On being a Student of Norman Ramsey

by Fraser Code, Emeritus Prof. of Physics, University of Toronto.

There is a story behind how I came to be a graduate student of Norman Ramsey at Harvard University in the late 1960's. When I was in High School in Toronto, Canada, I had watched a Sunday afternoon discussion show on US television from Buffalo NY, which dealt with current issues on Science and Society. The only panelist I remember by name was the distinguished American Physicist Isidor Isaac Rabi. I paid great attention to what he said, because his comments were insightful, well spoken, and kind. After the program was over, I remember thinking "If that's the type of understanding that the study of Physics leads you to acquire, then I'd like to become a Physicist!"

After High School at Etobicoke Collegiate (in a comfortable suburb of Toronto), where I studied math and science with ease, and enjoyed receiving a good background in modern and classical languages, I enrolled as an undergraduate student in the Mathematics, Physics and Chemistry program at Victoria College in the University of Toronto. Throughout the four years of that program I was inspired by many brilliant lecturers to learn new things, and think in new ways about everything. I was fortunate to graduate in June 1965 as the top student from Victoria College, receiving the Governor General's Silver Medal. I also placed first in the Canada-Wide Canadian Association of Physicists Undergraduate Prize Exam, and was granted one of the coveted Woodrow Wilson Fellowships for graduate study in the United States.

I mention these things because they were the cards I was dealt when I was 21 years old. But as we all know, the first cards you are dealt in any game do not necessarily predict the outcome.

I remember that about six years before graduating from Toronto, an older cousin of mine had married a graduate student in physics from Harvard. As I was introduced to her new husband, he said many good things to me about his academic experience there. As it turned out, I applied to Harvard for graduate studies, was accepted, and enrolled without hesitation. I

looked forward to being thrust into a stimulating, well rounded academic environment.

Overshadowing those happy thoughts were the turbulent days of the early 60's in the United States: the Cuban Missile Crisis, the assassination of President Kennedy, and the 1964 Gulf of Tonkin resolution which firmly involved the US in the Vietnam War. But I had received a safe US F1 student visa, and so I travelled in September 1965 from Toronto to Boston by train, with all my treasured physics books in heavy suitcases. I arrived at Harvard, moved into Perkins Hall, a men's graduate student residence on Oxford Street, and settled in.

One ritual at Harvard was for new students to meet the Dean of the Graduate School. He welcomed me, but said not many science students came from Toronto to do their PhD at Harvard. Another formal greeting came from a meeting with the Chair of the Physics Department, W. H. Furry, who was bubbling over with enthusiasm for the Boston Red Sox, and their special baseball player named Carl Yastrzemski.

As I came to know the other graduate students, and the resident post-doctoral fellows, I did a quick estimate, and concluded that about 10% of them were Canadian, roughly in proportion to the relative populations of our two countries. Harvard apparently had an "equal opportunity admissions policy" for Canada and the United States combined.

It was normal for graduate students in Physics to be admitted to a "Course Master's" program. Then they proceeded to being PhD students if they achieved honor standing in the required number of course examinations and went on to pass a qualifying oral exam. There was no language requirement or a formal comprehensive examination, such as the 5 hour written part of the General Doctoral Examination in the Physics Department at MIT.

Over the years, I've lost track of some of the courses and the order in which I took them, because some were research courses and reading courses as well as the standard lecture courses. Having studied Mathematics and Physics at Toronto, I was very well prepared. Some of the introductory courses at Harvard were an in-depth review of some material I

had previously seen at Toronto, but my understanding of basic physics principles was greatly increased by them.

The biggest surprise to me was that a “half-course” at Harvard, and many other US universities, had three lectures a week for 15 weeks, rather than at Toronto where the corresponding load was two lectures and a tutorial hour for 12 weeks, with no Saturday classes ever! In my first two years, I remember taking Quantum Mechanics from Wendell Furry, Group Theory from John Van Vleck (using Michael Tinkham’s textbook), Electrodynamics as well as Molecular Beams from Norman Ramsey, Solid State Physics from Henry Ehrenreich, Topics in Nuclear and Particle Physics from Bruno Zumino, Quantum Optics and Coherence from Roy Glauber, Magnetism also from Van Vleck, a reading course on NMR from Robert Pound, another lecture course from Frank Pipkin, and a few research courses working in the lab of Norman Ramsey.

After the first year of courses, which I successfully completed, I was asked by Ann Guest, the Administrative Assistant for the Chair of Physics, to act as the Marshall of the Graduating A.M. class of 1966, but I had to decline. I had been planning for a year to return to Toronto and marry Jennifer Page at the very time of the June graduation. It was an easy decision for me to make about which was the more important occasion.

When Jennifer and I returned as a couple to Boston, we moved into Haskins Hall, Harvard’s married graduate student apartment block at 28 Irving Street, Cambridge, Mass. Of course, it was located on the student side of Irving Street, on the “other side” of Kirkland Street from where a few professors, and the famous French Chef on PBS, Julia Child, lived. We became great fans of Julia, and enjoyed walking up Irving Street and across Kirkland Street to see her house. We were rewarded once by watching her wash her Volvo station wagon. It had a cooking spoon fixed to the top of its radio antenna, while she was dressed in Wellington boots with a rubber kitchen apron over her good clothes.

In many ways, it seemed a different and easier time to live, but the main reason this appeared to be true was that we were young and energetic. In retrospect, every stage of our married life together has been challenging and at the same time deeply rewarding.

To give a few numbers of how different a time it was, I recall in the lab liquid helium was supplied on a throw-away basis for $1.49 per liter. Our one-bedroom student apartment rented for $105.00 per month. There were strong student protests when a year or two later the rent was increased to $115.00 per month. In 1968, Gulftane gasoline (the cheapest grade) was $0.29 per gallon, and a new Dodge Dart automobile cost $2,600.00.

During the summer of 1966, I approached Norman Ramsey to ask if he would be willing to supervise me as one of his PhD students. Among the graduate students that were my contemporaries, Norman Ramsey had a widespread reputation for being easy to work with, and an extraordinarily kind and considerate person. I was attracted to doing fundamental physics experiments in his molecular beam laboratory.

Later, when I informed another professor of my desire to work on molecular beam experiments with Norman Ramsey, he simply asked “Why don’t you do something new for your PhD?” Of course, I was already convinced I could learn much about the foundations of physics from Norman Ramsey. In the back of my mind was that he had studied much earlier for his own PhD at Columbia under I. I. Rabi. Indeed, I've found over the years that, at times, all things old can become new again.

I began my research with Norman Ramsey in the summer of 1966, attending the weekly group meeting of his graduate students, and working closely with a former student of his and now a post-doctoral fellow, Irving Ozier, who was also from Toronto. Much of my work involved assisting in ongoing research activities, learning to maintain the vacuum system, helping with data collection, and analyzing the radio-frequency spectra of molecules. To me, it was an interesting period of apprenticeship in experimental physics, a subject I had avoided as an undergraduate because I had many of the required skills from hobbies in electronics and photography. I always felt an intuitive confidence with both technical work and data analysis techniques, and had therefore concentrated on the more mathematical aspects of physics at Toronto.

New graduate students were encouraged to be active in the Department’s Graduate Student Association, and I was certainly in the thick of organizing

with my friends some very good parties for both students and faculty. We found, however, that it was somewhat difficult to obtain fresh Canadian ale in Cambridge, Mass.

The Department organized a summer Physics picnic at a farm in Topsfield Massachusetts, which was a great success. Robert Pound had a liking for expensive British cars, and had just bought a new Rover. I was in the Departmental machine shop when he announced something to the effect to everyone “Well, I have just successfully removed every anti-pollution device from my Rover, and now it runs much better!” He drove that new Rover to the picnic at Topsfield, and when one of the grad students deduced from its smoking rear wheels that he had left the parking brakes on for the entire trip, Pound replied “That’s no real problem. Those brakes are tested to a white heat!” Another memory I have about that picnic was how I, an experienced woodsman and camper from the great Canadian North, was shown in painful detail by one Professor’s enthusiastic teenagers about how to light a campfire. They said to me “Let’s do it the way we did it last year in Paris.”

In my second year (1966-7) I mainly took research courses in Molecular Beams from Ramsey. I also took that reading course from Robert Pound on NMR. Early on he introduced me to the “Purcell Effect”, on how an optically resonant cavity can influence the spontaneous fluorescence lifetime of an excited atom inside. He went on to explain how the effect also applied to an isolated nuclear magnetic spin precessing in an external magnetic field when it was surrounded by an *rf* (radio frequency) coil having a high value for its quality factor *Q*. The book I read for his course was Abragam’s treatise on “The Principles of Nuclear Magnetism”. Pound recollected that he and Abragam discussed writing a book together on Nuclear Magnetism, but that Abragam went on to complete the actual book by himself.

A special treat was to take the course on Magnetism from John Hasbrouck Van Vleck (JHV[2]), who was approaching the retirement age of 70. His lectures were lively and laconic, and a bit too chaotic for taking good notes by ear or from the chalkboard. I obtained permission to record the lectures and rewrite my own personal handwritten set of notes. For me, these were a treasured memory of the work of a wonderful Physicist who had so far made a career of “missing out” on some significant prizes and also had

unfortunate timing with his writings (especially his 1926 book on the old quantum theory, which was published just in the middle of the revolutionary formulations by Heisenberg, Schrödinger and Dirac). Later his luck did change for the better, and he was jointly awarded the Nobel Prize for Physics in 1977.

Van Vleck was locally famous for challenging his students with enigmatic assignments, especially in his other course on Group Theory. He cryptically wrote what we thought was “DOOCS” on the blackboard as a homework problem. But this was our misreading of his handwriting. He meant “DO OCS”, which was short form for “Do the group theoretic analysis of the vibrational modes and degeneracies of the OCS molecule”.

Van Vleck was a great character, a fan of baseball and memorizing train schedules. He was a little bit envious that I had enjoyed a train ride through the Rocky Mountains from Edmonton to Vancouver in the daytime. His only train trip through those Canadian Rockies had been scheduled for overnight.

I remember the introduction of Wang calculators throughout the Department, first one by one, and then everyone had access to them. One of the early converts was Wendell Furry, who explained to me how he was solving recurrence relations and difference equations by learning to do an appropriately patterned “waltz” with his fingers over the calculator’s control keys.

Only a year or so later, Ramsey acquired one of the first mini-computers for his PhD students - a table-top Digital Equipment Corporation PDP 8/S computer, serial number 10. Everyone took great interest in this computer, which ran programs from teletype-punched fan-folded paper tape. Norman also enjoyed the mini-computer, but came in after hours to run programs from the teletype keyboard, so that he wouldn’t interfere with his students’ use of it for their research.

This computer became quite stupid when its power was turned off, because it had a woven ferrite magnetic core memory that lost all its stored information when its internal currents stopped flowing. It also contained circuit boards made from discrete transistors. After it was turned on, it

required a two-stage combination of a panel switch key input and a punched paper-tape boot-up process before it could load its FOCAL programming language system (similar to BASIC).

The Department held weekly Colloquia, which attracted the most interesting of the international physics community as speakers. Much attention of the Physics Department was focused on the new Cambridge Electron Accelerator, and many of the Colloquium speakers gave talks on topics in “high energy” physics. This was too much for students like me who were doing their research on atomic and molecular physics.

A few of us who worked with various atoms and molecules met with Roy Glauber, the Chair of the Colloquium Committee, to request more talks be presented on “low energy” physics. Roy’s immediate response was that the talks were indeed balanced, because half of them dealt with physics above 1 MeV (Million electron Volts) in energy, and the other half were on physics topics below 1 MeV in energy. So we learned there are many ways of viewing how to establish a balance of topics for Colloquium speakers, and that Roy Glauber was having a good deal of fun teasing us about it.

During the time between my research courses with Norman Ramsey and my completion of the PhD qualifying examination I became thoroughly familiar with all aspects of the molecular beam laboratory. I worked largely in conjunction with Irving Ozier, who had just finished his PhD thesis using the molecular beam magnetic resonance apparatus. I remember completing an analysis of the magnetic resonance spectrum of hydrogen chloride, doing a small project on the magnetic field deflection of the XeF_6 molecule, which proved to be diamagnetic rather than paramagnetic, and making a magnetic deflection measurement of the algebraic sign of the electron-coupled spin-spin interaction constant in the molecule HD. I also helped Irving take data for his postdoctoral work on the rotational magnetic moment spectrum of methane. These short experiments were to me a time of apprenticeship, where I learned all the ropes of the molecular beam magnetic resonance method.

In the late 1960s, the large research group led by Norman Ramsey varied in size from time to time, having a few senior undergraduate and many graduate students, as well as a few postdoctoral fellows. I remember taking

a picture of the group during the summer after we had lunch under a shady tree in the yard beside the Lyman Physics Lab. During those lunches, we listened to stories told by Norman of his past physics experiments, and bought from a nearby lunch truck some delicious chocolate éclairs. That group photograph eventually was included by David Wineland into his 2012 Nobel prize lecture, much to my delight.

But I particularly remember one outspoken undergraduate from Harvard who was merciless in teasing the graduate students he worked with. He came up with what he called was the “Law of Propagation of Idiocy”, which stated that if someone in a laboratory made a mistake, then another person working there was sure to copy it.

As I indicated above, during those summertime lunches in the Harvard Law Yard, Norman gave us a relaxed perspective on the knowledge of physics he had gained during his career. Once, we discussed the relationship between the energy E and the momentum p for particles that had rest mass compared to the same E and p relationship for photons of light, which do not have any rest mass. We all were familiar that at high enough values of the momentum p, any massive particle became indistinguishable from photons on the dispersion graph of energy E versus momentum p. They became “light-like” (i.e., they behaved very much like light quanta) on such a “dispersion curve”. We asked him why this is occurred and his reaction was simple: why not?

Norman always mentioned to us the importance of fundamental tests of the symmetries and assumptions in physics. He said "You can’t just trust your own ideas. You've got to do an experiment.”

When I was in his molecular beam group, Norman was conducting magnetic resonance experiments elsewhere on beams of slow neutrons, first at the Oak Ridge National Laboratory (Tennessee) and later at the Institut Laue-Langevin (Grenoble), using ultra-precise resonance methods to search for a non-zero electric dipole moment of the neutron that would violate CP conservation symmetry. He explained that a low-velocity “cut-off” filter for neutrons could be made to extract only the slow neutrons from a reactor by means of a curved beam pipe made of certain metals that had a velocity-dependent neutron absorption coefficient. This gave rise to a 100%

total reflection for glancing angles of incidence of sufficiently slow neutrons at the beam pipe wall surface from a vacuum.

This would be in full analogy with a total (internal) reflection of a beam of light at a glass-air surface. Because of the wave-particle duality in quantum mechanics, a single velocity neutron beam has an associated complex “index of refraction” in the metal which would depend on the absorption coefficient of neutrons in the metal of the beam pipe. And for ultra-slow neutrons, even a “bouncing-box” container could be made for them because the index of refraction in the metal compared to the vacuum allowed for total reflection at normal incidence. Hence, the various versions of Ramsey’s neutron electric dipole moment experiments were conducted under exotic circumstances.

One reality of research Norman mentioned to us was that there was never enough time to do everything you wanted to. He recalled his early interest and involvement with particle physics. But as other opportunities arose (such as atomic hydrogen masers, his leading role as president of the URA consortium associated with the founding of Fermilab in Chicago, and his search for an electric dipole moment of the neutron), he chose to no longer be directly involved with high energy particle physics experiments.

He also cautioned us to be very conservative with determining systematic and statistical error estimates for our experimental results. He reminded us that in the history of the various Lamb shift experiments in atomic hydrogen, the final agreed result for the fine-structure constant α (alpha) was shown to be five times outside the maximum error range for it as stated by the initial experimenters.

One amusing memory that Norman recalled to us dated from a sabbatical he had taken at Oxford University. He was having a draftsman prepare a slide stating the result of the experimental ratio of the hyperfine frequencies of atomic hydrogen and atomic tritium maser transitions. The ratio was $1.0677945149734 \pm (50) \times 10^{-13}$. The draftsman, an engineer, mentioned that result seemed ridiculous, because every rigid material he knew of was subject to much greater gravitational creep and distortion as a function of time. Ramsey said he had replied that in the world of quantum mechanics,

simple atomic systems were incredibly stable, and that his result was indeed true.

At that time, in one of Norman's laboratories, work was proceeding on evaluating the accuracy and stability of atomic hydrogen masers. While individual masers were highly stable, they differed very slightly in frequency between themselves because of an effect known as the “wall shift”. This effect originates from the frequency shifts induced by collisions between the radiating hydrogen atoms and a Teflon-coated spherical quartz containment vessel inside the maser cavity. Because of random impurities, no two quartz vessels can be made with identical Teflon coatings on their inside walls, so that different hydrogen masers have different “wall shifts” that “pull” their oscillating frequencies away from the hyperfine transition frequency of a truly isolated hydrogen atom. Hydrogen masers do not therefore provide a frequency standard that is as accurate as their individual stability. By itself, a single hydrogen maser is a very stable 21 cm radio-frequency source.

If made small and rugged enough to be launched into space by a rocket, a hydrogen maser should be stable enough to detect first order Doppler shifts from differences in relative velocity, from the gravitational potential, from general relativity, as well as the second order time dilation effects from special relativity. On one occasion Ramsey invited a member of the quantum electronics division of Varian Associates, Beverly, Massachusetts, to visit our group and talk about proposals for a space-borne microwave hydrogen maser. These experimental dreams were eventually realized and published in Phys. Rev. Letters, 45, p 2081, (1980) under the title “Test of Relativistic Gravitation with a space-borne Hydrogen Maser”.

My time at Harvard coincided with the Vietnam war. Some graduate students were conscientious objectors. A few expressed interest in coming to Canada. I also remembered the stinging smell of tear gas that lingered overnight in the sunken entrances to basement apartments near Harvard Square, the day after a large student anti-war demonstration was broken-up and dispersed by the Cambridge police. I wrote a letter to a Canadian cabinet minister, Robert Winters, the member for my home riding of York West in Toronto, to ask him what I should say to anyone who asked under

these conditions about coming to Canada. He graciously replied that anyone eligible to enter Canada was always welcome.

I came to like Norman Ramsey's supervisory style very much. He gave insightful advice either when talking to him in his office or whenever he visited the lab, but he never stayed and hovered over my actual experiments. He let me make and learn from my own mistakes.

On one occasion Ramsey brought the Dutch physicist H G B Casimir into the lab for a visit. He was a very tall distinguished man. I enjoyed my conversation with him immensely. Another visitor to the lab was C W F Everett, an outstanding low temperature experimentalist from Stanford. He talked to me at length about exploiting the elasticity of materials for the design of cryogenic equipment, such as the low temperature hydrogen beam effusive source I would later build.

I remember some special eccentricities of Norman's. He always carried a transparent woman's plastic raincoat (with a hood) in his small travel briefcase. Although much more compact and practical than an umbrella, the mere thought of their father wearing it made his teenage daughters cringe with embarrassment. He found the raincoat very handy, for he often travelled at short notice during the day from Boston to Washington, or to Chicago. He always spoke in a loud voice, and his secretary just had to listen in the corridor to see if he was somewhere in the building. When he left his office with the door open, and she couldn't hear him, she often concluded it was a very busy day, and suspected that he had escaped into the men's washroom to get some peace and quiet to finish important work.

Norman was a tall man, with distinguished snow-white hair, who carried himself with enthusiasm and dignity. I rarely saw him flustered. But when he held a group meeting with graduate students to discuss house-cleaning his labs of unused equipment, he mentioned that there was a large solid brass tube under a table in one of the offices. It had been left over from the construction of a vacuum chamber for a molecular beam experiment. When he asked if anyone wanted to make use of this, someone commented that he had seen a cat sleeping in it. Ramsey replied "Well, we can't afford that expensive a cat house!" Then he realized his *double entendre*, and instantly turned very red and stammered with embarrassment.

Ramsey once commented that he didn't know how poorly he spoke, until he was presented with an exact record of what he had just said in a session of congressional testimony. He indicated he was most grateful for being given the editorial opportunity to complete his verbatim sentences before those portions of the official transcripts were printed.

Norman commented occasionally about his own graduate student days at Columbia University. His supervisor, Isidor Rabi, loved to whittle on wood with a pocket knife. Whenever Rabi (affectionately known simply as Rab) came into the lab to talk, it was always one student's duty to produce a convenient block of wood for him to whittle on. Apparently, Rabi had once carved absent-mindedly into a leg of the wooden laboratory tables supporting the experimental equipment. After that incident, his students were always quick to prevent the great Professor from destroying the wooden supports of their equipment tables.

Before I began my work on my own PhD thesis, I presented a seminar to the Ramsey group entitled "A Guide to the Lamb Shift". I put a lot of effort into this, because it wasn't easy for me to learn the quantum-electrodynamic calculations. But I finally mastered them and cast the results into understandable language. My talk was well received. I felt also that I had learned a lot from this exercise.

In the late 1960s, there were several veteran Harvard professors who had important military research experience during the Second World War. Among them were Kenneth Bainbridge, John Van Vleck, Edward Purcell, Robert Pound, and Norman Ramsey, whom I personally knew. Kenneth Bainbridge, one of the best of American experimental physicists, played a significant role in the Manhattan Project, being the head scientist of the July 1945 Trinity test, the world's first nuclear explosion. John Van Vleck, Edward Purcell, Robert Pound, and Norman Ramsey all were involved in the MIT radiation laboratory. In addition, Norman Ramsey later contributed to the Manhattan project, and served as the Chief Scientist in charge of the entire atomic bomb delivery and drop on Japan at the tender age of 26.

Norman mentioned that during his wartime duties on Tinian Island in the Pacific Ocean, he had to wait a long time for all the preparations involved in

the atomic attack. To make better use of his time, he requested that the Air Force fly in a small library of the journal “The Physical Review” for him. These he read from cover to cover. Having once started this endeavor, he continued with it after the war. However, physics research was flourishing dramatically. The thickness of the issues of the Physical Review steadily increased. Later, there were so many articles that the Physical Review itself had to split into separate different series. Norman's wartime habit eventually had to be modified from full cover-to-cover reading of all articles, to reading only the abstracts of every article published. But the number of papers still increased, and by the time Norman was supervising me, he had to abandon this whole cover-to-cover approach entirely, and use new strategies for reading the physics literature.

Professor Kenneth Bainbridge amused me and another friend from Montreal, Art Olin, over lunch one day at the cafeteria behind the Harvard Law School (the old Harkness Commons building), by telling wartime stories of how radar clutter was easily generated for the protection of Allied bombers flying over German-occupied Europe. The bombers simply dropped crumpled aluminum foil behind them[1], or threw out parachutes with small operating 1P28 photo-multiplier tubes to generate radar snow from the microwaves radiated from their shot noise output currents.

Art also told me that during the 1960s when the Chinese were testing atom bombs in the atmosphere, Bainbridge collected rainwater (containing fallout particles) to see what types of radioactive isotopes were being released, and from them determine what materials their bombs were made of.

During one of Ramsey's research group seminars in his large office, when there were about a dozen of us present, Professor Sheldon Glashow appeared at the open door to ask Norman to sign a petition. Norman asked for it to be left for him to read and think about, and then he would get back

---

[1] In 2016, a Canadian friend of mine, who had grown up in wartime Germany, had recalled to me how many German children collected the small pieces of aluminum foil dropped from Allied bombers, rolled them up into dense balls, and returned them to the German government to assist with their metal recycling program.

to Glashow later. At another time, Norman said to us that he was frequently asked how he felt about his role in the wartime atom bomb attacks against Japan. He said that it was not a simple question. He mentioned that his work on another wartime project, the development of 3-cm radar in the MIT Radiation Laboratory, probably led to more cumulated deaths in Europe than the deaths from those two atom bombs dropped on Japan. I am personally convinced that Norman had felt the full import of the inhumanity of war in his heart, but had the courage to continue using his impressive leadership skills for the betterment of science and society.

Norman Ramsey was always a highly ethical and kind man. He never seemed haunted by what was done in the past, and he was fully engaged in the present. He was superb in handling confidential information, and had the reputation of having never criticized any one of his graduate students to another.

As an example of his ethics and leadership, Norman once mentioned that he would be spending more time in Washington DC than he wished to. He was sitting on the General Committee of the Atomic Energy Commission. He told us that particular committee was one that had to approve the use of nuclear weapons in Vietnam. He added that he trusted we all approved of his periodic absences from the lab to sit in on those sessions.

I do remember Norman as being a socially progressive person. He argued in our group meetings and in a short article in the April 1970 issue of the ‘Bulletin of the Atomic Scientists’ that the United States desperately needed a pollution tax. He also mentioned that young people should not despair of voting in elections because it was easy to check the voting record of incumbent politicians to see what were their real policies and actions.

However, one of the strangest things that happened to me as a graduate student at Harvard had to do with my apartment key. You will recall that my wife Jennifer and I lived in Haskins Hall, a Harvard apartment residence for married students. One night as I stood in front of the locked door of Lyman Laboratory, I fumbled in my pocket for the key and opened the door only to realize that I had used my apartment key. Well, I was surprised! I examined the departmental key blank and my apartment key blank and found that although my apartment key was slightly thinner, it had roughly the same

shape as the Harvard departmental key, so it had no difficulty entering the key hole. I wondered what else this key would open, but feared to try, for it was my apartment key, and I did not want to get in trouble because of it.

I regularly used the Physics library in the adjoining Jefferson Laboratory, that had a locked rear exit into the Lyman Lab, which anyone could use to leave the library. I noticed that the professors had a key that would open that exit door from outside the library, but none of the graduate students did. One day, I deliberately used my apartment key to enter the library through this locked door, and much to my surprise, it opened easily! I didn't tell anyone of this fact.

On a later day, I passed by Norman Ramsey's office, and his secretary was outside the door in distress. She said to me "Oh my, I've just locked my keys in Prof. Ramsey's office and he's not here today!" I said to her "Don't look, and I'll see if I can help you." I then tried my apartment key in his office door - and it actually opened! I swore his secretary to secrecy because it was my true apartment key and I stupidly was terrified of losing it. I was amused that this breach of security in one place at Harvard was caused by a key that came from a totally different Harvard building. What are the chances of that happening at random? I never further investigated what my apartment key would open elsewhere in the department.

Socially, Norman and his wife Elinor were very active with his students. They always were willing to attend when invited to any of the parties his graduate students threw. In 1967, Norman and Elinor organized a summer beach excursion to Cape Cod with his students.

In fact, Norman brought along his own surfboard to Horseneck Beach on that occasion. On other occasions, he invited his students a few at a time to sail with him in his small sailboat at a club on a lake in the Boston suburbs. My wife and I thoroughly enjoyed these outings with him.

Another favorite professor was Edward Purcell. During the Apollo Moon Landing project, he had approached Edward Land of Polaroid Corporation to design a 3-D camera on a wand, that would allow the astronauts to take close-up pictures of the undisturbed lunar soil. He showed the results to us

in a Physics Colloquium (complete with a special screen and polarizing 3-D glasses) that I shall never forget.

Also, Edward Purcell was involved in a clearly apocryphal story which I heard circulating in Ramsey's group was about a conversation he once had with Howard Berg, a former member of the hydrogen maser lab. After Howard had just been appointed a Junior Fellow at Harvard, he approached Purcell for advice about what type of research project he should do. Apparently, Purcell replied something to him along the lines "How should I know, Howard? I received the Nobel prize a while ago and now I still don't know what research to do myself!"

In the first part of June 1968, when I was 24, I suffered an acute psychotic episode, which occurred without warning over a period of a few days. My wife quickly went with me to the Harvard Infirmary for help. I stayed there for about two weeks. Norman and many of my close friends came to see me there. But then I was transferred to the Massachusetts Mental Health Center, which was an old and shabby asylum-type building, just a renamed version of the original 1912 Boston Psychopathic Hospital. (Much later in 1994, it appeared on the National Register of Historic Places as being a representative of one of the oldest Psychiatric Hospitals in the USA.)

A low point in my life was the day I had to use the only pay phone on the exposed wall of a busy public ward in "Mass Mental" to contact the Dean of the Graduate School and withdraw from the fall academic term because of illness. The huge problems in my heart were that I did not understand why I was ill, or have any reassurance that I would ever recover. I also feared being deported from the US, if illness prevented the renewal of my temporary F1 student visa.

Some of my graduate-student friends visited me there. They shared my sister's view that it was a scary place. I also remember Norman came to see me while I was on those run-down wards. This surprised me because he was always so busy.

With the help of experimental psychotropic medicines, and skillful counselling, I began feeling more like myself, and returned to my studies in at Harvard before the start of winter. I remember how painful my first trip

back to the laboratory was. Every single step I took seemed like it was a mile long. My heart was full of chagrin because I had been ill in a way that at that time I perceived as being a devastating humiliation.

The experience of such an illness gave rise to a profound sense of loss within my heart. The resulting inner darkness persisted throughout my professional career, and remained with me for at least another decade after I had retired in 2003.

When I got back to Ramsey's labs, my friends were there, and they greeted me, and everything was apparently fine.

I was very fortunate to recover enough to re-register in the Graduate School of Arts and Sciences for the winter term. Of course, when I next saw Norman Ramsey, he was his usual positive self, and was entirely supportive of me.

Gradually I regained my strength and my spirit, and I finished my doctorate as planned, but only a half-year delayed. At that time, I had no satisfactory explanation as to what had happened to me. But much later in life I learned that I suffered from a type 1 bipolar disorder.

To complete my doctoral research, I had built a low temperature helium gas-cooled molecular beam source for hydrogen molecules and its deuterated isotopes. I studied their magnetic hyperfine properties very close to zero external magnetic field, and used the lab's existing McIntosh high-fidelity audio amplifier as my *rf* transmitter. Norman's recent molecular beam students all had been very demanding of that faithful device. We ran it with such a large fully-resonant output current that the anode plates of its dual KT-88 tubes glowed a bright cherry-red color.

But when I came to finish my experiments and started to type up my PhD thesis, I developed a case of severe writer's block. My wife Jennifer never criticized me directly, but she did indirectly encourage me by secretly training our pet budgie to say the words "Write your thesis!" I found this out by surprise one morning when the budgie landed on my shoulder (as I was staring at the typewriter) and said those exhorting words.

Things progressed slowly with my writing, but the thesis was finally finished. I owe much to Jennifer for her typing of the final draft. In that day, there were no word processors available and the thesis was typed manually in triplicate using carbon paper, with handwritten symbols and equations inserted in the appropriate places. Thanks to Chester Carlson and his Xerox Corporation, there were good photocopiers available for making archival quality copies.

Using the values of my measurements of the hyperfine interaction constants in the molecules H_2, HD and D_2, a theorist at UC Davis, Roderick V Reid, and his student Amy Chu recalculated the spin-rotational interaction constant in hydrogen molecules using a properly gauge-invariant electromagnetic formulation. They showed that by using a transformation of coordinates to the position of the nucleus being observed, that the various relativistic spin-rotation corrections introduced by Ramsey were properly accounted for, and now agreed with experimental values. (R. V. Reid and A. Chu, Phys. Rev. A 9, 609 (1974)). Also, my observed value of the quadrupole interaction constant *eqQ* in the second rotational state of the deuterium molecule led to an improved value for the quadrupole moment of the deuteron. (R. V. Reid and M. L. Vaida, Phys. Rev. A 7, 1841 (1973).)

I defended my PhD thesis in June of 1970, and in early July my wife gave birth to our first son at Mount Auburn Hospital in Cambridge Mass. When the Ramseys came over to our apartment to see our new baby, Elinor helped my wife learn how to hold and burp him.

Soon, I had to ask Norman to write letters of recommendation for me for postdoctoral positions in Canada. He asked if he should mention my psychotic illness in these letters. I told him that my psychiatrist had stated that I had recovered from the episode and was unlikely to ever experience another. Norman seemed to accept this as a matter of fact, but I don't know what he said about me in those letters. I had complete trust in him that he would be fair.

Afterwards, we moved back to Canada, at first to Toronto in August, and then in early September on to Vancouver, where I had an NRC (Canada) postdoctoral fellowship in Physics at the University of British Columbia for

the 1970-71 academic year. Jennifer and I were only 26 at that time and we could manage a crazy schedule like that, and do many things in a short time. Of course, we had a lot of help from many other people to cope with these events.

Once you had been a student of Norman Ramsey, he continued to play an important part in the rest of your life. In July 1971, I was hired as an Assistant Professor on the Mississauga Campus of the University of Toronto, near the village of Erindale, but was cross-appointed to the main Department of Physics on the St. George Campus in the heart of Toronto. I continued to correspond with him, at least once a year. He visited Toronto several times over the following years. In November 1979, Norman came to the University of Toronto to present part of the Annual Welsh lectures in Physics. I remember him on that same trip taking time to visit my wife and me at our home in Mississauga. Our children were 3, 6 and 9, and Norman plunged in to play with them in our backyard. They still have happy memories of Mr. Ramsey's visit.

Earlier in 1979, on April 5th, Isidor Rabi himself came to Toronto to deliver a somewhat reflective Physics Colloquium on his post-graduate experience in mid-1920's Europe, while the ideas behind Quantum Mechanics were maturing. I obtained permission from him to record his lecture for later transcription. It was usual for one of the professors in the Department to act as host for the Colloquium speaker at dinner afterwards. It was arranged for me to be that professor, and the restaurant was to be Fenton's in Toronto, not far from the University.

As it turned out that night, only my wife and I accompanied Rabi for an intimate dinner. I did not tell Jennifer much about Rabi beforehand. She found him to be a very interesting and quite enjoyable person. But she would have been very apprehensive to talk with him so freely if she had known exactly how famous he was. We had a wonderful time with him. At one point in the evening, we talked about Harvard and he turned to me and commented that they had made his former student Julian Schwinger into a "Tin God".

A little later in 1983, his former students got a Christmas letter from Norman indicating that since Elinor had just passed away, he would no longer be

able to keep up with his correspondence as regularly as he had been accustomed to. In fact, he said there was nothing for him to look forward to but “death and taxes”. But life would not be so grim for Norman in the future. In 1985, he married Ellie Welch, and life became much better for him. And in 1989 he was jointly awarded the Nobel Prize in Physics "for the invention of the separated oscillatory fields method and its use in the hydrogen maser and other atomic clocks”. Norman generously distributed a portion of his prize winnings to each of his former and current graduate students and post-doctoral fellows, encouraging us to use our portion to get either an HP-45 calculator (the fanciest graphing model), a cordless telephone, or a CD player.

On the last weekend of July 1999, Jennifer and I had an opportunity to meet Ellie in person. She and Norman arrived by train in Toronto on Friday night following a cross-continental railway trip from Jasper, Alberta. We met them at Union Station, and they came with us to Mississauga for a visit all day Saturday. Ellie turned out to be a very warm and delightful person, and had a lovely visit with Jennifer. She was very interested in getting to know Norman’s “family” of former students and post-docs. We took them to the Robinson-Bray Tea Room in nearby Streetsville, where much to his delight Norman had English scones with Devon cream. That reminded him of his earlier sabbatical time at Oxford University. We also went to the Port Credit Harbor and its surrounding waterfront parks, where he admired the R/C model boats that hobbyists had built. Smiling at these model boats, he commented they reminded him of the toy boats that sailed in the pond of the Luxembourg Gardens in Paris. Early on Sunday morning they left by train to Buffalo/Niagara Falls, and they flew back to Boston from the Buffalo N.Y. airport.

After I retired at the end of June 2003, I mentioned to Norman, when he was an invited speaker at the ISMRM (International Society of Magnetic Resonance in Medicine) conference in Toronto in July 2003, that I still had a rough typed transcript of the historical talk that Rabi had given in 1979. This turned out to be the last time I saw him in person. In contrast to the advice I had been given at the time in 1979 by a senior colleague at Toronto not to work further on publishing this, Ramsey strongly encouraged me to get it into print. Finally, after much work, I had an appropriately edited version of this talk published in the August 2006 issue of ‘Physics Today’.

My fellow students and I treasure all those times we were privileged to share with Norman Foster Ramsey.

A Biographical Memoir of Norman Ramsey has been written by his former student and colleague, Daniel Kleppner, for the National Academy of Sciences in 2015, and is available online at http://www.nasonline.org/publications/biographical-memoirs/memoir-pdfs/ramsey-norman.pdf .

Other excellent obituaries for Norman Ramsey have been published, and among them is one written by David Wineland (Nature, 480, p182 (2011)), and another in the New York Times (November 6, 2011. http://www.nytimes.com/2011/11/07/us/norman-ramsey-dies-at-96-work-led-to-the-atomic-clock.html).

Acknowledgement - I wish to thank Professor Amar Vutha for encouraging me to put these memories in print, Patrick Gibbons and David Wineland for photographs and comments, and especially Art Olin and Irving Ozier for their helpful editorial suggestions.

Appendix: Photographs

Figure 1. The Ramsey Group, Summer 1966, Harvard Law Yard. Back Row (Left to Right) - Ed Uzgiris, Andrzej Chakulski, Tom English, Doug Brenner, Ashok Khosla, Randy Wolfe , Tom Follett, Dave Wineland, Norman Ramsey, Pat Gibbons, Paul Zitzewitz, Bill Edelstein, Roger Hegstrom; Middle Row: ?, Keith MacAdam, ?, Peter Moulton, Bob Hilborn, Peter Valberg, Charles Minter; Front: Frank Winkler, Fraser Code. (Photo by Fraser Code, scanned by David Wineland)

Figure 2. The Jefferson Library Guild 1966
Back Row (Left to Right) - A. Weinrub (Pres. and Founder), P. Carson, L. Price, D. Wineland; Middle Row - B. Burns, G. Brandenburg, P. Gibbons, G. Gladding; Front Row - J. Peech, L. Beeferman, W. Johnson, K. MacAdam. (Photo provided by Pat Gibbons)

Figure 3. Norman Ramsey carrying his surfboard on Horseneck Beach, Cape Cod. (Photo by Fraser Code, 1967)

Figure 4. The Ramsey Group, Lyman Laboratory, 1968
Back Row (Left to Right) - Keith MacAdam, Tom Follett, Norman Ramsey, John Vitkevich, Dave Wineland, Dan Larson, Paul Zitzewitz, Tom Gallagher, Pat Gibbons; Front Row - Fraser Code, Pierre Debely, Joe Cecchi, Bob Hilborn.
(Photo provided by David Wineland)

Figure 5. Norman and Elinor Ramsey attending the wedding reception for Pat and Jane Gibbons (August 1968)
Left to Right - ?, ?, Norman Ramsey, ?, Elinor Ramsey.
(Photo provided by Patrick Gibbons)

Figure 6. A tribute to David Wineland (Nobel Laureate, 2012) composed by his graduate school roommate Patrick Gibbons.
Top Row - Two portraits of David Wineland (1968 and 2012)
Bottom - Groomsmen at Patrick and Jane Gibbons' Wedding (1968)
Left to Right - ?, David Wineland, ?, Patrick Gibbons, ?, ?.