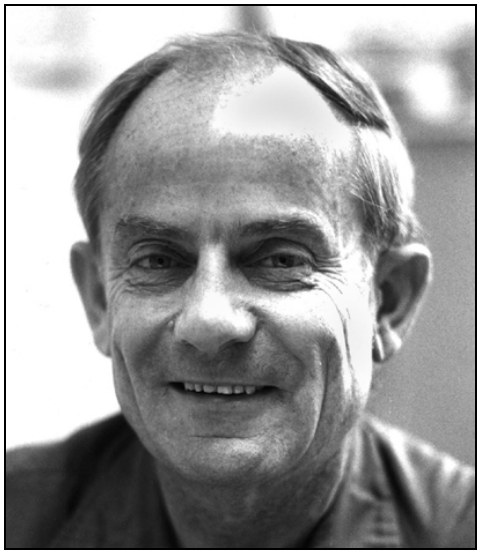

WHEELER J. NORTH
(1922–2002)

INTERVIEWED BY
SHELLEY ERWIN

October 6–December 1, 1998

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Engineering, environmental engineering, marine ecology

Abstract

Interview in 1998 with Wheeler North, professor of environmental science, emeritus, in the Division of Engineering and Applied Science. North received a BS in electrical engineering (1944) and biology (1950) from Caltech, and PhD (1953) from the University of California, Scripps Institution of Oceanography. His principal research interest is marine ecology, specifically the kelp beds off Southern California and the sea urchin population. He discusses effects of sewage outfalls and El Niño on kelp beds, the predations of sea urchins, and consulting for California's kelp-harvesting industry. Recalls diving and experiments with early scuba equipment as student at Caltech. At Scripps, he worked with group studying the physiology of diving. Postgraduate work with NSF fellowship at Cambridge. Returned to Scripps with fellowship from Rockefeller Foundation, worked on photoreception in *Metridium,* taught diving course. In 1963, he joined Jack McKee's environmental engineering science program at Caltech. Comments on early days of the program; his work at Caltech's Kerckhoff Marine Laboratory at Corona del Mar; growing interest in the environment in 1970s and popularity of his ecology course among undergraduates and graduate students in various disciplines. Discusses 1969 oil-

well blowout off Santa Barbara; contrast with *Tampico* oil spill off Baja in 1957. Discusses funding from National Science Foundation, after 1973 oil crisis, for kelp farms to produce biomass as an alternative fuel; later funding by General Electric, Department of Energy, and Gas Research Institute. Discusses kelp farming in China. Discusses work as consultant for Southern Cal Edison at San Onofre and Pacific Gas & Electric at Humboldt Bay and Diablo Canyon, on ecological effects of warm-water discharges from nuclear power plants. Discusses project funded by Electric Power Research Institute in early 1990s to reduce atmospheric CO_2 using marine biomass and hydrates.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

North, Wheeler J. Interview by Shelley Erwin. Pasadena, California, October 6–December 1, 1998. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_North_W

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Wheeler North has been studying California kelp since 1956. By 1970, when this picture was taken, he and fellow researchers had begun seeding coastal areas with embryo plants in an effort to restore the state's decimated kelp beds.

California Institute of Technology

Oral History Project

Interview with Wheeler J. North

by Shelley Erwin

Pasadena, California

Caltech Archives, 2001

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WHEELER J. NORTH

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Dr. Wheeler J. North By Shelley Erwin

Pasadena and Corona del Mar, California

Session 1	October 6, 1998
Session 2	October 13, 1998
Session 3	October 20, 1998
Session 4	November 4, 1998
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Erwin: Well, let's start at the beginning. You were born in Mexico.

North: Well, no. I was actually born in San Francisco [1922]. The family was living in Mexico. There were no hospitals or any kinds of facilities there, so my mother came out to San Francisco.

Erwin: And why San Francisco? Just out of curiosity. Why not San Diego or LA?

North: There was a method of helping with birth called the "golden sleep." To reduce the pain of childbirth. The outfit was operating in San Francisco, so that's why my mother went there.

Erwin: And then you were taken back to Mexico as a small baby?

North: Yes, immediately.

Erwin: Immediately? So she wasn't worried about medical care after the childbirth?

North: The medical care was what the family had available. There was a doctor in the general area of Zacatecas, where we were. Zacatecas was the largest city, and we were in a place called La Fe, which was maybe a half-hour drive over a dirt road, out a ways from the main city. And there was a doctor in Zacatecas, an American doctor. So if anybody got really seriously ill, he came out and ministered to them.

Erwin: Now, what had taken your family to Mexico at that time?

North: There was a British company called Union Corporation. It was a conglomerate; they owned a controlling stake in American Airlines, for example, and they had a lot of mining operations in Mexico. A geologist from the Mexican operation apparently felt that in La Fe there was a zinc and silver ore deposit that would be economic to mine. So they arranged to get a mining operation set up there which would involve, oh, at least a dozen engineer types and lots of labor, which was mainly drawn from the Mexican populace. My father was the engineer in charge of the mine. So they moved there about two years before my birth, and he started the operation. The decision to establish a mine there was a poor one, because after about seven years of operation the ore body petered out and the mine had to close down.

Erwin: So was that really the geologists' mistake? Or is that just one of the things that happens?

North: I'm not familiar enough with the geology or mining engineering to know. But the engineer in charge of all the Mexican operations was an uncle of mine. And when the mine at La Fe ran dry, they didn't make a suitable position available for my father. The family felt that probably what happened was that this uncle, to the people in Union Corporation, put the blame for the mine on my father. Ordinarily, you know, they would find him a position of equal

importance at another mine, but they just said, “Bye-bye.” Well, they offered him a caretaker position at La Fe, and he said, “No, that’s not satisfactory.”

Erwin: Let’s talk about your parents a little bit. Where had your father been educated? Was he a Californian?

North: He was born in Genoa, Ohio. I guess my grandfather North decided it would be more lucrative to go west. So my grandfather and a brother of his moved their families west. They started a farm in Kansas and gave up on that, and they went on to California. My father was still a baby, I guess, when they first started this process. And they located a very rich lime deposit in Tehachapi, California, and formed a company called the Summit Lime Company. It was apparently a very good decision. It was profitable, and the children of both families were raised on the money from the Summit Lime Company. My father and his brother were sent to Stanford for education. Both became mining engineers.

Erwin: I guess that was kind of in their blood at this point, you might say. They had been raised to it.

North: Yes, no doubt. My mother came from Wabash, Indiana. She was sent to Stanford, too, for an education. She was in chemistry. She and my father met and were married.

Erwin: What kind of chemistry was this at that time?

North: I don’t know. She graduated, but she never.... She said that in the first job that they got—which was near the Arizona border. I can’t remember the name of the settlement or the mine, but my father was employed there when he graduated from Stanford. And one of the duties was to run about a hundred assays every day of samples from the ore that was brought up. And my mother helped him enormously with that.

In those days it was a twelve-hour workday. And once a month, people who were working at night would shift with the people who were working in the day, so they weren't on the night shift continually. And at the changeover, it was a twenty-four-hour day.

Erwin: So when you shifted, you worked twenty-four hours straight? You didn't get any time off?

North: That's right.

Erwin: So up until the time that the Zacatecas mine failed, your father and mother had lived in these mining communities and done this kind of work all their married life.

North: Yes.

Erwin: And you say you had a brother who was killed in an accident. Do you want to talk about that?

North: Well, OK, yes. Before I was born, they had two children: a daughter and a boy. My mother said that their cribs were cardboard boxes—that was all that was available in the mining camp. But they sent my brother to a new boys' school—Thacher School, in Ojai. He was killed in an accident. His stirrups were being fixed. At Thacher, every boy had a horse, and you had to ride it regularly and take care of it. You had your own saddle and everything. And the stirrups on his saddle were being fixed, but he wanted to go riding with a friend. So they had a girth rope, that fastens the saddle on loosely, and he had his feet tucked in on either side. The horse shied, and the whole saddle and everything turned upside-down and the horse was very skittish and started running. My brother was dragged across the road. The other kid got the horse stopped, but my brother was unconscious. He lived for about twenty-four hours and died. The back of his head was beaten in from the horse's hoof hitting him. So my parents decided they wanted a replacement child, although by this time my mother was sort of at the limit of

childbearing capability. She was forty-four years old when I was born. As a result, growing up I had middle-aged parents.

When the mine ran dry, we moved to La Jolla, California. In La Jolla my father had an uncle, Uncle Wheeler [J.] Bailey. My father was Wheeler O. North, and he was named after this uncle. The uncle had a very successful building-construction operation—just in materials, not constructing the buildings. It was sort of the precursor of today's lumberyard, where they have everything. Uncle Wheeler had this in San Diego, and between jobs the family would go to La Jolla and my father would work for Uncle Wheeler. Uncle Wheeler had a large house. He was a bachelor. And he had a little guest house, so we lived in the guest house. We came out of Mexico in 1927, and in late 1928 my father began getting very ill. He didn't say anything about it. But in January it became obvious that he was becoming incapacitated. And finally he got a doctor and they took a blood sample. His liver was deteriorating. They gave him four days to live.

Erwin: Oh, my!

North: It was a terrible shock to my mother. She had lost her first son about ten years before, and now my father passed away, with very little advance notice. My father and mother had saved their money. They had, I guess, fairly substantial savings for those days; I don't know how much it was. It was probably a few tens of thousands of dollars. My father was very worried that the stock market was way too high, so he had very conservative investments. My mother didn't know anything about finances, so after he died she got ahold of Uncle Wheeler and said, "What should I do?" And he said, "Oh, these are terrible. You're getting very low interest rates." So he got her into investments with much higher interest rates but which were somewhat risky. My father died in January and the stock market crashed in October of '29. My mother lost practically everything.

Erwin: So these were hard times.

North: Uncle Wheeler felt very beholden, because it was his advice that did it. There was still a little money left, coming in from some of the investments. My uncle, I think, made a lot of beneficial loans and gifts and kept the family going.

Erwin: Now, was he someone that you could look to as a sort of father-figure?

North: Yes, very much so.

Erwin: He played a role in your growing up—more than just a financial role? Did he advise you on school?

North: Well, my father died in '29 and Uncle Wheeler died in 1935.

Erwin: So you really were still a kid.

North: Yes. I was still a kid. But my uncle left [money]. My mother was very comfortable from then on, and there was some gift bequest to me. And my mother used that to send me to Thacher School, because both she and my father were very impressed with Thacher School. They didn't feel that my brother's accident was any fault of the school. People at Thacher School were astonished when the application came from the Norths.

Erwin: Yes. Just going back a little bit in time, do you remember Mexico? Do you remember your life as a small child there? You said in your outline that you only spoke Spanish when you lived there.

North: Yes. Typically in these Mexican camps there were lots of women available as servants. So I remember we probably had five or six servants. Essentially they raised me. So that's how I learned the language; it was with the servants.

Erwin: So you were truly bilingual, probably.

North: I knew a little English.

Erwin: Didn't you speak English with your mother?

North: I honestly don't remember. I remember I had a hard time knowing enough English to communicate when we moved to the US.

Erwin: You were more comfortable in Spanish.

North: Much more comfortable, yes.

Erwin: So you kept the Spanish, I assume.

North: My vocabulary has deteriorated enormously, but I have a very good accent. So I go down to Baja California, and I start talking a little bit. I can ask where things are and so forth. They hear me talking and they think I'm perfect in Spanish. They start rattling away, and I have to say, "Pardon, pardon, no entiendo"—I don't understand.

Erwin: Interesting.

North: My father always carried a gun. In Mexico during that time, Pancho Villa was raising hell. And one of the head engineers at another mine was kidnapped for ransom. The company paid the ransom. He wasn't harmed, but it was a nasty experience, of course. It was like the Wild West frontier in the movies.

Erwin: Were you, as a child, allowed to just run free and do what you wanted to?

North: The camp was, I think, encircled with a fence. I was allowed to go anywhere. There were about eight homes with people—mothers and children—like ours. I would go to the various homes. From about three on, I'd play with the other children.

Erwin: So you moved back to La Jolla. Other than the language part, did you feel like you had lost something?

North: No. I really didn't quite understand why we had left. We had very good family friends who lived in Los Angeles, and they had a boy who was a little older than I, but not that much. And we became fast friends; he was my best childhood friend. So no, I really don't recall having any problems. It was a lovely place where we were in La Jolla. The little guest house was right on the edge of the cliffs. You could look out and see the ocean, and there were wonderful tide pools when the tide went out.

Erwin: And so you spent all your time just exploring the shore?

North: Yes, and I quickly began bringing home stuff. I had a museum. I guess I had read about museums and decided to start one for myself.

Erwin: And this was filled mostly with sea animals?

North: Seashells. And there were large fossil deposits. I had lots of fossilized shells in the collection. I remember there was one oyster or clam shell, I guess, about that big, and my mother insisted we use that for an ashtray. I remember at La Jolla elementary school I was asked to give a talk with a demonstration, so I brought much of my museum in. And when I got to that shell I said, "This is an oyster shell. My mother uses it for an ashtray." My mother blew her stack when she found out about it, because women smoking in those days was considered improper. You did
it surreptitiously if you were a woman.

Erwin: Right. So this museum was clearly the beginning of your scientific pursuits.

North: Initially I was a biologist.

Erwin: So you went to elementary school in La Jolla?

North: Yes.

Erwin: And then to the Thacher School when you were in high school or junior high?

North: After my father died, we visited Mexico City, because we had lots of friends in Mexico, and quite a few of them were in Mexico City. They were shocked because of my father's death, and they asked my mother to come down and spend some time. So we went down. I think it was the summer of '31. And my sister met a young Canadian man and was fascinated. So they got married the next year. My mother was anxious for me to practice my Spanish so that I wouldn't lose it. So every summer we'd go down [to Mexico City] and spend it with my sister and her husband.

Erwin: So she was a good bit older than you, from the family chronology.

North: Yes, she was fifteen years older. They persuaded us to stay down for a full year at around 1934, I think it was. So I went to an institution called the American School. There were lots of Americans with children there, so they had this English-speaking school. I remember lots of English people and Canadians. It was a very cosmopolitan area, with a number of non-Spanish-speaking groups there—enough to have this school. I guess around Christmastime, an aunt who lived in La Jolla with my uncle—in a separate apartment, though—wrote Mother a letter that Uncle Wheeler was getting awfully feeble. So in February we came out. I was put in a small boys' school called Bonita School, out in the backland of San Diego County. And about three weeks after that, my uncle died. My mother felt I should start getting away from her apron strings. So my uncle had agreed to fund me at this boys' school. Then he died. It took

part of a year, or more than a year, to get my uncle's estate completely settled. And, as I said, there was enough money there to send me to Thacher School. By then I was in the ninth grade. They bundled me off to Thacher.

Erwin: And I suppose it was considered that this would be the best thing for you—that since you had only your mother, it would be better as a boy to go to a boarding school.

North: Yes, I presume so. My mother just said, "You'll go."

Erwin: Yes, well, one didn't argue with one's parents.

North: She was tyrannical, dictatorial.

Erwin: But as far as you were concerned, it was OK? You had a good time?

North: Oh, no. I hated to leave La Jolla. I just looked forward to vacation, when I could come back to La Jolla. I liked the ocean very much, and these schools were all inland.

Erwin: Was it a good education, as far as you could tell, at the Thacher School?

North: Yes. That was another reason. *Fortune* magazine had put out an edition on the top boys' boarding schools. They picked ten schools. Nine of them, including Exeter, were on the East Coast, and the only one west of the Mississippi was Thacher. A big factor was the quality of the education.

Erwin: Right.

North: So Thacher was very highly regarded.

Erwin: Well, I think it still is. It's still in existence.

North: Yes. But I had no trouble there. I got good grades, except I had a lot of trouble with algebra. I had no trouble understanding how to do a problem, but I'd be sloppy at adding and subtracting and so forth. The kid who sat next to me used to copy my homework. But he was very careful: he'd get the answer right. I'd get it wrong. But he wouldn't understand how to do it. [Laughter] Then I got into geometry and I loved that. And I loved science. I had chemistry and physics and good courses in mathematics. My mother started beating on me to improve my grades in algebra and be more careful, and I did. I really had no trouble understanding the content of the courses; it just came to me like that. I didn't have any trouble at all. We had a two-hour study hall each night, and I'd do the homework quickly, and then I'd read a book or do I-don't-know-what else. So my study habits were sloppy. I got to Caltech and it was like night and day. They didn't have any scheduled two hours for a study period at night. There was lots of distraction. I very quickly fell behind. I had all kinds of trouble with math and with physics. At about the middle of the first term, I was telling this to my mother and she arranged for me to have a tutor, and that saved my life.

Erwin: It did. Well, now, was Caltech where you wanted to go?

North: Well, Thacher had catalogs from the various schools, but the information on Caltech was very sketchy. There was a good bit of information in the catalog from MIT. So my choice was MIT.

Erwin: Just because you understood more what that would be like?

North: Yes.

Erwin: Had other people from the Thacher School gone to Caltech that you knew of?

North: Yes, well, one person who went there was one of the sons of Robert A. Millikan.

Erwin: Oh, is that right?

North: But that was before me. We didn't overlap.

Erwin: That was too long ago. Do you know which son it was? Was it the aeronautical engineer, Clark Millikan?

North: No, I don't think it was. I think it was the other. I can't remember his name.

Erwin: Glenn or Max—there were three sons, actually. [Glenn attended the Thacher School—ed.]

North: I see. But at any rate, my mother said, "If you flunk out, just think of the expense that you'll have, coming home from MIT. If you go to Caltech, you can drive home." [Laughter] So I applied to Stanford, in case I wasn't accepted at Caltech. I was accepted there, and I was accepted at Caltech. But the faculty at Thacher felt I was too immature. I was born in January, which is sort of midterm. And they just had one-year classes at Thacher. That is, they didn't start a new class in the middle of the academic year.

Erwin: So you were young for your class.

North: I was young for my class. Well, they wrote my mother that it would be a tragedy to send me to Caltech in my present immaturity, and they gave me a scholarship to fund me for another year. So I took an extra year and took courses that I wouldn't have otherwise taken: German and—

Erwin: Well, that sounds very enlightened, actually.

North: I was just crushed, but my mother said, "You're going back there for another year," so I did.

Erwin: So it was five years?

North: Well, I entered in the tenth grade, so I was there four years. Ten, eleven, twelve, and then the postgraduate year.

Erwin: And you entered Caltech then in 1940. Is that right?

North: I think so, yes. The reason I'm fairly sure of that is that when the Japanese bombed Pearl Harbor, I was a sophomore.

Erwin: Yes. Everybody remembers that.

North: I entered in September of '40, and then in September of '41 I'd have been a sophomore.

Erwin: So things were not too easy for you at Caltech. Who were some of the professors, whether in your freshman year or other years, that you might like to mention?

North: The math and physics was primarily—math was totally taught by graduate students. The class was split up into sections of about twenty students each, and instruction was by a graduate student. I never met any of the math professors. In physics it was the same. About once a week we'd get a lecture, usually from Earnest Watson. They rotated a little bit, but not much. I remember there was one lecture by Robert Millikan.

Erwin: So you did hear Millikan early on?

North: Yes. I'm not sure, but I think it was my freshman year.

Erwin: He would have been fairly elderly by this time.

North: Yes, he was. Once a week the Millikans would invite one of the sections of the freshman class to their house for dinner on Sunday evening. That way we got to meet Dr. Millikan and his wife in a cordial atmosphere. It was very nice. The wife was very cordial to me, because I was a Thacher boy.

Erwin: Oh, so she honored that connection. That was nice. One of their sons died, and maybe it was the one who went to the Thacher School. [It was.—ed.]

North: I'm not sure.

Erwin: I'll have to look it up.

North: Anyway, I think there were two lectures a week in chemistry, and it was abysmal.

Erwin: Oh, it was not well done?

North: I was oriented toward chemistry when I entered Caltech, and the freshman chemistry course just wiped out that desire for me. There were still a few teachers—old men—hanging on, who had been teachers when Caltech was Throop Institute. And one of the oldest was the guy who taught freshman chemistry. And he'd just drone on and on.

Erwin: Can you mention the name?

North: I can't remember the name.

Erwin: Was it Lucas?

North: No, it wasn't. Howard Lucas I had later. He was excellent.

Erwin: The only other one I can think of was William Lacey.

North: I never met him. It wasn't Lacey. But at any rate, I think in the second term Linus Pauling, who was head of the Chemistry Division, slipped in and sat down and listened to a lecture. And he was just appalled at the quality of the freshman lectures. So he demoted the instructor from Chem 1, and each week we'd have different professors from the division.

Erwin: That must have been an improvement. Anything would have been an improvement.

North: Oh, yes. The lectures were very good.

Erwin: So you didn't lose your interest in chemistry entirely?

North: Well, by then I had decided I wanted to be an electrical engineer.

Erwin: Oh, OK. Now, was that because of any course or any professor?

North: During one of my summers in Mexico, I got ahold of a book at a friend's house on elementary physics, and Millikan was one of the authors. It was very good, very nicely written. And I was fascinated by the chapters on electricity and magnetism. When I went back to La Jolla, I'd get batteries and do certain experiments. I'd wind my own coils to make a magnetic field, and on and on. I built a little motor that whirled around. So when I entered Caltech, as I say, I thought I might like to be in science. I had enjoyed very much the chemistry lectures I'd had at Thacher, but I also had in the back of my mind that electrical engineering would be good. And all the men in my family were engineers of one kind or another. So when I decided to become an electrical engineer, they were all very happy.

Erwin: I see. Now, who was teaching electrical engineering then? [Royal] Sorensen wasn't here anymore, was he?

North: Oh, yes. He was head of the department. There were five or six professors. One of them became head of JPL [Jet Propulsion Laboratory].

Erwin: [William H.] Pickering?

North: Pickering, yes. He was a great lecturer. I had trouble understanding the subject—

Erwin: Oh, I thought you were going to say that he had an accent, too, but not that much of one.

North: Oh, no.

Erwin: He was a New Zealander, I know.

North: I see. No, I didn't have any trouble understanding Pickering. Sorensen I understood perfectly, and I did well in his class. Then there were laboratories; I didn't do very well in the laboratories. I didn't have the grasp of math and physics that I should have. When we got out of the basics after two years and into our own subjects, I really was very weak in math and physics. So I struggled through electrical engineering. I got poor grades. If it had been a five-year course, I'd have probably flunked out. But in four years I got my diploma.

Erwin: OK, so you got your bachelor's degree?

North: Yes, I got my bachelor's degree in engineering in 1944.

Erwin: Let's take a little pause. [Tape is turned off]

Begin Tape 1, Side 2

North: At any rate, the US had entered the World War after the Japanese bombed Pearl Harbor. So the draft came into being for getting young men into the services. All the different

services—the navy, the army, and that's all there was in those days—had programs whereby they would, if you joined the program, allow you to finish your education. But you had to enlist to become part of the program. So I enlisted in the electronics training group of the Army Signal Corps in December of '42. It turned out that that was the only group that allowed you to finish your education.

Erwin: Had you known that in advance?

North: I lucked out. My best friend at Caltech had joined an air corps program, and he was taken at the end of his sophomore year. The people who joined the navy were put into uniform and most of them were allowed to continue with their education at Caltech. And the navy essentially took over the campus. We few civilian students had to vacate the student houses and go out and rent rooms on our own.

Erwin: Now, was this what was called the navy V-12 program?

North: Yes, the V-12 program.

Erwin: But you were not a part of that?

North: No, I was in electrical engineering. There were two areas you could specialize in: electronics was one, and generating electrical power was the other.

Erwin: Were you taking classes then, in a normal kind of way?

North: Yes. Up to my last year, we had the usual quarter system. Well, they changed to a semester system. So in 1943-1944, my senior year, we studied all through summer, and instead of graduating in June I graduated, I think, in February or March.

Erwin: That was to get people through faster, no doubt.

North: Yes.

Erwin: And you mentioned in your outline that you had a job as an air-raid warden.

North: Yes.

Erwin: Did all the students have these sorts of duties?

North: I think this was volunteer. They had blackouts. You had to have your windows covered with curtains, and you weren't allowed to turn on headlights. You could turn on parking lights and drive that way. But they envisioned that if a heavy Japanese attack occurred, electric power might go out, in which case, at all the busy corners, the signal lights wouldn't work. So you had to have a person standing there directing traffic. So that was my duty, and my station was California and Hill streets. One night a Japanese submarine made it to the West Coast and dropped a few shells on—I can't remember the name, but just after you get to Ventura, there's a small place there, before Carpinteria. It's not Summerland; I can't remember the name of it. There were a few oil wells there. So the Japanese shelled it, and immediately the word went out: "Everybody go to your stations." At about 1:00 a.m. I was there with a half-covered flashlight. I think maybe one car went by in an hour. [Laughter] And then they said, "All clear," so I went back to bed.

Erwin: In the meantime you were just pursuing your studies more or less as usual?

North: Yes.

Erwin: So the campus was different, but you could still go on with your life?

North: Let's see. We entered the war in 1941. In 1942 you wouldn't know the difference on campus, except that people were disappearing as services pulled them in. And then starting in '43 was the navy V-12 program. And at that point you could really tell the difference.

Erwin: So after you graduated, you went on active duty?

North: Yes. About a week after I graduated, I had orders to report to Fort MacArthur, in San Pedro. A couple of days before I was due to report, I had this awful sore lump under my ear; I had mumps. So the doctor got in contact with the army and they deferred my orders for, I think it was, six weeks or two months—a fair amount of time. The mumps eventually went away, but you can contract mumps on one side and then get it on the other side. So they gave me an extension just in case that happened. So I reported to Fort MacArthur about two months later. There were probably fifteen of us in this electronics training group. The other fourteen had already gone through.

Erwin: So you had lost all your pals.

North: I lost all my pals, yes.

Erwin: Was this basic training, or something beyond basic training?

North: Well, this was just to issue you army clothing. You sat around doing odd jobs.

Erwin: Were you an officer already?

North: Oh, no. I was a private. If they didn't assign you to anything, they'd take you out and make you learn to march in unison. You'd spend all day marching. You also got KP—kitchen police—duty, and that was awful. I was at Fort MacArthur about a week. And about five days into the week, I got the job of being latrine orderly, which meant that I went in in the morning

and cleaned up the whole place after everybody else was out marching or doing whatever. And I got the rest of the day off. That was the best job in the whole army.

Erwin: I see. So even though it didn't sound glorious, it actually had its advantages.

North: And two days later, orders came through for me to go to a Signal Corps camp in Missouri—Camp Crowder—for basic training. Initially the Army Signal Corps had said, "When you graduate, you'll be an officer." Then that offer was withdrawn, apparently, and you had to go through basic training and Officers Candidate School.

Erwin: How long did that take?

North: Basic training was six weeks. And then I was shipped to Fort Monmouth, on the East Coast.

Erwin: That's in New Jersey, isn't it?

North: Yes. I sat around there for the summer. And I think I entered Officers Candidate School in September, which would be three months. And I barely got through that. You'd get demerits for not shaving properly or if your bed wasn't made properly and so forth. I got ten demerits during the three-month period.

Erwin: Was that the maximum?

North: Well, if you got ten demerits, you had to appear before a board of three officers for an interview. Based on that interview, you were either out or you went on. They decided that they liked me. [Laughter] I had fairly good grades in the courses that they gave us, and I was a Caltech graduate, so I made a good appearance, I guess. They said, "What about this? You have ten demerits." And I said, "Well, the first nine were in the first month. The last one was

in the third month. So I think that shows some improvement." And they agreed with me. [Laughter]

Erwin: So was it your housekeeping skills that were lacking?

North: Well, this was the last one: Every Friday we had to stand inspection. You had to be absolutely perfect. Your boots were shined and you were shaved and everything. Your rifle was cleaned. While you were standing for inspection, you were supposed to look straight ahead. The officer was a first lieutenant with a silver bar. He was inspecting the line in front of me. And the silver bar reflected the sun into my eye, and it just caught me by surprise. I moved my eyes to get out of this bright sunlight, and he saw it. So I got a demerit for it.

Erwin: Well, that was bad luck then.

North: That was bad luck. There were never any instructions as to how they liked the bed made or how your footlocker should be arranged. You asked the guys who had been there a month or two months how to do it. Sometimes they wouldn't tell you something, and your shoes would be in the wrong place.

Erwin: Oh, dear!

North: It was very hectic.

Erwin: Well, you got through that.

North: And then, at the end of the three months, you spent a month in the field doing simulated setting up of communications for headquarters. And about every nine or ten hours, you'd get a message: "Move to such-and-such a place." During each week, there would be a group of about ten, and they'd rotate who was in charge. So during a week, you'd have ten people having been

in charge for part of the day. It just got tremendously hectic. I remember at the end of the first week I had gone fifty-four hours without sleep.

Erwin: Oh, my!

North: Friday we came home, and the only thing I could think of was, Go to bed! They had stacked beds. I was in the top. I started to climb into my bed, and I went to sleep and fell back as I was climbing into bed. That's how sleepy I was.

But at any rate, I got a bad grade on one of them. So as we were nearing the end, suddenly an officer came by and said, "Come with me." And I was before the board again. And they asked me which one of the four commands I had that I didn't do well. And I knew which it was, which helped. They said, "Well, what was the matter?" Several times I kept saying, "The officers just said I wasn't doing a good job." What had happened was that at that point I was in charge of a message center. Everybody was hurting for sleep, so I sent everybody off to the headquarters tent, where you could get some sleep, except one person and me. So we didn't handle the number of messages that was expected. So I drew a bad grade for that one.

Erwin: I see. But you were humane.

North: I was very humane. [Laughter] So they liked me the second time, too, and they took me back out into the field. You know, they never told you whether you passed or not; you just went back to what you were doing, and if you didn't disappear it meant that they had passed you. So the end of the fieldwork came and I was still there, so I decided I'd passed. We came in and they gave us a week's vacation. I managed to hitch rides on military planes back to California. By then I had my brand-new uniform and everything that I had on order. By the time we got back, why, they had the official paperwork done, saying that I was now an officer.

Erwin: You give the date in your outline as December of '44. You had no sense that the war was ending?

North: That's right. In fact, it was just about then that the Germans made a huge effort to push the Allied forces back into the ocean. For a little bit, it looked like they might succeed. It was called the Battle of the Bulge, because the lines bulged in, where four German armies were pushing hard. But they held, and from then on it was—

Erwin: The Allies pushed them back.

North: Yes. The Germans fought till the end, but we kept going farther and farther, and pretty soon we got to Berlin.

Erwin: Right. And that was still a few months away.

North: And at that point, against the Japanese, we were still trying to take one island after another. After I got my officer's appointment, then they gave us about six months of training. By then it was summer. I was given two weeks off.

Erwin: Now we're in 1945?

North: We're in '45. I remember the news of VE Day—Victory in Europe—came on a Friday, I think. So we celebrated over the weekend.

Erwin: Right. That was May.

North: Yes. But we were still at war with Japan. So I reported at the end of my vacation to Camp Beale, in California, up near Sacramento. They were staging getting all the troops over into the Philippines for the invasion of Japan. I was there for three weeks. And the day before I shipped out, they dropped the atom bomb.

Erwin: And you, of course, like everyone else, had no idea about this, right?

North: Yes, that's right. Nobody knew, and nobody that I knew knew.

Erwin: Everybody was geared for an invasion.

North: Yes. Even when we shipped out, we still thought we would be in the invasion. But halfway across, the Japanese sued for peace. By the time I got to the Philippines, we knew the war was over. There were just thousands of men in the Philippines, and enlisted men were just sitting in fields, out in the open. It would rain, night would come, and three times a day a truck would come by and throw K-rations out. As officers, we were given a big tent, which was a tremendous improvement over the enlisted men. I really felt sorry for them.

Erwin: So this was an invasion force that then had no invasion to do, so they were just sitting there?

North: Yes, that's right. And men were still coming in from stateside. They couldn't get it turned off overnight. A lot of the people who were in the Philippines and on the various islands had been at war for two or three years. So instead of us invading Japan, we were going to replace these people. So the people who had seen battle could go home, and we would be the army of occupation. That was the policy.

Erwin: You said that after the Philippines you went to Nagoya, Japan.

North: Yes. I was put on an LST—a landing ship tank—which was a big amphibious vessel with a huge hold to carry tanks. Off we went. We didn't know where we were going. They just said, "Get on board." About three or four days out, we ran into a typhoon. We were in a convoy; there were lots of ships. This typhoon almost blew us back to where we had been. But everyone survived. I think it took about two weeks to get up to Japan. Our boat put in at Nagoya, and we unloaded. I found out I was attached to the 25th Division. We were at headquarters. The chiefs met everybody at the dock and took us off. I went to a big compound that had been reserved for the 25th signal company.

Erwin: What month was this?

North: Let's see. I think it was October.

Erwin: I see. So it was only two months after the bombing.

North: Yes.

Erwin: So when you landed, the headquarters was already established and the occupation was essentially established?

North: I don't know what had gone ahead. But I know that when I got there, the signal company was operating and there was a headquarters. A division usually has three regiments. I don't know if the regiments were in place or if they were on the way. But the headquarters was in the process of being established. The signal group people are usually the first, because they set up
the communications.

Erwin: So you were in the vanguard, then, of the occupation.

North: Yes, I was in the vanguard. It was interesting: if you went out and walked on the street, the Japanese—particularly the women—would see you coming and just disappear.

Erwin: I wondered about that—what it was like being on their soil. No one talked to you?

North: The concept was that when you were invaded, the troops rape and pillage and murder and everything else. Of course, in the US Army, anything that you could possibly think of doing you'd end up facing a court martial for. It was very, very strict. But at any rate, for about the first month, as I say, the Japanese would just disappear as you walked down the street, and

behind you they'd appear again. And they realized that we were well-behaved, so about the second month we were objects of curiosity. You'd see this group of Japanese people moving down the street and in the middle would be a couple of GIs with everybody looking at them. It was a very funny feeling. After about two months, everything settled down normally. The Japanese would go about their business and we'd go about our business.

Erwin: This is my own bad geography: Were you close to the Hiroshima or Nagasaki site?

North: Oh, no. I think we were probably about 200 miles north of Hiroshima. One of my good friends—Bill Trimble was his name; he and I graduated together; I think he was one of the ETG [electronics training group] people, too—at any rate, we had been together during six months of training at Fort Monmouth after we went to OCS. He ended up in Hiroshima. We would correspond. He'd just say that it was sickening. The people who survived were decimated by the blast. People would have burns on them. It was just a very demoralizing thing.

Erwin: Right. But I suppose at the time the attitude was that it was justified, that this had done its job of saving American lives.

North: Well, yes. We didn't think of it in moral terms. What we thought of was, it saved our skins. And the Japanese were an enemy. So anything that you did to them, sort of within the rules of warfare, was completely moral. Hiroshima, the region around the bomb, was just flat; there was nothing left. We were eventually moved from Nagoya to Osaka. We'd see chimneys and maybe two walls of a house or a building standing. It wasn't flat, like it was in Hiroshima. But none of those places was habitable.

Erwin: So this was the effect of the bomb?

North: This was the effect of ordinary bombings. I hear people saying that the atomic bomb was immoral. What they don't realize is that the conventional bombing was just as bad, in terms of destruction and lives lost. There were over 200,000 lives lost in one night in Tokyo

from a heavy bombing episode. Fires would start. The house might survive, but the people would be trapped in the fire with no way of getting away from it. So the whole thing was immoral. That's the way life was.

Erwin: That's right. They were trading one atrocity for another atrocity.

North: Yes. That's the way I look at it now. If you had seen these cities, the carnage and damage was as bad as Hiroshima. It just wasn't as violent. It wasn't all in one enormous blast. But the end result was pretty much the same, in terms of the number of lives lost and the rebuilding that had to be done. The Japanese were very industrious. You'd see these people go into these ruins and get the bricks that were good and pile them up and—

Erwin: Start over.

North: They were trying to start over and rebuild. In the Philippines, the cities and towns were damaged as much, but the people weren't industrious. They'd find a piece of galvanized tin and put some poles up and sit there living under it. You didn't see the entire populace trying to rebuild the community.

Erwin: How long were you in Japan?

North: Let's see. I shipped over in August and was there a full year. So I left Japan at the end of August, 1946. [Tape is turned off]

WHEELER J. NORTH

Session 2

October 13, 1998

Begin Tape 2, Side 1

North: After the war, I had about a three-month vacation, and then I started looking for a job. I had a couple of medium-attractive jobs offered: Bell Telephone and I can't remember the name of the other one—it was an engineering job in Utah. But then I got an offer from the US Navy Electronics Lab in San Diego, and that sounded very attractive. So I took that and spent about a year and a half with them. It was interesting work all the time; most of it was classified. It involved quite a bit of time at sea, which I didn't too much care for.

Erwin: Why was that?

North: It was boring. But I had the opportunity to set up some stations on a couple of the Channel Islands. I got to roam these two islands and that was lots of fun. I found an Indian graveyard on one of them—that was San Nicolas Island. I was on San Nicolas and Santa Cruz.

Erwin: Were they inhabited then?

North: They both had navy bases on them. And Santa Cruz had—I can't remember the name of the man who owned it, but he had a ranch at the other end of the island from where the navy was operating. All in all, it was interesting work. But I had lots of spare time on my hands, particularly on the boats, so I did quite a bit of reading in biology. Of course, I had always been interested in biology from my youth in La Jolla in the tide pools. It just seemed like a very fascinating field. As a result of my tour of duty with the army, I had five years of education coming under the GI Bill of Rights. They would pay the tuition and a small monthly fee to keep a student going.

Erwin: That was a big deal for a lot of people.

North: Yes, it was. It was good for the country and it was good for the people who had been in the service. It was a wonderful thing. I decided to switch from electrical engineering to biology, keeping in the back of my mind that my experience in electronics might help me—if I went into physiology, for instance. So I explored [the idea] with the biology department at Caltech, of whether they would admit me. Professor Albert Tyler was in charge of recruiting undergraduates. And he seemed very encouraging. So he said I could be admitted. And I don't think he had looked at my grades from my engineering career, because they were lousy. [Laughter] So let's see. It was in the fall of 1948 that I returned to Caltech. It was a big change from the leisure hours with the navy.

Erwin: Right, plus you were an undergraduate again.

North: I was still an undergraduate, but I was technically also a graduate, so I couldn't get accommodations in the student houses. But they had a building called the Old Dorm.

Erwin: Yes, I've seen pictures of it.

North: That housed graduate students, and I got in there. In the Old Dorm, the graduate students slept in the top and underneath was a little cafeteria called the Greasy Spoon. So I had my meals in the Greasy Spoon and I had a little room upstairs. So it was very nice. I had more to do with the professors in biology. The classes were very small.

Erwin: When you say you had more to do, do you mean in comparison to your previous period?

North: In comparison to electrical engineering, yes. In my early undergraduate years, the classes were very large. The electrical engineering class was about twenty to thirty students. So I was now in classes of the order of, say, three to five students. And we interacted with the professors to a much greater extent, which was a real pleasure.

Erwin: And who were the professors now?

North: This was biology, so I had to take a lot of chemistry, because in engineering you didn't go beyond one year in chemistry. So I had Ernest Swift's analytical chemistry course, which was a tough course. In chemistry there were lots of students—from thirty to fifty students. And then I had organic chemistry from Howard Lucas, and that was a wonderful course. I really enjoyed it. And then they had physical chemistry; the name of the professor escapes me. But the physical chemistry was still taught by professors who had been here when it was called Throop Institute.

Erwin: Oh, really?

North: Yes. There were two very old men. [One of them was] Professor [Stuart J.] Bates. The instruction was very good; it wasn't like the experience I had had in Chem 1 with one of the old holdovers who just sort of droned on and on. In physical chemistry, some of the concepts were very difficult to master. I still don't feel comfortable with thermodynamics.

Erwin: Thermodynamics comes in in that course?

North: In physical chemistry, yes. I had had engineering thermodynamics as an electrical engineer, and I understood that. But they didn't have the concepts that one gets in the physical chemistry, of entropy and so on. The professors were never really able to explain what these concepts referred to in a chemical system. [Even] the chemistry students had trouble with it.

But anyway, George Beadle was chairman of the Division of Biology. And he interacted continually with all the students. He was very pleasant to talk to. With some professors I've had, I've felt that I was being judged—you're on edge and you don't want to say stupid things—but talking with George Beadle was totally relaxing. He was wonderful.

Erwin: Did you have any classes with him? Was he teaching undergraduates?

North: Bi 1 was taught by Beadle and Max Delbrück.

Erwin: Ah, two heavy guns there. Two Nobel Prize winners.

North: Two Nobel Prize winners. Neither of them had a Nobel Prize at that point in time. Delbrück gave most of the classes. All the science majors—physics, chemistry, biology, and geology students—had to take Bi 1, so it was a big class. Delbrück had been a physicist who became interested in biological questions. So at the start of a class, he said, "I am the instructor, but I don't know any more about Bi 1 than you do. We're sort of taking this course together, and I'm going to learn a lot." Well, he was a very clear instructor. I had no trouble mastering the material, and in fact I already knew quite a bit of it, from the previous reading that I had done. So I got quite good grades—not top grades. At that time, I was more interested in animals than in plants, but the biology curriculum had a great deal of plant physiology in it, and that was kind of a bore.

Erwin: But you built on that later?

North: Yes. I ended up studying marine plants. But I got good grades in the plant physiology classes.

Erwin: Who were your plant physiology teachers? Did you have Frits Went, for example?

North: Frits Went taught two classes, and he was a delightful person. We all liked him very much. Then Sam Wildman taught a course in plant physiology for one term. He was a postdoc here. And then another postdoc—I can't remember his name—taught the second term of plant physiology.

Erwin: Was James Bonner teaching at the time?

North: James Bonner was here then, but we never took a class from him. I took genetics from Ray Owen. Again, that was a fairly large class; I guess some of the other options had genetics as a requirement. And then as a senior, as a special research project, I had Dr. [Alfred H.] Sturtevant. He was just a wonderful person to get along with. I enjoyed that. The project involved studying fruit flies: whether the pigment in the eye went along with some other feature—I can't remember whether it was wingless flies. But at any rate, I produced the combination that they were interested in seeing if it could be produced. I gave my final report in person to Dr. Sturtevant. I had about four or five bottles of flies, some of which were very interesting. He didn't want to see any of them. He just wanted the one bottle where I had the right pigment with the wingless—

Erwin: Oh, yes. So that proved something about the chromosomes, no doubt.

North: The position of genes on the chromosomes was what I presume that was leading to.

Erwin: Had the department gone to the molecular biology approach?

North: No.

Erwin: This was what you might call classical biology?

North: Yes. Well, it was strongly oriented toward physiology and genetics.

Erwin: Because those were the strong professors.

North: Yes. There was a little neurobiology. Professor [Cornelis A. G.] Wiersma was the first to show that certain nerves were inhibitory. If you put your electrode on that and zapped it, it would turn off other nerve processes. That was the first time that inhibition had ever been demonstrated in the nervous system. He worked primarily with crayfish. In the animal physiology class, we worked with frogs and crayfish. We duplicated some of the experiments

that had been done, illustrating nerve and muscle reactions and so forth. That was fun. And Anthonie Van Harreveld cotaught the animal physiology course. I used to have the physiology book, but I got rid of it, I guess. I don't see it over there.

Erwin: Perhaps we have it in the Archives. Was it written by Van Harreveld?

North: No. It was mainly about human physiology. Then, every student had to take the summer marine biology course in the Kerckhoff Marine Lab, down at Corona del Mar.

Erwin: When did you do this in your student career? Did you do it toward the end?

North: Usually it was at the end of the sophomore year. It was a six-week course. And characteristically it had been taught by George MacGinitie. But MacGinitie had decided to leave the marine lab. He ended up in Alaska; I don't know specifically where he went. But we got one or two lectures from MacGinitie before he left. And then the rest of the marine biology course was taught by a professor from back East. Gene [Harvey E.] Lehman was his name. He had just been recruited for this summer course. And then right after my senior year, they had gotten another professor—Arthur Geise, from Stanford—to teach the marine course. They needed a lab instructor. So Beadle talked me into being that, since I had had the course and I knew where the animals were that they needed to collect and so forth.

Erwin: Did this turn you on to marine biology? Or do you think that was already going to be your specialization?

North: Well, I wasn't sure what my specialization was. I was mainly interested in animals.

Erwin: Marine animals, or just animals?

North: Just animals, particularly animal physiology. And biochemistry. I did some student research under Professor Wiersma. He asked me if I'd be interested in going down to the

Scripps Institution of Oceanography for graduate work. And I said yes. So he put me in contact with Professor Denis Fox down there, who was a biochemist by training, specializing in marine biochemistry. That was a mile away from my home, so I said, "Yes, that sounds wonderful." I applied for graduate entry here and at Scripps, and I was accepted here. But I think it was Beadle who said, "You'd do much better to go to another institution for your graduate work."

Erwin: I see. So that was driving it, as well as your interest in the marine business. Well, I think that's often been said. And people will bring that up—that they were glad they didn't do all their formal study at one institution. After all, you had been to Caltech twice already. [Laughter]

North: Yes. [Laughter] I had a degree in science and a degree in engineering.

Erwin: Right. But did you get your biology degree faster than normal?

North: Yes. I did it in two years.

Erwin: You must have worked pretty hard to do that.

North: But I didn't have a heavy load. I had the usual units.

Erwin: How did you do it in two years? Were you just working through the summer, or did you have credit for some of the work you had done before?

North: I had credit for the first two years. I had to take a few courses that a biology major would take that were given in the sophomore year: Ernest Swift's analytical chemistry and Bi 1, I think, were the only two. Swift's course was tough, but I survived.

Erwin: You graduated in '50. So then you were at Scripps.

North: I entered Scripps in September of 1950. I was immersed in totally new course work in oceanography. It turned out that biology at Scripps was all oriented toward the ocean. And it was not so much oriented toward the physiology of marine creatures as toward what the marine creatures could tell us about the ocean. The biology was called biological oceanography.

Erwin: I see. But this was OK with you, presumably. Or did this come as a surprise?

North: It came as a bit of a surprise. My major professor was supposedly a biochemist, and I expected it to go on more or less in the direction that Caltech had taken, but it didn't. It was just a completely new line. But with the rigorous training I had had in mathematics and physics and chemistry, I was head and shoulders above all my classmates.

Erwin: So Caltech had given you a very solid foundation for this.

North: Yes. This was the first inkling I had that I really had a strong, solid foundation in the sciences. I remember that the most difficult lab, in chemical oceanography, involved taking a water sample and determining the partitioning of the different compounds involving carbon dioxide. There was carbon dioxide, carbonic acid, bicarbonate, and carbonate. We had one three-hour lab where we did the lab work, and then there was another three-hour lab in analyzing the lab work. I did the analysis in an hour, and Professor [Norris W.] Rakestraw said, "OK." And all the other students were struggling. Some of them took four hours. A couple of them asked me to leave my analysis with them. They came down after about three and a half hours and said, "You made a mistake." That was very typical of me; I make arithmetical errors all the time. I can understand the concept, but I make errors. And sure enough, in the last step I had made an error, and it was very obvious. I said, "I'm sorry," and one student said to the other, "Chew him out!" [Laughter]

Erwin: [Laughter] Well, they were all annoyed at you anyway to begin with, I suppose, because of your speed.

North: No, it was very friendly. They were sort of amazed that anybody could finish it in that amount of time. The next big thing that happened to me was that I fell off a cliff. When was that? I think it was in January of '52.

Erwin: And how did that come about?

North: Where I lived, the family residence, which was Uncle Wheeler's old guest house, was at the edge of a cliff—or maybe fifty feet back from the edge of a sandstone cliff. And there was a little place at the bottom. I had built a small boat, and I didn't want to haul the boat up the cliff every time after I went out in it, so I thought I'd build a little boathouse where it could be secure. The cliff was about a sixty-degree angle. I thought if I made it a ninety-degree angle I'd have room at the bottom for my boathouse.

Erwin: Ah, so you were digging. You were removing part of the cliff face.

North: Yes. Well, the night that I fell, I was working up there. I had a jackhammer and I was jackhammering the cliff. It was after two days of rain, and darkness had fallen. At the time, my sister and I were living in the house, and she was away at a cocktail party. So I was digging away, and a little stone hit me on the head. I put my hand up because I thought something was loose up there, and the whole cliff—a landslide—was coming down. So I thought, "This is it." I rolled down. Fortunately, if you look at it from this direction, I was digging up here, and the landslide was here. So I fell in this direction and the landslide went down in that direction.

Erwin: I see.

North: But in this place there was a free drop of about fifteen feet. And there were boulders down there, but before I started the jackhammer I had shoveled quite a bit of dirt down, so there was a pile of dirt there, and I landed on that.

Erwin: But you had a straight fall?

North: Yes. I rolled down about ten feet and then I went off the edge and had a straight fall of about fifteen feet. I can remember during the fall thinking, "Goodbye, cruel world!" [Laughter]

Erwin: You did have that flash?

North: Oh, yes. I thought it was the end. But I landed on my seat, and that sent a compression wave up the spine. Usually the first lumbar vertebra is the weakest one, and it squashes. So I was paralyzed from the waist down.

Erwin: And did the vertebra actually fracture, or just compress?

North: It just compressed, but part of it was pushed into the spinal cord. It was a low tide, and this place was under about six feet of water when it was high tide, so I had to get out of there. So I pulled myself with my hands around this ledge and got up there. By then it was maybe 8:30 or 9:00, and I was just shaking with cold. My sister came home from the cocktail party. She had a boxer dog named Bennie—Bennie the boxer—and he was very intelligent. She said, "Go find Wheeler."

Erwin: Oh, so she actually sent the dog to look for you. She knew you should have been back.

North: Yes, she sent the dog to look for me, and the dog found me there. I had been working on this project for weeks and weeks, so he knew where to look for me. And I was in old clothes. The only thing I had was a handkerchief. I tied the handkerchief onto the dog's collar and said, "Go home." He went back and scratched on the door and my sister opened it. In he came, with this white handkerchief. She said to me later, "I thought, If Wheeler's playing a joke on me, I'll kill him." But she went down and found me lying there on the ledge. She got help. Lifeguards came down with a stretcher and got me back up the cliff to a hospital. By then my left leg was moving a little bit. The dog got his picture in the paper, and I was the brains of the operation. [Laughter]

Erwin: Well, it's quite a dramatic story, of course. It's the sort of thing that people are thrilled to hear about—that animals can perform these jobs. Of course, you're extremely lucky.

North: It saved my life.

Erwin: Were you in pain?

North: I'd say I was in discomfort. I knew that I had a hurt back there, but it wasn't excruciating.

Erwin: But you knew you couldn't walk.

North: Oh, yeah. I had to pull myself just with my hands. Toward the end, this foot started to work a little bit, and I could push. But I didn't want to push very much, because I thought I might cause a lot of damage up there.

So I was six weeks in the hospital. At the end of about a week, I was beginning to recover. I had bruises all over my face, but I was beginning to recover. So they started putting me in hyperextension. What they do is, they really bend you around, and that takes the weight off the centrum of the vertebra—which is where the damage had been done—and then your weight rests on the back side of the vertebra. And when they did that, my [right] leg started moving. But when they started hyperextension it was tremendously painful.

Erwin: Was it? Oh, gosh. It was like being racked.

North: They had me on Dilaudid, which is a morphine derivative. The Dilaudid hardly did any good at all. What they did first was just to take a fairly soft object and put it under my back. And just that much movement in the spine was enormously painful. But by the end of two days, I was really bent back. They'd turn the hospital bed; the bottom end of it can come up so that your feet are up. So they turned it around and hyperextended me with that kind of a

contraption. And when I was fully hyperextended, they put me in a full body cast. And at that point, they started making me walk. I had to learn to walk all over again. By six weeks, I was ready to go home.

At the time of the fall, I had been at Scripps for three semesters—a year plus three extra months. And at that point I had taken the first oral exam and I was studying for the second one. That had to be put off. But in the summer of '52 I took the second oral and then started work on my thesis. By then I was moving very comfortably, although the right leg was about half as good as the left leg. But I could walk without any trouble.

Erwin: You didn't have to have crutches or canes?

North: I started out with crutches. That was about the first week after I went home. But I got rid of them. So after about three months they took the full body cast off, and, oh, that was painful.

Erwin: Then you had to settle back into your normal position?

North: Yes. And it was painful, but only for about three days. Then at the end of each day I'd begin hurting and would be tired out. Then I'd take codeine and I could get through the evening. But I've had trouble with pain. And as the nerves begin to regenerate, you get stabbing pains. And I've had those the rest of my life.

Erwin: So it's unpredictable when you're going to get them? They just come?

North: That's right. It often happens after I've been asleep for about an hour or two in the night. And then I have to take quite a bit of pain medicine to get that under control. But I made a good recovery and I've had a very active life. So it's a story with a happy ending.

Erwin: When you walked, did you walk pretty normally, so that someone looking at you would not notice anything?

North: That's right. I couldn't run. I used to play lots of tennis. There was no way I could play tennis anymore. I couldn't really run, but I could walk and keep up with anybody.

During my recovery I began studying again for the second oral—called the qualifying exam in those days, and I think I took it in June. Some of the professors were a bit grouchy at me, but I passed. My professor had wanted me to do the thesis with a deep-sea shrimplike creature called a euphausiid. And I had been out on this boat several times collecting euphausiids, but it was clear that I wasn't going to be able to pursue that, so he switched me to a study with a little intertidal snail called the periwinkle.

Erwin: Does a euphausiid have a common name? Is it a mollusk?

North: It's a shrimp—or it looks like a shrimp. It isn't a shrimp but it looks like a shrimp. [Laughter] There are three big—I guess they are—orders: the Mysidacea and the mysids, the Euphausiacea and the euphausiids, and the shrimps belong in an order called Decapoda. At any rate, the euphausiids occur in just enormous swarms, but they're usually about 1,000 feet down. You just tow a plankton net, and if you hit a group of euphausiids and bring up the net, it will be crawling with thousands and thousands of these little shrimplike creatures.

Erwin: You netted them from boats.

North: Yes. Good-sized boats.

Erwin: Well, also around this time you did two other things: You got married and you started diving. Is that correct?

North: While I had been in my second reincarnation at Caltech, there was a graduate student named Howard Teas. He was very interested in—[Tape ends]

Begin Tape 2, Side 2

Erwin: OK, so Howard Teas got war-surplus equipment.

North: There was equipment from the bombers. The flying fortresses and the superfortresses flew at very high altitudes, and the pilots and the crew had to wear oxygen masks and breathe oxygen. So a lot of this oxygen equipment—masks and lines to tanks—became available in the war-surplus stores. So Howard was experimenting, trying to make diving equipment out of them. As I remember, we tried out one of the things he concocted up in the swimming pool at Pasadena City College, which was Pasadena Junior College in those days.

Erwin: Yes, right.

North: It sort of worked.

Erwin: In other words, it was a self-contained breathing apparatus? That's what you were aiming for here?

North: Yes, that's right. But then the first French aqualung became available through a sporting goods store in Westwood, and I bought one of those. It was just toward the end of my stay here at Caltech. So I was studying for finals. I never connected with Howard after that. The following year, Howard got another war-surplus thing—a compressor that compressed up to 3,000 psi. They were used on airplanes to pump the hydraulics that lowered the landing wheels. So it was a Cornelius compressor. Howard Teas had gotten that and was using it to pump up the air for his diving apparatus. And he overstressed a tank and it exploded and blew out the sides of the laboratory. I guess he took a hunk of shrapnel in the gluteus maximus. He later went on to the University of Miami, as a professor there.

But I tried out my aqualung.

Erwin: Was that an expensive item at this point?

North: It cost $150 for the regulator and a little-bitty tank.

Erwin: And that's all it was? It wasn't the whole setup?

North: No. Well, I had used face masks and swim fins before. So I tried out the aqualung a couple of times and enjoyed it. I didn't know anything about it. There were no textbooks or courses or anything.

Erwin: Right. But it was French, you say.

North: Yes, it was French. It was a Frenchman who owned the store in Westwood. René Bussoz was his name. He had connections with [Jacques] Cousteau.

Erwin: Well, that was what I was wondering. Because one thinks immediately of Cousteau, who was such a pioneer in this whole thing.

North: Yes. Cousteau had formed this company to capitalize on his scuba [Self-Contained Underwater Breathing Apparatus] invention.

Erwin: Cousteau actually invented it. Is that correct?

North: Yes. Well, there were two people: Cousteau and [Emile] Gagnan. Gagnan was an engineer; Cousteau was a lieutenant commander in the French navy. I was very busy with my course work at Scripps, so I didn't have a lot of time to go out and go free diving. And that landslide had landed on my boat and just totally demolished it. As I crawled by it, I saw this huge rock, a huge boulder, sitting right in the middle of this splintered, crushed boat.

But anyway, we're back to diving. That summer a group of people who were very knowledgeable in military diving wanted to do some work at Scripps on investigating diving physiology. At that point in time, a young man named Conrad Limbaugh, who was a student in ichthyology at Scripps, had also purchased some equipment from René Bussoz. I'll say this:

the budget of his major professor, I think, purchased this equipment. And Limbaugh had started a training course in diving at Scripps. It was quite separate from what I was doing—I was working on intertidal organisms by that time. But this group of people—these military divers—wanted to do their study in the summer of 1952.

Limbaugh had commitments to go elsewhere, so they assigned me to this group. They all came from the East Coast. They didn't know where to go to buy this or that, and they needed some help in what they were doing. At that point I was wearing a brace; I wasn't in my cast. And I was still pretty fragile, so I couldn't do any diving with them. But I learned the essentials of diving. They were very, very nice people.

Erwin: Were these guys actually in the military?

North: Some of them had been, in World War II. Three of them were MDs and very conversant in diving physiology. One of them was an Italian who'd been a frogman in World War II.

Erwin: So that's all they had done during the war: any military diving had been done without air. Is that correct?

North: Oh, no.

Erwin: Or had they had the air pumped down from above?

North: No. Well, salvage work they had done with a hose; that was called hardhat diving. But the scuba equipment used in military diving was a small bottle of oxygen that you carried. There was sort of a jacket that you put on, and a little bottle of oxygen was there. It also had a canister containing—what did they call it? It's essentially sodium hydroxide pellets, and there's a commercial name for it, soda lime. You'd take a breath and exhale, and the exhaled air went through the canister, which would take all the carbon dioxide out.

Erwin: I see. So you were recycling.

North: You recycled the oxygen. It was very important, however, when you first put it on, to get rid of all the nitrogen, because when the oxygen bottle runs out, if you have a bunch of nitrogen, you'll be breathing nitrogen and you'll just go to sleep. The lack of oxygen doesn't trigger anything in the head—it's a build-up of carbon dioxide that tells me I have a shortness of breath. This is getting way off the subject, but anyway—

Erwin: Well, no. I was interested just to get a little sense of the background of the diving and what the technology was like at the time you became involved.

North: It was mainly military, with recirculated breathing. "Rebreather" is what the device was called. It had actually been invented for going down into mines where the air might be contaminated with something else, so you had to go in with a rebreather. So this was modified for military work and for diving in the ocean. And this group was interested in the free nonrecirculating scuba system that Cousteau had developed.

Erwin: OK. So he was the first to come up with that technology.

North: But the tank you wear is much bigger, because it's only one-fifth oxygen and four-fifths nitrogen. When you exhale, bubbles go out. With the little oxygen tank, you'd have a small tank that would last an hour or two.

Erwin: I see. So that's what was used during World War II.

North: Yes. So there wasn't a trail of bubbles. If a lookout saw this trail of bubbles, they'd drop some depth charges over.

Erwin: Right. Is the rebreather still used at all?

North: I imagine it's used in the military.

Erwin: I'm just curious. For the Navy Seals or these other exotic groups?

North: I remember at one point the group was discussing, "Well, maybe if we can make the air bubbles a little smaller or something like that, we can get away with it." And the Italian frogman said, "There's no way I would ever have dived with a free-breathing device against the British."

Erwin: It's too dangerous.

North: It's too dangerous. At any rate, one interesting thing is that one of the participants was Dr. Hugh Bradner, who was a very practical-oriented physicist from Berkeley. He was very interested in diving. He had this concept about the suits. The suits that had been worn up until that time were full bodysuits. They were airtight, so no water got in. It was like a hardhat but without the hardhat.

Erwin: And they were made out of what?

North: Usually rubber with a hard coating on the outside.

Erwin: Were they heavy?

North: Yeah. They weighed about fifteen or twenty pounds. It was an appreciable weight, but it was much less when you got underwater. You had to wear lead weights, or you'd come up. Hugh Bradner had this concept of just lining the body with a thin insulating material, and he had located this foam-rubber material that was coming on the market. He got big sheets of it and he made a suit out of foam rubber. We were testing it and it worked beautifully. But the principle was that it wasn't watertight. But if you didn't exchange more than a cupful of water a minute, you could stay warm. So the suit had to be very tight.

Erwin: Is this what we call the modern wet suit?

North: The modern wet suit was invented by Hugh Bradner that summer. They needed another one, and Bradner had to go back to Berkeley, so I made the second wet suit.

Erwin: Oh, really?

North: He gave me the material and the instructions for how to do it, and the glue, and I made the second wet suit. But at any rate, I learned the most important thing of all when you're diving is don't hold your breath when you come up. The apparatus gives you air—or air-gas—at the surrounding pressure of the water. If you're thirty-three feet down, that's two atmospheres of pressure. If the gas that's in your trachea, lungs, mouth, sinuses, is the same pressure as the outside pressure, you don't get crushed. So that's the whole principle of the self-contained underwater breathing apparatus: the apparatus gives you gas at the same pressure as the pressure outside. This had been learned by the military and had been passed on to the civilians, like Cousteau. But if you start going up and hold your breath—let's say you're at sixty feet and your gas is at three atmospheres—you can rupture your lungs. That can lead to death very quickly. It's a very serious problem; it's called an air embolism. Little bubbles get into your bloodstream via the lungs and they make their way to the brain and that's it. It blocks the flow to the brain. So to avoid an air embolism, you have to breath normally and be sure not to hold your breath when you change your depth.

Erwin: When you're changing depths and coming up.

North: Yes. And I was very lucky in the little bit of diving I had done that I hadn't given myself an air embolism, because I didn't know about it. So I learned that, and several other things, diving and hobnobbing with these people all summer long. My major professor was very annoyed that they had taken me away.

Erwin: I see. But as it turned out, this was a good thing.

North: It was a good thing, yes, but from his viewpoint I was his student. And the Scripps director, Roger Revelle, had told me to go with these people, so I had no choice. Fox was miffed at Revelle rather than at me.

So anyway, the little snail project went very well. I did quite a bit of biochemistry on the snails, and ecology and physiology. I guess I've given you the thesis, haven't I?

Erwin: Yes. Remind me what the name of it was.

North: The snail was *Littorina planaxis*.

Erwin: Yes, and what was the title of the thesis? Do you remember that?

North: I don't remember, frankly. "The Ecology and Biochemistry of the Marine Snail *Littorina planaxis*," or something like that. ["Size distribution, erosive activities, and gross metabolic efficiency of the marine intertidal snails *Littorina planaxis* and *L. scutulata. Biol. Bull.* 106 (1954), pp. 185-197—ed.] At any rate, the rest of the research was straightforward, and I was getting better and better all the time, so I could get out into the tide pools and study the populations. My professor was not an ecologist, but he realized the value of doing this type of study and relating it to the physiology and biochemistry. These little snails—as they get older they migrate up out of the tide pools onto the cliffs. They only get water once or twice a day when the waves splash up there. They were rather interesting to study from that aspect.

Erwin: What I wanted to ask you was, When did the term "ecology" become widely known?

North: This was back in the fifties. "Ecology" was a useful term then.

Erwin: And it was used in the scientific community?

North: But it didn't become really publicized until around the late sixties or early seventies.

Erwin: Right. But you were using the term in the scientific community at that time?

North: Yes.

Erwin: Were there a lot of people working in ecology?

North: No.

Erwin: It was a pretty new idea then? A new field?

North: Well, when I say "no," I should say that [that's in the sense of] most ecology today. But in those days ecology was essentially natural history but done in a scientific way. You know, studying populations and measuring densities and sizes and so forth and so on. That is the definition of ecology: it's scientific natural history.

Erwin: Yes.

North: So at any rate, I graduated in June of '53. Essentially I had been there two and three-quarter years, which is almost a record. But in that time I was incapacitated about six months.

Erwin: So that made it even more of a record.

North: And then three months I was out with this diving group. So essentially my graduate training was done in two years. And, as I say, my background at Caltech was a big factor in mastering the graduate work. I had to learn a lot of oceanography: physical oceanography and chemical oceanography, which is all very interesting. But it wasn't biology. It was vital if one was to understand the ecology of marine organisms, so I could appreciate that. I didn't feel put upon because I had to master all this extra stuff.

Erwin: Yes. Well, when you graduated with your PhD, you received an NSF [National Science Foundation] fellowship for postdoc work. This is when you went to Europe?

North: Yes. Professor Fox had lots of connections in England, primarily at the University of Cambridge. And one of the professors there—or the equivalent of a professor; they called them lecturers, and then a professor is equivalent to a chairman of a department. So Dr. Carl Pantin had come to Scripps for about two months. And he was housed in Dr. Fox's laboratories, so I came to know him very well. And he agreed to take me if I could bring my own funding. So I got this NSF fellowship. I think it was very close to the first year that they were available.

Erwin: I was going to say that this must have been very early in the NSF's history.

North: Yes. I think probably the most important reason I got it was that one of my recommendations came from George Beadle at Caltech. He very kindly agreed to give me a reference. So my wife and I bundled off to Cambridge. It was a very interesting year there. I was able to audit lots of classes in advanced biology.

Erwin: This had more of a biology direction to it.

North: Yes. It was similar to biology here at Caltech, but I would say it had at least three times the size of the faculty at Caltech. And it had lots of ecology. I took a course from—oh, what's his name? A very nice guy. His specialty was in X-ray crystallography: Max Perutz. Just shortly before I had arrived, a young man who had been there as a postdoc, James Watson—of Watson and Crick—under this professor had established the structure of DNA, for which he and Crick eventually got the Nobel Prize [1962]. Right in the same laboratory. They published the first account in *Nature* [25 April 1953], and I had read about it. I thought, Wow, this is an earthshaking piece of research!

Erwin: Had you known Watson before?

North: No. But at any rate, it was just a very stimulating experience taking courses from people who eventually won Nobel Prizes and people who were world-renowned for the work that they had done. And Dr. Pantin was a fellow of Trinity College there, so he used to have me to dinner once a week at the great hall in Trinity College. In the entrance to Trinity, in the entrance to the courtyard, there's a little room upstairs. That was Isaac Newton's room, and they had a statue of him in a room just off of the entrance. So it was a place that was just full of history. And Nancy and I got out a lot and saw a good deal of Great Britain and Scotland. The cathedrals were just very inspiring architecture. And then they have a big marine station, sponsored by several universities, at Plymouth, which is on the southwest coast. And I spent three months at Plymouth. Pantin was interested in a sea anemone. Do you know what a sea anemone looks like?

Erwin: Well, I do, because I used to see them in Maine when I was a kid. Where the tides were the lowest low tides, you could walk out on the rocks and actually see them.

North: Sure. They're sort of flowerlike.

Erwin: Yes. They're beautiful.

North: Yes. I doubt if it was in the literature, but Pantin said somebody had found that if you shine a light on one of these—this particular one can come in three color varieties, and the white one.... It has a column and tentacles at the top, and if you shine a light on the body, it will bend toward the light. Somehow it's perceiving that there's light. It's called photoreception.

Erwin: Right.

North: So Pantin got me started on looking at photoreception in this *Metridium;* that's the name of the anemone. We did a year of work together, and I wrote a paper and he went over it. He's the coauthor on it. It was published in the *Proceedings of the Royal Society*. [North, W. J., and

C. F. A. Pantin, "Sensitivity to light in the sea-anemone *Metridium senile* (L.): adaptation and action spectra." *Proc. Roy. Soc.* B, 148 (1958), pp. 385-396.] So that was just wonderful.

Erwin: That was your first scientific paper?

North: No, that was the second one. The first one was on the snails.

Erwin: And that was your thesis, basically.

North: Yes. Denis Fox had indicated that he would love to have me as a junior colleague, so I was looking forward to coming back to Scripps after Fox said, "Yes, come back, come back." I came back there just as a big happening was occurring at Scripps. They were angling for a million-dollar proposal in marine biology from the Rockefeller Foundation. And they succeeded, so they set up fellowships. I was awarded one of the fellowships. I continued my studies of *Metridium*. Fox was very interested in that, too. His specialty was natural pigments. You think of pigments as something an artist uses, but in chemistry any chemical, or any molecule, that absorbs part of the light and reflects another part of light is called a pigment. There's a special class of natural pigments called photoreceptor pigments; we have them in our eyes. So Fox's specialty was pigments. He was very interested. There was apparently some kind of a photoreceptor pigment in *Metridium*. He wanted me to go out and get a lot of anemones and chop them all up and extract pigments from them and see if I could identify the photoreceptor, but anemones were very hard to come by. Limbaugh told me that he had seen *Metridium* down at sixty to a hundred feet in the La Jolla submarine canyon. So I took his diving course with the intent of being able to collect my anemones at depth. It turned out that I couldn't find any.

Erwin: Really? After all that.

North: So in that sense the diving course was a waste of time, which was about three weeks. But it turns out that you can find anemones fairly shallow farther up north, in the Monterey

region or even down near San Luis Obispo. So I collected all my anemones from up north. Much later, I did once see a *Metridium* in the La Jolla submarine canyon—just one. So Limbaugh wasn't lying to me. [Laughter] At any rate, I very much enjoyed the diving.

Erwin: You were doing now what we would call scientific diving—you had a scientific purpose?

North: It was the first of my scientific diving. I actually published a couple of papers. I did quite a bit of work on *Metridium*. It turned out that if you get them totally acclimated in the dark, they are very sensitive to light. I could get them to respond—and I forget the actual number, but the light intensity was equivalent to what you'd get from a streetlight if you were a mile away, so it was a very faint intensity.

Erwin: Really? So it was a very refined photoreceptor.

North: Whatever was working there to cause a muscle contraction to make the anemone bend was a very finely tuned affair. Some physiologists a number of years earlier—five or ten years earlier—had been studying reception in the human eye and had proved that the eye can respond to a single photon, which is the ultimate grain of light. And I used their techniques to show that the photoreceptor in *Metridium* was probably responding to a single photon.

And I remember there was a professor at UCLA—a physiologist called Professor Frederick Crescitelli. He had been on my doctoral committee. He was one of the people who was disappointed in my qualifying exam. He thought this was a really important finding. I remember that he and I talked about it. So fortunately I brought him around. [Laughter] But at any rate, from that I was able to calculate the order-of-magnitude concentration that the pigment must be at. And it turned out that it would take about a hundred tons of anemones to get enough pigment so you could measure it with the instruments—the spectrophotometers—that were then available. So I told Denis that it would be essentially useless to try and go after it, but he wasn't convinced. He kept wanting me to try and see if I could find a photoreceptive pigment. But he

was a very friendly person. He would accept the fact that I didn't want to do that without getting annoyed about it, even though I was taking up space in his laboratory.

Well, then I'd had an NSF fellowship and a Rockefeller postdoctoral marine fellowship, so that brought me up to the fall of 1956. Scripps had gotten another grant, this time from the state of California, to do a kelp study. It was enough to hire about four or five principal investigators and then technicians. So they figured they needed an ecologist who could dive. I was the only PhD around who could dive. I had never had a course in ecology, but I had done good ecological studies for my doctoral thesis, so they hired me as the ecologist in the kelp program.

Erwin: And you actually were called the project officer.

North: Well, yeah. I was the first one. They had invitations out to other people, but I was the first PhD to come on the program. The man who was in charge of it had to be very diplomatic with the public. These were public funds, and the fishermen were mad at the kelp harvesters for ruining the fishing and so forth and so on, so the head of the program would have to be able to deal with irate people.

Erwin: So now the politics begins.

North: The other two PhDs were in their way sort of ivory tower types, so they made me the project officer.

Erwin: I see. I think we need to stop.

North: OK. [Tape is turned off]

WHEELER J. NORTH

Session 3

October 20, 1998

Begin Tape 3, Side 1

Erwin: We were talking last time about the kelp investigation program—the first one that you participated in.

North: Yes.

Erwin: Where did the funding come from for that?

North: It basically came from the actual state organization that had the responsibility for seeing that the research was going on properly and so forth—the California State Department of Fish and Game. The money was $200,000 for a five-year study.

Erwin: And what were the objectives?

North: The objectives were to get a lot more basic information about kelp and try and assess the biological and ecological effects of the kelp harvesting that goes on, because the sport fisher group, which is very well organized and very important politically, was complaining that the fishing wasn't as good as it used to be and it must be [because of] the kelp harvesters.

Erwin: I see. OK. What kind of kelp harvesting was going on? Who was harvesting kelp at this time and what for?

North: At that time, there were three companies harvesting kelp. Two of them were in Los Angeles. They were very small companies. One was Kelp Organics Products, or KOPCO. I can't remember what they did with the kelp.

Erwin: Was it for fertilizer?

North: I think it was more for food supplements. And the second one was in San Pedro and was called the Phillip R. Park Company. They harvested it initially for fertilizer, but about the time that we came onstream, that business wasn't holding up well, so they were also using it as food supplements, not only for humans but in farm animals like chicken and cattle and so forth.

Erwin: I see. Now, would humans buy kelp in some form, like a pill?

North: It would be in the pill form, or it would be, say, a kelp salt that you'd put in a salad—sprinkle on a salad.

Erwin: Was kelp also used at different times in cosmetics?

North: Yes, I think it was. Then the third company, which is the big company, is still harvesting kelp. Those two [smaller] companies went out of existence. The third company is in San Diego—Kelco Company. They developed a process for extracting algin—alginic acid is the chemical name. They marketed it as algin. It's a colloid, and it's extremely useful in its properties. For instance, if you pour a glass of beer, it has foam on it but the foam very soon disappears and people say, "Oh, this beer is stale," because it doesn't have any foam on it. Well, if you put a little algin in the beer, it will form a head and the head will stay there for hours and hours. [Laughter] So that's one product. Another product is ice cream. People will eat more ice cream if it isn't very cold.

Erwin: Oh, is that right?

North: Yeah, the ice cream companies found this out. If you take ordinary ice cream and the temperature gets above freezing, you get a little mound with a puddle. But if you put alginic acid in it, it stays; it doesn't start melting for several degrees above freezing. So people will eat

more ice cream if it has a little algin in it. So that was a big, important customer—the ice-cream industry—and [Kelco] had over 300 other uses for algin.

Erwin: Really? That's remarkable.

North: And it still continues. They're a very resourceful company. In the ice cream example, there was another product developed—I don't know by what company—and when you put it in ice cream, it allowed the temperature to get up. And it was sold for less than what Kelco could sell algin for, so it captured that market. But Kelco is very resourceful. They have a bunch of chemists and they keep finding new uses for algin, so if they lose one industry they can rely on another industry.

Erwin: Right. So this sets the stage a little bit for what was going on with kelp, generally speaking, when you started this first project, which was funded by the state. And you wrote in your outline that there was something called the Institute of Marine Resources, or IMR.

North: This was a sister organization on the same campus as Scripps Institution of Oceanography. The concept was that the basic research would be done in the Scripps Institution of Oceanography, but for things that had practical applications, the Institute of Marine Resources people would be in the same buildings as the scientists. It wasn't a separate building somewhere over a mile away; we all mingled in the same buildings. The IMR people were the ones who could turn the products of basic research into items that might be of general use to industry.

Erwin: I see. So this was sort of the technology-transfer arm.

North: Yes. So this type of study fit more in the IMR bailiwick than it did in the Scripps. It was supposed to be an applied study—seeing what the effects of kelp harvesting were. And we also gathered basic research. But the prime thing was what effects a kelp harvester had.

Erwin: So what did you find out?

North: We found out that we couldn't find any important effect. The kelp harvester just cuts off the top part of the plant. It's not allowed to harvest below a depth of four feet. These are plants that are situated in water that's anywhere from thirty to a hundred feet deep. So it's like mowing your lawn, except it's in the ocean. The kelp regenerates the canopy in two or three months, usually, and then it can be harvested again.

Erwin: Is it a pretty fast regrowth?

North: One of the things we found out was that the kelp fronds are the fastest-growing plant in the ocean, and almost the fastest-growing plant on earth; apparently there's a species of bamboo that can grow faster. But a frond I measured in its adult state could elongate by about two feet per day. If you could stretch it out in an aquarium, you could actually see it elongating.

Erwin: Amazing. The human eye could actually capture that!

Well, this would be a good place to introduce my question about the kelp-bed maps that you've given to the [Caltech] Archives. These were maps that dated back to 1910-1911. And what do those maps show, exactly?

North: Let me give a little background.

Erwin: Please do.

North: At that time, the situation in Europe was becoming pretty tense. The United States depended for its fertilizer on the guano deposits in Peru, and these were all run by German companies. The policy makers were fairly smart. They said, "Well, look, if there's a war between Britain and Germany, Germany won't be able to transport the guano—the deposits of fertilizer—from Peru up to here, because the British will have command of the sea. So what will happen to our agriculture?" It was pretty well known that kelp was fairly rich in plant

nutrients. So this project was initiated. They were looking at all possible sources of, primarily, potassium salts, which are called potash in the agricultural field, and kelp was a promising source. The US government funded a study that began in 1910 to map all the kelp beds from Alaska down to however far they went into Baja California, so that the policy makers could have some idea what the resource was and if it could be depended on for potash if a war erupted in Europe. How much potash could we get out of the kelp beds? A man named William [C.] Crandall did the study, and he took two years to do it, I think. The kelp maps are a product of that. I acquired those maps. There used to be a place on Palos Verdes called Marineland of the Pacific.

Erwin: Oh, yes. I remember going there when we first came here.

North: The curator of it was a good friend of mine. He had been a graduate student at Scripps. And when they opened up, I guess somebody at Palos Verdes had these maps. They gave them to Marineland of the Pacific and Ken Norris, this curator, figured he didn't really need them, and that I and my kelp project could use them. So that's how I acquired them.

Erwin: Right. So actually they had come from an unknown source, a private source, in Palos Verdes. And the correspondence that you have that goes with them: that had been with them?

North: That was a file in the Department of Marine Botany at Scripps. And Professor [Francis T.] Haxo, who was in charge of marine botany, gave me the file.

Erwin: I see. Did you use the maps yourself for practical things?

North: Oh, yes. I've used them throughout the years. Just as one recent example, there's a group in Santa Monica called the Santa Monica Baykeepers. They are very environmentally oriented laypeople doing their bit, and they wanted to learn how to transplant kelp and where was a good place to transplant cultivated kelp. So I made a Xerox copy of the relevant map

area, showing them where the kelp beds used to be there—a lot of them are gone now—so they could know where it might be useful to get kelp going again.

Erwin: So about what percentage of the kelp beds are gone now, compared to 1910 and 1911? Can you even estimate?

North: When you say "now," what do you mean? Right today, October 20? Because we just had an enormous El Niño, so the kelp resource is maybe somewhere between one and ten percent of what it was eight or ten years ago. Eight or ten years ago, when we had a big La Niña, which is the opposite of an El Niño—at that point I would say we probably had twenty-five percent of the kelp that existed in 1910.

Erwin: So that would be in recent times the maximal amount that—

North: I started recording the kelp in 1967 by aerial photography. And 1989 was the big year—more kelp than any year previously in my study or since.

Erwin: Just out of curiosity, how does El Niño adversely affect kelp? Is it water temperature?

North: Well, what happens in the El Niño is that a whole mountain of warm water comes rushing back along the equator and hits the coast of South America. Some goes north and some goes south. In the big El Niños, the stuff going north gets up to the Gulf of Alaska, and we start getting tropical animals up here. But there's an inverse correlation between nutrients and temperature: when the temperature is high, the nutrients nitrate and phosphate are low, and when the temperature is cold, the nitrate and phosphate are high. So when the warm water comes up, it has no nutrients in it. And accompanying the El Niño are large storms. So the kelp plants are weakened from lack of nutrients and these big storms just pull out what's left and destroy the beds. So right now the adult population—I think there's only one place in Orange County and San Diego County where kelp occurs, and that's at Point Loma, down in San Diego.

That's the biggest bed locally. But baby plants are coming up, because El Niño is gone and [we're in] the early phases of a La Niña.

Erwin: So the regeneration is on the way. And this is a cyclic thing then, presumably.

North: Yes. Right now it is. Back when I started the project, El Niño was thought to be a rather small climatic disturbance that occurred in Peru. Our textbook in physical oceanography was about 1,000 pages thick, and two pages were devoted to the El Niño. And in the intervening years, we find that global climates are perturbed by these El Niño episodes. And they're getting bigger and bigger.

Erwin: So this is actually a change in El Niño. It's not just an observation of a phenomenon.

North: Well, the El Niño phenomena are coming more frequently, and they seem to be getting bigger. It may have to do with a global climate change.

Erwin: Global warming?

North: Global warming. Nobody really knows. They now have records of El Niño from writings of the Spaniards. They were occurring 400 years ago. We have a relatively short body of information.

Erwin: Right.

North: But at any rate, to get back to the kelp: We essentially decided that the kelp harvesting as currently practiced was probably, from a biological viewpoint, no different than a storm coming through, because storms often just clip off the canopies. The kelp has adapted to survive situations like that. We did study a place where kelp was being cut continually. Kelp harvesters have a big investment in their boats and facilities. They want the kelp beds to persist; they aren't out there to destroy them. So they were very interested in our study, too,

quite apart from the public-relations problem they were having. We went to this area called Paradise Cove, in the northern part of Santa Monica Bay, where there's a big, thick kelp bed. And there was a pier there, where you could launch boats to go out and fish—rowboats and small craft like that. The rowboats would have outboard motors on them, and they'd go through the kelp bed, continually cutting it. Every day there were lots of boats going out and back. What we found in that area was that there were many more young plants on either side of this path, because there was more sunlight coming down to the bottom. But overall the plant density was smaller, not counting the babies; there was a lower density of adult plants. So a continuous cutting would be harmful. But a cutting two or three times a year might—if you happened to cut it just before a storm—reduce the effects of the storm, because there wouldn't be that drag of the storm waves, trying to pull the plants out from the bottom.

So we concluded that we couldn't find an effect from harvesting, but there were certainly large environmental effects, primarily from storms. In the course of our study, there was a massive El Niño. It really destroyed a lot, or a lot of beds just disappeared from '57 to '59. All the oceanographers were scratching their heads. The water had been warm for twenty-four months. This is absolutely unique in our record. It had never happened before, so they had no idea it was an El Niño. Initially they thought, Well, that's just an abnormal fluctuation that we get from time to time. But it began stretching out. It went on for three years.

Erwin: Three? So when you say a massive El Niño, you're really talking about the length of time. Is that right?

North: Yes, the length of time and the degree to which the water warms up, particularly in the summertime, and how deep the water warms up. The big El Niños warm the water down to seventy or eighty feet. And the whole length of a kelp bed lies above that, so it's a very severe effect. With a small El Niño, the effect maybe goes down ten or twenty feet, so the rest of the plant is down there in cool water and survives.

Erwin: Right. Well, perhaps we should talk about the *Tampico* episode, because this occurred during the same time period that you were doing the kelp studies.

North: The *Tampico* wreck occurred in March ’57, and the study started in late ’56, so it was about four months after the study started. I had heard rumors going around that there had been an oil tanker that went aground in Baja California. We mounted an expedition to go down and see what had happened, and we couldn’t find it—we didn’t go quite far enough. So I got more information. And at the end of April, I think it was, I went down there with a man from Kelco who was also interested. And sure enough, there was this oil tanker. It was an old, old oil tanker. They had decided to cut it up for scrap.

Erwin: So it was just abandoned?

North: No, they had decided to send it to Japan, because I think it could be cut up for scrap much cheaper in Japan than it could in the United States. So for that one last trip, they filled it with oil to send to Japan. Japan needed oil.

Erwin: Where was it coming from?

North: It came from San Pedro.

Erwin: So it was an American ship?

North: It was an American ship; it probably had Panamanian registry. It was full of oil from the Union Oil Company. So they took off and they got out a little ways. The boat was so old that a seam opened up between two steel plates on the side, up fairly high. So they wanted to come back to have it welded. The people at San Pedro wouldn’t give them permission, so they went down to Ensenada and got two welders. But the Mexicans didn’t want them to do welding on an oil tanker in their port, so they put out and went offshore about sixty miles to do the welding.

Erwin: Was there a fire danger?

North: That's what the Mexican port authorities were worried about. I don't think it was near any oil reservoir in the boat. The captain was sure the welding could be done without any hazard. But just to calm the port authority, they went offshore and did the welding. And then a very heavy fog settled in. As the fog was settling in, they got one sun—what do you call it?—where you shoot the sun and tell where you are in the ocean. They had one sun sighting before the fog settled in, and on the basis of that they steered back to Ensenada to drop off the welders. But they missed the Ensenada port by about sixty miles and ran aground. As I say, it's about sixty miles south of Ensenada on the open coast. So the decision was made not to attempt to rescue this hulk. They just let it deteriorate there, and all the oil spilled out. And essentially our study was, as far as I know, the first study ever done on the effects of an oil spill. The effect was just disastrous. We found only two species alive. One was a sea anemone and one was the little snail that I had done my thesis work on, because the little snails could get up out of the water and weren't influenced by the oil.

Erwin: I can hardly imagine this—everything just dead.

North: Yeah. It was something like 350,000 barrels of oil. And it didn't leak all at once. It just kept leaking over a period of about nine months.

Erwin: I see. And nothing was done to stop that?

North: No. This was back in 1957.

Erwin: I understand—'57 seems like the Dark Ages now.

North: That's right. There was very little concern about environmental systems. The estuaries were considered horrible places full of birds and mosquitoes. And in Mission Bay, San Diego, they just completely modified the estuary and built Mission Bay Amusement Park there, with artificial islands, and on and on. Anyway, the *Tampico's* oil spilled, and most of it went to the

south. It killed everything for about five miles. And we found piles of dead lobster and abalone on the sandy beach. There were pismo clams washed ashore. They just came out of the bottom, because they were so agitated by the oil, and then they just washed ashore. The Mexicans were picking them up and eating them. [Laughter]

Erwin: Oh, goodness!

North: At any rate, we returned to the *Tampico* on the average of about once a year. At first we made an expedition about every other month, until December of 1957. By then the winter storms hit, and they broke the ship up completely. But what we found was very interesting. A kelp population developed where there hadn't been kelp before. The *Tampico* acted as a breakwater across this little cove. There was this cove, and the *Tampico* hit the shore and grounded on its bow. It swung around and hit submerged rocks. The back end was the lowest, because of all the engines there. So it was grounded across this cove and served as a wonderful breakwater. So after the initial effects of the spill were over, why, a nice little kelp bed developed there.

Erwin: Really? How long did it take for the oil to go away?

North: We didn't see any oil in the cove after about two or three months. It was the outside of the ship that was breaking up; the shoreward side seemed to be pretty intact. You could see oil going down the coast. So after the first two or three months, the little cove was fairly clean.

I think our last expedition down there was about 1984. By then you couldn't have discerned any difference between—we don't know what was there before, but by 1984 it looked completely natural compared with other places where the oil spill hadn't occurred. Really, after the first two or three years, the only effect you could find was that for the longer-lived animals, like the abalone and the sea urchins, all you could find was little babies. But by 1984, why, the abalones were normal size and there were lobsters. And the urchins were the normal size that you'd find along the coast. So after three years, things were pretty normal, but they weren't completely normal for quite a long time.

Erwin: So how do you account for that regeneration? The assumption is that [the area] was able to regenerate fully and normally.

North: Yes.

Erwin: So nothing was made extinct? Or did stuff just come in from other places? The animal life, for example.

North: Yes. Marine animals—most of them—propagate not by giving birth to young but by spawning eggs and sperm, and fertilization occurs in the water. A little larva develops and gets pushed along in the current. Finally it reaches a certain point [in its development] and says, "I need something to settle on," and it moves down and settles. The larval form is planktonic, so larvae are coming in from all directions all the time. By the time the oil pollution had subsided, why, the new community could start again.

Erwin: And the plants?

North: The plants came in first.

Erwin: They were hardy enough to just regenerate themselves on the spot there?

North: Well, for the plants inside the cove, yes. It was just like a salad bowl. And as I say, that happened within two or three months. We made a trip down there in December of 1957. By then the winter storms were hitting and the *Tampico* was in two pieces: the stern and the bow. You can't see anything there now, but you dive and there's wreckage all over the bottom.

Erwin: What about the sea urchins? At some point you discovered that sea urchins were a problem for kelp. Does that come into this study? Or are we getting into something else?

North: That comes into the sister study. There were also the sport fishers and many common people who were dismayed at the beach pollution that was occurring from the discharge of sewage into the Pacific Ocean.

Erwin: Right. That had been going on a long time, though, hadn't it?

North: Oh, yes.

Erwin: But with population growth and so on, it obviously became larger.

North: It was getting worse. The state of California decided to set up a pollution regulation system for water, to include streams and lakes and the ocean estuaries. It initially was called the State Water Pollution Control Board, I think. These were bureaucrats, initially, so they hired three consultants to go around and have regular meetings four times a year. In addition to making regulations, the state put in enough money to do some research to actually determine how bad the situation was: Was pollution affecting the kelp beds? Was there a public health concern? And so forth and so on. So there were these three environmental consultants who were hired to ride herd on the research studies. And one of the three consultants was Dr. Jack McKee of Caltech. So Jack and I got to know each other quite well. IMR got funding from this State Water Pollution Control Board. It was about April or May of 1957.

Erwin: OK. So this study followed the first. This was really the second study that you did.

North: Yes, it was the second study, but we had this kelp group going by then. So the particular problem we were to study was, "Is pollution really impacting the kelp beds adversely?" Half my salary was paid out of the pollution study and half was paid out of the Fish and Game study. I was the project officer for both of them. For example, one of the things we did was to actually get samples of water coming out of a sewage treatment plant and dilute it down—say, at 1-to-10 dilution and 1-to-50 dilution and 1-to-100 and 1-to-500, and so forth—and then put little pieces of kelp in and see what would happen to them. I didn't do the study,

but the kelp physiologist that we had, Dr. Kenneth Clendenning, did the study. And he found that at a 1-to-100 dilution, the seawater enriched with sewage was actually beneficial; it was as if you were giving nutrients to the plants. So the consulting board was just thrilled with that, because the sewer outfalls were supposedly getting the 1-to-100 dilution. Another aspect of the study was to develop charts of the kelp beds, an historical series of charts, as best as possible, because the fishermen who were complaining were particularly concerned with a sewer outfall at Whites Point.

Erwin: Where is Whites Point?

North: It's on the Palos Verdes peninsula. Practically all the sewage from Los Angeles County goes into that. The sewage from the city of Los Angeles goes into Santa Monica Bay. So the fishermen had been watching this for years and years; the sewage pipe ended right at the outer edge of the kelp bed. And they had noticed that the kelp began disappearing in the region of where the pipe was, and then it kept disappearing away and away and away over the years. So this was their story. They were absolutely convinced that it was the effect of the sewage. Part of the project that I was involved in was to make historical charts. There was nothing available since Crandall's study. So we got historical charts to see if this is what happened. Did the kelp bed die? [Tape ends]

Begin Tape 3, Side 2

North: So the kelp did indeed disappear near the outfall. As you can imagine, we had trouble finding photographs of the kelp beds over the years—of this particular Palos Verdes kelp bed. But I did dig up one fairly good source of information. It was Fairchild Aerial Surveys. And periodically they did very detailed black-and-white surveys for oil companies, particularly for Standard Oil, which was Standard Oil of California in those days—it's Chevron now. Their gasoline stations had maps available. You know, you go into a gas station hunting for some such place. You buy the little map there, and it shows all the streets, and you can find the place you're looking for. Well, these maps were made from the Fairchild aerial photos; that was the

basis of them. And periodically the photos would be retaken, as new developments occurred and you needed new maps made. In some of them, they managed to get pictures of the coast, going out far enough so you could see the kelp beds on them. But only some, not all. So our picture was fairly shaky, but in our final report we said, "It looks like the photos we were able to get confirm this."

And then there was a great deal more work done. I did a lot of diving in and near the sewer outfalls. We took kelp and planted it near the sewer outfall to see what would happen. The region near the outfall was very, very barren. By the time the state had given us some money and we'd started operating, they had already extended Whites Point outfall out to a depth of about 150 feet so that it was no longer discharging at the outer edge of the kelp bed; it was way beyond. But even so, the regions were pretty barren.

Erwin: Did you, by the way, perceive any health hazard to yourself, diving in this?

North: We never had any problem. In those days, the San Diego sewer outfall went right into San Diego Bay.

Erwin: Everybody was swimming there anyway—is that what you were going to say?

North: I made several dives there and went right up to the edge of the pipe [laughter], and didn't suffer any effects. In one of them, I found an old umbrella on the seafloor. I opened it up and came up with an umbrella over me. I used to be a real ham act.

Erwin: Yes, I can imagine! I hope someone was there to photograph it.

North: No. The people on deck just laughed. [Laughter] But what we found in these devastated areas was that there were very few plants and not many animals, except for sea urchins. There were just gobs and gobs of sea urchins. In a square meter, you might find a hundred of them. And nothing could grow, because these urchins were going around grazing on everything.

Erwin: So that in itself would account for the barrenness.

North: Yes. So at that point I put out a paper to the effect that the regions near San Diego and Los Angeles County outfalls seemed to be dominated by these tremendous hordes of urchins. And this looked fairly unnatural, because it was mile after mile of urchins—so there might be a relationship between sewage disposal and sea urchins. But at that point the money ran out. And by that time we were down to two scientists—myself and Dr. Clendenning, the physiologist. And he committed suicide, so there was only me.

Erwin: That's terrible.

North: I had a couple of graduate students, and just before the money ran out we did a very critical experiment. At this point, we had reached the conclusion that the biota was abnormal in the region of the outfalls and was dominated by urchins. The acting head of IMR at that time was a man named John Isaacs (he later became permanent director in 1971). He said, "Go out there and get rid of the urchins and see what happens." So one of the students, David Leighton, remembered reading a paper about the problems that oyster fishermen have with starfish—this was on the East Coast. So he wrote the expert on oysters, whose name was [Victor L.] Loosanoff, a Russian American, and asked him if they were able to solve the starfish problem—because starfish and urchins are relatives. And Loosanoff wrote back, "Yes. We found that we could kill the starfish by spreading pellets of quicklime over the oyster bed." Any quicklime that lands on a starfish burns it and kills it, but the oysters can close up and stay closed for a while. And quicklime rather rapidly combines with water and goes to calcium carbonate, which is harmless; that's what the oyster shell is made of. So in this way they were able to get the starfish without killing the oysters. So Leighton got that advice and went out to see if it would work on urchins. And sure enough it did. In a very small patch—I'd guess about half the size of this room—he put the quicklime on the urchins, and sure enough, seaweed began growing there, but only for a while. Urchins came in from the surrounding area—"Look at all this yummy, yummy stuff"—and wiped out the seaweed. So Leighton and I got more quicklime and

did a bigger study on a little place near Palos Verdes where there was a sort of submerged island where rock came up, with sand all around it. It was a small reef, and it was dominated by urchins. The main reef was about fifty feet away, with sand in between the main reef and where all these urchins were. And here we were about two miles away from the huge outfall at Whites Point. So we quicklimed that reef, which was about a hundred feet long and twenty feet wide. And we got exactly the same result: the urchins were killed, and within a month you could see baby plants coming in. No kelp, but it was very rich in plant life. And pretty soon the urchins came marching across the sand and started eating; we didn't realize they could do that. Here we were, within the influence of the big Whites Point outfall, and we had been able to get some plant growth started. But we later showed what the problem was: to get kelp started, you had to have adult plants nearby to put spores out to start a kelp colony.

So on the basis of that experiment, Kelco Company said, "See if you can do something." Their big, best bed in Southern California was the bed off Point Loma. That was sort of a golden bed as far as Kelco was concerned, because a kelp harvester could go out there and get a load and bring it back, whereas if they have to harvest up at Santa Barbara, it's two or three days of travel time, which cuts down the profit on a load of kelp. So Point Loma was an important bed. Well, as a result of the big 1957-1959 El Niño, the Point Loma bed was reduced to a small patch, and there were urchins everywhere. So Kelco gave IMR a grant to try and see if we could control the urchins there and preserve that little patch of kelp, which we did. The only place to get quicklime was a lumber company up in Los Angeles. I drove up from La Jolla in a pickup truck and got a ton of quicklime, which way overloaded the capacity of the truck—it was a half-ton truck. But the truck held together and I got it back to La Jolla. I was worried, because halfway down, there was one of these stations where the trucks have to pull over and get weighed.

Erwin: Oh, a weigh station.

North: But that day they were busy, so they put up a sign: "No pickups". [Laughter] So I sped on by and got the stuff delivered down to San Diego, where Scripps had a larger boat. And we loaded it on the boat and went out and quicklimed all around this little patch. That time we got

kelp coming up. The Kelco grant was about $5,000, compared to the $20,000 grant from the pollution board. So I spent half of my time on the IMR kelp project. I also got a job at Lockheed, which was just starting up an ocean division, and I was half-time there, with the thought that when this grant ended, at the end of a year, I'd go on full-time at Lockheed.

During the spring of 1963, I got a call from Jack McKee. He was building up an environmental group here at Caltech, and it had a microbiologist named [Karl R.] Johansson as part of the group, to handle the biology. Anyway, Johansson and McKee didn't get along.

Erwin: When you came to Caltech—I looked this up in the catalog—McKee's title was professor of environmental health engineering. That was kind of a long title. And your title was associate professor of environmental health engineering.

North: I came on in September, and by that time this other biologist was gone.

Erwin: OK. So you actually replaced Johansson.

North: Yes, I replaced him.

Erwin: Was it just the two of you then, you and McKee, who had this title?

North: Oh, no, no. We had two chemists. One was named Bill Samples and one was Andrew Gram. And then there was Norman Brooks down at the other end of the hall, in hydrology and water resources. We interacted with Brooks and another young professor there named Fredric Raichlen. We were all part of the environmental group.

Let me mention one other thing that I didn't put in the outline. In 1960, I was full-time at IMR. And I got a request from Pomona and Caltech to teach the summer course in marine biology at their marine station.

Erwin: Oh. Pomona was part of the marine station then?

North: Well, Caltech would have a course that would run for six weeks, and then Pomona would rent the lab for a six-week course. Now what they both wanted to do was to reduce expenses by having Pomona students and Caltech students together and only run the course for six weeks. So each would pay half of the professor's salary. So I went up there for six weeks. I couldn't take much longer away from my job down at Scripps, but I figured teaching was very important to have on my résumé.

Ray Owen was chairman of the Division of Biology at that point. I had taken his genetics course, and I admired him very much. So I taught the [marine lab] course. It was apparently successful, because they invited me again for the summer of '61. But at that point Caltech had taken on Charles Brokaw, who had a lot of marine experience and could teach up here essentially the zoology and ecology that was involved with the summer course. So the second summer Caltech opted out of it and it was just Pomona students. I was asked again in '62, but I had some other commitment, so I couldn't do it. In '63 Pomona asked me again, and I taught the course in '63, just before I came on here in September with Jack McKee.

Erwin: So you eased your way in.

North: At any rate, the story goes a little deeper. I guess in the spring of '63, when Jack and this microbiologist Johansson weren't getting along, it was evident that Johansson was looking for a job elsewhere. Anyway, Jack McKee realized in the spring of '63 that he was going to have to replace him. So Jack went to Ray Owen for suggestions for a good ecologist type. Ray Owen thought a bit and recommended me. So that's how [that happened]. And McKee said, "Oh, yes. I know him." So Ray Owen ultimately was the person responsible for my coming back here, or at least he had a big hand in it.

Erwin: I see. So your old professors can actually do you some good.

North: Yes. [Laughter] So in '63, I left Lockheed and IMR and came on at Caltech. By that time, Kelco was really sold on the quicklime approach and controlling the urchins. So they upped their grant to $10,000 and asked if I would continue doing that at Caltech. McKee was

very enthusiastic about that, because this was a big problem—the disappearing of kelp and the huge sea urchin population. And it could maybe be reversed by controlling the urchins.

Erwin: I'm not sure if we actually got the name of the Kelco project on the tape.

North: OK. It was called the Kelp Habitat Improvement Project. And the first publication came out of IMR, and then I began producing my own annual reports up here. And you have that first publication in the three big books that have the kelp program that I gave you. [Tape turned off]

WHEELER J. NORTH

Session 4

November 4, 1998

Begin Tape 4, Side 1

Erwin: Last time you told me about the wreck of the *Tampico* and your subsequent explorations in Baja. And this led to the discovery of an unusual underwater canyon. Let's go back to that.

North: Yes. Early on in the kelp program, we did a lot of survey work, just to see where all the main beds were and in what condition they were. We also did an aerial survey of Baja California, because the kelp beds go down, oh, about 300 or 400 miles into Baja California.

Erwin: Did Kelco have access to that kelp for its harvesting program?

North: The answer is yes, but it has to be done by a Mexican company.

Erwin: I see. Subcontracted.

North: Kelco owns forty-nine percent of Productos del Pacifico, a Mexican company. And the Mexican government owns fifty-one percent of it. And Productos has their own harvester. The Mexican government would love to have the processing done in Ensenada, but it takes so much fresh water to do it that it's just ecologically impossible. So the harvester brings the kelp up to San Diego and unloads it at the Kelco factory.

Erwin: I see. Well, back to your canyon.

North: Yes. We did this aerial survey. When you're up, if the water's clear you can see quite a bit of detail in the shallow coastal waters. And as we flew over Cabo San Lucas, right at the bottom end of Baja, suddenly the blue, deep water came right in to shore in sort of a V-shape.

So it looked like that was a submarine canyon. So a month or so later we started out on a diving survey of all these beds that we looked at. And I, being in charge, ran the boat down to Cabo San Lucas so we could dive and see if there was indeed a submarine canyon, which there was. It was a really spectacular canyon, because the water is so clear there. We have submarine canyons up here, but the water is so dirty. In good, clean water you can see maybe thirty feet; most of the time the visibility is one to ten feet.

So anyway, we saw this submarine canyon, and the word got around in the Geology Department at Scripps that we had found this phenomenon down there. So the Geology Department in 1959 organized a trip down there with two or three vessels. It was a big operation to really get a lot of data out of that submarine canyon, because one of the prominent professors in the Geology Department had a special interest in submarine canyons.

Erwin: Yes, and who was that? Do you remember the name?

North: Professor Francis [P.] Shepard. So I arranged another kelp survey by air to coincide with this geology affair, and the survey again ended at Cabo San Lucas. The pilot dropped us off
there. The airstrip was a dirt road.

Erwin: At this time, it was not anything like the resort that it has become.

North: The only thing there was a small fishing village, where they fished for shark. So the pilot managed to land us; all the pigs and chickens got off the dirt road. Then we began diving in this canyon. This time we had motion picture and still cameras.

Erwin: Who was diving with you this time?

North: Conrad Limbaugh, the chief diving officer at Scripps, was down there. And Jim Stewart, who was my chief diver on the kelp program. In the short time we were there, a large storm came in from the Pacific and sent big waves in against the beach on the other side of the

point. That pushed a lot of sand over. Limbaugh was the first to notice that in the shallows the sand was slowly moving down the slope. And in this movie you can see Limbaugh pointing to the sand—I'm taking the pictures—and then the camera zeros in on the sand and you can see it slowly moving. Then, as you get farther down the slope, the sand picks up speed and there are actually sandfalls—you see this river of sand moving over a cliff. This turned out to be very exciting information, because a number of the marine geologists claimed that there was probably a phenomenon that they called turbidity currents, where loose sediment—mud and sand—would flow down the sides of the steep areas in the continental shelf and scour out regions which would then become submarine canyons. And other geologists scoffed at that idea. One of the people who didn't believe in it was Francis Shepard. I must say, he was very pleased to have this living proof on film of a turbidity current in its initial phases up in the shallows, moving faster and faster as it got deeper.

Limbaugh and I and seven others of us had a small company. One of our projects was to make fifteen-minute movies to sell to TV stations. So we made a movie of this footage that we got in Cabo San Lucas. The title of the movie is *Rivers of Sand*.

Erwin: And so now the Archives is going to have a copy of this.

North: Yes.

Erwin: Very nice. And to whom did you show it? All sorts of people?

North: Well, at that time there was only one underwater film festival in the country. Now there are dozens. The underwater film festival was in Los Angeles; we showed it at that. I wrote the script for it, and Limbaugh and I shot the footage. And then we gave a copy to Francis Shepard, at Scripps, but no movie companies or TV stations wanted to buy our product. So we eventually gave up on the movie business.

Erwin: Were there a lot of people involved in this whole idea of underwater films and underwater film festivals?

North: There was a nucleus of sport divers and semiprofessional divers in the Los Angeles area. They started programs of diving instruction, and they had this underwater film festival going, which was very, very successful. At least a thousand people came to the underwater film festivals.

Erwin: That's interesting. So this was in 1959?

North: This was 1959. In 1960 Conrad Limbaugh lost his life diving in a submarine cave in France. As a result the company lost its leader. We put money into another project, which was a diving store called The Diving Locker, in San Diego, and that was a tremendous success. The proprietor, Charles Nicklin, eventually bought us all out. He now owns Scientific Diving Consultants. He has all the stock.

Erwin: I see. So Scientific Diving Consultants was your original company that you formed with Limbaugh and who else?

North: Well, there were eight of us. They were all divers, and they had biological interests. There were various people from Scripps, ranging from technicians to researchers such as myself.

The geologists had formed a company called Geological Diving Consultants, so we formed our company. The geologists had all kinds of business from the oil companies; we had an occasional contract, which was very lucrative when it happened. It wasn't enough to justify keeping the company going as a diving organization, but it was doing great at selling diving equipment.

Erwin: Then you went on from your underwater film interests to a magazine, actually.

North: Yes. We were at the underwater film festival the next year. At that point Limbaugh had lost his life, and I made up a short film to show some of his work.

Erwin: As sort of a tribute to him?

North: A tribute to him. During the film festival, a gentleman came up to me. His name was Allan Hubbard. He was a businessman. He wanted to start a small magazine. He was very impressed with the quality of the films and the beauty of the underwater and the potential. So he wanted to start a magazine similar to *Arizona Highways,* which is mainly pictorial. So he wanted me as the editor. I agreed. We solicited pictures and stories. We put out two really high-quality issues of the magazine, which was called *Fathom*. But it was just taking up oodles of my time.

Erwin: Yes, I can imagine.

North: So after the second edition came out, I resigned as editor, thinking they could easily get somebody else. But the people who were backing the project financially said, "No go." It wasn't making money at that point, but you wouldn't expect to. It was drawing considerable interest, but the whole thing folded when I resigned as editor, which was too bad.

Erwin: That is too bad. Well, I can see how that and your diving—and also you were teaching classes in diving at the same time. Is that right?

North: At one point I taught the Scripps course when Limbaugh had some other commitment. Probably at the time I was the only person besides Limbaugh that had ever taught the Scripps course. After Limbaugh died, my chief diver, Jim Stewart, took over as the Scripps diving officer. He retired about five years ago.

Erwin: Oh, really? And was he diving for all those years?

North: Oh, yes. At any rate, the City of San Diego was also interested, in their Parks and Recreation Department, in having a diver training course. There were only two diving stores in

San Diego, ours and another one. I can't remember the name of the other one, but they were just selling equipment, they weren't holding classes. So the city felt that there was a need for public classes in the Parks and Recreation Department. So I became the chief honcho of it. I think we went on for about two years. And at that point the business at the two diving stores was sufficient, so they opened up their own diving courses. So at that point the city was no longer interested in conducting the courses. By then the public had become quite interested in diving; all the courses were well attended. So there were courses being given by various diving stores, sport equipment stores, and so forth, up in Los Angeles, and the quality of these varied considerably. Some of the courses were just an hour long. That clearly wasn't enough to produce a safe diver who knew all the problems and understood what he or she should do or shouldn't do. So Parks and Recreation of Los Angeles initiated a diving instructor course, which was modeled on the Scripps course, to give high-quality instruction and really train instructors beyond what the average diver should know.

Erwin: I see. And where were those courses given then?

North: I don't know where they were held. The county has several swimming pools.

Erwin: OK. But they were made accessible through the county to the public.

North: Yes. The diving stores found that if their instructors weren't certified by Los Angeles, the attendance dropped off. So the diving instructor course is still going on.

Erwin: Really? So this was a good outcome then—that the quality went up.

North: Yes. And Stewart and I were made honorary instructors. [Laughter] I still have a letter opener that says "Honorary Underwater Instructor."

Erwin: Well, just on a personal note, I was appalled, when I was in Hawaii this year for the first time, to see people just signing up to go diving. Anybody could go. You'd just put on a wet

suit and follow someone down underwater with all this gear on. And I thought, This looks like about the scariest thing you could do.

North: You can get into serious trouble if you don't understand the physiology of diving.

Erwin: Right. Well, all this time, too, you were lecturing to the public on kelp.

North: Yes. Early on in the kelp program I realized that there was a big public-relations problem. The sport-fishing community was saying that the kelp harvesters were destroying our kelp beds. The kelp company didn't have much of a public-relations department. Scripps had a public-relations department. They handled requests for speakers from the Lions Club, Rotary Club, and other organizations. They began funneling all these requests to me. Very soon I became known. I began getting requests on my own; I got a lot of calls. I had these films from Scientific Diving Consultants I could show. I seem to have lost a lot of my footage, but that's essentially what's left.

Erwin: People borrow it and don't give it back.

North: Or I move and it gets lost. But at any rate, I began showing people pictures of the kelp beds and the ecological importance of the kelp beds and [talking about] the commercial uses of kelp and the fact that the beds weren't being destroyed by the harvesters. I think it did a tremendous amount of good—educating the public and making them aware of the glorious underwater forests of the California coast.

Erwin: So you used film and you spoke about this. And you say in your outline that you had some of your colleagues join you in this from time to time.

North: Yes. They wanted anywhere from a twenty-minute to a one-hour show. In fact, I still get requests. The latest one was from the Newport Harbor Yacht Club. I gave them a talk last year. And I'm not diving now, so I'm not up to date. It was about a week and a half ago that

they called me and asked me if I'd give another talk. I said, "Well, wait a while, until we get over this El Niño cycle. And then I might be able to give you a talk that wouldn't just be repetitive—what I've already given you." The last talk I gave was just a talk on the El Niño, explaining what it was, why it originates, and what it does. It's very harmful for kelp beds.

Erwin: Yes. You came up for promotion in 1961 at Scripps, and you said that there was some difficulty with that. Do you want to go into it?

North: Yeah, sure. I was not aware of the promotion system. I kept getting salary increases every year. And then in 1961 I got a notice of a salary increase which was small—it was zero, from the basis of the kelp program, but then they made up for it. It was sort of a split arrangement, where I'd be the kelp administrator of Scripps. I thought, "This is peculiar." The director, Roger Revelle, and I were good friends; he had always been supportive of me. I asked him and he said that I had been rejected for promotion, so he had manufactured this title of kelp administrator so that I would get an increase in salary. Well, I thanked him for his efforts, but I said, "My goal is not administration. I'm a scientist. So let's just leave the salary as it was and do away with the administration post." So then, I think it was about a year later, apparently another committee met. I was informed of this, actually, by Roger Revelle's secretary. So she knew what was going on and that the committee had produced a favorable report. My promotion papers lacked one signature. As soon as that came through, it would go on up to wherever it went to. And Roger Revelle was away at the time, so it was obvious that the signature needed was his. When he came back, I didn't hear anything of it, but apparently there was a large—I don't know whether it was a confrontation or what—but at that point they were looking forward to producing the large UCSD [University of California at San Diego] campus. In those days, Scripps was an outpost of UCLA.

Erwin: Oh, is that right? So now a new campus was in the works then.

North: Yes. And Roger had wanted very much to be chancellor. They rejected him. So he got an offer from Harvard and left for Harvard. A new director eventually came in, and I guess my

papers were lost in the shuffle. But at any rate, around that time the state-funded kelp program came to an end. I was told I had to get research money from somewhere. This Kelco grant that kept us going after the state program ended was to expand on David Leighton's experiment with the quicklime and the sea urchins, which I mentioned earlier, and see if the urchins in the beds that Kelco harvested could be controlled. So they gave us enough money so that I could hire Leighton, myself, and an undergraduate student—all part-time—but I also had to get the job with Lockheed.

Erwin: OK. So this was a result of the fact that you decided to leave Scripps—or Scripps didn't give you what you needed, really—for your livelihood. It was a result of all these things happening at once, like Revelle leaving and the building of the UCSD campus and so on.

North: Yes.

Erwin: By the way, didn't Revelle get his name on a college at UCSD?

North: Oh, yes.

Erwin: So he didn't lose out completely.

North: He eventually got his name on a college, and he made a rather cynical remark: "That's like giving a person a gold watch after fifty years of work."

Erwin: Anyway, back to Lockheed.

North: The arrangement with Lockheed was that after this one year of working with the quickliming of the urchins, I would go on to Lockheed full time, because it was fairly obvious that my future at Scripps was very cloudy.

Erwin: Yes. OK. What was the reason given for that? I don't think you've addressed that.

North: Well, OK. In that talk I had with Revelle, back when I was trying to find out why I hadn't gotten a salary raise directly, he looked at the report from the committee and he said, "Basically it's that you're not doing enough science."

Erwin: And the mark of that would be publication?

North: Well, a requirement of our state kelp program was that every quarter we had to submit a progress report. It took substantial time to write these things up. So all my publications were these progress reports. I had a few papers before I went into the kelp program, but these were described as "gray literature" and not real publications. So Revelle's comment that I wasn't doing enough science was [based on] a) a lack of publications and b) [the fact that] I was spending a lot of time on outside activities, such as teaching the diving, giving the talks—

Erwin: And running the diving-store business? Or was that already over by then?

North: No. Well, the company still existed. And the diving store was just getting started at that point. But I had heard from several sources that Revelle frowned on his faculty members doing consulting.

Erwin: I see. Of course, that's the name of the game here at Caltech.

North: Yes, very much so. I was not aware of that at the time. So immediately, after talking with Revelle, I started out writing manuscripts to send in for publication. I think I was up to six.

Erwin: You had plenty of material, and you had no trouble? You just hadn't put it into form?

North: That's right. Anyway, the Kelco program was a fantastic success. The huge Point Loma kelp bed was down to a small patch, about 200 by 50 feet. It used to be six square miles, and now all this huge area was sea urchins. So we managed to save this patch. And we found

that you don't have to kill every last urchin; you just have to kill enough so that it allows the kelp to grow. And the kelp gives off debris, or leaves, from dead fronds. If the urchin population is small enough, and the productivity of the kelp satisfies them, their normal habitat is underneath rocks and in ledges. They only come out and start foraging when there isn't enough food for them. So once we got the urchin population greatly reduced, they quit marching into the kelp bed and the situation was ecologically stable.

Jumping on, we found that as you get the kelp bed big enough, it produces enough seaweed drift not only to satisfy the urchins over a great area but an excess, so that the kelp starts to expand. And you get instability again, on the side of the kelp: you get the kelp bed expanding on its own, if it's producing enough to feed the urchins. If you can reduce the urchin population down to—we define it as concentrations less than one per square meter—

Erwin: OK. So you did that?

North: Yes. But at this point I ran into some more trouble. Lots of pure scientists at Scripps and elsewhere were very upset because we were killing large numbers of sea urchins. The kelp company was delighted. Sport fishers were delighted. But the scientific community had reservations.

Erwin: This sounds like a continuing story, becoming more familiar, let us say, as the whole ecology movement grew.

North: Yes. It was at this point [1963] that I got the invitation from Jack McKee to join the group at Caltech, which I accepted. It was an offer I couldn't turn down, but I had reservations. First there was the publications problem.

Erwin: Were you able to talk these over with McKee at the time?

North: No. We didn't talk them over. He had been on the board, the little committee of consultants, that supervised all these marine programs that the state was supporting. He was

very much aware of what we were doing. And not being a scientist but an engineer, he was among the groups that were tremendously pleased that we were finding a way to counteract the problem of the kelp beds disappearing near the large sewer outfalls. And he said, "The next step is to try and show whether or not the sewage is directly benefiting the sea urchins."

So when I came to Caltech I was very pleasantly surprised that my progress reports were considered as valid publications in the Engineering Division and that the attitude was that I was doing great work and wasn't "killing God's little creatures," as one scientist told me at Scripps. Moreover, consulting was recognized as a valuable occupation, provided you didn't do too much of it. The administration recognized it, and there were very well-defined limits on what the [ratio of] consulting should be to the Caltech work.

Erwin: Right. And this was in 1963? Am I correct that you came in 1963?

North: It was September of '63 that I came on. [Tape is turned off]

Begin Tape 4, Side 2

North: I should also mention that during the closing years of the kelp program, in 1960, we had a permanent transect line 100 meters long set in the bottom of the La Jolla kelp bed where we'd go, continually making observations of tagged plants. And on May 22, 1960, we were out there doing our routine studies, when all of a sudden a mighty current came up. Usually if there are currents in this coastal region, they are at the surface, and their force diminishes with depth. There's usually hardly anything on the bottom. And particularly in kelp beds, where the current is retarded by this mass of foliage in the water. But this was about a one-knot current, roughly a mile an hour or more, coming along the bottom. And the kelp plants were bent over maybe five to ten degrees from the bottom, streaming out in this current.

Erwin: And suddenly you saw this? Or you felt it?

North: It happened while we were counting fronds on the plants.

Erwin: So your first impression was just that everything was bending over?

North: It didn't happen like that. But in a matter of five minutes or so, we began feeling this current. And it increased and increased. Very soon the kelp plants were streaming out at low angles to the bottom. We just kept on. We had no idea this was unusual. We had never experienced it before, but we had a job to do so we kept on working. After a bit, the current dissipated and disappeared. I remember that we came back to the boat and put on our new tanks to continue the work. And the current came up again at about the time we finished. So we came up during the spell of the current. I expected to have this enormous current at the surface. If it was a knot at the bottom, it would be four or five or six knots at the surface, but it wasn't; it was about the same throughout the water column. So we got in the boat and headed back for Mission Bay. And as we approached the landing, we saw sections of docks drifting along and people in boats trying to get them back to where they had been. What had happened was that a tsunami, which originated from an earthquake down in Chile, had arrived up here. One of the things that happens in a tsunami is that it has a crest and a trough, like any wave. But in the open ocean the crest and trough are only about a foot apart vertically but may be separated by a hundred miles horizontally, while overall moving at 500 miles an hour. If you're in a boat, you don't realize that this thing is coming along; in a span of five minutes or so, your boat goes up a foot and then it goes down a foot. But when a tsunami reaches the coast, the height between the crest and the trough increases. So in Mission Bay, what they were seeing was the tide suddenly going down in a matter of two or three minutes. And all this water in Mission Bay came rushing out, carrying these docks with it; it broke them loose from their moorings. That was the trough. When the crest came by, why, the current brought them all back in, but they were all adrift. So by the time we got there, the tsunami was over and the proprietors were trying to get their docking back in the right place.

Erwin: So it was experienced on land as an outflow of water and then an inflow, but not as a huge wave breaking over [land], the way you hear about in Japan?

North: Yes. Not as a huge wave. But that happens. They don't really understand what causes it, but it probably happens where you have bays with [narrow] sides. So as a wave comes in, it builds up, and you have this situation forming. But if you're close to the source, why, then the waves are much bigger, and when they come into shore they just break and keep on coming in. Dr. Francis Shepard was in a tsunami that hit Hawaii. He was at a resort, on vacation. And he had to climb up a tree to save himself from this wave.

Erwin: Really?

North: He said that the waves, when they came in, looked like ordinary waves, but they didn't stop at the beach. They just kept coming in.

Erwin: So you don't see it? You don't have a visual warning necessarily?

North: You don't have a warning. That's right. Until suddenly all this water is hitting you.

The word got around that we had been underwater when the tsunami came by. We were probably the first people that that ever happened to. So I had phone calls from several geologists asking me what had happened. They said, "You ought to get that published," but I never got around to it. I mentioned it to Fred Raichlen, who is our tsunami expert on campus, and he would like to have me publish it. So I wrote up a brief outline of it, but I need to refer to my data book for that day, which you people now have.

Erwin: Oh, well, you're welcome to come and get it. Or I'll get it for you, if you just can tell me where to find it.

North: I'm not quite sure. I'll have to look through them to see what it is. But I'm going to try and publish it.

Erwin: That would be very interesting. Well, you got to Caltech. And I think we talked a little bit about that last time.

North: I might say that another reservation I had was Dr. McKee himself. I had known him as a member of this committee, and he was a very critical person to work for. In one case I made an arithmetical error in making a calculation, and he was just sarcastic about it. So I really had my reservations about working for this man. But he was wonderful to work for.

Erwin: Really? So it turned out well?

North: Yes. Well, I wasn't working *for* him, I was his junior colleague. He was interested in what I was doing. He gave suggestions. But I never had any adverse experience with him.

Erwin: When you came, this was the end of the [President Lee] DuBridge era at Caltech.

North: It wasn't quite the end.

Erwin: Not quite. Well, he retired in '69. So you had, let's say, six years when he was still at the helm. What was your impression of the status of engineering in the whole context of Caltech in those years?

North: I'd say that engineering ranked at least equally with the other divisions. It was much bigger than any of the other divisions.

Erwin: Yes.

North: And it had many more students. I often had lunch with biologists over in the Athenaeum. As far as I was concerned, I was an equal colleague.

Erwin: Yes. Well, the Biology Division was perhaps smaller in comparison.

North: Much smaller.

Erwin: And smaller conceptually than it is now here.

North: Yes. The head of the Engineering Division was Fred Lindvall, and from time to time I had interactions with him. Again, they were very favorable. He seemed interested in what I was doing and felt I was going in the right direction. My paycheck each year would go up a little bit. And then at one point—this was probably around 1967 or 1968—USC [University of Southern California] had decided to open up a marine station on Catalina Island. They had a marine operation, but the scientists were all situated in downtown Los Angeles; they didn't have a marine station. After the first few buildings were there, they wanted it to be open to all universities in the greater Los Angeles region.

Erwin: Do you think this was kind of a time-share thing, to help offset the cost?

North: Yes, to offset the operating cost. So they had an excursion, to which they invited all the presidents of all the universities around here, just to go out on one of their vessels and see the area and the facilities that were available. They invited Dr. DuBridge, and they invited me. So I had an opportunity to chat with Dr. DuBridge, because the trip was two hours long getting out there and getting back. I don't remember what we talked about. It was mainly oceanography. I very much enjoyed that brief time with him.

Erwin: Caltech had its own marine station, and had had for quite a while. Would you like to talk a little bit about that? Would this be a good place to do that?

North: Sure. The marine station started back in the days when [the Biology Division] was first formed and Thomas Hunt Morgan was brought in as division chairman [1928]. His interests were very broad. He felt that the Biology Division should have a marine station.

Erwin: So it was his idea, actually?

North: It was his idea. The impetus was his. So they looked around. I guess they got around to this during the Depression. And they found a facility in Corona del Mar. It was, shall I say, going out of business. There had been a hotel there, and they built a large bathhouse down in a place called China Cove, where people in the hotel could go down and rent a canoe or a boat or go swimming on the beach next to it and come in and take showers and get dressed and so forth. So this bathhouse was built in 1926, and as a result of the Depression it was up for sale. I don't know how long it had been up for sale, but Dr. Morgan became aware of this and he got William G. Kerckhoff to put up $50,000 to buy this building. Caltech purchased the bathhouse in 1930 and converted it to a biology laboratory. And Kerckhoff gave the money to the Division of Biology, not to Caltech. So Biology is very strong in maintaining that that's their facility and that I'm a guest there.

Erwin: Oh, I see. Well, that's one comment on your relationship with the biologists here. I'm sure there's more to that.

North: I don't think it's been limiting to me at any time.

Erwin: It hasn't made for an uncordial relationship with the Biology Division?

North: No. Morgan hired a man named George MacGinitie to run the marine station. I guess the marine station was just not used by the faculty very much. Albert Tyler used it the most.

Erwin: I wondered, because I know Tyler worked on marine organisms.

North: He was down there. But it was mainly just for collecting organisms that they brought back to the campus to work on. So when I came back for those two postgraduate years [1948-1950] to get a bachelor's degree in biology, by then Morgan was gone and George Beadle was the chair of Biology. And Caltech at that time was having a six-week summer course for all biology students. I was there just before MacGinitie left. He gave two lectures in the course; the others were given by an outside person whom Beadle brought in. But MacGinitie was a

very good naturalist. He was not oriented toward the kind of work that was being done up here at that time—genetics. The animal physiologists were oriented toward neurophysiology. The plant physiologists were working toward getting a facility constructed where they could control the climate in which plants were grown. That was Frits Went's main objective.

Erwin: Now, was that what came to be called the phytotron?

North: Yes, that was the phytotron. At any rate, after I went down to Scripps and got my doctor's degree [1953], and then had my year in Cambridge, I came back and I came up to Caltech and talked to Beadle, saying, "I'm looking for a job. I'm a marine biologist." He said, "We're not really going to appoint a faculty member to be down there. MacGinitie rarely came up and interacted with the campus. The only person who really uses that facility is Albert Tyler, for collecting. Each summer half of our faculty goes 2,000 miles east to Woods Hole for the summer. Nobody goes down to the marine facilities. As far as I'm concerned, we could sell it, but there's just enough resistance to that in the faculty so that we probably won't sell it. We'll just barely keep it operating and have a caretaker there."

That was the condition when I arrived at Caltech as a faculty member [1963]. Ray Owen was then the [Biology Division's] chairman. He said, "One of the problems with that facility is that if anybody wants to do any significant work there for more than just an afternoon, they have to go out and rent a room somewhere at atrocious prices. If we just put a little money into it...." The lab—the main structure—was much the same as when it had existed as the bathhouse. There were four huge rooms and a couple of showers and a couple of bathrooms.

Erwin: Were those rooms being used as laboratories?

North: Yes. There were benches in them and what we call wet tables; there was a running seawater system. A lot of Albert Tyler's specimens were in aquaria on these wet tables.

Erwin: So he had enough specimens that he kept them there, he stored them there. He didn't bring them all back.

North: He didn't bring them all back. In Ray Owen's regime, they hired Charles Brokaw [1961], who was interested in marine biology, and myself. And then around 1970 they took on Eric Davidson, who had large interests in marine animals. So it was around 1975, I think, that the Biology Division got a large grant from the National Science Foundation to refurbish the marine laboratory: build four little apartments there and chop up these big rooms into small laboratories where people could, say, have microscopes that weren't being exposed to salt water and air coming off the wet tables. Eric Davidson has a couple of technicians there all the time. A Carnegie fellow named Roy Britten [Distinguished Carnegie Senior Research Associate] came down. He has several people. So the marine lab has been pretty full and occupied and used.

When I arrived as a faculty member, the teaching was sort of something that you did, but very few people had their heart in it. The main qualification for coming here would be that you were excellent in research. During the course of my time here, the emphasis on teaching has changed tremendously. We now have systems where the students, at the end of a course, turn in grading sheets on the instructor, and the chairman of the division looks at those fairly carefully.

Erwin: Yes. When did that start happening? Shortly after you came? I think that's a phenomenon of the sixties or something—the late sixties, with all the social turmoil that came in
then.

North: I think it was after DuBridge left that this change came about.

Erwin: Yes, that would make sense. Well, when you first came, how was your position as an environmental—let's see, what was your title again?

North: At that time, I was associate professor of environmental health engineering.

Erwin: And there were a few other people who had similar titles?

North: Yes.

Erwin: And did you form a sort of discrete unit within the Engineering Division?

North: Very much so.

Erwin: How did the relationship between you and the division work?

North: The environmental engineering group consisted of Jack McKee, myself, and two junior assistant professors, Bill Samples and Andrew Gram. I remember that about once a month McKee would take us all over for lunch at the Athenaeum and lecture us on what we should be doing.

Erwin: Scientifically or politically within Caltech?

North: Well, publications. I remember specifically once he was talking about publications. He said, "This doesn't so much apply to Wheeler as it does to you two." I don't know whether [Samples and Gram] actually ever got around to publishing much material or not, because they were eventually phased out—Samples after about three years and Gram about five years after I arrived.

Erwin: So did that mean that your group was being downsized? Or were new people brought in?

North: About six months after I arrived, another senior faculty man, an air pollution expert, came aboard—Sheldon Friedlander—as a full professor.

Erwin: So he was in environmental health engineering?

North: Yes. So that brought us up to five. Bill Samples departed, and his place was taken by Jim [James J.] Morgan.

Erwin: I see. And Morgan has stayed here. Well, did you come in at a time when this field was just starting to grow?

North: Yes. I later heard a story—this was some time before I came on board—that the administration had some organization or people, I don't know who, evaluate the Engineering Division. I guess aeronautics and mechanical engineering and applied physics got very favorable reports. But civil engineering, they said, was a disaster.

Erwin: Really?

North: So I guess they hired McKee from Harvard as a person who would build up the sanitary side of civil engineering. There were other people hired in other phases of engineering. McKee had more or less free rein to hire. I remember talking with him about it later. I asked him, "When you hired somebody like me, did it have to go through a search committee?" And he said, "No. In those days I just went over to Fred Lindvall and said, 'I want this person.'"

Erwin: I see. So the idea was to assemble a group where you had chemists and biologists and what else?

North: And engineers—sanitary engineers, as they used to be called. They are now called environmental engineers.

Erwin: OK. But it took various expertises, you might say, to put together—

North: To make an environmental engineering program, instead of just a person who designs sewers and sewage treatment plants.

Erwin: Was this a new approach to creating a group?

North: Yes, I think it was. I think other universities were starting to do it, because I met my counterpart, a biologist at Harvard in the Engineering Department. So this was the trend in which a number of universities eventually went. I don't know if Caltech had the lead or not.

At any rate, McKee had broad opportunities to do what he wanted in the way of hiring. And he wasn't just a total dictator. I remember that I had to give a seminar, and McKee solicited opinions from people that attended the seminar; a number of my old biology professors came over to the seminar. I'm sure they were all favorable. They were very friendly and seemed pleased that I was involved. And actually Ray Owen tried to get me a joint appointment in biology.

Erwin: Now, were joint appointments unusual at the time?

North: I don't know. But Lindvall quashed it. He said, "I want this man to be completely oriented toward engineering."

Erwin: I see. Was this a territorial thing, do you think? Or was it conceptual?

North: I don't know. I was very happy with the prospect.

Erwin: You thought a joint appointment would be good, actually?

North: Yes. It didn't happen. But I tried to maintain my relationships with the Biology Division. For a while the professors that I knew retired. Most of them were involved in genetics. And the science of molecular biology emerged from genetics. But I have maintained relationships with the people who are involved at Corona del Mar.

Erwin: Right. The marine lab people. And at this time the kelp versus sea urchin project was still going on?

North: Yes. The Kelco grant followed me up here.

Erwin: And how long did that actually go on?

North: The last year we received funding for that, I think, was 1975.

Erwin: About how much of your time did that take? I'm wondering what you did with your time otherwise.

North: The Kelco grant followed me up here. So I had to write a proposal and get it through the administrative process. I arrived here in September, and I think we were working on the sea urchin problem by December, or maybe a little earlier. I don't think we put out a progress report for '63, but we did for '64.

But McKee wanted me to write a proposal to see if there was a relationship between sea urchins and sewage, which I did. And it was funded. In those days the precursor to the EPA [Environmental Protection Agency] was the Federal Water Pollution Control Administration. And they funded me with a grant. We were strongly oriented toward sea urchin research, and destroying sea urchins, and finding out what their status was around the large sewer outfalls. And then the FWPCA changed to the EPA. As I recall, I got a two-year grant initially and then applied for an extension. They gave me a five-year extension; they were very pleased with the work. But it was a big, tough problem. We never actually proved that there was a relationship, but I'd say that there was quite a bit of circumstantial evidence that the urchin populations benefited by the discharges. I think Kelco was so convinced of the value of controlling urchins that they expanded their [operation]. When the first grant started out, there was a part-time man who devoted a little bit of attention to marine biology. By the time we closed the grant, in 1975, they had five people working continually—over $100,000 per year—going around to the various beds they harvested, controlling the sea urchins. So in that sense, that program was a great success.

Studying the relationship of sea urchins to sewage, we were building up a good case for it. And then EPA terminated their five-year grant abruptly, after three years. So we never really established an ironclad case. What we think happens is that the sea urchin, as it develops, changes from what you'd call a rather open, soft-bodied animal to one with a hard shell. The larger ones have to have quite a bit of food intake in order to survive. What we were finding in the sewage areas was that where there wasn't any kelp or any other seaweed, the urchins were all very small. It looked like they were grazing the slime and small stuff on the rocks. And they could get enough food from that so that they survived and grew slowly. But occasionally we'd find these larger urchins who had lost most of their spines and couldn't hang onto the bottom. They were just sort of rolling back and forth, and were very clearly sick animals. So our final report essentially said that circumstantially it looked like in the region of the outfall there was much more nourishment of a certain kind for small urchins. The populations are persisting, but the early life history of urchins—when they are larvae—is planktonic; they are floating along. So these larvae are settling in these areas of good nourishment for them, but they can't survive to adulthood. So the populations are being replaced by larvae coming from other populations in well-nourished areas.

Erwin: I see.

North: After I had been here for two or three years, the government started what was called a sea-grant program. For a hundred years or so, they had had a land-grant program in the Department of Agriculture, where federal funds were given to universities and colleges to do agricultural research. And as a result, the US farmers were way ahead of everybody else in the world in terms of technology. So a senator—I think he was from Rhode Island—was very interested in getting marine research funded. So he started a big program called the sea-grant program, as a counterpart to the land-grant program, to give money to universities to do applied research on marine populations of commercial interest. Caltech was among the first recipients of a grant from the sea-grant program, because we were working on kelp.

Erwin: You were already positioned, as they say.

North: Yes. We were doing applied research. That program brought in two dollars for every dollar of nongovernment funding that I could come up with. I had the Kelco grant. And then the Department of Fish and Game received criticism that the fines they were taking from people who were hunting without licenses or fishing without licenses were going into Sacramento, and the outlying regions—the state was divided into small regions—weren't getting any benefit from them. So the politicians decreed that half the funds taken from—[Tape ends]

Begin Tape 5, Side 1

North: I was saying that I got this funding from the sea-grant program. They would give us two dollars for every dollar I could generate. So I had the Kelco grant. And then I was getting grants from what were called the Regional Fish and Game Commissions.

Erwin: Oh, the Fish and Game fines, yes.

North: So the state was divided into territories, like San Diego, Orange County, and Los Angeles. And small regional commissions were appointed to essentially give out these funds for research activities, in the counties or places from where the fines came. So I got small grants from the San Diego Fish and Game Commission, the Orange County Fish and Game Commission, and the Los Angeles County Fish and Game Commission. So all in all, with the sea-grant funds, I had about a $100,000-a-year program going, which back in 1969-1970 was big money.

Erwin: Yes.

North: And I remember Sheldon Friedlander and Jack McKee were beating the bushes trying to expand our environmental group. And they got a hearing with Dr. Robert Bacher, who was then the provost. And we all went over there to have a talk with Bacher about what needed to be done. And Bacher commented that I had gotten the first of the sea-grant funding. He seemed

very pleased with it. And he made a comment that struck a chord: "Those people back down at Scripps just had no idea what they were about."

Erwin: Oh, see, there you are: vindication.

North: I got it almost from the top. They were pleased with me. And they were aware of my problems down there. So that was very rewarding.

Erwin: Yes, good. So what were you doing with your money?

North: With the sea-grant funding, we expanded the process of controlling the urchins. We called it "controlling" instead of killing, because, as I say, there was this sentiment among pure scientists who were not in favor of this. In addition to just bringing back kelp beds which had disappeared, we did research on what the concentration of urchins is that can prevent kelp from coming back. And, as I said earlier, it's about one animal per square meter. Well, that's for the big species. For the small species—there's a purple species—it's about ten per square meter. And in these dominated areas, the urchins were often a hundred per square meter, or even more.

Erwin: Were you still using quicklime?

North: For about two or three years, we were doing joint operations with Kelco. Kelco had reserved a harvester for us to use in quickliming, but on weekends. They were dumping twenty to forty tons of quicklime each operation, because the harvesters were big vessels that could take that kind of a load. And we were monitoring the effects of these large operations. The Department of Fish and Game, quite interestingly, was very interested in this. They didn't close us down for destroying marine life; they regarded it as a research program. They had their own biologists do some quickliming operations. But by about 1973 or 1974, Kelco had shown that if you had enough people you could control urchins by crushing them with hammers, so the department outlawed quicklime.

Erwin: Right. But not before Kelco had proposed an alternative.

North: Yes, that's right.

Erwin: [Laughter] OK. But it was the hammer.

North: And by that time Kelco had six people on their program. And I had two or three.

Erwin: So you were diving down with a hammer in your hand, taking out the sea urchins?

North: Well, yeah. But we were more interested in the ecology. How long did it take abalones to come back? We participated, to a small extent, with the Kelco liming operations, and followed what happened. We showed that you had to have adult kelp plants nearby. If you destroyed urchins in an area where there weren't adult plants, you didn't get baby kelp; you got other species coming up. So to bring back a big bed like the Point Loma kelp bed was a slow process, taking several years. You'd quicklime near the adult plants and let that kelp get started.

We also did a lot of experimenting with cultivating small kelp and bringing in plants in areas where there weren't any adult plants. We'd get rid of the urchins and then bring in our own small kelp plants. And that didn't work at all, because what we found was that there are other things besides urchins that eat the kelp. Very importantly, there were two species of grazing fishes. They would come in. Apparently the taste of this particular species of kelp was very good to these fishes. They wipe you out right away. So what we found was, after we got the kelp beds going down in the San Diego region, we got money from Orange County and Los Angeles County. We did our own restoration projects in Orange County and another one in Palos Verdes. And we found that we had to bring in adult plants, enough to feed the grazing fishes, so that little baby plants could get started. Pretty soon the Department of Fish and Game joined with us in these kelp restoration experiments, particularly the one up at Palos Verdes. Together we got kelp started. It took about five years just to get the first colony.

Erwin: And at the same time, I suppose, you were discovering or explaining the interrelationships of the different forms of marine life.

North: Yes. And we had the sewage/sea-urchin study going at the same time. As one example, we brought in a postdoc who had experience with radioactive tracers. Her name was Mary Clark. And she showed that sea urchins, even when they're adults and have the shell, have a thin layer of skin over all the shell. She found that the urchins could take up dissolved organics right through the skin. That was another mode of nourishment. And that would explain why the baby urchins could survive in a nutrient-enriched area near a sewer outfall. It was because they could scrape the rocks and then get the dissolved material through their skin. So there was a lot of very interesting marine ecology that got done. [Tape is turned off]

WHEELER J. NORTH

Session 5

November 11, 1998

Begin Tape 6, Side 1

Erwin: Well, you reminded me to ask you about sea otters, and to add a little bit more about the kelp and sea urchin projects. We'll start there.

North: Yes. You'll recall that in these areas around San Diego and Los Angeles the kelp beds had disappeared and there were just jillions of urchins all over the bottom. There was an argument that maybe this was the natural condition. So an explanation was needed as to why the kelp had disappeared in these areas and the urchins had come in. Perhaps there used to be a lot of predators of sea urchins, and they kept the system under control, so that the sea urchins couldn't get out of balance. The main predator of sea urchins was sea otters. The sea otter's range used to extend way down into Baja California, but the otters were essentially almost exterminated by the Russian and American fur hunters.

Erwin: And when did this happen?

North: Well, that happened in the last century. But then there are other predators—fishes primarily, and a few starfish—that prey on urchins. A researcher down at Scripps showed that at the rate that they eat urchins, they could have a significant effect also. Oh, the lobster was another animal that ate the urchins. It does appear that the stocks of sea urchin predators are now greatly depleted.

Erwin: From commercial fishing?

North: From commercial and sport fishing. So the result is that the ecosystem that kept the urchins under control is completely out of balance. If you go down into Baja California, where

there's little sewage of any kind, and you watch kelp beds down there, they come and they go. The sea urchins eat up the kelp, but then they starve and the kelp comes back. And the cycle is on the order of ten or fifteen years. In other places it's two or three years—it varies a lot. So the introduction of sewage into the system allowed the urchin populations to survive near these big cities. Sewage apparently made a difference and kept the sea urchins going.

Erwin: Did you put this into your official study? Because you had indicated to me earlier that there were certain inconclusive aspects of your study. In other words, it wasn't shown definitively that the sea urchins were encouraged by the sewage.

North: Well, what we never got around to doing was to cultivate urchins in the laboratories and feed them on sewage material to show that, at least in their early stages when they are little, young ones, they can use that material. We'd planned to do this, and the project was originally funded at five years, but it was cut off at three years, so we didn't get around to that final step.

But then, as I had indicated, there was quite a bit of disapproval by the scientific community, or certain members of the scientific community, because we were going out and killing urchins on a huge scale. But the word that got around very widely was that we had succeeded in restoring our kelp forests. And scientists elsewhere began to document that urchins were causing tremendous damage, not only to kelp on the Atlantic Coast but to corals in tropical waters. And then other studies were done on the Pacific Coast, up in British Columbia. They duplicated our study, showing that urchins move into kelp forests and dominate the bottom in what are called urchin barrens. So gradually the disapproval died down. Then finally—about twenty years ago, I think—the Japanese, who love sea urchin roe as sushi and were running out of urchins over there, started looking for urchins in other places, and the Pacific Coast is very rich not only in this little purple one, which can't be used for sushi, but a much bigger one, which is a black color and makes excellent sushi. A few of them are red, also with big roe with an excellent taste, apparently. So the Japanese started harvesting our large black and red urchins.

Erwin: Yes. And how did that come about? How did they get permission to do that? Who controls that?

North: The California Department of Fish and Game.

Erwin: They're free to make their own arrangements with the Japanese?

North: Yes, but they didn't do it on a grand scale. They started hiring American divers to harvest the urchins.

Erwin: That seems fair.

North: And now the urchin collecting is the biggest fishery in California.

Erwin: Really? Solely because of the Japanese market? Is that right?

North: The urchins are collected. The roe is harvested and put on an airplane as freight, and it's in Tokyo on market within twenty-four hours.

Erwin: That's amazing.

North: So as a result the urchins now have a new predator, and they're fairly much under control. In the little purple one, the roe are too small and I guess they don't taste good. So those are still around, but they tend to be in shallow water. It's the big ones out in deep water that really cause damage in the kelp forests.

Erwin: Well, that's a nice sequel to the story.

North: Yes, isn't it?

Erwin: That brings up a more philosophical question in my mind. When some kind of change in the ecological system is perceived or discovered, as you did when you were basically investigating why the kelp had gone away, how do you approach the question of the degree of intervention or the kinds of intervention that human beings then undertake? For example, the idea was to kill off sea urchins. But then, of course, there was one segment of the population—scientific, and maybe the public, too—that felt that any killing off of some poor sea creature is not the way to address the problem. Is it always a series of balances and trade-offs and so on in this environmental kind of work?

North: No. I think this story is unique.

Erwin: You do? So, in other words, this is a happy story.

North: This is a story with a happy ending, but with a very complicated and tortuous path to that happy ending.

Erwin: Right. Well, perhaps we'll come to some other stories that didn't have such a happy ending. Maybe we should go back to Caltech now.

North: Fine.

Erwin: DuBridge retired in 1969, and there was a new president, Harold Brown. This was also at a time when there was a lot of social change in the country—the late sixties and so on. Would you like to comment on how you perceived, first of all, the advent of Harold Brown, and any changes that happened at Caltech—administrative, academic, intellectual, and so on?

North: I don't have much to say about the administrative changes. Harold Brown, I think, impressed everybody as a rather cold person to deal with. DuBridge was a warm personality, and Brown was not. We'd pass him on the campus and he wouldn't say hello. He probably had

lots of things going on in his head—problems that he was trying to solve. But his wife was very lovely.

Erwin: Colene?

North: Yes. She interacted with the students. She came down to the [Kerckhoff] marine lab once.

Erwin: Oh, she did?

North: Yes. I wasn't there at the time, but I had a couple of my graduate students down and a bunch of undergraduates in the summertime. They were just all atwitter; they were so pleased that Mrs. Brown had come down.

I think it was during President [Marvin L. (Murph)] Goldberger's administration [1978-1987] that the interest in teaching began to pick up.

Erwin: OK. So in the Brown administration, then, that hadn't happened?

North: Well, it's a long time ago, but I remember Murph Goldberger was pushing the concept, or the attitude, that teaching was also important at Caltech.

Erwin: And did you think that was a timely message?

North: Well, I think so, yes. I recall, at the time of the Reagan administration [in Sacramento], a public perception that a lot of the higher institutes of learning weren't doing a good job, and that their job was teaching and not research. I think this had its origin in the following. You may recall at the University of California at Berkeley the students were—the term was "hippies." These were people who were rebelling against the conventional way of life. The student body at Berkeley was very strongly oriented toward hippyism.

Erwin: Right, and I think questioning the authority of faculty and administration, finally.

North: Caltech was not at all that way. The student body here was made up of hardworking kids who wanted to get ahead.

Erwin: Right. Did you have any sense during this time that even at Caltech, quiet as it was, there was some sense that the students should be treated differently?

North: Yes. I think this was a general consequence of the public feeling that the role of universities at least included students and that more attention should be paid to them. And at some point around this time, they began to have students evaluate the professors—or not so much the professors as the classes and the quality of education. The students had a—I forget what the acronym was; it was entirely a student endeavor to assess the quality of the different courses. This was for student use.

Erwin: Right. They would disseminate that information to their buddies.

North: To themselves, yes. But the administration very soon became aware of this. It may have been in Harold Brown's administration that this got started. One year, one of the yearly publications that the students put out was very critical. And the faculty involved who got panned thought it was quite unfair. So at that point the administration decided it would be a good idea for the administration to take over the evaluation. They developed forms for students to fill out, and the instructor would pass these out at the end of the course and then collect them and send them in. And then around that time, or shortly after the changeover to having the administration do this, they began giving awards for the best instructors. So it was during this period that considerably more attention was paid to the role of the faculty in instruction. And I think it had its roots in this broader public attitude, which began with the first Reagan administration as governor.

Erwin: So how did this translate for you personally? Did you have undergraduates to teach?

North: Caltech's environmental program has always been primarily graduate students. But a couple of our faculty felt, about this time, that more attention was being given to students and that we needed an undergrad course that would introduce the students to what was being done. So Env 1, the course, came into being.

Erwin: I see. Did you teach that?

North: No. Originally they had different instructors come in and give a lecture or several lectures.

Erwin: So it was a team kind of thing.

North: It was a team kind of thing, with one faculty member in charge. And I gave a couple of the lectures. My own course in ecology underwent lots of changes. Initially it was a course oriented toward graduate engineers who were coming into the program and had never had any biology. A lot of sewage treatment involves action by microorganisms on the material in a sewage treatment plant. The microorganisms essentially eat, or consume, the more decayable material. And then the main reason, of course, for sanitary facilities such as sewage treatment plants is to prevent diseases, particularly the water-borne diseases such as typhoid and cholera—to keep those under control and to prevent them from spreading into the population. There may be an occasional case. It used to be that ninety percent of the people at some point in their life would have typhoid fever from drinking unsanitary water. Now typhoid is very rare. Many students go through medical school and never see a case of typhoid. And in the early 1800s, cholera plagues were common in the United States, and now it's a nonexistent disease in the US. But you can culture cholera organisms from estuaries and bays. The bacterium is there. It's fairly common.

Erwin: So could you get it from swimming in these waters, for example?

North: I guess theoretically you could. But the dilution of the organisms is sufficient. For typhoid, for example, you have to have a massive dose of, say, 100,000 to 200,000 organisms. The stomach's digestive process is very destructive to organic matter. You have to have enough so that if one or two get through, past the stomach and into the intestinal region, they can prosper and multiply. So when you go swimming, you might swallow one or two, but it's not enough to establish the disease organism.

Erwin: I see. So that explains why people weren't dying by the thousands from going swimming in these polluted waters.

North: Yes.

Erwin: Well, I'm just recalling from when I was young. In coastal regions particularly, there wasn't very much, if any, sewage control. There was a lot of just dumping of raw sewage, and everybody went swimming. [Laughter]

North: Well, in Latin America, on the beaches of Rio De Janeiro, the raw sewage comes floating in on the beach.

Erwin: Terrible.

North: Yes, it's terrible. But anyway, getting back to my course—

Erwin: I'm sorry.

North: No, that's OK. So the rationale for the course was that the engineering students in the environmental field needed to have at least an elementary introduction to biology and ecology and microbiology, so that as they advanced in their careers and became the director in charge at the city waterworks and the city sewage treatment plant, they understood and could talk intelligently to the biologists who were their employees and understand what the biologists were

trying to get across. So that was the rationale for the course. But the way it evolved was that a lot of students from geology and biology would come over and take it, because biology was very strongly oriented toward the neurosciences and genetics and eventually molecular biology. In the wider realm, ecology was becoming a very popular concept to the public, so students wanted to have the course in ecology. And my classes began to be filled up with undergraduates.

Erwin: I see. But this was actually a graduate class?

North: It was supposed to be a graduate class, but undergraduates could take it.

Erwin: Was this early on at Caltech?

North: No. I forget when the public interest in ecology began to increase. It was probably around 1970. So then the various schools that had civil engineering in the undergraduate area started having ecology and microbiology for engineers at the undergraduate level. So a number of the engineers that came in as graduate students had already had the material. I'd end up with maybe one or two engineers and a bunch of biologists and a bunch of geologists. As it started out as a course for graduate engineers, I had to teach quite a bit of basic biology—like what a cell was, and a nucleus, and how they developed, and the genetics of how they reproduce. I had to teach a little organic chemistry also. I had one lecture in organic chemistry and one lecture in fundamental genetics. And for the biology students, they just went to sleep. [Laughter] The course ran for two terms. I put this very basic material into the second term, because in the second term I usually just had engineers. I went directly to ecology, but I kept it at a level where the engineers could understand what was being taught even if they had never had any biology. It was a concept of populations, species, communities, and so forth. So that worked out very well. The greatest number [of students] that I ever had during the first term was thirty-six.

Erwin: That's pretty good.

North: It was usually fifteen or twenty students. But then during the second term it would be anywhere from two to four.

Erwin: Did this distribution of undergraduates versus graduate students keep going then, more or less, through your active teaching time?

North: Well, it more or less leveled off. There'd be two or three engineers and anywhere from twelve to twenty biologists. Well, usually I'd have one or two geologists, and occasionally I'd have a physicist or a chemistry major.

Erwin: It was really their own interest that prompted them to do this.

North: Yes.

Erwin: And this course was called Ecology 1, or something like that?

North: Initially it was called Environmental Health Biology. And then, as the nature of the course changed, we had Env 145. That was the course for engineers that went two terms. And Env 144 was just the lectures in the first term. If they took Env 145, they had to have a lab in the first term, which was microbiology and a little bit of ecology. So the engineers could take Env 145, and the biologists could take Env 144. The biologists would just get the lectures; the engineers would get the lectures plus the lab.

Erwin: And where did they do their lab? Did they go over to the Biology Division to do this?

North: Oh, no.

Erwin: So you had your own lab?

North: We had our own lab, yes.

Erwin: Was that in Keck [W. M. Keck Engineering Laboratories], then?

North: That was in Keck. And the labs would include some field problems. We'd go out and visit the seashore and natural communities, terrestrial communities. So it wasn't entirely benchwork in the laboratory.

Erwin: Right. Did you use the word "ecology" to label classes? How was that term used, and was it widely used? I think it's a term that's used kind of popularly.

North: Oh, very much so.

Erwin: But not everybody really understands what it means.

North: There's even an auto-wrecking company called Ecology. But in the scientific sense, it's scientific natural history—the study of populations and communities, and how they develop out of the very complex food webs that occur in communities.

Erwin: Did that term start being used in the curriculum at a certain point, or in the course catalog?

North: Well, OK. Env 144 was named Ecology.

Erwin: OK. So you did use the word. Do you want to go now to a part of your outline where you talk about the gasoline crisis? This would be in 1973. So we'll go sort of chronologically here.

North: Well, historically the Santa Barbara oil spill occurred before the gasoline crisis.

Erwin: That's right. I have here 1969. OK.

North: I had done that *Tampico* study, of the *Tampico* wreck, while I was at Scripps, or IMR. Was the oil-well blowout in the Santa Barbara channel in 1969 or 1970?

Erwin: January 1969 is the date you give in your outline.

North: OK. That's correct. That was just a disaster. President Nixon came out here. The federal government was blamed, because it was out beyond the state line, which is three miles out. This was about six miles out, as I remember. And the federal oil regulations weren't as strict as the state ones. If it had been built under state regulations, the blowout wouldn't have occurred. But at any rate, the newspapers were talking about the enormous biological destruction that was occurring. There were oil-covered birds that were very pitiful. You know, they can't fly, they can't swim anymore. They had usually ingested quite a bit of oil. So the Union Oil Company, whose well it was, had an environmental person who called me up and said, "Would you do an investigation of the area for us?" So we said yes and I outlined what I would do. The first thing to do was to make a reconnaissance of the area. And very soon, as the concept of the disaster began to swell—"This is getting worse and worse," and so forth—why, the western organization called WOGA, Western Oil and Gas Association, took over. In the *Tampico* study, the seashore had been just littered with animals that had been destroyed by the oil spill. So there were several undergraduates who wanted to be involved. So in Santa Barbara I assigned a section of beach to each person that was on my team. It included undergraduates, graduates, and technicians. They were to walk that whole beach. It would be a mile or two. And they were to collect all the dead critters that they could see or find. But what we ended up with was two birds. These were corpses that were totally dried out. They didn't have any oil on them. They were way up beyond the usual reach of the waves, which means that they had been washed way up there in a large storm. So there wasn't anything.

Erwin: Really!

North: So I went up there and looked very briefly at the area. I made some dives, including one out at the oil platform itself, which was in 190 feet of water. I figured that with my 200-foot card, I was legal to go down to the bottom and see the situation on the bottom. Well, we got down a hundred feet, and it was so black you couldn't see anything—the turbidity was so bad. So we never actually got to the bottom. There was an enormous rainfall just after the blowout occurred. A tremendous amount of sediment—fine, suspended sediment—had been brought in through the rain, and that was what was causing the turbidity. So then I went to the kelp beds. There are a lot of wonderful kelp beds up in Santa Barbara. The kelp just looked fine.

Erwin: Really! It wasn't black or oily?

North: Well, there were bits of tar on it, and some oil. But where the canopies were thick, it trapped the oil and kept it from going on shore. So we made a dive there. Again, we got to the bottom—this was in about fifty feet of water—and it was black. We couldn't see anything. But I felt around and got a couple of starfishes and some urchins and two or three other organisms and put them in a bag and brought them up. They were alive and seemed to be completely normal. Kelp often has little microscopic organisms growing on it. I collected several blades that were heavily encrusted and took them back to land. We had a microscope set up in the motel where everybody was staying. And we looked at these things under the microscope, and the stuff was all alive. The encrustations were doing fine. As I say, there was oil in the canopy where I made this collection. I couldn't really find any kind of a biological disaster. I could only spend one day there. I had a lot of other things going on on campus. [Tape ends]

Begin Tape 6, Side 2

Erwin: OK. So you came back to campus.

North: I had the team go out and look at the Channel Islands, which are offshore from the Santa Barbara area. They never went ashore. I guess there were many sea lions and seals that got

oiled. They looked pitiful, but I don't think there were any corpses. This is what I've read, from what other people saw. So again, it didn't look like a biological disaster.

Erwin: How do you account for the differences between this one and the *Tampico*?

North: Well, here Jack McKee, my boss, came to my aid. He and I were discussing the apparent fact that at Santa Barbara not much had been killed. And he said, "Well, crude oil is just a tremendous collection of different organic molecules that got fossilized." Some of the molecules are harmless and others are quite toxic. And the toxic ones are usually smaller molecules, and they're volatile, which means that if you spread the oil out, the small molecules go off into the atmosphere and the whole area stinks, because you are breathing these vapors that come off the oil. So oil that sits around for a while in the sunshine loses its toxicity.

What had happened was that the source of the Santa Barbara oil was six miles offshore, and it took several days for the oil to spread out on the sea surface. And the toxic elements had evaporated, so what came ashore was the stuff like tar and thick material. It just made a horrible mess, but it really wasn't very harmful biologically. But when they refine oil, they usually take out the tars and the high-molecular-weight things. So a refined oil, say for use in lubricating cars and machinery, has the toxins in it. The *Tampico* was loaded with refined oil, and that's what [made the difference].

Erwin: And also it was right on shore.

North: Yes, and it was right on shore.

So I began reporting to WOGA that there wasn't biological damage of the proportions.... There were oiled birds and oiled seals and sea lions and so forth—

Erwin: But they survived, is what you are implying?

North: Well, no. Some of the birds died. The government formed a blue ribbon panel to investigate. McKee was one of the people on the panel, along with Norman Brooks. The panel

had an ornithologist on it. I remember Jack saying, "I was just worried as to what this ornithologist was going to come up with when we had to prepare a report." And the ornithologist said, "Most of the birds that are dying are western grebes, and that's a very common bird. It's a stupid bird—"

Erwin: [Laughter] A stupid bird. Oh, dear!

North: Most of the other birds see oil and they go somewhere else. They don't allow themselves to come in contact with it. But grebes just go right through it.

Erwin: I see. So this was sort of a natural phenomenon, you might say—survival of the fittest?

North: Yes. Survival of the smartest. So the ornithologist said, "Western grebes will repopulate the area. It's no big deal." It was just pitiful that these birds were being killed. But in a broad ecological sense, it wasn't important.

Erwin: But of course it aroused public indignation.

North: Oh, very much so. And the people in Santa Barbara were just outraged, because this was essentially a retirement and tourist community. The beaches and the ocean were very important, and here was this awful sticky mess all over the beaches.

Erwin: But you didn't have to be concerned with that. I mean, your job was to determine the biological effects, not the—

North: Not the social part. I hoped that was going to be the case, that my role would be scientific.

Erwin: But you got more than you bargained for.

North: At any rate, WOGA was very pleased with our results. Then they decided that what they needed was a very large continuing study. The person in charge at WOGA was Harry Morrison. He and I talked usually at least once a day, and sometimes several times a day. Harry proposed to his directors that a study on the order of $100,000 a year be funded for several years, and they went along with it. $100,000 in 1969 would be the equivalent of a $1-million program nowadays. Big money.

Erwin: As far as this group WOGA was concerned, this was in large measure a public-relations kind of move.

North: Yes.

Erwin: But a responsible one, in the sense that they wanted to know the truth.

North: Oh, yes. There's no doubt it was an awful mess, but it looked like it was more a social problem than an ecological problem. But they felt that to help with the social problem and to show their concern, they needed to put big money into a continuing study. So the board of directors agreed to this. And then came the question of who would do the study. And Harry Morrison, of course, had already hired us. It turned out—I heard this later—that several of the directors were USC alumni, so the board of directors decided that USC should do the study. So Harry Morrison was very embarrassed. He asked us for a billing. We had had about eight people up there for two weeks, and there were a lot of expenses for motel, travel, and hiring a boat for three days to go out to the Channel Islands. The bill came to $5,000. Harry Morrison thought that we were taking him for a ride, but we were really being very reasonable.

Erwin: Yes. By today's standard, of course, it sounds like not very much money.

North: At any rate, Harry got over his distress, to the point that he had me on the lecture circuit describing our findings. I probably gave five lectures in all, but he sent me up to Santa Barbara to give a lecture.

Erwin: I see. So this was walking into the lions' den.

North: Yes. I could just feel the hostility. For the lecture I gave, I started out with slides from the *Tampico* wreck and showed the pictures of abalone and lobster and clams and crabs and everything that had washed up dead on the beach. And then I went to the Santa Barbara one as a contrast. I showed the picture of the two dead birds. That's all we could find.

Erwin: So you made an effect?

North: Well, I don't know.

Erwin: Did people come around, or did they still have sort of a simmering hostility?

North: It turned out that [the Department of] Fish and Game did a study and they came up with the same conclusion, but they were too scared to say anything. There was another group—Westinghouse had a marine group—and they did a study. Again, they were too afraid to do anything. I remember one of the lectures I gave was to the Marine Technology Society branch in San Diego. They had me give a talk. They had the chief biologist from Westinghouse give a talk. And the guy from Fish and Game gave a talk. I was the last one. The first two essentially didn't say anything. They said, "Well, we went up and looked, and here's a picture of the oil and here's the stuff on the beach," and so forth and so on. Then I came on and specified what we did and that we couldn't find anything. There were some rather influential people at that lecture, including the director of Scripps. So I guess word got up to the Reagan administration, and I became a hero.

Erwin: I see. But to the state.

North: To the state and to the federal government I became a hero. In the following year, I got put on three different state commissions.

Erwin: I see. Can you tell us what those were, for the record?

North: Yes. One of them was the Navigation and Ocean Development Commission, the California Commission on Coastal and Marine Resources. And then there was a very small commission. It was the State Oil Commission, or something like that—it never met.

Erwin: Here you mention Underwater Parks Advisory Board.

North: Yes, that was only a committee in the Department of Parks and Recreation, which is responsible for lots of public parks. Two or three of them said, "Why don't we have some underwater parks?"—which was being done in Florida. So the first thing they did was to assemble a board of competent people: diving biologists, diving geologists, and so forth. They brought me in. So I served on those commissions for a while.

Erwin: Right. Was that a lot of work?

North: The commissions usually met every other month, or something like that.

Erwin: And Caltech was happy to have you on these things?

North: Yes, I think so. By then Francis Clauser had come in as the chairman of the Engineering Division.

Erwin: And did he bring something new to the division? He had succeeded Lindvall.

North: Yes, he succeeded Lindvall. I think he was interested in the environmental field much more than Lindvall was. He was aware at that time that the Great Lakes were an environmental disaster. I remember he had a plan for helping to solve the Great Lakes problem. He

approached me about it. I know he also approached [Jim] Morgan. I was just completely snowed under, working on the commissions and keeping my marine program going down here.

Erwin: Did his plan come to anything that you know of, by the way?

North: No. I don't think he ever found anybody at Caltech who was willing to go in on it. But he was very interested in environmental problems.

Erwin: Well, good. You know, I meant to ask you something earlier. We were talking about your teaching and undergraduates coming into your classes and so on. Was this around the same time, too, that SURF [Summer Undergraduate Research Fellowships] began to operate? Because I know you had some students doing projects in that program.

North: I had this big sea grant, and as part of it I had enough money to hire Caltech undergraduates for the summer. We would teach them to dive. At that time, our main effort was going in and restoring kelp at Palos Verdes. They'd go up there and hammer urchins for us. They loved it.

Erwin: Yes. Oh, I'm sure.

North: They were doing something for the environment.

Erwin: Right. Well, the SURF program was a great success in many ways at Caltech. Was it still pretty new at this time?

North: It came into being toward the end of my using summer undergraduates at Caltech. I think it evolved completely independently from what we were doing.

Erwin: Oh, I see. So in a sense you anticipated that program. All right. Well, let's see. We still have some time. Do you want to talk about the gasoline crisis then?

North: We can get started on that.

Erwin: OK. Because I think the gasoline crisis had a big effect on your field. It had a big effect on society.

North: Oh, yes!

Erwin: It seems like a dream. I mean, one drives up to gas stations today and there's nothing to it, but everybody remembers the long lines and dire predictions about what was going to happen to our lives in every way as a result of this.

North: Oh, yes! About every two or three years, I would be approached to give a lecture on Seminar Day, in late spring. And probably around '71 or '72 I gave a lecture, and at the end of the lecture, a gentleman came up and introduced himself as Dr. Howard [A.] Wilcox. At that point in his life he was at the Navy Electronics Lab in San Diego.

Erwin: Was he an alumnus?

North: Well, no. He wasn't an alumnus. But he was present as a guest of an alumnus, who was also there, and the guest was the director of the Navy Electronics Lab. I think the alumnus's name was something like McLean. Wilcox was aware of my reputation for restoring kelp beds, and he had this concept of growing some kind of seaweed out in the open ocean on platforms of some kind. The concept involved taking the seaweed, which would be essentially biomass, and holding it anaerobically, digesting it with certain bacteria, and producing methane. Depending on what kind of bacteria you used, you could also get methanol out of it, and that's a good fuel.

Erwin: Right. So this was the alternative-fuels movement.

North: The alternative-fuel movement was beginning. And Wilcox was a very brilliant guy. He told me he had spent his graduate years with two or three of the physicists that helped with the atomic bomb. I can't remember their names. It wasn't Oppenheimer, but two or three of the others. At any rate, the concept was that we really needed to start using solar energy and get away from our dependence on fossil fuels. There was solar energy, [but] photocells at that time were just totally out of the question; they were so expensive. And green biomass was a very cheap way to go. If you look at the land, all the good places to grow plants are already taken up with agriculture for food or for fibers. And if you're going to grow enough biomass to supply a significant amount of fuel for industry and the public, it would take an area the size of the continent of Australia—a huge area. Well, the only place that's left on the globe, on the planet, where you could do it is the ocean. So Wilcox had heard of me as an expert in growing kelp, and I had this facility here where we were producing billions and billions of kelp embryos and using them in our restoration work. He only talked to me for about five minutes, but it sounded very interesting and I said, "Yes, I'd like to know more about it." So we got together at a later date and he suggested a small proposal to Caltech to do a survey of all the seaweeds, what their properties were, could they be cultivated, and what might be suitable plants for growing in artificial farms out in the open ocean. As I remember, the proposal was for $2,000.

Erwin: I see. And where would this money come from?

North: This came from the navy.

Erwin: From the navy. He had this money at his disposal?

North: Yes. He had a small amount of money. So I had a graduate student, George Jackson, who actually did the literature work. George and I wrote it up together. Our conclusion was that if you're going to do it in Southern California, the best thing to use is the giant kelp.

Erwin: Because that would be the easiest to grow?

North: The big advantage is that you can harvest it and it will grow back, just like in the harvesting industry, which has been going for fifty years. We know it works. So that seemed the way to go, and Wilcox agreed with us. So at that time the National Science Foundation was interested in alternative-energy programs. So Wilcox contacted them and they said that they'd be interested into putting money into a marine farm, but they didn't want to give money to the navy. So at Caltech I wrote a proposal, and we got funding from the NSF. And Wilcox talked the navy out of $100,000, and they built a structure out at San Clemente Island in water about 300 to 500 feet deep. It had anchors and cables coming up to buoys. About 60 feet under the surface, there was this grid, 500 feet by 600 feet.

Erwin: Made out of metal?

North: No. The buoys were metal, but the grid was rope cable, polypropylene rope. Caltech went out and planted the grid with kelp.

Erwin: So you had to dive?

North: We took plants from kelp beds at San Clemente Island. The difficulty was that it was a sixty-mile trip across the channel to get there. So we took some native plants there and put them on the grid. And the navy team started putting plants on the grid. The navy team actually preceded our work. I went out there to look at it, and it was a disaster. The navy tied the plants—the holdfast, or roots of the plant—directly onto the cable. And that area had strong currents. The plants would just come down in the current and wrap around the cables. When the current let up, the plants wouldn't come back up. There was no way you could harvest it. So what we did with our plants was we attached a small bed of rope, going up from the main cable—it went up three feet to a buoy—to keep the plants from coming down as easily when the current would come up. So our system lasted a little longer, but eventually the plants got caught in the cables. The problem was that our buoys had enough lift so that the cable, instead of being straight, would come up in a curve. And the plants that were down low on this curve would get tangled. The current would come this way and the plants over here would get tangled; the

current would go that way and the plants over there would get tangled. So it didn't work out. And then some kind of a big boat went through. There was no marking, so the whole grid just got tangled up in a huge ball.

Erwin: Oh, dear!

North: The navy was very unhappy. There went their $100,000. But we had the NSF grant to keep the program going, so we made our own small grid off Corona del Mar, about two miles down the coast, in 150 feet of water. And instead of using cables, we had long stretches of board, so that it wouldn't bend when you tethered the little buoys. And we kept kelp on that for a year. It did fine, except that it got very pale—it lost its color—and it wasn't growing very well. It didn't have nutrients. The nutrients are in the deep water. So we had an airlift to bring up deep water to nourish the plants. That helped a tremendous amount.

Erwin: How did you do this? Did you just dump this deep water on them? I guess I'm not understanding.

North: Here's our grid up here. And this is the bottom at 150 feet. The water down here below is full of nutrients, but there isn't much light. So we did an airlift, bubbling air through the water, and that brought the cold, deep water up.

Erwin: I see.

North: So this actually turned out to be a boon to the concept. If you're going to get high productivity, whether you're on land or at sea, you have to artificially feed nutrients to the plants. We do this on land. But if you make the fertilizers that we make on land, they require a tremendous amount of energy. By the time you harvest the biomass, you've put more energy into making the fertilizer than you get out of the biomass. But in the ocean you can bring up deep water rather simply, and everywhere, regardless of where you are. With very little energy, you can bring up the water. So the ocean is the ideal place to grow biomass.

This is what we developed essentially—the concept—with the NSF grant. Wilcox was a very persuasive person. He got the gas industry interested in the project. At that time the organization that was funding research was the American Gas Association, the AGA. AGA had been the principal research funding agency of the gas industry. The industry decided to establish a new organization, the Gas Research Institute (GRI), to fund research in which they were interested because AGA was overloaded with activities they were supervising. Our proposal came along just before the transition. So we started out being funded by AGA but were turned over to GRI after GRI came into existence.

Wilcox got the director of AGA to bring in really big money. If things worked out, there would be hundreds of millions of dollars available to do this kind of research. So Wilcox, using the results of our NSF project, conceived of what the next step should be. And that was to put some kind of a module out in the ocean and grow kelp on it. This would be the prototype of what could be put on a marine farm. The module was to be a hexagonal shape, because you can fit hexagons together and completely cover an area. So the AGA was willing to put the money in to fund this first modular experiment. And this went on without my knowledge. So all of a sudden, Wilcox had this concept that they would anchor a prototype—

Erwin: Can you call it a platform? Is that right?

North: Well, do you have piece of paper there? It would look like an upside-down umbrella. The handle of the umbrella would be a buoy, a flotation device. And then spokes would come out to produce a hexagon. And there would be cables, and you could tie the plants to the cables.

Erwin: And you literally tied them on? It seems mysterious to me why they stayed there and why they didn't float away. But they were obviously affixed.

North: The base of a kelp plant is called the holdfast. It's like roots. It's a big tangle of stuff. What we'd do is make large needles, about fourteen inches long, and thread them with quarter-inch nylon rope, and then—

Erwin: Sort of sew them on?

North: Yes. You could take these and tow them from the shore out to wherever you wanted them.

Erwin: Right. And they'd be pretty sturdy. They obviously didn't come off easily.

North: Yes. But then, as we learned at San Clemente Island, you had to put a buoy under that, going down to the main cable. That was the concept. Oh, one more thing. At the bottom end of the buoy, there'd be this long pipe going down to get the deep water to disperse among the plants.

At any rate, the AGA's head of research was named Ab Flowers. Flowers was very enthusiastic about this. He agreed to fund it. So they went into a planning phase. At this point they brought me in, and I said, "It won't work, because if you bring the nutrients up and this thing is anchored out there and a current comes along, the nutrients, by the time they get up to the surface, just disappear, carried away by current. You can put the plants on there, but they won't grow."

Erwin: Was this because they were out in the ocean, as opposed to being closer to the shore?

North: Yes. The currents are much stronger offshore than they are inshore. So Wilcox said, "OK, we'll let this thing just drift in the current and there will be a puddle of nutrients around it. We'll let it go that way for a day or so, then send a boat out to bring it back to where we started from."

Erwin: I see. So he had ideas. He was a big idea man.

North: Oh, he was a very brilliant man. At this point, Ab Flowers became very disenchanted with Wilcox, and he cut the program off.

Erwin: So that was that?

North: That was that. I still had NSF funds. We were continuing with our research on our little grid off the coast here. And one day two men came up and wanted to talk to me. They were from General Electric. They had been approached by Ab Flowers to take on this project.

Erwin: So he tried to hand it on to somebody else?

North: Yes, it was Wilcox he didn't like. He liked me; I was doing fine. He felt Wilcox was just too abstract and not able to envision what things were like out in the ocean. He was absolutely correct. Wilcox was very good theoretically. He just didn't have sufficient practical experience working in the complex ocean. [Tape ends]

WHEELER J. NORTH

Session 6

November 18, 1998

Begin Tape 7, Side 1

Erwin: Well, good morning. You wanted to add something more about the Santa Barbara oil spill in this session.

North: Yes. Well, I came away with a moral to the story. The oil companies were very relieved with my findings that there was almost no biological damage, but the people at Santa Barbara and the environmentalists were just furious, which totally blew me away. I didn't expect that. I would have thought they would have been the happiest of all. They weren't, because that meant that the results of the oil spill weren't as bad as everybody thought they were, and they didn't have quite the argument against oil drilling offshore that they would have had if the oil spill had caused a lot of biological damage.

Erwin: So that was the issue, really. This was a lever to work against the oil companies.

North: It was a lever. You can understand the feelings of the people in Santa Barbara. It was just a terrible mess, and anything that they could do to get rid of those platforms was fine. But I had a number of friends and associates in the public environmental interests, and they would hardly speak to me. [Laughter]

Erwin: Yes. Has that abated over time?

North: I became very disillusioned with the environmental activists at that point. And shortly after that, I got involved in the marine biomass project. So I really got out of environmental surveying, with a few exceptions in consulting work that I was doing with Southern Cal Edison and Pacific Gas & Electric Company.

Erwin: Well, did your disillusionment come from a feeling that there was a kind of radical or extremist view among many of the environmentalists?

North: Oh, there certainly was. There was another fraction of them that were really terrible. People would realize that this was an issue and that it was catching the public interest, and they wanted to ride the wave and acquire power. That, in my view, was just despicable—that the only interest you had in the environment was what it could do for you personally. And it was about that time that I had a set-to with the president of the Sierra Club. He was a lawyer. I felt he was very representative of this class of people who were taking advantage of the environmental movement.

Erwin: Did you ever have contact with other groups, like Greenpeace or some of the ones that have been noted for their extreme approach?

North: No, no, I haven't ever had any contact with Greenpeace. But there was a group of people who met irregularly. The leader was a woman named Ellen Harris. She was a member of the Los Angeles Water Pollution Control Board. They would meet from time to time to plot strategy on what they were going to do next for this project or to slow down activity. As one example, the Irvine Company in Orange County wanted to develop part of Newport Bay called the Back Bay. It was still as nature had created it, and they wanted to convert it into marinas and houses, where each house had its own pier and so forth. Really an upper-class type of area. I was the representative in Ellen Harris's group that could tell them about what was going on. I never took sides on it.

Erwin: I see. You could tell the environmentalists what was going on?

North: I did explain it to them. I gave them the scientific hard facts about it. At one time, there were twenty-three estuaries in Southern California, and now there were only two or three that were in their native state. But at any rate, I was dropped out of that group right then.

Erwin: I see. So your relationship with environmental groups has not been a happy one, largely. Is that right?

North: Well, it was up to that point in time.

Erwin: Up to the Santa Barbara oil spill?

North: Yes.

Erwin: OK. Well, is there any more about that, then?

North: I think that's what I wanted to have in the record—that I was very dismayed by the feelings of the environmentalists about my findings in the Santa Barbara Channel.

Erwin: Well, do you think the environmental movement is still off-center? Do you see any tendency to correct the abuses?

North: Perhaps with the exception of Ralph Nader, I don't see people getting into environmental movements just as a means of elevating themselves to positions of power. I haven't seen that anymore. The interest in ecology and the environment has died down among the public. It's still there, but it isn't what it was back in the early 1970s. There are a lot of very sincere people who like to have things as native as possible, and that's fine. But the activists, I think, were going too far.

Erwin: Well, perhaps that is correcting itself from inside the movement, which is probably how it has to be—that the people who are sincere have to make sure that they don't get taken over by the people who are radical, if you want to use that word.

North: Yes. Well, and by the people who are interested in environmental movements for their own advantage—to elevate themselves and get into positions of power.

Erwin: Do you follow some of the Green movement, for example, in Europe?

North: I'm aware of it, but I don't follow it closely.

Erwin: It seems to me that their environmentalism is aligned quite closely with political radicalism, the left, and so on.

North: Very much so. And I think the Greens in Germany are making a big mistake—they are so anti nuclear power. And that's the one type of power generation on a large scale that can produce power without creating CO_2, while the others use fossil fuels.

Erwin: Yes, right. Well, I was told by a German acquaintance that she receives pressure about even growing a lawn. She has friends in the Green Party, or whatever.

North: Oh, my goodness. That's pretty extreme.

Erwin: It is quite extreme, because you can't grow a lawn for various reasons. Water isn't so much a problem there, but no chemical fertilizers can be used. And I guess the natural fertilizers can be expensive or difficult. No herbicides allowed, and so on. Well, anyway, we should get off that topic, perhaps because I'm apt to talk too much myself. So let's go back to the marine biomass story.

North: All right. So two men walked in the door.

Erwin: Two men from GE, you said.

North: Yes. The project was essentially collapsing, because the American Gas Association representative—or the head of research, Ab Flowers—was totally disenchanted with Howard Wilcox. But I had the National Science Foundation grant, so we were still moving ahead. And

one day, without making an appointment or anything, two men from General Electric walked in the door.

Erwin: Now, what door was this?

North: [Laughter] At the marine lab. It wasn't the front door; it was the door to the shop.

Erwin: The back door, so to speak. [Laughter]

North: [Laughter] The side door. Apparently General Electric had been approached by Ab Flowers to take over this research project. And they would be in charge of it if General Electric took on the project. So they were on a reconnaissance, getting opinions from here and there. My opinion was considered very important, because I was the head biologist and the kelp expert. The essence of the project was to carry on with Wilcox's idea of putting a test module out—the upside-down umbrella structure—in order to grow plants on it, and to have nutritious deep water dispersed into the cultures. I explained to them that I thought it was an impossible experiment and that it would fail, primarily because when you get well offshore, there are strong currents, and this deep water would just be carried away before the plants could absorb much nutrients. It takes a little time for the plants to catch a nutrient molecule and haul it inside. So they were astonished, and they said, "Here is essentially one of the project leaders saying that the experiment won't work." So I said, "Well, that's my opinion." So they said, "Thank you," and disappeared. And about a month or two later, they reappeared and announced, "We're in this with you. We're going to go ahead with this modular experiment. And we need your help." By then the National Science Foundation grant was transferred to—I can't remember the name of the federal organization.

Erwin: Department of Energy?

North: Well, it [was one of the agencies that became] the Department of Energy. It was essentially the program that had been in the NSF to look at alternative sources of energy. I

remember somebody saying, "This organization is going to die, and like a phoenix out of the ashes will rise the Department of Energy."

Erwin: I see. And lo and behold!

North: Lo and behold. So instead of getting money from the National Science Foundation, I was getting it from the Department of Energy. And the director, the research manager, was not the same person who had been running it at the NSF. It turned out that the DOE people were very interested in the Caltech project. They wanted to keep it separate from the General Electric project in the way of funding, but nonetheless we were supposed to work together. Caltech was not to take money from the Gas Research Institute. It was to get funding from the DOE.

Erwin: But all the while you thought this project wasn't going to work anyway.

North: Well, what they told me was that they were going to hang a curtain around the farm so that the nutrients would be in their own little puddle and stay there.

So I said, "Well, OK, let's go ahead," but I still had my doubts. My doubts proved correct. [Laughter] They hung the curtain out before they ever got the thing in place. They got a lot of oceanographic data for the region and did a lot of calculations. The curtain was just to be fastened at the surface with a cable. It would hang down and there would be weights at the bottom. They decided that some of the currents were so strong that they couldn't possibly hold that curtain with the anchorage that was humanly possible to produce. So they made allowance for the curtain to come up during the very strong currents. But that would mean that, say, thirty percent of the time, roughly, plants wouldn't be getting nutrients. So I said, "Well, that's too bad, but maybe it will still work." So the project went ahead. The module was put in.

It was a very skillfully engineered project. They constructed a 1,500-foot-long pipe in Dana Harbor and, 50-foot section by 50-foot section, they welded these big, huge plastic pipes, which are about 3 feet in diameter. They welded them on one end and then pulled them 50 feet out into the harbor and welded on another, until they got their 1,500 feet of pipe. And then they

towed that out to the farm and had a big anchor on one end. It was very skillfully done. I was very impressed. Everybody was. It was a difficult project, and done very professionally.

Erwin: Now, who were the people who did the engineering? Were they employees of General Electric?

North: General Electric had a subcontract to Global Marine. And Global Marine did part of the engineering, particularly the parts that the General Electric people didn't have proficiency in. That included the curtain.

Erwin: Yes. I suppose that was the main marine element.

North: Yes. I don't know who it was that calculated that they couldn't keep the curtains down there a hundred percent of the time. It might have been Global Marine, it might have been GE. Global Marine had very high-quality people. So everything got in place [1978]. I think it was 103 kelp plants. We collected them from the Laguna Beach kelp bed. The module was about six miles offshore from Laguna Beach, which is far better than having the structure that Wilcox had out at San Clemente Island, where you had to wait for a good day and then run for two hours to get out there and do a little work and then get back before the afternoon winds came up. So here was the experiment, just out ten miles from our laboratory, which was great. The curtains would come up more often than calculated but, even so, that wasn't too bad. Much worse, after the first storm that came along, there were no curtains left. So from that point of view, we were back to square one. But then—even much, much worse—the module was tearing apart the kelp plants. We got one series of growth-rate measurements from them. I had a system for measuring the growth rate in adult kelp plants from my old days at Scripps; different plants will grow at different rates, so you have to get a statistical picture. So we got one set of growth rates before the curtains disappeared. Let's say there were 200 measurements, two each on each plant. Most of the measurements were just in a normal statistical bell-shaped curve. But right out at the extreme end were three plants. Ordinary plants were growing at about seven percent per day, but there were three plants that were about

thirteen and fourteen percent per day, which I had never gotten. I had droves of divers doing this work for me, and a postdoc in charge of them. The DOE was giving me a lot of money. The postdoc's name was Valrie Gerard. Valrie and I decided that those three measurements were questionable and that it just might have been an error from these divers, who had not done much of this kind of work at all—they may have just made the wrong measurements. So if it happened again, why, we would begin to take it seriously. But it never happened again, because the curtain was torn loose and the module ate all the plants.

So the head guy at General Electric called me. His name was Al [Alan N.] Tompkins. He phoned me after I sent in our report and he said, "What about these three really great growth rates?" And I explained to him that Valrie and I hoped that they were good measurements but that we weren't ready to stand behind them. And Tompkins said, "Well, there is a lot of money waiting to come into this project." And about those hundreds of millions of dollars he was talking about, if we could verify those growth rates, why, we'd be off and running. But, as I say, the module curtains were torn off and plants were destroyed. The module went in in September. By the time they got the pipe in, and the curtains and everything, it was mid-November, and then the December storm hit it. So we didn't want to put any more plants out there during the winter, particularly without a protective curtain. So that part of the project came to a complete stall.

Erwin: I see. Did you feel that you were being pressured, actually, to approve those measurements in a way that you just couldn't, ethically?

North: Well, no, no. Al wasn't pressuring; he was just explaining to me the importance of it, I think. He was not trying to get me to change my scientific judgment, not at all. The pumps that were bringing up the deep water began to fail during the winter. There was a curious phenomenon: Even though these pumps were pumping hard, some of the plants got sucked into the pipes over in the water. What happened was that, when the module would be carried up in a wave, the water, instead of coming out the pipes, would go back in, because the pressure had changed. It sucked some of the fronds from our plants into the pipes. So that had to be fixed, which was very easy; they just put a little cap over the pipe so that it would open and close. But

it took General Electric almost three months to put those caps on—to design it properly and get it built and everything.

Erwin: So did you get back to putting more plants out on this rig?

North: We never put any more plants out. The stormy weather usually continues through March and into April, and suddenly there were baby plants all over the module. The spores had come off the adult plants and landed on the solid structures of the module, and little baby plants were sprouting all over.

Erwin: And did you expect this? Was this a surprise?

North: Yes, this was completely unexpected. So of course General Electric and Ab Flowers were just really turned on by that. They finally got a machinery specialist in, who got the pumps [working]. I don't know if they changed the type of pump, or the engine that was running the pump, or what. But it was a lot of work. I can remember him spending hours in that thing, rocking back and forth and going up and down. He said it was the worst job he had ever done. He was a very nice person.

Erwin: And this was all underwater?

North: No. This was up in the umbrella handle. Way up high—twenty feet above the water.

At any rate, about this time I felt that we needed to get a little more information about the plants. I was probably the only one who got on the module in a storm, and it was just like being on a bucking bronco, just up and down and up and down.

Erwin: So you dove to do this?

North: Yes. These spars were sixty feet underwater. I said, "This isn't conducive to good kelp growth. I think we need to do a little bit of experimentation." So I thought, Well, instead of

getting back on this module, where there are all kinds of cables and everything.... The module had three big steel cables going out to buoys; it was in what's called a three-point mooring. So I went out along one of those cables, and I had my group fasten a buoy. The cables were pretty deep out there, so I had the buoy at a depth of about fifty feet. And it would go up and down. The buoy was a beer barrel. [Laughter] They make wonderful buoys, because they're built to withstand pressure. And they are stainless steel. You can buy them for peanuts, compared to what a real marine buoy would cost; they cost about thirty bucks each. And I had three of them out there with kelp transplants on them. We studied the movements, and sure enough the buoy would go way up and the top part of the plant would trail down. The plant moved with the water and not with the buoy. So it would get down here, but then the buoy would go down and the plant would slowly adjust and the buoy would go back up and the plant would trail down. Well, I was very lucky. The experiment showed that if you were on this module, with cables going everywhere and all kinds of stuff, when the module comes up, the plants get entangled in the part that would be beneath them if the water were calm. And that was what was destroying the plants. And General Electric, about this time, fired or got rid of Global Marine. They figured they had enough expertise on their own. They got very interested in this experiment.

Erwin: So what you're saying is that the plants on the buoy were doing all right, even though they were flopping up and down.

North: Yes, because they were on an isolated buoy and there wasn't any construction around them in which they'd get tangled. By this time, a year had gone by, and another catastrophe happened to the module. It became a biological zoo. All kinds of little larvae were settling on it.

Erwin: Of what sorts?

North: Marine larvae, including sea urchins. The sea urchins gobbled up all the juvenile plants.

Erwin: Yes. Back to the same old story.

North: Another catastrophe happened to the Caltech group. The person at DOE that was supervising the program was demoted, and they switched the funding. At that point they had so many activities going on that they couldn't handle it in Washington, DC, so [in 1979] they hired a private group called SERI—for Solar Energy Research Institute—which was doing energy studies. They were based in Golden, Colorado. They asked SERI to manage this kelp project. By the time SERI got it, my experiment showed that the module was not a good way to grow kelp. So SERI started thinking about the problem and suggested several other types of structures. Again, I was the bad guy, because I explained why they wouldn't work. At any rate, finally the DOE cancelled the whole project.

Erwin: On what basis, finally? That it just wasn't productive or wasn't doing what was hoped?

North: I don't know what the basis was. I never had any contact with the DOE people. It was the SERI people. And they said, "Well, sorry. We're not getting any more money." But Ab Flowers picked up the funding for Caltech, so we were still in business.

The big scientific question we were trying to answer was, What is the productivity of kelp when it has plenty of nutrients? So I and my postdocs—at that point I had four postdocs—urged General Electric to forget about the module for right now. [We suggested], "Let's go out to Catalina Island in fairly shallow water, in a harbor where it's protected, and have some kind of a large container and just put nutrients into it and put kelp transplants in there and get the answer to this important question: What's the best productivity of kelp?" We had good reason [to wonder about this] from some of my observations of kelp in nutrient-rich waters up by the Diablo Canyon Nuclear Power Plant, where I was a consultant. People were saying, "This project can't possibly go, because the productivity of kelp will be so poor. It won't be economically feasible, because the engineering will be so costly to build whatever the structure is they decide on." It was a very good question to ask.

As part of the project, we had a big investigation going at Kerckhoff marine laboratory, studying what nutrients the kelp needed. These were small plants held in aquaria. We determinted the nutrients needed, how fast plants could take up nutrients, how much light was

necessary—just studying the basic biology of the plants. And we did a tremendous amount of work which was very good—first-class work, I felt. I'll pat myself on the back. A lot of the results have been published, but just a small fraction of the data. It's in those data books that I brought up to the Archives.

So we had the information. GE got interested in this concept. We had the information on how many nutrients kelp needed and, given the size of the impoundment, how much water you'd have to pump into it and how many nutrients you had to bleed into it, and so forth. We calculated all that out, feeling great confidence in it. I remember when we made the final decision to go this way. It was [at] a big meeting of Caltech biologists and the top General Electric people. I remember Al Tompkins saying that he was just uneasy that we wouldn't be working out on an offshore structure, because that was, after all, the basic thing that we were going to have to prove—that we could grow plants on an offshore structure. I had done that already, but not on a big one, just on a small grid. At any rate, he voiced his concern, but then he went along with the concept. And General Electric had devised a very ingenious kind of an impoundment: It was a floating donut—a big ring, fifty feet in diameter, and it supported a plastic bag shaped like a half a sphere. It was called a hemidome. [Tape ends]

Begin Tape 7, Side 2

North: Anyway, so here is a hemidome, a hemisphere fifty feet in diameter, and it's held by bungee cords to reduce the amount of motion. We were in USC's marine station. I can't remember the name of the harbor, but it's very nicely protected. It's on Catalina Island, on the shoreward side, so we weren't exposed to the ocean waves. And it's in this harbor, with only a narrow exposure in one direction. That direction was where the Santa Ana winds come from, but GE felt that this structure would do well in any six- or seven-foot waves. People at the marine lab said that there were never any waves of that size that came in, even in Santa Ana conditions. So by January 1982 the hemidome was constructed and installed out at Catalina Island. But then they had to put in electric cables to shore, to get electricity to run the pumps and instrumentation and so forth.

Erwin: So the kelp would be growing in the bag?

North: It would be growing in the bag, which would accommodate fifty kelp plants. In the center they had a pipe going down and then four little pipes coming out. It looked like a swastika; the pipes came out and turned the corner, so it looked like a swastika. The idea was to produce a slow circular motion so the nutrients would be moving over the blades of the plants—so there wouldn't be any limitation due to kelp leaves depleting the nutrients in the center of the bag. It was beautifully designed. I think it was in late April that we brought the first fifty plants in there. It ran for, I think, six weeks, and we harvested the blades of the top canopy. The prediction was that we would do well if we got one dry ash-free ton per acre per year. It actually came in at about thirteen—or maybe it was ten—from that first experiment.

Erwin: Oh. So it was very good then.

North: It was well beyond what some skeptics had said would be possible. So by this time, about June 1982, the biggest El Niño of the century hit. And the General Electric people had said, "There's not enough power available on the island to refrigerate this water. It will just have to live with the temperature of the water that comes in. We can put pipes down." But the deepest point of the cove, where we were anchored, was forty feet.

Erwin: Which is not terribly deep, is that what you're saying?

North: That's not terribly deep, but it would work in an ordinary year, not in an El Niño year.

Erwin: What was the difference in the temperature? How many degrees?

North: In the summertime, the difference between an ordinary year and an El Niño year was about one or two degrees centigrade, but the water didn't cool off in the winter. Normally in the wintertime the difference could be five or six degrees centigrade. So all the kelp around there began rotting and disappearing.

Erwin: That's what happens to it? It just rots?

North: Yes. That's because it doesn't have nutrients. Let me just say again that in the ocean, water temperature and nutrients are inversely correlated: when the water is warm, the nutrients are down; when the water's cold, the nutrients are up. And to explain why this is so would be a half-hour lecture.

Erwin: OK.

North: So here's all this warm water. We had nutrients in the bag, but what happened was that the kelp began rotting anyway. What we were doing was getting a bacterial culture growing in all these nutrients in warm water. So the kelp very quickly began to get what we call shot holes in it. It looked like someone had taken a shotgun and hit it—it got holes and the blades broke up and fell to the bottom. The bottom was just rotten with all this stuff. So the hemidome experiment failed.

Then we thought, "Well, wait till winter comes along. Maybe the water will be cold enough." Our kelp plants did last longer than the natural kelp plants around. But it was about October of '82 when a big Santa Ana wind came along and split the hemidome bag. But by then the geologists and oil and gas specialists had figured that there would be plenty of oil and natural gas available—no shortage for decades, maybe centuries. You will recall, with Wilcox, that this biomass program had started because of the gasoline shortages.

Erwin: That's right, which was almost ten years before this.

North: Yes. So the Gas Research Institute decided to cancel the project after the bag split open. They were very good to us; they continued with one year of funding so that all the people could go out and find employment elsewhere. I never had a funding organization be so considerate of the personnel involved.

Erwin: So the lesson learned from this project was that there was not a biological problem, really.

North: Yes, that's right. I should go back to this module out six miles offshore. It was a very interesting biological experiment, because things began growing on the module and on the plants that we wouldn't find in the kelp beds—animals that were very rare in the kelp beds. Some I had never seen in local kelp beds; you could find them up north around Monterey. Sometimes they'd appear in abundance—hundreds and hundreds of them. So it was biologically a very interesting place. I think Al Tompkins's little worry that we weren't working out at sea was probably valid. If we had gone ahead and designed the module with, say, these buoys ten feet up, so that when the module came up the plants wouldn't contact the structure, then we could have shown that kelp can be grown out in the open ocean on a modular structure.

Erwin: I see. So that was never successfully done.

North: That was never successfully established. The Gas Research Institute, at the point when they were very interested in the program, funded an additional kelp project at the University of California at Santa Barbara, where a professor named Michael Neushul was. And I had hired Mike when he was a graduate student at Scripps. I had hired him on our kelp study back in the late 1950s, so he and I were very good friends. I had suggested to General Electric that it might be a good strategy to not have all the money going into one kelp program—to have another kelp project going. So the Mike Neushul group also constructed a grid of plants. The plants were anchored to the bottom with bags of gravel. The experiment was done in forty feet of water. And they had a square with 300 or 400 transplants at different densities. And Santa Barbara waters get much more nutrients than we get farther south. So the plants had pretty good—I won't say perfect, but pretty good—nutrition. And Neushul got several productivity figures versus density, and they confirmed what we had found with the hemidome—that the productivity is on the order of ten or more dry ash-free tons per square acre per year.

So that was the end of the biomass program. It was just a fantastic program the ten years or so that it ran—twelve years, actually.

Erwin: Well, you mentioned that you had some interaction with China.

North: Yes, toward the end of the biomass project. Every three years there's a conference called the International Seaweed Symposium. And each conference is held in a different country. About 500 people typically attend it. In 1983, the seaweed symposium was held in China, in a city called Qingdao, which is in a harbor—it's on the coast. All the Orientals are way ahead of the Western world in cultivating seaweeds.

Erwin: For industrial purposes, or food?

North: Both. Many of the Japanese dishes have nori, wakami, and various other seaweeds that have Japanese names. And in China they used to have enormous cultures of a small kelp—the stalk is maybe four inches long, but the fronds can be five or ten feet long and very broad. So they have these farms. It was very interesting to go over. Val [Gerard] presented a paper on the hemidome work, and I presented a paper comparing the plants inside the hemidome with natural plants that were immediately outside. It was very well received. And it was very interesting to be in China and see some of these kelp farms.

Erwin: So how were they doing their kelp farming?

North: On our Pacific Coast, the continental shelf slopes rather steeply, so you only have, say, half a mile or a mile of shallow water to work in. In China, the continental shelf goes out and out and out, so they can work in shallow water for several miles. It's called rope cultures. The tiny spores are attached to ropes, and then they put the ropes out at sea. They have anchors with cables coming up to submerged buoys, and then the ropes are held between the buoys. These units are maybe twenty feet apart. And they have buoys on the surface, so that after the plants are full grown they can bring them up and harvest them. I remember standing on the shore in a

city called Yantai, which is a big production area. And looking as far as I could see upcoast and as far as I could see downcoast, there were just buoys, as far out as you could see. It was just an enormous activity.

Erwin: And how long had this been going on? It sounds like a rather simple concept, really.

North: Yes. It was pioneered by the Japanese, actually; the Japanese have been cultivating various seaweeds since about the 1600s. A young Chinese marine botanist named C. K. Tseng came over to get some education from an American university. He came to Scripps Institution of Oceanography—I think it was around 1939 or 1940. He got trapped over here by the war.

Erwin: Before the revolution, in other words.

North: Yes. He got his PhD at Scripps and then stayed on doing experiments with seaweeds and learning how to culture them. After the war was over, he went back to China. He wanted to go back to his homeland. He had learned about the Japanese activities and how they culture seaweeds. So he took this information back to China. And they got some of the Japanese kelp and began cultivating it. He was responsible for the whole huge activity that developed in China. One of the things that kelp has in it is a high iodine content. The soils in Asia and that part of the world are very poor in iodine, so lots of people had goiter. So everybody in China was very pleased to have kelp available in the market to take care of this problem. And in addition, there's some nutrition from the kelp itself, besides the iodine.

Erwin: Yes. So people were just eating it as an additive or a vitamin sort of thing?

North: Yes. Well, they make soup out of it. You'd get it in salads, and so forth.

Erwin: This would be a commodity you would see in the markets routinely. Unlike in the United States.

North: Yes. At any rate, the Chinese government has an organization corresponding to our national marine fisheries. And there are various stations up and down the coast. The station in China in Qingdao was called the Yellow Seas Fishery Research Institute. And two scientists from YSFRI [J. Chen and R. Suo] had gotten some giant kelp [*Macrocystis*]; they had come over and collected it themselves from a place in Baja California.

The Yellow Sea gets very warm. It's tropical water in the summertime. So all the kelp they brought back and tried to grow died in the summer temperatures. But they still had it propagating in the laboratory. They grabbed me from this symposium and told me their problems. So I asked them where they had gotten it from, and it was from a place in Baja California called Santo Tomas, which is about, oh, seventy miles on the coast south of Ensenada. And it's an extremely cold-water area. So I told them that there are various strains of kelp that seem adapted to the different water temperatures, and that what they got was a very cold strain, and that I knew of another strain that Mike Neushul and I, when he was a graduate student, had studied a little bit. It was in Turtle Bay in Baja California, which is about 600 miles south of the border. And it seems to grow fine in summertime water temperatures—twenty to twenty-two degrees centigrade, which was available in some parts of the Chinese coast. So they were very interested. We had a correspondence. And as the marine biomass program was ending, I got approval from people at GRI and GE to go down to Turtle Bay and get some of these plants and bring them up to Newport Beach to get them in a culture so that the Chinese could come over and get spores, which they could take home and cultivate.

The first trip down was in the fall of 1983, and we didn't find anything. The tremendous kelp beds that had been around Turtle Bay were just gone. All we had was a small boat, which was what I thought would be all that would be needed, and we talked with the natives there. They said that at the onset of the El Niño back in 1982 there had been horrendous storms. We had them here in Southern California; they caused all kinds of damage. And it had torn out the kelp beds completely. "But they will come back, Señor. Come back another day." I thought, Well, what we really need is a larger boat to look in other places. We might be able to find some plants. So we made a second trip, bringing down our large boat, the *Osprey*. And sure enough, as I suspected, I went into an area where there was cold water—but not the tropical water that can happen there—which would have had some lee from the storms. There was no

kelp on the surface, but there were plants growing on the bottom. So we got some kelp and brought it back and I got it in culture at the marine lab, and I wrote to my friends at the Yellow Seas Fishery Research Institute. It took a year for the scientists at the Yellow Seas Fishery Research Institute to get permission from—I guess the word had to go up to Beijing and back. And El Niño was continuing, and my plants were suffering. We got a telegram from them that they were on their way. I think this was in October of '84. About four hours after the telegram arrived, I was working up at my house and I got a call from the marine lab saying, "There are two Chinese people here asking for their kelp." So I went down.

Erwin: So the telegram was slower than everything else. OK.

North: [Laughter] I went down. There were the two people who had been leaders on this project.

Erwin: And you knew them?

North: I knew them. They were exhausted, so we put them up in one of the little apartments in the lab. They stayed about two weeks. But we had an accident. I brought in the last plant that I had—the spore-producing, reproductive part of *Macrocystis*. I gave it to them. I had it in chilled seawater. I explained to one of them that out of this hose you get seawater and out of this hose you get fresh water. And he thought "fresh water" would mean that the seawater was much more fresh. So they put fresh water into the petri dishes and it killed everything.

Erwin: And that was your last plant?

North: That was my last plant. But GE, in funding my trips down to Turtle Bay, had said, "Give some plants to Mike Neushul." And he had them in culture, so we went up to Santa Barbara and Mike gave us some spore-bearing blades. We took them back and the Chinese got them in culture. And they were here about two weeks, then off they went. And they got their culture to China. Then in '85 I got a letter from YSFRI, inviting me over to give a series of

lectures and to consult on the seaweed project. I went over for three weeks and gave a bunch of lectures, and then we went on a trip up to the Bo Hai Sea. They weren't trying to cultivate the *Macrocystis,* our kelp, in the Yellow Sea. It was just too warm. They were doing it in a little inlet called the Bo Hai Sea, which is in the northern part of China. It's close to South Korea. They couldn't get permission for me to go in and see the plants that they had, because it was on a military island and they didn't want a foreigner being on that island. So the Chinese went and got the kelp and brought it back to us on shore. This was at Yantai.

Erwin: OK. Now, this is where all that cultivation had been going on for a long time?

North: Yes.

Erwin: So what did this add to their program?

North: The reason they wanted to culture *Macrocystis* was, first of all, you don't have to have these huge structures for [their kelp,] the *Laminaria*. And after you harvest the *Laminaria*, you have to start the culture over again. With *Macrocystis*, it comes up to the surface and forms a canopy, and you can just harvest the canopy, like we do here in Southern California, and the rest of the plant grows a new canopy. So it's much easier to cultivate and harvest.

Erwin: I see. So it was a different kind of kelp, then, from what they had previously used.

North: That's right—their kelp was called *Laminaria*. And C. K. Tseng has an article describing the Chinese culture of it.

But the *Macrocystis* plants that they brought over to Yantai didn't look very good. And they were having trouble. The problem was that the Bo Hai Sea gets freezing in the winter, and in the summer it gets up to twenty to twenty-two degrees centigrade. And what I was looking at was kelp that had survived this warm water. From the culture work I had done at Kerckhoff marine lab, I knew it should survive the winter, and it apparently had. But we agreed that something would have to be done about the summertime. There was another reason why they

wanted California kelp. This is the story they told me: If you feed kelp to chickens, the composition of the eggs from the chickens would be a little different and would help reduce blood pressure. I asked C. K. Tseng about that. He said he thought it was a fairy tale. He didn't think much of the scientists at the other fishery research institute, and I think he was dismayed that I had agreed to do some experiments with them. He was very polite; he never said anything; but I just had the feeling. At any rate, they found—when they had gotten this *Macrocystis* from Santo Tomas and tried feeding it to chickens—that the eggs were much better at reducing blood pressure. That's another reason they wanted *Macrocystis*.

Erwin: What did you think of Chinese biology in general? Were you able to form an opinion about that?

North: Yes. I think they had very competent biologists. Now that the Chinese are opened up to some extent to the world, we get some feeling of what has been done. They've just done remarkable things in aquaculture.

Erwin: Did you have a chance to observe anything beyond that? In medicine, for example?

North: No. I didn't have a chance. While I was in Yantai, I visited one of the laboratories where they start the cultures of their kelp, *Laminaria,* on ropes. And that was very interesting, and I wrote a paper on it with Chen and Suo as coauthors. ["Cultivation of *Laminaria* and *Macrocystis* (Laminariales, Phaeophyta) in the People's Republic of China," with T. Liu, J. Chen, and R. Suo: *Phycologia* 27 (1988), pp. 298-299] I think they're still cultivating kelp there, but it's not—

Erwin: But the California kelp wasn't completely successful?

North: Well, they can continue it, but they have a very long summer, so they can't keep it in the field. They have to have the reproductive leaves in the laboratory, keeping them cold and in the nutrients. And then, after the water cools off in the fall, they put new baby plants out there.

Erwin: I see. So they have to keep replanting all the time.

North: Yes, just like their *Laminaria* cultures. So that was a lot of fun, and very, very interesting.

Erwin: Did you get to travel much? Not on your own, presumably.

North: After the seaweed symposium, they had excursions. And I took one of the excursions for several days, which ended in Beijing. We saw museums and temples. One day they had an excursion up to the Great Wall and the Valley of the Kings, where the emperors' tombs are. It was all fascinating; the Chinese certainly were doing remarkable things centuries and centuries ago. I learned a little bit about their history.

Erwin: It seems that during this time Caltech had a fair amount of rapprochement with China. I know the president, Murph Goldberger [1978-1987], was very interested in China. Were you aware of that? Because there was a big influx of Chinese visitors to Caltech starting in those years.

North: Yes. There was a foundation called the Durfee Foundation, which was funding American scientists who had connections with China. They funded American scientists to go over and work in China. And I submitted a proposal. They approved my project. I can't remember why I didn't accept the grant. I think by that time I had made the trip to China and decided that I really couldn't help them anymore. They put me up in the best hotel in Qingdao, but one night I was sound asleep and there was something crawling in my ear—it was a cockroach that had fallen off the ceiling. [Laughter] But the accommodations in Beijing were very up-to-date and very nice.

WHEELER J. NORTH

Session 7

November 24, 1998

Begin Tape 8, Side 1

Erwin: Well, good morning. Let me ask you today about some of your consulting work. One of your projects was at the Diablo Cove nuclear power station.

North: Yes. We had a grant for one graduate student who worked from there, and then I did a tremendous amount of work consulting up there.

Erwin: Where is that, actually?

North: It's near Los Osos, in central California, which is near San Luis Obispo. There are two reactors, owned and operated by Pacific Gas & Electric Company. Early on in my career at Caltech, two men walked through the door one morning without an appointment or anything—they were lucky to catch me here. One was the director of research at Pacific Gas & Electric. They had recently hired a very young biologist named James Adams. They were very interested in taking me on as a consultant.

Erwin: Did they have a problem at this point?

North: No. They were looking ahead. The property had been purchased and the decision had been made to build a nuclear station there, but they foresaw that there would be biological questions. And it would help the young biologist if they had an experienced marine biologist as a consultant to work with.

Erwin: And what year was this?

North: This was 1966, three years after I arrived [as a faculty member at Caltech]. So I agreed; it sounded like very interesting work. Most of the work that I did for them was at Diablo Canyon, which is a very picturesque part of the coastline. At that time it was completely in a natural state, because the big landowner—it must have been 10,000 acres of coastline—had used it for farming and raising cattle and things like that. But it was fenced off, and he had guards with guns to enforce his property rights. I guess he needed some money, so he leased the Diablo Canyon site, which was more or less in the center of his property, to Pacific Gas & Electric Company. It was a long-term lease, of course. Eventually Pacific Gas & Electric bought most of the land. I guess maybe the old man died and the heirs wanted to sell it. I don't know what the situation was. It was absolutely a fascinating place, because it was completely natural. Pacific Gas & Electric was at the same time considering a number of other coastal sites, so I did work up near Davenport, about an hour's drive north of Monterey, and about half an hour north of Santa Cruz.

Erwin: Now, about how many nuclear power plants, if any, were on the coast of California at that time?

North: Well, at that time Edison was in the process of construction of their first small nuclear power station at San Onofre, south of here. They had hired me as a consultant in 1963. So it was perhaps through that that the people at Pacific Gas & Electric heard of me. I was beginning to get a good reputation as a person very familiar with the marine part of California. And then a third site—a very controversial site—was up at Point Arena, which is in Northern California. Pacific Gas & Electric wanted to build a nuclear power plant there. PG&E also had a site up in Humboldt Bay—a very small station. Similar to Edison, they built two very small plants to get experience operating them before they went into the really big power plants. So PG&E had me diving at four different locations along the coast. Three of these were where they were considering building power plants, and one was this small reactor up in Humboldt Bay.

Erwin: And your task was to do what?

North: At the one site north of Monterey and at Point Arena, it was to do background biological surveys.

Erwin: And then you would give a report on the existing—

North: And at Diablo Cove they wanted a background biological survey. And I knew both the intertidal and subtidal. I could draw a map of what the principal species were and what their distributions were.

Erwin: You weren't looking at the land formation necessarily. It was just the biological part?

North: To the extent that the land formation might influence the animals and seaweeds that were there—whether it was cobble or sand or solid rock outcrops or boulders or whatever. PG&E never built at the Point Arena site. It turned out there was a very active fault near there. And they also decided against Davenport, up from Santa Cruz. So most of my studies, ninety-five percent, were done at Diablo Canyon. The cove was about, oh, 1,500 feet across. And there was a big rock in the entrance, projecting up about 150 feet. It was spectacular, and the coastline was just beautiful. The intertidal was very rich in marine organisms, and the shallow subtidal had a good deal of diversity. But the deeper parts of the cove were totally dominated by sea urchins.

Erwin: Oh, our old friends.

North: Our old buddy the sea urchin was really dominant there, and the rocks were barren, because the sea urchins were very actively grazing anything that came along that they could catch. This was back in 1966-67. I think it was in the early seventies that the sea otters, which prey on sea urchins—they had been hunted almost to extinction for their fur in the eighteenth century, but a little colony of them near Monterey escaped being slaughtered, and that colony grew and grew. They started moving south.

Erwin: Were they then a protected species?

North: They were, and still are, regarded as an endangered species. They were indeed protected, but now and then a carcass of an otter would wash up on shore with bullet holes in it. One of the prey of otter is abalone, and they just completely wipe out a region of abalone. And there are lots of professional abalone divers along the coast, and they hated the sea otters. But at any rate, the herd expanded to several thousand animals, and they moved south. And pretty soon they moved into Diablo Cove, around the early to mid-seventies. They cleaned up every single urchin there. So I got to see them and describe a situation where the urchins had been dominant and then the sea otters took over and new things returned. So that was a very rewarding study. Then finally the two reactors became operational in 1984 or 1985. So I stayed on until 1989 doing studies of what happened. What happened was quite different from what was expected to happen.

The cove was a deep cove, and here was this big rock in the middle. And the discharges were designed to go out through the south channel, between the rock and the mainland. They were oriented that way because there was very deep water there, so this warm water could get out into the open ocean with a minimal impact on bottom-dwelling animals and plants. The water was warm down to a depth of about ten feet or so, maybe a little more. So there was just this short section of bottom that received contact with this stream of heated water going out.

Erwin: Yes. And that's principally what was coming out—the heated water? With some chemical components in it?

North: They had a sewage treatment plant there, because they probably had at least 1,000 people.

Erwin: Just for the little community of workers at the power plant.

North: Yes. And the sewage was secondarily treated and then put into this river of water. It was a mighty river. And the sea otters loved it. They'd come and swim inshore from the side and get into this plume and ride it out and then get off and come back and ride it out.

Erwin: Now, why were they doing that? Were they finding something to eat, or were they just having fun?

North: Oh, they're very playful, inquisitive animals. The sea otters arrived before the power plant was completely built, and there was concern that this might be harmful to them. Instead, it proved to be a fun park for them. [Laughter]

But at any rate, as I say, the discharge was designed to have all that water go out this deep channel. Unfortunately, at low tide a substantial part of it would turn north. And it went more or less along the coast. The reason was that in the shallow subtidal, for quite a ways, there were jagged bedrock formations, and at low tide they would block this water and turn it toward the north.

I had a team of two other biologists to work with. And any time we had a special need for somebody.... At first there was just Jim Adams, but as the project went on, they hired a whole staff of biologists—I imagine thirty or so. We had a special laboratory for them. And Pacific Gas & Electric started doing their own research work, and they hired a consulting company, Tenera, to do studies. And they had geologists and oceanographers also. So the whole place was very intensely studied.

Erwin: And you were an independent contractor, essentially.

North: I was an independent contractor.

Erwin: But you didn't have any trouble interfacing with all these other people?

North: Oh, no, they were all wonderful people. So what we found was that there were severe biological effects on the north side of the cove. I think PG&E was a little uncomfortable. I had

complete freedom to go where I wanted, report what I wanted, and publish what I wanted. But they got a little nervous when they realized that there was much more biological damage than had been anticipated. At any rate, I did publish with my two biological helpers one paper on what we saw in the subtidal. [North, W. J., E. K. Anderson, & F. A. Chapman, "Abundance changes in *Laminaria setchellii* and *Pterygophera californica* (Laminariales, Phaeophyta) Near the Diablo Canyon Power Plant." *Hydrobiologia* 204: 233-239, Sep. 28, 1990.] Maybe someday I'll get around to writing up the studies we did in the intertidal. [Laughter]

Then one of the other biologists, from Tenera, at one point was studying the shallow region of the discharge. And some of the plants there suddenly just started looking very sick. I think Tenera was hoping to get some extra money to study the cause of it. Instead PG&E asked me for a proposal to see if we could find out the cause of this mysterious malady. I think this was at the time just before the power plants became active, so the discharge was there, but they weren't putting out hot water yet. I shouldn't call it hot water. It's about ten degrees centigrade above background. If there are lots of cold-water species there, that's enough to drive them away. But there are also warm-water species, so presumably they would proliferate in the areas vacated by the cold-water species. That was the gist of our finding. But at any rate, here were two groups of plants, coralline algae and then some brown algae species, that suddenly looked very sick. A few weeks before this situation was observed, there was a point when the flow was very low. They had shut off most of the pumps, and there was just a slow movement of residual water going out. And during that period they had put into the stream some hydrazine in dilute solution. Large boilers develop calcium carbonate scale on the inside, and hydrazine prevents that. But you have to treat the boilers fairly often, so the plant had dumped some hydrazine into the outgoing stream when it was very low. So that was the suspect, because it's a very toxic substance. And there were no state regulations as to allowable levels of hydrazine. I imagine the state has since corrected that, but at that time the engineers were not required to be careful when they disposed of hydrazine.

Erwin: This is interesting. Did the engineers have suspicions about this sort of thing? That there could be a toxicity problem?

North: No. They didn't have any training in biology. This was just their routine task.

Then the head biologist told me that he made a report on the damage to the Regional Water Quality Control Board, the state agency that monitors these things. He told them that Caltech had received a grant to study it, and the executive officer of the control board told the biologist, "Well, if Wheeler North is studying it, we won't worry about it." [Laughter]

Erwin: That was a nice compliment.

North: That was a very nice compliment. I had done some other studies along the central California coast at outfalls, so the pollution board was familiar with me.

Erwin: And confident in you.

North: They felt that I was competent. [Laughter] At any rate, David James established the toxicity level that was—

Erwin: He was your grad student?

North: He was my graduate student. He established the toxicity level at which these things would have problems. And it looked very plausible that it was the discharge of hydrazine that did it. And it was a very minor amount of damage. There were only a few of the dozens of plant species that were affected.

Erwin: And easily correctable, I'm imagining, once it was understood.

North: Yes. You'd just have to be careful when you did the hydrazine cleaning of the boilers—to do it at a time when there was a tremendous flow of water going out. So that was a very successful program.

Erwin: Would this be a typical problem of a reactor station? Or were there no typical problems? A question of establishing a level of dilution at which you would need to proceed if you were going to let substances flow out of the reactor?

North: I would say that this was an unusual problem. The state and the Environmental Protection Agency had funded many studies of toxic levels for many of the different industrial chemicals that are used. It just so happened that nothing had ever been done with hydrazine.

I was a little dismayed when Pacific Gas & Electric Company decided that I had done enough for them. In 1989 they discontinued our studies. I was getting very interested in what would replace the species that were disappearing. But, as I say, they had a lot of biologists up there, so they bid me a fond farewell, although on very friendly terms. One of the biologists told me, "I think they're worried that you'll get out in Diablo Cove and drown someday, and they don't want it to happen on their property." [Laughter] So that may have been one factor, because I used to go out in weather that none of the others would dive in.

And Southern Cal [California] Edison—I did studies on their San Onofre station. There are big reactors there.

Erwin: Yes. Of course, they are familiar to most people who travel that highway.

North: You can't go by there on Highway 5 without seeing them. I did the background study before any work commenced in 1963. It's a rotten place to dive. [Laughter] That's the best way to describe it.

Erwin: Oh, is that right? Why is that?

North: The water is very turbid. Oftentimes if you put your hands out, you can barely see them, but that's about it. So I did the background study, and then I said, "No more. Thank you so much." The study ran, I think, until about 1965.

So in 1975 back they came. By then the smaller reactor was operating; the big ones they were just getting started on. They had a biologist; he may have been from San Diego State

College. He came up and did some diving in the area, just on his own. He had a couple of other people with him, I think, and they started making noises that the whole region was biologically stressed and in trouble. So Edison called me up and said, "Please, will you start doing regular surveys in there?" So I did. They were mainly concerned with the San Onofre kelp bed. They had lots of studies going on onshore. But they wanted Mr. Kelp. [Laughter]

Erwin: I see. And this was a big kelp region?

North: Yes. There's a beautiful kelp bed. I think it's maybe 2,000 feet downstream. At that point there was just the little, tiny power plant—a 400-megawatt plant—operating. And they were in the process of getting their final permit from the San Diego Regional Water Quality Control Board. They had me appear before the board as their consultant. And I made the suggestion that down in central Baja California there were kelp plants that could just survive the much warmer temperatures that were down there and that it might be interesting to try and bring some of them up and get them established at San Onofre. So Edison proposed to the board that this be done and that they would fund the work with a grant to Caltech.

Erwin: Was it clear that the indigenous kelp was having trouble?

North: No. Well, it was having trouble, but this was caused by a minor El Niño that came along in 1976.

So Caltech got a grant from that power company, and we went down to Turtle Bay and Baja California about 600 miles below the border, and got little plants and brought them up and started culturing them in Newport Bay at the marine lab. We got them up to the point where they were reproducing, and then we took the leaves that had the reproductive spores and took them into the laboratory and cultured them and got lots of young baby plants to plant out at San Onofre.

A little before this time, the California Coastal Commission came into being, and suddenly Edison had to have permits from them. It's a regulatory agency of the state, and they have complete control over the shoreline and the immediate offshore area: I think, the coastline

to 2,000 feet inland. So any development in that region has to have a permit from the California Coastal Commission.

Erwin: So that was a big change in the life of the shoreline.

North: That was a big change, yes. So Edison had to get a permit from them.

Erwin: Just to operate their plant?

North: Just to construct the plant, even though construction was under way. So Edison told the commission that they had this warm-water kelp project going. I made a guess that if the discharge were 2,000 feet away from the kelp bed, there wouldn't be significant biological damage. The outfall design was being supervised by Norman Brooks and John List.

Erwin: Oh, here at Caltech.

North: Yes. So I talked a good deal with them and got a feeling for what the plume would be. And then they wanted information from me on the allowable temperatures in the kelp bed. So the coastal commission decreed that the discharge must be 2,000 feet away from any kelp growing in the water. This affected the Mexican kelp program significantly, because the geological surveys began turning up hard substrate rocks and boulders and stuff within this 2,000-feet limit. So Edison began thinking, "If the Mexican kelp gets on those stones, we'll be in violation of the permit." Because the Mexican kelp can probably tolerate the temperatures.

Erwin: They would migrate over—

North: And then Edison would be in violation. At this point we were getting ready to plant the little baby kelp plants, and they said, "Don't plant them in San Onofre." And they had me plant them in a kelp bed that was about four kilometers away, at San Mateo Point, which was of course ridiculous.

Erwin: Yes, because that wasn't the point of the project.

North: The whole program suddenly became without any purpose. So Edison discontinued it after we put out several thousand little plants at San Mateo Point.

Looking back to the Diablo Canyon plant, Caltech had put in a proposal to PG&E to design their discharge, and instead PG&E gave it to the University of California at Berkeley—they gave part of it to Berkeley and part of it to Caltech, and then they cancelled the part to Caltech. So Brooks and Raichlen and List were very disturbed and tremendously annoyed by it. And they were chortling when the discharge didn't go where it was supposed to. [Laughter] Any engineer should know that would happen.

Erwin: Yes. So were Brooks and List working at the Environmental Quality Laboratory at that time? That had been established by then.

North: Yes, back in the early seventies. But even when I came, in 1963, hydrology and environmental engineering worked very closely together. We had joint faculty meetings. It's always been that way. The Environmental Quality Laboratory was instigated by Lester Lees [then a professor of aeronautics], who was not in the environmental group at all. He was an aeronautical engineer, as I recall. This happened just about when the environmental craze was hitting the whole country. It was becoming a big deal. And I was invited to join the EQL group, as everybody in our environmental group was. I talked with Sheldon Friedlander about it, and he was very negative.

Erwin: Really? And why was that?

North: Apparently he'd had this happen many times. He said, "The outside people"—people outside the environmental work—"think they see the solutions and research that should be under way, and they think we're dummies. They have no concept of how complicated a lot of these environmental problems are." And he said, "This is an outstanding example of an outsider

[Lester Lees] getting interested." So I declined the invitation to join EQL, and in retrospect I'm glad I did, just because I began to get so much activity from my Caltech research and the consulting. I was being called every place for consulting work. And I handed it off to my technicians. At that time, I had two head technicians. Pretty soon they both left me and started their own little companies; one of them is Marine Biological Consultants.

Erwin: Now, who were these people? Can you name their names?

North: One of them was Charles Mitchell.

Erwin: And they had gotten degrees here? They had been your graduate students?

North: No, no. They were hired as staff. And the other one was Einar Anderson. He ended up with the biologists up at PG&E. He went to several other places after he left me.

Begin Tape 8, Side 2

Erwin: Let's finish the San Onofre kelp bed story.

North: Yes, with Edison. I continued working there—diving there three or four times a year, doing extensive surveys—up through 1993, when I had to stop fieldwork because my leukemia was getting out of control.

Erwin: Yes.

North: But at any rate, as with PG&E, Edison had their own biological staff. A condition of their permit from the coastal commission was that the program would be funded by Edison but the control over what would be done technically was to be beyond Edison's jurisdiction. So Edison agreed to that. These companies make agreements behind my back, and they get into awful problems. PG&E agreed that if any sharks or stingrays appeared near the discharge, that

was it—they'd have to stop business. I told them, "That's an outrageous thing to agree to." A huge school of sharks and rays loves that warm water; they are just in there all the time. [Laughter] The regional water-quality control board went along with it when it finally happened.

Erwin: So they had to sort of backtrack.

North: Yes. But Edison funded this large program. It was to be supervised by a committee of three called the Marine Review Committee. One member of the committee was to be an Edison biologist, another member was to be from the environmental activists in California, and the third was to be an academician.

Erwin: And that was going to be you?

North: No. By that time, my reputation among the activists was mud, because of the Santa Barbara oil spill, so the academician was from UC Santa Barbara. It ended up that there was a succession of people. Edison put about $43 million into the Marine Review Committee program. They did a lot of excellent work, but they came up with some rotten conclusions. The first San Onofre nuclear reactor went into operation in 1983, in the middle of a horrendous El Niño. The kelp beds all up and down the coast were suffering. And then the second one went on in April or May of 1984. And the El Niño didn't abate here until December of '84. So the kelp bed was in poor condition. When the reactor started up, the Marine Review Committee's overall study plan was to compare San Onofre with two control kelp beds—one at San Mateo Point, where we put in the Mexican plants, and another farther down. San Mateo Point was upcoast; immediately downcoast was another kelp bed called Barn kelp. Before the San Onofre plant even started up, the Barn kelp went extinct. So there goes one of their control beds, and they can't blame that on the power plant. So they said, "Well, too bad. We'll just compare San Mateo Point and San Onofre." And you're not supposed to do that. If a control bed goes extinct from natural causes, one can presume that San Onofre might go extinct from natural causes. They didn't consider that. So they compared the status at San Mateo Point with San

Onofre, and they came up with a very complex statistical analysis indicating that, if the power plant hadn't been operating, the kelp bed at San Onofre would be sixty percent larger than it actually was.

Erwin: What was the politics of this? Did you know?

North: Well, let me say that the Marine Review Committee felt that they were representing the California Coastal Commission, which had essentially brought them into existence. That was to whom they were reporting, not to Edison.

Erwin: And the politics of the coastal commission were presumably—

North: The environmental activist on that committee was dying to see San Onofre just put out of business. And the Edison person on the committee was a very nice guy. He would go along with anything that was reasonable.

Erwin: And then that leaves us with the academician from Santa Barbara.

North: Yes. But at any rate, this was their overall conclusion: that the [power] plant hadn't harmed the fish life and that there was some damage to the plankton when it got sucked in. But their main worry was with the kelp. The basis for that conclusion was that after the El Niño abated, San Mateo Point came on strong, and it took San Onofre several years to recover. So if you compare the two soon after the El Niño, San Onofre should have been much bigger, because San Mateo Point was. A couple of the biologists at Edison were very interested in artificial reef technology and the possibilities for mitigation, so the Edison guy on the Marine Review Committee didn't object to this finding, because he thought, Well, we can build an artificial reef and mitigate for any damage to San Onofre kelp. Then they suddenly realized they were fighting essentially for the existence of these two [power] plants that cost a billion dollars each. The activist on the Marine Review Committee was anxious to put them out of business.

So Edison went along with this conclusion. I was just furious. I said, "That is totally wrong! It's just that San Onofre took a little longer to recover from the El Niño than San Mateo Point did. It's now OK. You *can't* grow any more kelp there; all the rocks are taken up with kelp plants." So Edison said, "Well, we're glad to hear that, but let's not rock the boat. We'll agree to spend $30 million on mitigation: $15 million will be to restore wetlands somewhere, $15 million will be to build an artificial reef and plant kelp on it." I said, "OK, if that's what you want to do." Then they agreed to build a 300-acre artificial reef. Three hundred acres is as big as San Onofre is.

Erwin: The kelp bed, you mean?

North: And then plant it with kelp. I told them, "You're absolutely crazy. Fifteen million dollars will just maybe get the rocks out there. You're looking at a $150-million to $200-million project." Then they realized they were in hot water, because the environmental activist on the Marine Review Committee got a group called Earth Island to sue Edison for the damage that had been done at San Onofre. And [Earth Island] complained to the San Diego Water Quality Control Board, the relevant agency that had jurisdiction. So suddenly there were these hearings down at the San Diego board. Additionally, Earth Island had brought a suit to cease and desist. It was before a federal judge down in San Diego. So Edison was facing this shutdown of their [power] plants. So the judge in San Diego said, "I won't issue any findings until we hear what the Water Quality Control Board says." So the Marine Review Committee made extensive presentations all day long. I think they made two presentations. I stood up in one presentation—not as an Edison consultant, just as an outsider—and said, "This is crazy. The San Onofre bed is back to normal. You can't get any more kelp growing there. This conclusion is just wrong." So at any rate, the board has a big staff to assist it in reaching decisions. The staff had essentially recommended that the Marine Review Committee study was extraordinarily good. And the board voted nine-to-zero to accept my conclusion. [Laughter] Here's this stupid little consultant, up against a $43-million program!

Erwin: This was quite a dramatic moment then.

North: Very dramatic.

Erwin: So in fact you saved San Onofre, one could conclude, single-handedly.

North: Yes. Well, by that time Edison had agreed to put in this 300-acre reef, so they were still on the hook for that to the coastal commission. And I'd told them that they were absolutely crazy; they should have consulted me. But at any rate, that's still going on, five years later. When they began realizing the extent of the task, they thought maybe it would be better just to close the plant down. [Laughter]

Erwin: [Laughter] This is like draining the ocean instead of raising the *Titanic*.

North: So they tried to get the reef reduced to twelve acres, but the coastal commission wouldn't go along with it. And it's still a big hassle.

Erwin: Meanwhile, people's electricity bills continue to go up.

North: Yes. [Laughter] Gee, you get me talking here—

Erwin: Oh, that's terrible. Well, maybe we'll leave San Onofre. We're going to talk about the second biomass project.

North: We can at least get started on it. All of us who had been associated with the old marine biomass program were very discouraged by its being discontinued, because although in 1984, when that happened, there was plenty of gas and oil available, there would come a time when there wouldn't be. And one needed to find the answer to the question, Where do you supply the world's energy from? And we felt that ocean farming was the only feasible answer. I might say that this was Wilcox's idea. There was another person who had this idea also: his name was Edward Hall. He was a retired air force general.

Erwin: Both of them are military men.

North: Both of them military-associated, but they had broad viewpoints. And they realized, "What's going to happen when oil and gas give out? We need to have an answer, even though it may be fifty years away. We need to have a good answer." So I think it was in 1989 that the National Research Council had a two-day conference in Washington, DC—not to talk about energy but to talk about greenhouse gases. The most important one right now is carbon dioxide. This conference was just talking; no programs, no awards of proposals or anything like that: "Would it be feasible to grow marine plants on a large scale to take up the CO_2? And if you did this, what would you do with the marine plants?" The carbon would be locked in them. But where do you put the tons and tons of marine plants that contain this carbon? Just before this conference there appeared a little article in *Science,* but instead of talking about seaweeds it had talked about growing trees.

Erwin: To take up carbon dioxide?

North: Yes. And somebody made a calculation that to essentially remove all the anthropogenic CO_2 that is coming into the atmosphere, you'd need a continent the size of Australia planted with trees. I forget how many millions of square miles more of trees that you'd need, but it seemed pretty discouraging. I made the calculation for marine algae, and it comes out to about the same area, and that's only a small part of the ocean. The ocean is huge. So from the point of view of available space, there is plenty of it in the ocean; there isn't on land.

At any rate, they had all the people who had been involved in the marine biomass program at this conference. They did a very good job of recruiting everybody. The person who essentially did most of the work calling up people and getting them to come was Dr. Peter Schauffler, who wasn't with the National Research Council. But back in the days of the marine biomass program, he had been very interested in it. And he lived in Washington; he was a political economist. So he did most of the spadework to get the conference going. And the conference was paid for with a grant from the Electric Power Research Institute. The vice

president [of the institute] was very interested in marine biomass; his name was Dwain Spencer, and he had been at the NSF when Caltech got its first grant.

Erwin: Right. And EPRI was a private agency?

North: It's a private agency—similar to the Gas Research Institute, which had funded the marine biomass program. Instead of being the gas companies, it's all the electric power companies that have put money into this EPRI organization to spend on research projects that will help the whole industry. Power plants put out thirty-five percent of the CO_2 that's generated by human activities. So Dwain Spencer, I think very thoughtfully, said, "This CO_2 problem is our problem, along with everybody else's. We need to be active in doing something about it." So he funded this little conference. Our conference was for the seaweed types. Two days before our conference, they had a similar conference of the plankton people. And the plankton people felt that it was not feasible to go out and try and fertilize the ocean—to bring up a number of phytoplankton, the tiny single-celled plants—with one exception. One proposal was made that there are large parts of the ocean where the element iron is a limiting factor. If you put in a little iron, there are plenty of other nutrients, and the plankton will take off. So the plankton group approved that: try fertilizing part of the ocean with iron. But it took them two days of arguing to get to that. Everybody was gung-ho in the seaweed part of the symposium. The arrangement of the symposium—I guess I hadn't realized it when Peter Schauffler called me—was that each one was supposed to get up and give a five-to-fifteen-minute presentation of research they had done. Instead I went ahead and made a lot of calculations as to the feasibility: Could you really attack this huge problem? So I tried to scope it out, and everybody was much more interested in that. [Laughter]

Erwin: I would think so, because you were taking the initiative and not just recapping.

North: Yes. And I also talked about the innovations in aquaculture that had happened in the five years since the old biomass program. So Dwain Spencer was very taken. And we all went

home happy, thinking that maybe something would come of this. It was a tremendous task, but it probably could be done. It was feasible, I thought.

So Dwain Spencer funded a second conference. Instead of doing it through the National Research Council, he did it through Caltech. He gave Caltech a $20,000 grant to do a second conference, which was done down at the Balboa Bay Club in Newport Beach. The first conference was in December 1989 and the Newport Beach one was, I think, probably in July or August 1990, about six months later. During that time, I read an article in *Science* about some Japanese findings. They had a deep-diving submarine that had gone down, and they had found an area over in the western Pacific where there was a carbon dioxide seep coming out from the seafloor at very cold temperatures and very high pressures. And at that pressure and temperature, carbon dioxide is a liquid, it isn't a gas. And the carbon dioxide was interacting with seawater to form an icelike substance called a hydrate. I had never heard of a hydrate before. So here was the hydrate lying around on the seafloor. And I thought, That's a way to dispose of this problem of growing the plants but then not knowing what to do with them. Well, you'd burn them and take the CO_2 and put it back in the ocean as hydrate. So I made this suggestion at the second conference. And there was a NOAA [National Oceanic and Atmospheric Administration] person there. There were several governmental people there: EPA and NOAA and the National Research Council guy.

So the NOAA man asked me to write a proposal on the hydrate concept, which I did. And because I knew Dwain Spencer was interested in the whole thing, I sent him a complimentary copy—not angling for any money from EPRI, just to keep him informed. The NOAA people consulted among their staff, and they felt that covering the seafloor with hydrate would be very detrimental to marine life on the seafloor, which it would—it would just destroy everything. So NOAA rejected the proposal. But Spencer was so interested in it that he had me rewrite the proposal for EPRI, and they funded it.

At that point, hydrate had been made in laboratories. You have to have very high pressure and low temperatures in little tiny chambers, which cost $1,500 apiece.

Erwin: It's a solid substance, then?

North: It's like ice—white crystals. Up to then, it had mainly been just a scientific curiosity. So the Caltech proposal was to think of it in terms of how does one mass-produce this stuff, and put a little on the seafloor and see what happens. And it turned out that EPRI was getting very seriously involved in the CO_2 research; they had a program going down at Scripps to measure the CO_2 in the atmosphere. At any rate, we got started on this hydrate program down at the marine lab. I didn't know beans about chemistry, so I had Jim Morgan as a coprincipal investigator.

Erwin: Was he a chemical engineer?

North: Yes. Jim runs the chemistry part of our environmental engineering [group]. He essentially is a physical chemist.

So in the meantime, I had given the whole problem some thought, which had been initiated by the fact that NOAA had rejected the proposal: if we do know how to make hydrate, we're going to have to face all the environmentalists if we try to put it out in the ocean. I began thinking, Well, if you grow seaweeds, or if you grow plants on land, and burn them, liberating CO_2, you're still way ahead, because any energy that you'd get from the burning is not fossil energy—it doesn't depend on fossil CO_2. You can do this and you won't be increasing the CO_2 in the atmosphere, because it's not coming from the ground into the atmosphere, it's recycling. And therefore NOAA got interested again, and has helped vitally in the project of getting a test marine farm going to prove that it can be done in the open ocean.

Erwin: Now, did they give money to Caltech specifically?

North: NOAA put in money for additional conferences, but [for] the big money—this is several hundred thousand dollars—they have the World Bank lined up to provide a good deal of the funding. So does this mean that we don't need hydrate? And now that I have this money coming in from EPRI, is this hydrate program really superfluous?

We essentially were making hydrate in small pressure chambers. We developed a chamber that could be built for $35.

Erwin: Better than $1,500.

North: Much better than $1,500. And we got a chamber with some hydrate in it and flushed it with seawater at high pressure and low temperature, and the hydrate dissolved. So if one did put hydrate down on the seafloor, you wouldn't have this big puddle—it would dissolve in the ocean. It turns out EPRI was funding another carbon dioxide program with a company called SAIC [Science Applications International Corporation]. It's a big engineering company. The objective of their EPRI-funded program was to consider and predict what would happen if you took a pipe and put CO_2 into the ocean—and regardless of what form it's in but assuming that it distributes itself—where would it go and what effects would it have? The big effect is that it would change the pH, the acidity, of the water, which is at a neutral pH of about 8. If you put CO_2 into it, you could get the pH down to 3, which is pretty acid. A few bacteria could live in it, but that's about all. And the SAIC people figured that the ocean could take all our CO_2, as I recall, for several hundred years. And if you put it in the ocean below the thermocline, it will stay there for a thousand years or so. And if you took all of the CO_2 for centuries and put it in, the pH would go from 8 to a trifling amount below 8. The capacity of the ocean is so huge and the buffering action in the ocean is such that it would be inconsequential. But the trick is that you have to get it below the thermocline. I don't know if you know much oceanography.

Erwin: Not really.

North: OK. The people who do computer models of the ocean consider it as two boxes: one box is the shallow water, another box is the deep water. The region between the shallow and the deep is called the thermocline. If you measure the temperature in a probe as it descends into the ocean, in the shallow ocean the temperature drops a little bit. But when you get to the thermocline it suddenly drops enormously. And then you get into the deep ocean and it drops very little.

Erwin: I see. So it's an interface.

North: The ocean is stable in this configuration. And if you get [the CO_2] into the top box, it will get back into the atmosphere, on the average, in a hundred years. If you could get it down into the deep ocean, it would take at least a thousand years.

Erwin: I see. That's fascinating.

North: The problem is, How do you get the stuff down below the thermocline? And it's changing all the time. Out in the big ocean, it's anywhere from 1,500 feet to 3,000 feet [down]. So Dwain Spencer, who is quite a capable chemical engineer, started doing calculations on getting carbon dioxide below the thermocline. If you do it with liquid carbon dioxide, there is a possibility it will come right back up. Carbon dioxide condenses as the pressure goes up. When you get down to 1,000 meters—or 3,000 feet—it's dense enough so that it will sink. You are well below the thermocline at 1,000 meters. But suspending a 1,000-meter pipeline from a boat or a platform is an engineering problem that hasn't been solved yet, and it may not be solvable. So Dwain started looking at hydrate; hydrate is denser than seawater. You wouldn't need nearly as long a pipe if you used hydrate. So suddenly hydrate became relevant again. EPRI gave us a two-year grant. They were overjoyed with the work that we did. We had a graduate student of Jim Morgan's named Veronica Blackwell doing her thesis on carbon dioxide hydrate research. So they gave us another two-year grant. [Tape ends]

WHEELER J. NORTH

Session 8

December 1, 1998

Begin Tape 9, Side 1

Erwin: You'd like to talk about the economics of your biomass project.

North: Yes, back when Wilcox was running the show.... He's a very diverse individual. He had extensive knowledge in several fields, even though his main training was in physics. But he endeavored to make an economic projection. He had the marine farm—the biomass—producing two different sorts of products. One was methane gas, or methanol.

Erwin: And that was used for fuel, strictly?

North: For fuel, yes. And the other general category would be marine foods. If you have seaweed, you can feed it to a variety of animals, like abalones and urchins. And you'd have this operation out on the ocean. One of the difficulties aquacultural operations has is that you have to have fairly expensive property near the seashore, and then you have to bring in seawater, pumping it continually through the system. And then, depending on the type of organism.... For raising something like abalone, which is done here in California, you have to go out and get a few tons of kelp every so often. And that all adds up in expense. But if you're out on the ocean growing kelp, you can do all those functions quite reasonably economically. And marine foods usually command a fairly good price. So Wilcox figured that with one percent of the biomass diverted to aquaculture, that would pay for the entire operation. Then when the Gas Research Institute got involved, the economics changed, because their requirement was that the gas alone had to pay for it. They didn't allow aquaculture or any other product.

Erwin: Do you think Wilcox was correct in his projection?

North: Oh, yes, absolutely. One of the things that I think led to GRI discontinuing the project was that they had an economic study done and it looked like gas from kelp cost about $12 per million BTU, and gas out of the ground could cost about $3 or $4 per million BTU. So that was a huge difference. To make it partially, or at all, attractive, I think you have to have products other than just fuel. Then we come to this new aspect of a marine farm—[as part of a plan] to remove CO_2. This has an effect on the economics. It hasn't been worked out rigorously yet, but various people are anticipating that in the future there will be carbon taxes. Particularly an industry like power generation, which produces thirty-five percent of the CO_2, would have to pay very substantial fees as a carbon tax.

Erwin: And how soon would this happen? Pretty soon? Or are we talking twenty or fifty years from now?

North: Well, we aren't talking fifty years. In some places—I think in Massachusetts—there already is a carbon tax, or at least it was proposed. And I think in Europe they are looking at carbon taxes right now. If an industry like the electric power industry or the gas industry gets involved in producing marine biomass, they might get credit for that and relief from the carbon tax. So this is going to alter the economics. As I say, it hasn't been studied carefully, but it's very clear that this aspect of the marine farm is going to make it more attractive.

Erwin: How closely has the whole area of the economics of environmentalism or conservation been studied, as a matter of fact?

North: I think it's been studied in a fragmented way. And I'm not competent to give you a good answer, but certain situations have been studied, and the economics are usually pretty unfavorable.

Erwin: Yes, but the tide may turn.

North: It's because it's hard to put a dollar value on saving a species, preventing a species from going extinct. Any such consideration is a little squishy, you might say. But I admit, it's very clear in using marine biomass to help control CO_2—I think you're on fairly safe grounds [saying] that there's going to be a new aspect of the economics of it.

Erwin: Yes. And so we can look for this pretty soon.

North: Yes. Let me follow this up with the current state of the marine biomass situation. In my outline in two or three places, I mention Dr. Peter Schauffler. He lives in Washington, DC, and he's very hip to the politics. He got his PhD in political science from Harvard.

Erwin: Oh, is that right? Didn't you say he taught at Georgetown?

North: George Washington University. He doesn't teach there; he's the equivalent of a research associate. Peter is very interested in marine biomass. He's one of the few who believe that eventually humanity is going to have to go about farming the oceans—like Wilcox, Dwain Spencer, and myself. We're all of the same mind. But Peter is in a position to do something about it. It looks like he has gotten a grant together from the World Bank to set up a test experiment down near the Galapagos Islands, on the equator. He won't be using kelp. The Ecuadorians are very conservation-minded about the Galapagos Islands.

Erwin: I've heard that. They're very careful what they allow to go on there.

North: They're going to have to use some of the seaweed species that grow on the Galapagos Islands. That doesn't seem to be a problem.

Erwin: It's not a problem for the Ecuadorians? You mean that they'll permit it?

North: It's not a problem for the Ecuadorians, and it's not a problem for the farm. One of the fastest-growing seaweeds in the ocean is a genus called *Gracilaria*. And there are about 150

species of *Gracilaria,* and they're all pretty fast-growing. There are some *Gracilaria* species in the Galapagos Islands, so they plan to use those. There's a very special situation that exists right on the equator, west of the Galapagos Islands. There are unusual current systems there. The deep water is brought up to the surface by natural means. So you have very nutrient-rich surface waters, with one exception, and that is that it's very poor in iron, which is an important nutrient. It's called a trace nutrient; you don't have to have very much of it. Because there's not much iron there, there isn't much plant productivity, and all this nutrient-rich water goes to waste. So what they would do is pump this rich water into their *Gracilaria* culture and put a little iron in it. Since such trace amounts are needed, it's not a big economic factor. Probably with a bottle of, say, ferric sulfate, you'd be able to fertilize several square miles of farm. Anyway, this is an ongoing proposal right now. They had a meeting in Ecuador, with an Ecuadorian scientist who is on the steering committee, to get a feeling of whether the country would be hostile to this or welcoming. And assuming that it's environmentally compatible, why, it looks like it would be very welcome, because if it is successful there probably would be a huge demand for jobs. It would help the economy. The project's initial test was supposed to take place last summer, but we had an El Niño, so they put it off a year.

Erwin: So this summer, then, they'll actually be out constructing—

North: Hopefully, yes. The World Bank has indicated that they're interested in funding it. I'm an advisor to the project. I've been an advisor to them from 1989 on. They very much appreciate my comments, because I'm the only person who's actually had hands-on experience trying to grow plants out in the open ocean.

Erwin: What happened to Wilcox? Is he still around?

North: No. He passed away a couple years ago. I just felt so sorry for him when the GRI people took the funding away from his control and gave it to General Electric. Wilcox's heart was in the project, and then they just yanked the rug out from under him. But he went to these conferences that we had. He stayed involved.

Erwin: Well, maybe you would like to tell me here about your leukemia problems, which interfered with some of your activity on the second biomass project.

North: I have chronic lymphocytic leukemia, which is a form that can be controlled but can't be cured.

Erwin: And did you know when you acquired this?

North: No. But I had to have a hernia operation in 1984. And as a preliminary they take a blood sample and analyze it. Lo and behold, I had high white-cell counts. And looking at the cells under the microscope, the pathologist said, "Aha! This is chronic lymphocytic leukemia."

Erwin: But you had felt no ill effects? You didn't feel bad?

North: No. I was completely unaware of it. In fact, in the typical case the person just takes mild chemotherapy and dies of something else eventually.

Erwin: So it really is not lethal.

North: It's not lethal. It used to be, before they knew how to give the chemotherapy that they now have. But it was just sort of a slow death, as the white cells built up and the blood circulation got worse and worse. So at any rate, as of 1984 I was apprised of the fact that I had this. I really didn't do much about it. The physician told me—I think at that point my white cell count was 20,000, and the normal is 10,000—that you don't have to worry until it gets up to 50,000 or 100,000 or something like that. Well, it got up to about 40,000. My wife is a physician, and she started pushing me to go see an oncologist, which I did. I went to see Dr. Okun in 1987, and after about a year he began putting me on this very mild chemotherapy. And it would bring the white cell count down. I'd go for six months or a year, and it would build

back up, and then it would go down. But there's a certain small fraction of people who have this where it takes on a more malignant form, and it turns out I'm one of them.

Erwin: Yes. Well, that's unfortunate.

North: It's one percent or so. Dr. Okun figured this out, because I wasn't responding properly to the chemotherapy.

Erwin: I see. And that was the tip-off that there was more going on.

North: He had me take a CAT scan. Lo and behold, there were lymphocytes—white cells—in the lymph nodes of the abdomen and the pelvis. So he said, "We don't have to do anything about it now. It can still be controlled, but it will take more aggressive chemotherapy."

Erwin: So how did this then affect your activities? Were you able to dive still?

North: Oh, yes. I was out diving, and getting along with a very active life. At the end of 1993, I began having a nagging pain right near my navel, and Okun was away on vacation for four weeks. He had three other partners. I thought, "Maybe I ate something wrong." But it kept staying with me and getting worse. So finally, between Christmas and New Year's, I went to one of the other partners, and he was fairly alarmed. He had me go do another CAT scan, and there was apparently a mass of good size forming near the pancreas. So they did a biopsy and established that it wasn't a different kind of tumor. It was this lymphocytic leukemia that was causing it. And about that time Okun came back. He said, "Well, we know what it is and we know the medicine that fits it." So we started in with a more aggressive chemotherapy, and it didn't work.

Erwin: Oh, dear.

North: That ended up in a bind, apparently, because they didn't dare start any other kind of chemotherapy until some time had passed, and this thing was getting very, very painful. So I think Okun was worried that because it was in such a critical place, I might not make it if they waited until they could start me on another type of chemotherapy. So he had me undergo radiation therapy on this pancreatic region. He put me in a nursing home over near where the radiotherapy was. They'd pick me up in an ambulance every day and take me in and douse me with—I don't know what kind of radiation it was, but it worked. It destroyed the concentration of lymphocytes.

Erwin: So it wasn't officially a tumor. It was something else.

North: Yes, it wasn't a conventional tumor. That lasted four weeks, and I went from 150 pounds to 116 pounds. I couldn't keep down food.

Erwin: Was this because of the radiation?

North: Yes, because of the radiation, and because it was such a sensitive area. I came out of that. They had to carry me up the stairs to my apartment. And I had to have nursing help. My daughter came and stayed with me for two weeks. I gradually built up and got everything back, but the next week I started the new chemotherapy.

Erwin: So you had a hard row here to hoe.

North: It was no fun. But I wasn't particularly nauseated by the new chemotherapy, so I began getting my weight back. There was a side effect from the chemotherapy. One of the agents in the treatment was very hard on the nervous system, so my old back injury—I just had probably a few nerves left feeding my right leg, and I got tremendous pains in my right leg after each chemotherapy treatment. It began to get more and more feeble, as the nerves would die.

Erwin: So the pain was an indication that the nerves were dying or being damaged in some way?

North: Yeah, being damaged in some way. So the chemotherapy went on for six months. By the end of the chemotherapy, I was quite sick. I was running a fever of about 102. They couldn't figure out what was wrong with me. I had dysentery. Then Okun said, "This is really beyond my competence, but we'll try one more test." And they did one more test, which was to take quite a bit of blood and try and culture organisms from that to see if it had gotten into my bloodstream. I had to go to a hospital to have my blood taken. I had it taken on a Thursday, and early Friday morning Okun's nurse called me and said, "Get to a hospital. Your blood is teeming with bacteria."

Erwin: Oh, so you had a huge infection?

North: Yes. I had a systemic infection, and it turned out to be salmonella.

Erwin: Oh, good old salmonella. Well, people get that from bad food, of course.

North: Yes. It's often in chicken. If you cook the chicken, it kills the salmonella. But people, when they barbecue, have barbecue sauce. They dip the chicken in it, and if the chicken is contaminated, the salmonella comes out into the barbecue sauce. So then they cook the chicken and kill all the salmonella, but stuff is still left in the barbecue sauce and they put that back on the chicken after it's been cooked.

Erwin: Do you think that you got it that way?

North: It could be, yes, because I have a friend who does invite me from time to time for chicken barbecues.

Erwin: But nobody else got salmonella?

North: Well, I had a totally compromised immune system. I was very susceptible. But at any rate, they got me into the hospital, and they put me in isolation, because it was a very infectious thing, and they put me on strong antibiotics. A week later I walked out, cured.

Erwin: Miraculous! It's quite a story; it has a lot of ups and downs.

North: Yes. By the time I began having the type of chemotherapy that worked, my abdomen was completely twisted and swollen. I looked at myself and said, "Is that a human being?"

Erwin: So actually you looked disfigured to yourself. You could see this.

North: Yes. As the treatment went on, the bumps and hollows began disappearing. By the end, I was back to Wheeler North, which is a rewarding thing to follow. But this right leg was in very bad condition, so I started using a cane. I was able to do a little more work finishing off my hydrate project, but it was very difficult for me to do. We got that done.

Erwin: Well, that's good. And about how long did all of this take, the radiation and the chemo? Was it a year?

North: As I say, it first started bothering me around Christmastime 1993. And my last chemotherapy treatment was August of '94. Well, then I was slowly getting back on my feet. I had agreed to teach my ecology class in the winter of 1995. On my birthday, January 2, 1995, I began having this funny back problem. It kept getting worse and worse. I first noticed it on a Monday. Finally I went to an emergency room. Okun said, "I have no idea what that is, what's bothering you. The CAT scan shows no problems." I had a terrible case of shingles. Okun said that that happened to a lot of his patients who've had chemotherapy.

Erwin: Is that right? But that's a virus.

North: That's a virus. And if you've had chicken pox, you are especially susceptible to shingles, because it's the same virus. So I had to cancel the class.

Erwin: Oh, you weren't able to teach. That's a shame.

North: I was just in bed for about three months.

Erwin: Really? That's a long time. So it was painful?

North: I could take pain pills and get to a grocery store and cook my dinners and breakfast and lunch, but I was in bad shape with those shingles. Well, they gradually got better, and I became reasonably active again. And I had a relapse two weeks ago.

Erwin: Oh, dear, of the shingles?

North: This time I knew what was happening, and they have good antiviral pills. If you get on it right away, why, you can kill the virus.

Erwin: But, now, didn't you tell me, though, that you have pain even when you're not having the flare-up?

North: Yeah. Right now it's very sore. About nine inches of skin right near the belt line.

Erwin: That's unlucky. Aren't you in a one-percent category again with that?

North: Probably, yes. Well, no, I think it's more like a five-percent category, if you've had both radiation therapy and chemotherapy.

Erwin: Yes, that's true. Well, the average population, I know, usually recovers from shingles without after-effects.

North: Yes.

Erwin: So you taught your ecology class, though, a year after that. Is that right? In '96?

North: I taught it in '96, yes.

Erwin: That was your last appearance in the classroom?

North: Yes. I was kind of clumsy, you know, going from my notes to the blackboard. I'd just set the cane aside and stumble back and forth. The students were very forgiving.

Erwin: Yes, they would be. Did you still have graduate students at that time?

North: No. But we had this grant from EPRI to study hydrates. Jim Morgan, our chemist, was a coinvestigator with me, and he had a student who was interested in the environment and global changes—global warming and so forth. So we put her on the hydrate project. This was Veronica Blackwell. She did a wonderful thesis and graduated last June. So I was very instrumental in helping her—giving her advice as to what she should be doing.

Erwin: Has she gone on to an interesting job?

North: Yes. She got a job with Chevron Oil, in their research facility, which I think is down near LAX [Los Angeles International Airport] somewhere. I haven't talked to her, but she came up and talked to the secretary. She said that she loves the job. And they are having her do hydrate work. The hydrates often form in oil lines. They have these offshore oil platforms with a pipe coming in, and the pipe plugs up, because hydrate forms there. The pressure is high and the temperature is cold along the seafloor. If water and gas are in there together, why, you get hydrate forming, and that plugs up the line.

Erwin: You said earlier that Clair Patterson had participated at some level in the second biomass project, or maybe the first.

North: It was the first biomass project.

Erwin: I wanted to ask you a little bit about him, what kind of a colleague he was.

North: Oh, well, he was a big help. In addition to growing kelp out in the ocean, we were growing it in the laboratory—little baby plants. We were trying to determine what the optimal nutrient mix was. We just couldn't get consistent results. So I talked to Clair Patterson, and he explained to us that when you're working with micronutrients like iron and manganese and so forth, you have to have very clean conditions. You have what's called a Class A laboratory setup, where people have to put on superclean clothes to go in there.

Erwin: Right. You see this in the movies all the time, but in real life I haven't seen it.

North: So his advice was very helpful. We had a special session with him and Dorothy Settle, who was his coresearcher, and several of my kelp culturists, and the people who were responsible for the laboratories. And they gave us information that was needed, and our experimentation at once began to improve.

Erwin: Now, Patterson was really a pioneer in a lot of things, wasn't he?

North: Yes. He was hated for one of the things he found out. [Laughter] He pointed out that lead was a horrible contaminant in our urban environments. The oil companies that wanted lead put in gasoline because it kept cars from knocking and they run better were very much against Patterson's findings. And then the chemical oceanographers were working on trace substances in seawater, and they couldn't make any sense of the results. Numbers were wild. And Patterson told them that they were getting contamination and that they needed to use the clean-laboratory techniques. Everybody was against him for that advice, until finally they decided

that maybe he was right and they started taking—you have to take very dramatic precautions. You have to soak your glassware in concentrated acid for two weeks.

Erwin: Now, did he figure out all of these techniques for obtaining this level [of cleanliness] himself?

North: Yes. It all came out of his lead experimentation. He found that he had to have a very clean laboratory in order to get consistent results with trace amounts of lead in the atmosphere and so forth. And in water, and in various phases of our environment. And he was right.

Erwin: So he was vindicated, really.

North: Oh, yes. When he retired, they gave him a very large retirement party. Oceanographers from all over the world came to that. I was invited. The Athenaeum was just overflowing with admirers of Patterson, some of whom were people that were very difficult to get along with. But they were all very jovial that night.

Erwin: So they wanted to give him his due.

North: Yes.

Erwin: Well, he was an example of a loner, really, at Caltech. He didn't have a big working group. He kind of went his own way.

North: I know of only one graduate student that he had. His name was Michael Burnett, and I was on his thesis committee.

Erwin: I see. I do think he had more graduate students than just that, but maybe not very many. He did an oral history with the Archives, too. We were very happy that he did that. When you

were at Caltech, did you feel that you were in some sense a loner yourself? You talked about this a little bit before.

North: Yes. My expertise is in oceanography. There's no department of oceanography here. There was a big problem in California back in the 1950s, and that was that the kelp beds were disappearing. There was a lot of public dismay about this. The sewer outfalls were being designed here by Norman Brooks. And I was Mr. Kelp at that point. So it sort of all went together.

Erwin: It did go together. And yet, on the other hand, you kind of played a lone hand, it seems to me, a lot of the time.

North: Yes. I was out pounding sea urchins— [Laughter]

Erwin: [Laughter] Yes, but Caltech let you do that.

North: Oh, yes. Caltech was just a wonderful place, right from the start.

Erwin: So Caltech gave you your freedom?

North: Yes, completely.

Erwin: Is that still true now, do you think, for the younger people? Can they be as much of a lone cowboy as you were? Do you have a sense about that?

North: Well, I think there was a little dismay in the environmental faculty that I wasn't as much of a team member as I might have been.

Erwin: I see. So there were people who thought things should go along in a more teamlike fashion.

North: Yes. [Tape ends]

Begin Tape 9, Side 2

North: In the environmental groups at universities, they usually have at least one biologist, maybe several, but they tend to be microbiologists. As such, they have laboratories and they share students with various other members. And a lot of the environmental problems are focused on microorganisms in water supplies and in the sanitary disposal of waste. And in some cases, in wastes that are hard to decompose, microbiologists have developed cultures of species of microorganisms that will chew up some of these tough things. That's called bioremediation. Caltech took on a second biologist, Mary Lidstrom [1987].

Erwin: She subsequently left Caltech [1996]. But was she cast more as the microbiologist—that team member?

North: Yes. Very much. She had lots of graduate students coming down and using her equipment to solve some of their problems. Mary fit very well. The problem there was that her husband was at the University of Washington. They'd get together on weekends. The University of Washington, I think, offered her the chairmanship of the Microbiology Department, so off she went.

Erwin: Has she been replaced here?

North: No.

Erwin: Is there a desire to find another biologist?

North: I'm kind of out of the loop now. I don't know if they're looking for anybody or not.

Erwin: Well, this would go to the whole idea of team building, which is kind of a popular thing now.

North: Oh, very much so.

Erwin: People talk about team building and so on. And everyone is curious how that works in science, because scientists are such individualists. One can hardly ever imagine teams, or if there's a team there's usually some despotic person at the top, like Robert Millikan with his underlings.

North: Yes.

Erwin: In fact we know that it doesn't really work that way, but it's interesting to see models of teamwork and how that happens, for example, in your area. So in your career you were a member of the team, and yet you had a great deal of freedom. It sounds ideal. Essentially you had the best of both.

North: Yes. Now and then our interests would converge. Jim Morgan and I did the hydrate project together. He gave me a lot of help on the marine biomass project. He was the one who suggested I contact Clair Patterson.

Erwin: Yes. So there was kind of a loose give-and-take here, but it was always very fruitful. That's what it sounds like.

North: Oh, yes.

Erwin: Do you think the institute consciously fostered that at the time?

North: Yeah, I think so. Jack McKee, who was sort of the founder of the group, came from Harvard, where they had very much of a teamwork type of operation. When I came on board, I

fit in as part of the team, because a big part of the effort was worrying about ocean pollution and the resulting problems. One of the very significant problems in the public eye was the destruction of important habitat.

Erwin: Well, what do you think the high points were of your time here? You've talked about your own successes; was there anything that your division or department could point to as special achievements?

North: Well, I don't think there's any clear.… There haven't been any resounding major achievements.

Erwin: Not in the breakthrough category, perhaps.

North: Yes, that's right.

Erwin: But I wanted to ask you about your aerial surveys, because I don't think we got that on the tape. And that was one of your projects—aerial surveys of kelp beds.

North: Yes. Early on, after I got here, the Department of Fish and Game retained me as a consultant. It involved a week in Sacramento. The problem they were having was managing the living resources of San Francisco Bay. It was just a hellishly complicated problem, because there were, oh, I forget, ten or twelve different communities of people. And somebody in this town would do something, and that town would be upstream from this town, and the sewage from that place would go down to another one. Additionally, the bay was getting smaller and smaller, because there were construction projects dumping in a great deal of fill so they could put a new building in. What was really a magnificent resource in terms of living things—not just of species but of important food species, like Dungeness crab and various fishes and so forth—there were a number of fisheries in the bay, several of them, that were imperiled by the degradation of the quality of the bay. So Fish and Game, who had the primary responsibility for

the living resources, got me and three other people up there for a week to plan what we should do.

Erwin: Yes, brainstorming, I guess.

North: Brainstorming, exactly. And I was the chairman of the group. I had to write a report after we finished. But at any rate, I came out $1,400 richer. Back in my days at Scripps, every year Fish and Game would do an aerial survey of the kelp beds and send me the results. The way that worked was that we'd get $50,000 a year for our research program, and I'd give about $1,000 to Fish and Game, because they had an airplane and an aerial camera and everything. I'd give them this money to do a year-by-year survey of the beds. It was tremendously helpful to have these photographs. So when I came to Caltech, my big effort at that point was trying to restore kelp beds, and I thought if I could just get photographs of these beds that we were working in, and I could get evidence that they were expanding, people wouldn't have to take it on faith when I'd say it was going very well—I could have the hard evidence. So I put this $1,400 to use learning how to fly. And then I bought an airplane and had two holes cut in the floor so I could photograph vertically. Over the next thirty years or so, I did lots and lots and lots of aerial surveys.

Erwin: Yes. You say you have 12,000 photos.

North: Yes, 12,000 35-mm slides.

Erwin: Which, for the record, you have donated to Scripps.

North: I thought that would be good. A lot of people were asking me, "Do you have a photograph of this place at this time?" And I thought, Well, since they're of the kelp beds and the coastline, it would be most useful at Scripps. So that's why I gave them to Scripps. But it put me way ahead of anybody else who was doing kelp research. People were always asking me for my charts that we made, and so forth.

Erwin: Yes. Now, how did this work? You obviously couldn't fly and take pictures at the same time.

North: Oh, yes. I'd be flying along over a kelp bed, and the hole was between my feet. I'd pick up the camera and take the picture—

Erwin: And go back to the controls. [Laughter]

North: If you leave the plane out of control for a minute, it just keeps going straight ahead, and then it starts twisting. So I would tilt it up on one side, knowing that it was going to twist the other direction, and as it came by the horizontal I'd take the picture.

Erwin: Right. So you had it all calculated to the roll of your ship, you might say.

North: Yes. It took a little skill, but it really wasn't all that difficult.

Erwin: So you really were up there all by yourself, just flying around taking pictures.

North: Toward the last, my hearing went, and I couldn't hear the controller, so I would often have another pilot with me to tell me what the controllers were saying. And they'd fly the plane for me and then I wouldn't have to do this rolling business.

Erwin: Now, where did you keep your plane?

North: At Orange County Airport, which is now John Wayne Airport.

Erwin: What kind of a plane was it?

North: It was a Piper Cherokee, a low-wing plane.

Erwin: Does that have two seats in it?

North: It has four seats. It was a 140 horsepower engine. The plane cost $12,000. It actually cost more than that, because I bought it on time, the way you do an automobile. So by the end you've probably paid $15,000. But by the time I was through flying, it was worth $18,000, because no more light planes were being built. So for the ones that were left in flying condition, the prices went up.

Erwin: Why is that? Was it considered unsafe?

North: There was so much liability involved. The litigation society developed, and if something went wrong with a plane, why, the heirs [laughter] of the unfortunate pilot would sue the company for $200 million. So companies just quit building small airplanes. I sold it for $8,000 to one of my former technicians who had started his own consulting company and was very successful. He took over the consulting jobs I had, doing aerial surveys.

Erwin: Do you know how many hours you flew?

North: Yeah. It was about 900 hours total.

Erwin: So, less than you dove.

North: Oh, yes, much less. [Laughter] But 900 hours is actually a fairly experienced pilot.

Erwin: Yes, I would think so. You say you started in 1967, so this was almost over thirty years of flying.

North: Yes. 1967, and then I sold the plane—I can't remember.

Erwin: Was it in the nineties?

North: Oh, yes.

Erwin: Well, all of these things that you've done—your diving and your flying—you've had to be physically fit for all of these things, and it's remarkable that you were able to keep them up so well. It's impressive.

North: Yes. I miss being able to do it.

Erwin: You miss it, I'm sure.

North: Of course.