Reminiscences and Memories of Charles H. Townes

Mentor, Advisor, Colleague, and Friend

Assembled by Paul Goldsmith
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My memories of Prof. Charles H. Townes
Fred M. Johnson

Ph.D., Columbia University, 1958

He was multitalented, a master of life, a role model, a superb scientist and above all, an exemplary human being.
Allow me to elaborate a little on a few of his talents, which were perhaps not so obvious to all of his many students: He could perform a decent job of glassblowing. I discovered this when in 1954 some of my glass assembly broke and needed repair, while I was engaged with my research on a public holiday. There was no-one present from the Columbia University Radiation Lab. employees –however, Prof. Townes was in his office working and was kind enough to fix the problem for me. I was truly amazed.
He had a fine baritone voice and appreciated opera and classical music.
Prof. Townes was in his office (or lab) from early morning to about 10:30 pm, 6 days per week. He clearly kept track on the progress of each of his Ph.D. students. If additional experts were required, he arranged for them to visit or obtain a visiting scientist appointment. Examples were Dr. Bleaney from England and Art Schawlow (from Bell Laboratories), as well as G. Herzberg from Canada.
He was both a good theorist and superb experimentalist. His knowledge of e-m theory obtained at Cal Tech and subsequently honed while at Bell Laboratories, enabled him to apply for (and obtain) an academic position at Columbia University in 1947. Prof. Rabi was no doubt impressed by his new faculty member. With an abundance of leftover microwave equipment from World War II and the availability of an excellent machine shop, spectroscopy of molecular gases could be pursued with gusto. The Radiation Lab on the 10th and 11th floors of the Columbia University Pupin Building was a busy bee hive of activity, where a host of eager Ph.D. candidates carried out their research. There were also post-docs present, including Arthur Nethercott, Akira Okaya and others whose names I do not recall.
C.H. Townes had excellent judgment regarding important decisions. Perhaps his most crucial decision was his choice of soul mate – Frances. This truly proved to be a superb choice on both their parts. His successes in life, I believe, owe a great deal to their joint partnership endeavors. Frances was a key partner to her husband for 74 years. I vividly recall her speech during Charlie's elaborate 90th birthday dinner. Both of them were gracious hosts at the year-end Christmas parties in their home in New York (1952-1957 were the ones I was invited to attend).
Unfortunately, time clocks are running for all of us. Nevertheless, I hope that there will be many others of my Columbia colleagues who will likewise contribute their memories of our mentor Prof. C.H. Townes.

Ammonia, Lots of Water and Ruby Masers

Sigfrid Yngvesson

Post-Doc with Charles Townes, University of California Berkeley, 1968-1970

After finishing my Tekn. Dr. at Chalmers University in June 1968 I wrote to Carson Jeffries in the UCB Physics Dept. to ask if he knew about jobs near Berkeley. Carson talked to Charles Townes who, luckily for me, just needed someone to develop a K-band (18-26.5 GHz) low noise receiver. That was a pretty high frequency for masers in those days. Townes was in the process of verifying his hypothesis that "there must be many more molecules in the interstellar medium", and his "old friend" from the 1950s maser work, ammonia, was the first target. As I arrived in Berkeley, ammonia lines were just being dug out of the noise with a not very sensitive receiver at the Hat Creek observatory (still the best receiver available). A maser promised at least an order of magnitude increase in receiver sensitivity. I set out to develop a maser amplifier using ruby, and Townes and EE professor Jack Welch gave me lots of resources and help from several graduate students. We ground thin slivers of ruby and deposited periodic metal structures lithographically on them, a first for maser technology. Students would spend days (and nights) winding superconducting magnets with 2,000 turns by hand! I remember group meetings when other Townes group members were quite skeptical about what we were doing, but also how Townes would speak up and explain that our ideas were good and would work out in the end. He always had time for all of us! One special problem to solve was how to keep the maser from oscillating by incorporating a ferrite rod near the ruby rod. Once we did that the magnetic field was distorted to the extent that there was no chance of achieving any gain in the amplifier. The solution was to add an extra ferrite rod that was not resonant with the ruby resonance. To the few people working in millimeter wave masers, that ceramic concoction became known as the "Swedish sandwich".

Thanks to the skill and hard work of the students in my group, Albert Cheung, S.Y. Wang (EE), Mike Chui and EE undergraduate Butch Cardiasmenos (the last two now deceased) the maser was completed and used on telescopes at Hat Creek, Naval Research Laboratory and Haystack, the latter after I had accepted a faculty position at UMass/Amherst. The years in the 1960s through the early 1970s were the golden year of maser receivers. Observatories were willing to put up with the inconvenience of mounting a large liquid helium dewar near the telescope focus (somehow) in order to be competitive. The Post-Doc period in the Townes laboratory was invaluable for launching my research career in the US and that is something that I have always been very grateful for.

And, oh yes, lots of water. I am sure Al Cheung will also write about this: The whole group was at a Christmas party at the Townes' when there was a phone call from Al Cheung who was spending the entire holiday diligently watching the telescope at Hat Creek. Frances Townes took the call and Al told her: "Mrs. Townes, it is raining

in Orion". The detection of water in Orion was so strong because the water molecules were masing, and the frequency fortuitously was in the band of the ruby maser. Interstellar water sources are still used as ideal point sources today, detected using VLBI (no maser needed, though!).

Memories of Charles Townes

Michael Werner

Postdoc, University of California Berkeley, 1969-1972

Although I was only figuratively, but not literally, one of Charlie's students, I had the great good fortune to be brought to Berkeley by him as a post-doctoral fellow [I think I was called a Visiting Lecturer in Physics] from 1969 to 1972. In addition to the tremendous energy and continual thought and wisdom that Charlie put into the scientific development of students and post-docs, I have very strong recollections of his deep concern for the well-being of his younger colleagues. In May of 1970 when the Ohio National Guard killed four unarmed students on the campus of Kent State University, he called us together in the familiar library on the 5th floor of Birge Hall. He wisely anticipated that the sight of people our age being massacred in Ohio might raise anxieties within us about what might happen in Berkeley. I have no specific recollection of the attitudes and issues that surfaced during the following discussion, but I am certain that we all left the room feeling less concerned than we had been about our safety and security.

Working with Charlie Townes

Neal Evans

Ph.D., University of California Berkeley, 1973

My first encounter with Charlie happened in the fall of my senior year at UC, 1967. Having become somewhat disillusioned with high energy physics, I was looking for other options, and someone had told me that Charlie had come to Berkeley to start an astrophysics group. I went to visit Charlie to ask about astrophysics and ask for suggestions for a class project. I was probably too naïve to be impressed that a person of his stature took the time to talk to a random undergrad who wandered into his office, but I realized later that this was characteristic of Charlie. He suggested a project on quasars, based on a book by Geoff Burbidge, which I followed. The project on quasars aroused my curiosity, and I went back to see him while I was deciding whether to take my NSF Graduate fellowship to Caltech or to stay at Berkeley. Charlie offered me the chance to start right away on research the summer before graduate classes, along with an RA position to supplement my fellowship. That, plus my now strong Berkeley bias, sealed the deal.

Along with courses, I continued my summer research into the fall, working on a question Charlie had raised: could certain non-linear behaviors seen in laboratory lasers occur in the astrophysical OH masers, which had recently been discovered. I ground through a lot of equations and studied a lot of lab laser papers. We decided it was worth looking for such effects, which would be manifested by a change in the amplitude statistics of the radiation from a Gaussian distribution. (A pure sine wave would have a bimodal, rather than a Gaussian, amplitude distribution.) I don't remember how, but I teamed up with a graduate student in astronomy named Richard Hills, a future telescope guru. With Charlie's connections, we worked out a deal to take an instrumentation tape recorder (analog in those days) to the Onsala Space Observatory, which had the lowest noise receivers at the frequencies of the OH masers. Getting the tape recorder (a large, heavy instrument borrowed from Dave Cudaback) to Sweden proved a challenge. After contacting a shipping company, I learned that I would need an export license. When I finally got through to someone in the Commerce Department, I asked why I needed this license. With weary patience, he explained "well, Mr. Evans, we wouldn't want this technology to fall into the hands of our enemies." It was 1969, Viet Nam was heating up, and Sweden was viewed as dubious at best. The irony of this statement will become apparent shortly.

I arrived in Sweden in December, testing my California wardrobe. The charismatic director, Olaf Rydbeck, greeted me and turned me over to Erik Kollberg and Bert Hansson, who took me in. While we waited for the recorder, now stuck in a strike by cargo handlers in London, they took me into their homes and helped me buy a fur hat, which I still have. The recorder did arrive, and we set everything up and recorded directly the maser signals. I checked everything I could think of in real time, packed up the recorder, visited an acquaintance in Copenhagen, and headed

home. Later, when Richard and I fed the signal into a special –purpose computer in the Electrical Engineering building, we discovered huge departures from Gaussianity, but they were not what we expected. Fortunately, we decided to do a control experiment, feeding Gaussian noise into the tape recorder. The same anomalies showed up! We eventually tracked the problem to internal interference from a clock signal. This "high technology" should only have fallen into the "hands of our enemies"! Thus I learned the importance of control experiments, preferably BEFORE you spend a lot of money on a wild-goose chase. I was so chagrined that I went to Charlie and offered to resign my RA. Of course, he kindly explained that these things happen in science and not to worry. Charlie's kindness in this situation is probably the most important thing I learned from him. We did eventually get a clean result from the NRAO 140-foot, assisted by Sandy Weinreb; the statistics were resolutely Gaussian.

While I was fooling around with OH masers and signal processing, my office-mate, Al Cheung had discovered interstellar ammonia, and he was looking for a highly unlikely line of water while the rest of us were at a Christmas party at the Townes's. When he called in to the party to tell us that he had a strong signal, it stole the show even from the fabled (and deadly) Townes eggnog. We figured that was about it for interstellar molecules until the detection of formaldehyde by Snyder, Zuckerman et al. hit the news (I still have a yellowed clipping of the news article, which connected this molecule of death to the origin of life, planting another seed for a lifelong interest). We were all astonished that a molecule with TWO heavy atoms could exist in space. More astonishing news followed. Toward regions with no background radio sources, the 6 cm line of formaldehyde remained in absorption. Had Penzias and Wilson not discovered the CMB a few years before, we would have had to invent it to explain this result. However, to absorb the CMB, the excitation temperature of the 6 cm transition had to be cooled below the temperature of the CMB, which seemed impossible. Charlie of course, knew from maser work that excitation temperature could be anything, even negative, if there is a pump. The 6 cm line arises in a K-type doublet of the ground rotational state of ortho-formaldehyde, and Patrick Thaddeus proposed that it was pumped by transitions to the next rotational doublet, which occurred around 2 mm wavelength, near the peak of the CMB. He showed that a slight deviation from a blackbody in the CMB would pump the 6 cm line into a super-cooled state. He dubbed this the "Cosmic Refrigerator." We playfully decided that this process needed an acronym, so DASAR for Darkness Amplification by Stimulated Absorption of Radiation amused us for a while, but never caught on.

At any rate, the stakes were now high. Formaldehyde could be probing deviations from a black-body in the CMB, a very hot topic. Charlie was suspicious of this audacious suggestion by his former student, and he and Al Cheung quickly published a paper showing that collisions could also cool the 6 cm transition. The only problem was that theirs was a classical calculation, highly suspect for quantum numbers of 1 and 2. Charlie suggested that Al and I work on a quantum calculation. We quickly showed that the Born approximation gave the opposite result from the

classical calculation. Pat Thaddeus showed that the Sudden Approximation also gave results opposite to the classical calculation. Charlie, with the characteristic intuitive feel for physics of a Nobelist, responded that we just had to do a better quantum calculation. This became the major project of my graduate years. We consulted Ken Watson, author of the classic book, Collision Theory, and he suggested that we turn the differential equations into algebraic equations using the Kohn variational principle. This turned out not to be much help, as the resulting matrix was ill-conditioned and hard to solve. The calculation required that many matrix elements be computed, and I went on the night shift, doing trigonometric integrals for hours on end until Al mentioned that he thought that there were these things called 3-J symbols that might be useful. This sent me into a long digression into the quantum mechanics of rotating tops. The topic was very confusing, but I finally figured out which textbooks were reliable about which part of it (none were exactly consistent all the way through), and made a look-up table of 3-J symbols. I then punched up computer cards and fed them into an IBM 1620 that occupied the basement of Birge Hall. It was so slow, you could watch it thinking, but the result came back, still opposite to the classical one. Charlie was unmoved; we just needed to include more levels, even virtual transitions to what I learned to call "closed channels". The matrix grew, and I needed a faster computer. I eventually wound up with two boxes of cards going into the fastest computer at LBL, the CDC 7600. I kept cranking up the level of the interaction (modeled in a very primitive way by modern standards), running longer and longer until, hallelujah, I saw the ratio of collision rates bend and then switch to the classical result. Finally, my massive quantum machinery had confirmed Charlie's intuition, and I could quit. The lesson here is obvious: Charlie's physical intuition could beat a lot of CPU cycles!

While working on the formaldehyde problem, I spent a week trying to understand some fine point about its hyperfine structure. When Charlie stopped by my office, while making his rounds, as he typically did, I told him about my puzzle, and he said something that sounded reasonable. At that point, we were interrupted by someone, I think Larry Greenberg, with a question about his lab work. During the hiatus, I tried to understand what Charlie had said. As soon as he finished with Larry, he turned to me and said, "What I said before was wrong," and he went on to explain the matter correctly. The neuroscientists tell us that we really can't "parallel process," but they never tested Charlie!

Dr. Townes – Some Memories and Thoughts

Tom Geballe

Ph.D., University of California Berkeley, 1974

I was fortunate to join the Townes Group in early 1969 in Berkeley. I had little to recommend myself, apart from being a graduate student in physics: virtually no experience in research or in the laboratory up to then. Like quite a few of us in his group, I had not taken any astronomy classes. What skills or experience could I bring to the group? It didn't matter. Dr. Townes, as we always called him, made me feel welcome and within a few months asked me to join graduate student Jim Holtz and post-doc Dave Rank, who were attempting with him to open a new observing mode for astronomy: high-resolution mid-infrared spectroscopy.

Back then I had little idea of the bright future of infrared astronomical spectroscopy, which Dr. Townes saw so clearly. But even without that perception there was for me the excitement of learning this way of observing the universe, understanding how to make our spectrograph work and how to determine its performance, observing on what was then the second largest optical telescope in the world, and learning a bit of astronomy as we proceeded. There also was the more sobering experience of obtaining an understanding of the signal-to-noise ratio, which was enhanced by the countless discussions we had with Dr. Townes about the significance, or lack thereof, of little "wiggles" in the spectra that we were obtaining.

Dr. Townes didn't exert his authority over us or pressure us; he treated us as equals. When in Berkeley he was always available for meetings. In my subsequent relationships with scientists and non-scientists, and especially with students, I have always tried to emulate his approach. And he was always open to suggestions, even radical ones. A key moment in our project came when, after several unsuccessful attempts in 1969 to detect emission lines in planetary nebulae, Jim and Dave concluded that an infrared Fabry-Perot spectrometer, which we would have to build ourselves over a period of many months, would be superior to the Fourier transform spectrograph which Dr. Townes had, probably with considerable trouble, obtained on loan from a colleague in Colorado and which we had been using until then at the telescope. Although it would have been easy for him to say "no," after careful consideration he approved of this change of course, which turned out to be the right decision.

That change led to Jim's successful PhD thesis, my thesis, and those of several grad students who followed us. The scientific results we obtained can each be achieved now in seconds or less, but at that time often took one or more all-night marathons, in which Dr. Townes frequently joined us at the telescope on Mt. Hamilton. I recollect several occasions of dawn arriving and we bleary-eyed grad students feeling more than ready to shut down both our equipment and ourselves, when Dr. Townes would brightly "suggest" that we observe ammonia (his favorite molecule)

in Jupiter. Of course a suggestion from him was not to be refused by any of us, and so we observed ammonia in Jupiter, often until 10 a.m. I did not and still do not understand how he managed to sleep so little and remain so alert.

Carole and I have treasured our warm friendship with Charlie and Frances, which continued and grew in the decades after I left Berkeley. I am deeply grateful for the opportunity Dr. Townes gave me, for his constant generosity, for the environment he created for all of our group, and for his support and encouragement after I left Berkeley.

Memories of Charles Townes and the Townes Group

Bob McLaren

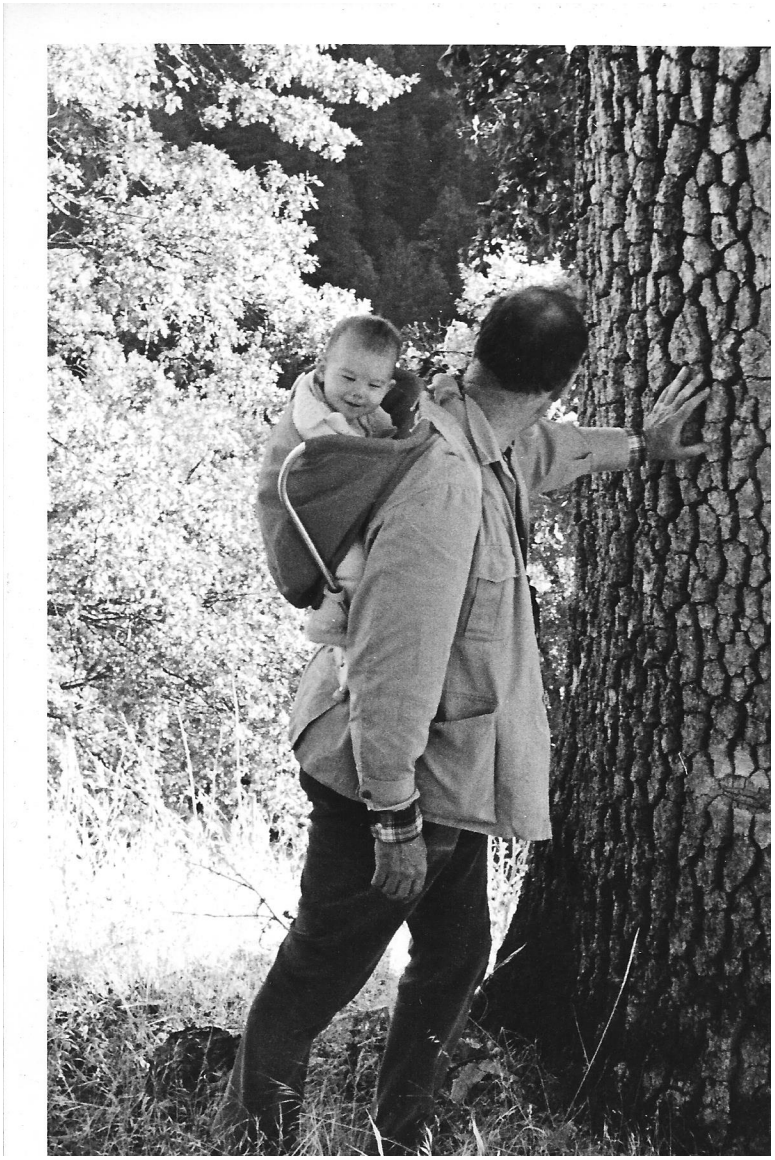
NATO Postdoctoral Fellow 1973-75
University of California, Berkeley

After completing my doctorate in laser spectroscopy at Toronto, I wanted to do something different, and preferably not in a basement lab. My thesis advisor, Boris Stoicheff had worked with Townes during a sabbatical and was aware that he had recently relocated to Berkeley and was applying laser techniques to astronomy. We contacted Charlie, and with my laser experience and a NATO Fellowship, he warmly invited me to join his group. And so it was that in November 1973 Marion and I packed up the two-seater Fiat and drove to Berkeley.

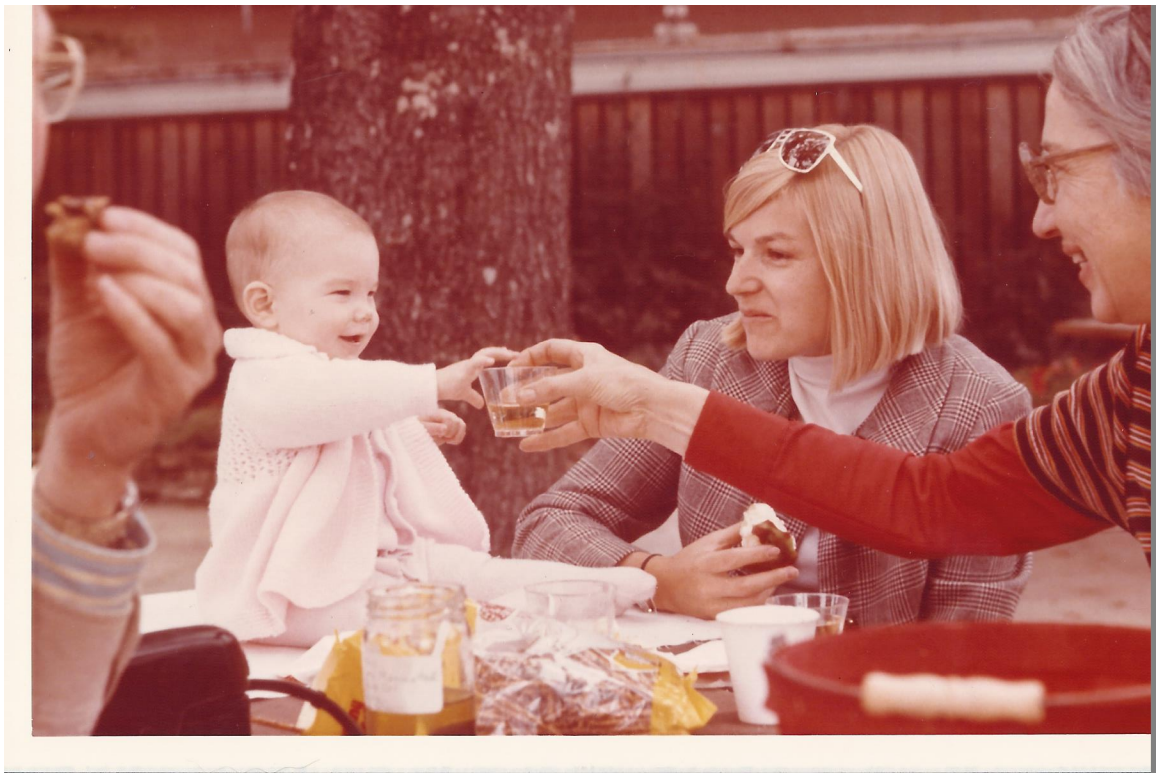
I worked first with Don Brandshaft on airborne far-IR spectroscopy using a piezo-scanned Fabry-Perot and bolometer detector system that Don had designed and built. The instrument was attached to a 12-inch telescope in the NASA Lear Jet based at the NASA-Ames Research Center. Some might consider it an exercise in futility trying to detect a faint celestial spectrum using a detector that is famous for its sensitivity to microphonics and located only about three feet from a jet engine. And they would be close to correct, but we did manage to detect the far-IR dust continuum in Orion, and we set an upper limit on the strength of the O III line at 88 microns.

My second project was with Al Betz on 10-micron heterodyne spectroscopy, using a CO2 laser as the local oscillator. We observed a wide variety of objects at the McMath Solar Telescope at Kitt Peak in a collaboration that continued for several years after I returned to Toronto to take a faculty position. We measured wind speeds on Venus and discovered non-thermal CO2 emission in the atmospheres of both Mars and Venus. Later we used the same technique to obtain resolved absorption line profiles of ammonia in several OH/IR supergiants.
Working with Charles Townes leaves an indelible impression. First there was his insatiable curiosity, his keen physical insight and his boundless energy. When everyone else was exhausted from a night's observing and wanting only to head for bed, Charlie was eager to spread out the spectra on the table and find the sought-after feature. Then there was his gentle nature. I never heard him say a harsh word to anyone in the group. The strongest admonition I recall is: "Why don't you think about that some more, and we'll talk about it again next week." Both Charlie and Frances extended exceptional hospitality to all members of the group. I particularly remember a trip to their cabin in Napa, recorded in the attached photos. It was November 1974 and our daughter Catherine was just eight months old. We stopped for lunch on the way and later went hiking with Charlie carrying Cathy in the backpack.

I was fortunate indeed to have spent a formative two years in Berkeley with the Townes Group. In my subsequent career I have tried to emulate Charlie's approach to doing science and to recreate the exhilarating collegial atmosphere that existed in his group. It has made me a better scientist and, more importantly for me, a better mentor of scientists. For this I am most grateful to Charlie and to that wonderful group of people who were the Townes Group.



Catherine McLaren (age 8 months) enjoying a hike near the Townes cabin in November 1974.



Lunch stop on the way to the Townes cabin in Napa. L-R: Charlie (only his hand and glasses visible), Catherine McLaren (age 8 months), Marion McLaren, Frances. November 1974.

Early and Recent Inspiration from Charlie Townes

Paul F. Goldsmith

Ph.D., University of California Berkeley, 1975

Two of my clearest memories of Charlie Townes almost completely bracket the years during which we interacted. The first of these, at which point I would certainly have been saying "Professor Townes", was when I first came to talk to him about a research project. He said that spectroscopy at millimeter wavelengths and in the mid infrared region were both very underdeveloped, would be very rewarding to work in, and would develop rapidly. He told me to think about things and come back in a week to talk further. I did some reading on 10 μm wavelength photoconductive mixers, which was very interesting stuff, but what really got me excited was to read about carbon monoxide, detected the year before (in 1969), which was defining a whole new class of objects in the Milky Way – Giant Molecular Clouds - in which new stars formed. Only the lowest rotational transition of CO had been observed at that time and it seemed to me that the higher rotational transitions would have a lot of additional information – if only one could build a receiver to work at (then) high frequencies above 200 GHz. When I went back to talk to Professor Townes I told him of my interest in carbon monoxide and high frequency observations. I remember him saying that this would be an excellent project, but that along with building an experimental apparatus I should start some modeling to the predict intensities of the different rotational CO lines.

So, in about three minutes Charlie laid out for me the whole structure of my dissertation work and a good part of the other research I've done during the intervening years! He added that he was sure that some other people would be working on the higher frequency CO lines (and he was right). The implication was that I had to move fast, but I also wanted to follow up on his suggestion about studying the collisional excitation of CO. I was aided immensely by the collaborative spirit of the Townes group – although I was a "newbie", Neal Evans (a relative veteran) was ready to spend time explaining subtleties of collisional excitation, and Al Cheung (so senior he seemed beyond being a graduate student) introduced me to a critical Fortran subroutine LNEQF, for inverting matrices and solving the rate equations of multilevel systems. With that help, and some struggle, I did write a paper on collisional excitation of carbon monoxide. I brought the first draft as a dual-author paper to Charlie. He made some very useful suggestions requiring a lot more work, but a few weeks later when I brought him the second draft, he asked to have his name removed because, he said "You have done all the work." While not the case, it illustrates Charlie's concern for making sure that a graduate student's work was not taken as the advisor's (irrespective of author order). I've tried to live up to that high standard with my graduate students, and it was just one of the ways in which I think Charlie influenced all those who worked with him.

A couple of years later, I finally had a setup together that seemed to have a prayer of detecting the second rotational transition of CO at 230 GHz frequency. This illustrated another of Charlie's qualities – his faith in his students and what they could accomplish. In retrospect it seems ludicrous to expect a student (or even a team of two since I had been joined by Dick Plambeck at that point) to put together essentially all of the equipment of a radio observatory and make it work – receiver, spectrometer, and data acquisition/recording. Dick did a lot on the spectrometer and making a balky multichannel analyzer read out the channels from a filterbank loaned to us by NRAO. The harmonic mixer at the input to the system was my baby, and this worked in a useful manner only thanks to another aspect of working with Charlie, namely his network of ex-graduate students who occupied important positions in the astronomical community. Arno Penzias was at Bell Telephone Laboratories, and in his laboratory a group was making small-area Schottky diodes for communications and for radio astronomy. Undoubtedly through Charlie's suggestion and Arno's encouragement, Gerry Wrixon sent me a vial with some BTL "chips" in it. I nervously soldered one onto a mounting post, and put it in the harmonic mixer. Instead of poking with a pointed whisker around the surface of a piece of silicon from a 1N23 diode, the whisker immediately contacted a premade diode – the IV curve was immensely sharper than anything I had ever seen with a point contact whisker diode. When I turned on the whole system I was elated that the noise temperature was something like 20,000 K compared to 250,000 K that I had had previously. There was hope to detect something and to get a PhD!

Another former Townes graduate student who helped was Pat Thaddeus, who on a visit told me "Don't let Charlie be cheap about your klystron." What this led to was a request to buy a klystron at 115 GHz to do second harmonic mixing rather than third harmonic mixing with the ~75 GHz klystron I had. But the higher frequency klystron cost over $5000 (big money then) and Charlie was reluctant. But he had a good suggestion – he knew that Professor W.J. Welch in the Department of Astronomy was working on high frequency receivers for the interferometer at Hat Creek, and might be able to help. I went and talked with Jack and also David Williams and Tap Lum, who were indeed building a system for 115 GHz and had the exact klystron I could use. As they were still some ways from having an operating receiver, they agreed to let me use their klystron for my project – a very generous offer since its lifetime was measured in 100's of hours! With it, the system worked even better and Dick and I, accompanied by Professor Ray Chiao (as the required faculty member) went to Lick Observatory and used the 120-inch telescope there to observe the 2-1 lines of CO and ^{13}CO. That is real interdepartmental collaboration!

Decades later I visited Berkeley to give a seminar in which I talked about work I had done on observing narrow absorption features in spectra of atomic hydrogen at 21cm wavelength. My theory was that these were due to residual atomic hydrogen in cold, essentially molecular clouds. The clincher was that the HI absorption line properties matched up beautifully with the narrow emission features that we could see in ^{13}CO and C^{18}O. If true, this was important because the amount of cold atomic hydrogen could be used to determine the age of the molecular clouds. After the talk,

Charlie told me “Paul, I think you really have explained those absorptions.” I never felt prouder than at that moment about anything I have done.

CHT reminiscences

Howard Smith

Ph.D., University of California Berkeley, 1976

I am deluged with positive memories and emotions when I think back on my interactions with Prof. Townes over the years. I thought they would be most interesting coming as spontaneous reminiscences.

I may be the longest-time student of Prof. Townes, spanning the twelve years from 1964 when I was an undergrad at MIT through the completion of my Berkeley PhD in 1976. I first met him in the spring of 1964 when, as an undergrad at MIT, I wanted a summer internship that could lead to a senior thesis. Elsa Garmire and Ray Chiao were his two finishing graduate students, and I was naturally assigned to work with them in the dark and sequestered depths of the MIT Spectroscopy Lab where the ruby laser system was set up to study Raman and Brillouin scattering; Sam Krinsky was a co-student. That work ended up producing my first paper, as a marginal co-author, on Light Trapping in Filaments. Townes was pretty busy being Provost back then, and then dealing with his Nobel Prize, so he did not come to the Lab every day, but I remember times when he did come, and I remember when Elsa told me that they had figured out why the laser beams in liquids resulted in those mysterious, bright little pinpoints on the Polaroid film: the nonlinear index of refraction induced by the laser pulses had confined the beams via totally internal reflection. Townes may have been the one who told me (and if he wasn't, he could have been): Remember, they said Optics in the 20th century was a dead end.

From those early days (to me) I also remember the excitement when the Prize was announced that fall. I remember being at the farm in the summer, and Prof. Townes driving a tractor, skillfully riding a rambunctious horse, and leading a Bible study session Sunday morning. I found the latter to be particularly interesting because I am pretty traditional in my Jewish observance and come from a Jewish Bible study tradition that takes some alternate approaches. Prof. Townes was always amazingly respectful and sensitive to my Jewish practices; I was still an undergrad, and only later did I come to appreciate what I now describe above as "amazing," having encountered since then many smart and successful scientists who lacked his personal skills and social sensitivity. In fact, I recall the comment of one scientist at that same Bible study session: "Prof. Townes, you surely don't believe THAT?!"

By the next summer he announced his move to Berkeley and invited Mike Johnson and me to join him, along with Ray Chiao of course, and Paula Dalapis, his administrative assistant. Berkeley was a wild adventure for everyone, not least about the science.

I recall the time one prospective graduate student brought a beer to group lunch in the group library – and Prof. Townes (who sometimes would treat us to pizza and beer dinner in a northside establishment) gently but forcefully ruled out that option. I also vividly recall a dramatic moment at another group lunch in the library when a student with some psychological problems began to talk in meaningless and rather obscene phrases; perhaps he was on drugs – it was somewhat scary. Prof. Townes was amazingly skillful in calming him down and getting things back on track. Needless to say, like all of Prof. Townes' students, I learned much from him, and I try to pass on to my own students some of these lessons. And not just science – though science was the primary lesson, of course, but that was not an especially unique gift from a thesis advisor. There was more: From him, I and his other students learned as well how to interact effectively with students and colleagues. I mention it here because this episode reminds me that just as I try to be good scientist like Prof. Townes, though my abilities fall far below his, so too although I try to be a good advisor and colleague, but in situations like this one I likewise find myself at a loss to match his skill and insights.

Here is a situation in which perhaps I can be more often successful in following Prof. Townes' example: answering a stupid question. At the Friday seminars, it occasionally happened that someone asked what at least I thought was an obvious or even dumb question. Lord knows I cringe at the many dumb questions I asked – and still ask! Prof. Townes had the amazing ability to turn every dumb question including mine into a deeply insightful one (one surely not in the mind of the questioner), and then to proceed to answer THAT question. The questioner was neither insulted nor left feeling ridiculous, but rather feeling smarter than s/he was. Prof. Townes had wonderful insight to appreciate the situation, amazing sensitivity and respect for others , and not least, the quick mastery to reshape the question into something profound – and then to answer it! He did this all the time.

I also try hard to be as optimistic as Prof. Townes was. The data looks like junk? Well, maybe there is something really in there to be found. Lab equipment refusing to behave? Well, let's just see, probably we can fix it. Your theory does not seem to agree with conventional wisdom? Well, maybe too soon to tell. Sitting in the bowels of Lick in 1973 observing the galactic center ("Well, maybe these clouds will blow away later tonight") with John L. and/or maybe Eric W., the last thing I believed really existed was a so-called black hole. Well, said Prof. Townes, why not? I recall at one Physics Department seminar Prof. Townes asked the speaker at the end: Well now, suppose that the speed of light is not constant?

In later years I ended up on the Board of the Templeton Foundation, an organization that funds "big ideas" and science and religion. Prof. Townes had recently won its "Templeton Prize," a distinguished and generous award that recognized his life-long contributions to probing the harmonies between these topics. I saw him frequently at the meetings, and I remember when, at age 92, he attended a Board Meeting in China and joined the excursion to climb along the Great Wall. I thought I would be

discretely helpful by walking just behind him on these steep and often treacherous steps -- but in the end I had to work hard just to keep up with him (as usual). Ever gracious and interested in many things, Prof. Townes read my book on science and religion -- and then wrote a glowing recommendation of it for me to use. During the past decade, I have written many OpEd pieces for newspapers and magazines. I was regularly flabbergasted to get a short note from him complimenting me on it, whether it appeared in the SF Chronicle, American Scientist, or elsewhere.

Other, random Prof. Townes moments:

I remember waiting at the Birge elevator with my bicycle to ride to the 5th floor, as Prof. Townes instead of taking the elevator, ran up the flights of stairs – not uncommonly singing something.

CHT driving up to Hat Creek on a sunny day with the windshield wipers in his car still going from yesterday's rain.

CHT: "How fast do I need to go to make that red light look green?"

CHT: "Would you and Larry G. mind staying in the lab a bit late tonight – I agreed to see a visitor later who might be a bit of a problem (at Columbia his secretary was shot and killed by an unstable visitor). Prof. Townes regularly made time to talk to people. He always had time for his students and gave them priority.

CHT, around 3AM on the Birge 5th floor: "Howard, what are you doing working here at this hour? Me: Prof Townes, I'm writing my thesis! - what are YOU doing here? CHT: Well now, I thought I'd come in and top off the dewars.

Of course Prof. Townes was fabulously brilliant, but what I want to remember here are those qualities of his that were not just gifts of birth, but which he cultivated and then transmitted to his students: optimism, hard work, perceptiveness, and respect for every person.

May memories of him be for a blessing.

My Memories of Charlie Townes

John Storey

Postdoc, University of California Berkeley, 1976- 1980

I spent four wonderful years (1976 – 1980) in Berkeley as a postdoc in the Townes group.

I arrived in Berkeley with my first wife Helen as a very immature 23 year-old. Far from my home in Australia, Townes and Frances became my "Berkeley parents" and helped me to develop both professionally and personally in ways I could not have imagined. They graciously had us stay us in their house until we found accommodation, often invited us to dinner parties and other functions, and were unfailing in their concern for and interest in us.

We were overwhelmed with their kindness. We were guests in their holiday house in Northern California and were encouraged to take our own friends there whenever we wished. We stayed as guests at the farm in New England. When Townes learned that I was interested in cars he insisted on my borrowing each and every one he acquired – sometimes for long periods when he was away on international visits.

Townes was the embodiment of a gentleman. Although the term is unfashionable these days, I understand it to mean gentle, kind, courteous, thoughtful, wise and patient. I never heard him raise his voice, or speak sarcastically to anyone. His sheer enjoyment of life was infectious, while the breadth of his knowledge and interests was simply astounding. Perhaps the most negative thing I ever heard him say was when he returned Dan Watson's LP record of the Sex Pistols, which he had borrowed so he could listen to it overnight. "It is not to my taste", he stated.

Next to my own father, Townes was the most influential man on my life. In a curious twist of fate my own father died just six days after Townes. He was four months younger than Townes; they both came so very close to celebrating their 100th birthdays.

I will miss them both.

Remembering Professor Townes

Eric Wollman

Ph.D., University of California Berkeley, 1976

When in 1971 Professor Townes accepted me into his group at Berkeley, he suggested that I join the infrared Fabry-Perot interferometer sub-group then composed of Tom Geballe, Dave Rank, and Jim Holtz. The interferometer was already working, and I realized pretty quickly that I was in a very good situation. The instrument advanced extrasolar spectral resolution in the 5 and 10 micron windows by about two orders of magnitude. Over the next several years, amazing experiences and opportunities came thick and fast: exploring new astronomical territory, improving the interferometer, using world-class observatories, attending and speaking at meetings, meeting prominent scientists from around the globe, and learning all the while. The world of astronomy and astrophysics, with which I had no prior experience, opened before me. Thanks to Professor Townes and my Fabry-Perot colleagues – including John Lacy, who joined the group a few years after me – it was an exhilarating time.

One can imagine relishing such circumstances while suffering difficult personal interactions. But not in Professor Townes' group! Always thoughtful and considerate, Professor Townes created an environment in which working was a joy and sympathy was the reaction to mistakes, even to spectacular blunders. On a run at the Lick 120-inch, I incorrectly loaded the magnetic tape onto the data recorder. Nothing was recorded. Worse yet, that run was to collect data specifically of interest to Howard Smith, who was not a regular member of the IR Fabry-Perot team. I don't remember exactly what Professor Townes said when I gave him the news. But I will never forget that it was compassionate – something along the lines of "These things happen." Howard was equally gracious.

A master of physical insight, Professor Townes demonstrated over and over again how to think effectively about physical phenomena. Ability to do so is a source of great satisfaction; and insofar as I have developed that ability, Professor Townes did much to help me along. I am so grateful for all that he taught me and did for me.

Some Reminiscences About my Interactions with Charles Townes

Robert W. Boyd

Ph.D., University of California Berkeley, 1977

I want to concentrate on one aspect of my interactions with Prof. Townes; this event occurred long after I left graduate school. After leaving Berkeley in 1977 I immediately took a faculty position at the University of Rochester. Then, in 2010, I took a position at the University of Ottawa where I hold the position of Canada Excellence Research Chair in Quantum Nonlinear Optics. One of my tasks there has been to set up a new research center devoted to the field of Photonics. The idea came to me that one way to promote our new research center was to establish an annual symposium series to celebrate the field of Photonics and advances in the field. After discussion with various school officials, we decided to name the series the Schawlow-Townes symposium series. The inaugural event of the Schawlow-Townes Symposium took place in November 2012, and Prof. Townes attended in person to present a plenary talk. Needless to say, Prof. Townes left the audience spellbound.

I would like to add one brief story related to the establishment of the Schawlow-Townes Symposium. I suspected that Prof. Townes would raise objection to having a symposium series named after him. I thus initially told him that the series was to be named the Schawlow Symposium, in honor of his brother-in-law, Art Schawlow. Moreover, Art was Canadian, so the ruse even made sense. I know that Charlie would come to speak at a symposium in honor of Art Schawlow. It was only long after Charlie had promised to attend that I told him of the complete name of the symposium series.

I have collected a few photographs that were taken during the first Schawlow-Townes Symposium. You can see them on the following pages.

Photo 1. Robert Boyd at the lectern introducing Prof. Townes.

Photo 2. Professor Townes delivering his plenary address.

Photo 3. Prof. Townes, Robert Boyd, and Walt Fitelson listening to a symposium speaker.

Photo 4. Professor Townes, Robert Boyd, and some of Robert Boyd's graduate students and post docs.

My Serendipity with Professor Townes

Demetrios Matsakis

Ph.D., University of California Berkeley, 1978

It seems all the good things in my life have just been due to chance. One of the best occurred when the time came to find a thesis advisor. I was taking the sole astrophysics class then, and was impressed when Prof. Cummings casually mentioned there was a Professor Townes who was inventing all kinds of interesting equipment. So I made an appointment to see him. When I went to his office, I almost walked out because I felt his secretary Wilma Keenlyside was very unfriendly to me. Fortunately I stayed (and just as fortunately I quickly learned that my first impressions of her were entirely incorrect). In the interview, Charlie asked if I had any hobbies. I told him it was astronomy. He smiled, so I guess I said the right thing.

Charlie quickly set me up with Al Cheung and Mike Chui, and it wasn't long before I was at Hat Creek helping them use the ruby maser they had spent six years developing. Nothing worked quite right that time, and it took hours to tune the thing, but I was entranced to be actually doing science on the Orion Nebula! They gave me a book to read about masers, and I was surprised to read that Townes had invented it. I asked Al if it was our Townes, and he said "of course, that's why he got the Nobel Prize". Oh. Had I known this, I might have been intimidated from ever going to him – even now it is hard to refer to him by first name.

As the weeks turned into years, there were two things that first amazed me and then just became part of life One was how brilliant everybody was; the other how friendly and patient they were. And the astronomy was everything I hoped for.

Skipping quickly through the next six years, I worked with several teams, observing at many wavelengths and many observatories, sometimes finding exciting things, and often sleeping very little. The startling discovery of ammonia in anomalous absorption against DR21 was the climax. I was observing this source at Haystack, on heavy doses of aspirin because my wisdom tooth was coming out, and was amazed to find the normal pattern of strong center line with four symmetric weaker satellite lines was not present. Instead, there was a crazy pattern in which one satellite line was the brightest of all the lines, while two other satellite lines appeared negative, as did a part of the center line. At first I thought there was something wrong with the equipment, because this countered the "Townes dogma" about the ammonia being composed of small cloudlets in thermal equilibrium. But then I realized that everything could be explained if there were small deviations from equilibrium in the ammonia that happened to be in front of the background source. With Don Brandshaft's help we discovered that these deviations could best come about if there was trapping of the infrared transitions. But trapping wasn't enough – the ammonia had to be in smallish cloudlets. So Charlie was right after all, and my data proved it.

My actual thesis was to put together still more masers, interface them with the Hat Creek Interferometer, take useful data, and do astronomy. It took years and much

help from Walt Fitelson and Al Cheung, but finally I got noisy plots that mapped the methanol maser-spots in Orion. However, by then the VLA was nearing completion, and I remember how Charlie responded when I bemoaned the fact that its 27 large antennas could surpass in one day what had taken me months of observing with two little antennas at Hat Creek. He said with a smile "Whatever you do will be done better by someone else someday. The solution is simple – don't do anything!" In fact, we later used that same VLA, with Andy Harris, to observe ammonia cloudlets directly. Another thing Charlie said that has inspired me is that "the person who succeeds is the one who works 10% harder, because those ten per cents add up." I think we all worked a lot more than 10% harder, and we loved it.

Back to the luck Charlie gave me. A part of the program was to install and use a maser at NRL, and that led to a postdoc. Unfortunately, NRL was transitioning out of molecular astronomy, but within a few months I heard about a job at the Naval Observatory, to do interferometry to measure the rotation of the Earth. It seemed interesting – but they had already offered the job to Dick Plambeck! Here again luck prevailed because he decided to stay at Berkeley, much to Jack Welch's delight. In the interview, I wowed Gart Westerhout with stories of Hat Creek, and ended up with a well-paying permanent and secure job. Ten years later my work with Charlie came through again. The USNO had an experimental clock that wasn't working well. It was based on optical pumping, and the director asked me to take it on because I had worked on masers with the great Dr. Townes. That work hit a resonance with me, and through yet another unforeseeable set of fortuitous circumstances I somehow became the director of the USNO's Time Service department. As director I was responsible for setting the time for GPS, and therefore for much of the world. I remember Charlie mentioning his work on atomic clocks when he congratulated me. Under my management, we built a set of "atomic fountains" that collectively measure time (frequency) to 16 decimal points, which means they are the most precise 24x7 operational system ever built by mankind to measure anything. Even these though, use a maser as the first stage.

Now, twenty years after my appointment and thanks to my over 100 clocks, of which 47 are masers, I can go to meetings, with my grey hair, and get respect while I debate the issues of the day. I occasionally impress my family when the media call me a Time Lord or even Father Time. None of this would have happened if I had walked away in the beginning.

I close with two pictures from my album. How young we all were then! The one on the right shows, from left to right, Larry Greenberg, Tom Geballe, Charlie, and John Lacy. Mike Chui is quietly reading in the background.

Thank you Charlie!!

Observing With Professor Townes

John Lacy

Ph.D., University of California Berkeley, 1979

I'm not sure what the moral of this story is. I think it might be about Professor Townes' willingness to let us do things our way even if he thought we were making a mistake, or just how much we both enjoyed going to Chile.

The first time we took our spectrometer to Las Campanas we were having a hard time getting it cold. We had filled it with liquid helium, but the detector wouldn't work. It acted like it was still too warm, even though we had helium in the dewar. The first time this happened we warmed the spectrometer back up to see if we could see anything wrong. We couldn't find the problem, so we cooled it back down. (A process referred to there as a Chilean overhaul, but I suspect all cultures get blamed for that approach.) We tried again to cool it down, and again the detector wouldn't work. So we decided to wait and see if it would cure itself. It got to be midnight of what should have been our first night on the telescope and it still wasn't working, and I was getting tired. So I told Professor Townes that I was too tired to think straight, and I wanted to go to bed. He looked at me as if he thought I was crazy to want to go to sleep when we were supposed to be observing, but he agreed to let me decide.

The next morning when I woke up and went over to breakfast, I ran into our support scientist who told me that Charlie had gotten up a couple of hours earlier, went to the telescope and found everything was working fine, and went out birdwatching. I didn't even know he knew how to turn on the electronics, but obviously he knew more about the instrument than I realized. (The problem was that with an unfamiliar helium transfer tube we hadn't done a proper slow transfer, as Larry Greenberg had taught me to do, and we had collected helium while the grating was still way too warm, so radiation from the grating was swamping the detector.)

There was also the time when Professor Townes decided we should take a day off (probably a Sunday) and drive an observatory pickup truck up a nearby streambed to see what was living there. We got the new truck a bit scratched up, which for some reason seemed to really upset the observatory superintendent. Later we found out it was because he couldn't sell a scratched-up pickup to finance his gambling habit. There may be no moral to that story except that Professor Townes didn't believe in working all of the time or just doing astronomy. Ornithology and botany are also worthwhile pursuits. I'll include a picture of Professor Townes in his Mao jacket (definitely not politically correct in Pinochet's Chile) outside of a local residence in the canyon.

Charles Townes: A Graduate Student Remembers

Sara Beck

Ph.D., University of California Berkeley, 1981

I met Charles Townes because my undergraduate advisor, the late Dave Wilkinson, recommended me, and Charlie called my parents' house in August 1976 and asked me to come out to Berkeley early; an extra pair of hands was needed on John Lacy's Fabry-Perot interferometer. That was the first phone call I ever had from California. On arrival in Berkeley I found the group library, met John Lacy (and if memory serves also Ed Sutton, Al Betz, Howie Smith, Larry Greenberg, Fred Baas and Dorn Peterson on the first day), drove with John and a truckload of equipment and LN2 up to Lick and started on what has become my life's work. Which explains how it had already become clear to me that infrared astronomy would be my research field, and that Charles Townes would be my PhD mentor, before my graduate studies officially started a month later.

In the late1970s astronomy was just beginning to open up in wavelength regions past the visible. 'Infrared' usually meant I band; the 8-13 micron window was far infrared (!) and very little work had been done there. Preparing for the first observing season of the Fabry-Perot at Lick in the winter of 1976 meant that I studied all the 10 micron sources that had been discovered (by single-detector photometry with 6'' beams); the master target list had perhaps 20 sources. In particular we had no idea then of the true infrared luminosity of galaxies. The few galaxies that could be detected in the infrared continuum were thought to host some special abnormality. We did succeed in measuring [NeII] spectra in 'the exploding galaxy' M82 that winter. The spectra showed that M82 has a lot of star formation, but no explosion. This was one of the very first looks at starbursts, still my greatest interest and now one of the most developed fields of extragalactic astronomy.

M82 was my first paper in the Astrophysical Journal, and the author order, which Charlie decided, was Beck, Lacy, Baas and Townes. He put the junior grad student first and himself last because that was how he saw the relative contributions to the work. This is one example of Charlie's remarkable courtesy to and respect for students. Friends doing theses with other professors told me they were jealous of how Charlie treated us; he was simply decent to everyone, always. He once said to me (during a crisis in the lab with which I was failing to deal calmly): 'Sara, it takes character as well as intelligence to do science". He had one of the very finest characters I have ever known.

I continued to work with John Lacy, and am still working with him, several decades and generations of spectrometer later. John and Charlie were both very focused on the Galactic Center, while I was excited about turning our new infrared eyes on

everything in the sky. So when the Galactic Center could not be seen we observed planetary nebulae (with Lawrence Aller) and HII regions. When the NASA Infrared Telescope Facility opened on Mauna Kea with time blocks dedicated to the solar system, we entered a partnership with Alan Tokunaga wherein he used the FP for daytime observation of planetary atmospheres while we worked on other objects at night. This gained us a wealth of observing time and required me to stay on the summit both day and night, quite against the rules for time at altitude; there is more respect for safety regulations today.

The publication lists from those years show how wide a range of projects we carried out. Charlie encouraged us to choose our own research projects and enter into collaborations; he treated us a scientific colleagues capable of independent work. Now when I review grant proposals for funding large research groups, graduate students so often appear as cogs in the machine. They are expected to run programs or catalog data as directed by the group leader; even their research schedule is set out years in advance. Perhaps this depersonalization is inevitable in a field as huge as modern astrophysics, and is a sign of how far we have come since the days when we knew of only 20 infrared sources in the sky. But I feel rather sorry for today's graduate students who I suspect have less freedom and less fun, and I wonder if this is the way to train creative scientists.

I would have liked to do my thesis on galaxies but only M82 and NGC 253 were bright enough to be seen with the detectors we had then, so I wrote on star formation in Orion, probed with the 12.3 micron line of molecular hydrogen. A few years ago, at a seminar on galaxy clusters in Boulder, the speaker showed Spitzer Telescope maps of the 12.3 micron emission from clusters of galaxies and I blurted out 'I discovered that line'. We discovered quite a few of the important infrared features.

Today there is wide concern over the position of women in the sciences. There are few women in physics now and there were fewer in the 1970s (although the numbers have not really changed much), and I am often asked about my time as the only woman in the Berkeley Townes group. I am glad to be able to say, in this record, that Charlie treated me with perfect fairness; I simply never felt that gender was an issue. Which may sound odd today, when gender must always be an issue, that was my experience and would that it were more common. I was also, after Howie Smith graduated, the only Orthodox Jew, which actually presented more practical problems: the other students were very good about taking over for me on Sabbaths and holidays, for which I am grateful. Charlie was quite knowledgeable about Judaism, and I remember him sitting in our living room poring over a Hebrew Bible with the help of one of our friends.

Berkeley was not utopia, work was often frustrating and we graduate students were not free of personal conflicts and squabbles. But those pass, and the intellectual excitement, enthusiasm and friendships of Berkeley remain fresh to this day. What

I learned from Charlie, both in and out of the lab, has been a touchstone and support to me for over 30 years.

May his memory be a blessing.

Reminiscences of Professor Charles H. Townes

Samuel D. Gasster

Ph.D. University of California Berkeley, 1985

I joined the "Townes Group" during my 3rd year as a graduate student in Physics at UC Berkeley, in the fall of 1979. I had initially worked in the Alvarez Astrophysics Group, but my sponsor left so I wanted to find another advisor. Someone mentioned that Prof. Townes was looking for additional students and that I should go talk to him.

I recall that when I approached Prof. Townes, he suggested I first talk with some of the current graduate students and post-docs to get the lay of the land. I talked to Dan Watson, John Lacey, John Storey, Ed Sutton and many others, and the projects seemed very diverse and interesting. Then, I discussed my options with Prof. Townes and we reviewed the existing projects, which were all in various stages of research. He mentioned that he was very interested in extending the technique of optical heterodyne detection that they were using at 10 microns, into the far-infrared. The group had been using this technique of mixing a laser local oscillator with the light from an astronomical telescope to perform high spectral resolution detection of various species. The group was also working with a Fabry-Perot based far-infrared spectrometer for studying molecules and ions in the interstellar medium and star forming regions. He said he had met someone at a conference who was making impurity doped Ge-detectors that operated at wavelengths in the 100 micron region and hoped would have the speed necessary for heterodyne detection at these wavelengths. Since no one else was working on this project, and I could sense a certain excitement on his part with the new work, I jumped at that project. He had to contact the person supplying the detectors to send him some samples, so while we waited he suggested I start building up the infrastructure necessary for the experiments and gaining experience on the existing projects.

Thus, I found myself traveling to Kitt Peak National Observatory to help with the 10 micron dual heterodyne system (I think I was a jinx on that experiment since I recall both times the weather was mostly bad with little usable observing time). I was also fortunate to be one of the "roadies" traveling with the team when they flew the Fabry-Perot Spectrometer on the NASA Kuiper Airborne Observatory (KAO). It was an amazing experience to fly on that aircraft. One of my favorite memories was surprising Prof. Townes on his 64th birthday with a celebration of cheese cake (kindly provided by my future mother-in-law) and sparkling cider at 42,000 ft over the pacific ocean while on an observing flight in the KAO. Prof. Townes was genuinely surprised and flashed a very appreciate smile and a good time was had by all.

Working with Prof. Townes was in part a fulfillment of a dream for me from a very young age, when I first read about lasers. I was very interested in lasers, building

my own dye laser in high school and working with several He-Ne laser devices built from kits. It was quite a fun time in graduate school when I had to build both a CO_2 laser (40 watts) to pump a CH_3OH (methyl alcohol) laser to generate the 100 micron radiation to test the detectors. Setting up the experiment required a great deal of patience with alignment of optics, which I found very time consuming and challenging. Once, after spending hours on this with no success, I went to Prof. Townes to ask for help and of course he said, "let's take a look". It only took him about 10 minutes of knob twiddling on various mirrors and everything was aligned!

It was often hard to get Prof. Townes alone for extended periods during the day since there was so much demand for his time, so it was easier to catch him upon his return to the lab after dinner. One of my favorite stories is when one evening, we met in his office to go over some analysis I had performed for my experiment. I was at the blackboard writing equations and talking about the analysis when I turned around and noticed he was leaning back in his chair with his eyes closed. I thought "Oh no, I put him to sleep, I'm in big trouble now!" Unsure of how to proceed, I decided to keep going, and not a minute later, he literally leapt from the chair to the blackboard, grabbing the chalk and commenting on my analysis. I realized that he had not fallen asleep but in fact had heard and understood everything I had said. I often wondered if his vision extended into the X-Ray region and was able to see through his eyelids!

I learned many things from Prof. Townes: a great deal about experimental physics, lasers, molecular spectroscopy, optics and much more. However, I believe that the one thing he imparted to me was that of being persistent in pursuit of your goals. This is a topic he mentioned many times and I found this to be very good advice. Thank you Charlie!

Figures: Posing with Charlie outside a hotel in China in 1982. We just happened to be at the same hotel on different trips to China. Scan of handwritten notes from Charlie that we made one night while discussing the heterodyne detection experiments. Posing on the Townes bench in Greenville, SC (2008).

5 Nov. 1982

Bandwidth of Hall sample no. 2.

Ge: Ga

$N_D \sim 10^{12}\ cm^{-3}$

$N_A \sim 10^{14}\ cm^{-3}$

4×10^5 cm/sec. / V/cm.

1 amp/watt.

H

1mm.

$$I = \frac{Pe}{h\nu}\eta \frac{4\times10^5 \times 1\ \tau}{0.1\ cm.}$$

$$1 = \frac{10^{-7}}{6.6\times10^{-27}\times3\times10^{12}} \times \frac{4.8\times10^{-10}}{3\times10^{9}} \times 4\times10^{6} \times \tau \times 1/2$$

$$\tau = \frac{2\times10^{-14}}{7.6\times10^{-19}\times10^{7}\times3\times10^{6}} \times 2$$

$$= \frac{2\times10^{-8}}{3.2} = 6\times10^{-9}\ sec.$$

$$\Delta\nu = \frac{1}{2\pi\tau} = \frac{1}{3.6\times10^{-8}} = 30\ MHz.$$

CHT Memories

Andy Harris

Ph.D., University of California Berkeley, 1986

My fondest memories of Charlie are small, personal ones that have to do with his deep understanding of the physical world and his gentle humanity.

If I had to pick a favorite memory, it would come from a discussion we had when I was a very young graduate student. In many ways it's typical of many examples of Charlie's deep insight and consequently clear explanations. We were working on developing a millimeter-wave maser amplifier for radio astronomy, and Charlie had set me to work on a quantum-mechanical perturbation calculation to work out how much pump power we would need to provide to make the maser work properly. This was a nice exercise for a first-year grad student then deep in his quantum mechanics classwork. Theory was not my strong point, but I had gone through the standard texts and had gotten things mostly worked out when he suggested we get together to look them over. We sat in his office in Birge Hall with a view of the Campanile and the Bay as he sorted through what I'd done. 'Ah, here,' he said, looking at a matrix I'd provided, with his finger moving across the page, 'this connects this state to this one, and this to this one,' and so on. Then, looking up, 'You see, when we have a field so strong that the change of energy with magnetic field in these states becomes a straight line, the states are nearly pure and it's very hard to absorb the pump. That's why these matrix elements are getting so small.' This almost took my breath away: suddenly, with his few sentences of explanation, my calculation (and in fact a big chunk of quantum mechanics), which until that moment had been extremely abstract to me, made absolute physical sense.

One thing that I appreciated was the amount of independence we had as students. Here being in the Townes group was a great advantage: the other students and postdocs were generally quite ready to offer help and criticism, and one's weaker ideas did not survive long. As long as we seemed to be making reasonable progress we had only the lightest touch of supervision. If we seemed to be drifting off track, however, we got a gentle but unmistakable invitation to sort things out. I remember one of these episodes:

CHT: 'Let's, see, Andy, I'm not sure if I'm quite up to date on what you are doing.'

AH: [Waiting expectantly for the next remark, but thinking that this didn't sound good.]

CHT: 'Why don't we get together to see what you are doing?'

AH: ‘That would be great.’

CHT: ‘Should we say 9 on Saturday? It’s quiet then.’

AH: ‘That would be fine’ [It will also give me a chance to verify that the Physics department really is quiet at 9 AM, a time few grad students saw on campus...]

And it was quiet, and we got things back on track -- and then he took me out to lunch. As usual, such a kind way to deal with people.