Charles Hard Townes, Physicist, Astronomer, University Professor

Interviewed at his office at the University of California, Berkeley. June 20th, 2005
Born: 1915 in Greenville, South Carolina

> *I was at once confused and amazed by Theodore. First, since he was obviously a scientist of considerable repute… In fact he was the only person I had met until now who seemed to share my enthusiasm for zoology. Secondly,… he treated me and talked to me exactly as though I were his own age… but also as though I were as knowledgeable as he."*
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> *— Gerald Durrell reflecting on Dr. Theodore Stephanides when Gerald was 11. Recounted in "My Family and Other Animals" (Penguin Books, 2000, p. 61)*

CHT:
When I was a youngster I was very interested in natural history. I used to walk in the woods and the streams and catch butterflies, and watch birds and look at the stars, and so on. All the universe was fascinating to me. I thought I would probably go into some kind of science.

I had an older brother who was also interested and we used to catch insects together and he was two and a half years older than I. He was always better than I was, we used compete of course [Laughs], and he went into biology. I sometimes say that's why I didn't go into biology: I felt I couldn't compete with him. But actually I didn't go into biology — even though I was fascinated by it — because biology at that time was descriptive largely. It didn't get down to basics, it was descriptive. And when I took my first course in physics, oh boy! That seemed like "it" because with physics you could pretty definitely prove whether you were right or wrong. Very specific, it was much more basic.

I took my first course in physics as a sophomore in college. I'd taken some biology, and I took some chemistry, and I took a lot of mathematics — I liked mathematics. But physics REALLY was it because you could apply mathematics, you could make good reasoning and so on, and figure things out. And I like to know how the universe works. I like to know how things work, and try to make them work. I used to tinker with things, and try to fix things. I like to… to make things work.

It was very clear to me: what I wanted to do was physics. So now the question was, "Would I be able to do it? Could I get a job in physics?" Physics wasn't very well known at that time actually. When they asked me, I told my friends I was going to go into physics and they would say, "Physics? What is that? Is that something like civics?" No! [Laughs] Not at all like civics. It's a science! [Laughs]

Well, physics is better known now. Much better known, partly because of what it did in World War II, it really won World War II. And of course then made the very frightening atomic bombs, but it did a lot of other things too.

Well, I had to get some kind of help in a university to finance my graduate work and I managed to get a teaching assistantship at Duke University. Duke was pretty good, but it wasn't one of the best. I applied to a lot of other very good schools and I didn't get any financial help. I applied a second year and I didn't get any financial help. Oh dear! What should I do?

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Well, I had saved up $500 — which was a lot more money than it is now — I'd saved up $500, and I decided I'll just go to the very best place that I'd ever heard of, and that was Cal Tech at that point. At Cal Tech (Robert J.) Oppenheimer was teaching there. (Robert A.) Millikan was President of the university. Cal Tech, California Institute of Technology that is. (Albert) Einstein went there from time to time. Linus Pauling was head of the chemistry department there. It was a wonderful place. It was a small school. And I like small schools where you can interact with everybody.

So with $500 I got on a bus and went out to California from South Carolina where I lived. I went out on the bus and it was easy to get enrolled in graduate school then, but it wasn't easy to get financing. This was the depression years. 1936 this was. So I got enrolled, all right, and after I'd been there one semester fortunately they gave me a teaching assistantship, so then I could pay the rest of my way. That made it so that I finished my degree at Cal Tech.

Now I was also wondering at that time, "Do I want to do theoretical work or experimental work?" I was having some eye trouble, I was reading a lot and my eyes were bothering me and a medical doctor, an oculist worked on it, and finally he said, "Well, you had just better stop doing physics because your eyes are not going to get any better. You'll probably have to change fields. Don't do any physics…"

Well, I wouldn't want to leave physics! Oh no! [Laughs] But I decided, "Well, maybe the thing to do is to do experimental physics, instead of theoretical physics." That was a great decision. Just the right thing to do: experimental physics. I think you can do a lot more in experimental physics than in theory, generally. You want to know as much theory as you can, but a combination of good theory and experimental work is very fruitful. So I did experimental work in physics.

I did experimental work with a professor who… he was a little tough. He was a nice guy but he was tough on his students. He really made them work hard, so he didn't have many students. And I thought, "Well, he didn't have many students so maybe I'll get a lot of attention from him." [Laughs] So that's why I went to work with him. W.R. Smythe was his name. He really gave me a lot of attention. I separated isotopes, I made pumps, vacuum pumps, to circulate gas and thereby concentrate isotopes of carbon, and oxygen, and nitrogen, and study their nuclear properties to find out what their spins were. The nucleus would spin a certain amount, and I could measure them doing spectroscopy.

I got my degree and I wanted to be in a university. What I wanted to do was think about things, and understand things. I wanted to be in a university. University jobs were very scarce and a Bell Labs representative came along and offered me a job. Well, I wasn't awfully interested, I didn't want to go into industry. I wanted to do fundamental physics and be in a university. But there weren't any jobs so my professor said, "Well, you'd better take this." And I took it. I knew Bell Labs was a good place, it just wasn't a university, but I went there. And it was a big success, even though I was there only a year before I had to do engineering.

The war had come along and everybody had to pitch in to the war, and I recognized that, and Bell Labs assigned me to work on radar. I was to design a radar bombing system for aircraft. For radar to decide where to drop bombs, and so on. And I had to work on that, design that, and for several years during the war I worked on radar of various kinds.

I learned a lot of engineering. It was very valuable to me. A combination of engineering and physics was very valuable. The engineers at that time didn't know a lot of quantum mechanics, and the physicists missed a lot of things that the engineers understood like oscillators, and

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amplifiers, and things like this,… circuits. And I learned how to build all those, and how to design them, and what not. That was very helpful.

One of the last radars that I was designing was supposed to work at a rather short wavelength: 1.25 cm. That’s about half an inch wavelength. And I studied this and I recognized that those wavelengths were absorbed by water vapor in the atmosphere. I tried to persuade Bell Labs and the government: “No, we shouldn’t build it at that wavelength because it will probably be absorbed.” Well, maybe they believed me but they said, “Well, no. We decided to do this. We’d better go ahead. We gotta go ahead.”

So we built it and put it up in the air and, sure enough, the waves were absorbed by water vapor. Well, what that meant to me was — I had studied the absorption to see how it behaved, and I realized — “Hey, this looks like a very good way for understanding water, and we can do spectroscopy and look at the absorption of microwaves.” We could do very high resolution spectroscopy, get very accurate measurements of the behavior of molecules by looking at the absorption of microwaves. I recognized that ammonia would absorb nicely, and water, and a lot of others. And so right after the war I urged Bell Labs to let me do microwave spectroscopy, that is studying molecules with microwaves.

Bell Labs didn’t really think that was going to pay off for them very much, they wanted me to do engineering. They said, “Look, you’ve learned a lot of engineering. We’ve got so many good things for you to do.” But I said, “No, I really want to do physics. I really want to understand things. Explore new things.” “Well,” they said. “Well, OK. We’ll let you do it. We’re not going to hire anybody else in that field, but if you want to do it, OK.” That was very generous of them, and that was Bell Labs in those days. It was willing to do long-term basic research and explore things. And I like to explore. That’s my pleasure: to explore and find out new things.

So I did that, and it was a very promising field. After a few years I published a lot of new findings about molecules, about nuclei in molecules, and I was offered a job at Columbia University. So I got a job in university [Laughs] as a result of that work!

I went to Columbia University and continued to do that kind of work. We found a lot interesting things: the properties of nuclei, the masses, the shapes, how they were spinning. And the properties of molecules, their structure and so on. I had a lot of students that worked with me and we had a great time of it.

But I wanted to get to shorter wavelengths. We were making these wavelengths with klystrons and magnetrons; these had been invented shortly before the war and very much developed in the war. They worked very well, but they couldn’t get to wavelengths shorter than about a half an inch, a centimeter or something like that. Maybe two or three times shorter than that, but not much more.

Now, as you go down from a centimeter to, let’s say, a millimeter we begin to call that infrared — shorter than a millimeter we call infrared radiation rather than microwaves. I wanted to get on down below a millimeter in wavelength because I saw there was a lot of spectroscopy that could be done. One could study atoms and molecules still better, and many more of them. I wanted to get down there but we couldn’t make any oscillators. Well, I knew a lot of engineering then and I worked on various kinds of oscillators hoping to make ‘em oscillate faster and faster and get to shorter wavelengths. My students worked with me. They (the oscillators) sort of worked a little bit but they didn’t work well enough.

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Now the Navy knew that I was interested in getting to short waves so they appointed me chairman of a committee, a national committee, to see if anybody knew how to get to short waves. To get down to, or below, a millimeter. We traveled all around trying to see if anybody had any good ideas. We traveled to Europe, the United States, and we didn't find any good new ideas.

So I was going to have one last meeting in Washington, D.C., of our committee, and say "Well, we'll wrap it up because we just haven't found any right answers." And I woke up early in the morning worrying about it. Breakfast wasn't ready yet in the hotel so I went out and sat in the park there in Washington. A nice sunny day and I thought "Well, why haven't we been able to get to short waves?" And I thought it, and I thought it. I thought of all the different ways that I'd considered.

Well, I knew molecules could produce short waves. Molecules and atoms can oscillate very fast to produce short waves, but the second law of thermodynamics says you can't get more than a certain amount of energy from them until you heat them up. The higher you heat them the more energy you can get, but to get enough radiation from them you'd have to heat them so hot the molecules would fall apart. I knew that was true, and that was too bad. And suddenly I said, "Wait a minute! Wait a minute! We can get molecules in a state that's not described by a temperature."

See, in a temperature (distribution) the molecules, some of them are low energy, some of them are high energy, some of them are still higher energy. The distribution in different energy states depends on the temperature.

"Wait a minute, they don't have to be described by temperature. We can pick out molecules all in a highly excited state." I knew how to do that because at Columbia University people had been working with molecular beams. As you take a gas of atoms or molecules, let it go through a small hole into a vacuum, and it's a beam then — it extends on out — and now you put an electric field, or a magnetic field nearby, and that deflects them. Atoms and molecules in one state will get deflected one way, and in another state will get deflected another way, so you'll pick out which ones you want. I knew about that because some of my friends at Columbia University were doing that kind of thing.

So I quickly wrote down… I pulled out a envelope and a pen, and I wrote down the numbers and the equations. I was very familiar with it all then and I said to myself, "Now, could this be done? Could we pick out molecules all in an excited state, send them in to a cavity, let them radiate, and the radiation would be caught in the cavity and would stimulate them to radiate still more?" That's called Stimulated Emission of Radiation.

Now Einstein first discovered that. We knew that he knew — and everybody knew — that atoms can absorb a quantum of radiation, or can emit a quantum of radiation. Depending on if the atom is in the lower state it will absorb, if it's in an upper state it will emit. But what Einstein also showed is that it would also be true that if light comes along — if a photon comes along — and comes close to the atoms in an excited state it will induce it to fall down and give up the energy. So that's induced, or stimulated radiation: stimulated emission of radiation. Einstein discovered that. Everybody knew about it — all the physicists knew about it — but they just hadn't had the idea that I had of how to use it.

I saw how to separate molecules and get a lot of them in an excited state, send them into a cavity and they would emit some, and the radiation would bounce back and forth in the cavity, and make them emit still more until we get all of the energy out of the molecules before they

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then passed on through the cavity. “Ah! There’s a way of getting just as high a frequency of radiation as we want!” And, oh boy, I wrote down the numbers and it looked like it would work.

But I wasn’t absolutely sure it was going to work, so I didn’t mention it to the committee, and we had our last meeting. I went home to Columbia University and I persuaded a graduate student to help me work on it: Jim Gordon, he was a good student. He’d had some experience with molecular beams and he was willing to try it. And I told him, “Well, it’s a little bit chancy. I think it will work. If it doesn’t work I think that even so you can get a good thesis out of it, by studying it.” So he tried it.

Well, now, it’s very natural that when you have a new idea there are going to be some people that don’t believe you. A breakthrough… any breakthrough kind of challenges the people in the field. And so a number of people told me, “Oh, that won’t work.” One of the professors at Columbia, a very well known theorist, every time he saw me in the hallway he said, “You know that’s not going to work. You should stop.” Well I looked at it very carefully, I looked at the theory very carefully. I was pretty sure it would work.

My student and I had been working on it about two years — we hadn’t made it work yet but we were coming along. And then the head of the department, Professor (Isidor Isaac) Rabi, a very famous physicist, a Nobel Prize winner, came into my office, and the person who was going to be the next head, Professor (Polykarp) Kusch, who also got a Nobel Prize, came into my office. They came into my office and sat down and said, “Look! That’s not going to work. You know it’s not going to work. We know it’s not going to work. You’re wasting the department’s money — you’ve got to stop.”

Well, that’s the picture, you see. A lot of people don’t always agree with you, especially if you have a new idea. Fortunately I had tenure. That is they couldn’t fire me just because they didn’t agree with me [Laughs]. If I did something wrong, let’s say ethically wrong, they could fire, but not if I just made a mistake in physics. So I said, “No, I think that it has a good chance of working. I’m going to keep going.” They marched out of my office kind of angrily. And we kept going.

Two months later, I was teaching a class, and Jim Gordon dashed into my class and says “It’s working! It’s working!” Oh boy! So we were getting oscillations produced by the molecules. We tried it first in the microwave region because we had a lot of equipment there. I thought, “Well, we want to get down to shorter wavelengths, but we’ll do that later.” We first tried the microwave region, about 1 cm with ammonia molecules. And it was working, made a very pure oscillation of 1 cm wavelength, very, very pure. And oh, that was great. We published it and everybody got excited about it. However, people were still skeptical because it was a new idea!

I went over to Denmark at one point, and I knew Niels Bohr’s son — he was a friend of mine — and I visited Niels Bohr and he was walking along the street with me and he asked me what I was doing. And I told him we had this device which produced a very pure oscillation from molecules. We called it a MASER, which my students and I invented, a name for Microwave Amplification by Stimulated Emission of Radiation: M.A.S.E.R. for “maser”.

I told him we had this thing, and it was very pure radiation. Now Niels Bohr, one of the most famous theorists at the time said, “Oh no, no. That can’t be. No, you must misunderstand. It can’t give such pure frequencies.” He was thinking, I believe, of the uncertainty principle. The uncertainty principle applied, the way he was thinking of it, to single molecules. But I was studying a whole beam of molecules, a whole collection of molecules in there. “Oh, it can,” I said. “Oh yes!”

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“Oh no. You must misunderstand,” he said. Well, finally he said, “Well, maybe you’re right.” I’m not sure he ever believed me.

And with another very famous physicist (it was) much the same. I was at a cocktail party in Princeton with John Von Neumann, a very famous mathematical physicist. And he asked me what I was doing, and I told him. He said, “Oh no. You can’t get such pure frequencies. Oh no.”

“Oh yes! I know we can — we’re doing it!”

“Oh no. You misunderstand. Something’s wrong.” Well, he went off and got another cocktail and about 15 minutes later he came back. “Heh!” He said, “Hey, you’re right! You’re right!” He’d thought about it some more.

Now, what was the problem. They were thinking about the uncertainty principle. The uncertainty principle says if you have a molecule that’s going through a cavity, and it’s in there a short time only, then you can’t measure the frequency very precisely. But, the point is, we had a lot of molecules that were going through there all the time, it wasn’t a single molecule. And we had feedback, a feedback oscillator.

Now any engineer would know if you have a feedback oscillator you’re going to get a very pure frequency. The physicists weren’t accustomed to the engineering ideas, and the engineers weren’t accustomed to the quantum mechanics and how molecules worked. It was just putting those together. So getting two different fields, you see, it’s very good to know different fields of science and… and put them together. Engineers recognized immediately, “Oh sure, this will give a pure frequency.” But the physicists thought “Oh no, it can’t!” [Laughs].

Well, it became very popular and very exciting. About two years later than that we’d been working on it and having good fun with it and made the most perfect, the most wonderful amplifiers, about 100 times more sensitive than any other amplifier that anybody had ever made before. And it made a very pure frequency. We called it “an atomic clock” and we could get very pure timing with it. Lot of people all over the world were working on them, and it was exciting.

Now, before we made it work nobody wanted to work on it… except me… and some Russians. And the Russians and I got the Nobel Prize together. Well, they worked on the idea too, they had it somewhat independently. People would come into my laboratory and see it and say, “Oh well, that’s an amusing idea, Uh huh.” But nobody was competing. Once it got going, then everybody was competing to make more of them and make better ones and so on. Got very exciting to everybody.

But they didn’t think it could get down to very short wavelengths. I wanted to get on down into the infrared. Shorter than a millimeter, but they thought, “Well, maybe a millimeter but not much shorter.” And nobody worried about it. I thought “Well, now, I want to get on down.” So I just sat down and thought, “What’s the best way to do it? I’m sure we can do it.” And I wrote down the numbers and what to do, and I said, “Hey! Wait! It looks like we can get right on down to optical wavelengths. We could make light with this!” Get light from molecules and amplify light, of all things! Wow! So I tried to figure out just the best way to do it.

I was consulting at Bell Telephone Laboratories then, and the Bell Telephone Laboratories asked me to come talk with their scientists and just sort of try to help stimulate them, and I would learn things from it too, and that was great. So I would go out there one day every two weeks and I would visit. My brother-in-law Arthur Schawlow was working there. He had worked

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with me as a post-doc, married my kid sister, and then gone to Bell Labs, and I talked to him about it. “Oh!” he said. “That’s very interesting. I’d been wondering whether we could get to shorter wavelengths. Could we work together?” So we worked together and we wrote a paper on it.

So Schawlow and I invented the Laser. That does the same thing that I did making microwaves but to get to shorter wavelengths it was now called “laser” for Light Amplification by Stimulated Emission of Radiation: L.A.S.E.R. Light amplification.

By that time we knew everybody would be excited about the field, but nobody had thought it would get to shorter wavelengths, so what we did was construct a theoretical paper showing how it could be done. Rather than trying to do it we showed how it could be done and let everybody try it. So we published that paper and then everybody jumped into the business and then tried to do it.

By now there have been, oh, ten or twelve different people that have received the Nobel Prize using masers and lasers as a scientific tool. I’m just delighted it’s so useful in science. Of course it was very useful in industry too, particularly in communications, fiber optics communications, also in computing and recording, recording information, and readouts, and all kinds of things. It’s an enormous industry now and very, very useful.

So I’m delighted. I’m delighted to see it used, and I’m particularly pleased to see it used for medical purposes. A friend of mine comes to me and says, uh… “You know this has saved my eyes.” And, oh boy, that’s very emotionally pleasing to me… Of all things, ah… A lot of these things I couldn’t imagine at the time. I saw there were going to be a lot of uses, but a lot of them I didn’t know at the time. I didn’t know about the kind of eye trouble that’s saved by lasers. I’d never heard of it! [Laughs]. I couldn’t imagine that.

So new things are new, and it’s great. You have new ideas; where do they come from? Well, you just work at it, and work at it, and sometimes an idea comes [Laughs]. That’s about it.

LS:
A student these days has a choice of following a path that’s prescribed for them, often. Or trying to find their own. And that’s a common struggle. People try to meet a standard even if it means sacrificing their enthusiasm. People, even at a young are trying to study for competitive tests more than going out and finding bugs.

CHT:
Um, hmm.

LS:
Do you know of the books of Gerald Durrell, did you read those?

CHT:
No, uh… no.

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LS:

He wrote these books when he was 30. He became a zoologist and he was trying to fund his own animal studies so he wrote about his childhood on the island of Corfu, in Greece, in 1932 to 1934, when he was an incredibly curious 10-year old.

He had a very odd family because his brother was Lawrence Durrell, becoming a famous Continental writer. And he describes being in the country with the Greek peasants. He got a doctor, who was a naturalist, to teach him. And they would go out together — you know an older man — and they would go out in the bogs and collect bugs. And he wrote these stories for kids. He was very popular in the 1940's. It just reminded me of your story.

I read these to my kid, and they were read to me when I was a little boy, very young: eight. In my own fathering I find it extremely important, critically important to keep that sense of discovery alive. I want to do everything I can to avoid quenching it. What would you say about that struggle?

CHT:

Well, I think curiosity and discovery, and sense of discovery, and wanting to discover, and wanting to figure out things, figure out new things is great. Now, that doesn't mean that you shouldn't read what other people have done carefully. And think about them, and work problems.

I learned an enormous amount by working problems in a book by my professor at Cal Tech. He wrote a book on electromagnetic theory and he had an enormous number of problems in them. And it was a new book, and I was the first one to sort of work all the problems. I learned a whale of a lot from just trying to understand and work those problems and read the book carefully, and so on. So you learn as much as you can.

On the other hand, I've always liked to go off in other directions that I feel other people are missing. For example, when I started on microwave spectroscopy there wasn't anybody else doing that. I started on microwave spectroscopy in World War II and it was a new field. I saw a lot of new things that could be done, and I thought people were missing that. After a while microwave spectroscopy gained popularity, of course I went and invented the maser and the laser, and those became very popular. I use them, we did a lot of good science with them. Then there were a lot of good people in the field.

I thought, "Well, I ought to do something that's being missed." And I went into astronomy. Infrared astronomy especially. There were very few people working in infrared astronomy at that time and it looked like a very rich field to me. So I went into infrared astronomy, I saw ways I could use the laser to do it too. And also radio astronomy.

Radio astronomy wasn't very much appreciated in the early part of the century. And after the war we had a lot of radar, radar equipment. We knew how to use the radio waves very well, and short waves. But, particularly in the United States, people weren't doing radio astronomy very much.

And so I went into radio astronomy, and infrared astronomy — new fields that people were missing. And that's just fun, to explore new things! [Laughs] Go out into new territory and try to find out new things. That's great. Well, you want to learn everything else you can, too, and the more you know the more likely you are to find out something new. You learn what other people are doing. Interact with a lot of different people, interact with different fields. Nevertheless, think

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of those things that are being overlooked, not recognized. New ideas, new ways of doing things, new fields that people are not really exploiting.

And I'd move from one to the other, uh… oh, about every 15 years the field I'm doing becomes popular [Laughs]. Once I make a success of it, it begins to become very popular, and then I move onto something else [Laughs]. There are enough people in the field: they don't need me anymore.

LS:
Did you ever have second thoughts about your ideas? I mean, you progress into these fields against the tide of better advice. But it's sort of contradictory because… it's as if you're talking from the point of view of an ice-breaker as if you had no ice to break.

CHT:
[Laughs]

LS:
How much soul-searching did you have to do because you were out on a limb, or didn't you think you were out on a limb?

CHT:
Well, I thought I saw some promising things to do. Other people disagreed with me. You have to know how to disagree with other people. If somebody disagrees with you, if it's a serious person you want to think carefully about what he's saying. Think carefully about whether you are really likely to be right or not.

You see, Professor Rabi and Professor Kusch wanted to stop me from doing this work toward the maser and the laser. They assured me it wasn't going to work and so on. Well, I thought about it carefully and I thought it had a reasonable chance and I was gonna do it. You don't want to be stopped by somebody that disagrees with you if you think you're really right, but you do want to listen to them carefully.

Now that's happened to me over and over again. In radio astronomy, for example, I thought of going into radio astronomy and I went out to see a very famous professor who was head of the two biggest observatories in the world at that time. Mr. Bowen, he was head of Mount Wilson and Mount Palomar observatories. And I said, "Well, you know, here I am with Bell Labs and I have all this good radio equipment, and I've learned a lot of engineering, I think I'd like to do some radio astronomy. What do you think would be the best thing to do?" And Dr. Bowen looked at me and he said, "Well, I'm sorry but I don't think radio waves will ever tell us anything about astronomy." Well, I was sure he wasn't right, but I didn't know just what to do, so I didn't do that and I did microwave spectroscopy instead.

Well later I saw ways of doing radio astronomy that I thought would be good. But now radio astronomy has turned around and become somewhat popular with astronomers. Astronomers like Dr. Bowen, and most astronomers at that time, didn't think there was that much in it, but it turned out to be a very promising, rich field.

I came out to California to do astronomy, and one of the things I wanted to do was look for molecules in space. Molecules in space. Well, using microwaves I wanted to look for ammonia,

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for example. Now one of the very important theorists in the department at Berkeley, Department of Astronomy, told me, “Look, that’s not going to be there. The molecules can’t be there. I can show you they can’t be there theoretically: they get torn apart by ultraviolet radiation. They can’t be there.”

Well, I thought I understood enough about astronomy, I thought there were ways they could be there. And he said, “Oh no, you can’t be right. You’ll just waste your time.” But there was an electrical engineer at Berkeley who was willing to work with me, and a good student, Al Cheung. A Chinese immigrant student. And we worked together, we looked, and we found it! We found ammonia, and then we found water. Of all things: the water was a maser. There were natural masers out in space and nobody had recognized that.

Now I’ve tried some things that don’t work. Well, OK. Let me tell you one of the ones that didn’t work. After finding molecules in space I wanted to look for Hydrogen out there. Now Hydrogen has been found in our galaxy, but I thought that maybe there was Hydrogen between galaxies. A galaxy is a big collection of stars, and that’s our milky way, and there are many other galaxies like it. I thought that between galaxies there might be some Hydrogen. We should look. I wanted to look.

Well, I persuaded a student, Arno Penzias, to work with me and look for Hydrogen in inter-galactic space. And he worked hard at it. We made a maser amplifier to get great sensitivity, and he looked. He didn’t find it. Well, that still made a thesis for him, he got his Ph.D. and he got a job a Bell Labs and he wanted to look some more. And so he made a good maser amplifier and he had a very good antenna at Bell Labs. And he and a friend of his, Bob Wilson, looked. They didn’t find Hydrogen, but what did they find? They found the Big Bang, the origin of the universe. The Big Bang. They found there was radio radiation there, coming from everywhere, due to the Big Bang: the initiation of the universe 13 billion years ago. What a wonderful discovery… So he didn’t find Hydrogen, a great disappointment maybe but — wow! — he made one of the most important discoveries in the world. [Laughs]

LS:
They stumbled on it, didn’t they!

CHT:
Yeah, they stumbled on it looking for this thing which wasn’t there, you see.

So it’s not bad to keep looking, keep looking. No telling what you’ll find. And of course he got a Nobel Prize for that, too. [Laughs]. And we’ve learned a great deal about the universe: it had an origin, and we understand a great deal about it now as a result of finding these microwaves.

So, you can be wrong. Don’t worry about it too much [Laughs]. Try hard to be right, but don’t be unwilling to take some chances. And look hard for things that might be there, that you think have a reasonable chance of being there. Look, and a failure or two won’t hurt you.

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