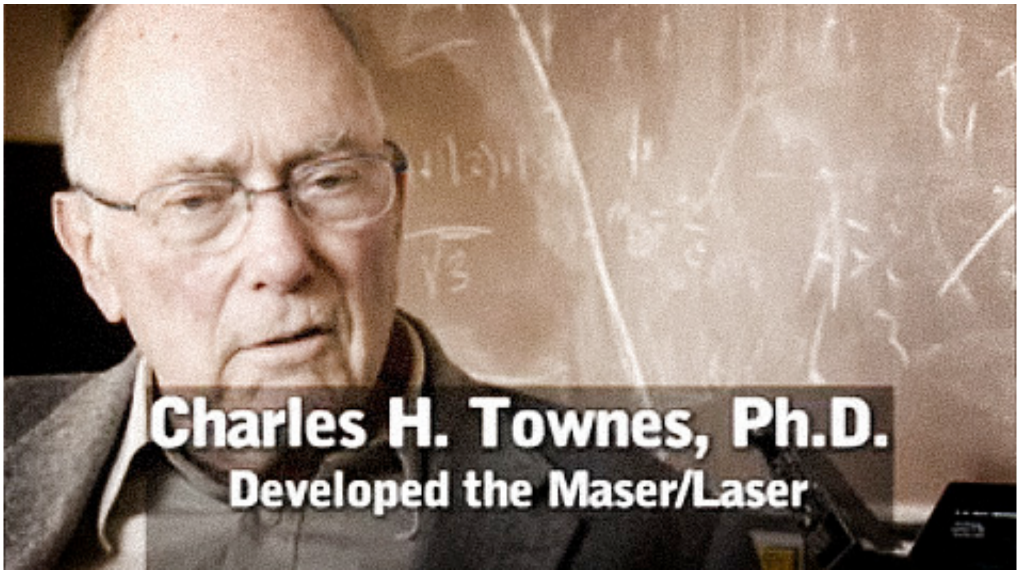
How the Laser Happened: Adventures of a Scientist



Exclusive excerpt from Nobel Laureate Charles H. Townes's book, *How the Laser Happened: Adventures of a Scientist* (published by Oxford University Press in 2002). In this book, Townes provides a highly personal look at some of the leading events in twentieth-century physics. Townes was inventor of the maser, of which the laser is one example; an originator of spectroscopy using microwaves; and a pioneer in the study of gas clouds in galaxies and around stars. Throughout his career he has also been deeply engaged with issues outside of academic research. He worked on applied research projects for Bell Labs; served on the board of directors for General Motors; and devoted extensive effort to advising the US government on science, policy, and defense.

This memoir traces his multifaceted career from its beginnings on the family farm in South Carolina. Spanning decades of ground-breaking research, the book provides a hands-on description of how working scientists and inventors get their ideas. It also gives a behind-the-scenes look at the scientific community, showing how scientists respond to new ideas and how they approach a variety of issues, from priority and patents to the social and political implications of their work. In addition, Townes touches on the sociology of science, uncovering some of the traditions and values that are invisible to an outsider.

A towering and energetic figure, Townes has explored or pioneered most of the roles available to the modern scientist. In addition to fundamental research, he was actively involved in the practical uses of the laser and in the court cases to defend the patent rights. He was a founding member of the Jasons, an influential group of scientists that independently advises the government on defense policy, and he played an active part in scientific decisions and policies from the Truman through the Reagan administration. This lively memoir, packed with first-hand accounts and historical anecdotes, is an invaluable resource for anyone interested in the history of science and an inspiring example for students considering scientific careers.

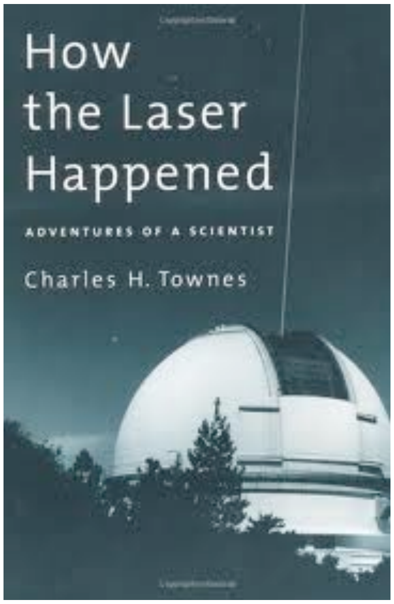
An engaging human story, intertwined with a first-hand account of some of the twentieth century's most significant inventions and discoveries. Fine reading for anyone interested in science, scientists, or the roles they play in our fast-changing world.
Arno Penzias, Nobel Laureate in Physics and former Chief Scientist of Bell Labs

In this book one of the greatest scientists of the twentieth century tells the story of his life, discoveries, and inventions, which include the maser and the laser. Charles Townes's pioneering research in microwave spectroscopy produced a wealth of new information on molecules and even on the masses of atoms and the structure of their nuclei. These studies also made it possible to discover and understand spectra of molecules in space and near astronomical objects like stars. They helped to provide compelling evidence for a giant black hole at the center of our galaxy. Townes also tells of his involvement in advising the government and of the problems of trying to give unbiased scientific advice in a political atmosphere. In all, a fascinating story of science and the people who discover it. The book is hard to put down.
Arthur Schawlow, Nobel Laureate in Physics and Emeritus Professor at Stanford University

Charles H. Townes is presently a Professor in the Graduate School at the University of California, Berkeley, and engaged in a research in astrophysics. He is known for a variety of researches involving the interaction of electromagnetic waves and matter, and also as teacher and government advisor. He secured his place in the pantheon of great 20th-century scientists through his investigations into the properties of microwaves which resulted first in the maser, a device which amplifies electromagnetic waves, and later his co-invention of the laser, which amplifies and directs light waves into parallel direct beams. His research, for which he shared the Nobel Prize in Physics in 1964, opened the door for an astonishing array of inventions and discoveries now in common use throughout the world in medicine, telecommunications, electronics, computers, and other areas. In addition to the Nobel Prize, he has received a number of awards and honours, as well as honorary degrees from twenty-five colleges and universities.

Interview with Charles H. Townes

Order your copy of *How the Laser Happened: Adventures of a Scientist* on Amazon now!



Excerpt from *How the Laser Happened: Adventures of a Scientist*

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Chapter 5: Maser Excitement—And a Time for Reflection

Before—and even after—the maser worked, our description of its performance met with disbelief from highly respected physicists, even though no new physical principles were really involved. Their objections went much deeper than those that had led Rabi and Kusch to try to kill the project in its cradle; fully familiar with oscillators and molecular beams, these two never questioned the general idea. They just thought it was impractical and that it diverted departmental resources from basic physics and more sensible work.

Llewelyn H. Thomas, a noted Columbia theorist, told me that the maser flatly could not, due to basic physics principles, provide a pure frequency with the performance I predicted. So certain was he that he more or less refused to listen to my explanations. After it did work, he just stopped talking to me. A younger physicist in the department, even after the first successful operation of the device, bet me a bottle of scotch that it was not doing what we said it would (he paid up).

Shortly after we built a second maser and showed that the frequency was indeed remarkably pure, I visited Denmark and saw Niels Bohr, the great physicist and pioneer in the development of quantum mechanics. As we were walking along the street together, he quite naturally asked what I was doing. I described the maser and its performance. “But that is not possible,” he exclaimed. I assured him it was. Similarly, at a cocktail party in Princeton, New Jersey, the Hungarian mathematician John von Neumann asked what I was working on. After I told him about the maser and the purity of its frequency, he declared, “That can’t be right!” But it was, I replied, and told him it was already demonstrated.

Such protests were not offhand opinions concerning obscure aspects of physics; they came from the marrow of these men’s bones. These were objections founded on principle—the uncertainty principle. The Heisenberg uncertainty principle is a central tenet of quantum mechanics, among the core achievements during the phenomenal burst of creativity in physics during the first half of the twentieth century. It is as vital a pillar in quantum theory as are Newton’s laws in classical physics. As its name implies, it describes the impossibility of achieving absolute knowledge of all aspects of a system’s condition. It means that there is a price to be paid if one attempts to measure or define one aspect of a specific particle or other object to very great exactness. One must pay by surrendering knowledge of, or control over, some other feature.

The most commonly encountered illustration of the uncertainty principle is the impossibility of learning both a particle’s position and its momentum to unconstrained accuracy. The scientist must sacrifice one to get the other. The problem lies in the nature of the universe, not in the shortcomings of instruments. A corollary, on which the maser’s doubters stumbled, is that one cannot measure an object’s frequency (or energy) to great accuracy in an arbitrarily short time. Measurements made over a finite time automatically impose uncertainty on the frequency.

To many physicists steeped in the uncertainty principle, the maser’s performance, at first blush, made no sense at all. Molecules spend so little time in the cavity of a maser, about one ten-thousandth of a second, that it seemed to those physicists impossible for the frequency of the radiation to also be narrowly confined. Yet that is exactly what we told them happened in the maser.

There is good reason, of course, that the uncertainty principle does not apply so simply here. The maser does not inform one about the energy or frequency of any specific, clearly identified molecule. When a molecule is stimulated to radiate (in contrast with being left to radiate spontaneously) it must produce exactly the same frequency as the stimulating radiation. In addition, the radiation in a maser oscillator represents the average of a large number of molecules working together. Each individual molecule remains anonymous, not accurately measured or tracked. The maser's precision arises from principles that mollify the apparent demands of the uncertainty principle.

Engineers, whose practical tasks up to that time almost never brought them face to face with such esoterica as the uncertainty principle, never had a hard time with the precise frequency the maser produced. They dealt all the time with oscillators and cavities, based on a wide variety of physical phenomena, which produced rather precise frequencies. They accepted as a matter of course that a maser oscillator might do what it did. What they were not so familiar with was the idea of stimulated emission, which gave the maser its amplifying power. Birth of the maser required a combination of instincts and knowledge from both engineering and physics. Physicists working in microwave and radio spectroscopy, which demanded engineering as well as physics skills, seem to have had the necessary knowledge and experience to both appreciate and understand the maser immediately. Rabi and Kusch, themselves in a similar field, for this reason accepted the basic physics readily. But for some others, it was startling.

I am not sure that I ever did convince Bohr. On that sidewalk in Denmark, he told me emphatically that if molecules zip through the maser so quickly, their emission lines must be broad. After I persisted, he said, "Oh, well, yes, maybe you are right," but my impression was that he was simply trying to be polite to a younger physicist. Von Neumann, after our first chat at that party in Princeton, wandered off and had another drink. In about 15 minutes, he was back. "Yes, you're right," he snapped. Clearly, he had seen the point. Von Neumann did seem very interested, and he asked me about the possibility of doing something like this at shorter wavelengths with semiconductors. Only later did I learn from his posthumous papers that he had already proposed—in a letter of September 19, 1953, to Edward Teller—producing a cascade of stimulated infrared radiation in semiconductors by exciting electrons, apparently with intense neutron-radiation bombardment. Along with his calculations, Von Neumann gave a summary of his idea:

> The essential fact still seems to be that one must maintain a thermodynamic disequilibrium for a time t1 which is very long compared to the e-folding time t2 of some autocatalytic process that can be voluntarily induced to accelerate the collapse of this disequilibrium. In our present case, the autocatalytic agent is light—in the near infrared, i.e., near 18000 Å [1.8 microns]. There may be much better physical embodiments than such a mechanism. I have not gone into questions of actual use, on which I do have ideas which would be practical, if the whole scheme made sense. . . .

His idea was almost a laser, but he had neither tried to use the coherent properties of stimulated emission nor thought of a reflecting cavity. There also seems to have been no reply from Teller, and the whole idea dropped from view. Later, in 1963, after the laser was well established, von Neumann's early thoughts and calculations were published; but by then von Neumann had died, and I never had an opportunity to explore with him his thoughts of 1953, about which he modestly kept quiet after we had the maser operating.

In the spring of 1954, the organizers of the Washington, D.C., meeting of the American Physical Society agreed to permit a postdeadline paper in which we described our new oscillator. Bill Nierenberg, who by then had left Columbia for Berkeley, told me later he recognized it as a very exciting development. Yet overall there was not a lot of immediate reaction. Our report was too late for the society's bulletin that described the proceedings of the meeting, so our first publication was early that summer in the Letters section of *The Physical Review*.

We started building a second maser almost immediately after the first one worked, in order to check the frequency of one against the other. We were joined by Tien Chuan Wang, a student from China with considerable engineering experience, and we had the second one operating in about 6 months. Each used the 1.25-centimeter transition in ammonia, with a frequency of about 24 billion cycles per second. Although they were essentially identical, they were not expected to have exactly the same frequencies. Slight differences in the dimensions of their resonant cavities could displace the two signals from each other by a tiny amount—by 1 part in 100 million or so. To test their constancy, we overlapped the outputs of the two masers so that they "beat" together. The signals came in and out of phase with each other at an audio frequency of a few hundred cycles per second. What resulted thus resembled, somewhat, the warble of a twin-propellered airplane, in which one engine is running just slightly faster than the other—the drone of one propeller alternately reinforcing, and then damping, the noise of the other. With our masers, the beat signal was very steady. Its pure sinusoidal form told us immediately that, indeed, both masers were operating at precise, nearly unvarying frequencies. If either of the maser's wavelength varied appreciably, the beat would have been noisy or irregular, but it was not. With data from this demonstration and other tests, we published in August 1955, a longer and more detailed paper on the maser in *The Physical Review*, which gave more complete information to other physicists on its intriguing properties.

As interest spread, we found ourselves with a steady stream of visitors. The Jet Propulsion Laboratory in Pasadena, especially keen on experimenting with this new device, sent Walter Higa to spend some time with us. We also got into a regular interaction with people at Varian Associates, Inc., near the Stanford campus in Palo Alto, who wanted to build commercial masers. By the late 1950s, after masers based on solids came along, so many papers on masers poured into *The Physical Review*, many of them speculative, that the editors declared a moratorium on maser publications! I believe this is the first and only time that journal has done such a thing. Maser research was so popular it became the butt of jokes. One making the rounds was that maser stood for "means of acquiring support for expensive research." It did help give us research support!

Even before Gordon, Zeiger, and I had gotten the first maser going, we realized that its steady, precise frequency would make it an ideal basis for an extremely accurate "atomic" clock. It was an obvious application, for the maser arrived on the scene with timekeeping already undergoing dramatic and rapid technological improvement. I had, in fact, worked with earlier types of so-called atomic clocks myself. Until that time, the best clocks, developed particularly at Bell Labs, used quartz crystals. Such crystals, however, gradually change their fundamental frequencies—in part because their mechanical vibration causes submicroscopic pieces of quartz to fly off. As a result, quartz clocks were good to only 1 part in about 100 million. This may seem impressive, but physicists wanted appreciably better accuracy than that. Several physicists had given the problem some thought. When I was still at Bell Labs, Rabi had suggested using the fixed wavelengths of radio-frequency transitions in molecular beams; and I had made an experimental "clock" based on a spectral line of ammonia.

Harold Lyons, at the U.S. National Bureau of Standards, was an electrical engineer with a good sense of basic physics and a particular enthusiasm for atomic clocks. He had enlisted my help as a consultant, and announced the first substantial and complete atomic clock in early 1949. Its accuracy was not much of an advance over the quartz-crystal clocks, but it was a move in the right direction and received a good public reaction. The radio broadcasting service of the International Communications Agency, called the *Voice of America*, as well as journalist Edward R. Murrow, and the U.S. secretary of commerce, to whom the Bureau of Standards reported, all played it up.

As the basis for a clock, the maser promised to provide the purest available frequency, at least over short periods of time—a promise it has fulfilled. As soon as the maser was working well, I let Lyons know that we had the perfect signal source. Such clocks are indeed accurate. The hydrogen maser, a later type invented by radio spectroscopist Norman Ramsey at Harvard, loses or gains only about one thirty-billionth of a second over an hour's time.

One must note that the maser is not the only good basis for such a clock. Another type of device provides a somewhat better average stability over a very long time period. That technique was developed during the 1950s by Jerrold Zacharias, an MIT physicist and former molecular beam colleague of Rabi's. His technique used a beam of cesium atoms without stimulated emission, and at present clocks of this general type provide the best long-term precision.

Historically, but somewhat inaccurately, the maser as well as cesium-atomic systems were all called atomic clocks, a term with great public appeal in the years shortly after World War II. With atomic bombs and atomic power in the news, an atomic clock seemed just the thing for keeping time. Of course the first maser, and the clocks stabilized on molecular lines, such as the one built by Lyons, are really molecular clocks. Nonetheless, the maser-based "atomic" clock, with its precision, was very satisfying to me for a deeply based reason. Very high precision physics has always appealed to me. The steady improvement in technologies that afford higher and higher precision has been a regular source of excitement and challenge during my career. In science, as in most things, whenever one looks at something more closely, new aspects almost always come into view. I could see that a clock built around a maser oscillator could be very useful; for example, in checking the precise rotational behavior of Earth or the motions of heavenly bodies. Precise timing would provide tests of relativity and its statements connecting rates of time and motion. Navigation and other practical fields would also profit from better timepieces. For the latter reasons, precise timing has been among the missions given to the National Bureau of Standards (later renamed the National Institute of Standards and Technology) and the U.S. Naval Observatory. Present-day global positioning systems (GPSs), which allow individuals with a small instrument to locate themselves within a few tens of feet anywhere on Earth or in the sky, are based on atomic timing.

My interaction and collaboration with Harold Lyons is just one illustration of the diverse, ever-surprising ways that relationships and friendships pay off in science. There is an unstructured, social aspect of science that is, I think, not sufficiently appreciated. By this I mean only that as developments and discoveries arise, scientists and their ideas are often thrown together, more or less by chance, or perhaps for reasons that at the time seem entirely utilitarian and single-purposed, which may pay important benefits in ways one could never anticipate.

The ripples of downstream consequence after Lyons' first involvement with me in 1948, shortly after I went to Columbia, were perhaps particularly chancy, but significant. In 1955, Lyons moved to the Hughes Research

Laboratory in California to set up a group to work on spectroscopy and quantum electronics (a name we later coined for maser research and technology). He took with him from the Bureau of Standards some of the people with experience in microwave spectroscopy. And while at Hughes he hired an excellent group of physicists, including Ted Maiman, a man who, as we will see, was to have one of the starring roles in the development of the laser. Maiman, in turn, had recently finished a Ph.D. in radio and microwave spectroscopy with Willis Lamb, who was then at Stanford, after leaving Columbia and the radiation lab, where we had been close associates. A web of personal connections first spun at Columbia eventually spread across the nation. In science, there is usually no cold, objective inevitability to discovery or the accumulation of knowledge, no over-arching logic that controls or determines events. There may be broad unavoidability to some discoveries, such as the maser, but not to their timing or exact sequences of progress. One has ideas, does experiments, meets people, seeks advice, calls old friends, runs into unexpected remarks, meets new people with new ideas, and in the process finds a career of shifts and often serendipitous meanders that may be rewarding and rich, but is seldom marked by guideposts glimpsed very far in advance. The development of the maser and laser, and their subsequent applications in my career and in science and technology generally, followed no script except to hew to the nature of humans groping to understand, to explore, and to create. As a striking example of how important technology applied to human interests can grow out of basic university research, the laser's development fits a general pattern. As is often the case, it was a pattern which could not possibly have been planned in advance.

What research planner, wanting a more intense light, would have started by studying molecules with microwaves? What industrialist, looking for new cutting and welding devices, or what doctor, wanting a new surgical tool as the laser has turned out to be, would have urged the study of microwave spectroscopy? The whole field of quantum electronics is almost a textbook example of broadly applicable technology growing unexpectedly out of basic research.

To return to the daily concerns of that time: our primary objective while working on the first maser was an oscillator with a high-frequency output. Not long after we had started work, I also realized that in addition to its use in spectroscopy, it would be a great amplifier. The maser can be several hundred times more sensitive than the old electronic amplifiers with which I had become so familiar while at Bell Labs. An amplifier, of course, is a device that has a small signal coming in one end, with a more powerful one coming out the other. The more sensitive it is, the weaker the starting signal may be and still come out cleanly in amplified form. Jim Gordon worked out, theoretically, many of the essential features of the maser oscillator, including its small fluctuations (approaches Art Schawlow and I later adapted to the laser). It would be a while, however, before we had a rigorous theoretical discussion of the maser's low-noise performance—that is, a precise statistical explanation of just how well it could amplify signals with little static or other clutter introduced during the amplification process.

In addition to using it for a variety of microwave spectroscopy studies in the year or two after the first maser was operational, I pondered how to extend the technology. The maser did a fine job demonstrating the principle, but as a useful tool it was severely limited. We needed masers that would work at shorter wavelengths, and also ones that could be tuned. The ammonia maser had an essentially fixed frequency, though several different ammonia-resonant frequencies might be chosen. An ideal generator for the spectroscopic study of atoms and molecules should provide signals tunable over a broad range of frequencies. One could then dial up and down through the generator's output range, probing for resonances in atoms and molecules and thus mapping their transitions and energy levels. For similar reasons the ammonia maser's value as an amplifier was limited. Many of my friends thought the ammonia maser was an interesting idea, but with such a narrow band, and no way to tune it, the thing seemed to them of little practical value other than for a clock.

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Conversations with History: Charles W. Townes

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