Remembrances of Life as Young Husband and Mathematician

by

Wendell H. Fleming

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1. West Lafayette, Indiana, 1948-1949.

Flo Tatum and I were married at 2:30 on Sunday April 4, 1948. It was a beautiful early spring day. The wedding was held in the Baptist Church in Chalmers, which is the village where Flo's mother grew up. It is located a few miles north of West Lafayette. Flo wore a brown suit, which was considered more practical than a traditional bride's dress since the suit could be worn again on later occasions. After the ceremony, there was a reception at the Tatum home on their farm east of Brookston, attended by relatives and friends from the community.

Since Flo and I had no car in 1948, after the reception my parents delivered us to our "apartment" on Sheetz Street in West Lafayette. April 5-9 was Purdue spring vacation. My father offered to drive me to Tatum's early Monday morning, so that I could help Flo's Dad with the farm work. This offer was respectfully declined.

When we got married, I was still an undergraduate student at Purdue. Flo's job at the Purdue Library was our only source of income. Our application for a marriage license included the question "What means of supporting your wife do you have?" My truthful answer," None" was the wrong one. However, the clerk improvised another answer ("parents, savings?") and gave us the license anyway. Soon after the wedding, I had the promise of a Teaching Assistant job in the Math Department at Purdue for the coming academic year 1948-1949.

Our "apartment" on Sheetz Street in West Lafayette consisted of two small rooms with a two burner gas stove and sink. It was conveniently located, only a block from the Purdue campus. The couch folded out to make an uncomfortable bed, and we shared a dingy bathroom with the owner's family. Flo had gotten this apartment after her brother Carl and wife Alyce vacated it in the fall of 1947. Despite the discomforts, we were lucky to have it, considering the dire shortage of married student housing. Like other universities, enrollments at Purdue rose dramatically after World War II. Among the students were many war veterans, supported by the GI Education Bill, and many of them were married.

In August 1948, we took a belated honeymoon trip by bus to the Great Smoky Mountains in Tennessee. Among our many trips together in the 57 years since then, this trip was probably the least successful. Without a car, our freedom to explore was badly limited. We both got digestive upsets from eating in a cheap and no doubt unsanitary restaurant. There was also an episode eating of candy which ants had invaded. We were relieved to return to our humble quarters on Sheetz Street.

I finished the Bachelor of Science degree in Mathematics at Purdue in the summer of 1948 and began work on a Master's degree. Before we married, I lived at Chauncey Coop House. Ralph Partington, who was a graduate student in Mathematical Statistics, also lived at Chauncey. It was Ralph who first gave me the idea of continuing my study of mathematics in graduate school. Like many others of my generation, I had no well thought out long term career plans. Mathematics was something which I liked very much, and which I could learn reasonably well. The chance to continue with it, and to enjoy newly married life with Flo was all I could ask for in the fall of 1948.

My Masters degree program at Purdue was satisfying, although rather heavy in course requirements. Arthur Rosenthal's lectures in mathematical analysis were particularly lucid and inspiring. Michael Golomb's course in calculus of variations was the first step toward my lifelong interest in that topic. The homework sets which he assigned were rather intimidating, however.

Both Professors Rosenthal and Golomb were refugees from Nazi Germany. They were fine people, as well as good mathematicians. Science and mathematics in the US benefitted enormously from the influx of refugees from Germany and other European countries during the Nazi years.

As a brand new 20 year old teaching assistant, I was given complete charge of two elementary mathematics classes in Fall 1948. I don't recall that there was any faculty guidance or supervision beyond the course syllabus and choice of textbook. One class was pre-calculus for freshman engineering and science students. The other class was at a lower level, and was intended to satisfy a university-wide general education requirement. Many of these students had poor math skills, and in some instances the terms "fear and loathing" fairly describe their feelings about mathematics. At the start of the semester, I had naïve ideas about giving this weaker class some feel for modern trends in algebra which I had been learning in my own course work. These ideas were abandoned after I tried (but failed) to convince a student in my class that $\frac{1}{2}$ plus $\frac{1}{3}$ does not equal $\frac{1}{5}$. He assured me that he had graduated with a "B" average in mathematics from an accredited high school in Indiana. Nearly 50 years later, I was a member of an Advisory Committee for the Mathematics Department at Purdue. At one meeting, there was a long discussion of the Department's difficulties and frustrations with a general education course which was quite similar to the one I taught in 1948. This discussion brought a strong feeling of "déjà vu."

I decided to continue toward a Ph.D. in Mathematics after finishing the Masters degree. Purdue did not seem the right place to do this. Professor A. H. Smith offered friendly advice about suitable places to apply. He had been my teacher in a fine undergraduate advanced calculus course, and also was in charge of the Mathematics Library in which Flo worked for a time.

Offers of financial support for Ph.D. study came early in 1949. The first was in the form of a telegram from the University of Oregon, which made a favorable impression. Wisconsin offered a WARF (Wisconsin Alumni Research Foundation) assistantship, with no teaching duties. Wisconsin was among my top choices, and all other offers were for teaching assistantships. I accepted the WARF assistantship, a good choice which I have never regretted.

In Spring 1949, Flo and I bought our first car – a pre-war Plymouth. We traded it in during the summer and bought a new black Plymouth. This car gave us five years of trouble free driving until a car salesman persuaded me (against my better judgment) to trade it in for a new 1954 Plymouth. We paid for the 1949 Plymouth with money which my parents had helped me save for college, but which I had not used. My parents were perhaps shocked at this apparent extravagance, but the 1949 Plymouth turned out to be a very good investment. It provided trouble-free transportation at a time when we had no money to spare for auto repairs. In August 1949, Flo and I loaded everything we owned into the Plymouth and departed for Madison, Wisconsin.

2. Madison, Wisconsin, 1949-1951

The University of Wisconsin in Madison has a well earned reputation of excellence in scholarship, in many fields including mathematics. The State of Wisconsin is less wealthy and less populous than states such as Michigan, Illinois and California, with state universities which compete with the University of Wisconsin. However, Wisconsin has a long tradition of support for education, as well as for encouraging independent thought and unconventional ideas. There was a strong liberal tradition in politics too. In 1924, Wisconsin Senator Lafollette received nearly 5 million votes for President of the US as the Progressive Party candidate.

Madison is a beautiful city, situated between Lake Mendota and Lake Monona, and the University campus is well situated on high ground above Lake Mendota. When Flo and I arrived in 1949, we encountered a married student housing shortage which was perhaps even more severe than at Purdue. One place which we looked at was a former cow barn, divided into living units with shared kitchen and bathroom facilities. We finally rented a summer cottage at 2724 Waunona Way, on the south shore of Lake Monona. The view across the lake toward downtown Madison was fine, but the cottage had some shortcomings. We "winterized" it by covering the windows with plastic, and kept reasonably warm with an oil-fired heater. Bathing was in a shower room at the end of a row of cottages. On winter days when the temperature was below zero degrees Fahrenheit, this encouraged a quick run back to the cottage after a shower. Flo found a job at the State Laboratory of Hygiene, which tested for disease cultures and tissue samples from around the state of Wisconsin. Among the Laboratory staff, the phrase "that's a fine specimen" had a special connotation, not complementary if applied to a person. Since the Laboratory was on campus, Flo and I commuted to the University together.

In 1949, the Wisconsin Mathematics Department had a rather small but distinguished permanent faculty. Faculty members taught three courses per semester, in addition to supervision of Ph.D. students and their own research. Nowadays, this would be considered a very heavy load for a "research" university. The department was housed in North Hall, which was the oldest building on campus and was overcrowded. I shared space with a dozen or so other Mathematics graduate students in a room which was converted later into a men's lavatory. This meant that I studied either in the library or at home. Despite these shortcomings in the physical environment, I found Wisconsin a very good place to foster my mathematical learning.

The academic year 1949-1950 was devoted to coursework. Robert Fullerton's course on general topology opened new vistas, and taught me how to think "abstractly" in a mathematical setting where intuition is not a helpful guide. Lamberto Cesari visited from Italy in the second semester. His course on calculus of variations and seminar fostered my interest in that area. Soon after, Cesari moved to Purdue University, and I joined his group there in 1955.

My Ph.D. thesis advisor was Laurence C. Young. He was an outstanding mathematician, and an interesting personality. L. C. Young lived to age 95. An obituary article about him appeared in the *London Math. Society Bulletin*, vol. 36 (2004), pages 413-424, with his daughter Sylvia Wiegand and myself as coauthors. L. C. Young was unusually vigorous, both physically and intellectually. His course in real analysis met at 7:45 a.m. during the winter of 1949-50. He sometimes arrived rosy-cheeked and out of breath, having skated several miles over Lake Mendota ice from the Young family home on the lakeshore. In the Spring 1950, Professor Young gave an inspiring lecture on his theory of "generalized surfaces." After that lecture, I asked him to be my Ph.D. thesis supervisor and he agreed.

All Ph.D. candidates had to pass an oral "preliminary exam" before starting work toward a thesis. The prelim is a humbling experience for most students, and former students still recall many years later prelim questions which they could not answer. In my case, I was caught by an unexpected question which Professor Young asked concerning the theory of complex analytic functions. I had not thought this to be within the scope of my prelim, and couldn't answer it. Nonetheless, I passed the prelim.

After the prelim, Flo and I took a brief holiday in Door County, WI along the Lake Michigan shore. We also returned to Indiana for family visits several times while we were in Madison and enjoyed day trips through the beautiful Wisconsin countryside. A favorite spot was Devil's Lake State Park near Baraboo.

My second year at Wisconsin was devoted to reading, advanced seminars and thesis research. Particularly influential was the seminar on Schwartz's Theory of Distributions, organized by Professor Eberlein. My classmates (and afterward long time friends) Bill Donoghue and Kennan Smith gave the seminar lectures. Schwartz's theory was quite new in 1950. It gave a completely new way of looking at many problems in mathematical analysis.

One day in Fall 1950, L. C. Young mentioned some conjectures about generalized surfaces on which he was working. A few weeks afterward, I found a way to answer them, and these results were the core of my Ph.D. thesis. Later, Professor Young jokingly said that I "stole his problem." A revised form of the thesis later appeared as a journal article in the *Transactions of American Mathematics Society*, with Young and me as coauthors. Probably this thesis would not have been possible without the Schwartz distribution theory seminar. Another key idea came after a literature search, in which I found a related paper by E. Hopf and W. Damkoeler.

Lawrence Young was a good mentor. He steered me away from the subject of "surface area theory" which was popular then, but which reached a dead end soon afterward. Once when I was suggesting a possible approach to a problem, he said (correctly) that I wouldn't get anywhere with a problem that difficult with such simplistic methods. On the other hand, Professor Young was not a "hands on" thesis supervisor. Unlike some of his colleagues, he did not schedule regular sessions to monitor the progress of his Ph.D. students. Most importantly, he led me toward a new approach to multi-dimensional geometric problems in the calculus of variations, based on his own theory of generalized surfaces. Methods of this kind later evolved as the basis of the subject of "geometric measure theory," which has flourished ever since.

The winter of 1949-50 was "mild" by Madison standards. The temperature did not fall below minus 15°F. However, the winter of 1950-51 was more severely cold. Many nights, temperatures fell below minus 20°F, and one morning we woke to a temperature of minus 37°F. The antifreeze in the car radiator had turned to slush. Fortunately, we had moved to a more comfortable apartment upstairs in the main house at 2724 Waunona Way, and thus we avoided repeating our first winter's experiences dashing back to the cottage after taking a shower. At times the severe weather of winter 1950-51 led to feelings of "cabin fever" and "winter time blues."

At Purdue, I had completed the Air Force ROTC program, after which I was commissioned as a Second Lieutenant in the Air Force reserve. The Korean War began in June 1950, and I expected a call to active duty. None came until March 1951, when I came home to find Flo in tears and an order for me to report to Scott Air Force Base near St. Louis for pre-active duty processing. Before leaving Madison, I had only two days to prepare a case to argue for a few months delay to finish my thesis. My appeal was heard by a sympathetic Air Force Major, and was granted.

Shortly before my Air Force orders came, my future boss, Alex Mood had visited Wisconsin interviewing math graduate students for possible jobs at the RAND Corporation. Although I had never heard of RAND, the Math Department secretary persuaded me to sign up for an interview. During my interview, I mentioned my Air Force Reserve status. Mood said that RAND was heavily involved in long-range Air Force research and development problems, and that RAND had been very successful in getting exemptions from military service for its young scientific staff members.

After returning from Scott AFB, the RAND possibility seemed very appealing and I anxiously awaited news. One day a letter from RAND came with a job offer with a salary of $6,000 per year. This was five times our current income, and we assumed (quite wrongly) that it would be more money than we could possibly spend. A later section on my time at RAND says something about my work there. Because of my vision, I could not have qualified for Air Force flight duty. I am convinced that my work at RAND contributed much more to the US defense effort than whatever I might have done as a ground-based Lieutenant in the Air Force.

The task of organizing my results in the form of a coherent Ph.D thesis turned out to be much more difficult than I expected. L. C. Young said that the first draft which I gave him was "unreadable," and he was certainly correct. We had to delay by several weeks our departure for Santa Monica, CA to begin my job at RAND. Our landlord evicted us from the apartment, saying that the space was needed for visiting family members. We moved to temporary quarters, where Flo sweltered through midsummer heat typing the thesis (original and carbon copies). Such conveniences as air-conditioning, copy machines and computerized text processing were not available in 1951. At the last minute, it was discovered that I had misspelled the word "admissible." L. C. Young (who was English) gently pointed out that "we don't spell it that way in England, but perhaps it's different in the US." Since "admissible" appeared often in the thesis, Flo had to retype many pages.

The thesis was finally finished in August 1951, and I successfully defended it. We set off for Santa Monica in our Plymouth. Despite our penury during the first three years of marriage, we had accumulated too much to fit into the car. Our worldly goods were shipped to Santa Monica in some large barrels.

I left Wisconsin with warm feelings and gratitude for the fine mathematical education which I got there. In the years since then, I was saddened to see that Wisconsin was not exempt from assaults by extremist political groups. These groups had agendas which were hostile to Wisconsin's great liberal traditions, and which sometimes threatened the University's standing as a great educational and research center. Wisconsin's Senator Joseph McCarthy was a right wing demagogue, who was a key figure in fomenting anti-communist hysteria in the years after WWII. (I say more about McCarthy in a later section.) There were also left-wing political activists in Madison during my graduate student days. Our next door neighbors during our first year in Madison were staunch leftists, who followed the Stalinist party line unwaveringly. During the Vietnam War years, leftist activism in Madison grew enormously, and eventually turned violent. The climax was the destructive bombing in 1970 of the Math Research Center next door to the Mathematics Department. I had worked in the MRC building during my visit to Wisconsin in 1962-63, and we had good friends in the Wisconsin Math Department. This violent act was particularly upsetting for us, even though we shared some anti-war feelings with the protestors.

3. Trip to California, August 1951.

Our trip from Madison to Santa Monica was a great adventure. In a real sense, it was the honeymoon which we had missed. In 1951, the Interstate highway system had not yet been built. We followed US numbered highways to the Badlands of South Dakota, and then on to Yellowstone National Park. The vistas of prairies, deserts, rivers and mountains became grander as we continued west. Yellowstone lived up to its reputation, with marvelous geysers, hot pools, and waterfalls. Many tourists stopped along park roads to offer handouts to bears waiting by the

roadside. In later years, this custom created so many traffic jams that the Yellowstone bears were deported to remote sections of the park.

We had planned to continue south from Yellowstone to see the Grand Teton mountains, but the road was washed out. Instead, we continued through Idaho, Montana and Washington toward the Cascade mountain range in the Pacific Northwest. While we were still many miles from the Cascades, we saw Mt. Rainier rising majestically in the distance. We arranged for a motel in Yakima, WA, the center of a major apple growing region just east of the Cascades. Then we spent a glorious day in wildflower meadows on the flank of Mt. Rainier. We were lucky to have bright sunshine that day. When we left Mt. Rainier, we promised to return, and later we did return twice. The first time was in 1954, camping for several days in fog and drizzle with 6 month old Randy. Some women who passed our campsite looked into Randy's baby carriage and commented, "Poor little thing." Finally, in 1989 Flo and I returned again to Mt. Rainier to another spectacular wildflower display. We were initially in the clouds, but managed to climb above them for another memorable day.

From Yakima, we drove to Grants Pass, OR to visit Flo's Uncle Billy and Aunt Laura. This was the first of several visits to Grants Pass, during which we enjoyed their warm hospitality as well as wild berry picking, swims in the Rogue River and Pacific Northwest scenery.

From Grants Pass, we followed US 199 to the Pacific Ocean. The last stretch of this route follows the beautiful Smith River, which tumbles out of the mountains down to the sea. We then followed US 101 and California Route 1 all the way to Santa Monica. Coastal California is typically foggy in late summer. We saw very little of the famous California sunshine during this part of the trip. In particular, it was a letdown to miss the spectacular view of the Golden Gate Bridge which one gets on clear days when approaching San Francisco from the north on US 101.

After arriving in Santa Monica, we soon found a furnished apartment in nearby Pacific Palisades. I joined a carpool for a pleasant and easy commute to my new job at RAND.

4. RAND, 1951-53.

The RAND Corporation was established just after WWII. Its purpose was to advise the US Air Force on a broad range of strategic, operational and technical issues. Among RAND's earliest successes was a 1946 study which explored possibilities of satellites to orbit the earth and future space exploration. This was more than 10 years before the first Soviet Union Sputnik satellite was launched. Although RAND was supported by Air Force funds, it operated completely independently of Air Force command structures. When I arrived in September 1951, RAND staff members enjoyed great freedom in choosing research topics. The Mathematics Division at RAND had an excellent staff in 1951, as well as such top-notch part time consultants as Richard Bellman, Sam Karlin, and John Nash. It was an exciting working environment.

In 1951, the Von Neumann-Morgenstern theory of games of strategy was only a few years old. Although their work was initially motivated by applications in economics, game theory seemed of possible military interest as well. Several Mathematics Division staff at RAND were working on game theory, and I chose to do so too. Eventually, this led to my long term (though sporadic) interest in the topic of differential games, and their connections with ideas of risk sensitivity in control systems.

Life in Southern California. As a young married couple with no children as yet, we found many interesting things to do. There were various cultural opportunities from which we could choose. The Santa Monica community theater was excellent, with talented actors drawn to the Los Angeles area by the entertainment industry. Westwood Village near UCLA was a good place to go for foreign films. Somewhat farther away were events such as music concerts at the Hollywood Bowl.

Southern California was a great place for outdoor activities. There are fine beaches in Santa Monica as well as up and down the coast nearby. RAND was only a block from the beach, and walks could be enjoyed at lunchtime. In the winter months, we took hikes in the Santa Monica Mountains immediately behind Pacific Palisades where we lived. (Rattlesnakes were reported to be active along those trails in the summer months.)

On weekends and holidays, we took memorable trips to deserts and mountains outside the Los Angeles metropolitan area. Often these involved tent camping. A favorite place for cool season camping excursions was Borrego State Park, in the desert southeast of Los Angeles. The fascinating desert vegetation was completely unlike anything we had experienced. The winter of 1951-52 was unusually wet in Southern California, and the display of desert wildflowers the following spring was spectacular.

In the summer of 1952 we travelled to Yosemite and Sequoia National Parks, in the Sierra Nevada mountains. A highlight of this trip was a long hike to the place where Yosemite Creek plunges over the rim of the plateau high above Yosemite Valley, to begin its long fall to the valley floor. We cooled off with a swim in the creek. We fell in love with the High Sierra during this 1952 trip. Fortunately, we were able to return with our sons several times during the 1960s and 1970s for memorable High Sierra hiking and backpacking trips.

Strategic air-base study. The RAND Corporation is one of the earliest and best known examples of the kinds of organizations called "think tanks." At think tanks, staff members were free to ponder large scientific or policy issues, without the kinds of pressures and distractions typical in university or business environments. Think tanks addressed matters of national economic or defense interest, and also sociological and environmental issues. RAND itself later opened an office in New York City to provide advice to the Mayor.

There were many very bright people at RAND in the 1950s, and most of them were young. There was an aura of brashness, which sometimes bordered on arrogance. There was the heady feeling that raw intelligence could overcome a lack of extensive practical experience or technical knowledge in finding answers to great world problems. One joke which circulated at RAND had a staff member beginning his presentation as follows: "Consider two worlds W_1 and W_2…"

At lunchtime, a popular pastime was a game called Kriegspiel. It has the same moves as chess, but with quite different information rules. In Kriegspiel each player has his own chess board, and a partition blocks the view of his opponent's board. The players are given only the minimum information about their opponent's position and moves necessary for play to continue. Kriegspiel was considered a kind of intellectual substitute for combat, which might enhance the players' skills for contributing to RAND studies of military operations.

By the time I arrived, RAND had a good reputation for organizing interdisciplinary teams to study long-range strategic and technological issues of interest to the Air Force. In 1952, the Cold War was in full force, and the USSR had recently made nuclear weapons. The US strategic bomber fleet was considered the main deterrent to possible attacks by our foe. Our long-range bombers could operate from overseas bases in Europe, North Africa, and the Far East. An alternative strategy was

for them to operate from bases in the US in case of war, using aerial refueling from tanker planes. RAND was asked to study which of the two strategies (overseas bases versus aerial refueling) would be more cost effective. Two RAND teams were formed for this purpose, and I was urged to join one of them. The team leader was Igor Ansoff, who had a Brown University Ph.D. in Applied Mathematics. Other members of our team included engineers, an economist and an Air Force colonel.

During our study, it became evident that overseas US airbases were extremely vulnerable to enemy attack. "Cost effectiveness" was the wrong question to ask when our overseas bases might be gone within the first hour of a possible war with the USSR. In 1952, airbase defense against possible nuclear attack was a new issue. Moreover, post WWII thinking in the Air Force had been conditioned by the fact that during that war US air bases were seldom attacked by enemy planes (except in the early months of our involvement).

The other RAND team, led by Harry Rowen and Albert Wohlstetter, finished their report before we did. Their conclusions were similar to ours. Wohlstetter's briefing of Air Force management at the highest level made a great impact, and led to a major rethinking about US strategic Air Force deployment. In his book, "Think Tanks," Paul Dickson calls the Rowen-Wohlstetter study, "The most important job probably ever performed by RAND (or any other military think tank for that matter) in the area of strategic thinking…" (Balentine Books, 1971, p. 59).

After the spectacular success of the Rowen-Wohlstetter report, the RAND management decided that a second report might detract from the impact of the first one. One day in 1953, our team was thanked for our labors and was told that our report would never appear.

Important family matters. Flo and I hoped for children, but medical exams before we left Madison indicated an obstacle. After we reached California, Flo contacted Dr. Williams, who was a skilled, caring gynecologist and refugee from the Nazis. She underwent corrective surgery in November 1951, followed by another operation in February 1952 to remove abdominal adhesions which formed after the first one. After an uncomfortable recovery period, she regained normal health. By summer 1952, Flo was ready for the mountain excursions already mentioned.

The surgical procedure was a success. Randy was born on February 26, 1954, followed by Dan on September 3, 1957, and Bill on January 31, 1960.

5. Madison and Santa Monica, 1953-55.

In the fall of 1953, L. C. Young offered me a half year visiting postdoctoral research position at Wisconsin. In my spare time, I had been rewriting my thesis for publication and trying to work on related ideas. This seemed a good chance to continue full time for a short while. RAND gave me a 6 months leave of absence, which we spent in Madison from December 1953 to May 1954. During this time, Professor Young and I had profitable conversations, which developed later into joint research papers. Otherwise, the visit was less satisfying mathematically than I had hoped. The University of Wisconsin was in a period of retrenchment, and there were very few young mathematics faculty or post-docs with whom I could exchange ideas. I was disappointed that only one short published research note came as an immediate consequence of this visit.

Dr. Williams thought that a long drive to the Midwest while Flo was pregnant might be risky. She flew to Chicago and waited for me at her parents' place. I drove alone from California to Indiana. The most scenic part of the trip was through Arizona, including the spectacular Oak Creek Canyon.

When we arrived in Madison, we scarcely knew anyone except the Youngs. Our friends from graduate school days had left. We rented an unfurnished apartment (except for stove and refrigerator) and bought a minimal amount of cheap used furniture, just enough to get by for a few months. Flo's pregnancy was uneventful until the last month, during which there were some worrisome times. Randy arrived safely at the University of Wisconsin hospital on February 26, just after midnight. A few days later we began intensive "on the job training," learning how to care for a new infant. Looking back more than 50 years later, I am forever indebted to her for the wonderful job she did nurturing our sons (as well as caring for my needs). She is very good with the grandchildren too.

L. C. Young's wife Elizabeth was a warm, gentle English lady. She showed us many kindnesses during this 6 months visit, and afterward. Randy's first bed after coming home from the hospital was a basket, which babies in the Young family had used before (Laurence and Elizabeth had six children.) In 1964 we visited the Youngs en route to our home in Rhode Island from a summer in Palo Alto, CA. It happened to be Dan's 7th birthday. Elizabeth organized an impromptu party for him, with cake and candles.

Anti-communist hysteria. After World War II, a wave of anti-communist hysteria swept the US. It was fomented by unscrupulous politicians, including Wisconsin Senator Joseph McCarthy and Richard Nixon, who was then a member of the US House Un-American Activities Committee. There were indeed some extreme leftists in the US, and a very few of them were convicted of spying for the USSR. However, many innocent people were accused of endangering the US through communist activities. Often they lost their jobs, and some went to prison for refusing to cooperate with their accusers. Charges of anti-American activities were sent to the FBI anonymously. Until late 1953, those accused had no legal recourse, and could not even find out the exact nature of the charges against them. One distinguished member of the RAND staff had to move to the "uncleared" part of the RAND building for several months. The charge turned out to be that his brother-in-law was a communist. This was true. However, his brother-in-law was aged 13 when our colleague had last seen him.

Matters came to a head early in 1954. Senator McCarthy held a series of televised hearings, which I watched while Flo was in the hospital with baby Randy. Television showed Senator McCarthy and his staff as no better than a herd of pigs. Soon after these hearings he rapidly lost public support. This shameful episode in American history wound down to an end a few months later.

Return to California. We left Madison at the end of May 1954 in our new Dodge sedan to return to California. The route went via Denver, then through the Rocky Mountains to western Colorado. The scenery along the "Million Dollar Highway," US 550, between Montrose and Durango, CO is spectacular. The highway was a bit scary, since it had no guardrails. Stops to view scenery were necessarily brief, since we had an infant who wished to eat whenever we stopped. At Mesa Verda National Park we took turns visiting the ancient Indian pueblo ruins, while the other parent stayed with Randy. The trip continued on to Santa Monica via the Grand Canyon.

In Santa Monica we easily found a comfortable two bedroom apartment, located a few blocks from RAND. For the next year, I would be among the few Californians who walked to the office. The year was rather uneventful and pleasant. At RAND, I did not succeed in joining any of the main

interdisciplinary teams but I was involved in a smaller Air Force-related project. My good friend Len Berkovitz and I worked together on differential games. In addition, I studied some problems of calculus of variations with inequality constraints. This was the start of my long-time interest in the field of optimal control theory.

Flo and I enjoyed watching Randy grow into an energetic toddler. The three of us often had picnic lunches at the sea shore or in the fine Santa Monica park located on bluffs overlooking the sea. We introduced Randy to camping in the mountains, with a trip to Sequoia National Park in June 1954 and another trip to Oregon and Washington in August.

Despite the attractions of life in southern California, RAND did not seem to be the right working environment for me. I wanted instead to do mathematical research and to teach in a university. In the spring of 1955, I began a rather unsystematic and poorly informed job search. The academic job market at US universities was depressed during the early 1950s, and had barely begun to recover by 1955. We hoped that a suitable job would turn up in the western US, which we considered an attractive part of the country in which to settle. However, this didn't happen.

Faculty at Purdue University knew me from my student days there, and Lamberto Cesari had recently brought together a group at Purdue in my research area. Purdue offered me an Assistant Professorship at an annual salary of $5,500. I accepted the offer.

During my time at RAND, I gained in maturity and self confidence. My views of what mathematics can offer to other disciplines, and to society in general, were broadened. These things counted for little in my 1955 academic job search. Universities were interested in my mathematical research credentials. Most mathematicians in academe had no idea how a mathematician might contribute to applied interdisciplinary projects. In the 1952-53 RAND strategic air base study, the only mathematical techniques needed were from calculus and probability-statistics at an advanced undergraduate level. However, complex models of different possible air base systems and effects of possible modes of enemy attack were developed. It was essential that model assumptions were clearly articulated, and that dependence of conclusions on critical model parameters were clearly understood. My mathematical training was quite useful in contributing to these matters. RAND was originally part of Douglas Aircraft Company. An Aeronautical Engineering mentality was still part of the RAND culture. Douglas engineers could design a new airplane, build a prototype and see whether it would fly. This perspective was irrelevant to the strategic airbase study. Our models involved parameters which depended on human decisions and political factors, not just on laws of physics and chemistry. The models could be simulated with computers, but they could not be actually tested except in a war with the USSR which everyone hoped would not happen. Fortunately, our hopes in that regard were realized when the Cold War between the US and USSR ended around 1990.

We left Santa Monica at the beginning of June 1955, headed for a new life in academe.

6. Summer 1955. Our summer began with a month-long conference on partial differential equations (PDEs) at the University of California in Berkeley. We rented a small living unit and enjoyed Berkeley's pleasant, relaxed atmosphere. The turbulent events which shook Berkeley in the 1960s and 1970s were still several years away.

The June 1955 conference was a very good one, although the subject of PDEs was rather tangential to my own research interests. Many famous older mathematicians were there, as well as younger people who were destined to lead the field of PDEs in the next generation. Among the latter was

Peter Lax, who encouraged me to explain L. C. Young's theory of generalized curves and surfaces to him.

After the conference, we left California for the Midwest. L. C. Young was with us until Denver. He and I shared the driving. We camped near Lake Tahoe the first night. Travelling by car through the American West, with a young child in the back seat, was a different experience for Laurence Young. He once mentioned that before WWII English gentlemen travelled with a trunk full of clothes, in order to be properly attired in various situations. A scenic highlight of the trip was a detour through aspen groves and meadows of wild roses near Mt. Timpanogos in Utah.

After a few days in the Denver area, Flo, Randy and I drove to Madison. We spent the rest of the summer there, housed in a fifth floor apartment in a commercial building near campus. Swims in nearby Lake Mendota offered some relief from the oppressive heat and humidity.

We had saved enough money for a down payment on a modest house. During the summer we bought a two-plus bedroom house on Sunset Drive, in the south end of Lafayette, IN. The price was $11,500. This was our home for the next three years.

7. Purdue, 1955-1958. Since I left in 1949, a few distinguished senior faculty (including Lamberto Cesari) had come to Purdue. Many active younger mathematicians near my own age had also been hired. Among them were members of Cesari's group, working on surface area theory, with whom I could have interesting technical discussions. Cesari's seminar met regularly, which enhanced the lively mathematical atmosphere.

My teaching assignments consisted only of elementary service courses. This was an undesirable feature of my job. In the six years since I had last taught, I had forgotten how to get through to students of very mixed mathematical abilities, and I had to learn again how to do so. We taught three courses each semester, and courses met from three to five times per week. Despite the heavy teaching load, I soon got restarted with mathematical research. My three years at Purdue were a productive period mathematically.

Both colleagues and local townspeople welcomed us, and we made new friends easily. Among them were a Dutch couple, Jan and Riet Zwiep, with whom we have remained close friends ever since. Our families were nearby. Flo's parents were only 20 minutes away in nearby Brookston. My parents were schoolteachers. After their retirement, they built a house just outside the town of Battle Ground, even nearer to Lafayette than Brookston. My parents' property had a large woods, and many fruit trees which my father had planted. Both the Tatum farm and my parents' place were great for our children to visit. The neighborhood where we lived in Lafayette was also good for kids. Sunset Drive was a quiet street, and many playmates were available.

In summer 1956, we took a short vacation trip to Wisconsin and Michigan. We camped for several days in Lake Gogebic State Park, near Lake Superior, and found many wild blueberries. In the West, we had cooked over campfires with dead wood gathered nearby for fuel. However, this didn't work in the damp North Woods of upper Michigan. We bought a camp stove in the town of Houghton. Lake Superior was too cold for swimming, except at its extreme eastern end near Sault Ste. Marie. On the return trip home, we found fine beaches along the eastern shore of Lake Michigan.

The summer of 1957 was hot and humid, as is typical in the eastern half of the US. The summer was particularly uncomfortable for Flo, who expected a baby in September. At the end of August, I travelled to the summer meeting of the American Mathematical Society, held at Pennsylvania State University. I met Herbert Federer from Brown, who had done the best recent work in my field. He

expressed interest in one of my recent research papers, and asked whether I might be interested in a job at Brown. I replied that I would seriously consider it.

I returned from Penn State on September 2. Dan was born on September 3, ten days earlier than expected. Soon after that I had discouraging news at Purdue. During my first two years, I had never had a chance to teach an advanced course for mathematics graduate students or undergraduate majors. In the preliminary teaching assignments for fall 1957, I was scheduled to teach one of the two sections of Advanced Calculus for math majors. I was surprised and annoyed to find that my section of Advanced Calculus had been cancelled, and was replaced by another elementary service course which met five days per week (including Saturday) at 8 a.m. This disappointment reinforced my feelings that, as a teacher at Purdue, I was a production worker in a large educational factory. From that point on I would definitely be receptive to outside job offers from Brown or other good universities.

Late in the fall semester, I was invited for a job interview at Brown. I travelled by train in January 1958. We had never been to New England. Views from the train of children skating on ponds in coastal Connecticut on a bright winter day left an appealing first impression. The train was late arriving in Providence. When I reached the Mathematics Department, the chairman, C. R. Adams, was waiting anxiously to take me for an interview with Brown's President Barnaby Keeney, scheduled for a few minutes later. Keeney was an impressive figure, and the interview seemed to go well. Later in the visit, I met my future colleagues in the Brown Mathematics Department. They were a friendly group, mostly young, and their enthusiasm for mathematics was contagious.

Not long after my visit, I was offered an Assistant Professorship at Brown. We had two reservations about accepting it. The first concerned salary. I had been warned that Brown salaries were low. My salary for the first year 1958-59 would be $7,000, scarcely more than my starting salary at RAND seven years earlier. This concern proved to be unfounded. Over the years, faculty salaries at Brown rose to levels comparable to its peer universities. A generous college tuition benefit which Brown gave to children of professors was of great help too.

Our second concern was that we were reluctant to move a thousand miles away from our parents. I approached Indiana University about job possibilities, and received an offer from their Mathematics Department. However, my visit to IU did not give the kind of positive impressions which I got from the interview trip to Brown. In the end, my career prospects and interest in trying life in a different part of the US overrode both of these concerns. I accepted Brown's offer and never regretted this decision.

I left Lafayette early in the summer of 1958 to attend a month-long conference on area theory at Bowdoin College in Maine. Flo was left with the thankless task of getting ready to move and selling the house on Sunset Drive. A flooded basement gave an added complication. I persuaded her to join me at Bowdoin for the last week of the conference, followed by a memorable trip to England, Scotland, Amsterdam and Paris. In Scotland, we attended the International Congress of Mathematicians in Edinburgh. With great reluctance, Flo agreed to a separation from Randy and Dan, who stayed behind with our parents.

After our return from Europe, we drove with the boys from Indiana to begin life in Rhode Island at a rented apartment at 165 Power Street near the Brown campus. At Brown, I found contentment and my career prospered. In 1959 we moved to the seaside suburban town of Barrington, and lived there for many years. By 2000, our big old Barrington house with large yard and grounds seemed too much for a couple in our 70s. We moved then to our present house in the historic town of

Bristol. After the wanderings of our first ten years of married life, we are truly rooted in Rhode Island.