

Autobiography: the Early Years to 1975

Vernon L. Smith

I was born Vernon Lomax Smith in Wichita, on the flat plains of Kansas, January 1, 1927 in the years leading to the Great Depression. Like many of my generation I am a product of the strange circumstances of survival, and of successes, built on tragedy. This narrative is written from memory and impressions, which are notoriously subject to error. Therefore I have carefully verified certain dates and events from family records, newspaper clippings and published Wichita history, that provide some mileposts for accuracy checks.

Grover Bougher, my mother's first husband and father of her two oldest children, was a fireman—they shoveled coal and maintained their locomotive's steam pressure—on the Santa Fe railroad. I have a letter Grover wrote to his brother, George, a Private in the American Expeditionary Force, dated October 3, 1918, postmarked in Newton, Kansas on the 4th. On October 5, Grover was killed instantly in a train wreck – which were not uncommon at the time – when his passenger train engine and its cars were diverted, by a manual cutover switch left open, onto the sidetrack, colliding with a waiting freight engine. The letter was returned to Newton, postmarked the following April with a notation by the Command P.O. that George had been killed on September 17, 1918 fighting the war in France. Neither brother knew of the other's death. When Grover was killed, my grandfather to be had been laid up for some weeks with a badly injured leg caused by a railroad accident. He was an engineer on the Missouri Pacific Railroad, and was injured as he and his fireman leaped from the engine cab when one of the drive shafts bolted loose from the drive wheel and flailed up through the cab's wood flooring

Grover's tragedy proved pivotal. The life insurance money provided to my mother by the Santa Fe Railroad, and augmented by a retail job selling shoes, guaranteed a less arduous survival to a twenty-two year old widow with two girls, age 3 and 4.5. In those days aid to dependent children came from family and friends, in this case her parents, whose home she

moved into. My maternal grandfather and his twin brother had been orphaned at age 3-4, and were sent to a relative's nearby farm, boys being in demand for farm labor.

Fortune smiled for us all when my father Vernon Chessman Smith (1890-1954), a machinist who had apprenticed in Cleveland Ohio, met my mother, Lulu Belle Lomax (Bougher) (1896-1957) in Wichita, and was delighted to find a warm and caring woman already with a family. My mother often mentioned that she had not intended to have any more children, but my father so loved children, particularly her daughters, that it was unthinkable for her not to have one child by him. If I had been a girl, my name would have been Verna. My father had a brother, Norman (1888-1946), a wildcat driller who had followed his father, a tool dresser, into the oil fields, and a sister, Izella (1892-1918), who died in the great flu epidemic, two months after Grover Bougher had widowed my mother.

The life insurance money was invested in a farm located about 45 miles from Wichita, which was to become our sole means of survival during the difficult years, 1932-1934. A machinist with the Bridgeport Machine Co., maker of oil field equipment, my father was laid off in 1932 for lack of enough work, and the three of us moved to the farm located near Milan, Kansas. My older sisters remained in Wichita, one finishing High School, the other having left high school to become married in 1931.

The farm brought hard work and hard times for my parents—our house had no indoor water, electricity or toilet facilities—but for me it yielded memories of adventure, and learning about chickens (one of which I befriended as a pet until its penchant for following us into the house resulted in a broken neck as the screen door slammed shut behind my mother), milk cows, hogs, gardens, grain crops, priming well pumps, Coleman Lanterns and Kansas windstorms. The farm proved an invigorating childhood with ample opportunity for daily, fatherly lessons in 'how things work' – an interest I have carried the breadth of my life. I learned when and how to milk cows and put them to pasture, feed the hogs, chickens and horse (we could afford only one horse, and perforce had to borrow another for ploughing), and to tag along and watch my father

repair fences, gates, hog sheds, and barn doors, store hay in the barn loft and shoot rabbits for the table with his father's 1890 vintage lever action Winchester 12 Gauge shotgun.

From my mother I learned about cooking on a wood stove in the Kansas prairie where there is precious little wood, so we supplemented by burning dried corncobs, and—euphemistically—dried sun-baked 'cow chips.' The early settlers had burned Buffalo (sic, Bison) chips, as had the Indians before them. Decades latter I learned that the first Americans who crossed to Alaska on the land bridge from Siberia burned Mammoth chips, and I felt connected to those ancient peoples of 14,000 years ago. I can still memory-taste the fresh buttermilk pancakes and hot buttermilk biscuits—both made with lard!—that were cooked on the top, or in the oven, of that ancient iron stove. If the egg basket was empty, I can remember being sent to the chicken coop to gather fresh warm eggs, occasionally containing two yolks, for the pancakes. I also remember my mother axing the head of a chicken for the table, noting that she did not relish that necessity. Yes, long hours and a hard life for my parents, but for a six to seven year old every new day dawned with fresh excitement when you have not a care in the world, and so much to learn and witness.

At age six I took my place alongside more seasoned farm children for primary education in the classic rural one-room schoolhouse. A neighbor, Mr. Hemburger, had the distinction of being able to read and write, and therefore was deemed fully equipped to be our grade school teacher. A wise decision, I think, although my mother was always a bit irritated when he used the word "ain't," a completely grammatical contraction of which English has many; mom was something of a language maven. Each morning my teacher/neighbor faced six rows of desks. The first row on his right, where I sat, was grade 1, the second row to his left, grade 2 and so on for all six grades. After some first row recitation, and left with an assignment, I had the opportunity to listen in on the second and third grade recitation lessons – the grades seated closest to me. As I later became aware, this classroom implemented the original progressive system, in which you were part of a single seamless community consisting of six grades. At the end of grade 1, Mr. Hemburger gave me a note to take home to my mother, stating unceremoniously, that 'Vernon

can read the second grade reader and therefore next year he will be in the third grade.' There were of course only three subjects—reading, writing and arithmetic. Reading was the litmus test; if you were less strong in arithmetic, or writing, the next year you could participate along with those in the row on your left, before Mr. Hemberger got to your row. The whole purpose of this management style was to move each person along at her own pace of accomplishment, get her through school and into farm work where she could be useful. I understand that the earliest achievement tests showed high performance in Kansas and Nebraska because of these rural schools.

In 1934 my father returned to Bridgeport Machine for part time work, and subsequently full time work. This was fortuitous, as we lost ownership of the farm to the mortgage bank, unable to meet payments on the loan, and we had to move back to Wichita. This confirmed my mother's political commitment to socialism, but my father seemed to take it in stride. At some point in the 1930's this man refused to apply for help from WPA (the Works Project Administration—my grandfather always said that WPA meant 'we piddle around') because he considered it demeaning, a point of contention with my mother who thought he was being completely unreasonable. In 1940 he lost his job permanently when the independent entrepreneur owner of Bridgeport, A. A. Bushaw, closed his factory rather than cede control of his oil field equipment factory to President Franklin Roosevelt's defense industry production. (Of course, the plant was taken over—by Culiver Aircraft—and used for defense, then war production). My father changed employment to the Coleman Lantern Company for about a year, and then he went to work for Stearman Aircraft, which had been purchased by Boeing in 1938. Lloyd Stearman manufactured the famous PT-13 and PT-17 Kaydet biplane that was a U.S. Primary Trainer then and through much of WWII. In 1941 Boeing started construction of Plant II, where the B-29 was to be built, and the Stearman plant was renamed the Boeing-Wichita Plant I. By 1945, 10,000 B-29s and over 10,000 Kaydets had been manufactured by Boeing-Wichita.

Wichita and farm life were separated by location, intellectual and economic activity. The city had homegrown a surprising breadth of business life. Beech, Stearman and Cessna Aircraft,

Coleman Lantern Company, Fred Dold meatpacking, and the Fred Koch, Jack Vickers and other petroleum companies provided tangible initial evidence of the machinery of markets, specialization and globalization. Bold independent actions by Coleman, Cessna, Beech, Koch, and many others, instilled a mid-western sense of freedom and entrepreneurship (if not always unqualified patriotism, as with Bushaw). When Walter Beech died, his wife, Olive Ann Beech, co-founder of their company, took over the management of Beech Aircraft to become one of the great early women executives. She built it into an internationally successful manufacturer of light planes, and continued to be active on the Board well into her eighties.

After my stellar first grade academic achievements, I continued to perform well in the city primary schools—except for penmanship, which was not my forte. My school performance, however, deteriorated beginning in the eighth grade and all through high school. I found High school very uninspiring—girls were far and away more interesting—but I always expected to go to College. I vividly recall that my mother helped me with my English homework when we were learning to diagram sentences in the 9-10th grades, and complaining about the deterioration in the quality of the public schools, circa 1941-2. She had learned to diagram sentences in the 7-8th grades. It was not evident to me why one should learn it in any grade, but what did I know? I have often wondered whether my mother's socialist constructions would have survived her pragmatism, and the reality of poor performance in socialist controlled production as well as education.

In the 9th grade I began my first wage work for the West Side Drug store delivering prescriptions and sundries on my bicycle to customers who called in orders. I still have my original social security card signed when I was 13. (To my mother, social security was a great invention. Not realizing that it was just a tax, she wanted me to enroll early. Here I sit 63 years later receiving SS payments of \$1900 per month. I wonder what the payments would be if SS had been vested). Between deliveries I waited on customers, learned to 'soda jerk' on an old fashion drug store fountain (cokes, milk and malt shakes, ice cream Sundays and sodas, etc.). I was paid fifty cents for a six-hour shift plus occasional tips. In 1941 I finished at Allison Intermediate School

(grades 7-9), and started at North High School, commuting by bicycle about 5 miles from home to school. On the way was a restaurant and fountain called the OK Drive Inn, owned and operated by Don Eaton, who was a restaurateur known to my family for years. Throughout the 1930's my mother home-baked desserts that Don bought for his restaurant—angel food cake, chocolate devil food cake and Boston cream pie were most common—for 25 cents each. At age 14, with my soda fountain experience, Don offered me a job at \$1 per day, summers and weekends. That seemed like good wages to me. I remembered in the early 1930s my father made \$1 a day working as a temporary construction carpenter making and installing windows. I operated the fountain, learned to cook and Don later gave me a raise to \$8 per six-day week.

In the summer of 1943, at age 16, I applied for an entry level job at Boeing Aircraft, and went to work at an incredible—to me—starting wage of 60 cents per hour, with a 10 cent premium for working the grave yard shift from midnight to 8am. I was earning \$5.60 per day. I also attended summer school at North High so that when the fall term started I could graduate in January 1944 by taking only two courses. In that way I could finish High School and continue at Boeing. It was very demanding, but did not become burdensome until the following December when Boeing converted from three 8-hour shifts to two 10 hour shifts per day. I made it through, and two months later, on January 20, I graduated.

At Boeing, because of my high school training in electricity principles and practice, I was in the Functional Testing Department where I studied the training manuals for the Central Fire Control (gun operation) System on the B-29. It was the first high altitude bomber with pressurized cabin for the crew—pilot, co-pilot, engineer, bombardier, nose gunner, two side gunners, an upper gunner and a tail gunner. The gun turrets were each remotely controlled by gun sights with electro mechanical selsyn (self synchronous) motors located in the heated and pressurized cabin areas. My division had responsibility for trouble shooting the system and aligning the gun sights with the firing trajectories. We learned about compensating for windage jump as the spinning bullets emerged from the barrels into the high velocity air, and 'leading the target' to account for differential air speed between source and target. This was the first Buck Rogers armament

system, and it was fascinating. Some time after I got up to speed I requested and was granted transfer to the second shift flight crew. On this assignment, I would be part of the crew that did the final alignment checks on the system, and fire tested all the guns on the ground. There was a large abutment of stacked wood beams backed by an earth fill into which all the guns were fired. Each plane was set parallel to the firing range. The lower aft, lower forward and upper aft turrets (two fifty caliber machine guns in each) and the upper forward (four fifties) were aligned on the target range, and we fired 25 rounds through each gun. The plane was then turned 90 degrees and the tail turret, consisting of two fifties and a 20mm cannon, were also fire tested, except that the air force required 50 rounds to be fired through the cannon; I never quite understood why it was called a cannon, as it spit out rounds like a machine gun. We pulled all the barrels and loading bolts, cleaned and reinstalled them in the guns, the air force bought the plane, and it was flown to its base. I was turning 17 years old and Boeing was actually paying me to do this thing called 'work!' It ended in August 1944, after 15 months, when I resigned to begin college.

Our sole family 'intellectual' was my mother's uncle, Sullivan Lomax, who broke his leg as a child in a farm accident. It was not set properly, and left him handicapped. Being ill suited for arduous farm work, he studied law by a home correspondence course, passed the Kansas Bar exam, and practiced law in Cherryvale, Kansas his entire life. My mother and indeed all of us were proud of him! Consequently, neither I, nor my parents, or anyone in my family, or any neighbor or friend, had any idea how to go about choosing a college. So, I went to the city library, found a book on choosing a college, and learned among other things that the 'best' college in the United States was CalTech. Being naïve and impetuous I decided that I should prepare myself to enter CalTech, as, without preparation, my 'C' average in High School would not even qualify me to take the entrance exam. A serious Quaker College, Friend's University, was located near my home in West Wichita. I enrolled in physics, chemistry, calculus, astronomy and literature courses for one year, earned top grades, and sat for the entrance exams for CalTech.

The exam consisted of problems: how fast must a snowball be thrown against a wall to melt on impact? Lets see now, if the mass of the snowball is 'm,' and its velocity, 'v,' then its

kinetic energy is $(1/2) mv^2$. Since it takes C (was it 528?) calories of heat energy to convert each gram of snowball (ice) into water, you had only to equate Cm with the kinetic energy, and solve for 'v' with suitable account taken of units.

I passed, took the Santa Fe's California Limited passenger train west out of Newton through La Junta, Colorado, Tucumcary, Clovis, Gallop and Albuquerque New Mexico, Winslow, Flagstaff and Kingman, Arizona, and Barstow, California, arriving in Los Angeles in September, 1945. It was 'all the way on the Santa Fe,' as the concessionaire would announce in each car as he went through with peanuts, gum, candy, pop and cigarettes. This route would become familiar in the next four years, whether I was traveling by train or driving the parallel Route 66.

CalTech was a meat grinder like I could never have imagined. I studied night, day, weekends and survived hundreds of problems, but what a joy to take freshman chemistry from Linus Pauling, hear physics lectures by J. Robert Oppenheimer on his frequent visits to CalTech, attend a visiting lecture by Bertrand Russell, and regularly see von Karman, Anderson, Zwicky, Tolman, Millikan and other legendary figures of that time, on campus.

I was majoring in physics, but switched to electrical engineering, which was in the same division (Mathematics, Physics and EE) as a senior. In this way I did not have to take the dreaded "Smyth's course," required for physics majors, but not EE, and received my BS on schedule in 1949. At the time I relished the unbending facts and mathematics of physics/engineering. Then, as a senior, I took an economics course and found it very intriguing—you could actually learn something about the economic principles underlying the claims of socialism, capitalism and other such 'isms.' Curious about advanced economics, I went to the CalTech library, stumbled upon two books, Samuelson's *Foundations*, and von Mises' *Human Action*. From the former, it was clear that economics could be done like physics, but from the latter there seemed to be much in the way of reasoning that was not like physics. I also subscribed to the *Quarterly Journal of Economics*, and one of the first issues had a paper by Hollis Chenery on Engineering Production Functions. So, economics was also like engineering! I had not a hint then as to how much those

first impressions would be changed in my thinking over the decades to follow. But in 1962, my book, *Investment and Production*, would have a chapter on engineering production functions.

After graduating in engineering I went to the University of Kansas to get an MA in economics as a vehicle for allowing me to decide if I wanted to continue in economics. At KU I took classes from Dick Howey: price theory, math economics, imperfect competition, but significantly, a full year course in the Development of Economic Thought. Howey was a surviving member of an endangered species, a History of Economic Thought scholar, but it was from him that I learned what scholarship really meant. To be good at whatever you did, you needed to acquire knowledge of all the supporting structure, tools and primary sources of inspiration. If you were Howey, that meant knowing mathematics and being fluent in French, German and Italian. As one who just barely knew English, he much impressed me. His model seemed just right and it generalized to whatever might interest you. With Dick as a mentor, I decided economics was for me, and I continued by pursuing an economics Ph.D. at Harvard beginning in 1952. I met Joyce Harkleroad at KU. We were married in 1950. A year later she gave birth to twins, Deborah and Eric in that order, and I was part of a rapidly growing family.

At Harvard, I had macroeconomics from Alvin Hansen—the foremost American Keynesian, but he was also very eclectic. You read everything from Foster and Catchings to Hayek, and not only Keynes, his interpreters and critics—Hicks, Samuelson, Metzler, Friedman, etc. The Keynesian economics was tempered by the dry wit of Gottfried Haberler, the sarcasm of Wassily Leontief, Guy Orcutt's deeply serious search for the messages hidden in all data, Alexander Gershenkron who lectured on 'ven Breetan vas ze voikshop of ze woild,' and a coterie of graduate students trying to make sense of it all for their own careers. When Fritz Machlup visited, I wondered how the two polite Austrians—he and Haberler—would determine which one would get through a door first. Schumpeter was no longer alive, but his ghost was lurking in the halls with Haberler countering any claims that inflation ('ze monster' to Schumpeter), if not too large, was good for the soul and spirit of the economy.

For micro I supplemented with courses Samuelson taught down the Charles River at MIT. After Caltech, Harvard seemed easy, and I got virtually straight A's. I remember that my classmate Dick Quant and I always scored high, and close together, on exams. But at best we were vying for second—the top score always went to Barbara Jay, who married an artist and dropped out before dissertation time. Graduate school is an endurance test, but was not that demanding for me after having survived the undergraduate meat grinder.

In the Spring of 1955, as I was finishing my PhD dissertation, and Joyce was typing it, my second daughter, Torrie, was born, and in August we moved to my first teaching post, at Purdue University, nestled in comfortably familiar midwestern plains. In the Autumn semester, 1955, I taught Principles of Economics, and found it a challenge to convey basic microeconomic theory to students. Why/how could any market approximate a competitive equilibrium? I resolved that on the first day of class the following semester, I would try running a market experiment that would give the students an opportunity to experience an actual market, and me the opportunity to observe one in which I knew, but they did not know what were the alleged driving conditions of supply and demand in that market.

But let me backtrack to 1952. Many generations of Harvard graduate students had been exposed to E. H. Chamberlin's beginning graduate course in Monopolistic Competition. On the first day he would set the stage for the semester using a classroom demonstration experiment showing that competitive price theory was an unrealistic idealization of the real world. He gave half the class buyer reservation values, and the other half seller reservation costs. The value/cost environment was like Bohm-Bawerk's (1884/1959, book III, pp 207- 235; 432-436) representation of supply and demand in a horse market with multiple buyers and sellers in two-sided competition—perhaps Chamberlin's source of inspiration. I knew Bohm-Bawerk because of Dick Howey's course, but I did not pick up on this similarity until much later. Chamberlin, unlike Bohm-Bawerk's description, had the buyers and sellers circulate, form pairs, and bargain over a bilateral trade; if successful the price was posted on the blackboard; if not successful, each would seek a new trading partner. This continued until the market was closed. The prices in sequence were volatile

and failed to support the equilibrium prediction. Chamberlin used this first-day exercise to set the stage for his theory of monopolistic competition.

I decided to use the same value/cost setup but changed the institution. Secondly, I decided to repeat the experiment for several trading periods to allow the traders to obtain experience and to adapt over time, as in Marshall's conception of the dynamics of competition. For the institution, I reasoned that if you were going to show that the competitive model did not work, then you should choose a more competitive trading procedure, so that when the competitive model failed to predict the outcomes, you would have a stronger case than had been made by Chamberlin. I went to the Purdue Library and found a book by Lefler, *The Stock Market* (1951), giving details on the bid/ask double auction used in the stock and commodity Exchanges. In January 1956 I carried out my plan. To my amazement the experimental market converged 'quickly' to near the predicted equilibrium price and exchange volume, although there were 'only' 22 buyers and sellers, none of whom had any information on supply and demand except their own private cost or value. I thought perhaps that it was an accident of symmetry in the buyer and seller surpluses. I shot that idea down with an experiment later using a design in which the seller surplus was much greater than that of the buyers. Thus did I seem to have stumbled upon an engine for testing ideas inside and outside traditional economic theory.

Over the years 1956-1960 I continued to do many variations on this original experiment, altering the supply and demand environment, examining shifts in the demand or supply, varying the trading rules and introducing cash rewards showing that they made a difference. I gradually became persuaded that the subjects, without intending to, had revealed to me a basic truth about markets that was foreign to the literature of economics. I reported my early experiments—crude as they were compared with what I would later learn to do—in a paper, accepted for publication in *The Journal of Political Economy*, 1962, after two revisions, four negative referee reports and an initial rejection. (See, Smith, *Papers in Experimental Economics*, Cambridge University Press, 1991, pp 157-158).

During these years most of my research and teaching dealt with capital and investment theory, and corresponding pricing problems (Smith, 1961, *Investment and Production*, Harvard University Press). In 1961-1962 I was a visiting associate professor at Stanford, and at the beginning of the Autumn quarter had the significant experience of meeting Sydney Siegel and discovering that we had both been doing experimental economics. Unknown to both of us at the time, Reinhard Selten had also been pioneering economics experiments in Germany. Syd—a truly powerful experimental intellect—strongly influenced me in becoming committed to experimental economics, but he died unexpectedly at age 45 within a few weeks of our meeting. Eventually, I read all of his publications, including his classic, *Nonparametric Statistics*, and his two books coauthored with L. Fouraker (1960, 1963). Syd was not only a master experimentalist; he also used theory and statistics with skill in the design and analysis of experiments.

The growth of my research interests in experiment, together with a modest literature by myself, Siegel, Fouraker and Siegel, Edwards in choice under uncertainty, Anatol Rapoport in Prisoners Dilemma, and others in experimental matrix games, led me to initiate a graduate seminar in experimental economics at Purdue in 1963, which continued until my departure in 1967. In that first seminar I had thirteen students, including Don Rice, Hugo Sonnenschein, Norm Weldon, and Tom Muench, whose careers I have followed. I published new papers in experimental economics in 1964 (with Don Rice), another in 1964, then in 1965 and 1967. Several working papers by students and faculty were also spawned by this effort. In 1963 and again in 1964, under the enabling and supporting influence of Dick Cyert, Jim March (and probably Herb Simon in the background), Lester Lave and I conducted Ford Foundation faculty summer research workshops at Carnegie Mellon. With several experimental papers in the pipeline, and a seminar going, experimental economics was becoming much more than a hobby for me.

In 1967 Joyce, Eric, Deborah Torrie and I moved to Sherborn, Massachusetts, where we would live until 1972, with Joyce serving in her first position as a Unitarian minister. I was able to get a tenured professorship, first at Brown University, then in 1968 at the University of

Massachusetts. Our children had finished high school by 1972, and I accepted a fellowship at the Center for Advanced Study in the Behavioral Sciences, 1972-3. During all of this period my research turned to the economics of uncertainty, financial theory of the firm, and natural resource economics, but I continued to think about experimental economics and to use it in teaching. This was good in that my brain was continuing to work on experiments, and I was developing a fresh perspective. Also, Charles Plott and I talked experiment (for example the idea of induced valuation) on many bass fishing trips to Lake Powell. At the Center, thanks to the encouragement of the anthropologist, Bob Heiser, I wrote my 'Pleistocene Extinctions' paper, and submitted it to the AER. After a year's delay I received a letter of rejection along with three favorable referee reports. The editor explained that he had been unconvinced by the first two, sought a third report, and was still not convinced. So I sent it to the JPE (1975) along with the letter and the three reports; the editor sent it to one referee, it was accepted and published in about six weeks.

While at the Center I continued to be in touch with Charles Plott, who wanted to join me in 'getting into experimental economics.' CalTech offered me a Sherman Fairchild Distinguished Scholar position for one year, and this provided the vehicle for us to offer a seminar for student credit in the Spring quarter of 1974. Charlie and I updated my old outline and notes from Purdue, and we had regular meetings attended by three paying customers (including an undergraduate, Ross Miller), and several faculty, including Mo Fiorina, John Ferjohn, Roger Noll, Jim Quirk, Lance Davis and Bill Riker, also a visiting Fairchild scholar that academic year from Rochester. Bill had done some political science game theory experiments and we were off and running. Afterwards at CalTech, experiments, including political economy, would be central to their teaching and research program. Later Bill reported that he had agreed to write a paper for an editor on experimental methods in political science, but after the course reversed his decision, because the seminar had changed his thinking about experiments, and he wanted to reflect more on the experience. I think that story summarizes well the intellectual ferment produced that semester.

As an interesting side-note, I had developed what I called the “Theory of Induced Valuation,” in lecture notes in my Purdue Seminar sometime in the period 1963-65. I talked from those notes in our CalTech seminar in 1974. Charlie pointed out to me that these ideas were catching hold and he and others needed something to cite so at some point I included them in a methods write-up in the CalTech working paper series. Later, I did a write-up of induced valuation for the AER (1976) and it was finally published long after its original genesis. The Nobel citation included that paper.

Except for some loose publication ends for me to complete in resource economics, I was firmly back into experiments, and Charlie, Mo, John and others were creating experimental public choice. I stayed on in 1974-75 with a joint appointment at CalTech and USC, we wrote up our experiments for Miller, Plott and Smith (QJE, 1977) (this must have been the first scientific paper in economics with an undergraduate coauthor), Charlie and I wrote our paper on comparing institutions for the RES (1978), and I started the experiments that would lead to a series of papers testing the incentive properties of various public good mechanisms (1977-1984).

I considered staying on at CalTech; also, going to Northwestern, where I had close friends like John Hughes and Stan Reiter, but I feared the tug of a silver cord, and that ‘you can’t go home again.’ I was looking for new opportunity; I could not describe it, but I thought I would know it when I saw it. In 1974 I gave a seminar at the University of Arizona. Rene Manes, a student from Purdue days, was Dean of the College of Business, and they were interested in bringing me to Arizona. I returned in 1975 to give another seminar, visited with their administration, and sensed that this was what I was looking for. They had some recruiting successes in the College, but had much work ahead in building the faculty. Most impressive, however, was a committed top administration: John Schafer, President; Gary Munsinger as Vice President, and Al Weaver, a tough minded Provost. I decided to move to Tucson.

In retrospect that was a good decision. I was there for 26 years, before leaving for George Mason University. That is a long and exciting story that continued the basic work begun

at Purdue, which formed the primary citation by the Nobel Foundation. That story begs to be told, but I cannot possibly do it justice in the limited time and space I have for this narrative.

Stay tuned.