Remembrances of My Career at Brown University 1958-1978

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Introduction

I came to Brown University in 1958 and continued there as a faculty member until I retired from teaching in 1995. Brown provided an unusually congenial and stimulating environment for my teaching and research. I never regretted our decision in 1958 to accept an Assistant Professorship in the Mathematics Department, nor did I ever pursue seriously opportunities to move from Brown to another university in the years afterward.

For convenience, these remembrances of my first 20 years at Brown are divided into several parts. Part I concerns the years from 1958-68 and Part II from 1968-1978. Part III concerns my research contributions during those 20 years. A brief Afterword mentions my later years at Brown and the years of retirement. I begin with the first year 1958-59 after we came to Brown. This is the year of my career for which I have the strongest and fondest memories. The next Sections 2 and 3 give some thoughts about teaching mathematics at Brown, and about the nature of mathematical research. During the 1960s Brown University changed greatly. These changes can be understood only in the context of broader trends in society and higher education in the US (Section 4). There are two mathematical science departments at Brown, namely the Mathematics Department and the Division of Applied Mathematics. Section 5 is about the Mathematics Department and its relations with Applied Mathematics during the 1960s, including the years 1965-68 when I was the Mathematics Department chairman.

Flo and I moved to Providence in 1958 with two young sons, Randy (aged 4) and Dan (aged 1). Our third son Bill, born in 1960, is the only native Rhode Islander in the family. Sections 6 and 11 offer glimpses of our family life during the 1960s and 1970s. My career could not have flourished without Flo's loving support. An anxious entering graduate student once asked me the following question: Can one do serious mathematical research and have a decent family life too? My answer was "yes" and I hope Sections 6 and 11 provide supporting evidence.

Part II begins with my sabbatical leave year (1968-69), spent at Stanford University (Section 7). It was during that year that I arranged for my position at Brown to be changed from fulltime in the Mathematics Department to a joint appointment in both Mathematics and the Division of Applied Mathematics. Section 7 also mentions the New Curriculum at Brown, which was adopted in 1969 in response to a push by student groups for major curricular change.

Like most mathematicians, I expected to focus my career on teaching and research, with little time spent on administrative and committee work. However, things did not turn out that way. Some of the committees on which I agreed to serve were a waste of time. However, a few dealt with quite serious matters concerning Brown and the mathematics profession. The Vietnam War years 1965-73 were a time of turmoil on the campuses of American universities (Section 8). The Faculty Policy Group, on which I served from 1970-72, had a significant role in helping Brown get through a particularly difficult period during the war (Section 9).

The rapid expansion of universities in the US during the 1960s opened up many faculty jobs, and the number of new mathematics PH.D.s per year rose correspondingly. However, there was a severe cutback in hiring new mathematics faculty during the 1970s. This caused an employment crisis involving many young mathematicians. My involvement with the American Mathematical Society committee concerned with this employment crisis is discussed in Section 10.

In 1976-77, I had another sabbatical, which again freed me from teaching responsibilities. This sabbatical was very productive, and gave renewed vitality to my research career. Part III is concerned with my main research contributions during my first 20 years at Brown. I have tried to describe what motivated this research, and the circumstances in which it was done. Technical details are merely sketched. Readers who may wish further information are referred to the list of research papers and books in Appendix A.

In these remembrances, I have been selective in mentioning names of individuals. This was done to avoid tedium and confusion for readers who are not familiar with Brown or with the mathematical communities outside Brown in which I have been involved. I apologize to Brown colleagues, friends and coauthors for whom I have the warmest regard, but whose names are omitted. Appendix B lists the Ph.D. students who finished under my supervision.

PART I (1958-68)

1. First year at Brown. Upon our arrival in Providence in September 1958, we moved into an old-fashioned but spacious apartment at 165 Power Street. This was only a few blocks from campus. It was pleasant to walk to the office past elegant old houses in this historic part of the city.

The Mathematics Department was then located in an old house on College Street. This building was demolished soon afterward to make way for the new Rockefeller Library building. Faculty office accommodations were austere, and the telephone system was archaic. There was a phone in the hall outside my office, shared with all of the colleagues on my floor of the building as well as with the graduate students on the floor above. Teaching loads were heavy in comparison with those in more recent years. In 1958-59, I taught three courses each semester. However, none of these apparent drawbacks seemed to matter very much to me. There was an exciting atmosphere in the Mathematics Department at Brown which I had not experienced before. Almost all of the faculty members were rather young. We were interested in learning from each other about progress in many fields of mathematics, some of which were far from our own areas of expertise. Most of us regularly attended the Friday afternoon Mathematics Colloquium. In the evening afterward, there was usually a congenial gathering of colleagues and their spouses at someone's house. Among the mathematicians present, talk often turned to mathematics. This was tolerated (although not encouraged) by their spouses.

My main research accomplishment during 1958-1959 was the joint work with Herbert Federer, which led to the paper, "Normal and Integral Currents," published in 1960 in the Annals of Mathematics. In 1987, the American Mathematical Society awarded a Steele Prize to Federer and me for this paper, which is regarded as a seminal one in the field of geometric measure theory (See Section 13.)

In 1958, the customs and rules governing student life at Brown were not very different from pre-World War II years. Women students belonged to Pembroke College, which was administratively separate from the Brown college for undergraduate men. However, men and women students were enrolled in the same undergraduate courses. There were strict parietal rules concerning male visits to women's dormitories. There were also required chapel services for undergraduates, although by 1958 these were of a more secular nature than the religious chapel services of earlier times. Attendance in classes was required, at least in the lower level courses. There was a limit on the number of absences allowed without penalty, and skipping a class on the last day before a holiday counted as two absences. All of these regulations disappeared during the 1960s, which was a period during which students challenged the wisdom and authority of their elders generally and, of university administrators in particular.

When I came to Brown, Barnaby Keeney was the President. He was an impressive figure, who governed Brown with a firm hand. One could even describe Keeney's style as authoritarian, in the mode of King Louis XIV of France. One night during the winter of 1958-59, we heard noises from a disturbance not far from our apartment. A group of drunken Brown undergraduates were pelting the President's house on Power Street with snowballs. After a Dean had no success breaking up this fracas, President Keeney was awakened. He told the students, "Boys, you have five minutes to leave," and they did so. President Keeney left office in 1965. He avoided the turbulent years soon afterward, during which his style of university governance would have been poorly suited.

We began to discover some of the pleasures which families could enjoy in New England. In the summer, Goosewing Beach in Little Compton was a delight. A stop at Gray's Ice Cream store at Tiverton Four Corners was mandatory en route home from Goosewing. The fall foliage at Mt. Monadnock in southern New Hampshire was stunning in October 1958. During the next summer we camped at beautiful White Lake State Park, also in New Hampshire.

2. Teaching. It is a tradition at Brown that faculty are expected to teach courses at many levels, ranging from introductory freshman courses to advanced graduate courses and seminars. In the fall semester of 1958, my teaching assignment consisted of three courses: first semester calculus, a junior-senior level probability courses (Math 161) and the real analysis course for graduate students. Among the three courses, Math 161 suffered. In 1958, I knew little about probability and did not have time during the semester to remedy this lack. Afterwards, I became "self-taught" in probability and stochastic processes as part of my research program in stochastic control. In later years, Math 161 became one of my favorite courses to teach.

I found that Brown students were a pleasure to teach. Brown's selective admissions policy ensured that students were generally talented, and often brilliant. Class sizes were reasonable, and small enough in upper level courses that faculty could get acquainted with students individually. Individual reading courses and advising students majoring in mathematics were other ways to know talented juniors and seniors well.

In 1960, the Mathematics Department made major changes in its undergraduate course offerings. The result was a curriculum much better suited to contemporary mathematics and its applications. Among the new courses was a new two semester sequence in mathematical analysis (Math 113-114), which replaced a more traditional Advanced Calculus course. There was no usable textbook for Math 113-114 in the early 1960s. The lecture notes which I developed while teaching Math 113-114 became the textbook, "Functions of Several Variables," which appeared in 1965. A second edition was published in 1977 and is still in print (as of 2007). (See reference B1 in Appendix A.) It is a source of satisfaction that quite a number of mature scholars in mathematics and the sciences have told me how much they learned from this book during their student years.

It is important to assess the teaching performance of mathematics faculty. However, this is not easy to do with confidence. The longer term effects of good teaching are subtle and impossible to measure quantitatively. At most universities (including Brown) students answer teaching evaluation questionnaires at the end of each semester. Looking back, I rate my own performance as a classroom lecturer as about a "B" on the average in lower level courses and higher in more advanced courses. This is more or less consistent with my ratings in student evaluations. There is a tendency in some state-supported universities to rely on data from course evaluation questionnaires as the principal measure of teaching performance. I hope that this never happens at Brown. Evaluation of teaching must not be done like the Nielsen ratings of TV audiences.

At the graduate level, supervision of Ph.D. students was a time consuming but rewarding experience. During my career at Brown, 23 students finished the Ph.D. under my supervision (see list in Appendix B). Many of them went on to distinguished careers in academia or government service, and former Ph.D. students are among my best friends in mathematical circles.

3. Nature of mathematical research. There are many ways to make original contributions in the field of mathematics. Original research might consist of showing that a famous conjecture is correct. It might also involve an insightful new mathematical theory or an elegant new proof of a known result. The term, "mathematical sciences" is often used to include both traditional core areas of mathematics such as algebra, real and complex analysis, geometry and topology, as well as topics more closely related to applications of mathematics. Researchers in traditional cores areas are often called "pure mathematicians," while those with applications-oriented interests are often called "applied mathematicians." Colleagues in the Division of Applied Mathematics at Brown work on modeling and developing efficient computational algorithms for such diverse applications as computational fluid mechanics, speech recognition, medical imaging and congestion in data transmission networks.

A distinction between "pure" and "applied" mathematicians is meaningful, although imprecise. Both terms can be applied to many of the best minds in the mathematical sciences. Gauss is famous both for his remarkable early work in algebra and number theory and for his later contributions to practical astronomy. On the other hand, mathematics cannot be meaningfully described as "pure" or "applied." There are many examples of "purely mathematical" results which turned out later to have important, unexpected applications in other fields.

In the field of probability, the Central Limit Theorem is a beautiful mathematical result, which is also profoundly important for applications. During the 1940s, Kiyoshi Ito developed his theory of stochastic calculus and stochastic differential equations. The Ito Theory was initially motivated by purely mathematical considerations. However, soon afterward, it became a tool for modeling randomly perturbed dynamical systems in such applied areas as chemical physics and noisy communications. In mathematical finance, the Ito stochastic calculus has become a standard tool for modeling price fluctuations of risky financial assets. In 2006, Professor Ito became the first recipient of the Gauss Prize. This prize will be awarded once every four years at the International Congress of Mathematicians. Its purpose is to improve public awareness of mathematics, and of the ways in which it enriches science, technology and indirectly daily life.

The most exciting moments in mathematical research are those when a new idea appears which becomes the key to real progress. For me, such ideas came only sporadically and usually at unexpected times and places after previous fruitless efforts. Often I failed to make progress at all on a research problem. I had to learn not to be unduly discouraged by such defeats. As young athletes are sometimes told, "You win some and you lose some, but you must show up to play for every game." During the summer of 1955, I deluded myself for several weeks into believing that I had a neat result. If true, it would give an interesting characterization of a class of integrands for geometric problems in the calculus of variations. We were moving from California to Indiana that summer, and the presumed new result cheered me during the move. My self-delusion ended once we were settled in Lafayette and I began to write down details of my faulty argument.

First-rate mathematical research is sometimes done in isolation, but over time there is the risk of stagnation without the stimulus of interactions with other mathematicians. An encounter with someone from a completely different field may lead to a fruitful new research direction. My work with Viot on measure valued diffusion processes (Section 16) was inspired by a lecture at a meeting of population biologists. Many of my research papers have coauthors. This became increasingly common during the later years of my career. I thank my coauthors not only for their many original contributions to our joint research, but also for the many mathematical techniques which I learned from them. These matters are discussed further in Sections 13-17.

4. Higher education in the 1960s. The successful launch into orbit of the Soviet Union's "Sputnik" in the 1950s was regarded in the US as frightening evidence that we were falling behind in science and technology. John Kennedy emphasized this possibility in his successful 1960 campaign for President of the US. Soon afterward, Kennedy initiated an ambitious space program, which succeeded in landing humans on the moon in 1969.

There was also the prospect of many "baby boom" generation young people to be college educated, starting in the mid 1960s. In 1960, a substantial percentage of college teachers did not have Ph.D.s. Kennedy's January 1961 presidential inaugural address mentioned the urgent need to train more college faculty with Ph.D. degrees. Soon afterward, the US Federal Government created fellowship programs, which provided support for large numbers of graduate students in Ph.D. programs. Science, mathematics and engineering were emphasized, but fellowships were also available to Ph.D. students in such other fields as Slavic Languages.

During the 1960s, mathematics and science curricula for secondary schools were revised. Science fairs for young people enjoyed great popularity. These factors promised a flow of well prepared college freshmen, eager to pursue mathematics, science or engineering.

Established universities expanded steadily during the 1960s, and new universities were started. For instance, the number of campuses in the University of California system increased from 3 to 8. Existing PH.D. programs in mathematics expanded, and many new Ph.D. programs were created. By the 1970s, there were over 150 Ph.D. programs in the mathematical sciences in the US. This rapid growth created a keen competition for top senior faculty, who would enhance a department's reputation and would take the lead in training Ph.D. students. Moreover, growth in the numbers of new Ph.D.s lagged behind the demand for junior mathematics faculty. There resulted a kind of brief "golden age" in the academic job market, with keen competition for faculty at both senior and junior levels.

The early 1960s were also a time of hope and optimism in American political life. The Civil Rights movement began to break down long-time racial barriers. Idealistic young people volunteered for Peace Corps service in Third World countries. President Johnson's "Great Society" and "War on Poverty" proposals were well received by the liberal US Congress elected in 1964.

Unfortunately, events soon began to cast a pall over these feelings of national optimism. There were frustrations with the apparently slow progress in remedying racial and economic inequalities. US involvement in the Vietnam War was a very divisive issue (Section 8). Many young people came to feel a great distrust and lack of respect for adult authority. These conditions led to growing unrest on university campuses. By the end of the 1960s, university administrators were faced with a multitude of unexpected challenges. Many university presidents were either forced to resign, or did so in despair. At one point in 1971, Donald Hornig was third in seniority among Ivy League presidents, even though he had been President of Brown for scarcely more than a year.

5. Brown Mathematics Department in the 1960s. When I came to Brown in 1958, there were only 10 faculty members in the Mathematics Department. Most of us were young, and the mathematical atmosphere was lively. Loss of faculty to other universities is always a danger, and the risk of this happening was even greater during the expansive 1960s era. During the 1960s, the Mathematics Department at Brown weathered two such crises. In

1960, two key department members accepted offers to move elsewhere. This left only a small core of faculty to build for the future. In the fall of 1960, David Gale began a successful five year term as Mathematics Department chairman. The department grew steadily during the 1960s, to about double the number of faculty in 1958.

I succeeded Gale as department chairman, with a three year term from 1965-68. During my first year 1965-66, most of the full professors in the department received offers from other universities. Provost Merton Stoltz was quite supportive in responding to these offers. When the crisis was over, only Gale had left to pursue a distinguished career as a mathematical economist at the University of California in Berkeley. The other full professors declined those outside offers, and remained at Brown for the long term. During my time as chairman, I had a role in hiring and tenure review cases for several junior faculty members, who also stayed at Brown for the long term and had distinguished careers in research and mathematics education. This is a source of lasting satisfaction for me.

The Mathematics Department suffered a more severe crisis in the late 1980s, with the departure of several of its younger "stars" to Harvard, the University of Chicago, MIT and other universities. Colleagues hired during the 1960s were among the mainstays on whom the department relied in the successful process of rebuilding afterward.

At the end of World War II, Brown established a Graduate Division of Applied Mathematics. An undergraduate Applied Mathematics degree program was added during the 1950s, and "Graduate" was omitted from the name of the department afterward. When I came to Brown, the Division of Applied Mathematics was a leader in solid and fluid mechanics, as well as in areas of mathematical statistics and applied probability. During the early 1960s, there was dissention among the Division of Applied Mathematics faculty. Many accepted offers from other universities. This left an opportunity for rebuilding the department, adding new areas of research emphasis.

The distinguished mathematician Solomon Lefschetz had formed a strong research group, working in areas of differential equations, control theory and stochastic filtering models. The aerospace industry provided applications for these research topics, and the "space race" between the US and USSR during the 1960s made them seem particularly timely. The Lefschetz group was part of the Martin Aircraft Company, and was located in Baltimore.

In 1964, Lefschetz's group was seeking another home in a university environment. By the fall of that year, most of the group had moved to Brown, with appointments in either the Division of Applied Mathematics or the Division of Engineering. In later years, the group evolved into what is now called the Lefschetz Center for Dynamical Systems. Bringing the Lefschetz group to Brown was a good move for the University. However, it was handled in a way which soured for several years relations with the Mathematics Department and with other colleagues in the Division of Applied Mathematics. The negotiations to bring the Lefschetz group to Brown were conducted in secrecy. It seems that at Brown only President Keeney and the chairmen of Applied Mathematics and Engineering knew what was going on. I first heard about it through an extravagantly worded publicity release posted on the Mathematics Department bulletin board.

Teaching calculus courses is a basic service function of the Mathematics Department. The department was upset when it learned from the chairman of Engineering during the winter of 1965-66 that Applied Mathematics proposed to teach its own calculus course. This proposed course would emphasize the use of computers and numerical algorithms for the approximate solution of calculus problems. It soon got the nickname "computer calculus." A long series of inconclusive discussions between Mathematics and Applied Mathematics followed. In the spring of 1968, Joseph LaSalle became the new chairman of Applied Mathematics, after his predecessor suddenly died. LaSalle and I agreed on a two year experiment in teaching computer calculus, using faculty from both departments. Considering the primitive state of computer technology in the 1960s, the course was arguably ahead of its time. After 1970, computer calculus was no longer taught.

In 1960, the Mathematics Department moved from College Street to a renovated house at the corner of Thayer and George Streets. The new quarters were cramped, and the shoddy blackboard and lighting provided were justified by the claim that these quarters would be only temporary. Herb Federer doubted this claim, stating that he would retire before any move would happen. He was correct. The department remained in this building for nearly 30 years.

In the late 1960s, the famous architectural firm of I. M. Pei was commissioned to design a grand new building for Mathematics, Applied Mathematics and Geological Sciences. It would have covered the entire block bounded by Thayer, George, Manning and Brook Streets. Pei himself came to Brown with a team from his company, bringing along a three-dimensional model of the proposed design. The Mathematics Department part of the building would have had an angular appearance, reminiscent of the East Building of the National Gallery of Art in Washington, DC, which Pei also designed. This proposed Math-Applied Math-Geological Sciences building project reached the working drawing stage, but no farther. US Government funding for new university buildings related to science and engineering had been generous early in the decade. However, by the late 1960s, Vietnam War costs had squeezed other government programs. Hoped for US government support for this building project perhaps disappeared instead in the jungles of Vietnam.

Eventually, the Mathematics Department moved across George Street to the comfortable accommodations which it currently occupies. Applied Mathematics continues to be housed in the historic "Richardson Romanesque" style house at 182 George Street. Geological Sciences occupies space in the new Geo-Chem/MacMillen Building, located in the same block intended for the ill-fated project designed by Pei.

6. Family life in the 1960s. After the year 1958-59 of apartment life, we moved to our first Rhode Island house at 107 Massasoit Avenue in Barrington. The house was of Dutch Colonial style, which was popular in the 1920s. In my boyhood I imagined living in such a house, perhaps influenced by pictures of homes in magazine advertisements. The local Hamden Meadows elementary school was excellent, and our boys walked to school through a small woodland and along quiet streets. They are part of the "baby boom" generation, born at a time when many families had three or more children. Like the majority of mothers then, Flo's job was to manage our home and family. She did this with great skill and patience. With the children, her style of mothering was gentle but firm.

In many ways, family life was simpler than it seems more than 40 years later. There was a convenient bus service to Providence, and we managed with one car until the late 1960s. We provided our home with books, music and constructive toys such as Lego's. When the boys were small, we were among a small minority of American homes which did not yet own a TV set. Such distractions as videogames were still in the distant future. Our sons did well in school, and we did not have the kinds of struggles over homework which many families experience nowadays.

Throughout my career, my mathematical research work was done mostly in a "study" at home. There were too many distractions when I was in my office at Brown. At the Massasoit Avenue house, my study was in a converted sun porch, with a children's play area outside. The happy sounds of small children at play did not bother me. There was a more ample study in the Colley Court house to which we moved in 1967 (see below). Although this study had thick walls, I could not help being annoyed by some of the music which blared loudly in our house during our sons' teenage years. Particularly irritating around 1970 were some recordings by the group "Country Joe and the Fish."

Starting with Randy in 1965, each of our sons was a member of Boy Scout Troop 1 in Barrington. I became involved too as a member of the Troop Committee. For the boys in Troop 1, scouting was a meaningful part of growing up. The troop's annual weeklong trips in late June to northern New England provided real adventures. On five occasions, I was among the fathers who went with Troop 1 on these trips. Two were canoe trips on the Connecticut, Saco and Androscoggin Rivers, and two were bicycle trips in Maine. The last one was a backpacking trip on the Long Trail in Vermont. The weather was quite rainy that week. We were ready to believe the (unofficial) sign which read "Long Trail – Vermont's longest river."

During the 1960s, traffic on Massasoit Avenue became heavy, as the population of the Hampden Meadows section of Barrington grew. In May 1967, we moved to a big old house at 3 Colley Court in Barrington. It had an acre of land, many trees and was located on a quiet cul-de-sac. There were many children in the neighborhood, and it was a five minute walk to the mile-long sandy Barrington Beach. It was to be our home for the next thirty-three years.

During the 1960s, Flo and I took many trips with the children. We made annual visits to the grandparents in Indiana. During 1962-63 the family came with me to Madison, Wisconsin where I was visiting the Mathematics Research Center at the university. Later in 1963, my parents moved from Indiana to a retirement community in southern California. We visited them in California regularly, and with increasing frequency during the 1970s when they became too infirm to travel to Rhode Island. Some of these family visits were combined with memorable hiking and backpacking trips in the Sierra Nevada mountains (see Section 11).

We spent summer 1964 visiting Stanford University, travelling across the US in our Rambler station wagon towing a small camp trailer behind. The weather en route from Rhode Island to California was stormy through the Rocky Mountain states. When we finally reached California, we basked in sunshine while camping at Lake Tahoe.

In June 1965, the family accompanied me to Italy. Our first stop was in Pisa, where I discussed geometric measure theory with my host Ennio de Giorgi and others. I rented a station wagon, which I soon found to be impractical for the ancient narrow streets of Pisa. We exchanged it for a small Fiat sedan. For the remainder of the Italian trip, Flo and I rode in front with the three boys squeezed together in the back seat. We caught a glimpse of Pope Paul VI, who came to Pisa one day during our stay. The Pope's visit caused huge crowds, and mind-boggling traffic jams. After Pisa, we drove to a conference in the picturesque village of Ravello, situated high above the Amalfi Coast south of Naples. From Ravello, we drove to Genoa, where I visited J. P. Cecconi's group. We had a relaxing weeklong trip back to the US by sea, aboard the liner Michelangelo.

We appreciate the many kindnesses shown by our Italian hosts, both on the 1965 trip and on other visits to Italy. The warmth and courtesy of those who served us in hotels and restaurants added to our pleasant memories of Italy. A memorable moment was our departure from Genoa abroad the Michelangelo, waving to the Cecconi family on the docks as they waved back to us.

Our good friends Gloria and Martin Billett introduced us to the beauties of the Maine coast. Their three children were of nearly the same ages as ours. Beginning in 1965, we and the Billett family often rented seaside cottages near each other in the seaside village of Birch Harbor. In recent years, Flo and I spend half of every September in a secluded summer house on the Maine coast, located on the Petit Manan Peninsula. This is a very special place to us, and also for our sons and their families.

PART II (1968- 78)

7. Sabbatical year 1968-69. During 1968-69, I was on leave at Stanford University. Half of my salary came from Brown's sabbatical leave program and the other half from a National Science Foundation fellowship grant. This year provided a quiet interlude between my term as Mathematics Department chairman and the eventful years to follow.

This sabbatical year was less exciting mathematically than I had hoped. Samuel Karlin, who later stimulated my interest in population genetics, was on leave that year. In fact, we rented the Karlin's house on Greer Road in Palo Alto for the year. The sabbatical gave me time to finish an invited survey paper on stochastic control theory (Reference [9] in Appendix A). This survey came at an opportune time, and influenced further developments in the field of stochastic control during the 1970s.

During the 1960s my research interests had shifted from geometric measure theory to stochastic control. No one else in the Mathematics Department at Brown was interested in control theory, and prospects for finding Ph.D. students in the department to work in that field were slim. On the other hand, after the arrival of the Lefschetz group in 1964, control was a major topic in the Division of Applied Mathematics. Among Applied Mathematics faculty, Harold Kushner had become a leader in stochastic control. I decided to ask that my status be changed from full time in the Mathematics Department to half-time each in Mathematics and Applied Mathematics as of fall 1969. This request was granted by Provost Stoltz, although a few Mathematics Department colleagues objected to the change. The new arrangement worked very well for me. Over the years, Kushner and I maintained a successful program in stochastic control. We trained a succession of PH.D. students and postdocs, many of whom went on to highly successful careers of their own.

During 1968, a student movement was organized with the goal of replacing the Brown undergraduate curriculum by a completely new one. At first, the students' goal was a free-wheeling curriculum, something like the one at the newly formed Hampshire College. This is something which could not have worked at Brown, and the faculty would not accept it. In the Spring of 1969, a compromise was adopted after lengthy faculty-student discussions. It was called the "New Brown Curriculum," and it still exists in somewhat modified form today.

The New Curriculum allows students great flexibility in choosing courses. In order to graduate, a student must obtain a required total number of course credits and must satisfy the requirements of a major area of concentration. Most of the students whom I advised used wisely this flexibility in course selection. Courses can be taken pass-fail. This option was used more often in early years of the New Curriculum than in later years.

Another feature of the early years of the New Curriculum was the Modes of Thoughts (MOT) program. MOT courses were intended to replace introductory survey courses, which students often found dry. They were open to freshmen only and had small class sizes. Like many experiments in college freshman education, the MOT program had limited success, and it disappeared later. In Spring semester 1972, I taught a MOT courses entitled, "Randomness." The timing was poor because of my heavy Faculty Policy Group duties to be mentioned in Section 9. Although my 15 MOT students enjoyed their term projects, I considered the course to be a "qualified failure." MOT courses were labor intensive. Besides my own time and effort, the course had two teaching assistants.

In France, the DeGaulle government had recently established a new institute dedicated to research in computer and information science. It is now called INRIA. The energetic and influential mathematician J. L. Lions was involved in the creation of INRIA, and afterward became its Director. Under Lions' influence, the INRIA research agenda included many topics of mathematical interest.

I accepted an invitation to visit INRIA during May 1969, leaving Flo in Palo Alto with the boys. This visit had no immediate effect on my research. However, it was a useful first step toward later productive exchanges of ideas with French colleagues, including Alain

Bensoussan, and to joint research with Bensoussan's students Etienne Pardoux and Michel Viot.

My stay in Paris was interrupted by a one week visit to Czechoslovakia, during which I attended a small mathematics conference near Prague. This was less than a year after the Soviet Union's army had invaded in summer 1968 to end a brief period of liberalization of communist rule in Czechoslovakia (called the "Prague Spring" of 1968). In Prague, the street signs were missing in May 1969. They had been removed hastily just before the Soviet tanks arrived. It was feared that mass arrests would come immediately after the invasion. Such crude measures of earlier times had been replaced by subtler, but nonetheless harsh, methods for suppressing dissent. My Czech hosts had to be very careful. Sensitive topics could be mentioned only in places where there was no possibility of being overheard. There were many indications of deep resentment toward the Soviet invaders, for instance signs saying, "Moscow this way 1000 km." The Czechs were to endure 20 more years of a repressive communist regime until the collapse of communism in Eastern Europe in 1989.

During my INRIA visit in 1969, I stayed at the Hotel du Palais Bourbon, located in the 7th Arrondisement of Paris at 49 rue de Bourgogne. This small, congenial hotel was owned and operated by the amiable Claudon family. We stayed there again several times during later visits to Paris.

The INRIA campus is located in the suburb of Rocquencourt, some distance from the center of Paris. On days when I did not go to INRIA, I was free to explore the historic sites, museums and cafes of Paris. One could sit indefinitely in an outdoor cafe, after even only a modest order of coffee or beer.

In contrast, workers in Prague had to report for work at 6:00 a.m. (or 7:00 a.m. for intellectual workers). Lunch was a quick standup affair at some nearby canteen. According to communist propaganda, Czechoslovakia was a "worker's paradise." However, Czech workers would have been delighted to change places with leftist intellectuals in Paris who enjoyed the café milieu while theorizing about a better socialist world.

8. Vietnam War years. During the 1960s and 1970s, the US and its NATO allies were in the midst of an ongoing "Cold War" with the Soviet Union. The Cuban Missile Crisis in the fall of 1962 was truly frightening. Many people in the US, both liberals and conservatives, feared a communist takeover of many Third World countries.

The US military effort in Vietnam began in 1965 and continued until 1973. It created bitter divisions in American society. Supporters of the war warned of the "domino effect." If we did not resist a communist takeover of South Vietnam, then other countries in Southeast Asia would afterward fall one after another to communism. Opponents questioned whether vital American interests were truly at risk in Vietnam. Many of them regarded our conduct in the war as immoral. They cited the killing of innocent civilians from aerial bombing and the widespread spraying of the countryside with toxic herbicides such as Agent Orange. The military draft was unpopular. Although college students in good academic standing had deferments from military service, the draft added to feelings of anger toward the war on college campuses.

Other factors contributed to campus unrest during the Vietnam War years. The issue of equal opportunities for minority students and faculty led to serious tensions at many American universities. Young rebels in the US, as well as in Western Europe, were influenced by the Red Guard movement in China, which provided some of the worst excesses of Maoist style communism. France was paralyzed by unrest in May 1968, and for a brief period another French Revolution seemed possible.

During the period1968-72, many American universities experienced serious disruptions. Occupations of the President's office or other university facilities by protesters were common. These were called "sit-ins." At Harvard, deans were carried out of their offices. Guns were brought on campus at Cornell, and there were fatal shootings at UCLA. Campus buildings with some military connection were often targets for violent acts. Several reserve military officer training (ROTC) buildings were burned. The Mathematics Research Center (MRC) building at the University of Wisconsin in Madison was severely damaged by a massive bomb explosion in 1970. This was the building where I worked during my visit to the MRC in 1962-63. Although the MRC did only basic research, and not military work, it was funded by the US Army Research Office.

The year 1968-69 when I visited Stanford was one of turmoil in the San Francisco Bay Area. In the fall of 1968, there were daily violent confrontations at San Francisco State University between radicals and police. These were ended when California Governor Ronald Reagan induced the state university governing board to appoint a new hard-line president of San Francisco State. Reagan was the darling of right wing Republicans, but was considered an arch-enemy by the liberal left. Berkeley was a center of anti-war activism, and a mecca for disaffected young people from the entire US. Violence erupted in Berkeley in the winter of 1968-69, and again the year after. When I visited the University of California Berkeley campus in June 1970, the broken windows of many buildings were boarded up. A distinguished older mathematician on the Berkeley faculty, who had been a refugee from the Nazis, wondered whether he still belonged at the university. Stanford's turn came in spring 1969. There were nasty confrontations between the university and protestors. In the end, the protesters were banned from the Stanford campus.

Early in May 1970, there was a confrontation between anti-war demonstrators at Kent State University and the Ohio National Guard. Some Guard members opened fire and several demonstrators were killed. Vehement reactions to this event spread rapidly across the nation. Few American universities were spared the chaos which ensued during the next few weeks.

Brown "muddled through" May 1970 with anti-war demonstrations and endless meetings, but no violence. The Kent State event happened near the end of the spring semester at Brown. Ad hoc arrangements were made about final exams and assignment of semester grades.

Some protest organizers hoped to use anti-war feeling to mobilize Brown student support for a broader agenda of leftist issues. One meeting in Meehan Auditorium (the Brown hockey arena) began with the invocation "All power to the people…." While Meehan was packed with students on this occasion, opposition to the war did not radicalize the Brown student body on other issues. In the post-Vietnam War years, leftist sympathies dwindled on college campuses across the US, even at Berkeley. When Reagan ran for US President in 1980, a friend told me that a majority of University of California Berkeley undergraduates planned to vote for him, instead of President Carter (his Democrat opponent).

Some memories of May 1970 are positive. Many students during that era were unselfish and idealistic, with hopes for a fairer society and peaceful world. A student in my mathematical statistics class said in May 1970, "We believe in cooperation, not competition." Many Brown faculty members (including myself) allowed time for free discussions in class. Students had many perceptive questions about the academic enterprise at Brown, and what motivated our individual faculty research agendas.

The intensity of May 1970 could not be sustained in the fall semester. In May, it was agreed to suspend classes at Brown for the last part of October 1970, to allow students to help elect antiwar members of Congress in the early November election. This experiment failed. Often there was no competitive race between pro-war and anti-war candidates in a student's home congressional district. Many students simply went on vacation.

At Brown there were no sit-ins until the Spring of 1975. The sit-in occupied University Hall briefly and was concerned with minority issues. There was a lot of noise, but otherwise this sit-in was respectful of university property and sensibilities. It attracted the attention of national media, which had been starved for such newsworthy events since the war ended in 1973.

The temporary disruptions of academic life during the Vietnam War were petty compared to the suffering of those directly involved (both Americans and Vietnamese). Although the US lost the war to its communist enemies, by the early 21st century, Vietnam was becoming integrated into the capitalist "global economy." Consumer goods for export to the US can be made in Vietnam even more cheaply than in China. It is reported that workers in East Asian countries, including Vietnam, endure working conditions which are no better than those 200 years ago in the early days of the Industrial Revolution.

9. Faculty Policy Group 1970-72. Pressure during the late 1960s for greater faculty and student involvement in university-wide decision making at Brown led to the creation of the Faculty Policy Group (FPG). It had 18 members elected by the Brown faculty, with two year terms. The official role of the FPG was to formulate motions which would then be brought before the entire Brown faculty for a vote. Unofficially, the FPG also served as a useful forum for discussion of controversial issues, and an informal channel for helping to defuse potentially explosive situations. In the early 1970s, there was no shortage of controversial issues on campus. They included university governance, implementation of the 1969 New Curriculum, medical education at Brown and possible training of Brown undergraduates as reserve military officers through ROTC programs.

I served on the FPG during 1970-72. The FPG Executive Committee for 1971-72 consisted of Ned Green from Chemistry, Jonathan Conant from German and me. I was the FPG chairman during the spring semester 1972, which was the most exhausting semester of my entire career at Brown.

During the years before 1972, Brown had a six-year program which combined an undergraduate degree with the first two years of medical education. Graduates had to transfer elsewhere to complete their clinical medical training. This sort of program was no longer viable, and the University proposed instead a full medical education program leading to an MD degree. There was opposition to this proposal among Brown faculty members, some of whom believed that professional schools such as Medicine or Business would change the nature of Brown. The University administration had to allay fears that the new medical program might drain financial resources from other parts of Brown.

As chairman of the FPG, I presented at the March 1972 Brown faculty meeting a resolution which recommended that the faculty should endorse the new medical education program. After extended debate, this resolution passed by a large majority. This meeting is also memorable since it happened on my birthday and I was recovering from the flu.

During the Vietnam War years, ROTC programs were suspended at many private universities in the US including Brown. There was considerable sentiment among alumni and the Brown Corporation (which governs the University) in favor of returning ROTC to campus. In the spring of 1972, there was a proposal to reinstate a Navy ROTC program at Brown. This came at a most inopportune time, at which President Nixon once again escalated the war in Vietnam. Although anti-war feelings on campus were intense, the faculty debate about ROTC was framed in academic terms. Some ROTC courses would be prescribed by the US Department of the Navy. It was argued that this was contrary to the autonomy of the Brown curriculum.

After an exhausting round of debate and attempts to find a compromise, a resolution concerning ROTC was brought by the FPG for a vote at the May 1972 faculty meeting. The monthly faculty meetings were held in Carmichael Auditorium at 4 p.m. They were preceded at 3:30 by a half-hour review of the agenda in the President's office, located in University Hall nearby. When I arrived at University Hall for the Agenda Committee Meeting, the building had been evacuated. An anonymous phone caller had warned that a bomb would explore in University Hall at 3:38 p.m. The Agenda Committee moved to another building nearby. At 3:38 p.m. nothing happened. Such bomb hoaxes at universities were fairly common in 1972, especially during final exam weeks.

Carmichael Auditorium was packed for this faculty meeting. Besides faculty members, many concerned students sat in the aisles and others gathered outside. The faculty voted against reinstituting Navy ROTC. Since 1972, ROTC has not returned to Brown, although Brown students can arrange to participate in ROTC programs at a nearby campus.

Soon after the faculty vote on ROTC, President Hornig suffered a mild heart attack. This added to the sense of confusion with which the 1972 spring semester ended. On a lighter note, I recall the following episode. It suggests that anti-war sentiment on campus, although real, did not exclude other interests. An anti-war rally was planned for a Sunday afternoon in mid-May. The organizers asked a well-known New York Times columnist to be the keynote speaker at the rally. As FPG chairman, I was given the task of asking Acting President Stoltz for Brown to pay the speaker's fee, which was surprisingly large. I was also told to warn him of another possible event which might cause an embarrassingly small attendance at the anti-war rally. In May 1972, the Boston Bruins hockey team played the New York Rangers in the final series of the Stanley Cup playoffs. If a 7th and final game of the series were necessary it would be played on that same Sunday afternoon. Stoltz refused to pay the speaker's fee. It turned at that a 7th Stanley Cup game was unnecessary, since the Bruins won 4 games out of the first 6.

Brown escaped the worst of the turbulent Vietnam War period. Merton Stoltz was Provost throughout this period, and served three times as Acting President of Brown. He deserves much credit for his patient, steadfast leadership. Faculty involvement both by FPG members and many other concerned faculty was helpful. Various informal channels of communication were important in avoiding potentially dangerous confrontations between student activists and the University administration. As a member of the FPG Executive Committee, Jon Conant was especially effective in the role of Liaison with students. For tenured faculty like me, FPG service was no more than a personal inconvenience for the greater good of Brown. However, the risks were much greater for nontenured faculty members like Jon Conant. By the end of the Vietnam War, Brown was overextended financially and entered a period of retrenchment. Conant was among those Assistant Professors at Brown who lost their jobs. Fortunately, his specialty (Old Icelandic) was in demand in the state of Minnesota, which has a strong Scandinavian heritage. He moved from Brown to the University of Minnesota in Duluth.

The fact that Brown escaped the kinds of violence experienced at many other universities made Brown popular afterward among prospective freshmen and their parents. The flexibility of the New Curriculum at Brown was also attractive to many young people. Admission to Brown has remained highly competitive ever since.

10. Mathematics Ph.D. employment crisis. After the rapid expansion of higher education in the US during the 1960s, there was a period of retrenchment. The stock market was stagnant during the 1970s. Many private universities (including Brown) had been too optimistic about the future growth of income from their endowments. State supported universities had to compete for funds with other demands for state resources, and tax revenues often fell short of expectations. Most college course enrollments in mathematics are in lower level service courses. During the Vietnam War years, many college students favored majors in political or social science, which required little or no mathematics. There was a decline nationally in mathematics course enrollments. At the same time, expansion of mathematics Ph.D. programs produced an oversupply of young Ph.D.s in mathematics. This situation caused a severe and unanticipated crisis in the job market for recent mathematics Ph.D.s during the 1970s.

The American Mathematical Society (AMS) established the Committee on Employment and Educational Policy (CEEP) in response to this job crisis. I was a member of CEEP from 1974-1979, and served as its chairman for three of those years.

No committee could magically find a solution to the employment problem for young mathematicians. However, CEEP did have a positive role in helping to improve matters. One of its tasks was to keep the mathematics community well informed. Responses to the job crisis based on facts were more likely to be useful than responses based on folklore. CEEP supervised the annual AMS Survey of mathematical science departments in colleges and universities, and summarized the survey results in the AMS Notices. The Survey gave a detailed picture of trends in student enrollments, faculty hiring and PH.D. production. CEEP also held forums at annual AMS meetings.

Another task for CEEP was to help broaden job opportunities for mathematicians, both at the Ph.D. and lower degree levels. Following suggestions by CEEP, some mathematics departments introduced Masters degree programs with an applied emphasis. Graduates from these programs were in demand for jobs in industry. Ph.D. students were encouraged to broaden their training. There was a demand for mathematicians who could teach undergraduate courses in statistics or computer science. Moreover, Ph.D. mathematicians with some credentials in those areas found it easier to get nonacademic jobs. CEEP also organized short courses in areas of mathematics closely related to applications. These courses were held at annual AMS meetings and were well attended.

During the 1970s, it was common for universities to delay until summer the authorization of funds to hire additional mathematics teachers for the fall semester. The visiting faculty hired at the last minute were usually appointed for only one year. Some young Ph.D.s became "mathematical gypsies" who moved several times from one temporary position to another. At the urging of CEEP, the Council of the AMS passed a resolution which urged that temporary appointments of faculty with Ph.D. degrees should be of at least two years duration. This resolution had some positive effect afterward.

CEEP also sought to find out what happened to mathematics PH.D.s who left academia. Many of them moved successfully to the rapidly developing computer industry. Some others took jobs which made no use of their advanced educational level. However, there was little or no evidence of the proverbial "Ph.D. taxi driver" among mathematicians.

Projected numbers of incoming college students and expected rates of faculty retirements suggested an even worse job market ahead for mathematics Ph.D.s during the 1980s. I wrongly subscribed to this overly pessimistic view. Job markets depend on broad social and economic trends in addition to demographic factors. During the 1980s, mathematics course enrollments responded to increased student interest in such fields as engineering and business. Numbers of new mathematics Ph.D.s decreased substantially. A steep decline in numbers of new Ph.D.s who were US citizens was only partially offset by an increase in the number of foreign student PH.D.s trained in the US. The academic job market for young mathematicians during the 1980s turned out to be better than during either the 1970s or the 1990s.

11. Family life in the 1970s. During the decade of the 1970s our sons grew from boyhood to manhood. Their academic records in secondary school were strong. As college undergraduates, Randy and Bill went to Brown, and Dan went to Stanford. After college, each son later earned a Ph.D. degree. They have gone on to successful careers in their respective fields, and each is happily married.

There were plenty of jobs for our sons to do at our big house on Colley Court. We paid them for their work. They did a lot of house painting, both inside the house and the entire exterior. Bill labored hard in constructing new beds for our expanding vegetable garden. This may have fostered his keen interest in gardening later in life. The boys always cut the lawn. This job was enjoyable since we had a riding mower.

The sport of ice hockey was very popular in New England during the 1970s. Each of our sons played hockey. Both Dan and Bill were members of the Barrington High School hockey team. Before the high school years, Flo and I logged many miles driving our boys and teammates to youth hockey games and practice sessions. Probably we visited every ice-skating rink within 50 miles of Barrington. The Brown University men's ice hockey team was good during the 1970s. We had season tickets and enjoyed many exciting evenings watching Brown hockey games. Those were good years for the Boston Bruins hockey team too, including Stanley Cup wins in 1970 and 1972. We watched the Bruins on TV.

As my career evolved, I became seriously overcommitted to various professional activities. It is not easy in mid-career to balance work and family. I could have done better in that regard. Sometimes my mood at home was foul. Flo once made a reversible sign to hang on my study door. One side said "welcome" with a smiling face. The other side said "Keep Out" with a skull and crossbones. I accepted too many invitations which involved travel. During those years, the slogan "Just Say No" was part of the US Government's War on Drugs. It took me too long to learn to apply this slogan in my own professional life.

On occasion we combined mathematical and family travel. Flo, Dan and Bill went with me to France, Switzerland and Italy in the summer of 1970. The mathematical events were a small conference in the village of Varenna on the shore of Lake Como, and the International Conference of Mathematicians in Nice. In April 1973, Flo and I celebrated our 25th wedding anniversary in Paris, during another visit to INRIA.

One of our favorite family recreations has been hiking amid beautiful mountain scenery. Among the early trips was an ascent of Mt. Monadnock in southern New Hampshire, when Bill was only 4 years old. Many of our finest memories are of hiking at high altitude in the Sierra Nevada Mountains of California. There is a system of High Sierra Camps in Yosemite National Park. Flo, Randy and I did a partial tour of these camps in 1964 and all of us did the complete High Sierra Camp circuit in 1966.

In later years, we explored other parts of the High Sierra far from roads, carrying tents and several days' supply of dried food. This is called "backpacking." In 1968 we made a family backpack trip to Lake Ediza, at the base of the spectacular Minaret range. This was followed by a hike up Mt. Whitney, which is the highest point in the US if Alaska is excluded.

Six years later, I had overscheduled mathematical trips for the summer of 1974. I decided to cancel my trip to the International Congress of Mathematicians in Vancouver, at which I had no duties. Instead, Flo, Dan Bill and I made another backpack trip to the Lake Ediza-Minarets region. This time, Dan and Bill were older and bigger. They carried more than their share of the load on their backs. Flo had only recently recovered from a broken Achilles tendon. It was a delight to see her leading the rest of us along High Sierra trails.

Randy and I made two more backpack trips into the High Sierra. The first was a memorable trek in 1973 into the Evolution Basin. In September 1976, we planned to do some climbing in the area just north of Mt. Whitney. Our plan was aborted by an out-of-season snowstorm. After spending 36 hours in our snow-covered tent, we retreated to our car. The car was at a much lower elevation and the sun was shining when we reached it. A year after that disappointing trip, Bill and I spent a memorable week in the Evolution region with fine weather.

In September 1990, I travelled again to the Sierra Nevada with my friend Martin Billett together with our sons Randy and Clifford. I hoped to view the Minarets once more from the shore of Lake Ediza, but this was not to be. Just as in 1976, there was unseasonably bad weather in the High Sierra in September 1990. However, all was not lost. Our planned backpacking trip was replaced by a visit to the Ancient Bristlecone Pine Forest nearby. Some of those trees are more than 4000 years old. At the end of the trip, Randy and I relaxed at the top of Mt. St. Helena at the north end of the Napa Valley and shared a bottle of local champagne.

12. Sabbatical year 1976-77. By 1975 I was due for another sabbatical but this was delayed until a year later. By the mid 1970s, I felt that my research efforts were stagnating. The distractions which I mentioned in Sections 9 and 10 had an effect. The book project with Ray Rishel (reference B2 in Appendix A) was time consuming. My 1976-77 sabbatical year was a productive one, and was of vital importance in reviving my research efforts for several years afterward. This sabbatical year was partially supported by a Guggenheim Fellowship. I am grateful to the Guggenheim Foundation for their timely support. My work with Michel Viot on measure-valued Markov diffusion process was done in spring 1977 (Section 16). I also began my work on large deviations and risk sensitive stochastic control (Section 17), and finished a paper on population genetics theory mentioned later in this section.

Unlike the 1968-69 year, I chose to use Rhode Island as my home base for the 1976-77 sabbatical. I made several trips of shorter duration. Among them was a visit to INRIA in spring 1977 during which the work with Viot was done. I spent several weeks of summer 1977 at Stanford. The process of “reentry” into the normal regime of teaching and other duties in the fall of 1977 was somewhat difficult for me. Colleagues have told me of similar feelings at the end of their sabbaticals.

During the 1970s, I became interested in population genetic theory. My interest was stimulated by Samuel Karlin, and also by colleagues in the Division of Biology and Medicine at Brown with whom I had many discussions. Population genetics theory is concerned with mathematical models of evolutionary changes which occur in natural populations over long periods of time. Frequencies of different genetic types in a population are changed by mutations, and also by selective advantages which some types have over others. There are also random matings and a random selection process by which those individuals which survive to reproduce are chosen from a much larger population of immature individuals.

The atmosphere at Stanford in summer 1977 was quite congenial. Stanford was a major center for research in evolutionary biology. Karlin's seminar was quite active. I finished a research paper (reference [13]) during the summer. That paper is concerned with the inheritance of quantitative characters, such as size or time to maturity. In [13] I studied a model in which genetic recombinations as well as mutations and natural selection are allowed. The main result is an approximate formula for the distribution of quantitative types in a population, assuming that the forces of mutation and selection are weak compared to recombination. I wrote this paper in the style of a "classical" applied mathematician, without rigorous mathematical proofs for the asymptotic formulas which I derived.

In the years after 1977, population genetics theory moved toward studying genetics at a molecular level. The colleagues in the Biomedical Division at Brown with whom I had interacted left for other universities. After 1977, I dropped population genetics as a research interest. I returned to work on topics related to stochastic control.

PART III. Research Contributions

13. Geometric measure theory. One part of geometric measure theory concerns the assignment of a k-dimensional measure to subsets of n-dimensional Euclidean space, when k is an integer smaller than n. Among several definitions of k-dimensional measure, the Hausdorff definition is the one which is most commonly used. Federer made fundamental contributions to the study of k -dimensional measures during the 1940s and 1950s. He identified a class of sets of finite Hausdorff measure, which he called rectifiable sets. Such sets differ in arbitrarily small k -dimensional measure from pieces of k -dimensional manifolds which are "smooth." The term smooth manifold means that the tangent plans vary continuously.

Another goal of geometric measure theory is to provide a general theory of k -dimensional integration, which can be applied in the calculus of variations as well as a variety of other problems in analysis and geometry. The Plateau problem is a typical example of the kind of geometric calculus of variations problems for which geometric measure theory methods are useful. In its original version, the Plateau problem was to find a two-dimensional surface in three-dimensional space which has the least area among all surfaces with the same one-dimensional boundary. The k -dimensional Plateau problem is similar. The k-dimensional area is to be minimized among all k -dimensional surfaces with given (k -1)-dimensional boundary. Of course, to make this precise, the terms "surface," "area" and "boundary" must be defined. The class of "surfaces" must be large enough that the existence of a surface with least k -dimensional area can be shown. For this reason, it will not suffice to consider only surfaces which are pieces of smooth k -dimensional manifolds, as in advanced calculus and differential geometry.

During the 1930's, the two-dimensional Plateau problem was studied using methods from what was called "surface area theory." J. Douglas received a Fields Medal in 1936 for this solution to the Plateau problem. However, the surface area approach had serious drawbacks. In surface area theory, surfaces are defined by maps from a given two-dimensional region into three-space. This fixes the topological type of the surfaces which are considered. Moreover, surface area methods were ill suited to the k -dimensional Plateau problem for $k > 2$.

The L. Schwartz theory of distributions provides a very convenient framework in which to study many problems in analysis. Functions on a finite dimensional space are a special case of Schwartz distributions. In taking derivatives, smoothess of a function is never an issue since every Schwartz distribution has derivatives of all orders. In 1951, L. C. Young introduced what he called "generalized surfaces," which have a role in geometric measure theory somewhat similar to Schwartz distributions. According to Young's definition, a generalized surface is a positive linear functional on a space of integrands for k-dimensional geometric problems in calculus of variations. Young defined the boundary of a generalized surface via Stokes' formula.

Other important steps during the 1950s toward a comprehensive geometric measure theory included Federer's work on the Gauss-Green theorem and curvature measures, as well as DeGiorgi's theory of sets of finite perimeter. In 1960, Reifenberg published a solution to a version of the k-dimensional Plateau problem. It was a "tour de force" but the technical difficulties which he had to overcome were formidable. Four years later, Peter Reifenberg died in a tragic mountaineering accident. While other people did not continue with his formulation of the Plateau problem, his ideas did have a significant effect on geometric measure theory afterward.

During the 1950s, L. C. Young and I collaborated on three papers on generalized surfaces. Among the questions which we sought to answer is the following. Consider generalized surfaces with given "elementary" boundary C. For example, for two-dimensional generalized surfaced, the boundary C can be identified with a finite number of disjoint closed curves of finite length. In the class of k-dimensional generalized surfaces with boundary C, does there exist one which is "rectifiable" and which minimizes the k -dimensional area? We were able to prove this in codimension 1 (k = n -1). A similar result when $k = n - 1$ could also be obtained from DeGiorgi's theory of sets of finite perimeter. When I arrived at Brown in September 1958, I hoped to prove the same result when $k < n - 1$. Success came a couple of months later. The key idea occurred to me while walking across the Brown campus, returning from a classroom lecture to my office.

I mentioned this new result to Herbert Federer, who immediately became interested. Before my arrival, Federer had been seeking a comprehensive approach to geometric measure theory which would be more usable and acceptable to nonspecialists than the older surface area theory. We began several months of intensive collaboration which led to our joint paper "Normal and integral currents" (reference [1]). Currents in the sense of DeRham are defined in a way similar to Schwartz distributions. In DeRham's definition, differential forms of degree *k* have the same role as test functions for Schwartz distributions. The boundary of a current is defined using Stokes' formula. Federer and I realized that currents provided a more convenient framework than Young's generalized surfaces. However, generalized surfaces reappeared several years later in some of Fred Almgren's work. Almgren gave them the more appealing name "varifolds."

An integral current T is one such that both T and its boundary are rectifiable, according to a definition similar to the one mentioned above for generalized surfaces. Federer and I each brought something to our collaboration at the beginning. My new result about generalized surfaces became the "closure theorem" in Section 8 of [1]. Federer provided the "deformation theorem" in Section 5 of [1]. Most of the other main results in [1] were produced collaboratively, during our nearly daily discussions. Federer also did a masterful job of organizing our work, and writing reference [1] in an appealing, readable style.

Reference [3] is a paper on the k-dimensional Plateau problem, formulated in terms of integral currents. There is one result in that paper which influenced the direction of further work by others. In three-dimensional space, with points denoted by (x, y, z), consider least area surfaces described by the equation $z = f(x,y)$ where the function f is smooth. The PDE which f satisfies is called the minimal surface equation. The Bernstein Theorem states that if f satisfies the minimal surface equation for all (x,y) in two-dimensional space, then f is a linear function. In [3], I gave another proof of this known result using geometric measure theory arguments. A key element in my proof was the result that any two-dimensional cone in three-dimensional space which is locally area minimizing must be a plane. An extension of the Bernstein Theorem would say that any function f of m variables which satisfies the minimal surface equation everywhere in m-dimensional space must be a linear function. In 1964, DeGiorgi used an approach like mine to show that this result is true for $m = 3$. The same result was extended by Almgren to functions of four variables, and then by Simons to functions of five, six and seven variables. However, Bombieri, DeGiorgi and Giusti showed in a remarkable 1969 paper that the conjectured extension of the Bernstein Theorem is false for functions of eight or more variables. If viewed as a theorem in analysis, such dependence on the number of variables m seems implausible. However, in geometry It is well known that new phenomena appear in higher dimensions. When viewed as a result in geometry, the Bombieri-DeGiorgi-Giusti result is less surprising.

In August 1962, J. P. Cecconi organized a small informal conference on geometric measure theory in Genoa. It was a good time to do so, since the field was evolving rapidly. I met DeGiorgi and Reifenberg for the first time at this conference. In 1962, neither Reifenberg nor I spoke Italian, nor did DeGiorgi speak English. The conference was conducted in what was called a "lingua mista" with the help of younger Italian mathematicians who spoke good English. Our Italian hosts guided Reifenberg and me around Genoa. However, we once set out on our own in a rented car to reach an afternoon session of the conference. We got badly lost and arrived two hours late. Our hosts had patiently adjourned to a nearby bar to wait until we showed up.

Afterward, I learned some Italian and DeGiorgi learned some English. He visited Brown in 1964. I met him in New York. On the drive to Providence, DeGiorgi told me about his Bernstein Theorem result, which he had proved during his voyage by ship across the Atlantic. The last time I saw Ennio DeGiorgi was at Cecconi's 75th birthday conference in 1993. DeGiorgi died in 1996. In October 1997, I was one of the speakers at a conference in his memory. It was held at the Scuola Normale Superiore in Pisa, where DeGiorgi had been a Professor for many years. There was a very large attendance at this conference, which is an indicator of the enormous respect and esteem for DeGiorgi in Italy and throughout the world.

Associated with any integral current is an orientation. If the integral current happens to correspond to a piece M of a smooth manifold, then the orientation is determined by assigning at each point x of M an orientation to the tangent space which varies continuously with x. Bill Ziemer considered the case when orientation is disregarded, in his 1961 Ph.D. thesis "Integral Currents Mod 2." Reference [7] is an extension of Ziemer's results. When orientation is disregarded, the DeRham current framework cannot be used. Integral currents are replaced by what are called "flat k-chains" in [7]. Flat k-chains are limits of polyhedral chains of dimension k, in a metric defined by H . Whitney. The main result of [7] is a "closure theorem" similar to the closure theorem for integral currents in [1]. It states that every flat k-chain of finite k-area is rectifiable.

Peter Reifenberg visited Brown during summer 1963. On one hot and humid afternoon during his visit, I outlined to Reifenberg some tentative ideas about how to prove this closure theorem. With his characteristic lack of tact, he said "Not even a third-rate mathematician would try to use that method." I took this comment as a challenge. By the following Monday morning, I had a complete proof of the closure theorem along the same lines which I had sketched on the Friday before.

Reference [7] is the last paper which I wrote on geometric measure theory. By the mid-1960s, the forefront of research was concerned with the "regularity problem" for integral currents which minimize k-dimensional area, or more generally which give a minimum in some other geometric calculus of variations problem. The goal in the regularity problem is to show that the support of any such minimizing current consists of pieces of smooth k dimensional manifolds together with a closed "singular set" of dimension less than k. Such regularity problems are very difficult. Almgren worked for many years to get his remarkable regularity results. By ability and temperament, I was not well suited to this kind of endeavor. I left geometric measure theory to work on stochastic control and other topics.

14. Stochastic control. I began working on stochastic control theory in the early 1960s. With some interruptions, I continued working on topics related to stochastic control throughout the rest of my career. The calculus of variations has been an ongoing interest of mine ever since graduate school. Around 1960, I saw a preprint by Bellman and Kalaba with the title, "On the foundations of a stochastic calculus of variations." I did not like the content of this paper, but the intriguing title caught my interest.

Optimal control theory became a "hot topic" during the late 1950s and 1960s. The work of L. S. Pontryagin's group in the Soviet Union was especially influential. However, the theory did not take into account unpredictable disturbances which may affect the control system dynamics. It had been suggested that such disturbances should be modeled stochastically. In addition, the Kalman stochastic filter model, introduced in 1960, received wide acceptance.

My first paper on optimal stochastic control (reference [4]) appeared in 1963. I considered a model in which the state of the system being controlled changes over time according to a stochastic differential equation. The disturbances were modeled as "white noise" processes, which are formally the time derivatives of Brownian motions. The objective is to choose controls which optimize the expectation of some criterion, which involves the integral over time of some "running cost" and perhaps also a cost associated with the final state of the system. In [4] the control is chosen as a function of time t and of the state at time t. These are called feedback controls, also Markov control policies. When a Markov control policy is chosen, the solution of the stochastic differential equation is a Markov diffusion process.

Optimal Markov control policies can be described by Richard Bellman's method of dynamic programming. In dynamic programming, the value function is the optimal expected cost, considered as a function of the initial data for the stochastic control problem. Under assumptions made in [4], the value function satisfies a nonlinear partial differential equation (PDE), which is of second order and of parabolic type. Fortunately, a recently developed theory of nonlinear parabolic PDEs was available. This theory included "*a priori*" estimates for solutions of the dynamic programming PDE and their derivatives. These estimates were essential for the analysis in [4].

The value function can be found explicitly only in a few special cases. One example with an explicit solution is the stochastic linear quadratic regulator problem, which had been solved by other authors before [4] appeared. Another example with an explicit solution is the Merton portfolio optimization problem. Merton's paper appeared in 1971. It was the first of many papers on stochastic optimal control methods in finance. See Chapter 10 of the Second Edition of [B3] and references cited there.

The assumptions of [4] include a "nondegeneracy condition." When this condition is not satisfied, the value function may not be smooth. Hence, it may not be a solution to the dynamic programming PDE in the usual sense. However, under milder assumptions the value function satisfies this PDE in a weaker (viscosity solution) sense. See Chapter 5 of [B3].

When the value function cannot be found explicitly, numerical methods for solving the dynamic programming PDE can be used to find it approximately. This requires that the state space of the system is of low-dimension, which is often a disadvantage from the viewpoint of applications.

Many results which are needed in stochastic control theory cannot be obtained using Markov control policies. Instead, controls are stochastic processes which do not depend on future random disturbances in the control system. For the controlled stochastic differential equation models considered in [4], such processes are called progressively measurable.

Reference [8] is joint work with Makiko Nisio. It concerns the existence of optimal progressively measurable control processes. Nisio was visiting Brown during the mid-1960s, having recently received her Ph.D. under the supervision of K. Ito. She brought to our collaboration powerful methods of stochastic analysis which I did not know before. Nisio became a widely recognized leader in the field of stochastic control. There are not many Japanese women mathematicians of Nisio's generation. Among them, I know of no others who reached Nisio's level of distinction.

My 1969 survey paper on stochastic control was already mentioned in Section 7. In 1970, Raymond Rishel and I began to write a book on control theory. This project took longer than anticipated, partly because of distractions already mentioned in Section 9. The book (reference [B2]) finally appeared in 1975 and is still in print. The first part, on deterministic optimal control, was an outgrowth of our lecture notes for graduate courses. The second part was intended as a readable introduction to optimal stochastic control. In the late 1980s, we were asked to make an updated version of [B2], but we declined. Instead, Soner and I wrote a different book, which is [B3].

15. Differential games. The theory of two-person zero-sum differential games concerns control systems in which there are two controllers with opposing objectives. One controller seeks to maximize a payoff P, and the other controller seeks to minimize P. Such differential games were first introduced by Rufus Isaacs in the early 1950s. He was motivated by games of pursuit and evasion. Isaacs' approach was based on considering a first order partial differential equation (PDE) for the differential game value function. This PDE is analogous to the dynamic programming PDE of deterministic optimal control theory.

It is now called the Isaacs PDE. In many examples, Isaacs was able to use the method of characteristics for this PDE to find what he called the value function. He then obtained optimal control policies for the two controllers.

Isaacs and I were colleagues at the RAND Corporation during 1951-53. My friend Len Berkovitz and I learned about Isaacs' method through discussions with him, and by reading Isaacs' RAND reports. One thing which was lacking in Isaacs' work was a mathematically precise definition of differential game value function. Berkovitz and I took an initial step toward addressing this issue, using the idea of "fields of characteristic curves" from the classical calculus of variations.

In the early 1960s, I gave a mathematically precise definition of value function for differential games on a given time interval (references [2] [5]). By considering time-discretizations of the differential game dynamics, discrete-time dynamic games were obtained. I proved that the corresponding discrete-time value functions tend to a limit as the time step tends to zero. The limit is what I called the differential game value function. In [2], the method of proof is elementary, but the differential game dynamics and payoff are of a special form. There is also an elementary proof if the value function is a smooth solution of the Isaacs PDE. Unfortunately, this smoothness property seldom holds. To avoid this difficulty, I introduced in [5] small random perturbations of the differential game dynamics. For the perturbed problem, the PDE corresponding to the Isaacs equation becomes of second order and of parabolic type. Solutions to this PDE with its boundary conditions are smooth. The desired convergence result is obtained in [5] by letting both the intensity of the random perturbations and the step size tend to zero.

The following question is interesting from a PDE viewpoint: can a solution to a given first order PDE be represented as the value function of some differential game? An affirmative answer is given in reference [6], under certain technical growth conditions on the PDE. This is a special case of a more general result in [6] about second order PDEs of degenerate parabolic type. For the general result, the differential game dynamics are governed by stochastic differential equations.

During the 1960s and 1970s, other mathematicians gave different definitions of value function for differential games. Among these is the convenient Elliott-Kalton definition. It turns out that all "reasonable" definitions of value function agree. This is shown by proving that the value function according to any such definition coincides with the unique viscosity sense solution to the Isaacs PDE with the appropriate boundary conditions. See Chapter 11 of the Second Edition of [B3] and the references cited there.

Many years later, I again became interested in differential games which arise as limits of risk-sensitive stochastic control problems. In 2006, I received an Isaacs Award from the International Society for Dynamic Games, based on research mentioned above and in Section 18.

16. Measure valued stochastsic processes. Etienne Pardoux and Michel Viot visited Brown in the summer 1974, at the suggestion of their advisor Alain Bensoussan. Both Pardoux and Viot had recently received the Doctorat d'Etat degree, in the area of stochastic partial differential equations. Soon afterward, I had fruitful collaborations with each of them, which will be discussed in this section and in Section 17.

In the fall of 1976, I attended a meeting of the Population Biologists of New England. One of the speakers (W. Ewens) gave an interesting lectures on a recent model of P.A.P. Moran. This model includes as a special case the Ohta-Kimura ladder model for interpreting changes in gene frequencies observed from laboratory data obtained by electrophoresis. The Moran model considers a population of fixed size N. Each individual in the population has a type, which is a finite dimensional vector of length J. The distribution of types in the population changes randomly over time. These changes are due to mutations and random matings of individuals in the population.

The Moran model is a continuous time Markov chain, with states in the unit simplex of J-dimensional space. I sought to find a kind of "diffusion limit" of the Moran model, obtained by rescaling time and letting population size N tend to infinity. One motivation for this was to obtain PDEs for the time evolution of mean joint densities of types. This would give alternatives to Moran's computational methods for finding mean joint densities.

In the spring of 1977, I visited INRIA and mentioned my ideas to Michel Viot. He was interested in diffusion limits of spatially distributed branching process models. It seemed that techniques which Viot was considering would also be useful for Moran-type models. Our collaboration led to reference [14]. The measure-valued Markov process which we obtained is now called the Fleming-Viot process. We collaborated in French. My bad French was better than Viot's English, and he was very patient.

For the Fleming-Viot process, the state is a probability measure m on the set of possible type vectors, with m(A) the frequency of type vectors belonging to a set A. A measure-valued Markov process with the required dynamics is obtained as the unique solution to a corresponding "martingale problem." The uniqueness property was crucial to the analysis in [14]. It is a consequence of the fact that there is a closed system of differential equations satisfied by moments of certain finite dimensional distributions associated with the measure-valued Markov process.

17. Stochastic control with partial state information. To apply the dynamic programming method mentioned in Section 15, the current state of the system being controlled is assumed to be known at each time t. This is the "complete state information" case. For the results in [8] about existence of optimal progressively measurable control processes, complete state information is also implicitly assumed. Late in 1978, I mentioned to Etienne Pardoux that the existence of optimal controls for problems with partial state information was an open question. During the following summer, Pardoux and I began a collaboration which led to reference [16]. We assumed that the controller can observe a nonlinear function of the state plus a white noise. These are the same kinds of observations considered in nonlinear versions of the Kalman filter model. In nonlinear filtering theory, the conditional distribution of the unobservable state given past observations has a central role. It is easier to work with an unnormalized version of the conditional distribution. This unnormalized version satisfies a stochastic PDE, called the Zakai equation.

In [16], we reformulated the original partially observed stochastic control problem as an equivalent problem in which the unnormalized conditional distribution has the role of a "hyperstate." The dynamics of the hyperstate satisfy a stochastic PDE similar to the Zakai equation. In the usual formation of the partially observed optimal stochastic control problem, only control processes based on past observations are allowed. In [16] these are called "strict sense" controls. To obtain optimal controls for problems with partial observations, Pardoux and I considered a somewhat wider class of controls which are limits (in a suitably defined sense) of strict sense controls.

The question of whether strict sense optimal control processes exist for this partially observed model aroused interest for several years after [16] appeared. Several people sought to prove that the answer is "yes" but failed. My intuition is that the answer is probably "no." However, some ingenuity would be required to obtain a counterexample.

18. Small random perturbations. Mathematical models for applications in the physical sciences and engineering typically depend on certain parameters. Results can often be found which hold asymptotically for parameter values which are very small (or very large), although it is not possible to find the quantities of interest exactly. During the 1970s, I worked on stochastic control problems in which the stochastic differential equation which describes the state dynamics depends on a small positive parameter e. The parameter e indicates the intensity of disturbances which randomly perturb the control system. For $e = 0$, the control problem is a deterministic one of the type considered by Pontryagin.

Reference [11] studies stochastic control problems of the kind mentioned in Section 14, for small positive values of e. It is rather easy to show that as e tends to 0, the value function tends to the value function for the deterministic optimal control problem with $e = 0$. However, this result is not useful in making statements about the behavior of optimal Markov control policies as e tends to 0. The reason is that these policies are expressed in terms of partial derivatives of the value function, rather than of the value function itself. In [11], approximate formulas for optimal Markov control policies are found. These are expressed as asymptotic series in powers of e, with coefficients which depend on the solution of the deterministic control problem with e=0. However, this asymptotic series is valid only in regions where the limiting (e=0) value function is smooth. These are called "regions of strong regularity" in [11].

Reference [10] considers the interesting case when the control problem with $e = 0$ is a calculus of variations problem. In that case, there is a largest region of strong regularity. The complement of this region is a closed set which is "small" in the sense of Hausdorff dimension.

In 1983, Takis Souganidis joined the Division of Applied Mathematics at Brown. The theory of viscosity solutions for nonlinear partial differential equations was new, and Souganidis was an important contributor to it. Viscosity solution methods made obsolete the kinds of complicated probabilistic techniques used in [11]. In [18], Souganidis and I gave viscosity solution proofs of results like those in [11], for the case of small random perturbations of calculus of variations problems.

The theory of large deviations provides asymptotic formulas for rare events associated with stochastic processes. In particular, consider a stochastic differential equation in which the random disturbances are of "low intensity" with the intensity depending on a small positive parameter e. The limiting differential equation with $e = 0$ describes the changes of the state of some deterministic dynamical system. Suppose that when $e = 0$ the state remains in some region D during a given finite time interval I. For small positive e, the probability that the state exits from D during the interval I is exponentially small. The Freidlin-Wentzell theory of large deviations gives an estimate for the large deviation rate. This rate depends on solving a calculus of variations problem. In reference [12] another proof of this large deviations result was given, using stochastic control methods. As a function of the initial data, the exit probability is a positive solution to the linear backward PDE associated with the stochastic differential equation. By making a logarithmic transformation of this solution, the dynamic programming equation for a stochastic control problem is obtained. Markov control policies for this problem correspond to changes of the drift coefficient in the stochastic differential equation. As *e* tends to 0, the value function for this stochastic control problem tends to the large deviations rate function.

In reference [15] Chun-Ping Tsai and I considered a similar situation for controlled Markov diffusion processes which depend on a small disturbance parameter *e*. The goal was to minimize the probability that this controlled stochastic process exits from a region D during a finite time interval I. We could not prove that the kinds of large deviations bounds used by Freidlin and Wentzell also hold in this case. Instead, we used the same logarithmic transformation method as in [12]. The logarithmic transformation changes the value function for the minimum exit probability problem into the value function for a stochastic differential game. One controller for this game is the same as for the exit probability stochastic control problem. The other game controller arises from a change of drift associated with the logarithmic transformation. The optimal large deviations rate function is obtained in the limit as *e* tends to 0. It is the value of the corresponding (deterministic) differential game. The methods of [15] depend on probabilistic estimates and stochastic control arguments. Later, Souganidis and I gave more straightforward proofs of these results, using viscosity solution methods. See [17].

The minimum exit probability problem is an example of what is called a risk sensitive stochastic control problem. For a much wider class of risk sensitive control problems, there are differential games associated with the small noise limit (*e* tending to 0). See Chapters 6 and 11 of the Second Edition of [B3] and the references cited there. These differential games have a natural interpretation in terms of what is called nonlinear H-infinity control theory. In the H-infinity approach, disturbances are modeled as deterministic (but unknown) functions rather than as stochastic processes. Risk sensitive control theory provides a link between deterministic and stochastic modeling of disturbances in control systems.

AFTERWORD

I continued as a Brown University faculty member for 17 years after 1978. During these years I was increasingly involved with Ph.D. students, postdocs and visitors. My book with Mete Soner (reference [B3]) appeared in 1993 with a second edition in 2006. I served two terms as chairman of Applied Mathematics, from 1982-85 and again from 1991-1994. There were also continuing obligations to find external grant funding to support graduate students and postdocs. From 1986-88, I chaired an international committee which produced the report, <u>Future Directions in Control Theory: A Mathematical Perspective</u>, SIAM Publications, 1988. This report was well received, but it required a substantial time commitment.

By 1995, I was ready to accept the generous retirement package which Brown offered. For several years after 1995, I was a regular visitor at North Carolina State University. Flo joined me for memorable visits to the Australian National University, with side trips to scenic New Zealand. I continued research on risk sensitive control and related topics. Since the 1980s, mathematical finance has become a major area of application for stochastic analysis. Several members of the stochastic control community have become leaders in academic research on mathematical finance. Among them are two of my former PH.D. students. Two other former Ph.D. students of mine have careers in the finance industry. Belatedly, I began in the late 1990s to work on some mathematical finance models too.

Mathematics is a young person's game. Already in middle age, it becomes more difficult to keep at the forefront of one's areas of research. While it is good to keep mathematically active in the later years of life, few groundbreaking new research contributions should be expected. A few years ago, Gian-Carlo Rota wrote an article for the Notices of the American Mathematical Society which I found comforting in this respect. According to Rota, younger mathematicians consider we old-timers as part of history. If one of us should happen to write a good research paper, it is a pleasant surprise. When we old-timers do work which is not very good, it is only as expected and doesn't matter.

Flo and I became "empty nesters" when our youngest son Bill left for college in 1978. During the 1980s, my parents and Flo's mother were in their 90s. They needed help in coping with the infirmities of extreme old age. The arrival of grandchildren, beginning with Sarah in 1992, provided us with new interests. By now (2007) we have entered a quiet period in our lives, which includes gardening, family visits, and our annual sojourn on the Maine coast. Flo is blessed by a strong Christian faith, which I am in the process of learning to share.

APPENDIX A

Short Vita

WENDELL H . FLEMING
University Professor Emeritus of Applied Mathematics and Mathematics
Division of Applied Mathematics
Brown University
Providence, RI 02912

Ph.D., 1951, University of Wisconsin

Employment

Rand Corporation 1951–55

Purdue University 1955–58

Brown University 1958–Present

Chairman, Department of Mathematics, 1965–68

Chairman, Division of Applied Mathematics, 1982–85, 1991–94

University Professor 1991 – 95

University Professor Emeritus 1995 –

Professor (Research) 1995 –

Visiting Positions

University of Wisconsin, 1953–54, l962–63

Stanford University, 1968–69 and Summer 1977

Institut de Recherche d'Informatique et d'Automatique

(one month visits: 1969, 1973, 1974, 1977)

University of Genoa (one month: 1973)

M.I.T., Fall semester 1980

University of Minnesota IMA, Fall semester 1985

University of Minnesota, Ordway Visiting Professor, spring quarter 1993

Honors

NSF Senior Postdoctoral Fellow 1968–69.

Guggenheim Fellow 1976–77.

Invited Plenary speaker, 1982 International Congress of Mathematicians,

in Warsaw. (Postponed until August 1983.)

Fermi Lecturer, Scuola Normale Superiore Pisa, 1986.

Steele Prize, American Mathematical Society, 1987.

Plenary Speaker IEEE Conference on Decision and Control 1988.

Doctor of Science, Honoris Causa, Purdue University, 1991.

Reid Prize, Society for Industrial and Applied Math., 1994.

American Academy of Arts and Sciences, 1995.

Isaacs Award, International Society for Dynamic Games, 2006.

Fellow, Society for Industrial and Applied Mathematics, 2009

Books

B1. *Functions of Several Variables*, Addison-Wesley, 1965, 2nd ed., Springer-Verlag, 1977.

B2. *Deterministic and Stochastic Optimal Control*, (with R.W. Rishel), Springer-Verlag, 1975.

B3.. *Controlled Markov Processes and Viscosity Solutions,* (with H. M. Soner) Springer-Verlag 1992, 2nd ed. 2006.

Selected Research Publications

1. Normal and Integral Currents, (with H. Federer), Annals of Mathematics, 72 (1960) 458-520.

2. The convergence Problem for Differential Games, J. Math. Analysis & Applic., 3 (1961) 102-116.

3. On the Oriented Plateau Problem, Rendiconti Circolo Mat. Palermo (2), 11, (1962) 1-22.

4. Some Markovian Optimization Problems, J. of Mathematics & Mechanics, 12, No. 1 (1963) 131-140.

5. The Convergence Problem for Differential Games II, Contributions to the Theory of Games, Annals of Math. Studies, No. 52, Princeton University Press, 1964, 195-210.

6. The Cauchy Problem for Degenerate Parabolic Equations, J. of Mathematics & Mechanics, 13 (1964) 987-1008.

7. Flat Chains Over a Finite Coefficient Group, Trans. American Math. Society, 121 (1966) 160-186.

8. On the Existence of Optimal Stochastic Controls, (with M. Nisio), J. Mathematics and Mechanics 15 (1966), 777-794.

9. Optimal Continuous Parameter Stochastic Control, SIAM Review 11 (1969) 470-509.

10. The Cauchy Problem for a Nonlinear First-Order Partial Differential Equation, J. Differential Equations 5(1969) 515-530.

11. Stochastic Control for Small Noise Intensities, SIAM J. Control 9 (1971), 473-517.

12. Exit Probabilities and Optimal Stochastic Control, Applied Math. and Optimization, 4 (1978) 329-346.

13. Equilibrium Distributions of Continuous Polygenic Traits, SIAM J. Appl. Math., 36 (1979), 148-168.

14. Some Measure-valued Markov Processes in Population Genetics Theory, (with M. Viot), Indiana Univ. Math. J., 28 (1979), 817-844.

15. Optimal Exit Probabilities and Differential Games, (with C-P Tsai), Applied Math. and Optimization, 7 (1981), 253-282.

16. Optimal Control for Partially Observed Diffusions, (with E. Pardoux), SIAM J. on Control and Optimization, 20 (1982) 261-285.

17. A PDE Approach to Asymptotic Estimates for Optimal Exit Probabilities, (with P. E. Souganidis), Annali della Scuola Normale Superiore Pisa, Ser. IV 23 (1986) 171-192.

18. Asymptotic Series and the Method of Vanishing Viscosity, (with P.E. Souganidis), Indiana Univ. Math. J. 35 (1986) 425-447.

Note: W. H. Fleming has written or coauthored a total of 134 research publications. The 18 papers listed above are those to which reference is made in Part III of these remembrances.

APPENDIX B

Wendell H. Fleming's Ph.D. Students

William Ziemer	1961	Mathematics
William Allard	1968	Mathematics
Virginia Warfield	1971	Mathematics
Wen-Hsiung Li	1972	Applied Mathematics
Charles Holland	1972	Applied Mathematics
Frank Lee	1973	Applied Mathematics
Chun-Ping Tsai	1974	Applied Mathematics
Paul Polansky	1977	Mathematics
Onesimo Hernandez-Lerma	1978	Applied Mathematics
Cherzad Shakiban	1979	Mathematics
Yu-Chung Liao	1982	Mathematics
Shuenn-Jyi Sheu	1983	Mathematics
Robert Laprade	1983	Applied Mathematics
H. Mete Soner	1985	Applied Mathematics
Robert McGwier	1988	Applied Mathematics
Dunmu Ji	1988	Applied Mathematics
Qing Zhang	1988	Applied Mathematics
Thalia Zariphopoulou	1988	Applied Mathematics
Toshio Mikami	1990	Applied Mathematics
Hang (Steve) Zhu	1991	Applied Mathematics
William McEneaney	1993	Applied Mathematics
Sun-Uk Park	1996	Applied Mathematics
Tao Pang	2002	Applied Mathematics