Photo courtesy A. Ingersoll

ANDREW P. INGERSOLL
(1940 –   )

INTERVIEWED BY
SARA LIPPINCOTT

April 13 and 26, 2004

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Pasadena, California

Subject area
Planetary science

Abstract
Interview in two sessions conducted by Sara Lippincott in 2004 with Andrew P. Ingersoll, Earle C. Anthony Professor of Planetary Science at the California Institute of Technology. Discusses parents' social activism in the 1930s. His youth and education at Amherst College (B.A. physics, 1960) and Harvard University (M.A. physics, 1961; PhD 1966); his early interest in atmospheres, oceans and meteorology; working with A. Arons and H. Stommel at Woods Hole on ocean acoustics. Recruited to Caltech in 1966 in planetary science; early atmospheric studies of Venus, Jupiter (Great Red Spot) and Mars; collaborates with G. Munch and G. Neugebauer. Involvement with NASA's *Pioneer 11* and *Voyager* imaging team at JPL; results of *Voyager*'s "Grand Tour" of Saturn, Uranus, and Neptune; his theories on winds and turbulence in outer space. The Shoemaker-Levy comet, Hubble Space Telescope observations, and Jupiter's effect on protecting the Earth from comets. Works with the Soviet *Venera* space program on Venus' atmosphere; visit to the Soviet Union in the 1980s. *Galileo* and photographing Jupiter's atmosphere; Europa lander to study its subterranean ocean. Discusses recent evidence of water on Mars, terraforming Mars, and colonizing planets. Concludes with administrative work at Caltech: Executive officer for planetary sciences (1987-1994); G. Wasserburg as division head

(1987-1988); Caltech committees; Caltech's core curriculum and the need for greater emphasis on research time. Teaching atmospheric dynamics; discussion of global warming; research in oceanography and the precession of the equinoxes.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

INTERVIEW WITH ANDREW P. INGERSOLL

BY SARA LIPPINCOTT

PASADENA, CALIFORNIA

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Executive officer for planetary sciences (1987-1994); G. Wasserburg as division head (1987-1988); participation on other Caltech committees. Discusses the core curriculum and the need for greater emphasis on research time than classroom time, the advisor system, and the planetary science option at Caltech; also discusses the present climate regarding greater funding for research but less support for the undergraduate program. Currently teaching courses in atmospheric dynamics; discussion of global warming; research in oceanography and the precession of the equinoxes.
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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Andrew P. Ingersoll **by Sara Lippincott**

Pasadena, California

Session 1	April 13, 2004
Session 2	April 26, 2004

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LIPPINCOTT: Tell us a little bit about where you were born and your family, and where you grew up. Do you want to start at the very beginning, and then we'll go on to your early interest in science?

INGERSOLL: Sure. I'll start before I was born. My parents were radicals from the 1930s, and that posed a sort of dilemma for me—at least, I saw it that way—because they truly believed that you could save the world, and they worked toward saving the world.

LIPPINCOTT: Were they Socialists?

INGERSOLL: They were Communists, and they were active in workers' rights and farmers' rights in the Midwest. My favorite subject was physics, and science, so I felt, as a teenager, that I wasn't following my parents' path and wasn't saving the world—at least not the way they were saving it. So that was the dilemma. The fact that I was good at science and loved it was the overwhelming thing that pushed me into science. It was sort of inspired; it was what I'd been meant to do.

LIPPINCOTT: Where do you think that impulse came from? There was no genetic component to it, was there? That is, were your parents scientists?

INGERSOLL: My parents were educated, smart people. My father's career was sort of messed up by World War II and all of his activist politics, but he was interested in economics. My mother was from the South. She went to college in Alabama, and she became radicalized just by seeing the segregation and hating it. She recognized that this was a bad situation. They met in Nebraska. She was spending a summer trying to help the farmers in the dust bowl—or in the northern extension of the dust bowl. My father was doing the same thing, and that's how they met. This was radical stuff; they were trying to prevent foreclosures of the farms. There were militant, strong-arm tactics in some cases, when they would organize all the farmers from a given region when a farm was being auctioned away and the bankers were there, or corporate agriculture, to buy up the farms.

LIPPINCOTT: This was in the Depression?

INGERSOLL: This was in the middle of the Depression, right—the mid-thirties. The farmers would pack the courthouse, carrying pitchforks and maybe shotguns, and discourage the outside buyers from bidding on the farms, and then the farmers would buy them back at a low price. That was the principle, but of course it was illegal and they got into a lot of trouble. It was partially successful—but it was militant strong-arm tactics.

LIPPINCOTT: Were your parents both members of the Communist Party at that point?

INGERSOLL: I don't really know. They didn't talk a lot about it.

LIPPINCOTT: And you weren't around yet.

INGERSOLL: I wasn't born until 1940. Now, my mother was from a lower-middle-class family in Alabama, but she'd gone to college.

LIPPINCOTT: What part of Alabama?

INGERSOLL: Montgomery. She went to the Women's College of Alabama, which was a small college. My father was from a well-to-do, successful family in Brooklyn, New York, so he was kind of the thirties equivalent of a hippie. He had all the privileges and decided to reject that privileged background. But he was not a hippie at all! He was very serious. Both my parents were very serious about their activism. It was like a religion to them, and they were very highly moral, principled, hard-working people, working toward their goals for the world. My grandfather, Raymond V. Ingersoll, was part of [Mayor Fiorello] LaGuardia's administration in New York City. My grandfather was the president of the Borough of Brooklyn. He had graduated from Amherst College in 1897 and, I think, had some connection to Quakerism. So there was a long family tradition of activism in causes—for unpopular but progressive causes, humanitarian causes. It was the continuation of a family tradition, in my father's case.

LIPPINCOTT: What was your father's name?

INGERSOLL: My father was Jeremiah Crary Ingersoll. Crary was my grandmother's maiden name.

LIPPINCOTT: And your mother's name?

INGERSOLL: Mineola Perry. So my middle name is Perry.

LIPPINCOTT: Did your father ever get to college?

INGERSOLL: He graduated from Amherst, just like his father, and I went to Amherst. So it wasn't really a disadvantaged background at all. Amherst was an elite college during that entire period.

LIPPINCOTT: And not particularly radical as a college, was it?

INGERSOLL: No, although my father had a mentor there—Colston Warne—who encouraged his activist philosophy.

LIPPINCOTT: Were your parents involved with the Wobblies at all—the IWW?

INGERSOLL: Well, not so much. My parents went from [organizing] small farmers in the Midwest to, in the late thirties, the South Side of Chicago. My father took a job in the steel mills in order to help with the labor organization of the mills. Up until that time, unions had been sort of elitist—there were machinists, bricklayers, and other craft unions. But an industrial union—where you organize, say, all the steelworkers in a company—was a new concept. There was the old American Federation of Labor, and then there was this new, radical concept of organizing an industry, which was the Congress of Industrial Organizations—the CIO. The steelworkers had never been organized. So my father took a job eight hours a day smelting steel in South Chicago so that he could organize the steelworkers. There were some real confrontations in the late thirties. They would call a sit-down strike, in which everybody would refuse to leave the factory but they wouldn't work. And the police would come in and drag them out.

LIPPINCOTT: Did your father get thrown in jail? Did he have any difficulties?

INGERSOLL: I don't think he was ever thrown in jail.

LIPPINCOTT: Or whacked on the head by a policeman?

INGERSOLL: Yes, there were some battles with police—and battles at picket lines trying to keep the scab labor from coming in and working in their places. Picket lines were not just a few people waving flags; they were solid masses of people at the factory gates, blocking traffic. Confrontational! Now, there was a limit to my parents' radicalism. My father was strongly inclined to go over to Spain and join the forces against Franco and the fascists. The elected government was a very leftist government, and Franco and the fascists were trying to take over the government.

LIPPINCOTT: The government was the side your father would have supported.

INGERSOLL: Yes. And my mother—so goes the family lore—said, "Don't go!" And she laid down an ultimatum: "I'll leave you, if you go," or something to that effect. So he didn't go. He went to the University of Chicago as a doctoral student, and then I was born in Chicago, in 1940.

LIPPINCOTT: What was he trying to get his doctorate in?

INGERSOLL: In economics. Then the war came. And although now he had a child and he was a little old—thirty by the time the war started—he still could see that he was going to be drafted. He believed in the war—he was not a pacifist. So he wound up as a tank commander in Europe and was in fact seriously wounded, but he survived. A bullet pierced his chest, nicked his pulmonary artery. And he had trouble with that, with clotting in his pulmonary arteries, ever since. He died, actually, at the age of fifty-one.

LIPPINCOTT: After he was wounded, was he then sent home from the front?

INGERSOLL: Yes, and I remember his homecoming. I remember an earlier one, too. He had a twenty-four-hour furlough, and I was almost four years old. I remember him coming in, in full dress. It was Christmas day and he had twenty-four hours, and I remember him in uniform coming to my crib and giving me a hug. Times changed, and my parents definitely supported the war, and they supported Roosevelt. I remember my mother—I guess it must have been in ’44—working on the Roosevelt campaign in Chicago. I remember stuffing envelopes. I don’t think I was of much use, but at least she dragged me along.

LIPPINCOTT: Do you remember the day the President died?

INGERSOLL: I remember the day the President died.

LIPPINCOTT: April ’45, I think it was.

INGERSOLL: Yes, I remember that very clearly.

LIPPINCOTT: It was quite a shock to everybody.

INGERSOLL: Yes. It probably shouldn’t have been, because he was very weak.

LIPPINCOTT: Yes, but I mean to children. I was very young, but I remember that day, too. It was as if somebody told you that God had died, because you were so used to having him there.

INGERSOLL: Yes. And during the war he was such a prominent figure, played such a prominent role.

So, that's my family background—and then after the war, we moved to Brooklyn, and my sister was born.

LIPPINCOTT: You moved to Brooklyn when?

INGERSOLL: '45. My sister was born in '45. She still lives in Brooklyn and works for City Hall in New York—Barbara Rothenberg.

LIPPINCOTT: Why did you move to Brooklyn? Because of your father's family connections there?

INGERSOLL: Yes, I think that was probably it. The reason for their being in the Midwest and Chicago was just labor organization.

LIPPINCOTT: Did your father get a job in Brooklyn?

INGERSOLL: He had some inheritance. He was not fabulously wealthy, but his family was well-to-do, so he went into publishing. He allowed his politics and his business sense to get mixed together, and that was probably a mistake as far as the business was concerned. He figured that all these veterans would come back, they'd be unemployed, the Depression would continue, and they would need a mouthpiece, a magazine that would appeal to them and also help them through the coming economic hardship. So he started a magazine with people who'd been publishing during the war for soldiers. It had some connection to *Yank* and *Stars and Stripes*, which were published during the war and directed toward soldiers, I understand.

LIPPINCOTT: Yes, they were.

INGERSOLL: So afterward, they rounded up some of the writers and people—and maybe some publishers—and started publishing something called *Salute*. But the environment was not what they expected.

LIPPINCOTT: No, things started to go rather well, didn't they?

INGERSOLL: The economy boomed, and the returning soldiers wanted nothing to do with the war. They wanted to buy houses in the suburbs and raise children. The baby boom was on, the building boom was on, and people wanted to forget the war—it was a bad dream. And *Salute* was a financial failure.

LIPPINCOTT: How long did it last?

INGERSOLL: Three or four years—throughout the late forties.

LIPPINCOTT: Did your father know Dwight Macdonald, who was editing *PM* about this time?

INGERSOLL: I don't know. Some of the names I do remember. My parents worked for the Henry Wallace campaign in '48, which got only two percent of the vote. My mother ran for a congressional seat in Brooklyn and got three percent of the vote.

LIPPINCOTT: Beating out Wallace.

INGERSOLL: Yes, she did better than the national ticket—the American Labor Party ticket. Paul Robeson, a black singer and also a radical. Vito Marcantonio, a congressman from Manhattan. Those are names I remember, and those people came to the house.

LIPPINCOTT: Were your parents still members of the Communist Party then, or had they been disillusioned by Stalin?

INGERSOLL: Well, the disillusionment came in the late forties and spread out over a period of time. And there was a split between those who became Maoists and those who sort of went to the right. The Maoists went to the left. There were Trotskyites, also, who went in some direction. The people who went right, I would say, became left-leaning Democrats, and that's the direction my parents took. They continued to work for leftist causes—against segregation, for unions—but it was mostly domestic leftist causes. [Laughter] They kind of left the international scene at that point.

LIPPINCOTT: So they were involved in the early civil rights movement.

INGERSOLL: Yes, although in the civil rights movement of the sixties, they were a little too old to really get involved, and my father's health was not good. For one reason or another, none of us participated in the sit-ins of the sixties. I was already a graduate student, and I was not a radical person.

I remember that my father, who was a philosophical person, liked to talk—liked to explain Marxist theory to his little children. I remember hearing this high moral tone of Marxism— "From each according to his abilities, to each according to his needs," how competition was wasteful and we would all just cooperate with each other in the final communist state. And I remember thinking—though I didn't say it, because I was too nice to my father—I didn't want to tell him that I didn't buy his beautiful picture, his religion. It was a religion. I didn't want to tell him that, but I remember thinking, "This is *too* simplistic, and either my parents are dumbing it down for me, in order to make me believe, or else they're terribly misguided themselves." And I never decided which—probably a little bit of both. But I remember feeling—

LIPPINCOTT: How old were you when you had this idea?

INGERSOLL: I don't know—probably anywhere from ten to fifteen, over a period of time. And I remember thinking, "Humans are not that nice. Competition is part of the world, and you're never going to abolish it."

LIPPINCOTT: Yes. That's almost a scientific observation, because that's the way nature works, isn't it?

INGERSOLL: Well, it was just my view of it. And at the same time, I was becoming a scientist in my own way, in junior high school and high school. That was my passion. I didn't have the same passion as my parents, and I felt sort of like a failure for that reason.

LIPPINCOTT: Were they puzzled by your interests?

INGERSOLL: No, I don't think so. They were very supportive. When I look back on it, they were wonderful parents. They had high moral principles of their own and they were supportive of their children.

LIPPINCOTT: Were they big readers?

INGERSOLL: They read stuff, yes. I would get an assignment in junior high school to read a book and write a book report, so I'd ask them, "What should I read?" And they'd give me amazing books on Marxist theory. [Laughter] Or psychology—they were into psychoanalysis; they may have had more problems than I recognized.

LIPPINCOTT: Well, those were hot topics back then, I think. You never actually gave a book report on any of these Marxist books?

INGERSOLL: Oh, I probably did. I remember that at some point I read the *Communist Manifesto* itself—maybe it was for a nonfiction assignment—and I gave a report. In New York City public schools—this was now toward the end of the McCarthy period; this may have been in high school—there were still a lot of sympathetic teachers who would encourage you to read the *Communist Manifesto*. Not so much that they expected you to believe it as that they felt students should be exposed to a wide range of ideas. I went to New York City public schools in Brooklyn.

LIPPINCOTT: All the way through? Elementary and high school?

INGERSOLL: Well, no. For elementary school, I went to Brooklyn Friends School, which was a Quaker private school, through sixth grade. Then I transferred to P.S. 246. Somebody decided that the “Public School 246” designation was not good enough, so they changed it to Walt Whitman Junior High School. And then I went to Erasmus Hall High School.

LIPPINCOTT: So you were going to junior high and beginning high school right at the peak of the McCarthy era, in the early fifties. Your parents must have really chafed under that.

INGERSOLL: They did, and a lot of their friends lost their jobs. They were schoolteachers or in sensitive jobs. By that point, my father had retrenched from his financial losses, and he was acting as a mortgage broker for poor communities in New York City. Somehow he had connections in the black community—areas where there were middle-class blacks or people who could buy a house but, because of prejudice and distrust, and maybe the low mortgage ratings of their neighborhood, they couldn’t get money from the bank. He would somehow help them, and that was his job. I don’t think he made much money. It gave my parents an income, but they had enough money so that they didn’t need the income.

LIPPINCOTT: Did your mother work?

INGERSOLL: She worked on volunteer projects and probably never earned much money.

LIPPINCOTT: What kind of neighborhood were you living in in Brooklyn? Whereabouts in Brooklyn?

INGERSOLL: Flatbush. Well, when I was going to elementary school, taking the subway two stops to Brooklyn Friends School, we were living in a rougher neighborhood, on the edge of Bedford-Stuyvesant, because my parents believed that we should integrate black neighborhoods. So we were living in a black neighborhood, and most of my neighborhood friends were black kids. But my parents drew the line about my going to the local school, so I went to the private school—Brooklyn Friends.

LIPPINCOTT: And then you moved?

INGERSOLL: Then we moved to Flatbush, which was definitely a middle-class, mostly Jewish, community. I was one of the few non-Jewish kids in my junior high school and high school. Suddenly all my friends were Jewish. When asked to write, "What's your religion?"—this was very important, because on the Jewish holidays the school would empty out—I put down "Protestant," even though I had nothing to do with religion at all. My parents were atheists, and my grandparents had been atheists, or Quakers.

LIPPINCOTT: But you put down "Protestant"?

INGERSOLL: [Laughter] Well, there was no atheist category, so I was a Protestant.

LIPPINCOTT: Your parents were never churchgoers?

INGERSOLL: Never churchgoers. I think my [paternal] grandmother was a practicing Quaker. So the class of thirty kids would empty out on the Jewish holidays, and two or three of us would show up—the Protestants. [Laughter] And the Catholics, of course, went to their own parochial schools.

LIPPINCOTT: Let's get to your interest in science. When did you begin to think about it?

INGERSOLL: I didn't like arithmetic in elementary school; it seemed like a bunch of memorization. But then algebra, in junior high school, was really a very profound experience. Suddenly I realized that by designating the unknown as *x*, or whatever, and then writing the equation that involved *x*, and then solving the equation and finding *x*—I said, "This is powerful! This is really powerful stuff!" And I could then suddenly solve problems that were.… I had resisted it, in fact. I said, "I can solve these problems in my head, and I don't need the *x*. This *x* is just some sort of fancy thing that my teachers are putting me through, and I don't want to deal with it." So I solved the problems in my head, and then as I got to *x* and *y*, I realized this is powerful stuff—that I can solve problems with *x* and *y* that I can't solve in my head.

LIPPINCOTT: Was that like an awakening?

INGERSOLL: It was sudden. The realization that it was powerful was a kind of awakening. I really liked it. And also you could suddenly appreciate numbers, negative numbers, zero, all these things you take for granted up until a certain point—you know, the rules of addition and distributive and associative rules. It all made sense to me, all of a sudden.

LIPPINCOTT: So it wasn't just the natural world you were looking at. You got into this in a kind of abstract way?

INGERSOLL: It started with math and then it continued into physics and chemistry in high school.

LIPPINCOTT: Did they have good physics in your high school?

INGERSOLL: I had a very good physics teacher, just excellent. My graduating class from Erasmus Hall High School had twelve hundred kids in it—the whole school was five thousand.

LIPPINCOTT: Oh, that's enormous, isn't it!

INGERSOLL: Yes. And they made no excuses about segregating students according to ability, so there were several levels, and if you were in the top level you were surrounded by very smart people who were very highly motivated. They were all going to colleges, and good colleges. So I had excellent classes at Erasmus Hall.

LIPPINCOTT: You must have been near the top of your class?

INGERSOLL: Well, I was the best physics student, although there was another guy—well, he and I were the best physics students. That's Sidney Ossakow, and he's a physicist at the U.S. Naval Research Lab in Bethesda, or someplace.

LIPPINCOTT: What sort of physics did they have at Erasmus Hall? Was it pulleys and levers, or did you go all the way into quantum mechanics?

INGERSOLL: No, no—no quantum mechanics. There wasn't much of a laboratory component; it was a theoretical introduction to mechanics and a little E&M—electricity and magnetism—and we used the book by [Francis W.] Sears and [Mark] Zemansky—a well-known high school text.

LIPPINCOTT: Any particle physics at all?

INGERSOLL: I don't think so, no. I really liked mechanics, you know—projectiles and levers and springs. That gave me a feeling of power, too—over the natural world. Suddenly, with the tools of algebra and simple physical systems you could make predictions about how the physical world would operate, and you could describe it quantitatively. It was beautiful in its own right.

LIPPINCOTT: Did you take apart radios, like a lot of kids did in those days, or anything like that?

INGERSOLL: I flew model airplanes. I also did woodworking in my basement. I had a small power-tool set and I built things. I didn't do radios and I didn't do chemistry. Some kids loved to make explosions; I didn't do that.

LIPPINCOTT: So your physics was all purely theoretical? That's what gave you pleasure in the subject?

INGERSOLL: Yes. I was not a terribly nerdy kid. I liked sports and I was good at sports—I was on the tennis team for Erasmus.

LIPPINCOTT: Where is Erasmus Hall in Brooklyn?

INGERSOLL: It's just about in the middle. Flatbush is in the middle, and Erasmus is in the middle of Flatbush.

LIPPINCOTT: Did you have any ideas about where you wanted to go to college? Did you think about Brooklyn College?

INGERSOLL: Since my father and grandfather had gone to Amherst, it was a natural for me.

LIPPINCOTT: Did they encourage that?

INGERSOLL: I had other relatives who'd gone to Swarthmore, so I applied to Amherst, Swarthmore, and Harvard and got into all three. I was a good student. In fact, I was discouraged from applying to all three by my guidance counselors, because there was a quota on students from New York City. No one wanted to admit that there was a quota, but there was, in which a big city high school like Erasmus would get only three students into Harvard each year.

LIPPINCOTT: That was because it was really a quota against Jews? And even though your name was Ingersoll, they felt that you would be—

INGERSOLL: No. Well, I don't think it was directly against Jews, it was just against New York City kids—because we could have flooded all those colleges. So I was discouraged from applying to all three because my guidance counselors thought I could probably get *into* all three. They said, "Make up your mind and apply to one."

LIPPINCOTT: Were they thinking that if you got into all three, then somebody else would suffer?

INGERSOLL: Exactly. But I did. [Laughter]

LIPPINCOTT: Well, good for you.

INGERSOLL: Well, I don't know, because I then went to Amherst. I was a slam-dunk for Amherst, and in retrospect I should have just applied to Amherst.

LIPPINCOTT: I guess you *were*, being a legacy and being at the top of your class.

INGERSOLL: I wasn't at the top; I was eighteenth in my class of twelve hundred. I had a really good high school experience!

LIPPINCOTT: Do you remember your physics teacher's name—the one you said was so good?

INGERSOLL: Isidore Lerner. "Mr. Lerner," of course, we called him.

LIPPINCOTT: And he taught you electricity and magnetism and the whole panoply of subjects?

INGERSOLL: Yes, and primarily mechanics. I just had a feel for mechanics, and that has stuck with me. I have a certain intuitive feel for things that I can visualize, things that move—macroscopic things. As I've gone on in physics, I have always liked macroscopic things. The subatomic world was too abstract. I could handle the math, but it was just something that didn't exist [laughter] except in theory. I really liked things that I could grab and push and pull on.

LIPPINCOTT: This is probably a diversion, but were you interested in cosmology at all?

INGERSOLL: Well, I had decision points as I moved into college and then into graduate school—What kind of physics was I going to get into? And here comes the next turning point: At Amherst, my freshman physics professor was Arnold Arons. It was a required course—much to the dismay of the average Amherst student. But I loved it!

LIPPINCOTT: You mean all freshmen had to take it?

INGERSOLL: All freshmen had to take freshman physics.

LIPPINCOTT: This is in '56?

INGERSOLL: The fall of '56. I was sixteen; I was very young. Another thing about New York City public schools, they would just skip you. So I skipped two grades—I skipped third grade and I skipped eighth grade. [Laughter] So I was sixteen when I graduated from high school, and I was twenty when I graduated from college.

Amherst had a core curriculum, but it included the whole gamut: history, English, physics, math. Everyone had to take the same core curriculum. And there was sort of a writing class, which was notorious.

LIPPINCOTT: [Laughter] Notorious? Why?

INGERSOLL: Well, they confused you in the writing class. What they were driving at was, they were encouraging students to think about the words you're choosing and think about what you're trying to say, think about how to say it. It's not just stuff that comes out of your subconscious; writing and speaking is a conscious, thoughtful effort.

LIPPINCOTT: You're supposed to take what's inside and get it outside, so that somebody else and understand it.

INGERSOLL: And to be aware of what you're doing when you open your mouth in class, or when you start scribbling. You know, "Be aware!" I think that's what they were trying to tell us, but they wouldn't tell us what they were trying to tell us. They just let us find out for ourselves what they were trying to tell us. They just stirred the pot and confused you. But eventually you began to sort things out and become aware of your own writing and your own speaking.

LIPPINCOTT: Were these all semester-long courses—these core courses?

INGERSOLL: They lasted for the whole year.

LIPPINCOTT: So as a freshman, you had no electives, really?

INGERSOLL: I think that's right—though maybe I had a foreign-language elective. I continued with French; I had had very good French in high school.

LIPPINCOTT: Were any of the other sciences part of this core?

INGERSOLL: Yes, chemistry and biology. By the time your sophomore year was over, you'd taken chemistry and biology, and it was all required. The English majors hated the physics course. [Laughter] I liked them all. I was kind of a trusting, accepting kid. The teachers said, "OK, we're going to give you all of the knowledge, and we're going to stuff it all into you in two years," and I said, "Great!" [Laughter] I was not a rebel. I said, "This is good stuff and they do a good job." I did want them to do a good job; I didn't want any namby-pamby courses. I accepted what they gave me.

LIPPINCOTT: How was Arons as a teacher?

INGERSOLL: He was very good. He was also instrumental in my career, because after my freshman year he found me a summer job at the Woods Hole Oceanographic Institution, where he worked and spent his summers.

LIPPINCOTT: That's a funny place for a physicist to work. What has it got to do with physics?

INGERSOLL: Well, ocean currents, ocean acoustics. He had worked during the war on ocean acoustics—sound [transmission] in the ocean—and that's very important for submarine detection. And he continued to spend his summers there, working with one of the great men in oceanography, who is now dead—Henry Stommel. Stommel and Arons had written a seminal series of papers in the fifties. I got a job in the same lab as Stommel and Arons.

LIPPINCOTT: What sort of work did you do there?

INGERSOLL: I was doing computations. This was before the era of computers, so I had a big machine, about the size of a breadbox. [Tape ends]

Begin Tape 1, Side 2

LIPPINCOTT: It was a calculator?

INGERSOLL: Yes. It would multiply and add big numbers. Of course, with a slide rule you could multiply numbers, certainly with three digits and maybe with four if you squinted carefully. But with this Marchant, I could multiply eight-digit numbers. I was doing a variety of things. I was analyzing data from some telegraph cables stretched from one point of land to another. The electrical current in a telegraph cable, believe it or not, would tell you how much water current was flowing through that channel. There was a telegraph cable between Cuba and Florida, and seawater is an electrical conductor, so the amount of seawater flowing through that channel between Cuba and Florida would set up an electrical potential between Cuba and Florida. And that would cause current to flow in the telegraph cable, which was an undersea cable. I was analyzing that data for a while. Then I did some calculations on underwater sound. I don't think it was much use to anyone. But I was at Woods Hole, and I was an undefined physicist. And I had always liked the outdoors. My father had taken me backpacking, and my uncle—also named Ray, the same name as my grandfather—had taken me skiing in the winters at Mount Washington. So I loved the outdoors, and here was a chance to do physics and apply it to understanding the outdoors and understanding weather and oceanography.

LIPPINCOTT: Yes. And there was also the beach. [Laughter]

INGERSOLL: There was the beach; the beach was good. But it was the chance to do physics and especially mechanics, the mechanics of ocean currents. So it was another awakening. I just said, "These are my people."

LIPPINCOTT: You turned more in the direction of applied physics, then?

INGERSOLL: Geophysics and meteorology and oceanography.

LIPPINCOTT: So, slowly, you were approaching planetary physics.

INGERSOLL: Well, I still call myself a meteorologist. My field is geophysical fluid dynamics. And that really happened after my freshman year in college, that summer at Woods Hole.

LIPPINCOTT: When you went back to Amherst for your sophomore year, were you determined on a major then—on the way you were going to go?

INGERSOLL: I was determined to be a physics major at that point; and I had this interest in geophysical physics.

LIPPINCOTT: When did the interest in fluid dynamics turn into atmospheric dynamics?

INGERSOLL: Well, it was simultaneous; it was atmosphere *and* oceans, right from that day. I went to sea later that summer. Arons really set a lot of things up for me. I went on a three-week cruise on the *Atlantis*, which was the original motorized sailing research ship that Woods Hole operated.

LIPPINCOTT: This would have been just when plate tectonics began to boom. I think they were starting to measure the midocean ridge.

INGERSOLL: Yes. I was not involved in that but I followed it, and these were very exciting times. It was a revolution.

LIPPINCOTT: But your ship wasn't taking those kinds of measurements.

INGERSOLL: No. We were collecting the dissolved gases in seawater.

LIPPINCOTT: How far out did you go in this ship?

INGERSOLL: Oh, we went to Bermuda—we sailed from Woods Hole to Bermuda, collecting seawater samples. Arons basically took one physics major—or one student—per year, and so I was working for the guy two years ahead of me. He was also at Amherst, a physics major—Winthrop Smith. He was doing a senior thesis, or an advanced project, and I was a freshman. We had to extract the gas from the seawater samples. The crew would pull these seawater samples up from four thousand meters, and we had to get the gas out of the seawater samples.

LIPPINCOTT: What kind of gas would you get?

INGERSOLL: Well, it's dissolved air, really, but it gets modified by biological activities, so it's worth studying. The way you get dissolved air out of seawater—at least with the equipment we had—you produce a vacuum over the sample, and it boils out. And how do you produce a vacuum? Well, you have a mercury pump. Mercury is terribly poisonous, and the vapor is poisonous—not terribly poisonous, but poisonous enough. So we had these big quart-size globs of mercury in this glassware, on this pitching ship. You had to put the seawater in the sample, then you opened a valve and the mercury poured down into some lower reservoir, and that created a vacuum above the sample, and the air then boiled out of the sample. But if you left the valve open too long, the mercury would get into the main pumping system and spew out on deck. [Laughter] It was pretty chaotic.

LIPPINCOTT: Did that happen when you were—

INGERSOLL: I don't think so. But it was chaotic.

LIPPINCOTT: How big a ship was this?

INGERSOLL: I think it was almost two hundred feet long.

LIPPINCOTT: A sailing ship, was it?

INGERSOLL: It had sails. It was a ketch. It also had a motor. I'm sure that a lot of mercury did get loose, and it probably found its way down into the bottom of the ship, rolling through three decks and slowly vaporizing in the bottom of the ship. I'm sure the bottom of the ship was full of mercury vapor. [Laughter]

LIPPINCOTT: You didn't suffer any ill effects from this.

INGERSOLL: No, I don't think I did. No, I had a good time—except for a little seasickness and trying to work too hard. We worked twenty-four hours a day, in shifts. Win Smith and I kept that mercury pump going twenty-four hours a day between us. [Laughter] It was, you know, boot camp for scientists. Kind of exciting. And then in our spare time, we'd watch the flying fish, some of which would land on the deck.

LIPPINCOTT: How many crew did you have? Was it just you and Win, and then some people to run the actual sailing part?

INGERSOLL: Well, there was the crew who operated the ship, and there was the scientific crew, and there was a chief scientist. There were other scientific experiments going on at the same time.

LIPPINCOTT: Was Arons on the boat with you?

INGERSOLL: He was not on the boat. Win and I were in charge of the dissolved-gas experiment.

LIPPINCOTT: This was the summer after your freshman year?

INGERSOLL: Yes, ’57.

LIPPINCOTT: And then you go back to Amherst and you become a physics major, I suppose, at the end of your sophomore year.

INGERSOLL: Yes. I briefly thought about going to med school, because I had friends who were in med school, and it just seemed like a way to combine science with doing something good. But the pre-meds at Amherst were not my kind of people. A lot of my friends were pre-med, but still they were not my kind of people in a deep sense, because they were more interested in their careers—their careers in the sense of the whole package, including the income and the comfortable life and the prestige. I was more interested in my career as something I would really enjoy doing.

LIPPINCOTT: Yes. And maybe you had some inkling that you might end up in an academic environment.

INGERSOLL: Yes, definitely. Let’s see, in the summer of ’58 I went to Europe with a friend, Steve Hulley, whose mother was British, so he had ties in Britain. We went to Europe and we hitchhiked around France and Italy and spent a lot of time with his relatives in England. We hitchhiked through England up to Scotland. That was a great summer.

Then in ’59, I went back to Woods Hole, and I had a job in a different lab—not Stommel and Arons, a lab with Duncan Blanchard and Al Woodcock. They were studying bubbles and sea foam and breaking waves, and processes at the air-sea interface.

LIPPINCOTT: Were they full time at Woods Hole?

INGERSOLL: They were full time at Woods Hole. I measured the electrical charge on the droplets that pop up when a bubble breaks. It sends up little droplets, and I measured the electrical charge. I was looking through a microscope. And down the hall, there was a lab where they were studying plankton, and there was a young woman who was sorting plankton species and *she* was looking through a microscope. And that was my wife, as it turned out. [Laughter] Sarah Morin—a French Canadian name.

LIPPINCOTT: This was a summer job for her?

INGERSOLL: A summer job for her.

LIPPINCOTT: Where was she going to school?

INGERSOLL: Well, I was nineteen, and she was seventeen. She had just graduated from Orleans High School, on Cape Cod. She was a merit scholar. There were two merit scholars from Cape Cod high schools, and Woods Hole, as part of its community relations, had offered summer jobs to these two merit scholars. The way I tell the story of how we met is that Woods Hole was the matchmaker. There were perhaps six or eight young men—mostly college students—working at Woods Hole that summer, and six or eight young women there that summer also, from either high school or college. And the way I tell it is that you could have taken any one of those young men and any one of those young women and put them together and they would have been a happily married couple—basically because we were all pre-selected, to have sort of a math-science interest and a scholarly interest. And my wife selected me.

LIPPINCOTT: She selected you, not the other way around? [Laughter]

INGERSOLL: Well, I'm sure I selected her, too, but she made the first move. And we've lived happily, more or less, ever since.

LIPPINCOTT: How long did you do your courting, so to speak, before you got married? She wanted to go to college, I presume.

INGERSOLL: She went to Wellesley.

LIPPINCOTT: Oh, so did I. She might have been there when I was there.

INGERSOLL: The class of ’63.

LIPPINCOTT: I was class of ’59. So we just missed each other.

INGERSOLL: She was married for the last two years at Wellesley. She was nineteen when we got married; I was twenty-one.

LIPPINCOTT: What house was she in? Do you remember?

INGERSOLL: Wilder? Is there a Wilder House?

LIPPINCOTT: No. Was she in the Quad?

INGERSOLL: I don’t know. Wilder may be a house at Smith—I dated a girl from Smith.

LIPPINCOTT: You dated a girl from Smith after you met Sarah?

INGERSOLL: No, before that. I met Sarah after my junior year.

LIPPINCOTT: So she went to Wellesley and you went back to Amherst. And you’re now a senior at Amherst.

INGERSOLL: Actually, the way we met, during that summer of ’59—there was a girls’ dorm and a boys’ dorm, and we would hang out in the common area of the girls’ dorm. It was early in the summer. We would hang out, doing nothing, in the evening, and someone said, “Let’s all go for a swim.” The water was cold. It was nighttime and it was still early in the season, and the water around Cape Cod is probably 65 degrees in late June. I said, “Oh god, I don’t want to go swimming!” [Laughter] And she said, “Oh, let’s go swimming!” She sort of encouraged me to go swimming, and I thought, “Well, an attractive young woman is encouraging me to go for a swim. I think I'd better accept, even though the water's cold." So a bunch of us did go down to the beach and went for a swim. There was a raft, which was used by everyone during the day. So she and I swam out to the raft and had a long talk.

LIPPINCOTT: And there was a moon?

INGERSOLL: Yes, it was very romantic. And we got dry on the raft, and then we had to get back in the cold water and swim to shore. Well, the way I tell this story is, she's never done any ocean swimming since, in her life. [Laughter] I'm the big swimmer. But she basically was doing it all for me. She said, "OK, this is the way to catch this man—go swimming in 65-degree water, get him away from everybody else, on the raft." That was a lot of fun.

LIPPINCOTT: So two years later, you got married?

INGERSOLL: Yes. Then I did a senior thesis at Amherst with Arnold Arons on a problem in geophysical fluid dynamics. I had an experiment, a big tank two meters in diameter, rotating, full of fluid. It was kind of a model of the ocean circulation. In fact, it was a laboratory analog of a paper that Stommel and Arons had written, which itself was a sort of thought-experiment analog of the ocean circulation. So I was doing the laboratory experiment.

LIPPINCOTT: Amherst seniors had to write a thesis?

INGERSOLL: Yes. I would like to sell that idea here at Caltech—the idea of a senior thesis. I haven't been very successful; I ran into a lot of resistance.

LIPPINCOTT: Resistance from whom?

INGERSOLL: Well, I installed myself as head of the Academic Policies Committee. [Laughter] Basically, I volunteered to be chair of it, so I've been the chair for one year, and I have found that some departments don't want to give up a slot in the curriculum of their required courses. They say, "Well, no, our students have to take this required course, and that one, and that one, and that one. And if you substitute a senior thesis, then they won't get the benefit of these required courses. They'll just kind of flop around and fritter away their time." It's that kind of resistance. And even some of the students resist it. They say, "Well, look, our time is totally filled with courses, and if you stuff something else into the curriculum and don't give up something, you'll just make our lives even worse."

LIPPINCOTT: Yes, and their lives are pretty tough now.

INGERSOLL: Oh, I don't know that their lives are so bad, but they do have a lot of work.

LIPPINCOTT: Well, we're going to get to your Caltech work, but I don't want to—

INGERSOLL: I'm just blabbing and blabbing! I haven't even gotten you to Caltech.

LIPPINCOTT: No, that's wonderful! It doesn't matter.

INGERSOLL: At least I've got you to the end of college.

LIPPINCOTT: Yes. So, you do your thesis with Professor Arons, and of course by now you know you want to go on to graduate school. Who advised you about that? Where did you apply?

INGERSOLL: Well, that was another slam-dunk, because my girlfriend, Sarah, was a student at Wellesley. Although she's two years younger, she was three years behind me in college, so she was entering her sophomore year as I was entering graduate school. So I applied to Harvard and MIT and got into both.

LIPPINCOTT: You were at Harvard in 1960.

INGERSOLL: Right. I could have gone to MIT. I applied in the meteorology department at MIT, and I had an unfortunate interview with the head of the department, who emphasized how important weather forecasting was and reading weather maps was. That seemed sort of old-fashioned to me. In contrast, Richard Goody, who was head of this much smaller and maybe more fledgling atmospheric department at Harvard said, "Well, we're all into understanding the basic physics behind atmospheres and oceans. We're not really doing weather forecasting day-to-day; we're interested in a deeper understanding." And that sort of sold me. In fact it was not a correct characterization of MIT by any means, and the two programs were much more similar than I had thought. But anyway, I chose Harvard. Richard Goody is an inspiring guy. He's in his eighties now, and he's still active, and he goes around inspiring people. He's spent a lot of time at JPL [the Jet Propulsion Laboratory] creating programs and getting people moving in the right direction, usually.

LIPPINCOTT: Were you focused still on the earth—on this planet we're on?

INGERSOLL: Yes, I was still focused on the earth. I did take a course from Goody on planetary atmospheres. But I also took a course from Henry Stommel on oceanography—he was the guy for whom I'd worked at Woods Hole. I took a course from Allan Robinson on oceanography. There was a visiting professor, who in fact was visiting from MIT, and he taught a course. His name was Ed [Edward N.] Lorenz and he had this ancient—I mean, it was not ancient at the time; it was in fact a rather new thing. It was an analog computer. It looked like an old telephone switchboard, with cables going from one place to another. And he was discovering chaos, but we didn't know it. Ed Lorenz was the father of chaos, and he was discovering the Lorenz equations.

LIPPINCOTT: And this was based on meteorology?

INGERSOLL: That was the motivation—meteorology. It was an oscillator that would suddenly change direction for no apparent reason. It was not a simple oscillator, but it was simple enough.

LIPPINCOTT: He was trying to create random motion?

INGERSOLL: Yes, from a deterministic system. The equations are perfectly well known: There's no randomness that you're putting *in*to the system; it's entirely calculable. But there's always enough uncertainty in the initial conditions so that it leads to unpredictable behavior.

LIPPINCOTT: So the randomness emerges.

INGERSOLL: It emerges from the system. And I remember—and I kick myself—he had this analog computer whirring away, flashing its lights, in the basement of Pierce Hall at Harvard. And this oscillator would suddenly change direction and go off in a new direction and then switch back for no particular reason. And we said, "Yes, we understand that, no big deal." We thought we understood it, and we didn't realize that we were seeing the birth of a whole field of mathematical physics. [Laughter] We just didn't appreciate it. I mean, it's not that we didn't realize that it was good stuff, but we didn't appreciate how profound it was.

LIPPINCOTT: Yes. And out of that, now, come the Santa Fe Institute and all the studies on complexity theory.

INGERSOLL: Oh, yes. It's all based on Lorenz's initial work. He was the pioneer. And it was going on in the basement, where I had a lab next door, and I didn't see that this was really revolutionary. I just didn't see it.

LIPPINCOTT: Well, that was a very early time for people to be interested in things like that.

INGERSOLL: Well, I somehow thought I understood the system and that it was nothing particularly new.

LIPPINCOTT: Is Lorenz still alive? Or working?

INGERSOLL: I don't know [Edward N. Lorenz is emeritus professor in the Department of Earth, Atmospheric, and Planetary Sciences at MIT—ed.].

LIPPINCOTT: Then Murray Gell-Mann got very interested in these questions, maybe not too long afterward.

INGERSOLL: Yes, but I had no idea who Murray Gell-Mann was. I had no idea what Caltech was, at that point.

LIPPINCOTT: I was at Harvard in that year, ’60 to ’61. When I got out of Wellesley, I went to work for the chairman of the Biological Laboratories.

INGERSOLL: Well, I’ll be darned!

LIPPINCOTT: I thought it was kind of funny, too, because I was an English major. [Laughter] But actually that’s what turned me on to science. It was the zeal of those people.

INGERSOLL: Yes, there is a feeling. It was like my first summer at Woods Hole—“These are my people,” the intensity of people who are really excited about their work.

LIPPINCOTT: Exactly! And not only that, they’re dedicated to it. As you say, the difference between the pre-med people and these other people—it’s night and day! The pre-med people are kind of secular. And all my experiences at Wellesley—being an English major, my friends being literary—

INGERSOLL: Well, secular. For the pre-meds, the science was secondary; it was a means to this good life that they were after, for many of them. Of course, many of my friends are now medical researchers.

LIPPINCOTT: But it’s not the same thing, is it?

INGERSOLL: Well, they’re scientists; they’re not treating patients. Or they’re treating patients in a more advanced research center.

LIPPINCOTT: But it was a completely different mindset—the people I met in the Bio Labs. One of them was Ed Wilson—E. O. Wilson. Did you meet him when you were there?

INGERSOLL: I don’t think I met him. I may have gone to a lecture.

LIPPINCOTT: James Watson was there. It was an interesting time.

INGERSOLL: Yes, it was. It was exciting.

LIPPINCOTT: So you were there, and you got your PhD in '66?

INGERSOLL: I defended my thesis in June of '65. I just missed graduation in '65.

LIPPINCOTT: Tell me a little bit about the work you did there.

INGERSOLL: A few years of course work—I studied meteorology and oceanography, and also took physics courses, because I was still a physics major at Harvard. As a graduate student, I was in the physics department, which sort of appealed to me. Instead of being in a meteorology department, I was in a physics department—although I was really specializing.

LIPPINCOTT: Whom did you take courses from there, again?

INGERSOLL: Goody and Robinson. Allan Robinson, who's still a practicing oceanographer. George Carrier, who was in applied math—a very good teacher, very inspiring.

LIPPINCOTT: Ed Purcell was there then.

INGERSOLL: Ed Purcell was in the physics department.

LIPPINCOTT: Norman Ramsey?

INGERSOLL: Yes, applied physics.

LIPPINCOTT: Did you have anything to do with—

INGERSOLL: I don't think I had courses with Ramsey.

LIPPINCOTT: Was Schwinger still there?

INGERSOLL: Julian Schwinger, yes—he would pace up and down the hallway thinking these thoughts. For a Harvard PhD, you had to be able to translate a paragraph of foreign scientific writing into two different languages. I had no trouble with the French, because my high school

French was good, but I had to take a cram course in Russian or some other language. So I took a cram course in Russian one summer and successfully passed the language requirement in Russian. Bernard Budiansky [professor of structural mechanics at Harvard] was in the same cram course, because he, for some reason, wanted to learn Russian. We all spoke Russian in this class, as best we could, and I remember someone was planning to visit the California Institute of Technology, and the language teacher said, “Oh, isn’t that like MIT, but not as good?” And Budiansky, in clear Russian—remember, our vocabularies were very limited—said, “Small, yes, but very good.” That was my first inkling of Caltech’s existence, in that cram course in Russian.

LIPPINCOTT: Meanwhile, at Caltech, people were beginning to talk about getting planetary sciences into the geology division.

INGERSOLL: Right. And we read this seminal paper by [Bruce C.] Murray and [James A.] Westphal, in which they had measured temperatures of Venus and made infrared observations of Venus’s atmosphere and Jupiter’s atmosphere.

LIPPINCOTT: Hadn’t the Russians sent some probes to Venus by the early sixties?

INGERSOLL: Yes, they had.

LIPPINCOTT: But a lot of them burned up, didn’t they?

INGERSOLL: Yes. They had been partially successful but they hadn’t reached the surface. The U.S. had sent a probe that flew past Venus—*Mariner 2,* I believe. And then there was a successful *Mariner* that flew by Mars. But I remember reading the paper by Murray and Westphal—Murray, [Robert L.] Wildey, and Westphal.

LIPPINCOTT: Where did it appear?

INGERSOLL: Oh, in *Astrophysical Journal* or something like that [B. C. Murray, R. L. Wildey, and J. A. Westphal, “Infrared Photometric Mapping of Venus Through the 8-14 Micron Atmospheric Window,” *Jour. Geophys. Res*. 68:4813-4818 (1963)]. They had used the 200-inch telescope on Mount Palomar, and they'd used it with this rather new kind of technology—infrared detectors—to measure the temperatures of the planets. So that was my second awareness of Caltech. But there's a great deal of East Coast snobbery, especially if you're at Harvard. Harvard considered the rest of the world substandard. [Laughter]

LIPPINCOTT: Well, it is, you know. [Laughter]

INGERSOLL: Well, I don't know. Harvard is stuck on that. So I dimly became aware that there was another side to the North American continent [laughter] and that there were some distinguished institutions out there.

LIPPINCOTT: But you had no thought of going to Caltech, at that point?

INGERSOLL: No. My thesis was both a theoretical study and a laboratory study of convection and shear in a fluid—another rotating fluid. The fluid we used, the experimental fluid, was silicone oil, which had nice properties—watery stuff, but silicone oil. It had applications to geophysical flows. In retrospect, I shouldn't have picked such a dull topic. I should have picked something that was more applied, maybe something that had more to do with planetary atmospheres. But it was a nice, clean laboratory setting, and I did some theoretical work.

LIPPINCOTT: Did this tell you something about what happens in the oceans?

INGERSOLL: Yes, I suppose relevant to the boundary layer between the surface of the ocean and the atmosphere flowing above it. Because you have a shear flow, where the velocity of the air at the ocean interface is very small but the velocity of air above the interface is large—that's what we call shear. And then with convection going on and the temperature difference between the atmosphere and the ocean.

LIPPINCOTT: Who did you do this work with?

INGERSOLL: Goody. Goody didn't really know a lot about that particular subject, but he shepherded me through it and provided me with funding and directed me to the right laboratory people who could help me. As part of my theoretical work, I used the Harvard computer, an IBM 7090, which was an amazingly big computer. It filled a room with electronics.

LIPPINCOTT: Was that one of the really early ones?

INGERSOLL: It was pretty early—’63, ’64. You’d punch your program out on punch cards, put the box of cards on the counter. The total memory in the computer was measured in kilobytes. So, if you had a hundred kilobytes, you were asking for a lot of memory. Of course now, we talk about gigabytes and terabytes—this wasn’t even megabytes, it was kilobytes. It was a big deal to be asking for kilobytes.

LIPPINCOTT: Now were you ready to go for a postdoc somewhere? Did you want to teach? What did you want to do?

INGERSOLL: Well, I talked to Walter Munk, the great oceanographer at Scripps, and he offered me a postdoc—very attractive—in oceanography. I talked to Knauss—John Knauss, an oceanographer at the University of Rhode Island. And then, out of the blue, Jerry [Gerald J.] Wasserburg [MacArthur Professor of Geology and Geophysics, emeritus] showed up at Harvard and asked, “Who’s a bright student, just getting a PhD?”

LIPPINCOTT: This was in ’66?

INGERSOLL: This was in ’65—the summer of ’65, in fact. So Goody said, “Well, Ingersoll’s here.” And Wasserburg said, “Well, we’re looking for assistant professors. We have tenure-track jobs in planetary science.”

LIPPINCOTT: Yes, I think that was just when they were trying to branch out. And Robert Sharp was still the chair of the geology division.

INGERSOLL: Robert Sharp was the chair, and a great man. And Wasserburg may have subsequently regretted the fact that he found me.

LIPPINCOTT: [Laughter] Why?

INGERSOLL: No one measured up to his standards.

LIPPINCOTT: I know; he's like that. He's very prickly.

INGERSOLL: Yes. But anyway, he did find me.

LIPPINCOTT: And Peter Goldreich [DuBridge Professor of Astrophysics and Planetary Physics, emeritus], too, came to Caltech at around that time.

INGERSOLL: I think Sharp knew of Peter's existence. Peter had a job at UCLA. He was already an assistant professor or associate professor at UCLA, and he got beaten up in the squash courts at UCLA by a football player. [Laughter]

LIPPINCOTT: You mean literally?

INGERSOLL: I think so—literally beaten up by a football player. And Peter, if not pressing charges, at least went to the administration and said, "Look, this guy beat me up in the squash courts." And UCLA said, "Look, he's the star of the football team." And Peter was incensed. Bob Sharp somehow got wind of this and said, "Well, Peter, come across town. We have our priorities straight. The faculty are more important than the football players." [Laughter] And Peter said, "OK." So we showed up together. I was an assistant professor, and he probably had tenure at that point.

LIPPINCOTT: So Wasserburg interviewed you when he came to Harvard?

INGERSOLL: Yes. But mostly he just was trolling for fish.

LIPPINCOTT: Were you the only person he took?

INGERSOLL: Yes.

LIPPINCOTT: So how did you two hit it off?

INGERSOLL: I remember a secretary. [Laughter] It was the summer of ’65, and I was at Woods Hole, and there was a secretary named Mary Thayer. I was nowhere to be found when Wasserburg showed up at Woods Hole—I was in the library or something—and he started pounding on the desk of this nice old lady, Mary Thayer, who had been the administrator for the summer course in geophysical fluid dynamics for twenty years. She was an institution herself, and he was pounding on her desk: “Well, you *make* him appear!!” [Laughter] So I showed up two hours later and he’d gone, and she said, “Well, there’s a man who wants to interview you about a faculty position, but I wouldn’t go work for him in a million years.” [Laughter]

LIPPINCOTT: Did he stick around town so you could see him the next day?

INGERSOLL: Yes, we had an interview. But then the whole official process had to happen. So Caltech flew me out there in December ’65. I had been awarded my PhD.

LIPPINCOTT: Except you hadn’t received it formally?

INGERSOLL: Right. I was just hanging on as sort of a postdoc with Goody. So the formal interview took place I think in December, and I came out and gave a seminar and spent two days here, met all the faculty of the division. I remember showing up with my Boston overcoat draped over me, a big heavy thing.

LIPPINCOTT: And your mittens? What was your impression of Caltech, and of Pasadena, then—physically?

INGERSOLL: Oh, it was pretty darn good. I slept out in the open air, at the Athenaeum.

LIPPINCOTT: In that loggia?

INGERSOLL: In the loggia. And I said, “I’m not in Kansas anymore. I’m not in Boston.”

LIPPINCOTT: What did your wife think about the possibility of moving to the West Coast?

INGERSOLL: She was game.

LIPPINCOTT: What did you give the seminar on?

INGERSOLL: On my thesis—on this thermal convection with shear, in a laboratory fluid.

LIPPINCOTT: Bruce Murray [professor of planetary science and geology, emeritus] was there?

INGERSOLL: Bruce was there. I met Bruce, Don Anderson [McMillan Professor of Geophysics, emeritus], Barclay Kamb [Rawn Professor of Geology and Geophysics, emeritus], Bob Sharp, and Jerry Wasserburg—they were all there. Those are the people I remember. On the airplane, I had read a book on plate tectonics, so I was able to talk plate tectonics with Don Anderson, who was kind of reluctant about it—he was not ready to embrace it yet. He was holding out.

LIPPINCOTT: Really? That's a bit late to hold out on plate tectonics, isn't it?

INGERSOLL: December '65. I don't really know how reluctant he was, but he was trying to be a healthy skeptic. But I had read this book, and I'd been following it anyway. So we talked about plate tectonics. [Tape ends]

ANDREW P. INGERSOLL

SESSION 2

April 26, 2004

Begin Tape 2, Side 1

LIPPINCOTT: In our last interview, we got you as far as coming out to Caltech to give a seminar. In December ’65, you were a postdoc at Harvard with Richard Goody.

INGERSOLL: That’s all correct.

LIPPINCOTT: So, at the seminar here, you had most of the department people there—Robert Sharp, who was the division head, and Jerry Wasserburg, who recruited you. Who were some of the other people you remember as being there?

INGERSOLL: I remember Barclay Kamb; he asked very penetrating questions. And I think Peter Goldreich was there—although I’m not sure about Peter; maybe he wasn’t there in ’65.

LIPPINCOTT: They got him from UCLA.

INGERSOLL: About the same time. But I do remember Barclay’s penetrating questions. And I talked—not during the seminar, but afterward quite a bit—to Don Anderson. I remember him. And I remember Clarence Allen [professor of geology and geophysics, emeritus]. And Bruce Murray, who was Caltech’s main planetary scientist. Harrison Brown had recruited Bruce Murray—Harrison Brown was a geochemist.

LIPPINCOTT: Where had Bruce Murray come from? Do you remember?

INGERSOLL: He came from MIT. Bruce was a sedimentary geologist, but by that point he had fully changed his field of research to planets, and he and Jim Westphal had pioneered the use of the telescopes on Mount Wilson and Mount Palomar for planetary work—especially infrared work. And of course infrared astronomy was growing by leaps and bounds, with Gerry

Neugebauer [Millikan Professor of Physics, emeritus] and Bob [Robert B.] Leighton [Valentine Professor of Physics, d. 1997]. But I didn't meet those people for a year or so.

LIPPINCOTT: Were you aware that you were being recruited as a planetary scientist at Caltech? And were you interested in planetary science at that stage?

INGERSOLL: Well, that's an interesting question. I was aware that I was being recruited for planetary science. I wasn't sure if I had the credentials, or if I knew anything about planetary science, because I had been trained to do fluid dynamics of atmospheres and oceans, and my thesis had been this laboratory theoretical study of fluid dynamics. And in the first year at Caltech, I even worked on some problems in fluid dynamics. I was twenty-six years old when I came here, and I wasn't fully prepared to run my own program and choose wisely and choose research topics with a long-range view. I sort of jumped around, tried a few things.

I guess my first significant planetary science paper was entitled "The Runaway Greenhouse: A History of Water on Venus" [*Jour. Atmos. Sci.* 26:1191-1198 (1969).] That was fun, and that actually grew out of one of the first classes I taught. I had taken a rather standard problem in atmospheric science, which was to compute the temperature at the surface, when the planet is in equilibrium with the sun and with the infrared radiation of space. And just for this homework problem, I added one novel twist. I said, "Let the amount of greenhouse gases in the atmosphere—the principal one of which is water—let that be controlled by the temperature that you get." There was a feedback loop built in there, because the idea was that the warmer it got, the more water vapor you would have. And the more water vapor you had the bigger the greenhouse effect; and that would make it even warmer.

LIPPINCOTT: So it would be like a runaway effect?

INGERSOLL: Yes, and that was a homework problem. And I had simplified it down so that students could do it in an afternoon.

LIPPINCOTT: Were these undergraduates or graduate students?

INGERSOLL: I think they were mostly graduate students. The problem blew up. It gave a surprise. And I had to sort of apologize to them, because they struggled and struggled with the problem. They got an answer for the Earth, but I had asked them to extend it to other planets, and there was no solution of the problem for Venus.

LIPPINCOTT: Why wasn't there? Can you explain why?

INGERSOLL: Well, because it ran away. We all agreed, finally, that on Venus the oceans had to boil away and you'd wind up with a water-vapor atmosphere. Your entire oceans would be in the atmosphere, and it would be a very heavy, thick atmosphere, made up almost entirely of water vapor.

LIPPINCOTT: Is that in fact what was known about Venus at the time?

INGERSOLL: No. Actually, planetary science was an amazingly exciting field. We didn't *know* how much atmosphere there was on Venus, and we didn't know what it was made out of. Or Mars.

LIPPINCOTT: You didn't know whether they had oceans, either.

INGERSOLL: It could have been cooler and wetter on Venus than it turned out to be. The estimates had been that, well, maybe Venus had twice as heavy an atmosphere as our own—in fact it's ninety times our own. And radio astronomy observations were just coming in, telling us that the surface temperature of Venus was *extremely* hot. The radio waves actually penetrated through the atmosphere. You could see the surface at radio wavelengths.

LIPPINCOTT: That was the work that Bruce Murray was doing?

INGERSOLL: No, that was some people at the Owens Valley Radio Observatory [OVRO]. Shortly after I arrived, Dewey [Duane Owen] Muhleman [professor of planetary science, emeritus] joined the faculty, and he continued that work. And Glenn Berge, who was a postdoc or something, maybe in the astronomy department—he was doing some of that work. But they were telling us—and then Russian probes confirmed—that Venus was incredibly hot at the surface, and that really ruled out an ocean.

LIPPINCOTT: It didn’t rule it out for the past, though, did it?

INGERSOLL: That was the question. Could you have had an ocean in the past, and how would it have lost that ocean? And this homework problem told us that Earth is on one side of a cliff and Venus is on the other, and if you slowly move Earth toward Venus, it will fall off the cliff too and you’ll get a totally unearth-like planet, with no oceans, incredibly hot temperatures, and a water-vapor atmosphere. Then you had to explain, well, what happened to the water-vapor atmosphere on Venus? And that was the other part of the research I did. Several famous people—Carl Sagan, Thomas Gold, and Isaac Asimov—had speculated about this whole subject.

LIPPINCOTT: About Venus?

INGERSOLL: About Venus, yes. And they had concluded that getting rid of the water vapor atmosphere wouldn’t work, and that’s a problem because Venus doesn’t have a water-vapor atmosphere today. But I extended the calculations and decided that the water-vapor atmosphere—which they had assumed would be shielded at the top from ultraviolet radiation by the remnant nitrogen and oxygen or whatever else was in the atmosphere—I concluded that it *wouldn’t* be shielded, that the nitrogen and oxygen would be mixed in and not all residing on the top. And then, since it was not shielded from ultraviolet radiation, the water would be broken up, and the hydrogen would escape and the oxygen would react with the surface rocks, and you could wind up with this hellish place that Venus is today. That was really my first foray into planetary science; I’d written a few papers on fluid dynamics.

LIPPINCOTT: Your paper was explaining why the water vapor wasn’t still there?

INGERSOLL: Yes. It was, first of all, explaining why Earth and Venus would have such different evolutionary paths. People like Tom Gold and Carl Sagan had speculated on, but hadn’t really developed, a quantitative model that showed there’s this cliff and Venus was on the other side, off the edge of the cliff.

LIPPINCOTT: But by this time, the components of Venus's atmosphere were known—is that right?

INGERSOLL: Yes. Radioastronomy had shown the high temperatures and the Russians had shown that it was mostly carbon dioxide and not much water vapor.

LIPPINCOTT: What was the date of the paper?

INGERSOLL: It was 1969, in the *Journal of the Atmospheric Sciences*. I had a terrible time getting that paper published. I submitted it to *Science*; I submitted it to the *Astrophysical Journal*—and it was rejected by both places.

LIPPINCOTT: Do you know why?

INGERSOLL: Yes, I do. They send you the referees' comments, and in both cases they said it was wild speculation. It was meteorologists who were refereeing it, and the meteorologists don't think like planetary scientists. They think in terms of Earth's atmosphere and Earth climate. And even with ice ages—which actually happened on Earth—that's still a very limited range of variation compared with, say, Venus, which is unimaginable right now as part of Earth's history. It was just outside their imagination, and they regarded it as too speculative to be worth publishing.

LIPPINCOTT: There were also things that happened on Venus maybe billions of years ago. Did you have any idea about the past—?

INGERSOLL: Well, once you have a watery planet at the orbit of Venus, this boiling away of the oceans would happen very fast. And then it would take quite a long time to break down the water-vapor atmosphere—maybe, who knows, a billion years. But the initial boiling away of the oceans would happen immediately. And in that paper I estimated how long it would take to break down the atmosphere.

LIPPINCOTT: Did you say anything in the paper about how long Venus might have been an OK planet?

INGERSOLL: Well, it would have been hellishly hot right from the start. It never would have had a chance, really.

LIPPINCOTT: Because of its proximity to the sun?

INGERSOLL: Its nearness to the sun, yes. Still, I was sort of naïve, and having this paper rejected twice before it was published, I kept modifying it. That was sort of a bad experience for me. I abandoned that research. I said, "Well, everyone's telling me this is too speculative, and I don't want to be labeled as a speculative person." So I abandoned it.

LIPPINCOTT: Did you have a coauthor?

INGERSOLL: No, it was all me. I coined the phrase "the runaway greenhouse," and I used it as the title of the paper, and that became sort of common—you know, everyone knows the runaway greenhouse effect. Some other people published a paper in *Nature* a few months after mine, based on talks I had given, and for a long time their paper was the standard paper to quote.

LIPPINCOTT: Who were these people?

INGERSOLL: Ichtiaque Rasool and Catherine de Bergh ["The Runaway Greenhouse and the Accumulation of CO_2 in the Venus Atmosphere," *Nature* 226:1037-1039 (1970)].

LIPPINCOTT: Where were they?

INGERSOLL: Catherine was at the University of Paris and Ichtiaque was at Goddard Space Flight Center in New York City. I don't think they were trying to steal my ideas; they were just trying to expand on them, and they were publishing after I did. But somehow their paper became the standard reference for the subject for about ten years, until people sort of rediscovered my paper.

So it's interesting—you learn a lot, and sometimes it hurts. I was not well prepared, as I said, for running my own research program, and I had to flounder around.

LIPPINCOTT: You used data from both OVRO and also Bruce Murray to base your paper on, is that right?

INGERSOLL: Well, yes, although I think by the time I finally got that paper published, it was generally known that Venus had a hot surface and that the atmosphere was made mostly of carbon dioxide. It was OVRO that had discovered the hot surface, and it was *Venera*, the Soviet space probes, that had discovered the carbon-dioxide atmosphere—and both the high temperatures and the composition measurements had ruled out substantial amounts of water. So Venus, unlike Earth, was just very dry. We knew that at the dawn of the space program.

LIPPINCOTT: How did your colleagues feel about that work? You had trouble with *Science* and the *Astrophysical Journal*. But did you have champions here at Caltech?

INGERSOLL: Well, Peter Goldreich was very supportive. I also wrote a paper about Jupiter's Great Red Spot in that period, and that was a good paper, too ["Inertial Taylor Columns and Jupiter's Great Red Spot," *Jour. Atmos. Sci.* 26:744-752 (1969)]. I was trying to understand why a fluid-dynamical object could hold itself together for three hundred years—you know, the incredible stability of something that's just a fluid-dynamical object.

LIPPINCOTT: It's almost like a perpetual-motion machine. You have to figure out why it keeps—

INGERSOLL: Well, there are two problems; one is, where does it get its energy?

LIPPINCOTT: It's a storm, basically?

INGERSOLL: It's a storm, yes. It's a swirling storm—an anticyclone, with the high-pressure center spinning counterclockwise in the Southern Hemisphere. But there are two problems: One is, What's its energy supply? And the other is, What keeps it from flying apart into pieces?

Where does it get its stability? You could have a stable structure that doesn't radiate away, but if there's some friction, it will slowly die out. You have to explain both these things. So I started a series of papers at that point—a couple of years after I'd come here—and I'm continuing that research even today.

LIPPINCOTT: By this time, did you have some graduate students?

INGERSOLL: Well, let's see. My first graduate student was Glenn Orton. I also shared a graduate student—Hugh Kieffer—who had been Bruce Murray's graduate student and worked with Barclay Kamb on Mars polar caps and stuff like that. I took an interest in Hugh's research on Mars ice and polar caps.

A pioneering paper had really started off an interest in the Mars polar caps; it was by Leighton and Murray—Bob Leighton in the physics department and Bruce Murray in planetary science. And in 1966, they published a great paper showing that the Mars polar caps were likely carbon-dioxide ice ["Behavior of Carbon Dioxide and Other Volatiles on Mars," *Science* 153:136-144 (1966)]. No one really knew whether they were or not. You could see them in the telescope, but you couldn't tell whether they were water ice or carbon-dioxide ice.

LIPPINCOTT: Why couldn't you, from a spectrograph, tell what they were?

INGERSOLL: Well, part of the trouble is that from Earth, you can't resolve—you can't put a spectrometer just on the polar caps. They're too small. And secondly, since we have water vapor and carbon dioxide in Earth's atmosphere, a lot of the spectral lines are blocked by our own atmosphere.

LIPPINCOTT: How did they, then, determine it was carbon-dioxide ice?

INGERSOLL: Well, they first of all used the results of one of the early *Mariners*—*Mariner 4*, which measured the thickness of the atmosphere; and the pressure at the surface was much lower than people had estimated from ground-based spectroscopy. Instead of being ninety millibars—one bar being Earth's atmospheric pressure—it was more like six or seven millibars. And then from the amount of carbon dioxide in the atmosphere—the spectral lines told you that amount—

Leighton and Murray were able to theoretically calculate that there was a great likelihood that carbon dioxide was condensing on the surface [of the caps], especially in the winter. So it was really the determination of the atmospheric pressure and the realization that there was enough carbon dioxide in the atmosphere to condense on the surface. And then they constructed a theoretical model showing that, number one, maybe a meter of CO_2 dry ice would freeze out in the wintertime at each pole. And number two, a much more important conclusion, was that the *permanent* dry ice that lasted throughout the summer—and lasted year-round—would regulate the atmospheric pressure, so that the entire climate of Mars, and the entire atmospheric pressure on Mars, was regulated by the dry ice at the poles. For instance, if you sprinkled black dust on the dry ice at the poles, you'd cause them to absorb more sunlight and it would warm up. And because of the warming up, more carbon dioxide would evaporate and you'd have a thicker and thicker atmosphere.

LIPPINCOTT: Isn't that what Carl Sagan was saying we ought to do?

INGERSOLL: Well, he said it *after* the Leighton and Murray paper. The Leighton-Murray paper showed how it all worked, and it was a great paper. I taught it in my classes; it was the core of the explanation for the climate of Mars. But in the early seventies, I began to examine that paper and compare detailed predictions of that paper with the observations of Mars, and I concluded that it was wrong. [Laughter]

The first thing was that if you watch the polar caps shrinking in the springtime—either polar cap—they shrink pretty fast. And if you plot the radius of the polar cap against time, it's shrinking so fast that you extrapolate it and that thing is going to shrink to zero! The shrinking part that you see in the springtime is clearly carbon dioxide, because water is just not that mobile, or volatile. So if you extrapolate its rate of shrinkage against the season—against time—and extend it a little bit, you have to conclude that it would vanish by the first day of summer.

LIPPINCOTT: But it never does.

INGERSOLL: But it never does. The white stuff stops shrinking. It's a very abrupt transition: It shrinks down to a little core and then it just stops shrinking and sits there for the rest of the summer. And that's just the way water ice would behave if it was covered during winter and spring with dry ice. The dry ice would shrink away and expose this little hard nugget of involatile water ice, and the water ice would just happily sit there for the rest of the summer.

LIPPINCOTT: Did you find that on both poles? Or more on one?

INGERSOLL: On both of them. So I wrote a paper ["Mars—the Case Against Permanent CO_2 Frost Caps," *Jour. Geophys. Res.*, 79:3403-3410 (1974)]. And this was before *Viking*. I'm getting ahead of the story, and there are a lot of other things I did in the early seventies that I'll have to get back to. But partly it was about the rate of retreat of the—well, we didn't know what it was, but of the white stuff. The rate of retreat made it look as though it was two different substances—the carbon dioxide retreating fast in the spring and extrapolating to zero by the first day of summer, and then some other white stuff that just happily sat there throughout the summer. So the obvious conclusion was that the first was CO_2 and the second was water.

Moving ahead, then *Viking* came along and measured the temperatures.

LIPPINCOTT: Was this the *Viking* lander you're talking about?

INGERSOLL: No, it was really the orbiter that was crucial to this discussion.

LIPPINCOTT: There was an orbiter plus a lander in each *Viking* mission?

INGERSOLL: There certainly was—two *Vikings*, two orbiters, two landers. The orbiter had two instruments that were crucial to this argument. One was the infrared detector, which Hugh Kieffer was the principal investigator on. Now we're at 1976, and people have matured and they've gone on to great things. And Hugh had a wonderful instrument.

LIPPINCOTT: Were you on that team?

INGERSOLL: I was not on that team. But they measured the temperature. I'll get back to this, but I was involved on the *Pioneers* to Jupiter and Saturn, and that was also the early seventies. And

I was also involved in a whole side issue about the shape of the sun—whether the sun was oblate and whether Einstein's general theory of relativity was right. So we've got to get to that.

But *Viking* then came on. The north polar cap on Mars remained cold throughout the springtime, just the way dry ice would. Dry ice has to remain cold because it's in equilibrium with a seven-millibar atmosphere; and you can just look up in your handbook of physical tables, that if you have seven millibars of CO_2 and you want it to be in equilibrium with dry ice, the dry ice *has* to be at 148° Kelvin or something like that.

LIPPINCOTT: What's that in Fahrenheit? How cold is that?

INGERSOLL: I don't know—it's much colder than anything on Earth—it is -193° F.

LIPPINCOTT: And yet the dry ice is...whatever you call it...evanescing?

INGERSOLL: Subliming.

LIPPINCOTT: So when that happens, it's not as cold as it was in the winter?

INGERSOLL: Well, here's what happens. In the springtime, the polar cap is 148° K, which is exactly the right temperature for dry ice in equilibrium with seven millibars—which is the Mars atmospheric pressure—of carbon dioxide gas. The atmosphere is almost all CO_2. But then, the first day of summer, the temperature suddenly just shoots up, to over 200° K, and dry ice could not do that. The only thing that could shoot up and still remain frostlike is water. So that's entirely consistent with the idea that the springtime cap is covered with dry ice, and then the dry ice goes away and exposes some other substance, which is much less volatile. At 200° K, water is almost like granite—it's not a very reactive substance. And then suddenly that stuff warms up, to be in equilibrium with the sun, and that's it, we're over 200° K.

So that showed that that aspect of Leighton and Murray's theory didn't apply to the north polar cap; there was no big reservoir of dry ice there; there was only this seasonal skin of one-meter thickness, which went away in the summertime, and the bulk of the north polar cap was water ice.

LIPPINCOTT: So the orbiter wasn't looking at the south polar cap? It was up in the northern hemisphere.

INGERSOLL: No, no, no. I'll tell you the story about the south polar cap. But there was another instrument that showed that, simultaneously with this sudden warming, a lot of water vapor appeared in the atmosphere, which is just what you'd expect. Because the water was suddenly exposed and suddenly warmed up to a relatively balmy 200° K—273° K is the freezing point.

LIPPINCOTT: There was some evaporation, even though it was 200°K?

INGERSOLL: Yes, it's not *quite* granite at 200° K. But in the south, there was much less water in the atmosphere in the summer, and it remained much colder. There's maybe some question about whether it was really 148° K or maybe a little warmer, but it really looked as if the south polar cap obeyed the Leighton-Murray hypothesis, in that the bulk of the carbon dioxide on Mars was in dry ice in the south polar cap. And that was what everyone concluded after *Viking*.

LIPPINCOTT: Do you know why there is more water in the northern hemisphere than in the southern hemisphere?

INGERSOLL: It was a big mystery—why should one cap have all the CO_2? Well, I didn't really let that one go, and it still kept nagging at me. But then it became the conventional wisdom that the south polar cap was CO_2 and the north polar cap was water. And that remained the case until the mid-nineties. Then I was at a conference with some terrestrial glaciologists and some Martian glaciologists—or whatever we were; we were experts on the polar caps. And the terrestrial glaciologists had done some experiments on the strength of dry ice at 148° K, and they found that it was really not very strong. It would flow under its own weight, the way a glacier does, but rather fast, actually. And in the meantime *Mars Global Surveyor*, which was a new spacecraft in the mid-nineties, had measured the shape of the south polar cap and found that it was domed, kind of like the Greenland ice sheet—almost as big, too. And given that the cap was domed and the dry ice was not very strong, you'd expect it to flow outward into a much flatter pancake, and rather quickly. And that cast doubt on the conventional wisdom since *Viking* that the south polar cap was CO_2. Because how could it maintain its domed shape if it was like molasses? It would flow.

LIPPINCOTT: So they thought it *wasn't* all dry ice?

INGERSOLL: Yes, they said it didn't look like it could be three kilometers of dry ice, which was the profile of this dome. The *Mars Global Surveyor* was getting close-up images—high-resolution images—of the south polar cap and it was pockmarked with these strange craters that had very flat floors. They weren't impact craters. They didn't have the rounded bottoms—they had very flat floors. And they were all eight meters deep, and some of them were hundreds of meters in diameter.

LIPPINCOTT: Well, what could have made those?

INGERSOLL: Well, a student—Shane Byrne—and I said, "Let's try and simulate those."

LIPPINCOTT: Were they round?

INGERSOLL: Round. If you look at them from above, they were round. So we said, "Let's see if we can simulate them using just the incident sunlight and infrared radiation." As the cap warms up—or as it absorbs heat—it sublimates into the atmosphere. If it gains heat, it sublimates; if it loses heat, it condenses. So we said, "First of all, let's assume we have a slab of CO_2 infinitely deep. We'll put a little topographic depression and see how that topographic depression behaves." Well, it annealed itself and disappeared.

LIPPINCOTT: It smoothed out again?

INGERSOLL: It smoothed out again. So we said, "Let's make it dirty, so that the inside of the [depression] absorbs more sunlight than the outside. Well, by making it dirty enough, we could make it develop a big bowl shape, but it didn't have the flat floor. So then we said, "Well, let's make it dirty and let's put it on top of a layer of involatile substance." And then we got it—it perfectly fit the appearance of these craters. But the involatile substance couldn't be CO_2, so we concluded it was water ice. And this was consistent with what these people had been saying about the strength.

So then we said, "Well, OK, if it's water ice, then we can calculate when in midsummer it should warm up and we should be able to see it with high-resolution temperature measurements." And indeed, the high-resolution radiometers saw that, yes, just at the right time of the season, in midsummer, the floors of these depressions warmed up, as water ice would do. They didn't behave like carbon dioxide at all.

So now, I'd say, the conventional wisdom is that both polar caps are basically water ice but that the south cap has an eight-meter veneer of carbon dioxide that is pockmarked and broken up by these circular craters. So it really looks as if the Leighton-Murray model—the whole atmosphere being controlled by a big chunk of CO_2—is not right. But it's taken about thirty or forty years to establish that. And it's been a fascinating struggle. Ultimately, it was the new data, from the *Mars Global Surveyor,* that clinched it.

LIPPINCOTT: What does control the atmosphere then? Would it be partly water vapor?

INGERSOLL: Well, I'm taking you up to the present on that particular story. We don't know. Why is there a six- or seven-millibar atmosphere at all, if it's *not* being regulated by dry ice? The conclusion in our paper—Shane's and my paper ["A Sublimation Model for Martian South Polar Ice Features," *Science* 299:1051-1053 (2003)]—was that this eight-meter layer of CO_2 is a small fraction of the CO_2 that's in the atmosphere, and it couldn't possibly act as a buffer for the atmospheric pressure the way Leighton and Murray had said it would. So we're kind of scratching our heads as to what is the buffer that's controlling the atmospheric pressure. We don't know. It could be that there's something going on under the ground—some huge reservoir of CO_2 that's hiding underground. But why doesn't it evaporate out into the atmosphere? That's a problem. Or it could be that Mars was born with very little CO_2 compared with Earth and Venus, and that what you see in the atmosphere, plus this little thin veneer, is all it ever had. We don't know, and that's kind of the fun of planetary science: There are big questions still out there. But we've gone almost full circle from the early assumptions that both polar caps were water, just because that's the way it is on Earth, to the Leighton and Murray model that both polar caps were CO_2; and now it appears that actually both polar caps *are* water but with this thin veneer of CO_2. And I was definitely a contributor to the demise of the CO_2 hypothesis.

LIPPINCOTT: Well, that's good. I want to come back to Mars later and get your thoughts on maybe the future of Mars.

INGERSOLL: Yes. Well, I would like to tell you about *Pioneer*.

LIPPINCOTT: Yes. Maybe we should talk about *Pioneer* now. So you were teaching courses to grad students and you'd get caught up in the planetary missions at JPL, is that right? JPL was running *Pioneer 10* and *11*?

INGERSOLL: It was really Caltech. It was Guido Munch, who was a professor of astronomy, and Gerry Neugebauer, professor of physics. And they were quite generous, I must say. They either, out of a political kindness to me or to my division—or perhaps because they genuinely were getting more interested in astronomy and less interested in planetary science—but for one reason or another, they made me a member of their team, after they had formed a team.

LIPPINCOTT: This team was for infrared radiometry?

INGERSOLL: Infrared radiometry, yes.

LIPPINCOTT: To go on both of those spacecraft?

INGERSOLL: *Pioneer 10* and *11* had identical instruments. The infrared radiometers were going to Jupiter, to measure the temperatures of everything—the satellites, the planet—and *Pioneer 11* went out to Saturn, too, and we measured the rings.

LIPPINCOTT: What would Neugebauer's and Munch's instrument specifically be measuring?

INGERSOLL: It was measuring the blackbody brightness temperature at two different wavelengths. One was about twenty microns, one was forty-five microns—very broad spectral channels.

LIPPINCOTT: Was it to determine the temperature of Jupiter's atmosphere?

INGERSOLL: Its temperature and its composition. But more important, you're measuring a sizable portion of the heat being radiated by Jupiter. Since you know how much sunlight is incident on Jupiter, and you know what fraction of that is reflected back to space, you can take the other fraction, which is absorbed by Jupiter, and you can compare that with how much infrared is being emitted, and take the difference. And it turns out that more infrared is being emitted than is being absorbed. [Tape ends]

Begin Tape 2, Side 2

INGERSOLL: The difference between the two is internal heat. And that was an important number. It had to do with the whole evolution and internal temperatures and composition of Jupiter. Because how much heat Jupiter had retained from its formation depended on all those quantities.

LIPPINCOTT: And this number wasn't known until *Pioneer 10* and *11*, is that right?

INGERSOLL: There were estimates of it, and the estimates were too high. They were based on ground-based infrared observation, and the ground-based astronomers had several problems. One was, they had to look through the Earth's atmosphere, so they couldn't sample at the wavelength that really mattered—you can't look through the Earth's atmosphere at all wavelengths. And also, they hadn't worked out the calibration sources very well. You have to have standard astronomical sources if you're going to do the job right, and they hadn't really worked it out. So they were saying that Jupiter's total infrared radiation was two-and-a-half times what Jupiter was absorbing from the sun. And that was too high, we now know. From *Pioneer*—*Pioneer 10* got to Jupiter in 1973—we learned that the number was between 1.7 and 1.8 total emission divided by the amount absorbed from the sun.

LIPPINCOTT: So you're saying Jupiter was cooler than you had thought?

INGERSOLL: The inside of Jupiter was cooler—it had lost a larger fraction of its heat over the eons than people had estimated. So, Munch and Neugebauer allowed me to play a big role in that project. Glenn Orton did most of the analysis. That was his thesis ["Spatially resolved absolute spectral reflectivity of Jupiter: 3,390-8,400 angstroms. The Jovian thermal structure from *Pioneer 10* infrared radiometer data. Observations and analysis of 8-14 micron thermal emission of Jupiter: a model of thermal structure and cloud properties" (1975)].

LIPPINCOTT: You didn't do any of the designing of the instrument, did you? You just did theory?

INGERSOLL: We analyzed the data. Larry [Laurence M.] Trafton, at the University of Texas at Austin, was also involved; he was a former grad student of Guido Munch's. He had done the models before the encounter with Jupiter. They had two wavelengths, and they had two parameters: One was the internal heat and the other was the hydrogen-to-helium ratio—which was another objective. The idea was that they were going to fit the data at the two infrared wavelengths to these two parameters and come up with, number one, the internal heat, and number two, the hydrogen-helium ratio, another very important number. But actually they had more data than they had parameters in the model, because you can look straight down at a planet and you can look at the edge of the planet, so you have a geometrical dependence called limb-darkening. And the limb-darkening didn't fit the data. Their models really were inadequate. Glenn saved the day by using methods that had been developed for the same kind of analysis—for Earth satellites to measure temperatures in Earth's atmosphere. And number two, he used ground-based observations to show that the stratosphere of Jupiter was quite warm. And so we combined the analysis and these ground-based observations of the stratosphere with the new *Pioneer* infrared-radiometry data. He did a great job, and we came up with an estimate of the hydrogen-helium ratio, and we came up with an estimate of the internal heat. Both estimates turned out to match subsequent measurements by the *Galileo* probe. So we were quite happy with that.

LIPPINCOTT: Close to what you had estimated.

INGERSOLL: Yes. So Guido and Gerry gave me a big start.

LIPPINCOTT: Did you actually go up to JPL for the encounters, as people did with some of the later missions?

INGERSOLL: The *Pioneers* were managed out of Ames Research Center, and we certainly went up to Ames for the encounters.

LIPPINCOTT: So JPL had little to do with those missions?

INGERSOLL: Yes. The infrared radiometer was built by the Santa Barbara Research Center. A guy named Stillman Chase was the instrument designer and system manager of the construction part.

Well, I have to continue to Saturn with this infrared radiometer. *Voyager* came in between, but I'm going to take that infrared radiometer to Saturn.

In the late seventies, Guido Munch, who was the principal investigator, decided to leave Caltech, to become the director of the Max Planck Institute for Astronomy, in Heidelberg. So he said to me, "Here, the infrared radiometer is yours."

LIPPINCOTT: So you became principal investigator?

INGERSOLL: Yes, for the Saturn encounter—the Jupiter encounters had already come and gone. The Saturn encounter was on the horizon.

LIPPINCOTT: Yes. *Pioneer 11* reached Saturn in 1979.

INGERSOLL: Yes, so I had two years to prepare. So we get to Ames Research Center in '79.

LIPPINCOTT: Is that up in Mountain View?

INGERSOLL: Mountain View, California—a NASA facility like JPL, but more so, because JPL at least is administered by Caltech and Ames is one hundred percent NASA. We'd done our job.

And I had my sixteen-year-old son with me, who was doing my programming: Jerry Ingersoll—Jeremiah. And Glenn Orton was there. The encounter had been a great success, and we'd gotten our data on the planet, and everything was good, and most people had gone home to bed. But Jerry and I stayed around for the Titan encounter—Titan is the big moon of Saturn that has its own atmosphere. And the infrared Titan encounter was later the same night. Glenn had gone off to his motel room, whoever else I had had gone home, but Jerry and I and one Ames guy were in this huge room full of TV monitors, and the three of us were waiting for the infrared data to come down from Titan.

LIPPINCOTT: This is the very first encounter with Titan?

INGERSOLL: The very first spacecraft to *Saturn*! This is summer 1979. There had been a secret agreement between the U.S. and the Soviets that they would maintain radio silence throughout the *Pioneer 11* encounter, while the weak signals came back from the spacecraft. And it was secret because we were in the midst of the Cold War and neither side wanted to appear to be cooperating with the evil enemy. But the U.S. side had gotten the hour wrong, and they had forgotten about the infrared-radiometer observations of Titan, which came later than the other observations of Titan. And they had told the Soviets, in this secret agreement, "You may turn on your Earth-orbiting satellites at such-and-such an hour, because we're through by that point." So there we were, in this big room, with TV monitors all sitting there, gently flashing. And all of a sudden, all the TV monitors started flashing big block letters [laughter] all around the room. All the Earth-orbiting satellites had just turned on at the appropriate time; they were not trying to do anything bad. The infrared radiometer was the last instrument to view Titan, and our data were just starting to come in. [Laughter] So we said, "What *is* that?" And the Ames guy said, "That's RFI." And I said, "What is RFI?" And he said, "Radio-frequency interference."

LIPPINCOTT: Could you call anybody up and tell them to stop?

INGERSOLL: Well, he was on the phone with the Deep Space Network all over the Earth—Canberra, Australia—our network that collects the signals with radio dishes.

LIPPINCOTT: And what did they say?

INGERSOLL: Well, they just said, "RFI." But the next morning, the final press conference of the encounter was scheduled, and one of the things they were going to report on was the infrared-radiometer observations of Titan. [Laughter]

LIPPINCOTT: Did you get anything?

INGERSOLL: We got a little bit of data, and then there was this radio-frequency interference. So the chief scientist for the *Pioneer* spacecraft—John Wolfe, I think his name was—he had to get up and say what had happened, but he couldn't mention the secret agreement, so he said, "Well, there was unknown radio-frequency interference and most of the infrared-radiometry data were lost." And that's all he said; he couldn't say what had really happened.

Well, the *National Enquirer* reporter was there, and the *National Enquirer* published a story saying that it was the residents of Titan who had jammed the radio signals, because they didn't want to be discovered. The Titanians did not want our spacecraft to discover their existence, so they jammed the radio signal. They were afraid the infrared radiometer would have detected the heat of their cities. Now, I'm sure the publishers of the *National Enquirer* were sophisticated people who knew they were making this up. But that was their explanation of it.

But I do give Guido great credit for helping my career along and getting me involved in this very exciting mission.

LIPPINCOTT: So, did you find some interesting stuff at Saturn, before that, with your instrument?

INGERSOLL: Yes. As I say, we did measure the hydrogen-helium ratio, a much better estimate than before. And we also measured the internal heat.

LIPPINCOTT: Could you tell from your instrument what the depth of the atmosphere was?

INGERSOLL: Both Jupiter and Saturn are effectively infinitely deep. There's just an atmosphere that just goes on down to—

LIPPINCOTT: The core is really teeny?

INGERSOLL: The core is ten percent at most—it's only five or ten percent of the radius of the planet, and the rest is just atmosphere, getting hotter and denser.

LIPPINCOTT: People say they're like failed suns. Would you describe them that way?

INGERSOLL: Well, the word "failed" implies that they at one time had a chance to be suns. It's like saying that a salamander is a failed person. The salamander never had a chance to become a person, and Jupiter and Saturn are too small to have even aspired to be suns, if you define a sun as something that generates its own nuclear energy. They're just too small; their internal temperatures are not hot enough.

What I should talk about now is—I've talked about Mars' polar caps; I've talked about *Pioneer*—I've got to get to *Voyager*. But before then, I should probably talk about the solar oblateness.

LIPPINCOTT: Oh, yes, whether or not the sun is round. Why did people think it wasn't round?

INGERSOLL: Well, in the sixties, a famous Princeton physicist named Robert Dicke published a paper. He and a student had done a series of measurements—Dicke and [H. Mark] Goldenberg ["Solar Oblateness and General Relativity," *Phys. Rev. Lett.*, 18:313-316 (1967)]. They had done a series of measurements in which they had put a circular disc, an occulting disc, in front of the sun and looked at how much light was spilling out around the edges and concluded that the sun's shape was oblate—meaning it bulged at the equator.

LIPPINCOTT: Not very much, though.

INGERSOLL: Not very much, but enough so that it would have had a measurable gravitational effect on the orbits of the planets, particularly Mercury's. And since Mercury's orbit was really the cornerstone of the tests of Einstein's general theory of relativity, having the sun be oblate means that that cornerstone was now pulled out of the building. [Laughter] Because the oblateness would give you a different value for the precession of Mercury's orbit and it would no longer agree with Einstein's prediction.

LIPPINCOTT: Wasn't there one other physical—

INGERSOLL: There were other tests.

LIPPINCOTT: There's Eddington's—

INGERSOLL: The bending of starlight.

LIPPINCOTT: Bending of starlight. That would have remained.

INGERSOLL: Yes, but any disagreement is very important. So Dicke and Goldenberg's observation in their paper in the late sixties was a very big deal.

LIPPINCOTT: And was that because a discrepancy would have cast relativity into question?

INGERSOLL: Oh, yes! And Dicke had a rival theory all ready to go, that fit the data. I remember he gave an astronomy seminar at Caltech, very well received.

LIPPINCOTT: It must have upset the physicists there.

INGERSOLL: Yes. It was an important piece of work.

LIPPINCOTT: You went to that seminar?

INGERSOLL: I went to that seminar, yes. And then in 1970, my old friends and mentors back at Harvard and MIT invited me to Woods Hole—because I still had a foot in the oceanographic meteorology camp. So there I was, in 1970, talking to Ed [Edward A.] Spiegel; and we started trying to think of ways to poke holes in Dicke's interpretation.

We said, "Well, what if the sun were not actually bigger at the equator but just brighter at the equator?" It's a pretty simple idea. Well, Dicke had taken that into account, because he let different amounts of sun spill outside the disc he put in front of the sun. And he had said, "Well, the bulge is not a function of how much light is allowed to spill out over the edge of the occulting disc—not a function of the radius of the occulting disc," and that this would be consistent with an oblateness rather than a brightness at the equator.

So we looked at his data, and actually there *was* some dependence on brightness and on the radius of the occulting disc. The more light you let spill out, the bigger the sun looked—and that wasn't right. Because if it was a brightness effect, and you let more of the sun spill out over the edge of the disc, then the apparent oblateness would go up—and, indeed, it did. So we quantitatively estimated how much it should go up, and it went up by about that amount. So Ed Spiegel and I wrote a paper on the solar oblateness ["Temperature Variation and the Solar Oblateness," *Astrophys. Jour*. 163:375-382 (1971)]. It's not oblate; it's just brighter [at the equator].

LIPPINCOTT: What would make it brighter, if you don't mind my asking?

INGERSOLL: We didn't really get into that. But then a guy named Gary Chapman, a solar astronomer who knows the sun intimately, called me up and said, "I know what's making this so bright." It's faculae, which are little magnetic regions that are bright and are clustered at the equator. They're transparent at normal optical wavelengths.

LIPPINCOTT: So you can't see them, you mean?

INGERSOLL: Yes. You'd see right through them when you look down straight on the center of the disc. But over at the very edge of the sun, where you're looking at them sideways through a long slant path, they would definitely have an effect. So Gary Chapman and I went to work. And Dicke had given us—or maybe he'd published—his daily log of the brightness of the sun, and Chapman had a daily log of faculae, which he'd been measuring using observations at special wavelengths—calcium K-lines and things which would make faculae visible. We compared the daily log of faculae on the limb of the sun with the daily log of the apparent oblateness, and [the data] jiggled up and down together very nicely. So we published a paper on that ["Photospheric Faculae and the Solar Oblateness," *Astrophys. Jour.* 175:819-829 (1972)]. And then Dicke wrote a very big rebuttal of that. And we published a rebuttal of his rebuttal. And we got into some real—

LIPPINCOTT: What journal was this?

INGERSOLL: *Astrophysical Journal*. And then, at about the same time, I was up for tenure.

LIPPINCOTT: I have it here, when you got it—associate professor, 1971. You became a full professor in 1976.

INGERSOLL: Yes, so probably it was for the promotion to full professor that they asked Dicke for a letter of recommendation. [Laughter]

LIPPINCOTT: Why was he so adamant? Was it an emotional thing? He wanted to rattle Einstein's cage? Or was he just that kind of person?

INGERSOLL: Well, I don't think he was doing anything wrong. He was not doing anything unethical. He was just passionate about his discovery. And it's true that overthrowing Einstein would have made him.... Well, he was already a famous man.

LIPPINCOTT: Yes, but one for the ages.

INGERSOLL: And then, to have a theory waiting in the wings to step into place! So he was just passionate about what he was doing, and all these rebuttals were just passionately defending his theory. There was never a point where he said, "OK, I concede that we were seeing faculae."

LIPPINCOTT: He never did?

INGERSOLL: No, but I think the issue just died away. [Laughter] Astronomers forgot it, and the conventional wisdom was that it was faculae. I think that's the way evolution in science happens. People go to battle passionately defending their theories, and maybe one side wins, but there's never a victor declared. It's just that people move on.

LIPPINCOTT: Well, in any case, he would have had to deal with what Eddington found about bending starlight.

INGERSOLL: But you can preserve some aspects of general relativity and throw out other aspects. So it was possible to come up with a theory that would give the right value for the bending of starlight and give the wrong value for Mercury's orbital precession. And you do this by introducing parameters to Einstein's theory. Einstein's theory had no parameters—or one parameter, the cosmological constant, about which you hear a lot today. But when you start inventing gravity theories that have numerous constants, then you can, you know, fit one set of data and selectively fit another set. And Dicke's theory was one of many with adjustable constants in it.

LIPPINCOTT: Did you ever meet with Dicke?

INGERSOLL: Yes, I met him once or twice.

LIPPINCOTT: How were your relations with Dicke?

INGERSOLL: Well, they were guarded and gruff. Peter Goldreich said that Dicke did write a letter when I was up for full professor. I never saw the letter. I never asked about what he said. My guess is that he wrote a very short, guarded letter, saying, "Well, he's an opinionated young man, but he's smart"—probably one of those kinds of letters.

And now I should tell you about *Voyager*.

LIPPINCOTT: Yes, *Voyager*, the imaging science team—you were a member of that team. I remember that Henry S. F. Cooper wrote a bunch of articles about that in *The New Yorker*. I was at *The New Yorker* at that time, and I helped Henry with those. I might even have spoken with you on the phone. [Carl] Sagan was the head of that team, wasn't he?

INGERSOLL: No. He was on the imaging team, but Brad Smith was the head of it.

LIPPINCOTT: Oh, yes, from the University of Arizona.

INGERSOLL: Now, the way I got involved in *Voyager*—I had originally not applied.

LIPPINCOTT: That was in ’78?

INGERSOLL: ’79 was the first encounter with Jupiter—it was March of ’79.

LIPPINCOTT: And by that time, you were on the imaging team.

INGERSOLL: Yes. They actually formed the teams in the mid-seventies, and I had not applied to be on the imaging science team. I thought—incorrectly—that I could just be a theoretician who sat in the wings and didn’t need to go to all these committee meetings in a big enterprise like *Voyager*. I was on the committee that selected the instruments, because since I hadn’t applied to be on any of the science teams, I was then free to be on a selection committee at NASA headquarters. The key choice was between two infrared spectrometers—the one that was selected, which was a Goddard instrument, and the one that wasn’t, which was a JPL instrument. I voted for the Goddard instrument. People knew who was on the committee, and afterward I got a phone call from someone at JPL asking, “Why did you do that?” Well, I did it because I thought it was a better instrument. The Goddard instrument had better ways of calibrating itself. It had higher spectral resolution. The JPL instrument was kind of broadband and not really such a discovery instrument, in my opinion. But it was a tough decision.

Well, Ed [Edward C.] Stone [Morrisroe Professor of Physics] and I had taught freshman physics together—we had alternated lectures in freshman physics for a few years. Ed Stone was the chief scientist on *Voyager*, and to his great credit he regarded *Voyager* as a national resource; he saw the great potential. And he decided to bring in as many experts on the outer planets as he could, whether or not they were initially part of a team. He was going to bring in people as experts, and he was going to convene international meetings of all the outer-planet scientists, to really brainstorm what *Voyager* should do. So I got involved in those international meetings.

LIPPINCOTT: When you say “international,” what other countries were—

INGERSOLL: People from all over the world. The French, especially, had a number of experts.

LIPPINCOTT: And they would be team members?

INGERSOLL: Well, I don't think he was allowed to do that. He could convene a meeting and brainstorm. Whether he could give these people official status or not, I don't know. I headed a subcommittee or something, and somehow someone was able to change the rules. So I actually became an official interdisciplinary scientist—or something-or-other official—and I had official status on *Voyager*. Brad Smith was also very generous. I was willing to work and he was willing to have me, so I became a member of the imaging team, and I think I had official status. I don't know how they handled that.

LIPPINCOTT: The imaging team was going to take pictures of these planets. There was a camera, right—which wasn't exactly the kind of instrument you'd been working with up until now.

INGERSOLL: No. I'd been working on the *Pioneer* infrared radiometer. But the camera was of great interest to me, because it was going to take sequences of images, temporal sequences. We were going to watch the clouds move, so we could measure the winds close up. You can measure the winds from a great distance from Earth, but now we would have much finer scales. The Red Spot itself is thirty thousand kilometers on its long axis, and we were going to be measuring things a hundred kilometers, moving around and within and in and out of the Red Spot. So it was going to be exciting.

LIPPINCOTT: Would you be looking at the moons also?

INGERSOLL: Certainly the camera looked at the moons, but I was not so much involved with that.

LIPPINCOTT: You were just involved in the atmosphere?

INGERSOLL: Yes, and it was amazing, because when we got up close, we saw incredibly energetic turbulence. And all that turbulence made the stability and longevity of the Red Spot even more mysterious, because here it's churning, boiling, bubbling, fluid, and yet the Red Spot, as a whole structure, just endured. So that was even more puzzling, and very interesting.

LIPPINCOTT: Do you know yet why it endures?

INGERSOLL: Well, we published a paper [R. Beebe, A. P. Ingersoll, G. E. Hunt, J. L. Mitchell, and J. P. Muller, "Measurements of Wind Vectors, Eddy Momentum Transports, and Energy Conversions in Jupiter's Atmosphere from *Voyager 1* Images," *Geophys. Res. Lett.* 7:1-4 (1980)]. Reta Beebe was a member of the imaging team.

LIPPINCOTT: Where was she from?

INGERSOLL: New Mexico State University.

LIPPINCOTT: And she was also interested in the atmosphere.

INGERSOLL: Yes. I'd come into it from the theoretical fluid-dynamics side, and she'd come into it from ground-based astronomy, just watching the planet. We wrote a couple of papers with other members of the team, detailing from these observations the energy transfer between the turbulent eddies and the large structures, like the jet streams and the Great Red Spot. And the surprising conclusion was that the turbulence was actually pumping up these large structures, rather than taking energy away. The small-scale eddies were pumping up the jet streams, for instance, and the small-scale eddies were constantly being absorbed by the Red Spot; it was, in effect, eating them.

LIPPINCOTT: I should have looked at Henry's articles, but he talked about bands of winds—one would be going one way and the other would be going the other way, and that interface was where eddies were and where these big storms were. Is that right?

INGERSOLL: Yes. And not all big—some of them were small. And the small eddies along those interfaces were carrying momentum into the jet streams on either side. Whereas your normal intuition would say that they extracted momentum, in fact they were doing the opposite. So if there was a westward jet, they were carrying westward momentum into the westward jet, and if there was an eastward jet, they'd carry eastward momentum into the eastward jet.

LIPPINCOTT: So that's kind of an explanation for this perpetual-motion machine?

INGERSOLL: Well, I wouldn't call it a perpetual-motion machine, because the eddies themselves are getting energy from somewhere, but it is a kind of order out of chaos, if you think of the small eddies as chaos. They were creating order, in the large flow features, in the large jet streams.

LIPPINCOTT: What does drive all those winds? Is it the internal heat of Jupiter?

INGERSOLL: Well, we're still debating that. Here's another analogy: It's like a food chain. The small eddies are the plankton and the algae, and then there's the little fish that eat the plankton and the algae. And finally, the big fish—like the Great Red Spot—eats the little fish. So it's like a food chain in the ocean. And then you have to ask, "Well, where do the smallest structures get their energy?" And that brings us a whole jump ahead, to *Galileo*, where we finally got around to looking on the night side of Jupiter and we could see lightning flashes on the night side. And we could correlate the lightning flashes with the most energetic of these small eddies. So they seemed to be thunderstorms, which were getting their energy—

LIPPINCOTT: So this is electrical energy.

INGERSOLL: Yes, but it really shows that there's water vapor and falling rain and snow and all that stuff. And it's likely that those thunderstorms are getting their energy from the internal heat of the planet, and somehow that energy is being transformed into kinetic energy, and the kinetic energy is going up the food chain to the large beasts on Jupiter. [Laughter]

LIPPINCOTT: Well, tell me a little bit about the team itself. Did you meet often at JPL? Or did you just work at your separate institutions?

INGERSOLL: Well, we met often at JPL, because this was the early days. Nowadays, a spacecraft team is distributed all over the world, and you send data back and forth on the Internet, and no one has to leave their offices. But in those days, you really had to get together at JPL and use the facilities, the image-processing laboratory. It was all centralized, and people would move to Pasadena for the encounter.

LIPPINCOTT: How long did those encounters last? A matter of hours, wasn't it?

INGERSOLL: Oh, no, much longer, especially for the imaging. You had fairly powerful telescopes onboard the spacecraft, so you could see details three months before the encounter, and it just got better and better for those three months. So I moved my office up to JPL for three months, for each one of the encounters—the two Jupiter encounters and the two Saturn encounters, and a Uranus encounter. And for each of those three-month periods, I—

LIPPINCOTT: Well, was it three months approaching and three months receding?

INGERSOLL: No, on the receding part you're looking back at the dark side of the planet.

LIPPINCOTT: So you couldn't get pictures?

INGERSOLL: That's right. The satellite encounters were more concentrated, because you really had to get very close.

LIPPINCOTT: So when you went to Saturn, you got another shot at looking at Titan, is that right?

INGERSOLL: Yes. Titan, in the visible, turned out to be pretty boring, because it was just shrouded in clouds.

LIPPINCOTT: It's methane or something, isn't it?

INGERSOLL: Yes, there's some kind of hydrocarbon haze—probably not just methane. The clouds are maybe higher hydrocarbons.

LIPPINCOTT: So you weren't particularly interested in Titan?

INGERSOLL: Well, I was, but there wasn't much to see. We couldn't see through the clouds.

LIPPINCOTT: What were you looking at on Saturn?

INGERSOLL: The same thing—the winds.

LIPPINCOTT: Does Saturn have spots like Jupiter's?

INGERSOLL: Not anything as big as Jupiter's, but it does definitely have spots. They merge; they eat each other. And they persist. The big surprise with Saturn was how fast the winds were blowing. You'd think that as you move from Jupiter to Saturn, things would get quieter, because there's less sunlight, and the amount of internal heat of the planet scales down proportionately. Saturn's internal heat and sunlight are both about a quarter that of Jupiter.

LIPPINCOTT: But some of the internal heat—just to digress here—is from radioactive materials in the core.

INGERSOLL: Well, a little bit, but almost entirely it is primordial heat left over from everything crashing together [to make the planet]. And these planets are so massive, and so cold at their surfaces, that they can retain some of that primordial heat of formation.

LIPPINCOTT: Just from slamming together, and four billion years later it's still warm on the inside?

INGERSOLL: Right. The Earth retains a tiny amount of its primordial heat, but it's such a small object that it can't retain very much.

LIPPINCOTT: Well, how come we have radioactive stuff; that's what causes some of our internal heat, isn't it?

INGERSOLL: Well.... But our internal heat is 1 part in 10^4 of the amount of sunlight.

LIPPINCOTT: Really? So it's insignificant with regard—

INGERSOLL: Yes, the internal heat coming out of the Earth is 1 part in 10^4 of the amount of sunlight being absorbed, whereas on Jupiter it's more than half the amount of sunlight.

LIPPINCOTT: And that's all from slamming together in the first place.

INGERSOLL: And retention over the years, because a very massive object can do that.

LIPPINCOTT: So it's because it's as big as it is, that it does that.

INGERSOLL: Yes. It's partly just the surface-to-volume ratio: If you increase the size of an object, its volume goes up faster than its surface area, and the volume is what retains the heat, whereas the rate of loss is from the surface. So larger objects have a better ability to retain heat.

LIPPINCOTT: So those two *Voyager* spacecraft went on to Uranus and Neptune, or did they go off?

INGERSOLL: They both went to Saturn and then *Voyager 1* was deliberately targeted to go by Titan—you could go by two objects in a planetary system. [Tape ends]

Begin Tape 3, Side 1

LIPPINCOTT: We were talking about how the spacecraft has a choice of two encounters.

INGERSOLL: Yes. So *Voyager 1* went zooming by Saturn. And they had calculated the distance to Saturn and the aim point so that *Voyager 1* would also go very close to Titan, but that sent it off on a trajectory that was not going to encounter any other planet.

LIPPINCOTT: But they were really interested in Titan. There were other instruments, besides just the cameras, that could have gotten better readings from Titan.

INGERSOLL: Right—the infrared radiometer, ultraviolet spectrometer, photopolarimeter. So, once they were successful with the Titan encounter, they targeted *Voyager 2* to go by—they chose the aim point at Saturn so that it would head on toward Uranus. So *Voyager 2* went on to Uranus, and then they chose the aim point at Uranus to head on to Neptune. This was possible because of a celestial alignment that happens every hundred and eighty years, or something.

LIPPINCOTT: Yes, lucky for NASA. I mean, they knew about it, but it was nice.

INGERSOLL: It was called the Grand Tour. And the *Voyager*s were designed to last, I think, through the Saturn encounters, but the engineers knew it would probably last to Neptune. But they were not allowed to build through Neptune, because that would have cost too much money. I don’t really know how NASA charges the cost of building a spacecraft, but there’s some kind of level of reliability that, if you say you’re going to Neptune, you have to build to that certain level of reliability, and that was going to cost more than they had. So they said, “OK, we’re just going to build to the level of reliability that will get us to Saturn, and then anything else that happens, well, we’ll just cross our fingers.” That was a lower-cost option, but it worked. It was a risk, and in fact the spacecraft did survive to Neptune. Although there were problems—the scan-platform bearings kind of got old and creaky and they had to use it at slow speeds. And one of the radio receivers conked out.

LIPPINCOTT: So they got fewer data.

INGERSOLL: No, they just had to get smart on the ground. They had to send the signal at the frequency that the receiver was expecting. And then the distances were so great that they had to compress the data. And that meant packing the bits in in a more efficient manner. And they succeeded in all of those things.

LIPPINCOTT: You were still involved with the imaging team?

INGERSOLL: Very much involved!

LIPPINCOTT: Through Uranus and Neptune?

INGERSOLL: Yes. And each one was better than the one before, because our prejudices and expectations were constantly being overthrown. That is the best kind of science imaginable—when, based on your prior knowledge, you have some expectations, there are hypotheses out there and you’re testing them—and many of the hypotheses don’t survive, but you learn something in the process. So it was real discovery. For instance, without thinking very deeply, most people had expected the moons of the outer solar system to be more dead and inert than our moon—ice and rock, with scarred surfaces bearing the signature of impacts 4.5 billion years ago, and nothing else. But in fact almost all of the moons had much more interesting surfaces, and Io had active volcanoes. All these moons had various evidence of reworking on their surfaces.

LIPPINCOTT: What about atmospheres? Were Uranus and Neptune interesting to you?

INGERSOLL: Yes! For one thing, Uranus is tipped on its side. It spins not like most of the other planets do, with the sun going around the equator. When the spacecraft got there, the sun was almost directly overhead at the South Pole; and you would expect that that would have an enormous effect on the atmospheric circulation. But it didn't.

LIPPINCOTT: Well, it wasn't the pole, if the sun was on it, was it?

INGERSOLL: Well, the planet spins on its side—actually, it's within eight degrees of spinning in the same plane as its orbit.

LIPPINCOTT: Its axis is parallel to the ecliptic, right?

INGERSOLL: Yes. The axis is eight degrees away from being in the plane of its orbit. Whereas with Earth, the axis is within twenty-three degrees of being perpendicular to the orbit plane, Uranus is ninety-eight degrees from being perpendicular, so it's only eight degrees from horizontal in its orbit plane.

LIPPINCOTT: Why did it get that way, do you know? Was it hit by something?

INGERSOLL: Well, probably it was hit during the last.... The way we understand the formation of the solar system is that small objects accrete to large objects, and then there's a rather smallish number of large objects still flying around. And the final collisions, before you settle into what we now have, were very violent—they were the most violent, involving two very large objects coming together. So there's a large element of chance in that last big collision, and it can change the axis of rotation and leave some planets, like Uranus, spinning on their sides.

LIPPINCOTT: So that's the thought now, the explanation of why it has a funny—

INGERSOLL: "Obliquity" is the word—the tilting. But from the point of atmospheric dynamics, with the sun almost directly overhead at one of the poles, you'd expect, I suppose, that that would have a big effect on the circulation patterns. But in fact it didn't. Uranus has cloud bands just like Jupiter and Saturn—although they're muted, the contrast is rather low, they're still there and they're bands, and they follow the rotation. They follow the constant latitude line, so that Uranus looks just like—in terms of the orientation of the bands—just like a tipped-over version of Jupiter or Saturn.

LIPPINCOTT: It looks like Jupiter on its side.

INGERSOLL: It looks like Jupiter on its side, although more muted, but the patterns are the same. That tells us that it's the rotation of the planet that's controlling the orientation of the bands. That was the really interesting result for me at Uranus. That was 1986.

LIPPINCOTT: Was this the first time people knew about this?

INGERSOLL: They knew that Uranus was spinning on its side, but they didn't know what the atmospheric patterns were like. And Neptune was remarkable, from my point of view, because here's this object receiving five percent as much sunlight as Jupiter—and hundreds of times less than Earth—on a per-area basis. If you picked out a square meter, held it up to the sun, it would get hundreds of times less sunlight than Earth and twenty times less than Jupiter. And yet the winds are stronger than on Jupiter—three times stronger than Jupiter's winds! And it's got weather; it's got large dark spots and several other spots. That was a mind-blowing thing—with less energy, you have stronger winds. Just the fact is fascinating. But why should it be? Well, we don't know.

I have a theory that I keep putting into the final pages of every paper I write—you know, when you get to the speculative part. Having done your quantitative analysis, then you get a chance to do a little speculation, and I always say: Why are the winds stronger as you move outward in the solar system? Perhaps it's because what you're really doing when you move out in the solar system is reducing the amount of turbulence in the atmosphere. The turbulence is connected to the amount of sunlight. The sunlight is what generates the thunderstorms and the tornadoes and the small-scale stuff. And the small-scale stuff, on the one hand, may be the source of energy, but it may also be the dissipation mechanism that reduces the energy. And you're turning down both effects. You're turning down the source maybe a little bit, but you're turning down the dissipation even more, as you move outward.

LIPPINCOTT: You mean the energy dissipates faster the farther away you go?

INGERSOLL: No, no, no. It dissipates slower. There's less turbulence in the atmosphere. Neptune is just a very smooth atmosphere with no turbulence at all. It's very fast-moving, but with no turbulence to remove that energy, it just coasts along. It's just coasting without any dissipation. So that's the theory—that Jupiter's atmosphere is more turbulent and therefore the dissipation of kinetic energy is greater, and therefore, the winds are smaller. Neptune's atmosphere is less turbulent. There's almost no dissipation—it's almost a frictionless place—and so the winds are larger. Kind of contradictory, but it's a fact—the winds are larger! There's no getting around that!

So that was the fascinating thing for me on Neptune.

LIPPINCOTT: Are you still involved in thinking about those topics?

INGERSOLL: Yes. I have a student today working on models to try and understand—well, we're actually trying to see how moist convection with falling raindrops and lightning could generate these small-scale sources of kinetic energy that drive the large-scale structures.

LIPPINCOTT: Did you know the chemical composition of Uranus and Neptune before *Voyager*?

INGERSOLL: In broad-brush terms, but there's still debate about the abundance of ammonia.

LIPPINCOTT: What about water vapor? Is there any?

INGERSOLL: And water is still a debate. The trouble is the cloud tops are so cold that there's no water vapor in the atmosphere at the cloud tops. You have to see down deeper to see the water.

And, of course, that means you have to have a probe, or some wavelengths that will let you see deeper. We're still debating how much water there is on Jupiter, the other giant planet.

LIPPINCOTT: Do you want to talk about the Shoemaker-Levy comet going into Jupiter? Because, I'll tell you, I have a memory about when they found that comet, and you gave a talk at Caltech that I went to. And I remember it was a big laugh, because I think you had Jim Westphal's camera, maybe, that was coincidentally pointing to the surface of Jupiter when you expected the arrival of the comet, or you could swivel it to actually image the arrival.

INGERSOLL: It was the Hubble. It was not coincidental at all! It was carefully planned. It was the Hubble Space Telescope.

LIPPINCOTT: Yes, but you said, in this talk, that we would have one pixel—and that's when everybody laughed. [Laughter] That at the site of the impact, we would have one pixel right at that place—and one pixel doesn't tell you much, does it. Do you remember that?

INGERSOLL: Well, let's see—there were the Hubble Telescope images, and I was on the team. Heidi Hammel was the principal investigator who designed the Hubble observations, and we had much more than one pixel for that.

LIPPINCOTT: I don't know why I remember that.

INGERSOLL: No, no, you remembered it right. The *Galileo* spacecraft was out there, approaching Jupiter at the time. It was quite a ways from Jupiter, but it had about one pixel for the impact site. But it actually did get some useful data, because that pixel was very bright.

LIPPINCOTT: So it was a heck of a slam into Jupiter.

INGERSOLL: Oh, yes, yes. There was some data about how much energy was involved—maybe equivalent to a million megatons of TNT, I think that was it. It was pretty energetic.

LIPPINCOTT: Yes. If that hit us it would be goodbye forever, wouldn't it?

INGERSOLL: Yes. But on the other hand, Jupiter *attracts* comets, because its gravity is so huge.

LIPPINCOTT: And it protects us, in a way—doesn't it?

INGERSOLL: It does protect us. But it means that Jupiter gets slammed a lot more than Earth does—just orders and orders of magnitude more often than Earth. And the impacts are greater, because things get accelerated by Jupiter's gravity as they get sucked in, so it's worse. And it's also much more frequent, because of the attraction effect.

LIPPINCOTT: What else should we cover? You were on the Vega Venus balloon science team in the eighties. What were they trying to find out from Vega?

INGERSOLL: Well, the circulation of the Venus atmosphere. It was kind of an incidental thing. The Russians had a whole line of spacecraft they successfully sent to Venus—the *Venera* spacecraft. And they kluged together a *Venera* with a package that they could drop off with the balloon in Venus's atmosphere and have a probe that went to the surface. And then a part of the mother spacecraft went on to Halley's Comet. It was all a very complicated mission, and all successful, too. The balloons were maybe not an afterthought, but they were a small part of this much larger Halley's Comet/Venus mission.

LIPPINCOTT: This was a Russian mission?

INGERSOLL: Soviet spacecraft. But the balloon payload was actually built and designed by the French. There were two spacecraft and two balloons—"the French" being Jacques Blamont. And somehow, both the Soviet side and the French side had realized that to really track the balloons in the Venusian atmosphere you needed radio observatories on Earth. So they got Bob Preston at JPL to head up a team of people who were going to array together the radio observatories on Earth, so that you would get a slightly different angle—on one side of Earth, you're receiving the signals from a slightly different angle than on the other side of Earth. And since these are very precise radio signals, you can measure the subtle differences in Doppler shift and pinpoint the motion and the position of the balloons in the Venusian atmosphere. Preston did a marvelous job pulling that together. And I was part of the U.S. team. There were instruments on the balloons that measured temperature and pressure, as well as sending out these Doppler signals. And from the temperatures and pressures, we realized that the balloon was bobbing up and down, in updrafts and downdrafts. So I got involved in the theory of balloon motions—something I knew nothing about.

LIPPINCOTT: Were you alone in this, or did you have students working with you?

INGERSOLL: Colleagues—Lee Elson at JPL and Rich Young at Ames. But I remember just a big rush. In fact, we were in Toulouse when we saw these huge oscillations in the balloon's pressure. Dave Crisp was my postdoc at that time—David Crisp—and we saw these oscillations. I remember going to the library in Toulouse and getting out all the articles on the theory of balloon motion. It's very complicated, because the balloon is floating, and if it's floating at its equilibrium altitude, it's just like another piece of the air. But when it gets pushed down or pushed up, then the balloon's skin expands or contracts, and it has its own behavior, and it starts to oscillate. And the frequency of oscillation is a complicated function of the stretchiness of the balloon material and the drag force of the balloon. But we worked out the data and had some fascinating glimpses of the Soviet Union just before *glasnost*. It was a strange place!

LIPPINCOTT: Was this before [Mikhail] Gorbachev?

INGERSOLL: It was before and during Gorbachev. The first visits were before.

LIPPINCOTT: Where were you working in Russia?

INGERSOLL: Well, in Moscow—IKI, the Institute for Space Research.

LIPPINCOTT: Was that where [Roald Z.] Sagdeev was?

INGERSOLL: Sagdeev was the director of IKI.

LIPPINCOTT: Did you meet him?

INGERSOLL: I met him, yes.

LIPPINCOTT: I think he wound up in the United States, eventually.

INGERSOLL: He married Susan Eisenhower and now lives in Maryland and teaches at the University of Maryland. But the main Soviet guy, who was really doing most of the work, was Slava Linkin.

LIPPINCOTT: What a funny name for a Russian.

INGERSOLL: Viktor Kerzhanovich was on the team.

LIPPINCOTT: Did you spend a lot of time in Moscow with the *Venera* scientists?

INGERSOLL: Yes, and that was fascinating. They took us on a bus tour, out to the countryside to see the area around Moscow—old monasteries and things. We stopped in a restaurant, and we were unannounced; someone had screwed up. So here we were, twenty people, showing up at a restaurant, and the last thing the owner of the restaurant wanted was customers, because he was paid by the state to run this restaurant. He didn't make any money from his customers; he made money from the state, which paid him, and if he didn't have customers then he could sell the food on the black market. So he said, "No, no, no! Go away!" [Laughter] So we maybe went to a different restaurant—or maybe this was a different day, on another tour; I don't remember how it happened—but we were at a different restaurant. And you know, the tradition is when you're entertaining guests, you toast one another with vodka. So we toasted one another with vodka, and everyone was having a wonderful time. But I was beginning to get a little sick from the vodka. We got up to leave, and I thought, "Oh god, here comes the big bus ride and I don't want any more vodka and I don't ever want to see this bus again!" And sitting at a table in this restaurant was a man and his wife—maybe sixty or sixty-five years old. He had two opened bottles of vodka on the table, and he said, "You're Americans, aren't you?" And we said yes. And he said, "You have to drink with me. I fought in the war. We were allies then." And he started weeping—he was so emotional about his war experience and how we had fought together on the same side. He was so emotional that we had to drink his vodka. I mean, it was a wonderful moment, but it was a terrible moment too. So I drank the vodka, but I tried not to drink too much. [Laughter]

LIPPINCOTT: You suffered for your country.

INGERSOLL: Yes, but it was a nice moment. [Laughter] That was a lot of fun, though, analyzing the balloon motions in the atmosphere.

LIPPINCOTT: This was all in aid of Soviet science, though, in a way, wasn't it?

INGERSOLL: Well, we published a number of papers in *Science* magazine on that.

LIPPINCOTT: About the atmosphere of Venus?

INGERSOLL: Yes. I think Sagdeev put himself as first author on most of those papers. [Laughter] But that was OK—we were all part of the team. I think, actually, Linkin was the first author on the paper I wrote on the analysis of balloon motions. [Linkin, V.M., V.V. Kerzhanovich, A.N. Lipatov, K.M. Pichkadze, A.A. Shurupov, A.V. Terterashvili, A.P. Ingersoll, D. Crisp, A.W. Grossman, R.E. Young, A. Seiff, B. Ragent, J.E. Blamont, L.S. Elson, and R .A. Preston, "VEGA Balloon Dynamics and Vertical Winds in the Venus Middle Cloud Region," *Science* 231:1417-1419 (1986)]

LIPPINCOTT: Well, by this time, were you pretty much firmly into planetary missions and not so much into being a garden-variety division faculty member at Caltech and teaching at Caltech?

INGERSOLL: I always taught courses on atmospheric dynamics and oceanography—every year.

LIPPINCOTT: Did you ever take sabbaticals?

INGERSOLL: Just summers at Woods Hole—to study geophysical fluid dynamics. They have an annual course, and I spent half-a-dozen summers there. And during *Voyager* encounters I would spend three months mostly at JPL. But I always taught, and I always had students, and I always enjoyed the Caltech part of it. Even recently I've written a paper on oceanography, and I'm trying to get into—with some of the young faculty we now have—trying to collaborate with them on Earth environmental and climate questions.

LIPPINCOTT: Yes, because the planetary voyages—do you think they're falling off?

INGERSOLL: No, no. They're not falling off at all. The Mars programs are exciting, and there'll be more Mars missions. Now they have evidence of ancient lakes, or oceans, or something.

LIPPINCOTT: Yes, I want to talk about Mars. But maybe we should talk about *Galileo* a little bit. That was in the late eighties, early nineties.

INGERSOLL: Yes, *Galileo*. Mike [Michael J. S.] Belton was the head of the imaging team. He's from Kitt Peak National Observatory; he's retired now. I played the same role as I had with *Voyager*—sort of an interdisciplinary job coordinating all the instruments representing atmospheric science, and then I was a member of the imaging team for my own science. Belton was the PI—the principal investigator—of the imaging team.

LIPPINCOTT: Is Brad Smith not active anymore?

INGERSOLL: Brad retired after *Voyager*; he'd done a great job and deserved to rest. And Ed Stone, who'd done a great job, became the head of JPL after *Voyager*. He did a good job there, too.

For *Galileo*, I had planned to make movies of the atmosphere of Jupiter in motion. And with a much better camera than *Voyager*'s, we were going to make movies at different wavelengths that would enable us to get three-dimensional structure. Rather than just seeing the winds at one level, as with *Voyager*, we would see the winds at many different altitudes in the atmosphere. And whenever the spacecraft wasn't looking at the moons or something else, we were going to constantly take data, and send the data back to Earth at the maximum rate. But then the main antenna of the spacecraft failed to open.

LIPPINCOTT: Oh, I remember that! That was a terrible—

INGERSOLL: And the experiment that was the hardest hit was mine, because I was going to be using the downlink, the transmission down to Earth, more than anyone else during all of this dead time, when they weren't looking at the moons or anywhere else. We were going to be taking all this data and making them into movies, and we couldn't do that.

LIPPINCOTT: And this failure occurred when the spacecraft was still rather far away from Jupiter, didn't it?

INGERSOLL: Yes, on its way there.

LIPPINCOTT: And they had one other antenna that they could use, but it was slower.

INGERSOLL: It was a small antenna—just a little tiny thing—and, yes, its capability was 1 part of 10^4 that of the main antenna.

LIPPINCOTT: You must have been crushed!

INGERSOLL: Well, yes, but you know, we scrambled around. First of all, you can take an image that is 800 x 800 pixel format and you can shrink it down to 400 x 400 pixel format. Since that's a factor of two in both directions, that's four times less data. Then you can do this thing called data compression, where you can reduce the number of bits you transmit by about a factor of ten, because the scenes are pretty bland and the contrast isn't very great, so you don't need to transmit the full information from everything; you just transmit the difference between it and its neighbors, and that's not a very large number.

LIPPINCOTT: So you don't lose much precision that way.

INGERSOLL: You don't lose very much precision. So you can reduce the rate by a factor of forty, right there. Then if you take a hundred times fewer pictures, you're up in the four thousand range and you pretty much are there. That was about the hit we had to take.

So we took a hundred times fewer images, and we shrank them down by a factor of forty, both with data compression and with this reduced format. So we were very selective about where we took our pictures. And instead of being continuous movies, these were often three-step movies. We'd take a picture at one time, and take it at another time, and take it at a third time; and instead of ten wavelengths, we took it at three wavelengths.

LIPPINCOTT: That main antenna never was jiggled free, was it?

INGERSOLL: They tried, but they couldn't do it. We actually got magnificent data. Some of the best data were night side, where we measured the lightning and the auroras. This was the first time that people had figured out which storms had the lightning, and what the structure of the lightning was, and how powerful the lightning flashes were, and all that stuff. Then we arrived at this whole idea of the food chain: The small thunderstorms being powered by falling raindrops, just like a thunderstorm on Earth, and how it ultimately was powered by the internal heat, and how these storms were merging to make bigger storms and the bigger storms got eaten by the Great Red Spot and were absorbed into the zonal jets. That whole idea—with the thunderstorms at the base of the food chain—that all came out of *Galileo*. So it was quite a successful mission. And then, of course, the geologists on the team got magnificent pictures of Europa, and the other satellites—which has led to a great interest in Europa as a possible satellite with an ocean underneath ten kilometers of ice sheet. You can tell by the way that the ice is broken up that it's probably floating on something, and that's most likely water.

LIPPINCOTT: Some think there might be evidence of life, if there's an ocean?

INGERSOLL: Well, you know, if you have an ocean you have warm temperatures—it's got to be above the freezing point. And life lives in those temperature ranges and needs an energy source. Underneath ten kilometers of ice there's probably not a lot of sunlight, but there might be chemical energy sources.

LIPPINCOTT: We have some of that in our own oceans, don't we? Sulfur jets or something like that.

INGERSOLL: Absolutely! Certainly Europa is worth looking at.

LIPPINCOTT: Are there plans to send a probe into Europa's oceans?

INGERSOLL: Yes. I'm on this planning team for a Jupiter icy-moons orbiter—a huge pie-in-the-sky spacecraft that may be launched in the next two decades. And the science planning team, which is still meeting—

LIPPINCOTT: Who's on that team with you?

INGERSOLL: Torrance Johnson—another veteran of *Voyager* and a former Caltech grad student.

LIPPINCOTT: He's a geologist?

INGERSOLL: Yes, he's a planetary geologist. Ron Greeley and Torrance are sort of running it, the science definition team. And I'm on that team. We're asking for a Europa lander. Now, whether the budget will support that, is not clear. That's an extra add-on.

LIPPINCOTT: You're just going to the icy moons, so you wouldn't be going to Io or—

INGERSOLL: That's right. Io, first of all, is a very hostile environment because of all the radiation.

LIPPINCOTT: You're not going to find anything crawling around on Io. [Laughter]

INGERSOLL: That's right. But it's also hostile for spacecraft, so you can't spend a lot of time there because of the electrically charged particles in Io's vicinity. You just fry your electronics.

LIPPINCOTT: Well, that's a good place to stay away from. So—Europa, and there's a few other icy moons you'll visit?

INGERSOLL: Yes. It goes to Callisto, then Ganymede, then Europa; and all the while, it will be in this ideal, circular orbit around Jupiter, so we can finally get our movies.

LIPPINCOTT: So you won't actually orbit the individual moons?

INGERSOLL: Yes.

LIPPINCOTT: Oh, you will?

INGERSOLL: Now, this is twenty years from now. We'll see if it actually happens.

LIPPINCOTT: You'll definitely need a lander to go to Europa. The whole idea is to check out the ocean, right? The subterranean ocean?

INGERSOLL: Yes, although you can learn a lot once you're in orbit around the moon—from how much tidal flexing it's doing; if its icy crust is thin, it'll flex more, and if the crust is thick, it'll flex less.

LIPPINCOTT: But you wouldn't be able to detect any biology that way?

INGERSOLL: Well, that's the trouble, yes. Among the members of this team, I'm not one of the real drum-bangers for biological experiments. I'm a little afraid that they're premature, in that we should spend a little time deciding where to go, and what the surface of Europa is like, and where to land the lander, and things like that. But everyone says, "Well, we'll never get another chance—we've got to try." It's not a bad argument, but I'm not the one who's advancing that argument.

LIPPINCOTT: It's not that you're skeptical about the presence of any biota; it's just that you think we need to know more of where to go, or how to look?

INGERSOLL: Yes. Mars is an example. *Viking* had these very detailed biology experiments, and they were premature. We really didn't know where to look, and we hadn't really characterized the surface. Now the Mars program is much more, I think, deliberate. They've now discovered the evidence of ancient lakes, and chemical evidence, and they're learning that there are places where water is seeping out of the rocks. And they choose their landing sites very carefully.

LIPPINCOTT: Well, anything they'd find there would be fossilized evidence of life, right? Not alive?

INGERSOLL: Probably so. But there *is* water seeping out of the rocks.

LIPPINCOTT: Really? Liquid water?

INGERSOLL: Liquid water. Gullies running down the sides of hillsides. And it doesn't seem to be rainwater; it seems to be coming out of the rock. You have a crater, and in the top few strata, just below the top of the crater, water seems to be coming out of the rock and running down the sides, eroding gullies and channels.

LIPPINCOTT: But they haven't found actual liquid water, have they?

INGERSOLL: They haven't *seen* the liquid water, but there's plenty of water in the polar caps and there's water vapor in the atmosphere. But all [the evidence] they have for liquid water is the gullies. They also have these huge flood channels, but those are ancient—they go back billions of years. But the gullies are almost certainly recent geological features. Because they're on unstable slopes; and those unstable slopes are recent.

LIPPINCOTT: Do you want to talk at all about the possible greening of Mars? Do you ever get involved in that? You know, Carl Sagan used to talk about it.

INGERSOLL: Yes, terraforming Mars. I'm not into terraforming.

LIPPINCOTT: Are you just being conservative? [Laughter]

INGERSOLL: Yes. I just don't think it's going to happen. The obstacles are too great. We can speculate about it—I have nothing against speculating about it. But when you analyze it—and it's the sort of thing you *should* analyze—but to the extent that I've analyzed it, I don't see a way to do it.

LIPPINCOTT: You don't think just putting soot on the poles is going to do it?

INGERSOLL: No. For one thing, that might have been possible if the poles were made of carbon dioxide, but if they're water, water's pretty inert. You really need that carbon-dioxide greenhouse effect, and there's not enough. So therefore I prefer, in my research, to pursue lines where we can gather evidence today. I think it's a very important question: What are the polar caps made of? And that's what I'm devoting my research interest to. [Tape ends]

Begin Tape 3, Side 2

INGERSOLL: We can't talk about terraforming until we've done some of the ordinary research.

LIPPINCOTT: Well, sometime we'll have to go there, don't you think?

INGERSOLL: That's another thing. I do not think that colonizing planets or moving off the Earth is an answer to humanity's problems.

LIPPINCOTT: But Earth will burn up in 5 billion years.

INGERSOLL: Oh, OK, 5 billion years—

LIPPINCOTT: Well, that's what I was thinking of.

INGERSOLL: That's true! But in the next few *hundred* years, when social and political problems get more intense and we're running out of certain natural resources—oil, for instance—I think we're stuck here on Earth during that period. If we can survive that period...who knows? In a billion years, humanity may be so powerful that it can fly around to other stars.

LIPPINCOTT: Well, we hope so. But when Earth burns up, will Mars burn up too? Or will that have a slightly longer life?

INGERSOLL: Yes, it would be nice to move to Mars a billion years from now, when the sun heats up. But we've got some serious resource problems—geopolitical problems, pollution problems, social problems that we have to deal with in the next hundred years.

LIPPINCOTT: Now you sound like your parents, a little—coming back to Earth. [Laughter]

INGERSOLL: Yes. Well, no—they were more interested in social problems and I'm maybe more concerned with environmental problems and resource problems.

LIPPINCOTT: Maybe we should talk more about your Caltech career. You got a chair last year [Earle C. Anthony Professor of Planetary Science].

INGERSOLL: Yes.

LIPPINCOTT: Does that mean that you don't have to do any more work? [Laughter]

INGERSOLL: Listen, I'm a workaholic. I like my work. I don't want to *be* a professor if I'm not carrying my load and pulling my oar.

LIPPINCOTT: Weren't you executive officer for planetary science for a while?

INGERSOLL: That was a rather minor job. Jerry Wasserburg decided, when he was chairman [of the Division of Geological and Planetary Sciences], that he was going to restructure the division. So he appointed executive officers, and I was one of them [1987-1994].

LIPPINCOTT: Oh, yes, Wasserburg—he was division head from 1987 to 1989 and it was a kind of fiery time.

INGERSOLL: Yes. He only lasted two or three years. Well, he's a strong-willed guy. He was trying to direct—I think he was trying to overdirect—the research, overdirect people.

LIPPINCOTT: Now, when he came in, then you became executive officer for planetary science?

INGERSOLL: Right; and the division didn't have executive officers until he came in.

LIPPINCOTT: So this was his notion, to get some help in the administrative end?

INGERSOLL: I suppose so.

LIPPINCOTT: Did you then have executive officers in the division for other kinds of geology, too?

INGERSOLL: Oh, yes. But it was a minor perturbation in the way we operated.

LIPPINCOTT: So you didn't have too much to do as executive officer for planetary science?

INGERSOLL: Well, I'd been doing most of it anyway.

LIPPINCOTT: Recruiting people and so forth?

INGERSOLL: No, mostly administering students and teaching and things like that. Finding young faculty was something the whole division did. And also, in planetary science we shared the work on advising students and worrying about our facilities and so forth. Maybe the executive officer coordinated things, but it was a shared responsibility. So it wasn't a big deal.

LIPPINCOTT: Barclay Kamb was a much longer-lived head than Jerry.

INGERSOLL: Yes. Barclay was division head for ten or eleven years [1972-1983]. Ed [Edward M.] Stolper has been in there for ten or eleven years [since 1994]. Ed's done a great job getting new faculty.

LIPPINCOTT: And now he's acting provost, isn't he?

INGERSOLL: Now he's acting provost; they're looking for a replacement—certainly for the division chair, and I don't know if he wants the provost job or not. I bet he'll be asked. Whether he'll accept, I don't know.

LIPPINCOTT: Did you have any other administrative chores at Caltech?

INGERSOLL: No.

LIPPINCOTT: Any other of the standing faculty committees, do you remember?

INGERSOLL: I was on the Faculty Board, the Academic Policies Committee, the Athletics Committee, and a few others, but those weren't too time-consuming.

LIPPINCOTT: You like to stay away from that kind of thing?

INGERSOLL: Well, I certainly did stay away from it. Because, you know, being on a spacecraft team is time-consuming. You have a lot of meetings, because it's a big operation. You have a hundred or two hundred scientists, and they're all pulling in different directions, and you have to make a lot of compromises on how you use the resources of the spacecraft—because you don't have limitless resources. So it's time-consuming. I devoted a lot of my administrative effort to those kinds of activities.

But just in the last year, I've been head of the Academic Policies Committee at Caltech.

LIPPINCOTT: What do they do?

INGERSOLL: Well, anything we want. If you look at the charter for the Academic Policies Committee, it says that the committee shall study and recommend to the faculty changes in the academic policies of the institute. It shall not be limited in the scope of its deliberations or in the topics that it takes up.

LIPPINCOTT: Do you look at the core curriculum?

INGERSOLL: Well, yes. One thing I would like to do, and I've found it very difficult to do, is increase the amount of time that students spend on research, as opposed to the time they spend in the classroom. I think the typical undergraduate's life at Caltech is problem sets—the academic life is solving problem sets. It even affects their social lives.

LIPPINCOTT: I know from the little I know of the students, that they're pretty weary. But there's so much to learn now, as opposed to what there was to learn at Caltech in the 1940s.

INGERSOLL: Yes, and that's where the resistance comes in. The options say, "Well, look, we've designed curricula according to what we think is necessary for a student to know, and if they're diddling around in the laboratory, they won't be learning all this stuff." On the other hand, you *do* learn when you're "diddling around" in the laboratory. You learn a different kind of thing. You learn how to do research.

LIPPINCOTT: But what about the basics in physics, and in biology now?

INGERSOLL: There's basics in every subject, and that's the eternal debate: Should you learn the basics, stuff your head full of basics, or should you learn how to do things with the assumption that if you learn *how* to learn, that's more important than *what* you learn? Since many students are going to be researchers, learning how to be researchers is very important. I didn't learn how to be a researcher even in graduate school. I had to flounder around at Caltech. I wasn't very good at it initially. In my early papers, I antagonized other people in the field, so I had problems getting stuff published. I think learning how to do research is important.

LIPPINCOTT: Yes. So they'd have to shave some of the courses off the core curriculum in order to have things the way you would like to do it. Are you getting anywhere with this point of view?

INGERSOLL: Well, we took up another topic that we got a lot farther on, which is the advisor system.

LIPPINCOTT: Is that working all right now?

INGERSOLL: Well, the way to improve the advising system is to give students the opportunity to provide feedback on the advising they're getting. If the students start reporting that a given option is doing a bad job of advising, then that's pressure on the option to improve the advising they offer. This is sort of a self-correcting system. So I'm very hopeful that it's going to have some effect.

LIPPINCOTT: Is planetary science its own option? Or is it just under the rubric of geology?

INGERSOLL: It's its own option for undergrads.

LIPPINCOTT: Is it popular nowadays?

INGERSOLL: Well, we have a few students every year. We've had a half a dozen undergraduates some years.

LIPPINCOTT: That's not a lot. You'd think there'd be more, with the planetary missions and so on.

INGERSOLL: Well, it's not as generalizable as something like electrical engineering or computer science. You have fewer career options if you're a planetary scientist. You have to go into planetary science, whereas an electrical engineer can really do a lot of things. So I think it's about right; we have the right number.

LIPPINCOTT: How about graduate students?

INGERSOLL: We have six or eight professors and four or five students each year for the option. So it's a good number. In my division, we don't have large research teams like the biologists or the chemists do, with twenty people on a team.

LIPPINCOTT: Would all your graduate students be involved in the missions that are run at JPL?

INGERSOLL: Many of them are. Some are doing purely theoretical work, and some are analyzing data from missions. Some really get involved in the mission planning and that sort of thing. There are different levels of involvement.

LIPPINCOTT: What do you see ahead for yourself? You have this chair now.

INGERSOLL: I'm not going to retire for a while. I like my work.

LIPPINCOTT: Are you involved in *Cassini* at all?

INGERSOLL: I'm definitely involved—I'm a member of the imaging team. I wouldn't mind doing more for Caltech and less for NASA, in the future.

LIPPINCOTT: You mean more in the way of teaching courses?

INGERSOLL: Teaching, and even administration. Finding people—you know, the continuous search for new faculty. And also improving the lot of undergraduates. Undergraduates are in danger of being left out at Caltech. The institute is growing, but those elements that are growing are postdocs, research staff, graduate students. Undergrads are staying constant. And they're an increasingly small part of the budget, a small part of the faculty attention. I don't think it's healthy.

LIPPINCOTT: So you think that's a current trend—that it wasn't so in the past so much?

INGERSOLL: Well, the trend is documented—that the research staff and the postdoctoral numbers are growing the fastest, and the undergrads are not growing at all. And the number of faculty is not growing.

LIPPINCOTT: When do you think that started? Under what president? Would you know?

INGERSOLL: I don't know when it started, but it's been going on for ten years at least. And it's not really due to any policy—it's mostly due to the economics of research funding. Everyone's more successful in getting research grants, so you hire postdocs and research staff.

LIPPINCOTT: Well, that's good, in a way.

INGERSOLL: That's good, but you don't want to forget the undergraduates, if you're going to have them.

LIPPINCOTT: Do you still teach a lot of undergraduate courses?

INGERSOLL: I've been teaching a mixture of courses. I taught freshman physics a long time ago.

LIPPINCOTT: You don't have to do that anymore.

INGERSOLL: No, I don't teach big courses. I teach two courses in the GPS division and I teach two courses in environmental engineering—well, they're joint courses, joint environmental engineering and GPS, and they're on atmospheric dynamics.

LIPPINCOTT: Is that in aid of avoiding things like global warming and stuff? Is that the idea?

INGERSOLL: Yes. It's the basic science of atmospheric dynamics and it gets into global warming, and it gets into atmospheric circulation.

LIPPINCOTT: Do you think we're in a bad way, as far as our atmosphere goes, right now?

INGERSOLL: Well, I think global warming is going to happen. It's one of the problems that humanity has to face. It's heresy to say this, as a good card-carrying green environmentalist, but I think it's not as serious a problem as warfare and the competition for natural resources and the competition for land, and the inequalities of the distribution of wealth. I think those are greater problems. Global warming will get us eventually, but we've got to solve some more immediate problems.

LIPPINCOTT: How soon is global warming going to get us? Aren't there factors that might mitigate against it? Isn't it kind of cyclical—so you can maybe emerge from a period of global warming still intact?

INGERSOLL: There are cycles, definitely—El Niño and longer-term cycles, ice-age cycles even longer-term than that. But there are also trends. And as we add greenhouse gases to the atmosphere, there are trends, and those don't correct themselves unless you stop adding the gases.

LIPPINCOTT: So humankind is a factor in this?

INGERSOLL: Yes, more and more so. People debate about whether the signal of human influence on climate is detectable now or not, and I think that is a misleading debate. Because if it were to be shown that all the ups and downs of the last decade or two were natural and none of them had anything to do with human influences, that doesn't mean that we should all fold up shop and stop worrying about the problem. Just because we haven't detected it yet does not mean that it's not going to happen. And yet many people in this debate are saying, "Well, look, natural variability is just as large as the effect of humans. Stop worrying about it." Well, that's crazy!

LIPPINCOTT: Isn't there a signal? You can go back to the industrial revolution, in the middle of the nineteenth century, and if you look back then and up to now, you can see something that isn't simply natural?

INGERSOLL: You can see something. It definitely has gotten warmer. But whether that warming is mostly natural or mostly human, we don't know. Now, actually there *are* signals. The human-induced part of climate change is getting increasingly clear. But there are people who argue the other side as well, and I just feel that that's not the issue. The issue is: What's going to happen in the next hundred years? And in this century—the twenty-first century?

LIPPINCOTT: Do you think about ocean currents, too? They say that Europe is going to freeze up because the Gulf Stream—

INGERSOLL: —will shut down because there's so much fresh water? Yes, I think about that kind of stuff. And I've written a paper, which is right now being reviewed—although it's been a shockingly long time since I submitted it; it's been six months, and I haven't heard from the editor yet. I write them once a month. This month I'm going to write his boss.

LIPPINCOTT: What journal is this?

INGERSOLL: The *Journal of Physical Oceanography*.

LIPPINCOTT: Have you published with them before?

INGERSOLL: No. Last month I wrote them and he apologized and said the referee that's holding it up is a very thorough and very thoughtful referee, but he's slow. OK, but this month I'm going to write him and say, "I'm writing your boss." [Laughter]

LIPPINCOTT: What's the paper on?

INGERSOLL: Oceanography—on how the ocean could possibly change states with salinity—salty water sinking instead of cold water sinking. ["Boussinesq and Anelastic Approximations Revisited: Potential Energy Release During Thermobaric Instability," *Jour. Phys. Ocean.* 35:1359-1369 (2005)] The sinking requires dense water, and you can make high-density water in two ways: One is to make it very cold, and that water is dense. But also, if you evaporate the fresh water away, you can make it salty, and salty water is more dense. So you can drive the oceans in two ways: One is to drive it with extracted heat and the other is to drive it by extracting fresh water. So I'm trying to get involved in that debate. And in the climate record from the last glacial period, in the midst of this generally cold period that lasted for fifty thousand or a hundred thousand years, there were these hiccups, where it got much warmer for a thousand years and then it cooled down again. Those [intervals] are probably caused by the ocean changing its circulation. There are astronomical cycles—Earth's tilt and Earth's orbit—but they come and go on a much longer time scale. In fact, the ice-age cycles themselves are probably linked to the astronomical cycles.

LIPPINCOTT: Are you talking about the precession of the poles? It tilts different ways?

INGERSOLL: Precession of the equinoxes. There are several effects. One is the tilt of the Earth's axis. When it's tilted over, the poles get more sunlight, and when it's less tilted, the poles get less sunlight. That's one of the factors. Another is that sometimes the Northern Hemisphere's summers occur when Earth is closest to the sun, and sometimes the Northern Hemisphere's summers occur when Earth is farthest from the sun. So you get warm summers and cool summers. And of course, whether it's warm or cool affects how much ice is retained and how much is melted. So that's another one of these astronomical cycles that have to do with the precession of the equinoxes.

Anyway, those have long periods, whereas these sudden hiccups last for a thousand years, which is much shorter than the astronomical cycles.

LIPPINCOTT: And the ice—can it retreat in that time?

INGERSOLL: Well, what we really know is that it got warmer [in that time]. You can tell that from the nature of Greenland ice cores—Greenland ice cores that go back a hundred thousand years—and you can see that it got warmer [in those brief periods]. You can't really tell the extent of the ice or what it was doing, but you do know that Earth got warmer. I got involved with that research. Kind of a new direction for me—but it's good to have a new direction. [Tape turned off]