Chapter 1
A Memoir of My Professional Life: What I Can Remember and What I Can Tell

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I did not jump at the chance to write this memoir of my professional life. Indeed, each time John Smart, the former (and long-standing) editor of this handbook asked me—usually informally but at least once formally—whether I might be interested in writing such a memoir, my answer was some variation of "no." When, in the late spring of 2013, Michael Paulsen, the new editor of this handbook, sent me another formal invitation I was in something of a quandary. If I did accept (which I was now inclined to do), what could I even remember; and of the things I could remember, which ones should be made public? What would be worth telling about? How can one judge one's own contribution to the field of higher education? How generalizable is one's own story, and does generalizability even matter? What personal warts would I want to display? And on and on and on. When I consulted with my wife, June, about accepting the invitation, her response was immediate and brief: "Do it." Still, I continued pondering. After a while, I finally decided to put aside all my qualms, said "yes" to the invitation, and got down to business dredging up, reviewing, and writing about some of my activities over the years.

   The bulk of my adult life has been devoted primarily to my family, to Stony Brook University (especially its sociology department), and to research and scholarship in the field of higher education. While I will obviously have some things to say about the first two areas, I will be concentrating on the third area. To the extent that I have contributed to (and made a mark on) the field of higher education, it is largely through my writings and related pursuits. This being the case, I will be looking at this scholarly work with some attention to its substance.

   Rather than engaging in a strictly chronological trek from my early to later years, I believe it would be of more interest to begin with the two-volume book,

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"The Impact of College on Students," which Theodore Newcomb and I published in 1969 (Feldman & Newcomb, 1969), 4 years after I finished my doctorate in social psychology at the University of Michigan. It was this book, after all, that established my presence in the field of higher education and helped to determine the path my career was to take. After relating some of the ins and outs of writing this book, hereafter referred to as the Impacts book, I will move back to earlier times in my life and then forward to the years following the book's publication.

Hitting the Ground Running: The Impacts Book (1965–1969)

I was not in on the very most beginnings of the Impacts book. In 1964, the Carnegie Foundation for the Advancement of Teaching contacted Theodore Newcomb to suggest he apply for funding of a project of his choice on the study or practice of higher education. Naturally, the Foundation could not guarantee that his proposed project would be funded; nevertheless, it did want to encourage him to submit a proposal. Ted explained to the Foundation that, given his already full load of other projects and obligations, for him even to think about and write a proposal for another project, much less actually engage in it were he to receive funding, would be more than he could do alone. He told the Foundation he would like to ask a certain graduate student he had in mind, one who was close to finishing his PhD in the Social Psychology Program at Michigan, whether he would be interested in working with him on proposing a project to Carnegie. And so it was that I was asked to join an intellectual and research adventure that eventually produced the Impacts book published in 1969 by Jossey-Bass Publishers.

Staff members at Carnegie had thought of some projects they believed would be worthwhile, and they had passed these along to Ted for his consideration—grist for the mill, as it were. I can remember three of the four or five suggested projects: (1) planning and executing a national conference (with invited papers) on a topic or issue of particular importance to higher education; (2) a "survey of cases and practices"—that is, surveying the variety of institutional responses to student behavior as well as the different experimental programs designed to alleviate certain problems that were identified; and (3) collecting and systematizing the growing knowledge about the impact of college on a variety of student characteristics. Thus it was the Foundation that in essence suggested the integration or synthesis of the research on college impacts on students—a project that Ted and I found fascinating to think about and the one we eventually embraced wholeheartedly. We did write a proposal for this particular project, and it did get funded. We started our project in earnest in the fall of 1965, just after I finished and defended my dissertation.

And so it was that I remained at the University of Michigan, with a joint appointment as a Study Director at the Institute of Social Research and a position in the Sociology Department (first as a Lecturer in Sociology for two semesters and an Assistant Professor for three semesters) with minimal teaching responsibilities. During that time, I lived and breathed the Carnegie project, clichéd as that might

sound. I could not escape its "pull" on me. Even though I was newly married (for about a year) when Ted and I first started our work in the fall, I worked on the project whenever possible, including as many hours during weekends that I could cram in while still having a meaningful marital life, a social and cultural life, and the like. I remember always having two goals in mind for the finished product: It had to be good in itself, and it had to be useful to the field of higher education.

Past literature reviews and bibliographies were particularly useful in compiling our initial list of material to be read. To keep abreast of the latest research, we relied heavily on the continuing issues of "College Student Personnel Abstracts." We also wrote to some 500 individuals and institutions, requesting whatever bibliographic aid they might be able to supply. As we read, abstracted, indexed, and analyzed any given piece, we made a point of tracking down any references given in that item that seemed to be relevant to our topic. After a year and a half of this, we found very little in the way of "new" references that we had not already come across and considered in one respect or another. Our collection of materials—both published and unpublished items—was done well before resources were readily available from the Internet. What seemed like endless trips to the library were made in order to take out relevant books, to make Xerox copies of journal articles, and to arrange for and read microprints and microfiches of various reports, and related material.

To help us with the project, we were fortunate in having two highly capable graduate students as research assistants. Not only did Stanley Morse share the task of gathering and abstracting selected material for our book, but also his subsequent analyses and thoughts interacted with our own. In the year that Walter Swap was with us he focused primarily on the initial compilation and interpretation of information dealing with the impacts of major fields, which formed the basis of our analysis in Chap. 6 of the book. Judith Vartanian was absolutely indefatigable in virtually every aspect of the gathering of bibliographic materials as well as in the editing and typing of the many drafts through which our initial report was forged.

Although I wrote the first drafts of most (but not all) of the chapters of the initial report, each of the initial drafts, regardless of who wrote them, occasioned much mutual rethinking and shared re-writing. When Ted and I began our collaboration, I was a young whippersnapper in my late twenties and he a late-middle-aged whippersnapper. Despite the differences in our age and status in the field—or perhaps because of these differences—our collaboration went smoothly. As far as I can remember, Ted and I had only one major disagreement. One day we found ourselves at loggerheads about the exact meaning of the "accentuation effect" we were positing in the book. As we debated, Ted became increasingly exasperated. Finally, he stopped arguing, looked at me, and said quietly, with a mixture of sly humor, some agitation, and perhaps a touch of anger, "Ken, let's quit now, and each of us go home and pray to God, and maybe one of us will find some humility." I am not at liberty to report whether either of us ever did.

Ours was hardly the first attempt to analyze the impact of colleges on students by summarizing available research findings—some of the earlier efforts were listed in the report—but it was the most comprehensive and systematic syntheses of findings to its day. I know it may sound strange at this point to say that although I was

in part responsible for the push to comprehensiveness and systematic analysis, the scope of the project did worry me. I was concerned not so much with the effort required but by whether the final results would be too cumbersome. Despite the existing reviews of the relevant literature, there really was no good ready-made template for an analysis as large and comprehensive as ours. We had to work out a guiding conceptual and organizational framework—for which my knowledge of various sociological approaches and Ted's experience in the social psychology of higher education were crucial. While I might not go so far as to say that Ted and I provided "the first comprehensive conceptual map of generally uncharted terrain"— as Pascarella and Terenzini wrote in the preface to their 1991 book (Pascarella & Terenzini, 1991, p. xv)—I would say that we were successful in finding a conceptual and analytic framework that worked and made sense.

We finished our report to Carnegie in late fall of 1967. The Institute for Social Research produced a suitable number of offset-printed, spirally bound copies of the report for distribution. It was entitled, "The Impacts of Colleges Upon Their Students," and was subtitled, "A Report to the Carnegie Foundation for the Advancement of Teaching" (Newcomb & Feldman, 1968). Given the history of the project, it was to be expected that Ted was the first author and I the second. The date on the inside title page was January 1968, the very month that June and I moved to New York to begin our life at Stony Brook.

During the first 8 months or so of 1968, while I was at Stony Brook and Ted in Ann Arbor, we added a new chapter to the report (which became Chap. 9 of the Impacts book). We also revised the whole manuscript, getting it ready for commercial publication. When it was time to think about putting the report into book form—even before I left for Stony Brook—I approached Ted, with some trepidation I admit, and asked him what he thought about reversing the order of authorship for the published version. His reply was honest and characteristically charming. He said that if there were any way that he could justify his having first authorship he would take it, but he just could not think of a way. Thus the Feldman-Newcomb authorship was born.

At the time, McGraw-Hill generally published selected reports and studies sponsored by The Carnegie Commission of Higher Education and the Carnegie Foundation for the Advancement of teaching. So we sent off our finished manuscript to this publisher for its consideration. We were more than a little surprised that it was not accepted for publication. If my recall is accurate, its rejection had something to do with the manuscript being seen as not being cost-effective for publication. I will not go into my feelings at that point, but as it might be imagined they were far from happy ones.

We lost no time in sending our manuscript to Jossey-Bass Publishers, a new publishing company (established in 1968) that had just started to publish books in higher education. A short while later, I received a brief hand-written letter from Nevitt Sanford—a consultant for Jossey-Bass and a noted figure both in the field of psychology ("The Authoritarian Personality") and in the field of higher education ("The American College") (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950; Sanford, 1962). I was absolutely thrilled to learn from his letter that he liked the

manuscript very much and was recommending its publication. Thus, in 1969 "The Impact of College on Students" was published in two volumes: "An Analysis of Four Decades of Research" (Vol. 1) and "Summary Tables" (Vol. 2) (Feldman & Newcomb, 1969). To make the cost of the book less expensive, rather than setting Vol. 2 into print, Jossey-Bass used offset printing of our typed manuscript. It worked just fine.

Prequel: My Early Years

Before moving on to my Stony Brook years, let me back up to my early years before the Impacts book. I was born in Saginaw, Michigan, in 1937. When I was around 3 years old my family moved 15 miles north to Bay City, Michigan. This city is located near the base of the Saginaw Bay on Lake Huron—50 miles north of Flint, Michigan and 100 miles north of Detroit, Michigan. It had a population of roughly 50,000 in the 1940s and 1950s, but has declined to 35,000 since that peak.

As did other students living in my section of the city, I spent Kindergarten through the seventh grade in the same school, changed schools for the eighth grade, and changed schools once again when entering high school in the ninth grade. I cannot fault my public education, and received sound grounding in the basics. I always did very well in my classes. I had a facility for memorizing portions of whatever I read. It is not that I had an eidetic memory by any means, but that if I put in the effort I could memorize whatever I wanted to relatively easily. Thus I did well in classes. As I progressed through the levels of education, I had to work on interpreting and critically thinking about what I was reading rather than merely rote-memorizing it. This was harder to do.

At the same time that I was intent on doing well in school, I always wanted to "fit in"; even at an early age I developed a certain sense of humor that I felt would help me do so. As the years passed, I hoped that my humor was witty, or at least wry, but according to my daughter in her teen age and early adult years my humor was merely "corny." Now in her 40s, she says she appreciates my humor more than she did. (It is possible, of course, that she was right the first time.)

The architecture of Bay City Central High School, which I attended between 1951 and 1955, has a certain bearing on my story at this point. This school was originally constructed in 1922 as a two-story building, but not long afterwards a third floor was added in order to house the Bay City Junior College. When I went away to college in the fall of 1955, I did not really "go away" but simply moved up to the third floor of a building in which I had already spent 4 years on the first and second floors. On whichever floor I was located for my 6 years in this building, I found the teachers very much interested in students and in teaching. And I remained (as I was in elementary school) a highly dedicated student, determined to do well in every class. Because my family was far from affluent—my father was a janitor in a plant manufacturing automobile accessories and my mother was the main (executive) secretary in an insurance agency and later in an auto parts store—Bay

City Junior College was a way they could afford my first 2 years of college. They and I always planned that I would continue on with my college education after that. (I worked part-time in high school and college to help out.) In the same year that I graduated with an associate's degree from Bay City Junior College, incidentally, the voters of Saginaw, Midland and Bay counties formed a community college district and approved the construction of a new college, Delta Community College, which replaced Bay City Junior College. This community college, located southwest of Bay City, opened for classes in 1961 with 1,800 students and by the year 2000 had an enrollment of around 17,000 students.

When I really did go away to college, it was to the University of Michigan for the junior and senior years of my undergraduate education (1957–1959). To go from a very small junior college on the third floor of a building that housed my high school on its first and second floor to a very large state university was a shock; I expected it to be. I was more than a little "scared" for the first semester, but I adjusted fairly quickly. I found the sophistication of the university and of the city of Ann Arbor liberating, and loved my new circumstances. I never wanted to leave, and did not—at least not for 11 years. I tell people, tongue in cheek, that I stayed so long in Ann Arbor because I was a slow learner. In truth, I progressed in those 11 years from an undergraduate student to an assistant professor in the sociology department at the University of Michigan (and a study director at the Institute for Social Research). Here is how it happened.

When I transferred to University of Michigan, I had to pick a major. I toyed with majoring in psychology, but instead majored in pre-legal studies (a mixture of relevant courses from different departments) thinking that I would like to become a lawyer. During my senior year, however, I decided I would like to become a college teacher. At that point, my thought was to get a master's degree in sociology (which I always liked, along with psychology) and get a position at a junior college. I only applied to, and was accepted by, the graduate program in sociology at University of Michigan. Naively, I did not apply for an assistantship or fellowship for the first year of graduate school (a lapse that I remedied in the years to follow by applying for and receiving different teaching and research assistantships as well as fellowships).

In my first year of graduate work in the sociology department, my values and motivations changed enough for me to realize that I wanted to teach at the college or university level rather than at the junior college level. I also realized that as much as I liked sociology, I missed the psychological approach. So, as I was finishing my master's degree in sociology, I applied to and was accepted by the social psychology program at the University of Michigan. This well-known and highly regarded joint doctoral program, founded by Theodore M. Newcomb in 1946, accepted only students who had (or were close to receiving) master's degrees (or equivalent). Students in the program took graduate courses in both the sociology and psychology departments as well as proseminars in social psychology—the latter being taught by the likes of Guy E. Swanson, Daniel Katz, Dorwin (Doc) Cartwright, and Helen Peak, as well as Newcomb. I enjoyed the social psychology program tremendously, and could say much about it. For present purposes, however, I want to focus only on its importance in my becoming acquainted with Ted Newcomb, who was chair of the program.

In my first semester in the program (in the Fall of 1960), I took a proseminar led by Newcomb. I did not particularly stand out in the class in any way, and I remember being rather quiet in it. So I was more than a little surprised when the next semester Newcomb contacted me and asked to see me in his office. Newcomb, who was one of the main pioneers in the establishment of the field of social psychology, was widely known and admired in the social sciences. I knew about his many achievements, such as: his early research on students at Bennington (which came to be known as the Bennington Study); his establishment of the influential joint doctoral program in psychology and sociology; his publication of a particularly popular basic textbook in social psychology (Newcomb, 1950); his theoretical work on basic principles of cognitive balance connected with interpersonal attraction; and his Group House project, data from which was eventually the basis for "The Acquaintance Process" (Newcomb, 1961). (These and many of his other accomplishments are detailed in an excellent biographical memoir written by Philip Converse for the National Academy of Science [Converse, 1994]).

When I met Ted in his office, curious about why he wanted to talk to me, he asked me to be one of his two teaching assistants for a large undergraduate introductory social psychology course he would be teaching the following fall semester (1961). He would be giving one lecture each week to the entire class, and then his teaching assistants would each lead a smaller discussion group twice a week. When he asked me to be one of his two teaching assistants, my immediate thought was that I had been in the social psychology program for less than a year and I did not feel I knew enough to be a teaching assistant. So I declined his offer. Because Newcomb carried the mantel of his many achievements so lightly, I was not prepared for—and was even taken aback by—the uncharacteristic tone and demeanor of his response to my saying "no" to his offer. "Ken," he said, "the graduate students in this program are lined up around the block to be my teaching assistant and I've asked you, so you can't say 'no'." Well, since I could not say "no," I said "yes." Agreeing to be his teaching assistant turned out to be a major turning point in my life, although I could not know at the time just how major.

Conducting the two recitation sections each week was my first experience with college teaching. I took to it immediately, and still am enthusiastic about teaching some 53 years later (as of this writing). At the time, Ted was using in his class a draft version of his new social psychology textbook (written with Ralph Turner of Oberlin College and Philip Converse of the University of Michigan). He was eager for me to report how students liked it and also to let him know any of my own thoughts about it. I took this request seriously by passing on to him students' comments on the textbook as well as giving him any advice I had about its contents. I continued to be his teaching assistant for two more semesters (spring and fall of 1962). As he and his associates continued to work on the new textbook, I continued to offer any comments I had.

My association with Ted hardly ended with my teaching assistantship. He and Gerald Gurin (who at the time was a study director at the Institute for Social Research) asked me to join the Michigan Student Study (as an assistant study director). This project that they were heading was a large-scale study of students

at the University of Michigan and their experiences at the university. I was involved with others on the project—primarily Patricia Gurin and John O'Connor—in constructing the extensive interview questionnaire for the study and in training interviewers. I was also able to add a few questions on the interview schedule about students' type of commitment to social norms and the child-rearing practices of their parents, which data formed the basis of my dissertation (Feldman, 1966). (I received my doctorate degree in 1965, with Ted as the chair of my dissertation committee; the other members of the committee were Robert Cooley Angell from the sociology department and William Hays and Daniel Katz from the psychology department.)

Some time before Ted's textbook was to be published (Newcomb, Turner, & Converse, 1965), he asked me whether I would be interested in writing a study guide to accompany the textbook. I said yes, provided that I could work with another graduate student, John O'Connor, who was also in the social psychology program and a close friend. At that time, unlike today, graduate students were not expected routinely to have publications when they graduated, nor did many of them have any. I thought it might be a good opportunity to try my hand at writing something other than class papers or a dissertation. John and I worked together part-time for at least 6 months, producing a study guide of which we were proud. Study guides at the time were generally pedestrian in nature. We tried to be more imaginative. For each chapter of the textbook, we came up with a diagrammatic outline (visual overview), a matching exercise for the concepts of the chapter, multiple-choice questions on factual and theoretical content, brief (reprinted) selections from other sources (other texts, literature, etc.) with multiple choice questions about these selections, discussions questions, textbook-like extensions supplementing the chapter, and informal research exercises in social psychology. Because I was still working for the Michigan Student Study, progress on writing my dissertation was slowed up, but I felt it was worth it. The study guide in fact was my first professional publication (Feldman & O'Connor, 1965), and I was hooked.

Roughly framing the beginning and ending of my years as a graduate student was a major event in my personal life—meeting and eventually marrying June Tiefenbrun. During my very first year in graduate school in the master's program in sociology (1959–1960), as required I was enrolled in a two-semester practicum associated with the Detroit Area Study. Typically each year a professor—Harold Wilensky, in the year I was in the program—was authorized to conduct a sample survey on a subject bearing on his or her professional interest (work and leisure in Wilensky's case) for which interview data would be necessary. The students in the class received training in survey research by participating in the planning and construction of the interview schedules, actually conducting interviews in Detroit (along with a small cadre of professional interviewers), coding the completed interview schedules for quantitative analysis, and writing individual reports on some aspect of the investigation (which could count as their master's thesis).

It was in this practicum that I met June, who was a senior undergraduate majoring in sociology. She was taking the practicum as part of the senior honors program. I found her an interesting, bright, and very pretty young woman. She was an out-of-state student from New York (the Bronx) who had attended the Bronx High School

of Science. We went out a couple of times. I enjoyed her company, but no great sparks were generated. At the end of the spring semester (1960) of that school year, when she graduated with her undergraduate degree, she left the university to return to New York City. In the fall of 1960, she attended Columbia University where she obtained her master's degree in social work in the spring of 1962. I thought it unlikely that I would see her again after she left Ann Arbor.

In the summer of 1962, much to my surprise and quite by chance, I bumped into June on the main diagonal (called The Diag) of main campus. She was visiting Ann Arbor for a week or two on vacation (and staying with friends). After chatting for a while, I said to her, "I have nothing better to do, would you like to go to dinner with me tonight." (Some invitation! What was I? A tenth grader?) She said she could not because she was busy. And that was that, or so I thought. (At some point in later years, she confided to me that she had not been busy at all, but she was not about to accept such a gauche dinner invitation.) A year after our accidental meeting, when she was once again visiting Ann Arbor in the summer of 1963 to see friends, she called me up to say hello. By that time, I had wised up enough to say, "I really would like to take you to dinner if you're free." And so we went to dinner. One thing led to another, including sparks. We became engaged that same year and were married less than a year after that (in June 1964). We lived in Ann Arbor while I finished my dissertation and then worked with Ted on the report to the Carnegie Foundation. (Let me interject here that we are still married some 50 years later as of this writing, so the marriage seems to have taken.)

In the fall of 1966, even while I was working on the report to Carnegie, I knew it was time for me to start looking for employment at a university after I left Michigan. Although I was as assistant professor on a tenure track (with a joint appointment as a Study Director at the Institute of Social Research), I did not actively consider eventually putting myself up for associate professor with tenure. It was time to move on. I only considered sociology departments in my search, although with a degree from the social psychology program either a sociology or psychology department would be open to me. I was new to the recruiting process. Nowadays, many academic departments (including Stony Brook) prepare their students for job interviews, but this was not true in my day—at least not at the University of Michigan. I blundered through at least two of my interviews and did well in others.

I ended up having two offers that I seriously considered: Santa Barbara and Stony Brook. I picked Stony Brook for several reasons: I really liked the members of the department when I visited, and they seemed to appreciate my interests and talents; for some time—probably since high school—I dreamed about one day being in or near New York City, accessible to its theatre, museums, and other cultural attractions; and June's father, step mother, one of her aunts (who in effect became like a mother to her when her own mother died when June was only 15), her brother and sister-in-law, and still other relatives lived in the wider New York metropolitan area. Realizing that I would need to stay in Ann Arbor for the fall of 1967 to finish our initial report to the Carnegie Foundation, I accepted a lead appointment and agreed to start working at Stony Brook in the spring semester of 1968.

Starting Out at Stony Brook: Offshoots and Related Matters

It was hard to break away from Ann Arbor and the University of Michigan, but my new surroundings and circumstances at Stony Brook were exciting, which definitely helped a lot. My first 7 years or so at Stony Brook were productive ones on more than one front. As I now look back on those days, I wonder how I got so much done on all these fronts. I must have moved more quickly in those days, and I mean more than physically.

On the Home Front

Before moving from Michigan to New York in January 1968, June and I bought a home in Setauket, a hamlet immediately adjacent to the hamlet of Stony Brook where the State University of New York at Stony Brook (now called Stony Brook University) is located. June continued her career as a social worker, which she had started in New York and then pursued in Ann Arbor. Once we felt settled in, we decided to start our family, a life-changing decision as is well known. Elena Kay was born in May of 1970 and Daniel Jason in March of 1972. They are now in their 40s, and we are very proud of them. Elena married Thomas Clouser, Jr. in 2000; they have one child (Tobin). Daniel married Erin Kenny in 2004, and they have three children (Ronan, Shannon, and Rory).

Like all parents I suppose, I have many stories about them. I will limit myself to one—one that is especially connected to my professional life. Both Elena and Daniel went to good colleges—Washington University in St. Louis and Emory College, respectively. Each of them took an introductory course in sociology. Unbeknownst to one another, in different years they phoned home when they were preparing for their first exam in the course. Each one said to me something like, "Dad I'm worried about the first exam in my introductory sociology course. There are kids in the class who find the material hard and have trouble understanding it. I find the material so easy that I'm worried that I'm missing something and not preparing well enough for the exam." I told each not to worry. I said that I was sure that there was no hidden subtext that they were missing. If they found the material easy, then it was. They each got an A on the first exam. Some time later, I talked to each of them— again separately. I reminded each of them that when they were growing up I did not particularly talk to them about what I taught; I did not engage in formal (or even informal) lectures in sociology at the dinner table. Each of them said to me in their own way that they knew that, but that they recognized that the approach being taken in the sociology course was the same approach or way of looking at things that both I (and June) often times took when discussing with them their personal problems and experiences (or even matters of wider interest). They were accustomed to the particular approach or way of thinking, and so the course seemed easy to them. Since I teach about socialization in certain of my classes, I should not have been surprised by any of this but I was—a reminder to me that parents really do influence or affect their children even when they think they are not.

On the Job Front

The State University of New York at Stony Brook (or SUNY at Stony Brook) was founded in 1957 in Oyster Bay, Long Island, as the State University College on Long Island. What would become the current research university moved to Stony Brook in 1962. So when I came to this university—now more commonly called Stony Brook University—at the beginning of 1968, I was close to being in on the ground floor of the university at its present location. These were busy times for the university as it grew steadily to a much larger size. In 1968 the campus had around 7,000 students (undergraduate and graduate students). Today it has over 25,000 undergraduate and graduate students. It has been exciting to be part of this growth, especially in its early years.

When I arrived at Stony Brook, if memory serves, the sociology department had seven full-time faculty members: Andrew Collver; Norman Goodman; James Hudson; Gladys Lang; Kurt Lang; Ned Polsky; and Hanan Selvin. I felt very much supported by these colleagues, and really liked working in the department. The department's roster of full-time faculty rather quickly doubled in size, and continued to increase in number after that. The department started its graduate program with three students in the fall semester of 1967 (the semester before I arrived); this program also quickly grew in size. I started teaching a full-load (for a research university) of undergraduate and graduate courses. By the end of 1975, I had established and already taught all but one of the main undergraduate courses I was to continue to teach—with varying frequencies—at Stony Brook: introductory social psychology; sociology of youth; sociology of education; and sociology of identity. At the graduate level I primarily taught a course called "Socialization and Self," which I also continued to teach in later years.

During these early years there was much administrative and committee work to be done both in the department and at the university. For two of these years (1972–1974), I was director of graduate studies for our department. In each of these years we accepted around 35 students into our graduate program, which kept me very busy. I will not go into my other committee activities in the sociology department, except to mention a couple of committees on which I served: the undergraduate committee; and the administrative council (the chairperson' advisory committee). At the university level, for several years I was a particularly active member of the Policy Committee of Stony Brook's School of Continuing Education (now the School of Professional Development).

On the Research and Scholarship Fronts

After arriving at Stony Brook at the beginning of 1968, much of my scholarly activity was initially involved in readying the Carnegie Report for publication in book form during the spring and summer months, as already described. After that, for the next several years, I worked on a number of articles and one collection of

readings that in one way or another were linked to the Impacts book. In essence, they were extensions, elaborations, and explorations of the themes of the book. The term "offshoots" might best describe them.

Asked by the editor of the *Review of Religious Research* to submit a piece based on the Impacts Book on the change and stability of students' religious orientations during college, I worked on such a piece (divided into two parts), which was accepted after peer review (Feldman, 1969a, 1970). Three other pieces (Feldman, 1969b, 1971a, 1971b), taken as a set, outlined the ways in which research on college impacts has been done, pointed out the theoretical orientations and the analytic strategies underlying studies of college students, and highlighted some of the concomitant methodological problems and research issues.

Hanan Selvin, the chair of the sociology department when I arrived at Stony Brook, suggested to me that I would be in a good position to put together a book of selected readings on higher education, which I did: "College and Student: Selected Readings in the Social Psychology of Higher Education" (Feldman, 1972a). The names of some of the section titles of the book suggest its linkage to the Impacts book: "Change and Stability during the College Years"; "Assessing the Influence of Different College Environments"; "The Student and College Substructures"; and "Students, Student Culture, and Teachers." I wrote rather lengthy introductions to each of the seven main sections of the book. I dedicated the book as follows: "To Theodore M. Newcomb, who has pioneered, endured and prevailed in the social psychology of higher education." Some may recognize this as a partial crib from the speech that William Faulkner gave at the Nobel Banquet in Stockholm in 1950 (upon winning the Nobel Prize for Literature in 1949), but it did seem to me to be altogether appropriate to honor Ted. Because the anthology had some 32 reprinted articles and reprint costs were likely to be considerable, I anticipated that finding a publisher would be difficult. And it surely was. At least 20 publishers rejected the manuscript, but I persevered until one day I received word from Pergamon Press that it had decided to publish the reader as part of its "Pergamon General Psychology Series."

I have more extended comments about three other pieces—still offshoots—I wrote during my first years at Stony Brook, each of which carved out areas that remained part of my scholarly consideration over the years. They have to do with the value of research integrations and syntheses, the workings of the accentuation effect, and approaches to the study of college student change and stability.

The Value of Research Integrations and Syntheses

After the publication of the Impacts book, two of my departmental colleagues—who were hired after I was (one of them at a more advanced level)—separately told me in so many words that I had wasted my time in working on and writing the book. In point of fact, those were the exact words of one of them; the words of my other colleague were pretty close to the same. To put the best face on it, it might well be that my colleagues were trying to be of help by attempting to push me to do

other kinds of scholarly work (most likely, original quantitative research). I am not absolutely sure what the "worst face" was (or would have been), but after all these years I do not see any point in dwelling on it. In any case, I do not want to give the wrong impression about the sociology department. Others in the department were clearly supportive of my efforts.

My two colleagues' remarks did get me thinking about the value of research integrations and syntheses. Nowadays—and for at least the past 30 years or more—systematic large-scale research integrations as well as meta-analyses are common, taken for granted, and generally considered worthwhile. Such was not exactly the case, however, in the 1960s when Ted and I began working on our project. I may be exaggerating a bit, but at that time the gold standard was original research (and usually quantitative research at that). Integrations of existing research were generally seen as secondary to so-called original research.

One of the articles I wrote after the Impacts book was published, was meant (at least in part) to defend systematic research synthesis and integrations as being "original" research in their own right. Although the title of my article, "Using the Work of Others" (Feldman, 1971c) was light-hearted, the intent of the piece was serious. Moreover, unknown to me, Light and Smith (1971) also were working on a similar piece. They, too, argued for the importance of systematic research integrations and information syntheses, while calling for increased attention to the distinctive methods, techniques and strategies involved. From the current vantage point, my own effort at illustrating some of these methods now seems rudimentary, although I did touch upon many of the basic concerns—including those of finding and selecting studies to be synthesized, handling the so-called file drawer problem, establishing a metric with which to compare separate pieces of research, and creating procedures for presenting results. Even Light and Smith's more technically sophisticated article was only a beginning, as evidenced by comparing it with Light's later work in collaboration with Pillemer (Light & Pillemer, 1982, 1984; Pillemer & Light, 1980).

As late as 1974, Taveggia could still write that social scientists "are only beginning to develop an understanding of the methodological problems involved in cumulating social researches, and we know even less about how this strategy relates to the generally agreed upon goal of theoretical development in the social sciences" (Taveggia, p. 399). Integrations and research syntheses—especially meta-analyses—came into their own with the work of Gene Glass (e.g., Glass, 1977; Glass, McGaw, & Smith, 1981; Smith & Glass, 1977) and Robert Rosenthal (e.g., Rosenthal, 1978; Rosenthal & Rubin, 1978a, 1978b). And by 1984, Harrison Cooper was able to insist that "locating and integrating separate research projects involves inferences as central to the validity of knowledge as the inferences involved in primary data interpretation" (Cooper, 1984, p. 10).

Meanwhile, the reception of the Impacts book was just about as positive as one could hope for, which reassured me that the book was of worth and useful to the field of higher education. Sales of the book were immediately brisk. Moreover, Elton and Smart (1983) found the Impacts book to be first in number of citations from 1968 through 1977 among 51 Carnegie-sponsored research publications

and 51 Jossey-Bass publications. Citations to the book remained high. Budd (1990), in comparing citation counts in all issues of *Journal of Higher Education*, *Research in Higher Education*, and *Review of Higher Education* for the years 1982–1987, report that the Impacts book tied for third in number of citations. Even two decades after that, Budd and Magnuson (2010) using all issues of the same three journals for the years 2001–2006, the Impacts book was tied for fifth place in the number of citations. In Pascarella and Terenzini's judgment, the book, as of 1991, was "a classic, a standard text in graduate courses dealing with college students, as well as a standard and frequently cited reference for scholars, students, and administrators of higher education" (Pascarella & Terenzini, 1991, p. xv). And I cannot resist mentioning that in 2001 the book was included in a list of 100 classic books in higher education, with the authors of the list noting the following about the book: "a pioneering work whose combination of thoughtfulness and breadth helped put higher education on the scholarly map as a legitimate, coherent field of study" (Fincher, Keller, Bogue, and Thelin, 2001, p. 16).

The Accentuation Effect

Between 1971 and 1973, while teaching at Stony Brook, I was among a dozen or so researchers who were named research associates of the Institute of the American College Testing (ACT) Program in Iowa City, Iowa. We associates, along with a few invited others, participated in the institute's several interdisciplinary seminars that took place during these 2 years. My work with John Weiler (a graduate student in the Sociology Department at Stony Brook) took shape at this time, as encouraged and supported by the Institute. The article John and I produced (Feldman & Weiler, 1976), along with the work of a number of other associates of the Institute and participants in the seminars, eventually appeared in a book edited by three of the research associates (Sewell, Hauser, & Featherman, 1976).

    To describe my work with John, let me start by noting that an important leitmotif of the Impacts book was the possibility and actuality of "accentuation effects" during college. The analysis in the book describes the accentuation effect in at least three different ways. The existence of these three meanings was not exactly hidden, but neither was it especially emphasized. Moreover, a fourth meaning was not considered at all. In our work, Weiler and I clarified the conceptual and operational meaning of "accentuation" as a construct. At a group level of analysis, accentuation is used to describe increases in existing (initial) differences among groups or categories of persons. To qualify as accentuation in this context, for instance, the average scores of individuals in the group on some characteristic must "pull apart" but the relative positions of the groups' averages must remain roughly the same. At an individual level of analysis, accentuation is used to describe an increase in emphasis of an already prominent characteristic of an individual. That is to say, a prominently "high" (or prominently "low") attribute of the individual becomes even higher (or lower). Ted and I incorporated both kinds of accentuation—"accentuation of (initial) differences among groups" and

"accentuation of an individual's (initially) prominent characteristics"—into our book. A third kind of accentuation, which entails the increasing dispersion or "pulling apart" of initial differences among individuals, is implicit but not named in parts of the Impacts book; Weiler and I called this phenomenon the "accentuation of (initial) differences among individuals." Having demarcated these three types of accentuation, we also noted the possibility of a fourth type: "accentuation of a group's (initially) prominent characteristics." In short, analysis can focus on change in initial differences (among either groups or individuals) or on change in initially prominent characteristics (of either groups or individuals).

John and I focused empirically on the accentuation of initial major-field differences on various self-description indices of students and on their scores on the scales of the Omnibus Personality Inventory. Some instances of this sort of accentuation were found, but so were certain other patterns of changes in initial differences among major fields. I was to return to the study of accentuation almost 25 years later—to be described later in this memoir—but obviously I did not know that part of my story when I was working with John.

Approaches to the Study of College Student Change and Stability

As I was working on the Carnegie report and the Impacts book, it became evident to me that much of the research in the area was being designed and interpreted more from a psychological perspective than from a sociological one. I elaborated on this observation in a piece published in 1972 (Feldman, 1972b). In this article, I noted that a psychological approach—in particular, a developmental approach— to the study of change and stability of college students, student outcome variables that are usually chosen are either direct "growth" variables (e.g., degree of maturity) or are characteristics presumably more or less directly interpretable in such terms. Although the social impetus for change may be analyzed, more systematic concern is paid to the psychological dynamics of change; environmental and social structural parameters tend to be considered (if at all) only in so far as they immediately impinge on personality development. By contrast, investigators with a sociological orientation tend to choose student outcome variables that are not necessarily interpretable in terms of maturity or personality growth, and their approach is considerably more on the structures and dynamics of social pressures impinging on students than (if at all) on the internal psychological dynamics initiating change or buttressing stability.

My interest in the contrast between psychological and sociological approaches was clearly strengthened (though not formed) by my days as a graduate student in the social psychology program at the University of Michigan. And this interest remains to this day. I was able to revisit the distinction between a psychological and sociological stance in a foreword to Pascarella and Terenzini's, 1991 book (Pascarella & Terenzini, 1991) as well as in an introduction to the 1994 Transaction Edition of the Impacts book (Feldman & Newcomb, 1994) and in a research article published with Peter Kaufman in 2004 (Kaufman & Feldman, 2004). I think it

makes sense to say something about these three writings at this point rather than to try to fit them into this memoir in strict chronological order of their publication.

Somewhere around the mid 1980s I received an unexpected letter from Allen Jossey-Bass (of Jossey-Bass Publishing). He had received a letter from Ernest Pascarella and Patrick Terenzini proposing an "update" to the Impacts book. He sent along a copy of their prospective for the proposed book for me to look over and give any thoughts I had. I knew these two researchers only from an article or two I had read of theirs; I had never met them. The prospective was well put together, and the proposed book seemed worth publishing though it clearly was going to take an approach different from the one that Ted and I took in our book. For instance, the bulk of our book was organized around how social structures and arrangements (the diversity of college majors, differences in residence groupings, student peer groups and subcultures cultures, etc.) affected college students, whereas their book was to be organized largely around how the individual characteristics of students (cognitive abilities, attitudes and values, moral development, etc.) are affected by the college experience. I did not see this difference as a problem, and I had no inclination whatsoever to undertake an update of the Impacts book. I communicated all this in my return letter to Allen Jossey-Bass. I cannot remember whether it was at my suggestion or Allen's for me to become a consulting editor for the Pascarella-Terenzini book. I was very excited to be able to read the book as it materialized in draft form and to offer comments, including a variety of editorial suggestions. It was more like fun than work. (It probably takes an academician to feel this way.)

In the foreword I wrote for the Pascarella-Terenzini book (Pascarella & Terenzini, 1991), I lauded it as "monumental, but ... accessible," as "indisputably a milestone in the analysis of college effects on students," and as "enormous value to researchers, educators, administrators, and others interested in higher education." In this foreword I also briefly pointed out the difference in analytic stance between the Impacts book and the Pascarella-Terenzini book, noting that a psychological orientation—in particular, a developmental perspective—is an important aspect of Pascarella and Terenzini's view of the interplay between student and college that subtly underlay their analysis of college student change and stability. Yet, as I noted, they were anything but doctrinaire or one-sided in their analysis. Indeed, their psychological approach was heavily tempered by considerations of the nature of interpersonal settings of colleges, the structural and organizational features of colleges' social environments, and the institutional characteristics of colleges. I then pointed out that Ted and I essentially took the reverse tack in synthesizing the research on college impacts. We heavily tempered our more sociological approach with psychological considerations. As I wrote further, "to put the matter in its briefest formulation, whereas Pascarella and Terenzini lean toward *psychological* social psychology, we leaned toward *sociological* social psychology" (Pascarella & Terenzini, p. xiii). Because this formulation put the matter a little too simply, I was pleased to be able to explore in more detail the distinction between the two social psychologies in the new introduction I wrote for the Transaction Edition of the Impacts book, which was reprinted and published in 1994 (Feldman & Newcomb, 1994) .

Finally, in the late 1990s and early 2000s, Peter Kaufman and I (Kaufman & Feldman, 2004) were able to base an analysis of identity formation in college on my 1972 article (Feldman, 1972b). In the mid-to-late 1990s, I was the main advisor (chair) for Peter's dissertation. Using data from 82 in-depth interviews with a randomly selected sample of college seniors (from a large northeastern public university), Peter's dissertation and immediately subsequent work was concerned with how college students reflexively monitor their social-structural reality and either produce or reproduce middle-class identities (Kaufman, 1999, 2003, 2005). In further analyzing the data Peter had collected, he and I used a perspective grounded in sociological social psychology to show how the experience of college plays an important constitutive role in forming the felt identities of students. We looked at three domains—intelligence and knowledgeability, occupations, and cosmopolitanism—in which college students were especially likely to acquire (or significantly modify) their felt identities. One of our aims to was demonstrate the sorts of information and insights that can be gained from a *non*developmental approach to the study of college student change.

Studying Teacher and Course Evaluations: Mid-1970s–Mid-1990s

After finishing the book of readings and various articles, all connected to the Impacts book in one way or another, I was uncertain about what scholarly activity to pursue next. Eventually I decided on exploring the literature on students' ratings or evaluations of their teachers and courses, although the initial path toward the series on teacher evaluations was hardly straightforward and even a little curious.

When I teach sociology of identity at the undergraduate level or socialization and self at the graduate level, the interconnection of identity, social interaction and social structure is at the heart of the course. Even when I teach introductory social psychology (at the undergraduate level), the study of identity is one important section of the course. Early in the series of class lectures and as part of the assigned readings on the topic of identity, I introduce the following analytic divisions: the person's felt identity (the self-concept); the person's claimed identity (presented self); the identity attributed or imputed to the person by others; and the perception by the person of the imputed or attributed identity of others.

At one point in the early 1970s, when I was talking to a graduate student who was acquainted with this analytic classification, she raised the possibility that a teacher who claimed the identity of a very good teacher (whether it was objectively so or not) was more like to be seen as such (an attributed identity) than were teachers who did not present themselves in such a way. And this difference in attributed identities would be reflected in the ratings of teachers. Being intrigued by this notion, I thought I might be able to write a little piece about it. I was not particularly acquainted with the literature on teacher evaluation. The topic was not part of the Impacts book except for two articles on student's listing of characteristics of good

teaching or their best teacher as related to major field (Feldman & Newcomb, 1969, pp. 255–257). What I thought I might do was to look at the relevant literature and analyze or re-interpret it in terms of claimed identity and attributed identity despite the research not being couched in this language.

So I began reading the scholarly and research literature on teacher and course ratings. Even though there was much research in this area, and even though there were relatively good reviews of it, I nevertheless found myself disappointed. There were inconsistencies, disagreements, analytic confusions, and the like. I could not quite tell what the field knew and what it did not. I thought that the field could use some clearing of the underbrush. I actually had this metaphor in my mind, although it probably is an incorrect one. Organizing a messy closet might have been a better metaphor. At any rate, I naively thought it might take an article or two to do this clearing or organizing and to synthesize the research in the area. It turned out that what I thought would take me a year or two to do occupied much of my time for 20 years, and I never did get back to the original conjecture about presented and attributed identities (which I no longer thought was true in any case).

As I began reading the available research, I soon realized that two questions usually had to be answered in synthesizing the research in an area of interest. What empirical associations could be found? Once these empirical connections were established, what did they mean? Neither question was necessarily easy to answer. To actually establish empirical associations could be time-consuming and difficult, and supplying meaning to the associations or connections could raise not-easy-to-solve issues.

As one instance, consider my first piece in the series, "Grades and College Students' Evaluations of Their Courses and Teachers" (Feldman, 1976a). In synthesizing the research in this area, I found that based on the available research both the anticipated and actual grades of college students were positively related to their evaluation of their courses and teachers. In general, the size of the association was small but not inconsequential. The question then arose as to whether grades were biasing the evaluations. To put the matter crudely, do students anticipating (or actually getting) higher grades "reward" the teacher with higher ratings and students who are anticipating (or getting) lower grades "punish" the teacher with lower ratings? This question, however, cannot be answered by only knowing the association between the two variables (grades and evaluations). For example, the student's interest in the subject matter of the course—either a stable interest or a teacher (or course)-induced interest—may produce a higher grade in the course and a higher teacher rating (and bias may not be involved at all). The matter is even more complicated than this, including the necessity of bringing in still other factors to be considered, as the article spells out. Few of the available studies considered and controlled for student interest in the course or other factors—either individually or simultaneously. This meant that from the available studies at the time, I was not able to conclude that the association between grades and teacher/course evaluation showed that grades biased evaluations, yet I could not rule out such a bias. However, I was able to point out the kinds of data that still needed to be gathered and the sorts of analyses that still needed to be done.

All told, I published 14 research syntheses and an "afterword" for one of them. They can be grouped into a smaller number of categories, as follows:

- Effective college teaching from the students' view (Feldman, 1976b) compared to the teacher's view (Feldman, 1988) and/or other sources of evaluation (Feldman, 1989b)
- Consistency and variability of students in rating their teachers (Feldman, 1977)
- Rating of teachers as related to various course characteristics and circumstances (Feldman, 1978, 1979, 1984)
- Evaluations of teachers as related to their seniority and experience, their research productivity, their personality and attitudes, and their gender (Feldman, 1983, 1986, 1987, 1992, 1993)
- Indicators of achievement of students and their evaluation of their teachers (1976a, 1989a, 1990)

Even as new projects began to materialize for me that did not involve teacher/course evaluation, I did grab on to two unexpected opportunities to work on new pieces on teacher and course evaluations. My goals in both of them were to overview and reflect upon my prior research syntheses in the area. I wanted to do so in ways that would be interesting and even a little provocative, which I saw as preferable to presenting a mechanical summary of my work on the topic. First, I was commissioned by the National Center on Post-secondary Teaching, Learning, and Assessment to present a paper at the Second AAHE (American Association for Higher Education) Conference on Faculty Roles, to be held in New Orleans in January 1994. Second, at a somewhat later time, I was invited to give an address (in conjunction with my receiving the Wilbert J. McKeachie Career Achievement Award) to be presented to the Special Interest Group on Faculty Evaluation and Development at the 1995 Annual Meeting of the American Educational Research Association.

After delivering these papers, I refined and expanded both of them for publication. The first of these pieces was published in 1997 (Feldman, 1997) as a chapter in a book of readings by Raymond Perry and John Smart (Perry & Smart, 1997). This chapter, "Identifying Exemplary Teachers and Teaching: Evidence from Student Ratings," first explored the various interpretations that can be made of information gathered from students about their teachers (which included a consideration of possible half-truths and myths that continued to circulate about teacher and course evaluations). It then analyzed the differential importance of different instructional dimensions to effective teaching. The chapter was reprinted in 2007 (Perry & Smart, 2007)—with a commentary and update written by Michael Theall and myself (Theall & Feldman, 2007).

The second of these pieces was published in 1998 in *Higher Education: Handbook of Theory and Research* (Feldman, 1998). I called it, "Reflections on the Study of Effective College Teaching and Student Ratings: One Continuing Quest and Two Unresolved Issues." I found this article particularly "difficult" to write, for it involved a great deal of thought about unsettled issues as well as additional study and reading. I began my reflections with remarks on what can be seen as

a "continuing quest" in all social and behavioral sciences—namely, establishing the conditions and contexts under which relationships are manifested, are stronger or weaker, and are reversed in direction or otherwise different. Illustrations are given in three areas of interest in the study of effective teaching and student ratings: the connection (if any) between research productivity and instructional effectiveness; the differential influence of specific instructional dimensions on learning outcomes; and the association (if any) between the teacher's gender and instructional effectiveness.

I then continued the analysis by elaborating on two long-standing issues in the field that had never been fully resolved: one dealing with the question of bias in college students' ratings of teachers; and the other concerning the applicability of the traditional model of psychological testing to student ratings. For the first set of issues, I raised such questions as how bias can be defined, and when and how to control for (or otherwise take account of) bias so as not to eliminate or ignore legitimate effects of the teacher and course. With respect to the second set of issues, I asked how to separate the objective from the subjective aspects of student ratings; when (and how) to control or adjust for student subjectivity in order to make comparison of ratings across faculty and courses meaningful and fair; and how best to interpret mean scores on evaluation items and multi-item scales (as well as when it even makes sense to average scores of individual students in the first place).

As I explored various measurement, psychometric and statistical issues involved in studying effective college teaching and using student ratings, questions of *substance* emerged—such as what characteristics of teachers, classes, and courses actually affect teaching (whether or not they affect student ratings), which sorts of teachers get assigned to teach which sorts of classes, what students have in their minds when they view and evaluate their teachers, how the particular composition of students in a class might actually affect the instructional effectiveness of teachers (as well as the ratings made by students in the class), and the extent to which students mutually influence one another in their judgments about teachers (and just how do they do so). We have some of the empirical information necessary to answer these questions, but not nearly enough. As it turned out, then, I was doing more than encouraging the kind of research that would make student ratings even more useful to educationists than they already were. I was also showing how focusing on certain technical issues in student ratings would help expand knowledge of social cognition and social attribution (within the field of social psychology) and of teaching and learning in the college classroom (within the study of higher education).

A Mid-course Correction: An Incident in the Mid-1980s

While I was working on the teacher/course evaluation series, probably in 1985 or 1986 (or thereabouts), I phoned Educational Testing Service (I believe) for an ETS report I was having trouble getting. I talked to a staff person who managed the files of past ETS reports. After I gave my name to her, she blurted out, "I thought you

were dead." I assured her I was not, as far as I knew. Most likely she had confused me with Ted Newcomb who had died at the end of 1984. This incident turned out to be an important turning point in my professional life. Her surprise at hearing my name struck a vein in me. It is not that she did not know who I was; there was no reason that she should. Yet, somehow the incident, as it reverberated in replay in my mind over time, made me realize that I was visible in print but not in person. I began to think more about my visibility in the field, not just my visibility through my writings but also—how should I put it—my "corporal" visibility. It dawned on me that I might be committing professional suicide by rarely venturing from Stony Brook University. I finally realized the importance of being more active in professional associations and conferences. I know it seems late in the game for me to have realized that it is helpful for people to be able to associate a name with a face and personality, but the cliché applies: better late than never. Others are more likely to think of someone (me, for instance) for a paper to include in a book, or to be on a panel, or to be affiliated with a research project. On another side of the coin, I also began to realize that there were face-to-face ways that I could contribute to the field of higher education besides publishing scholarly papers.

My 20 years as a consulting editor for the *Journal of Higher Education* began in 1974, and did not involve face-to-face contact; my work was done in my home (or university) office. Likewise, my 30 years as a consulting editor for *Research in Higher Education*, which began in 1982, did not involve face-to-face interaction. It is true that I had become a member of the American Sociological Association in 1965, of the American Psychological Association in 1967 (and elected a fellow in 1984), of the American Educational Research Association in 1970 (and named as a fellow in 2008), of the Association of Higher Education in 1977, and of the American Psychological Society (as a charter member and fellow) in 1988. Yet I was not much involved in any of these associations. I am not saying that I was agoraphobic academically. I had attended conferences and symposiums, but they were few in number and primarily early in my career. I gave a paper at the American Sociological Association in 1967 (before the Impacts book was published). I also presented a paper at the American Educational Research Society in 1971 and 1972, at the Eastern Sociological Society in 1982, and at two or three other one-time or lesser known conferences. (I have already mentioned, earlier, my participation in the interdisciplinary seminars given under the auspices of the American College Testing Program between 1971 and 1973).

It is perhaps partly understandable in a way—but only partly—that I was not more involved with educational conferences and symposiums. My colleagues in the sociology department were primarily involved in sociological associations; they were not going to education conferences (yet alone higher education conferences), unlike faculty in departments and institutes of education or higher education. Moreover, Stony Brook did not (and still does not) have a school of education or a department of higher education. (There had been a short-lived department of education before I arrived in 1968).

The upshot of my newfound consciousness of the importance of face-to-face interaction with my academic colleagues in higher education was a decision to

attend relevant educational conferences. Thus, in 1987 I gave a paper at the American Educational Research Association (AERA) held in Washington DC that year. It was there that I first met John Smart in person (with whom I only had had written contact up to that point in his position as editor of the journal, *Research in Higher Education*). He gave me a warm welcome. I met some other people active in higher education, who seemed both surprised and pleased to meet me in person. I did attend several more AERA conferences after that, but the size and length of the AERA conferences as well as their timing (in the spring) did not mesh well with my academic schedule at Stony Brook and were not particularly convenient for me.

I participated in my first annual conference of the Association for the Study of Higher Education (ASHE) in the fall of 1988. I had been a member of ASHE since 1977, but had never attended any of its conferences nor been on any of its committees. There was a nice bonus of my attendance that year. The conference was in St. Louis, and my daughter had just entered Washington University in St. Louis a couple of months before the conference. So I was able to have dinner and visit with her one evening.

Being at that first (for me) ASHE conference was a wonderful experience. I felt as though I had found my professional home. I attended the ASHE conference almost every year after 1988, and became enthusiastically engaged in its activities. I presented papers, reviewed papers submitted for presentation at the conference, chaired sessions at the conference, and acted as discussant at certain sessions. I was program co-chair (for symposia) of the 1991 ASHE conference. I served on at least one dissertation of the year committee, on the Editorial Board of the *Journal of Higher Education* (as member in 1994–1996 and its chair in 1997), and on both the National Panel of the ASHE-ERIC Higher Education Report Series (1995–1998) and the National Advisory Board of the same series (1998–2005). I also served on the ASHE Board as an elected member from the fall of 1998 through the fall of 2000; I believe I made worthwhile contributions in my participation on this board.

After the 1988 ASHE conference, which I attended alone, June came with me to ASHE conferences. In this way, we were able to have some time together away from our routine activities at home, often in cities we had never before visited. During the day, while I was attending various sessions, she enjoyed investigating the environs in and around the city. She also would scout out interesting restaurants where some of my colleagues and we could eat dinner. Over the years, we met new acquaintances and made wonderful new friends.

The Years of Collaboration: 1990s–2000s

I found the years in which I focused on producing the series of research syntheses on teacher evaluations most satisfying. I had control over what I did—both in content and in timing of the 14 articles. The bulk of the work was usually done during

summers and intersession breaks. By the early 1990s, I had finished writing the integration of the research on college students' views of male and female college teachers, published in two parts in 1992 and 1993 (Feldman, 1992, 1993). I did not have another research synthesis in mind in the area of teacher evaluations. And I had not yet received invitations to write the two conference papers in 1994 and 1995 (Feldman, 1994, 1995)—previously discussed—that became the wrap-ups for the series on teacher evaluations when published in 1997 and 1998 (Feldman, 1997, 1998). So there was a moment—I am not sure it was much more than a moment—when I was really uncertain about what to do next. I even wondered just how much more I would be doing in terms of published scholarship. Quite fortunately, certain events led me toward long, happy productive collaborations with people I really liked in educational areas of great interest to me (teaching and learning; and the significance of academic majors). The following two subsections detail what these collaborations were and how they came about.

Teaching and Learning in Higher Education: Collaborating with Michael Paulsen

Teaching and Learning in the College Classroom

Having been a member of ASHE from 1977 and actually attending its annual conference from 1988 onwards, I obviously knew about the ASHE Reader Series. It occurred to me in the early 1990s (as I was thinking about what my next project could be) that I might produce one of these readers. I had already published a reader on the social psychology of higher education in the early 1970s (Feldman, 1972a), as mentioned earlier. Now, based on all the reading I had done on teachers and their evaluations by students—but branching out as well—I thought I could put together something on the college classroom, particularly the teaching and learning that goes on in this setting. No ASHE Reader existed in this area at the time.

I worked diligently to collect a group of readings that concentrated on actual research on the college classroom, and added some conceptual/analytic pieces on the topic as well. Upon sending my proposal off to the ASHE Reader Advisory Board, I must admit I felt rather confident about the proposal being accepted. During the 1992 ASHE conference, Daryl Smith, who was editor of the ASHE Readers Series at the time, arranged a meeting with me. She was very politic about letting me know that the proposal needed more work. The members of the advisory board liked the readings I did include, but felt that the contents of the reader were incomplete. Primarily using a social psychological lens, I had focused mainly on studies of classroom dynamics. My proposal was noticeably shy on practioner-oriented pieces as well as literature reviews. Daryl suggested that maybe I should search for a collaborator who knew better the areas with which I was less well acquainted. I did make an attempt during the conference to find a possible collaborator, but without success.

When I got back to Stony Brook, I was not quite sure where to turn next, and thought about giving up the project. Then, out of the blue, I received a phone call one day from someone who introduced himself as Michael Paulsen—someone whom I did not know and whose name I was not sure I even recognized. He told me that he and Daryl Smith had been talking, and that she suggested he call me. After a brief conversation, I told him I was looking for a collaborator and asked him if he would be willing to look over my proposal, and let me know what he would drop and what he would add to make the proposal more acceptable to the advisory board (and presumably more useful to the field of higher education). I figured it would not do any harm to learn about his knowledge in the area and any other relevant characteristics about him.

Not very long after our initial phone call, I received in the mail Mike's suggestions and a possible reworking of my set of readings. He recommended a more balanced representation of articles that presumably would better resonate with and meet the needs of the students who take courses in college teaching and learning. His suggestions included, but were not limited to, reviews of theoretical literature with implications for teaching practices (such as articles on learning theories and student development theories). He also recommended reviews of both theoretical and research literature with implications for teaching practice (for example, articles on teaching models and strategies). I knew immediately that I had found my collaborator.

Mike and I got to work right away—first by writing a new proposal for the reader, and then after its acceptance, making the final selection for the reader, writing introductions for the various sections of the reader, and supplementing each of these introductions with a list of additional readings. The reader, which was published in 1994 (Feldman & Paulsen, 1994), was sufficiently successful to warrant an updated and revised (second) edition in 1998 (Feldman & Paulsen, 1998).

As we started our collaboration, I learned from Mike that when Daryl suggested he contact me, she also said to him something to the effect that she believed that we would get along very well with one another. In today's parlance, she sure got that right. Mike and I became the closest of friends. We are obviously different enough to rule out having been separated at birth. Yet, we discovered important more-than-surface similarities in our character and personality—not the least of which is a certain compulsivity—that let us easily understand one another and work well together.

When Mike and I began working on the reader (and for some while thereafter), it was not the time of wide and easy Internet usage as we have it today. We had to send our typed drafts of our work for feedback to one another through the postal mail. We also needed extensive long-distance phone calls (not inexpensive at the time), which sometimes went late into the night. In those calls, when our academic work was done, we continued with exchanges about our professional and personal lives. Being 15 years Mike's senior in age as well as having more experience in academia, I became something of a mentor to him. And he, in turn, found opportunities to give me advice or counseling. Moreover, on those occasions when June and I were in

New Orleans (Mike's residence before he moved to Iowa), we spent some time with Mike and his wife, Laurey, at their home; she and June were able to befriend one another.

Toward a Reconceptualization of Scholarship

As Mike and I were finishing the first edition of the ASHE Reader, we talked about the possibility of our working together on another project having to do with a broader conceptualization of "scholarship." The focus of the project would be on expanding the construct of scholarship beyond the traditional and narrow notion that equates scholarship with research, toward an enlarged view that identified and articulated the multiple dimensions of scholarship. In the early 1990s, as were many others in academia, Mike was taken with Ernest Boyer's four component conceptualization of the scholarship construct (Boyer, 1990). Although he liked the spirit of Boyer's approach, he had some reservations about it. He felt that Boyer had not supplied a theoretical foundation for his proposed elements of scholarship. He began thinking about applying the systems theory and four-function paradigm of Talcott Parsons to the concept of scholarship—believing that the Parsonian approach offered a potentially meaningful theoretical perspective and analytic framework for an understanding of the elements of scholarship. He asked me if I would like to join him in his exploration.

Mike knew the Parsonian framework primarily from his reading of Parsons and Platt's book, "The American University" (Parsons & Platt, 1973). I was well acquainted with the Parsonian framework from studying his earlier work during my graduate days in the late 1950s and early 1960s (Parsons, 1951; Parsons, Bales, & Shils, 1953; Parsons & Smelser, 1956). Even so, I was initially lukewarm about working with Mike on this project. My hesitation was not because of not wanting to work with Mike, far from it; I was already very much enjoying our collaboration on the ASHE Reader. Rather, I was worried about how we would be able to make a rather complex conceptualization accessible. (Once we were actually working on the project, we found ways of doing so.) A larger issue, as I saw it, was that structural-functionalism had fallen out of favor in sociology, and to use this approach seemed at the time to be regressive. That is, Parson's approach has limitations because of its being primarily a consensus approach to the analysis of social action and thus avoids approaches embedded in conflict theory. Mike was able to assuage my misgivings— at least in large part—by pointing out a certain revitalization at the time in the Parsonian framework and improvements in it (e.g., see Alexander, 1984; Munch, 1987; Sciulli & Gerstein, 1985).

Previous investigations and analyses of scholarship had used *inductive* methods to identify the dimensions of scholarship. In contrast, we used the Parsonian four-function paradigm to *deduce* the four basic categories of scholarship. That is, we used this Parsonian paradigm as a heuristic device or guide to characterize and explain the activities of faculty in the scholarship action system (Paulsen & Feldman, 1995b). Our answers to the questions posed by the paradigm enabled

us to derive the four functional subsystems or dimensions of the construct of scholarship: (1) the pattern-maintenance function performed by actions constituting the subsystem of scholarship of research and graduate training; (2) the adaptation function performed by actions constituting the subsystem of scholarship of teaching; (3) the goal-attainment function performed by the actions constituting the subsystem of scholarship of service; and (4) the integration function performed by the actions constituting the subsystem of scholarship of academic citizenship. Of course, being able to put various activities of faculty into categories does not automatically mean that the classification system is useful. To guard against having created an essentially vacuous classificatory schema, we exerted much effort exploring some of the uses and applications of the four-category scholarship action system we developed. We also considered how a conflict approach would be relevant to our analysis.

Some years after the publication of our article, we extended our analysis to the study of the scholarship of the teaching action system, one of the four functional subsystems of the overall scholarship action system (Paulsen & Feldman, 2003). By again applying the Parsonian four-function paradigm to the scholarship of the teaching action system, we suggested that this particular subsystem itself has four subsystems, each one characterized by its own distinctive functional imperatives: (1) scholarship of pedagogical content knowledge (pattern-maintenance function); (2) graduate training (adaptation function); (3) reflective teaching (goal-attainment function); and (4) faculty evaluation and development (integration function).

Taking Teaching Seriously

At the beginning of the 1990s, Edgerton, Hutchings, and Quinlan (1991) wrote the following: "A movement that Patricia Cross labeled 'Taking Teaching Seriously' is spreading throughout the country. Campus after campus is reexamining its commitment to teaching and beginning to explore ways that teaching might be rewarded and improved" (p. 1). As Mike and I were working on our initial piece on scholarship, he was at the same time thinking seriously about the call from different sources for instructional improvement at the college level. He had already done some work on the subject and had preliminary plans to write a book-length monograph for the annual series of ASHE-ERIC Higher Education Reports. He asked me at some point if I would like to join him in his effort. This time I had no hesitation whatsoever in saying immediately, "Of course."

After we finished the first article on scholarship, we went to work on the monograph, which was published in 1995 (Paulsen & Feldman, 1995a). In this monograph, we addressed the question of what deans, department chairs, and other faculty leaders can do to encourage and support efforts of individual faculty members to improve their teaching. As a part of our analysis, we examined the nature of instructional improvement and the challenge of motivating faculty to improve their teaching, make the necessary changes in their teaching, and maintain those changes. We also explored the important factors in the creation of a supportive campus

teaching culture and presented detailed explanations and illustrations of five sources of feedback for improving instruction—teachers themselves, students, colleagues, consultants, and chairs—based on a review of the literature on successful practices. Finally, we analyzed the special needs of new and junior faculty for instructional improvement.

Whenever possible, our report emphasized the results and implications of research in an area of discussion. Within this empirical approach, we stressed the results of various research integrations, meta-analyses, and other sorts of research reviews. Many single pieces of research were also included—particularly those that were especially important to the development of an area of discussion, related most directly to a section's theme(s), presented distinctive data or otherwise filled certain research gaps in the field, or had important implications for practice and were likely to be useful to teachers, chairs, and administrators. Certain selective ideas, propositions, speculations, and suggestions were also included that had not necessarily been verified by research but about which there was some degree of consensus among analysts and practioners about their usefulness. At the same time, particularly fresh approaches that appeared to have some potential to improve teaching were included; that is, opinion was not avoided so long as it was *informed* opinion.

Epistemological Beliefs of Students

For some time, Mike had been interested in the importance of epistemological beliefs of college students—primarily the degree to which the *nature* of knowledge was considered by students to be "naïve" rather than "sophisticated" (simple knowledge; certain knowledge) and the degree to which the *acquisition* of knowledge was considered by students to be "naïve" rather than "sophisticated" (fixed ability; quick learning). Using data collected from students in a large public urban university in the spring semester of 1996, he and Charles Wells (Paulsen and Wells, 1998) found that these dimensions of epistemological beliefs were related to the disciplinary contexts in which students select and experience their specialized course work in college.

For our next project, Mike and I wanted to study the learning strategies used by students by further exploring the importance of the four distinct components of a system of epistemological beliefs he had already begun to study. In early and preliminary studies, based on the experiences of students taking undergraduate courses in education, we examined the four dimensions of epistemological beliefs and their relationships with six measures of motivational learning strategies (Paulsen & Feldman, 1999b) as well as four measures of cognitive learning strategies and four measures of behavioral strategies (Paulsen & Feldman, 1999a). We eventually used a sample more than twice as large as that in the two earlier studies and examined students taking classes in a variety of disciplines representing the humanities, natural sciences, social sciences and education. We examined both the conditional and interaction effects of the four dimensions of epistemological beliefs

on motivational learning strategies in one article (Paulsen & Feldman, 2005) and, in a second article (Paulsen & Feldman, 2007), on cognitive and behavioral learning strategies.

The Importance of Academic Disciplines in Student's Lives: Collaborating with John Smart and Corinna (Bunty) Ethington

Some time during the mid 1990s, I received an e-mail from John Smart, asking if I would be interested in working on some piece of research that we would mutually choose. He had acquired a data set obtained from surveys done by the Cooperative Institutional Research Program (CIRP) sponsored by the Higher Educational Research Institute at the University of California, Los Angeles. He wondered if there were some portion of the data we could work on together that would meet both of our research interests. At that point, I would not say that I knew John well. I had had contact with him in his capacity as editor of *Research in Higher Education,* and we had had enlightening and enjoyable conversations at ASHE. Yet, being intrigued by his offer, I replied that indeed I would be interested in seeing if we could work something out.

We decided to try to integrate two analytic frameworks in analyzing the data. The first framework was brought by John based on his longstanding interest in John Holland's theory of careers (Holland, 1966, 1973, 1985, 1997), a part of which can be used to develop theoretically meaningful and empirically defensible clusters of academic disciplines or departments (Rosen, Holmberg, & Holland, 1989). I brought the second framework by returning to my earlier interest in "accentuation effects" as an aid in the study of complex student change and stability during college years. In a paper we published in 1998 (Smart & Feldman, 1998), we used the Holland-derived classification of major fields to explore all four kinds of accentuation: accentuation of initial group differences; accentuation of groups' initially prominent characteristics; accentuation of initial individual differences (within a group); and accentuation of individuals' initially prominent characteristics.

Our collaboration went smoothly, and as we were finishing our research, John inquired about my working with him and his wife, Bunty, on a book that he had in mind and hoped one day to write. I thought it was definitely worth a try. I knew Bunty at that point from talking to her at ASHE conferences. The roundabout way in which I first met her is worth telling. As I mentioned earlier, in the late 1980s I had begun to attend ASHE conferences regularly. I believe it was at one of these conferences, in 1989 or 1990, that I attended a session where a Corinna Ethington was one of the presenters. She gave what I thought to be an interesting paper—intelligent, nicely written and very well presented. I was much surprised when the discussant for the session offered what I thought were particularly ill-considered comments on the paper. At the end of the session, I went to the presenter (whom I had never met), not to talk to her about the comments of the discussant, but to

tell her about how clearly she presented the material of the paper. I remember my saying to her that she most likely was a wonderful teacher in the classroom. Little did I know at the time that Corinna Ethington was "Bunty" Ethington and was the wife of John Smart. And I obviously had no inkling whatsoever that 10 years or so into the future I would be agreeing to write a book with her and John. All I can say is that Bunty and I surely got off on the right foot.

It took a little while—but not as long as might be imagined—for the three of us to adjust to working with one another. We became a productive working team rather quickly. John was clear about the point of view he wanted to take and how he preferred to interpret findings, yet he was always open to suggestions and was usually willing to modify his position if a reasonable argument could be made to do so. Bunty was the methods/statistical expert for the team as well as the executor of the computer runs. But she was even more—a sort of gatekeeper. If there were a mistake in logic, or an implication not spelled out, or an unwarranted inference, or a fuzzy explanation, or a contradiction between different parts of the manuscript, she would find it. I took to calling this ability her X-ray vision, and often marveled at it (although I am no slouch myself at this sort of thing.)

Much of our collaboration could be done by exchanges through the Internet. But not all of it. Our collaboration entailed my making a half dozen (or so) 3-day weekend trips to Memphis where John and Bunty lived. We worked very hard during those weekends, but not every minute. When we were not working, usually at nights, John and Bunty showed me the sights and attractions in Memphis and surrounding areas. A few times, we even ventured to the casinos of Tunica, Mississippi—primarily for their diner buffets, I might note. I do like electronic poker, and would hand over $20 or $30 to the machines on the way either to or from dinner (the last of the big spenders, so to speak).

I believe it is fair to say that John does not have a demeanor that can be characterized as effusive. So I was not prepared to read the following sentence referring to our collaboration in his initial draft of the preface for our book: "A collaboration that works is a joyful experience." Feeling that this characterization was a little "over the top," I convinced him to dial down the emotion one notch by changing the word joyful to exhilarating, which is how the sentence now reads in the published preface. But I was wrong; John was right the first time.

Regarding the substance of the book (Smart, Feldman, & Ethington, 2000), working with John and Bunty let me reexplore my long-standing interest in the significance of major fields in students' lives as well the comparison between a psychological approach and sociological approach to the student change and stability of college students. I both respected and admired John's steadfast allegiance to the usefulness of Holland's theory (Holland, 1966, 1973, 1985, 1997)—originally developed to explain vocational behavior—as an illuminating, theory-based framework for research on college students and faculty. Following John's lead, I became convinced of the appropriateness of Holland's theory for exploring aspects of the professional lives of faculty and for examining both the choice of academic majors by students and their subsequent patterns of change and stability in abilities and interests.

The findings of our book generally supported, albeit with varying degrees of strength, the three basic assumptions of Holland's theory: self-selection assumption; socialization assumption; and congruence assumption. To examine the validity of the self-selection assumption that students search for and select academic environments that parallel their personality types, we explored differences in the self-rated abilities and interests of those who initially selected different kinds of academic environments and also sought information about the proportions of students who initially selected academic environments analogous with their dominant personality types. We examined the validity of the socialization assumption that academic environments reinforce and reward differential patterns of abilities and interests in students—irrespective of their personality types—by exploring longitudinal patterns of actual change and stability in the self-rated abilities and interests of students in the four academic environments we studied and by their estimates of self-growth over a 4-year period. Finally, our analyses to assess the validity of the congruence hypotheses also involved the longitudinal patterns of change and stability in the self-rated abilities and interests of students and their estimates of self-growth over a 4-year period. In this case, however, we focused on the congruence or fit based on the dominant personality type of students *and* their academic environments.

A particularly interesting and important finding of our research was that the congruent and incongruent students in the same academic environment made parallel gains in interests and abilities during their college years (though incongruent students started and ended lower than did the congruent students). We further explored this finding in a separate article (Feldman, Ethington, & Smart, 2001) in terms of the relative importance of psychological forces and social forces in their respective contribution to students' change and stability in college. We interpreted the results in terms of a socialization dynamic being more prevalent than a personality dynamic (though both dynamics were evident). We continued to reflect on and expand these results when we were commissioned by the National Postsecondary Education Cooperative (NEPC) to participate in (and prepare a report for) the 2006 National Symposium on Postsecondary Student Success (Smart, Feldman, & Ethington, 2006). This report formed the basis of a chapter we wrote for the 2008 edition of *Higher Education: Handbook of Theory and Research* (Feldman, Smart, & Ethington, 2008).

For both the report and the handbook chapter, we analyzed additional data (that did not figure into our earlier book) and also reframed our analyses in terms of "student success." We found support for both the traditional definition and an alternative definition of student success derived from Holland's theory. Support for the traditional definition, based on the *congruence* assumption of Holland's theory, was shown by the likelihood of students further developing their initially prominent characteristics was basically contingent on their selection of a congruent rather than an incongruent academic environment. Support for an alternative definition of student success, based on the *socialization* assumption of Holland's theory, came from the clear evidence of a consistent pattern of student growth in the distinctive set of abilities, interests and competencies required, reinforced and rewarded by each of the four academic environments we examined irrespective of the students'

dominant personality type. For each of the four personality types we examined, any appreciable growth in the four sets of abilities, interests and competencies was, for the most part, dependent on the academic environments of their major field of study (whereas students tended to remain stable or decline in the three other sets of abilities, interests and competencies that are not reinforced or rewarded by the academic environment of their major field of study).

Having analyzed additional data on student profiles for patterns of change and stability, we were able to conclude that, on the one hand, the more psychologically oriented component in Holland's theory, manifested in the congruence assumption, leads to a more peaked profile of student success in which students' initially prominent characteristics become more pronounced and their other sets of abilities, interests and competencies tend to remain essentially stable or to decline. This profile has more in common with the vocational or occupational perspective of student success in that it is wholly reflective of the most common application of Holland's theory, which intends to assist individuals in selecting careers where they have the greatest likelihood of success. On the other hand, the more sociologically oriented component in Holland's theory, manifested in the socialization assumption, leads to a more balanced profile of student success in which students remain stable or decline slightly in their initially prominent characteristics and grow considerably, sometimes dramatically so, in the set of abilities, interests, and competencies reinforced and rewarded by their chosen (albeit incongruent) academic environment. The more balanced profile of student success that emerges from greater attention to the socialization assumption of the theory has more in common with the liberal arts perspective of student success, which emphasizes the need for students to develop a broader repertoire of competencies and interests to function successfully as citizens of a democratic society. In either of these two cases—the more peaked profile or the more balanced profile—the environment of the academic discipline is absolutely central in importance.

Rounding Out the Picture (a Little)

Because of the selective focus of this memoir, I may have given the misleading impression that most of my adult life has been spent researching and writing. Even professionally, this is not true. If I were to count up hours, I would say that my teaching (and closely related) activities have actually taken up more of my time. This certainly is the case if I add in service/volunteer activities. Generally, my intensive periods of writing have been done during my winter and summer breaks from teaching. At other times, when I was not entrenched in my university, family and community life, rather than working on the first (original) draft of a piece I more likely would be engaged in background preparation for writing a piece or additional editing and re-writing an already carved-out first draft. A case in point is this memoir itself. I started it in the summer of 2013, put it mostly aside when the fall semester (2013) at the university started, worked a little on it during the winter

break between semesters, and again put it mostly aside during the spring semester (2014) of the university, and only now am completing it during the summer of 2014 (which has included re-writing parts of it and adding material I did not have time to consider earlier).

I will not be able to delve into my teaching, service and family activities in any great detail, but I do have some things to say about them. First off, I have greatly enjoyed teaching, and have done so (except for sabbatical semesters) every semester for the 46-plus years I have been at Stony Brook. I even taught during the summers for eight of my first 10 years at Stony Brook. In fact, one of the reasons I have not yet retired is that I want to continue teaching. I listed previously the courses that I taught in my early years at Stony Brook and have continued teaching them (introductory social psychology, sociology of youth, and sociology of identity). At some point, I dropped sociology of education from my repertoire of courses and at another point I added sociology of the "life course" (the term sociologists prefer over "life cycle"). I have taught other undergraduate courses, but each only infrequently, and I will not mention them further.

When my undergraduate courses are not large, I do my own grading as well as advising of students who need extra help or who just want to explore further the material of the class. When these undergraduate courses are large, I am assigned at least one graduate student assistant (TA) to help with grading and assisting students in the class. With very few exceptions, the graduate students with whom I have worked over the years have been excellent—hardworking, clearly enthusiastic about helping undergraduates, and conscientious in their grading of exams. Whenever I had good teaching assistants, I would try to arrange working with them (if possible) for more than one semester.

As for graduate courses, I continued to teach "Socialization and Self" after my early years at Stony Brook. I also began to teach "Sociology of Education" at the graduate level (for a while) and added a course called "The Three Faces of Social Psychology," based on an article of the same name written by James House (House, 1977), a fellow graduate of the University of Michigan's social psychology program. In the spring semester of 1992, I initiated a dissertation seminar in which I helped prepare graduate students in our department for their oral specialty exams and the defense of their dissertation proposals; I directed this seminar many times after that. Again I am skipping over some other graduate courses that I taught infrequently.

I should note what, perhaps, might be considered an "oddity": the substance of my teaching at both the undergraduate and graduate levels has had little overlap with the research topics I have pursued. Few readings in higher education appear in my syllabi, though there is slightly more about higher education in some courses than in others. I could argue that keeping my teaching and research largely separate has kept me "fresh" in both areas. This may well be an after-the-fact rationale, however. I am not quite sure. In any case, the relative lack of overlap has not bothered me.

Not only have I been active in the classroom, I also have been busy over the years mentoring students individually (especially graduate students) and working with graduate students on their dissertation (either as their main advisor or a member

of their dissertation committee). I feel honored that my teaching and mentoring activities were recognized by a 1994–1995 President's Award for Excellence in Teaching, Stony Brook University, a 1994–1995 Chancellor's Award for Excellence in Teaching, State University of New York, and the 2004 Dean's award for Excellence in Graduate Mentoring by a Faculty Member.

I should add some information here about four other awards I have received that are national in scope. The first one, which I mentioned earlier, was the 1994 Wilbert J. McKeachie Career Achievement Award of the Special Interest Group for Faculty Teaching, Evaluation, and Development (SIGTFED) of the American Education Research Association (AERA). I did expect that I might someday possibly receive this award because of my many integrative pieces on teacher evaluation. As for the next two awards—the 1995 AERA Distinguished Research Award for the Postsecondary Education Division (Division J) and the 1996 Research Achievement Award of the Association for the Study of Higher Education (ASHE)—I am honestly not being disingenuous in saying that they came as a surprise since they were not particularly on my radar as possibilities. Still, I felt most grateful to have received them. Although I did not know about my chances for receiving the fourth award, the Howard R. Bowen Distinguished Career Award of AHSE, I obviously was aware of its existence since this is one of the major awards awarded by ASHE. I did hope in the later years of my career that I had accomplished enough in the field of higher education to warrant the award. When, in fact, I received it in 2009, I was thrilled (and still am).

So far I have said little about a set of activities that has taken a large chunk of my time and energies over the years. I refer to my service on various local committees, councils, and boards. Being an active member of them has been a significant part of my professional life. There have been a lot of them over the years—too many of them even to list here. I will single out six of these committees—three in the sociology department and three outside of the department—to help further round out the picture of my professional life.

Regarding departmental committees, I was chair of the sociology department's Comprehensive Exams Committee for several years (from 1976 to 1978 as well as during the spring semester of 1982 and the school year of 1985–1986). When the department replaced the system of comprehensive exams to be taken by graduate students with a track-paper system for graduate students, I (eventually) became a member of the Track Paper Editorial Board (Fall 2003–Spring 2005), and have been the chair of this board since the fall semester of 2006. In addition to assigning faculty members to students writing their track papers, the board also is in charge of evaluating papers (whether or not they are track papers) submitted by students for departmental awards. To continue, a particularly important committee of the department is its Faculty Recruitment Committee, the committee that helps solicit applications for faculty positions in the department, reads the files of applicants (including their submitted papers whether published or unpublished), and hosts the visit to Stony Brook of the top three or four candidates. I was chair of this committee for 2 years (1983–1985) and a member of the committee for 12 of the subsequent 18 years (between the fall semester of 1983 to the spring semester of 2003).

In considering committees outside of the sociology department, I note first the Standing Committee of the Art and Sciences Senate known as the Committee for the Academic Standing and Appeals or CASA. The members of this committee primarily consider the appeals of students who have not received an approval of the College of Arts and Sciences through the Executive Officer of CASA of their petition for a change in academic standing or a related request. In submitting a new petition to the members of CASA the students usually give more information and greater documentation than they had in their original petition. I became a member of this committee in 1990 and have continued to be a member for 16 years of the past 24 years (and will continue to be a member of this committee for the school years of 2014–2015 and 2015–2016). I have been continually impressed by how well the members of this committee work together in granting or rejecting appeals by balancing out the circumstances and motivations of the student with the academic policies of the university.

As a prelude to mentioning my next service activity, I must start on a personal note. I have a great love of classical music. As only one of many indicators, whenever I am working at my home office, I have classical music playing—either from my extensive collection of vinyl records and compact discs or from my tuner set on one of the classical-music radio stations in the area. So it was "natural" for me to become involved in the fine arts organization at Stony Brook. Since opening in 1978 as the Fine Arts Center at Stony Brook University and transformed in 1988 as the Staller Center for the Fine Arts, the center has presented an ever-expanding schedule of music, dance, theatre, and fine art exhibitions. I was a member of the advisory council for the center from the spring semester of 1987 to the spring semester of 1998. I could not have been more pleased to be on this council, comprised of both university and community participants, as it helped expand the fine arts for Stony Brook University and the surrounding community.

In 1987, I joined the Board of the Stony Brook Child Care Services, Inc., and am still a member some 27 years later. This child-care center is loosely affiliated with Stony Brook University with a semi-independent status. It currently has about 160 children registered, roughly 80 infant/toddlers and another 80 pre-schoolers. The board is made up of both university and community members (including parents who have children in the center). This very active, hard-working board meets at least once a month during the year, and has various subcommittees that meet regularly. The board handles a wide variety of matters, including formulating policy, creating and maintaining the budget of the center, overseeing the administrative staff, fund-raising, and many other activities. Although I have been heavily invested in this child-care center over the years, I will not try to detail all the exact ways in which I have been involved. What I will point out, however, is that I have been known to say publicly—and jokingly (?)—that my activities on behalf of the child-care center may well be my ticket to heaven.

I have a little more to say about my personal life. Even as June has unfailingly supported me in my career, she has maintained a career of her own. From the beginning of our marriage we have had a two-career marriage. From the first day of our marriage June worked full time except when our children were small (when

she cut back to 3 days a week). When we moved from Ann Arbor to Long Island, she worked as a psychiatric social work supervisor at one psychiatric center for 9 years and then at a different psychiatric center for another 3 years. After that she was an Associate Director of Social Work Services at Stony Brook University Hospital from 1980 to 2005, and then for a year before she retired she was the Acting Director of Social Work Services. She also was a clinical assistant professor in the Department of Psychiatry (1979–1988) and the School of Social Work (1980–2006) at Stony Brook. Further, from 1988 to the present she has had a part-time private psychotherapy and counseling practice.

Since we both were intent on supporting each other's career, some "juggling" of our activities was involved, but we managed pretty well. To "unpack" further our strategies in doing so would take a second, more personal memoir, most likely requiring an assist from June in the writing. What I do want to note, however, is that a particularly important factor in making our two-career marriage possible was the services of an exceptional nanny/housekeeper. "Our" Ann was with our family for 40 years (1972–2012), anywhere from 3 or 4 days a week when the kids were very young to one day a week after they left for college. Our children loved her when they were growing up and still do. June and I continue to maintain close contact with her (as a part of our "family") by phone and mutual visits. [Sadly, I must report that about two months after the initial submission of this manuscript our beloved Ann passed away at the age of 94].

Although June has always been good about giving me a certain amount of "space," she also has expected me to be a full partner in our marriage. If this expectation is taken to mean an absolute "fifty-fifty" in all responsibilities, then I cannot claim to have gotten to my "fifty." I will say, however, that I have tried hard to get and keep my percentage fairly high. We have had a variety of parental obligations, community obligations, religious commitments (as members of the North Shore Jewish Center), cultural interests, and leisure activities (including concert going, museum attendance, theatrical visits, etc.). Moreover, June has always liked to travel, and sometimes needed to "encourage me"—a tactful, euphemistic phrase—to do so since much of this travel came during a couple of weeks of my working summers. In addition to our visiting various parts of the United States and Canada, we have spent some time overseas—with visits to Israel (three times) and to parts of England, France, Turkey, Italy, Sweden, Finland, Russia, Egypt, and Czechoslovakia. Once on these trips, I very much enjoyed them.

Like all of us, I have had times when things in my professional and personal life did not go as I had hoped or planned, which I call "bumps in the road": goals and needs deferred or not met at all; disappointments and frustrations; unexpected occurrences; personal and professional frictions, to name a few. I have already referred to a few of the *professional* "bumps" and do not intend to mention any others. How important can it be for me to report that I was upset that a merit increase I expected to be good-sized was pretty small? I realize that a discussion of some of these "bumps"—especially less trivial instances—conceivably could be somewhat informative about my personality and character as well as the workings of academia. Yet, too many of these instances now seem to me in retrospect to fall

into the category of personal gripes, perceived inequities, and the like. In any case, many of them worked themselves out or became moot. I just cannot see any great payoff in introducing them here.

There is one category of "bumps" in my *personal* life, however, that I do want to mention explicitly, even though I do not think it is appropriate to go into specifics. Like all families, June, my son and daughter, and I have faced and coped with various medical problems—some minor and some not so minor. Two or three of them turned out to be life threatening. We managed to get through them alive whether—depending on ones' perspective—by luck, the miracle of modern-day medicine, or the will of God. One uncontested positive factor, however, was June's unparalleled commitment to the physical health of the family. By activating her professional connections, her networking skills, and her own well-honed problem-solving ethos, she navigated the family through these medical crises. I remain amazed by her character, perseverance, success, and just plain "smarts" in this realm.

Wrapping It Up

The main thrust of this memoir has been to review my research and scholarly work—not only to present its substance but also to relate just how it came about. I embroidered this core by describing some of my teaching and service activities as well as by giving bits of information about my family and personal life. In thus reviewing my past, I have come to realize (more than ever) that the good parts of both my professional and personal life—of which there have been many—have been very good indeed. Also, I gained a renewed awareness of how much I am indebted to people who helped me along the way.

As I thought about how to end this memoir, Erik Erikson's well-known theory of eight stages of psychosocial development (Erikson, 1963) popped into my mind, and I could not will it away. This is ironic in and of itself since I have mixed feelings about the Eriksonian scheme. In class I caution students about its claim of universality, its insistence of a set sequence of stages with particular age ranges, its over reliance on internal laws of development and its under reliance on social and cultural forces (among other caveats). Yet, the scheme can be useful for descriptive purposes.

In each of the eight stages posited by Erikson, the person confronts and hopefully masters new challenges. That is, each stage is characterized by a conflict or psychosocial crisis that must be resolved by the individual. Even though I am still active in a work setting, I suppose at my age I have entered stage eight, the one that is labeled "ego integrity vs. despair." In this stage, people look back on their lives and accomplishments. On the one hand, if they believe that they have led happy and productive lives, they develop feelings of contentment and integrity. If, on the other hand, they have many regrets and see their lives as having been wasted or their life goals as having not been met, they are left with feelings of bitterness and despair. I am thankful to have landed—professionally and personally—on the positive side of this stage.

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