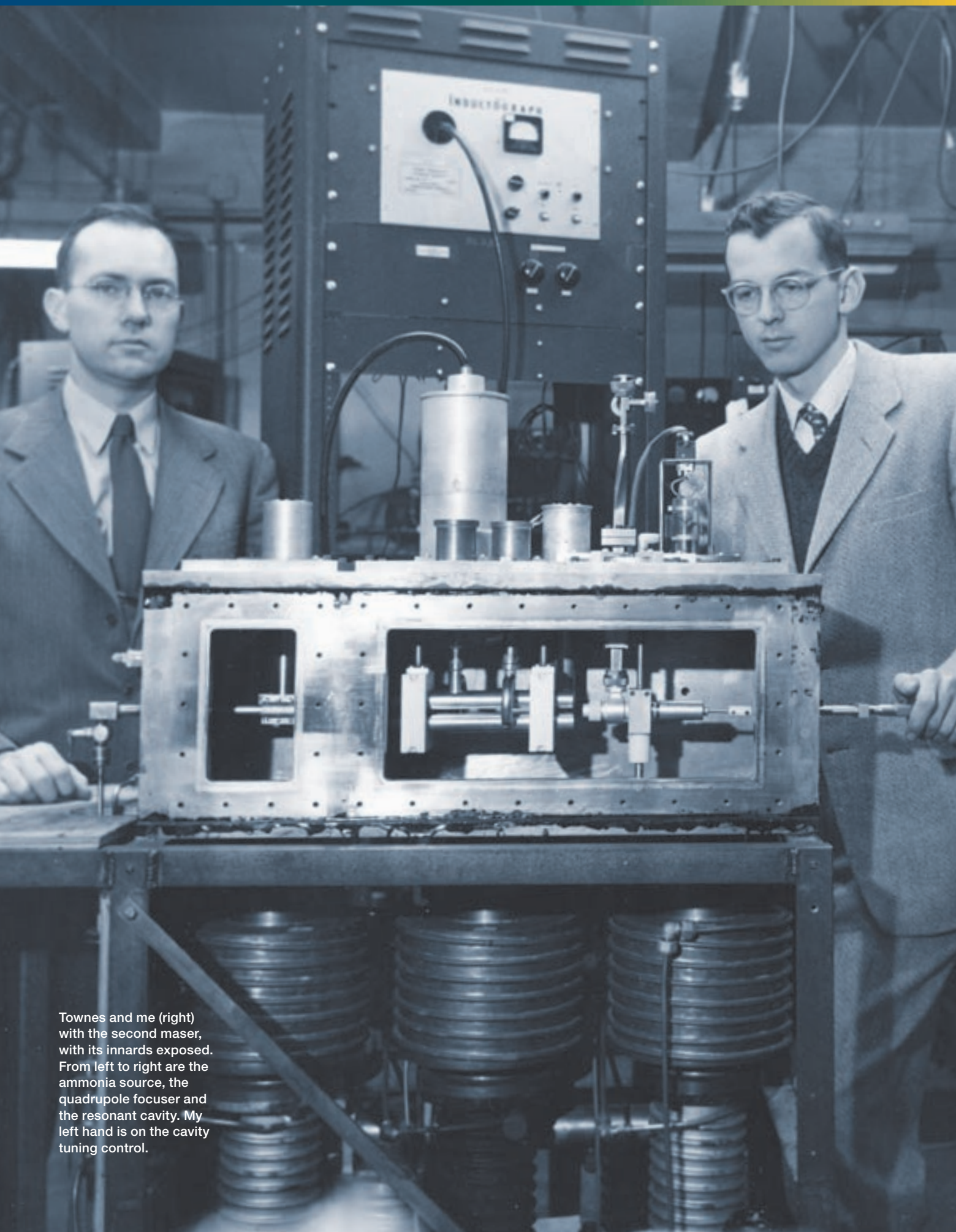
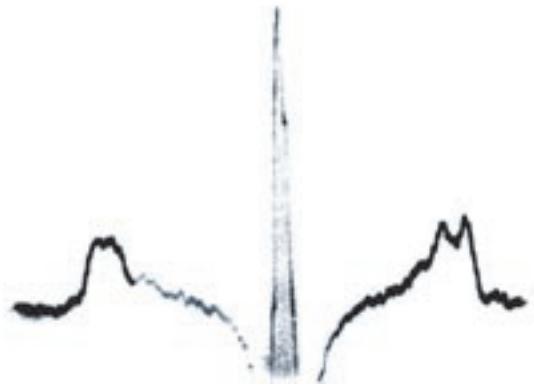


Townes and me (right) with the second maser, with its innards exposed. From left to right are the ammonia source, the quadrupole focuser and the resonant cavity. My left hand is on the cavity tuning control.



Reflections on the First Maser

James P. Gordon

Before the laser, there was the maser, and, before that, an idea: to build a microwave amplifier using ammonia molecules. Here, Jim Gordon takes us back to the early 1950s, when he had to decide whether Charles Townes's vision for creating a coherent oscillator was promising enough for him to commit to for his Ph.D. project.

In April 1954, when five of us were having lunch in the Columbia teacher's college cafeteria, Charles Townes proposed that we name the coherent oscillator that we had just created. He vetoed any name that ended in "-tron." Before we left, we had created the name maser, an acronym for "microwave amplification by stimulated emission of radiation." Before long, Arthur Schawlow had re-imagined the maser acronym to mean "money acquisition schemes for expensive research." Back then, none of us could have imagined how critical the maser would be in shaping optical technology in the 20th century and beyond. These are my recollections of how the maser came to be.

How it all began

Sometime in mid-1951, I got a call from Professor Townes asking if I would care to join him on a project he had in mind—to build a coherent molecular oscillator. I had come to the Columbia Graduate School of Arts and Sciences after graduating from MIT in 1949 with a B.S. in physics. (I had also applied for graduate studies at MIT, but I was not

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Our calculations had shown a bare chance that we would achieve an oscillator, but there was no safety factor. If it didn't work, then what?

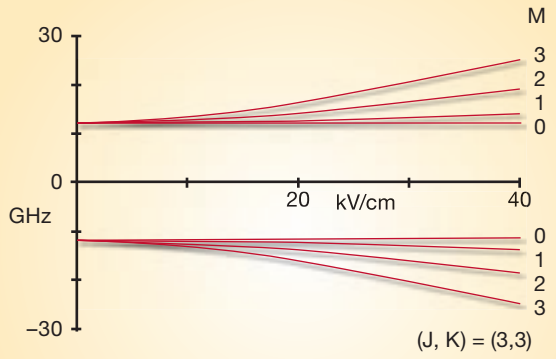
Laying the groundwork for the maser

The maser was not created from scratch; there were antecedents to our work. The first was Einstein (who seems to be everywhere in fundamental physics); in 1917, he studied the conditions for the equilibrium of energy and momentum transfers between radiation and atomic or molecular systems. He didn't much like quantum mechanics; nevertheless, he identified the processes of absorption, stimulated emission and spontaneous emission and the relationship among them. He also found that the emission of energy quanta (photons) needed to be completely directional— apparently quite unlike the classical picture.

In 1924, Richard Tolman discussed the possibility of negative absorption (amplification) by molecules. Then, in 1939, the Russian V.A. Fabricant conceived of eliciting amplification from an excited gas. His experiments were not successful, however, and no one followed up on them. Willis Lamb and Robert Retherford later resurrected the idea of negative absorption in their 1950 paper on the Lamb shift. Also in 1950, Edward Purcell and Robert Pound invented the term "negative temperature" to describe quantum systems with inverted populations with transitions within a finite frequency range.

In 1953, Joseph Weber at the University of Maryland discussed a scheme for obtaining coherent microwave amplification from ammonia gas—but it was clearly impractical. In addition, his work was not exactly an antecedent, since our research on what would become the maser started in 1951.

[The Stark effect in ammonia]



The rotational angular momentum of the molecule is denoted by the quantum number J. The projection of J on the molecular axis is labeled K, and the projection of J on some laboratory axis, as provided, for example, by an electric field, is labeled M.

accepted there—fortunately, as it turned out). The Columbia physics department at that time comprised an excellent group of people.

In my first two years, I had an initial course in atomic physics from Isidor Rabi; a class on quantum mechanics from Willis Lamb; and a seminar in advanced quantum mechanics from Hideki Yukawa, who had come to Columbia in 1949. Among my memories was the first session with Lamb, where he wrote the wave function symbol ψ on the blackboard along with the comment: "Don't worry about what this means, you'll get used to it!" Another is of Yukawa, whose English was poor; he mumbled towards the blackboard as he wrote in tiny symbols. He was not the best teacher, but of course he was a great theorist. Townes and Polykarp Kusch were also members of the group.

In Townes's office, I met with Herbert Zeiger and George Dousmanis, who had already been thinking about the project. Herb was a post-doc, having earned his Ph.D. at Columbia working with molecular beams, and George was a student from Greece who was then doing calculations of beam trajectories in the electrostatic focuser. This device was called a focuser because at that time we thought that its focusing properties would be important to the design of the apparatus.

If I decided to join Townes's project, it would have to turn out something new to provide me with a Ph.D. thesis. I recall having an important meeting in Townes's office during which we decided to move forward. Our calculations had shown a bare chance that we would achieve an oscillator, but there was no safety factor. If it didn't work, then what?

As I remember, I pointed out that the relatively long cavity resonator that we had designed to increase the interaction of the molecules with the field would also cause a tenfold increase in the resolution of the ammonia spectrum. Townes immediately pointed out that there was as-yet-unseen hyperfine structure in the spectrum to be found that had been previously hidden by Doppler broadening. Thus, happily, we went ahead.

The first time the project was written up in some detail was in the December 1951 quarterly report of the Columbia Radiation Lab—a report that was required by the Joint Services Command in return for its financial support. As Townes recounts in his 1999 book *How the Laser Happened*, these reports were not official publications; rather, they were generously distributed to whoever asked for them. Whether they influenced what came later will never be known, but for sure most initial reactions were ho-hum. For example, Townes had arranged for Herb and me to visit Prof. Malcom Strandberg at MIT, who had had some acquaintance with ammonia beams. He listened to what we were trying to do, gave us some advice which I don't remember, and wished us well.

Getting to know ammonia

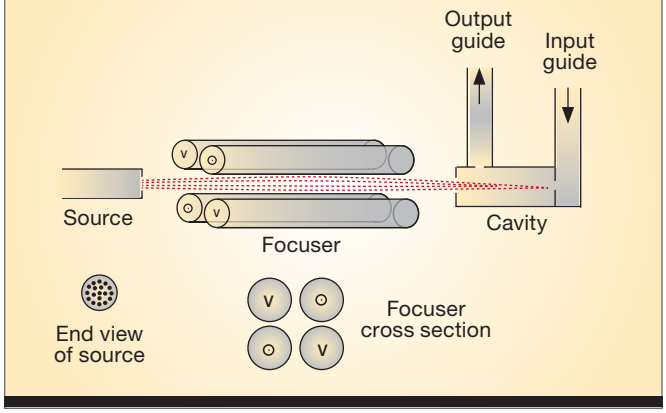
That year I learned about the microwave spectrum of the ammonia molecule—an equilateral triangle of three hydrogen atoms with a nitrogen atom off to one side. It has a hindered vibration wherein the nitrogen atom tunnels through a small potential barrier in the plane of the hydrogen atoms and comes out the other side. The resulting spectrum is called the inversion spectrum, and it occurs in the microwave region. Of course, since the nitrogen is heavier, the hydrogens do most of the moving. The molecule has various rotational states that modify the inversion frequency.

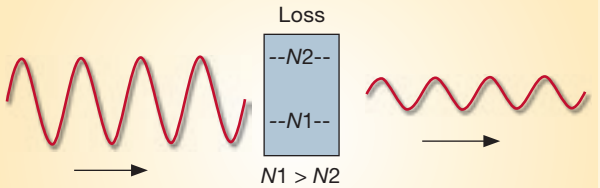
The rotational angular momentum of the molecule is denoted by the quantum number J. The projection of J on the molecular axis is labeled K, and the projection of J on some laboratory axis, as provided, for example, by an electric field, is labeled M. If K=J, the rotation is mainly around the molecular axis; then the hydrogen atoms are pulled apart, and the inversion frequency is increased. Conversely, if K=0, the rotation is mostly perpendicular to the molecular axis, and the inversion frequency is lowered. The inversion line we settled on was the J=K=3 line, the strongest one at room temperature.

In the presence of an electric field, the energy levels of the J=K=3 inversion transition are split. This splitting is called the Stark effect.

The figure below shows the basic design of the first working maser. The maser had three main elements. On the left is the ammonia beam source. Ammonia from a room temperature tank was allowed to effuse out of a source consisting of an array of fine tubes, which at the appropriate pressure should result in a beam of molecules more or less directed at the focuser. The focuser consisted of four cylinders held in place by a Teflon structure.

[Design of the first working maser]





Field picture of attenuation: The loss is proportional to *N*1-*N*2. How can that be?

Why the maser worked

One of the reasons I became convinced that our experiment had realistic chance of succeeding is illustrated in the figure above. It is a field picture of the familiar process of attenuation. A wave impinges on a lossy medium. Molecules resonant at the frequency of the incident wave have level populations, *N*1 and *N*2, respectively, in the lower- and upper-energy states of the transition. Usually *N*1 >*N*2. The wave comes out of the lossy medium diminished in amplitude. But the loss is proportional to *N*1 – *N*2. This can only be true if the processes of loss and gain are competing coherent processes (where the oscillations of the many molecules are correlated in phase with the field).

Loss is provided by the *N*1 molecules in the lower state. The incoming wave induces in these molecules a dipole moment oscillating at the wave frequency in quadrature with the incoming wave, in the phase that absorbs energy from the field, thus increasing the energy of the molecules. These dipoles in turn emit a forward-going wave that destructively interferes with the outgoing wave, thereby reducing its amplitude. Gain is provided by the *N*2 molecules in the upper state. Since the net loss is proportional to *N*1 – *N*2, it is clear what these molecules must do, and indeed what they actually do.

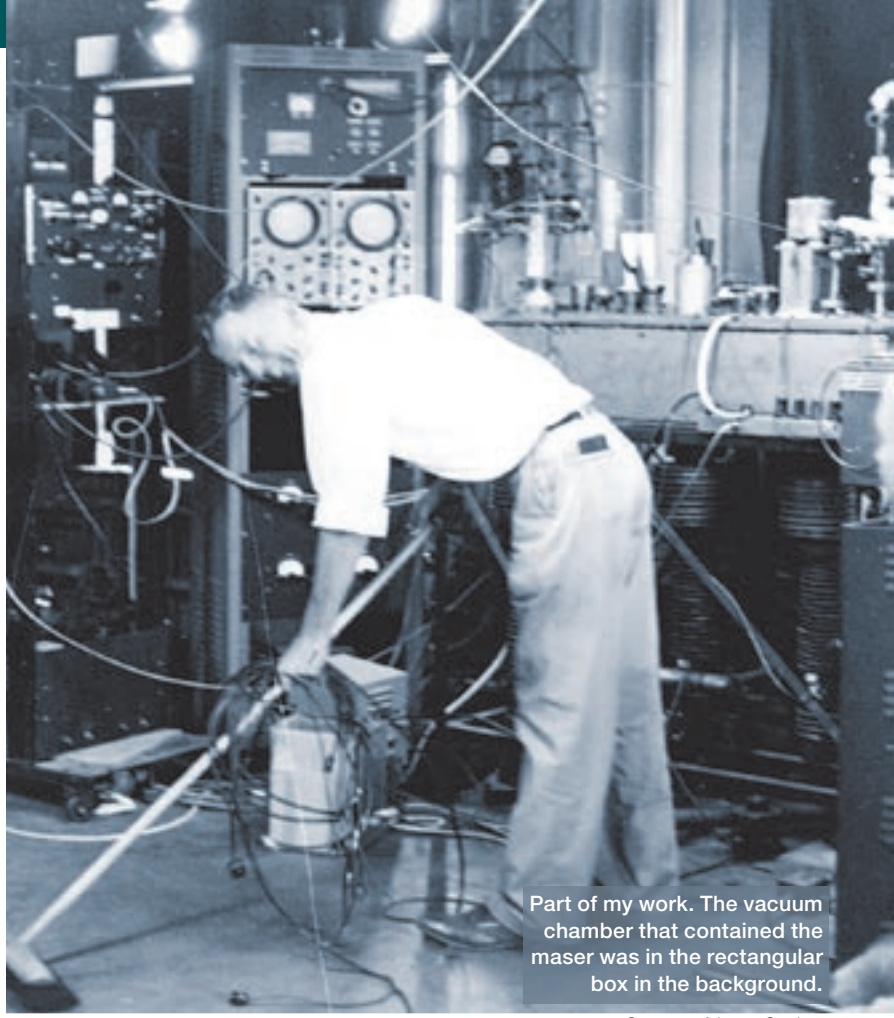
The incident wave causes in these molecules a dipole moment that oscillates at the wave frequency in quadrature with the incoming wave, in the phase that emits energy into the field, thus reducing the energy of the molecules. These dipoles in turn emit a forward-going wave that constructively interferes with the outgoing wave, thus increasing its amplitude.

It was pretty obvious to me that this picture was not confined to plane wave fields, but that it would work with any other field configuration, such as the field of a cavity resonator. Thus, induced emission must be the inverse of absorption as well as a coherent process. Microwaves, like other forms of energy, have an annoying habit of sometimes acting like waves and other times like particles, depending on what you look for.

At the end of 1952, George Dousmanis had left the project, and Herb Zeiger's post-doc had come to the end. He departed in early 1953 for Lincoln Laboratory. The oscillator was then my baby.

We designed it to give a quadrupole field, zero on the axis, increasing quadratically with distance from the axis. This would produce a sinusoidal trajectory for the molecules. We thought that the ammonia beam could be focused to our advantage. Upper-state molecules are traveling uphill as they depart from the zero-field axis, and so are forced back toward the axis, while the lower-state molecules go downhill and are lost.

On the right is the cavity resonator. It was a cylindrical cavity, with inside dimensions of about 1.5 cm in diameter by about 11 cm in length. The resonator mode we aimed for and used had only one-half wavelength of the microwave field in the direction of the beam. Since the free space wavelength of the microwave field was 1.25 cm, this was a win-win situation. It increased the time for the molecules to interact with the field, and it also narrowed the molecular resonances by a factor of about ten.



Part of my work. The vacuum chamber that contained the maser was in the rectangular box in the background.

Courtesy of James Gordon

Working out the kinks

At the end of 1952, George Dousmanis had left the project, and Herb Zeiger's post-doc had come to the end. He departed in early 1953 for Lincoln Laboratory. The oscillator was then my baby. The design was pretty much set except for one thing. Since we were counting on the focuser to actually do some focusing, the ammonia beam source consisted of an annular ring of small tubes, and the entrance to the cavity was a corresponding annular ring. The cavity could be moved around and the voltage on the focuser could be varied to find the best result.

The vacuum chamber that contained the maser was a rectangular box, which you can see just behind my back in the photo on the right. It was bolted together, using gaskets to provide the vacuum seal. That was not the best design, but it was what our shop could make. It had many leaks, and we were forever sealing them with a black wax called glyptal. We even had names for the leaks. There was the necktie leak, where your tie got sucked into the box. Another was the dirty sock leak, where a bloodhound stationed at the vacuum pump outlet detected the odor of a dirty sock when you waved it near the leak. What we actually used was a helium leak detector, which was akin to the dirty sock type.

Since ammonia accumulated in our apparatus while we were using it, at night we would warm it up and pump it out. The experiment was located in a rather large room in which several other microwave spectroscopy experiments were being run by others in Townes's group. One was a high-temperature spectrometer, run by Malcolm Stitch and Arnold Honig, wherein a waveguide was hung vertically to prevent it from sagging. They recorded many mysterious spectral lines, which were eventually attributed to ammonia.

An incident happened on the way to the maser. The tubes that formed the focuser were filled with liquid nitrogen to capture stray ammonia molecules and to keep the pressure in the vacuum chamber low enough. At the point where the liquid nitrogen was poured in, there was an O-ring seal that had to be heated to keep it from leaking. We put some heater wire around it, and we connected it to a variac (a variable transformer). The mistake was that we connected it wrongly, so that the heater wire was more or less at line voltage. One day my left forearm touched the heater wire. Both hands clamped on pieces of equipment they were near, and I had this feeling of being shaken (60 Hz current will do that). The good news is that my brain still worked. My hands were stuck, but I was able to move my arm away from the contact point after a second or two. I still have a scar on my left arm from that incident. One must be lucky to survive the mistakes of one's youth.

Nay-saying from authority figures is common, in physics and elsewhere, and that hasn't changed much over the years. The farther out of the mainstream a proposal is, the more often it is resisted by the powers-that-be.

One evening in December 1953, my fellow grad student Hobart Ellis and I obtained a faint emission spectrum. There was the sought-after narrow resonance line of the ammonia spectrum and its never-before-seen nearby hyperfine satellites.

Making it happen

It took almost all of 1953 to finish putting the experiment together. Toward the end of that year, several interesting things happened. One was that Profs. Kusch and Rabi, then the physics department head and elder statesman, respectively (and experts in molecular beam studies), came into Townes's office to dissuade him from pursuing the experiment. They said something along the lines of: "We all know it won't work. Why don't you just stop?" The other interesting thing was that we got the first indications that it actually would work—at least well enough to give me a thesis.

Nay-saying from authority figures such as Rabi and Kusch is common, in physics and elsewhere, and that hasn't changed much over the years. The farther out of the mainstream a proposal is, the more often it is resisted by the powers-that-be. This is understandable; after all, most such proposals do not work. Look at Einstein: He got his Nobel Prize for the photoelectric effect, but surely his greatest achievement was the theory of relativity—which was too far out of the mainstream for recognition. When Zeiger left for Lincoln Lab, Kusch upbraided him for wasting his two-year post-doc on this "harebrained scheme."

Llewelyn Thomas was a theoretical physicist at Columbia. He insisted to Townes that the maser could not emit a pure frequency based on Heisenberg's uncertainty principle. After the first maser had shown oscillation (significant power output with no input signal), Michael Danos, a young physicist in the department, bet me a bottle of bourbon that it would not give a pure frequency. I found out later that he had also bet Townes a bottle of scotch. He paid up on both wagers.

Even after the second maser had been constructed and we had observed an audio frequency beat note between the relatively pure frequencies of the two masers oscillators, other scientists objected. Bohr commented to Townes that what we had done was not possible. As Townes relates, at a meeting with Von Neumann at a cocktail party in Princeton, Von Neumann's first reaction was skeptical.

One evening in December 1953, my fellow grad student Hobart Ellis and I obtained a faint emission spectrum, as shown in the top figure on the right. There was the sought-after narrow resonance line of the ammonia spectrum and its never-before-seen nearby hyperfine satellites.

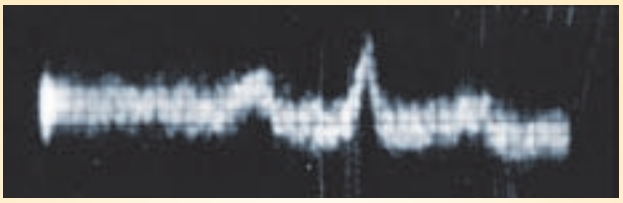
Looking for an even better result, I moved the cavity resonator around and varied the voltage on the focuser. There was no evidence of a distinct optimum, or anything close to oscillation. It became clear that, while the focuser was separating out the upper state molecules, it was not doing any appreciable focusing of those molecules back into the annular ring shape of the source.

Based on these observations, the next step came pretty naturally. I replaced the annular ring source for the ammonia beam with a two-dimensional array of small slots and opened the entryway to the cavity resonator. Because of the geometry of the resonator, this did not significantly increase its losses. The result was a large increase in the strength of the emission spectrum—enough to produce the hoped-for microwave oscillator. When this finally occurred, I poked my head into the room where Townes was presiding over the Friday morning microwave seminar and announced success. The group went back to the lab to witness it.

The bottom figure shows the same spectrum we had captured earlier, just below the beam strength that was necessary for oscillation. It is a Polaroid picture of an oscilloscope trace. The cavity resonator was tuned so that the ammonia emission

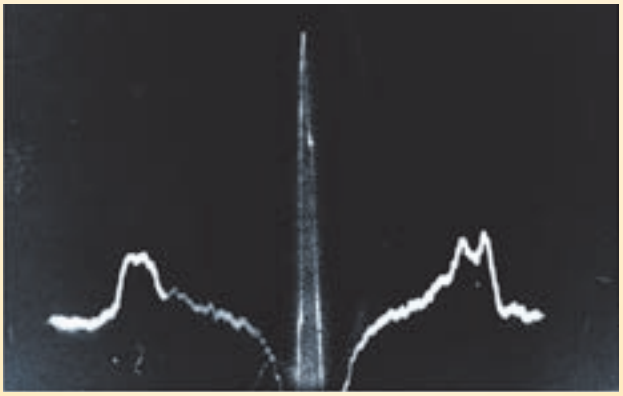
[First emission spectrum of the J=K=3 transition]

James Gordon



The spectrum shows the sought-after resonance line of the ammonia spectrum and its never-before-seen nearby hyperfine satellites.

James Gordon



Same ammonia spectrum near oscillation strength.

Listening to the maser. This is my favorite photo of that time. Charlie Townes is surrounded by T.C. Wang, a Columbia post doc, Koichi Shimoda from Tokyo University in Japan, Walter Higa from the Jet Propulsion lab, and James Lotspeich, a Columbia grad student. They are listening intently to the relatively pure audio beat note at about 100 Hz produced by combining the outputs of the two masers.

James Gordon

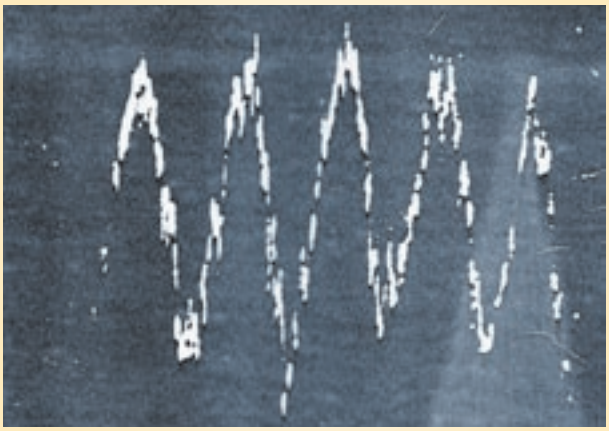
lines occurred at the peak of its response. The sharp central peak is barely visible in the original due to the speed of the trace. It has been enhanced, which accounts for the snow around it in the image. The dips at the sides of the central peak are the result of the molecular dispersion, which effectively detunes the resonant frequency of the cavity.

At that point it was pretty clear to me that oscillation would have to occur at the observed peak of the main emission line.

The uncertainty principle was not then on my mind. However, while the uncertainty principle dictated the 7-kHz spectral width of the resonance lines and prohibited me from knowing the resonance frequency of the ammonia molecule much better than that, it did not prevent highly coherent oscillation. The uncertainty principle is saved because the oscillation frequency can be altered by varying the central frequency of the cavity resonator. Thus, one cannot assess the fundamental frequency of the molecular transition much better than by carefully observing the shape of the resonance line well below the oscillation level.

After we observed the oscillation of what would become the maser, we submitted a short 1.5-page paper early in May of 1954 to the letters section of the *Physical Review*. It was titled "Molecular Microwave Oscillator and New Hyperfine Structure in the Microwave Spectrum of NH_3," by J.P. Gordon, H.J .Zeiger and C.H. Townes. It was published in the July 1 issue. That spring, I gave a postdeadline talk about our work at the New York meeting of American Physical Society, which was then held at Columbia University. My parents, who

[Beat note at about 100 Hz]



James Gordon

My parents listened to my postdeadline talk about the maser at the American Physical Society meeting. They came away pleased that I was showing some signs of becoming an adult.

The oscilloscope trace of the beat note was proof that the maser oscillator's output was very nearly a pure frequency.

lived in the New York suburb of Scarsdale, came and listened. They came away pleased that I was showing some signs of becoming an adult.

The second maser and afterwards

As we learned later, the Russians Nickolai Basov and Alexander Prokhorov independently and almost simultaneously published a paper in 1954 proposing a molecular beam method of creating a microwave oscillator using alkali halide molecules and a cavity resonator. Their scheme was closer to ours than Weber's had been, but it was still impractical. The first maser they made was an ammonia maser; it was essentially a copy of ours.

Professor Townes (I knew him for a long time by that name) immediately proposed building a second ammonia maser to measure the bandwidth of the oscillation, as there was no better way to do it. It took us only six months to make the second maser (see photo on p. 34).

We listened intently to the relatively pure audio beat note at about 100 Hz produced by combining the outputs of the two masers. The oscilloscope trace of the beat note was proof that the maser oscillator's output was very nearly a pure frequency, thus confounding some of the experts. The oscillation frequency of each maser could be varied by making small changes in the frequency of its resonant cavity.

We had demonstrated the first practical microwave amplifier using neutral particles (here ammonia molecules), and we showed that it behaved much like other electronic amplifiers. With the positive feedback provided by the cavity resonator, it could yield a coherent oscillation whose fluctuations were mainly due to the noise associated with spontaneous emission from the upper-state particles.

In the fall of 1954, I took some time off for a trip to France, along with the same grad student friend (he was one of my roommates) Hobart Ellis, who had witnessed the first early emission spectrum. On a very pleasant trip over on the Queen Elizabeth, we met a couple of pleasant French girls who offered to show us Paris. Of course we accepted. I rather foolishly carried my unfinished thesis around with me, hoping that I would have the chance to work on it. (I didn't.) Then, in January 1955, I started work at Bell Labs, with a promise that I could continue on with my thesis work for awhile there.

In 1955, we published two papers in the August 15 *Physical Review*. They were submitted in May. These papers became my Ph.D. thesis. (I somehow avoided writing a separate dissertation.) One was a spectroscopy paper, entitled "Hyperfine Spectra in the Inversion Spectrum of $N^{14}H_3$ by a New High-Resolution Microwave Spectrometer;" which was authored by me alone, and the other introduced the maser with the title "The Maser—New Type of Microwave Amplifier, Frequency Standard, and Spectrometer," by me, Zeiger and Townes.

The former described and analyzed the spectra that could have provided me with a thesis, even if we didn't reach the beam strength needed to make an oscillator. Needless to say, it didn't light the world on fire. The second discussed the properties of the maser, low noise amplification, stable oscillation, and high resolution as a spectrometer. Thus began the field of quantum electronics.

OSA Member **James Gordon** (jamespgordon@verizon.net) is a retired scientist from Alcatel-Lucent (Bell) Labs.

Some of the participants in the first quantum electronics conference, held at the Shawanga Lodge in the Catskills in September 1959. From left to right: Gordon, Basov, Zeiger, Prokhorov and Townes.

Courtesy of James Gordon