Making waves

How a stroll in the park led to the beginning of quantum electronics.

Charles H. Townes

Immediately after the Second World War, physicists in several laboratories began working on the microwave spectroscopy of molecules. The field grew rapidly, but was soon given up by industrial labs — they saw no useful applications for the work — and moved to universities.

Almost everyone in the field wanted to obtain shorter waves, because the wealth of molecular lines and their intensity of absorption increased rapidly as one moved from centimetre to millimetre wavelengths, and then down into submillimetre or infrared wavelengths. Harmonic generation with electronic equipment could achieve millimetre wavelengths, but not the submillimetre range. At Columbia University, New York, I worked hard for several years looking for ways to obtain shorter waves, without much success. The US Navy was also interested in short waves, and in early 1950 asked me to form a national committee to search for ways of extending radar technology into the shorter wavelength region. We visited many centres and laboratories in the United States and Europe, looking for good ideas for producing short waves. None surfaced.

I called for one more meeting of our Navy committee on 26 April, 1951, in Washington DC. I was sharing a hotel room with Arthur Schawlow, a young postdoc from my lab, who was in town for an American Physical Society meeting. I woke up early in the morning, worrying about our lack of success in generating shorter waves. I didn't want to disturb Schawlow, so I left the hotel and went out to sit in nearby Franklin Park. It was a nice morning, but I was fretting over why we had found no solution for shorter wavelength oscillators. High frequencies needed a very small oscillator — hard to build and perhaps unable to take all the power needed to make it oscillate. Of course, molecules and atoms were small enough and easily produced very short waves. But to get a respectable amount of light out, high temperatures would be needed to excite sufficient numbers of molecules. Too high in fact — the molecules would all disassociate.

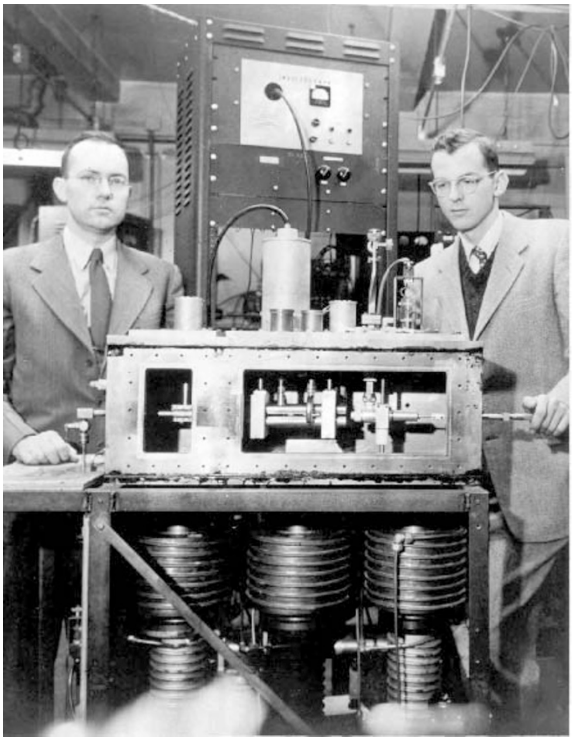
But wait a minute! This assumes that the molecules are in thermal equilibrium. What if they weren't? A collection of excited molecules in complete non-equilibrium would have no such limit to their potential radiation intensity. This was, at least, a possibility — but in practice would it produce an appreciable amount of power?

I pulled out a pen and an envelope and wrote down the necessary equations and numbers, using my favourite microwave molecule, ammonia, as an example. The most obvious way of obtaining many molecules in an excited state was to use molecular beam methods to separate those that were in the excited state from those that weren't. Of course, a resonant cavity tuned to the emission wavelength was also needed, to ensure that the emissions could be effectively captured and corralled. Great! The numbers said it could probably be made to work.

I raced back to my hotel room to tell Schawlow about the idea. He agreed, but didn't seem particularly excited. I decided to mull it over more before going public, so didn't mention it in the committee meeting.

Back at the lab, I decided to test the idea by first making an oscillator at longer centimetre wavelengths, where ammonia has intense resonances. If successful, I could then push into the submillimetre or infrared region. I began to look for a student who might give it a try and before long Jim Gordon, an outstanding student, turned up. With postdoc Herbert Zeiger we were set to start.

Two years later, Gordon and Zeiger had still not obtained oscillation. Polycarp Kusch, the departmental chairman at that time, and Isidor Isaac Rabi, his predecessor, came into my office and sat down. "Look Charlie," they said. "That experiment is not going to work. We are in the molecular beam business and know it won't work. You know it won't work. You must stop, you are wasting the department's money." Rabi and Kusch were, of course, outstanding scientists — both received Nobel prizes. But as an associate professor, I had tenure. A department chairman could not fire me simply because of a disagreement or my incompetence. "No," I said. "I think the experiment will work and I am going to continue."


Charles Townes (left) and Jim Gordon with a beam-type maser.

Two months later, Gordon dashed into the classroom where I was lecturing and excitedly declared: "It's working." The whole class left with me to go to the lab to witness the demonstration. We named this new kind of oscillator a MASER, for microwave amplification by stimulated emission of radiation. My students later suggested laser for light emission, and iraser for infrared emission, although the latter was never used.

Most scientists at that time didn't believe the maser idea could be extended to such short wavelengths, because of the much higher decay rate of excited atoms or molecules as the wavelength became shorter. But I was sure we could go into the submillimetre or infrared range, and probably even to visible light. By the fall of 1957 I had figured out just how an optical maser (laser) could be built.

On a consulting visit to Bell Labs, I ran into Schawlow again and I told him about my ideas for an optical maser. I planned to optically excite atomic gas, have it radiate by stimulated emission, and use a cavity as a resonator. I wasn't completely happy with the cavity, as it would probably have multiple mode oscillations. Schawlow said, "Oh, I've also been wondering if that could be done," and suggested using two parallel mirrors, a Fabry–Perot, as a resonator. After a short delay while Bell Labs fixed up an appropriate patent, we published our ideas in 1958. There was immediate excitement. The maser had convinced industry that this was a valuable field, and very quickly there were many efforts to build a laser. The field of quantum electronics had begun. ■

Charles H. Townes is in the Department of Physics, University of California, Berkeley, California 94720, USA.