]. And I should mention this, too. About that time, when I began teaching, a department of engineering science was established here. I think the idea originated with Charlie De Prima. He was a professor—

COHEN: Of mathematics, no?

WU: That's right, but in the early fifties he was also in applied mechanics, as I recall. Later he changed to mathematics and stayed in mathematics. He came from the Courant Institute. In engineering science there was De Prima and H. S. Tsien, and then quite a few—to name a few, we had Milton Plesset, Lester Lees, then also Sheldon Friedlander, who left for UCLA [1978]. And later Noel Corngold, Harold Wayland, Gilbert McCann, and Derek Fender. These were the people who shared a conviction that engineering science was a field they defined as the frontier bordering engineering and the other various areas of science. They talked quite enthusiastically about information science—that was McCann and his group. And then biomechanics—Harold Wayland, Derek Fender, and so forth. Derek Fender, I think, was jointly appointed by biology and engineering. And nuclear science—that was Milton Plesset. He was a high-level consultant

to the Nuclear Regulatory Commission and he also chaired a subcommittee called ACRS, Advisory Committee on Reactor Safeguards.

COHEN: So these were people who were working on the edges of two things. They were not traditional engineers or traditional physicist/mathematicians.

WU: That's right. They wanted to develop this new frontier land. The interesting thing is that engineering science became the birthplace of new options. Soon applied mathematics was formed, and Charlie De Prima joined them and helped them to grow. And then, later on, applied physics started to grow and Noel Corngold moved over there. And then the biomechanics—we wanted to give it a good thrust. At that time there was a good group. Burt Fung [Yuan-Cheng B. Fung] was here. And Benjamin Zweifach, who was a physiologist. He was a visiting faculty, but he visited, oh, for a good long time. He moved his home here and worked very diligently on this new subject.

COHEN: Was there engineering science anywhere else?

WU: Yes. Some places are still going strong. For example, at UC San Diego they are still quite strong. They call the department AMES, Applied Mechanics and Engineering Sciences.

COHEN: But when you were forming this department here, did it exist anywhere else?

WU: Yes, I think so. It was an increasingly strong mainstream. Many places, like Harvard, also had a branch like that. And even now there are quite a few universities that still do.

COHEN: So that meant, in some sense, moving engineering from its traditional narrow—

WU: That's right, yes. It was very exciting. And it's even more exciting right now, if I may say so. [Laughter] I'll come to that later, OK? The multidisciplinary people really worked together; they talked to each other instead of, you know, working separately and occasionally talking.

COHEN: Do you think that was possible here because there were not so many people? The

people, of course, were all very good, but there weren't so many of them.

WU: Exactly. Now, this touches on a point. I think it is a most admirable principle, set by our three giants, the founders of our institute. I'm talking about Robert A. Millikan and George Ellery Hale and Arthur Amos Noyes. In our institute, if we decide not to do something, that's all right, but if we do do something, we excel. We do the very best we can and show the very best results. And being still small and adhering to this guiding principle—that our freshman class will be only 200 or so every year—fine, great! And in developing a new field, most of our colleagues are so able that they can almost master the research and the teaching singlehandedly—nearly. There are groups, of course, but we don't have a very rigid structure of so-called departments, with sixty professors in one engineering area.

COHEN: Well, let's get back to you now. Let's get to the part where you develop your interest in an exciting new field, and now you're an assistant professor. You must have had your own group, your own students. And you were working, I would guess, by yourself at this point.

WU: That's right. We have had very good sponsorship from the institute. They leave us alone to go and develop our research, in the spirit of academic freedom. First of all, I express my deepest appreciation and gratitude for that. The next is that we were fortunate to have outside recognition and sponsorship for new research. ONR in those years was our great source, providing the capacity for us to take on new students, outstanding students, and equipment and so forth. Then later on I changed my field, going more into bioengineering. And this was more or less related to fluid mechanics. We studied birds flying and then fish swimming. Those were macro scale.

COHEN: What put you onto this?

WU: I think the first step is to have curiosity.

COHEN: When did you start thinking about this? What year are we in now?

WU: 1960. And I have to attribute this to a few good leaders in the field. There was Sir

Geoffrey I. Taylor, G. I. Taylor. He first wrote a paper on the subject, and that inspired many people immediately.

COHEN: On the hydrodynamics of fish?

WU: The first paper he wrote was on microorganisms. The second one was on larger animals, sort of in between macro and micro. Elongated swimming animals, that's what he called them. And this inspired, oh, countless people. I don't know how many. In his early career, he was appointed Royal Society Professor. Someone delivered that appointment to him during his class, and he put down his chalk and said, "Well, now I'm following practice and obeying this tradition." So he stopped lecturing. [Laughter]

COHEN: Right in the middle?

WU: Well, that's the story, because a Royal Society Professor—their only responsibility is to give a public lecture once a year. [Laughter] He was a giant. And then Lighthill—Sir James Lighthill. He later became secretary of the Royal Society for a number of years. He was a very colorful person. He wrote a paper [Lighthill, M. J., "Note on the Swimming of Slender Fish," *Journal of Fluid Mechanics*, 9 (1960), pp. 305-317], and I became interested in this. I started to work on fish locomotion and bird flight. And then it happened that I published a paper, I think in the same year or the one following that, in 1960 or '61 [Wu, T. Y., "Swimming of a Waving Plate," *Journal of Fluid Mechanics*, 10 (1961), pp. 321-344]. Then our two groups started to interact.

COHEN: So you started to work with people in Cambridge? Did you personally work with G. I. Taylor, or were you just inspired by his writing?

WU: I was inspired by his writing. I paid a visit to him personally, and we had a very nice visit, chat. Lighthill actually was closer. He visited the campus at our invitation, which I arranged, and he delivered a series of lectures here. And I also visited Cambridge University—his group—as his guest. We were quite close. The scientific interest was a good match. And then later on, without telling each other, we started to work on microorganisms again. We worked on the so-

called flagellated and ciliated microorganisms.

COHEN: Now, this was not all theoretical? Did you have water tanks set up and watch these animals go?

WU: That's right. This was sponsored by NSF [National Science Foundation] now.

COHEN: Oh, so you went from ONR to NSF.

WU: Actually, it was sponsored jointly. Both of them were interested in this. We started first on the large animals. Well, fine, we could do the experiment in the hydrolab, just using the water tunnel. We actually put mechanical models as well as live specimens in there. We'd put them in there and then we'd put a screen—a quite loose screen, but it was too small for the fish to go through—upstream and downstream. Then we'd watch them. Then we tested and we recorded all the data we needed. Then, later on, when we started to move into the subject of flagellar and ciliary locomotion—you know, those are microorganisms like the cilia in our trachea and in our bioducts and so forth—I had two physiologists join my group, biology graduates from UCLA, Howard Winet and Tony [Anthony T. W.] Cheung. They stayed with us for a good period. And then also we had a good number of students. They all got interested in this.

COHEN: So you were cooperating with many of the biology people.

WU: That's right. We understood that it is important to really work together. We talked to each other, and we picked up not only their language but all the significances behind those terms. What do they mean? Can they lead us on, from step to step, in our own work? And we worked, oh, very congenially together. Then in 1974 we held an international meeting. Lighthill and other biologists—well, their biology school and also zoology school, their professors also came. And people came from all over the world. The symposium was called Swimming and Flying in Nature.

COHEN: Here at Caltech?

WU: It was here, yes. That was appreciated by our profession—many other universities had professors coming. They said, "Well, we don't have a paper yet this round to present, but we would like to come and listen to all the interesting discussions."

COHEN: So was that one of the first meetings in this field?

WU: That was the first meeting, that's right. The second meeting was held in Cambridge. Lighthill organized that, two years later, I think. And then it followed periodically. The third one was in Germany.

COHEN: Now, what was this field called?

WU: Lighthill coined the term. He called it biofluiddynamics—not separated, not hyphenated, all one word. And now it's being used. I don't know if the Oxford Dictionary has fully recognized it or not, but anyway it's in print. In hundreds of papers we all use this term—biofluiddynamics. It has an interior and exterior, and we started in the exterior—you know, like fish swimming and little single-celled microorganisms. We attracted interest and also we stimulated new interest in other places that were starting. However, we realized, "Well, a field would naturally be well nourished if there were an industry corresponding to it. Without that, look out." We had never failed to observe that. So in our group, the students all wanted to work on this, and I said, "Now, look. You have your future. Where are you going after you finish this PhD thesis?" We still have to look after our own, you know—livelihood and so forth. That's on the practical side. And I said, "Well, anytime the time is ripe, you can immediately come back, and you'll become an expert." So in preparation I always advised them to take the core courses and master that.

COHEN: Core engineering?

WU: Core engineering courses, so they would be diversified and broad-based—a so-called generalist rather than a specialist, though they would have their own specialties. They can talk about microorganisms backward and forward [laughter], but then they still master the core courses.

COHEN: Dr. Wu, explain something to me. You're saying that you had this marvelous field and it was very interesting and good science, but there was no real application. It was just of academic interest?

WU: Later there were applications. We were early. Now there are lots of applications. Now many of my students are, say, the head of a university—back in China—president of a university in Harbin, a chaired professor and the head of mechanical engineering at Hong Kong University, and so forth. So they all can go back to the fundamentals and still do very well.

COHEN: But then how about—

WU: The field? Now, if you don't mind, I will just touch on Burt Fung. Y. C. Fung. He was a professor here in aeronautics at that time. He had graduated from aeronautics summa cum laude, here at Caltech [1948]. And then he started to work in aeroelasticity—airplane wing vibrations and so forth. Then he became interested in biomechanics—partly because his mother was ill and he wanted to help, to find some ways of therapy. He and Harold Wayland worked together, and then came the need of forming a group. Now, the pull from UC San Diego was so strong that it was a loss to us.

COHEN: Fung went to UCSD?

WU: Right. He played a very important role in persuading a foundation, called the Whitaker Foundation, to lend their strong support to bioengineering. This foundation now is sponsoring work in that field at about twenty universities. And then, three years ago, this foundation conferred the title of model institute of bioengineering on UCSD.

COHEN: What practical value came out of this?

WU: The practical value now goes in many directions. His students now are teaching and also chairing bioengineering in many universities. They are looking for various ways to develop—say, prosthetics. Bioengineers understand biosystem functioning, and that is basic knowledge the medical schools were very happy to have. So their department, to begin with, was the entire

second floor of the medical school at UCSD. They cover all kinds of work on the frontier between biology and medicine and engineering and mathematics. You know, they need applied mathematics for high-speed computation, and now they are going into DNA—that is, molecular dynamics in a living system. There's a great difference between inanimate and animate systems at that scale, the molecular scale. I got interested in this during Burt Fung's eightieth-birthday symposium [June 1999]. I learned of possible new directions, and I gave a talk on the so-called exciton. An exciton is what the nonlinear waves are propagating along a DNA long-chain molecule in two modes: One is a phonon mode, and that is quantum dynamical; the other is called elastic mode, and that is mechanical. These two are far apart in the spectrum; they just go on by themselves, without changing or mutual interaction.

COHEN: These biological systems move, as an airplane moves, as a boat moves.

WU: That's right, yes. So engineering science tried to establish good foundations for assisting further research in the central area that can exhibit the underlying mechanisms of all these natural phenomena we wanted to understand. Without this very basic, strong foundation—namely, physics, mechanics, mathematics, applied mathematics, computational mathematics, engineering, aerodynamics, fluid dynamics, and so forth—

COHEN: So it all has to come together.

WU: That's right.

COHEN: You're still involved in doing all of this.

WU: Yes.

COHEN: Let's go back and take a look at what else you did at Caltech. Of course, you were part of an institution, with committees. Was there anything that particularly interested you?

WU: Oh, yes. Well, I think our institute is such a nice family. We all know each other, and we all appreciate being members of this family. So in this area I tried to work together with

colleagues here, as well as at other places. We organized an important symposium, and there were many, many others. I served on this so-called naval architecture. There was an international organization called ITTC, International Towing Tank Conference, meeting every three years, and all the shipbuilding and naval or marine sciences people would gather. I wasn't educated in that field, but somehow they took me in, my German friends. I was taken in as one of them, which I appreciated, and I got to know an outstanding leader. He built the School of Naval Architecture at the University of Hamburg. His name was Georg Weinblum, and he invited me to spend nine months visiting. That was the only sabbatical I ever took at Caltech.

COHEN: What year was that?

WU: That was 1964-1965. For that, I applied for a Guggenheim Fellowship, and I used that fellowship to good effect. I learned a lot there, and I also helped them.

COHEN: Did you take your family?

WU: Yes, we took our two children. They were very young—one was four, a boy named Fonda; the other was two and a half, our cute Melba. She immediately picked up the German kindergarten language—no problem. One weekend we drove to Lübeck—that's their port city. The Hanseatic League, you know, seven major cities. We were buying something and the clerk said, "Oh, you're from Hamburg." I said, "How would you know?" He said, "Your daughter—her accent." [Laughter] That year we traveled all around Europe.

COHEN: Was this your first trip to Europe?

WU: No, no. That's another story. I was taken into this professional society quite early. I graduated in 1952 and in 1956 ONR sponsored me to attend an ITTC meeting in Madrid. There were two of us from Caltech—Charlie De Prima and me. And Mrs. De Prima, Anne, went along, but Chin-Hua [Mrs. Wu] couldn't. She was studying chemistry at UCLA.

COHEN: I'm going to turn this tape over. [Tape ends]

Begin Tape 2, Side 2

WU: So that was my first trip, 1956—not long after the war. We stayed in Paris and London, and I did some shopping for my family. I could see the financial situation and the economics in those areas, and I highly appreciated that we had peace. All the countries under the Marshall Plan were coming around. So that was my first trip, and after that I made a few more. Then finally Weinblum said, "You come to Hamburg. We want you." He was excellent. His centennial commemorative symposium was in 1997 in Marseilles.

COHEN: Did you go back to China at all?

WU: Yes, my first visit to my homeland after my departure was in 1979.

COHEN: So you spent a year in Hamburg, and it was a very profitable year for you?

WU: Very profitable, yes. I told my colleagues, too, that we are just one of very few institutions acting as the host rather than being the visitors. If we could become more flexible and go elsewhere, we would see different benefits.

COHEN: What you are saying is that people don't take advantage of spending time somewhere else.

WU: That's right, yes. I think this is important.

COHEN: Did you learn German while you were there?

WU: [Laughter] I am not very linguistically equipped. I did manage to use the daily German. And the greetings I am quite fluent in. But for the lectures, no. And now, having not spoken German for some time—

COHEN: But you were able to manage.

WU: Yes, that's right.

COHEN: When you came back, did you start on other work or did you continue what you were doing? It takes a little while to get back into—

WU: Yes. Our work in biofluiddynamics was actually after that.

COHEN: Were you doing naval architecture then?

WU: That's right.

COHEN: So you were working with boats and ships?

WU: That's right. I served on some of the ITTC committees. We had an international meeting every three years, and I chaired one committee. I contributed what I could. They especially appreciated our approach—namely providing fundamental understanding of some of the things which were not easy to understand. I contributed also in China, where I was invited to give talks and so forth.

COHEN: So naval architecture was not your main field but a very interesting secondary field.

WU: Right. And then, for example, I was also asked to join a special committee of the Nuclear Regulatory Commission. It was called the ACRS, Advisory Committee on Reactor Safeguards. We studied all kinds of problems—anything about reactor safeguards. If, say, a pipe leaks—

COHEN: So this is hydrodynamics.

WU: That's right, yes. And very hard dynamics, because in those pipes the flow is never homogeneous. It has bubbles and chunks of, say, drops and so forth. There are two-phase flows.

COHEN: It sounds very difficult.

WU: That's right. You hear this jumping sound in your faucet? That could occur much worse in reactors. We talked about the physical model tests and so forth. And our country naturally was the leader in nuclear reactors. But then there were a few countries—like Japan. I think their

model was another scale. And then also in Germany and so forth.

COHEN: I see. I'm understanding how your work touches on almost everything—anything that moves. [Laughter] So then you came back after the sabbatic and you continued with your group here.

WU: That's right, yes. And also the new—for example, the multiple-phase flow. Now the multiple-phase flow is everywhere. Biosystems are invariably in multiple-phase.

COHEN: Were you involved with GALCIT then? Were you ever a part of GALCIT?

WU: Officially not. Unofficially, spiritually I was, always there. [Laughter] You know, I'm an alumnus and then I have my close colleagues. We'd talk. Nearly all of the professors in aeronautics I considered to be close to my interest. Like jet propulsion—Frank Marble and so forth.

COHEN: Have you ever had anything to do with the Jet Propulsion Laboratory, JPL?

WU: Yes. In scientific work not as close—but in general, yes. I always keep good contact, and I know what they are doing. And I appreciate their great contributions.

COHEN: Did you take any other sabbatic leaves?

WU: No.

COHEN: You didn't follow your own good advice.

WU: [Laughter] Partly that was because of the work demands, and also I had my interests here. Without Weinblum, I probably wouldn't have taken the first sabbatical and gone to Germany.

COHEN: So you became that kind of person, drawing other people here.

WU: Yes, right. [Laughter] You say it so nicely.

COHEN: How were you affected by the administration at Caltech. You served under several presidents. When you came, Millikan was still here.

WU: That's right, yes.

COHEN: And [Lee] DuBridge [Caltech's president 1946-1968] was president. How did you find that? Was he interested in your work? Did you ever speak with him?

WU: From time to time. Our contact wasn't really close enough to talk like this—that I could describe our interest, and how it was growing, and to what extent. I'm afraid that I cannot recall—

COHEN: Conversations.

WU: That's right. But we did meet on social occasions, and we did talk about some of these activities in general. But I still would regard that as good encouragement. And sometimes people say, "If you are left alone, that is a gift." [Laughter] In many ways, perhaps that's characteristic of our nice small family. We hosted the American Physical Society national meeting here, with Milton Plesset. And then also the national counterpart of the ITTC is ATTC, American Towing Tank Conference, which is at many universities, like MIT and Harvard and Michigan University and UC Berkeley, and the national laboratories in Rockville, a suburb of Washington. We don't have that department, but they would come. I organized and chaired one ATTC conference here. So there were all these professional societies, and we were active in bio things. People would ask me, "Why don't you come and tell us your story?" So I would go.

COHEN: OK. So you're saying that President DuBridge was friendly, but you can't say that you specifically spoke to him about what you were doing.

WU: Yes, more or less.

COHEN: And later [Marvin L. (Murph)] Goldberger [Caltech president 1978-1987] came.

WU: Yes, that's right, and Marvin was also very friendly. We talked about quite a few things. You know, "What is the future of your field, your option?" I wrote some of the memo, which I can show you—the memo about the future, you know, the concern about the future. Now, you know how small our department is. You are seeing—

COHEN: I'm seeing the whole department. OK. [Laughter] And then, of course, before Goldberger there was Harold Brown [Caltech president 1968-1977].

WU: We visited him in his office of the air force. [Laughter] Yes, before he came here. We happened to be in DC, so we paid a special courtesy visit.

COHEN: But, again, the main thing you're saying is they just let you be. When did you become emeritus, then?

WU: 1996, I think. And I'm still doing work as before. I feel very indebted to our institute for providing us with the opportunities to follow our own interest.

COHEN: OK. I'm going to ask you one thing, and then I'm going to wait till next time to talk about some other things. You went back to China for the first time in 1979?

WU: 1979, yes.

COHEN: OK. Did you still have family there?

WU: Well, I'm the only child. I may have told you that. But I have distant relatives and some not so distant—first, second, third cousins, and so forth. So we had a very nice visit. Shall we leave it for the next time? [Tape ends]

THEODORE Y. WU

SESSION 3

March 12, 2002

Begin Tape 3, Side 1

COHEN: Good afternoon, Dr. Wu. You wanted to talk about the work that you're doing now. And I was going to ask you about some of these lectures you've given in the last few years.

WU: Yes, very good. Thank you. I really appreciate this opportunity to express some of my appreciation for such a nice environment to continue the pursuance of research.

With all modesty, I am still very active after my retirement. As a matter of fact, the recent few works that I have succeeded in wrapping up is what I enjoyed almost the most. There are two works in two areas. The first one is to bring the modeling—theoretical model—of water waves under the most general condition. For example, there would be no limitation as to its height—and even so high that the waves start to break. And there is no limitation for the dispersion. By dispersion I mean waves of different lengths travel with different velocity. And then, again, that may differ from the velocity at which the energy is being transported. So, no limit on that. And on this, of course, we can find a very rich literature—in the hundred years or so ever since the great Sir George Stokes. However, most of these so-called nonlinear and fully nonlinear and fully dispersive waves are confined to an undisturbed water layer of uniform depth. So that is a very severe limitation. Now, in order to model some of the waves, currents, in water—for example, in a natural condition, such as near the coast—well, the water depth, the bathymetry, varies quite arbitrarily. So I put that factor in—that's a third important factor I put in. And I succeeded in making that work. Naturally, I must say that much of this couldn't have been successful without working with my good colleagues and very bright students, from whom I learned a lot. And putting all these together, we hope that we make a contribution to our profession—that they can use this for further valuable and practical applications.

COHEN: Do you still have students?

WU: I no longer have graduate students, but I do have some undergraduates. I have two seniors

right now who come from time to time. They are working in my laboratory [in the Thomas Laboratory of Engineering], and they are outstanding, very smart. I enjoy working with them. And I still have visiting associates. Distance is no longer a hindrance, because we use e-mail—communication is very simple. And we work across national boundaries, with groups in the Far East, Europe, and so forth. So we are carrying on. Here is a copy of this paper I mentioned.

COHEN: "A unified theory for modeling water waves." 2001. So this is your latest piece of work.

WU: It's almost the latest. I have others that follow this.

COHEN: It looks like a nice substantial piece of work. Now, I see that you've given a series of lectures in Israel.

WU: Yes, right.

COHEN: And in some other places. Was it on this work that you talked?

WU: Why did I succeed in doing this, whereas this approach had not been seen before? It's the idea that the classical work of the past hundred years—the basic equations, partial differential equations—is all nonlinear. So in order to take a perturbation approach, we don't want to go to the highest wave, the first step. So, very small waves—and that we called linear theory. Everything is linear. And when the amplitude is doubled, everything doubles, and so forth. The relationship is along a straight line. And that started from George Stokes, after 1850. He made several important contributions, until 1888. He was writing, summing up his collected works. Then he made another step forward. But all because of the idea of making the perturbation around the so-called equilibrium position—namely, when the water surface is flat. And when you do that expansion, then the algebra becomes so complicated. So one cannot get to the highest wave very easily, except for uniform depth. For nonuniform depth, it's hard. I'll just take a very short while to point out the difference. I started to put the flow variables right on the water surface, wherever it is, so that these variables depend only on two dimensions on the surface, rather than three dimensions in the physical space—that is, the vertical position is

absorbed in there. So however the water surfaces evolved into different positions, in different shapes, my variables will stay the same. And that simplifies. That is the key point.

This idea first came to me when I was preparing a lecture for the international centennial celebration of Georg Weinblum, which was held in 1997. He had a good influence on so many people, and they were all gathered together to attend a celebration of his life and work. And there I got this idea. And from then on, there have been quite a few celebrations, and each year I made some step. The second one was the celebration for Julian Cole, a distinguished alumnus of Caltech. He left our institute, oh, in the late sixties.

COHEN: Where did he go?

WU: He went to UCLA first and then to RPI, Rensselaer Polytechnic Institute. He was one of my mentors, too. So we celebrated his work, and I made another presentation. And then afterwards—oh, there are others. And then this came as a final version, in which I put everything together.

This picture is one of the nonlinear waves. Let me take a minute to explain this. You see, usually a ship's wave—in deep water, in the open ocean and so forth—is always trailing behind. Can we ever imagine that these waves under different circumstances—namely, in shallow water—will grow and outrace the ship and get to the front? It's hard to conceive, to imagine. Now we find in such a case that indeed the waves would grow and then outrace the boat and get to the forward area.

COHEN: You mean they would grow by themselves?

WU: By themselves, that's right.

COHEN: And go ahead of the ship?

WU: Yes. Now, this phenomenon was seen in 1842. A person by the name of John Scott Russell—very famous in physics and mathematics—was hired as a consultant for a steamship company. They wanted to put steam engines in canal ships, so he was hired because he was a good engineer, for consultation. Now, it happened that he was watching a canal boat, still drawn

by a pair of horses, and for some reason the horses stopped. So the boat stopped, too—but not the water around it. The water organized very vigorously. And this vigorous reorganization formed a wave, which was very gentle and quite tall. And it stretched straight across that canal and then raced ahead of the boat. This was just one wave. And John Scott Russell jumped on horseback and chased this wave, timing its speed and so forth. He found everything was quite uniform—finally he lost it in the thickets. When he came back, he said it was the happiest day of his life. And this phenomenon stimulated further theoretical development. We call it a solitary wave. In 1965, people found out much more profound things about that wave. It actually has a countable infinite number of conservation laws. And that is very, very rare. Now people call it a soliton.

COHEN: Is that the same as a tsunami?

WU: Almost. The tsunami is of that class. "Soliton" means the wave maintains its entity in size and shape—insofar as the medium is uniform—and never changes. A tsunami—if it is in a uniform layer of water, yes, it's a soliton. But because of varying depths—this bathymetry of the ocean water—it can amplify. For example, the 1946 Alaskan earthquake generated a series of tsunami waves, due to an abrupt uplift of the submerged seabed by meters over an area of about 200 kilometers by forty kilometers. When the lifted seawater runs off the raised seabed, it generates waves. Those sea waves then ran off like very long waves in shallow water in the open ocean—which is about four kilometers in mean depth in the Pacific—with their height nearly a meter or half a meter or so and their wavelength stretched to twenty kilometers. So these waves were very, very long, and only a meter or so in height. And when it arrived at the harbor of Hilo, Hawaii, it magnified to, oh, a fifty-five-foot-tall wave.

COHEN: I've seen the memorial.

WU: The first wave was a negative wave; it receded from the beach. And the people were so ignorant that they ran out on the beach to gather all those marine animals left behind by the rapidly falling sea. The fish left behind were jumping on the wet seabed and people tried to grab them, not knowing that in the next few minutes a fifty-some-foot wave would be—

COHEN: The wave was going to come back. So you then worked on the mathematics of this.

WU: Yes. Now, what we found is that—suppose that the canal boat John Scott Russell saw had not stopped, if there was more horsepower dragging it along. What would happen? He would have discovered everything we did here. I developed an excellent mathematical model, and we used that theoretical model to compute results. And it showed how the waves would grow. That's all due to the nonlinear feature. And because the taller waves go faster, that's why the waves outrace the boat, and periodically, one after another. There is a definite period in generating these forward-radiating waves, and this period is a functional, meaning that it involves an integral of the solution, so that you do not know it until you solve the problem. Here are some of the pictures.

COHEN: Ah, very nice—wonderful pictures, which we will put in the historic document room. So you use this material for the invited talks you've been giving. I see you gave one as recently as last year. You proceeded to do a good bit of traveling these last years. Where are you going this year?

WU: This coming May, I have been invited to attend the centennial celebration of Nanjing University, one of the oldest universities in China. It's an outstanding university and they are now inviting a few scientists to join them for that celebration. They have a forum, called the World's Leading Scientists Forum. I am pleased and honored to be invited. That's May 20th. And another university in that neighborhood also invited me, on May 31st. They are going to have an inauguration of a memorial building for Madame [Chien-Shiung] Wu. Madame Wu was a physicist at Columbia University. She did the experiment that verified the theoretical prediction of C. N. Yang and T. D. Lee. The latter two shared a Nobel Prize, but Madame Wu did the experiment. I remember sitting at a luncheon table in the Athenaeum, and we always asked the leading experimentalist—you know, like our leading physicist—"How is it now? Are you still doing this experiment?" And they'd say, "No, we stopped doing it. We only pick up our telephone and call Madame Wu, because we cannot really keep up with her pace and her progress." [Laughter] This was Willie [William A.] Fowler [professor of physics 1939-1982].

COHEN: Ah, OK. So Willie called Dr. Wu.

WU: That's right. [Laughter] So I was invited to join the inauguration honoring Madame Wu.

COHEN: Now, I see you have some position at one of the universities in China. Was this Hong Kong?

WU: Hong Kong, that's right, yes.

COHEN: And what obligations does that put on you?

WU: I have been giving lectures. For example, in 1979 I was invited by the Chinese Academy of Science to give a summer course.

COHEN: So you were there all summer.

WU: Yes. In '79, all summer.

COHEN: You're an honorary professor at Hong Kong University.

WU: That's right.

COHEN: Do you go with some regularity?

WU: Yes, it's quite regular. I have also been honorary professor at Xian Northwestern University and then also Harbin University of Engineering and Chungking University.

COHEN: How often do you go to China?

WU: Oh, from time to time. Not yearly. For example, this time I'll be in China in May. Then in August we have an international IUTAM [International Union of Theoretical and Applied Mechanics] symposium over in Shanghai, so I will be going there again. I have been invited to give a plenum lecture.

COHEN: In these last years, you've had really close ties with China—more so than in your

younger years.

WU: Yes, that's right. Last year I didn't give many lectures, because we had an invitation from the Chinese Academy of Sciences. They invited seven academicians—namely, members of the US and other academies. I'm in the US National Academy of Engineering. Two from the States, four from Taiwan, and one from Hong Kong.

COHEN: That's interesting. Even though the celebration is in China, they invite their colleagues from Taiwan.

WU: Yes, that's right.

COHEN: So on that level there's no big problem.

WU: That's right. In recent years, the academic interactions and exchanges have been very nice, very encouraging. I'm pleased and gratified to see this going on. We talk openly about scientific matters. Sometimes we tease each other [laughter], light and enjoyable things, so it's a good friendship. On that basis, everything seems optimistic.

COHEN: Oh, that's good, because when you read things in the newspaper you don't always have that impression.

So, Dr. Wu, let me now go a little bit to your interaction here in Pasadena, when we suddenly started having Chinese students at Caltech. And I'm talking now about twenty-five years ago.

WU: I think this began in 1978—near Christmas, December 12th or so—when President Carter reestablished diplomatic relations with China. Then in early '79 the Chinese Academy sent a delegation to visit our government, led by Professor Pei-Yuan Zhou. He was a 1928 physics PhD of Caltech, and he returned to China and spread his influence all over academia and the universities. He was the president of Beijing University—Peking University. And now he returned after so many years to Pasadena, which he left in, I suppose, 1946 or '47. During the war, he was working here on a committee for underwater physics—and naturally his first stop on

his return visit to the US was here. When Dr. Arnold Beckman learned about that, he said, "Oh, yes, my classmate is coming back!"

COHEN: Ah, he was in the same class.

WU: Yes, that's right. He received his chemistry PhD in 1928. I remember so vividly that we all gathered at the Athenaeum, and Dr. Beckman was sitting there looking out the window, and President Goldberger dashed out when the delegation arrived. Then after that stop, they went on to discuss with our State Department establishing a two-nation agreement for sending their graduate students and visiting scholars to the States. And in return they'd welcome all the graduate students and visiting scholars to China. So that started, and it gradually increased. And now you know how many there are. I think, by and large, these students are very seriously interested in learning. They work hard and they also—

COHEN: But they don't always want to go back to China, right?

WU: That's also right. Now, I think that is a matter that involves many personal, societal, and professional issues. By and large, what they say is that they want to keep on developing, to achieve some success before they return. Here the facilities are so good that they can hardly match them back home. So if they are still welcome—and usually they are—to stay on and so forth, they will prolong that period.

COHEN: So you think that's mostly it—in their minds, they think they will go back eventually.

WU: Right. And if we look at Taiwan, in '50 to maybe '75 or '80—in that period—it was the same situation. The students from Taiwan coming to the United States became professors and engineers here. Now many of them—probably most of them—return to Taiwan.

COHEN: But they come here because they have the facilities to work.

WU: That's right. The opportunities, and also there are many other attractive factors.

COHEN: We suddenly had a lot of Chinese students. And you were quite active, with other people, in getting them to come to this school in Pasadena.

WU: That's right. I think in the early time my colleagues—you know, so many colleagues and I—we were all feeling the same, with that enthusiasm to offer a helping hand. And three years after that 1979 visit, the vice-president of Tsinghua University—Tsinghua University is like their MIT—came. That was Zhang Wei. He asked me, "Do you know how many delegations from China have visited Caltech?" I said, "I have no idea. [Laughter] There were so many I lost count." There were so many, very many. And we did keep some of the visitors. One is my namesake, but we're not related—Professor De Ming Wu. He was the one who programmed our computational study of solitary waves and saw from the numerical results the picture of the forced generation of forward-radiating solitons that I mentioned earlier. He became well known as one of the discoverers of this remarkable phenomenon. And then returning to China, he was appointed to serve as president of Harbin University of Engineering.

COHEN: So you have liked being at Caltech?

WU: Oh, yes. I think it is such a fortune to me that in early times I heard about the young, distinguished Chinese leaders—you know, like Pei-Yuan Zhou and Tsien and quite a few others. Tsien was already very well known internationally as the Goddard Professor for his significant achievements. We also heard a lot about the other famed teachers—von Kármán and Bateman, and Richard Tolman, who wrote a book on statistical mechanics. Oh, we read it from start to finish.

COHEN: So in China all this was well known to you.

WU: Yes. And in the preface he says not a single page did he put down without a long chat with Robert Oppenheimer. And all these things were coming together. I said, "OK, I've got to come." So that led me to the beginning of my work. And now I still feel so gratified that I'm still, almost like before, keeping active and getting things done. That's most rewarding and gratifying.

COHEN: So they've made it comfortable for you to continue your work.

WU: You bet they did. Every meeting I go to, I still have new things to tell them. [Laughter] And I feel I'm very, very fortunate. [Tape ends]