

Wanderlust in Academia
by Neil J. Smelser

TABLE OF CONENTS

Prefatory Note

Chapter 1 Beginnings, 1930-1942

Birth and Infancy	2
Family Background	9
The Family Drama	12
Our Gang	19
Early Schooling	21
Early Work	24
Summary Reflections on My Youth	27

Chapter 2 Later Schooling and Pre-professional Education, 1942-1954

Late Grammar School and High School Years	29
Collegiate Years at Harvard	35
The Oxford Years	46

Chapter 3 Graduate Training at Harvard, 1954-58

1954-55: An Impossible, Awful, and Wonderful Year	53
Collaborating with Parsons	54
Slogging Through Graduate School	57
Formulating a Dissertation Topic	60
Life in the Society of Fellows	63
The Thesis, the Book, and Its Reception	65
Recruitment to Berkeley	70

Chapter 4 Early Years at Berkeley, 1958-64

Dramatic Career Beginnings	76
Work on Collective Behavior	79
Other Scholarly Enterprises	83
Working with My Brother	87
Editing the <u>American Sociological Review</u>	89
Stirrings of Citizenship	94
 Chapter 5 Rupture, Recovery, Broadening, 1964-73	
A Psychoanalytic Decade	97
The Free Speech Movement and Its Aftermath	101
Teaching Graduate Students in the Late 1960s and early 1970s	109
The Crumbling of the Sociology Department	112
The National Scene: A Move into Premature Statesmanship	115
Research and Publication in the Chaotic Decade	119
Coda	124
 Chapter 6 New Ventures at Home and Abroad, 1973-80	
A Splendid Year in Europe	126
A Strange Interlude	130
Two Tough Years as Chair	132
A Brush with Medical Education	139
Enter Erik Erikson and Psychoanalysis Again	140
An Intellectual Misadventure and Setting it Right	142
Abroad Again	144
A Return to “Normalcy”	147

Chapter 7 A Decade of Service, 1979-89

“Mr. Report”	151
The Academic Senate	163
Two “Service” Episodes Derived from My Senate Activities	165
A Re-link with Professional Medicine	167
A Policy Voice in the National Academy of Sciences	168
Troubles at Harvard and Yale	173
Lectures on Academic Cultures	175
The International Scene	176
The National Laboratories	179
The Stanford Center	181
The Center for Studies in Higher Education	182
Squeezing in Academic Scholarship	183

Chapter 8 Professional Confirmations, 1989-1996

Paralysis Finished, Paralysis Ended	185
Looking About at Age 60	186
The Office of the President	192
VERIP, the Prospect of Retirement, and the Center	194
Academic Work: Ambivalence Front and Center	197
Other Academic Work: Reflections on Sociology	199
Election to the National Academy and the Presidency of the ASA	202

Chapter 9 The Center Years, 1996-2001

Joining the Center	205
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Keeping up with the Social Science Establishment	214
Keeping a Finger in the Natural Sciences and Medicine	219
Some Miscellaneous Academic Writing	220
A <u>Festschrift</u> by Any Other Name . . .	222
Editing the <u>International Encyclopedia</u>	223
Chapter 10 Unemployed but Not Retired, 2001-	
A Thought on Retirement in General	233
First Plans for Retirement	234
The Social Sciences and Terrorism	235
Back to the Odyssey	240
Adventitious Teaching	242
A Return to General Education	243
John Reed and Usable Social Science	245
A Return to Higher Education (“Reflections”)	249
More on Higher Education: The Kerr Lectures	251
An Unanticipated Joy: My Oral History Project	254
References	256
Appendix A: <u>Curriculum Vitae</u>	269
Appendix B: Bibliography	279
Index	298

Prefatory Note

I have thought a long time about what word to use to describe this book. I came up with “autobiography,” “memoirs,” “reflections,” “career,” and more complicated terms such as “life search,” “personal history” and “intellectual journey.” I both accepted and rejected each of these terms, because what follows is simultaneously none of them in pure form and all of them in altered form. In one sense I do not like this lack of categorical neatness because it offends my lifelong commitment to intellectual order. In a more important sense, however, I want to stress that my life course—and others’ I daresay—inevitably has elements of indeterminacy, chance, contingency, change of direction, and some disorder. I think that the word “wanderlust” captures all of these facets of personal biography. The “wandering” has been not so much geographical (I stayed at the University of California, Berkeley, for most of my professional career.) Rather it has been through the thickets of the social-science disciplines, and in the organizational, institutional, and political environments of American university life.

I have organized the chapters like an intellectual biography, that is, in sequential age periods. For each period, however, I have recorded a different set of major themes, preoccupations, and lines of activity. Some of these themes weave in and out of the different periods. Throughout I have emphasized social and cultural aspects of my life, minimizing intimate personal experiences and problems except as they weave into this emphasis. I am hopeful that readers may learn about the following topics in reading these pages: (a) something of the history of societal preoccupations in the second half of twentieth century America; (b) the penetration of these preoccupations into the history of the social sciences; (c) the story of one scholar’s confrontation and coping with these histories; and (d) how personal biography, social involvements, and cultural history intersect.

Chapter 1

Beginnings, 1930-1942

Birth and Infancy

All children inherit a myth about their birth. They are not responsible for the myth. No one can remember his or her own birth, despite all speculations about birth trauma and other mystical constructions. A birth-myth is based both on solid facts and on memories and other mental creations of parents (mainly mothers), older siblings, and relatives. The child learns about the myth later, and thereafter takes more responsibility in elaborating its meaning and status in his or her mental life. Such myths become the basis for endless imaginary adventures, referrals, and distortions as life proceeds and preoccupations with life change.

As birth-myths go, mine seems on the rich side. The simple facts are that I was born in the middle of the night on July 22, 1930, in a bedroom of a farmhouse outside Kahoka, Missouri, a small town located near the Mississippi River where Missouri, Iowa, and Illinois touch one another. Subsequently I was told repeatedly how unusual I was as a newborn. I weighed ten-and-one-half pounds (my mother was small, weighing around 100 pounds); I had pronounced red hair; and, most unusual, I had two fully exposed lower teeth. So the raw materials for thinking myself a special being were there at the beginning. (When I was about seven years old I met the midwife who delivered me, and she described the event and said she would never forget that birth among her hundreds of other deliveries.) I was a large infant, and, I was told, a late walker; my dad was fond of saying that I spoke full sentences before I could take full steps.

At the same time, there were darker sides. For one thing, my two special teeth turned out to be more a problem than a wonder. I was told that they became severely inflamed when I tried to nurse, and that they had to be clipped out at the age of two weeks (payback for my early fame?), leaving a gap that was filled only when my permanent teeth came in at age six. In all events, I had a soup-dribbling early childhood.

More gravely, my life was nearly taken back six weeks after I was born. The circumstances were as follows. My family had moved to Phoenix, Arizona in 1929, where my father had luckily been appointed to a junior-college teaching position in the year the Great Depression struck. (I once made a backward-in-time calculation that if

you subtract nine months from July 22, 1930, there is a good probability that I was conceived on October 24 of 1929, Black Thursday on the stock market. Were my parents, in my fantastic elaboration, ruing that tragic day or celebrating the collapse of capitalism-gone-crazy? Or, more grandiose, was my conception an omen for the tragedy?) I never entertained the more reasonable explanation that my parents decided to have another child because they had recently gained the promise of longer-term economic stability.

Later in youth I was told that my parents drove back to Missouri in the early summer of 1930 so I could be born on my mother's parents' farm. (In fact, I was born in the same bed as my six-year-older brother, Bill, and indeed in the same bed as my mother—more raw material for my fantastic elaborations.) At the end of that summer, the little family of four made the return trip to Phoenix. While we were driving outside Kansas City, our car was struck head-on by another driven by an inebriated, uninsured driver. We had no seat belts in those days, obviously. In the crash my older brother Bill was thrown forward and cut his head on the broken windshield, yielding a life-long scar. I was in a baby-basket on my mother's lap in the front seat, and, it is told, I would also have been thrown forward in the car, perhaps with fatal results, if my mother had not been grasping the basket and me tightly as the collision was looming.

Later in my childhood another profound birth-and-death event was related to me. About three years after Bill was born (and three years before my birth), my mother conceived a child but lost it through miscarriage. The fetus was a girl. By the time I learned of the event it was past history and reported without the great sadness it surely occasioned. But for my fantasy life the reported death was very consequential. What if she had been born live and was in life as an older sister? More profoundly, would I now be that girl if she had lived? I would not say my young or subsequent years were dominated by these ruminations, but they cemented and complicated my sense of the fragile relations between birth and death.

The reason I regard this birth-myth as a rich one is that it provided so much material for so many fantasies. Was I really so special, and if so, was I a record-setting prodigy or a freak, or both? And if special, were the removal of my teeth and my early near-death to be regarded as warnings or retributions for my pride at being so special?

Such questions have no right or wrong answers—indeed, no answers at all—but it is true that they persist and become a kind of infantile kernel of fantasies to which subsequent events and situations became assimilated as life moves on: how special am I, and what perils await the special?

Many people also develop a mythical relationship about an event or situation that constitutes their first memory. Sometimes it is an actual memory; sometimes it is based on what parents or siblings recount; most often it is some mixture of both. I cannot produce such a memory, but I was told that as of age two I took a keen interest in children's jigsaw puzzles, worked them avidly, and was very good at doing so. I cannot verify this, but I do know that I continued this apparent passion into and through childhood. (I had a ritual of calling my mother to come and watch me put in the last piece as a moment of final conquest). Family lore also had it that when I was three years old I savagely attacked a young playmate who messed up one of my puzzles as I was working it. My passion for puzzles has continued to the present. I reached some kind of career climax a number of years ago by working a 9,000-piece puzzle of Breughel's Tower of Babel. The love is bringing pattern, order, and closure out of chaos. I am absolutely certain that this motif found expression and gratification later in my style of intellectual work as a social scientist.

Family Background

I never looked carefully into my family background—though Smelser genealogies exist, I know. What follows is what I learned from my experience and from direct lore from parents and relatives; some of it may be erroneous, but those errors are also part of meaningful psychological reality.

The background is mainly German. "Smelser" is an Americanized spelling for "Schmeltzer," ("ironmonger" in German). My father's parentage traces on his father's side to "Pennsylvania Dutch" of the early nineteenth century. My paternal grandmother, whose maiden name was Kendall, was always said to be "Scotch-Irish," with American roots in Kentucky. My father's parents owned a farm near Perry, Missouri, to the south and west of Kahoka. My maternal grandfather's maiden name was Hess and my maternal grandmother's maiden name was Mohr, both unmistakably German. They owned a farm near Kahoka, which was an ethnically dominant and German-speaking

community until World War I, when anti-German pressures forced the discontinuation of the German language in the schools and the local German Evangelical Church, a sect deriving from Lutheranism. Lore has it that the migrants to Kahoka were from the Protestant minority in Bavaria, and fled to the United States in 1848 to avoid persecution at the hands of Catholics. My maternal grandparents were devout members of the church (my mother's sister was its organist), and my paternal grandmother was a fundamentalist religious believer.

My parents did not deny this ethnic-religious background, but they made little of it in family life or to others outside the family. Each of them was the only child in the family to have gone to college. Both seemed to have left their family backgrounds behind, though neither broke from their families. We visited my relatives every other year in the 1930s and less often in World War II, and family relations with them were always affectionate and cordial, by my reading. I loved the car trips across the country, and the space and freedom I experienced on the Kahoka farm. We also visited my father's parents, who then had left their farm and were living in Farber, Missouri, west of St. Louis. I have to report the circumstance of their departure from farm life because it loomed so large in my father's and our lives. At a certain moment around 1930, a bank foreclosed the mortgage on my grandfather's farm because he could not make the payments, only a few of which remained. This threw him into poverty, from which he and his wife never rose. My father never forgot this, and from that moment forward he became a sworn enemy of capitalism for its injustices. He referred to this tragedy, bitterly and nearly in tears, scores of times in my childhood. He did not become a radical leftist, but he was among the most devout followers of the Roosevelt New Deal and the liberal wing of the Democratic Party thereafter. His politics and political passions later caused personal problems for me, to which I will come.

It now seems odd that my parents should have downgraded if not forsaken their German identifications after moving to Phoenix and that my young consciousness was so free of them, particularly because the fact of being of German extraction became so salient with the rise of Hitler and Nazism and the profound years of war with Germany. True, German-Americans were not so regarded as sinister as they were in World War I or as were Japanese-Americans in World War II. I do not think my parents hid their

German backgrounds; we even joked from time to time about “Cousin Rudolf” in Germany on account of my mother’s maiden name. As I say, I cannot account for this but I do know that my parents, especially my father, were so violently anti-Nazi and anti-Hitler that those feelings overshadowed any sentimentalism about being of German extraction.

The coming and course of World War II confirmed this pattern. I was old enough (11) to remember December 7, 1941, and the attack on Pearl Harbor. My brothers and I were watching a movie in downtown Phoenix when it occurred, and we emerged from the theater into a kind of milling crowd in which everyone was talking about the shocking event. I also remember listening intently to Roosevelt’s “date that will live in infamy” radio address the next day (it was broadcast into the classrooms of Garfield school). Our family, and I in particular, followed the course of the Pacific and European war theaters, and experienced the apprehensions and joys over the course of events, always from the perspectives of Americans involved in a colossal conflict with unequivocal enemies. I remember one telling event in 1942 when I was in seventh grade (and after American involvement in the war). A schoolmate commenced to taunting me on the playground of Emerson school, calling me “Nazi,” no doubt because he had been told or figured out that “Smelser” was a name with German origins. I remember my reaction to this day: I thought he was spewing out utter and harmless nonsense, so remote was I from the linking of Hitler, or Nazism, or Germans with any personal identifications in my life. I never thought of my family and myself as anything but unqualifiedly American. I realize that this meant that we were probably ignoring a great deal, but I have to report this as a psychological fact of my childhood.

Nor did the religiosity of my grandparents survive. In their early years in Phoenix, my parents (mainly through my mother’s initiative) joined the Grace Lutheran Church, the closest thing, I suppose, to German Evangelical. My mother sent me to Sunday school in my kindergarten and first-grade years, but two circumstances conspired to undermine this. First, I found Sunday school and its stories boring, and may well have communicated this in some way to my parents. Second, the church began hounding my mother to become involved in its voluntary activities, for which as a young mother she had no time, and this alienated her. The family drifted away from organized religion, and

I have never returned. In the meantime I learned of my father's growing anti-religiosity, which was mainly directed against what he regarded as the superstitions and the authoritarianism of the Roman Catholic Church. In high school I occasionally went to church services at the invitation of girl friends, but my heart was not in that, either. Though drifting from their religious past, my parents maintained a sentimentality about their Missouri roots, joining a "Missouri club" in Phoenix, a social group of migrants from that state who met periodically for potluck dinners that expressed their geographical origins and imagined solidarity.

Another aspect of the German-Protestant culture survived with full vigor in the family. That was the ethic of work, responsibility, and the principle of no reward without working for it. My brothers and I never had an allowance as such, except for a "nickel day" once a week to buy a coke or a candy bar. Everything had to be earned. When I was as young as seven, spending the summer in Columbia, Missouri, the family would resort to the Missouri countryside on weekends to pick wild blackberries and gooseberries, which my brothers and I sold door-to-door. During World War II, when my Dad and my older brother Bill were working part-time in summers in cantaloupe sheds outside Phoenix (labor was in short supply in those years), they would bring home "culls" (cantaloupes too ripe for shipping), which my younger brother Philip and I would sell door-to-door in the neighborhood for a nickel each, six for a quarter, keeping the proceeds. Later my "allowance" was always for some work or performance—drying dishes, mowing the lawn, or practicing the piano assiduously. Nothing earned, nothing given.

The Family Drama

In the early 1920s my parents met and fell in love in college. It was Kirksville Teachers College (later to become Truman University), located in a town of that name in northeastern Missouri. It was the nearest and most accessible college for both of them. It was also understandably parochial. (In the 1990s I had occasion to telephone a fellow sociologist at Truman University and I mentioned that my parents had fallen in love at Kirksville. He was interested in the story, but remarked that there was not much else to do, other than fall in love, at Kirksville). My father, with vague ambitions to be a journalist or a teacher, and with two years of military service (one in France) during

World War I, discovered a love for drama. Neither of his younger siblings went beyond high school. My mother, the youngest of four children, was a bright girl with a talent for music. She had actually taught at a little-red-schoolhouse near Kahoka at age sixteen. My parents married before their education was completed. My father went on to the University of Iowa and earned an M.A. in drama. Bill was born in the summer of 1924 (on Bastille Day, part of his birth myth). Until 1929 and the offer from Phoenix Junior College my father had no really steady employment, even working as a gas-station attendant for a time. I came on the family scene in 1930, and Philip was born in January 1934, when I was three-and-one-half.

I suppose the most notable fact about my family was its insulation as a family. Much of this was owed to its migration to Phoenix, 1,500 miles from all kin, who were visited at most for a couple of weeks every two years. The family did not need childcare because when the boys were young my mother did not work, except when she was periodically invited to substitute-teach in Phoenix's public schools. At those times she hired a local person to be there when we came home from school, and Bill the older son did some child minding, too. Moreover, the family could not afford, or did not want to afford, a cleaning-person. We were an ideal-type "isolated nuclear family" that sociologists came to characterize as a product of modernized industrial societies. More, and perhaps more decisive, the family culture was quietly exclusive of though not hostile to neighbors and friends. We invited both into the house, but we did not reveal intimacies about the family. Throughout my childhood my family was something of a ship in the desert, though nobody in the family seemed to deem this as anything other than normal.

No simple description can portray the tone of the family, because it, like all families, had its complexities. As I read (and reconstruct) its culture, it was on the serene side. My mother and father were generally openly affectionate with one another, and quarreled only very infrequently. I attribute this accommodative style mainly to the patience of my mother, who was a quiet woman who frequently diverted, de-fused, or fell into silence when conflict loomed or broke out. I trace this to her own childhood of subordination to a very emotional and domineering mother, with whom she did not identify. My father, a generally reasonable man, had a more volatile temper that would

find expression when we misbehaved or when he was opining about the state of the world at the dinner table. This frightened me sometimes, and I will write later how I came to terms with his volatility. At the same time he was very affectionate, supporting, and loving to his sons, and I loved him more than I could admit to myself until later in life. He was, finally, a great humorist, raconteur, and teller of exaggerated stories and tales, keeping his sons in stitches at the dinner table with stories of his farm boyhood, imaginative tales about imaginary creatures (the fantastic adventures of a green rabbit, which I took over as an entertainment form for my children and grandchildren) and accounts of the humor of Shakespeare and Mark Twain. He supported his sons, and liked to teach them. In those years when he directed plays at his junior college, he recruited me to help him construct sets for the stage, teaching me some carpentry in the meantime. He continued this education in high school when we did construction on our new property. I have the fondest feelings for him for taking this kind of interest in me.

The family was highly cultured and academic as well. My parents moved far from their origins, and their journey was through education and cultural advance. They evidently took it for granted—but did not frequently say—that their boys would do well in school and go on to college. I find it remarkable that this family culture could emerge in the union of two who came from modest farm families in the American Midwest. Yet it did. My father was a true, inquiring intellectual, and rewarded any expression of these interests in his sons. My mother's intellectual development was, if anything, more remarkable. A young and perhaps overshadowed youngest child, she developed musical talent in song and in the piano, and interested herself in wide ranges of literature, language, and history. All three of her sons took music lessons. I remember frequently being handed a book or a play when bored. Bill also taught me a lot about books and ideas. Subsequently, when I was in high school and college, my father and I had many rewarding intellectual conversations on philosophy and literature. A remarkable fact was that I did not experience this exposure as forced upon me. Furthermore, I never regarded my parents' involvement in high culture as pretentious or precious. If I were to attribute my own involvement in the world of ideas, culture, and intellectual cultivation to any single most important factor, it would be to my parents' commitments to that world.

One notable element of my parents' working family philosophy was their frequently expressed sentiment that family life was about "the boys," each of whom was good and deserving. All were called "good boys" and special in their respective ways, but none superior to the others. Favoritism seemed a sacred taboo. This proved a good working philosophy for them, and indeed fostered fair treatment of us, but I am certain that each boy harbored secret fantasies of wanting to be the favored child (I know I did), and regarded being treated equally as a form of personal deprivation. We were occasionally disciplined physically (a spanking), mainly for fighting and mainly between Philip and me, and for other occasional misdemeanors. I recall no violence between my father and mother.

One telling bit of evidence of family solidarity was found on the playgrounds and vacant lots of our neighborhood. All three of us sons were outstanding marble shooters. We played endlessly with neighborhood kids. We played "ring," a game in which each player places the same number of marbles in the center of a circle drawn on the ground. The object of the game is to take turns shooting a master marble (a "taw") and to hit marbles in the center of the pot, keeping those that one could knock out of the ring. Bill, Philip, and I would always win. Furthermore, we played for "keeps" and kept the marbles for ourselves, sometimes selling them back to those who had lost them. The telling aspect of this game was that the Smelser brothers never played against one another, but always in tandem, like three young Musketeers, often conspiring and cooperating with one another to beat the other boys. When we brothers played among ourselves, we never played for keeps. Once a year an older boy (we called him "Biggie") would appear from another neighborhood, bringing 100 marbles. He would challenge Philip and me to a game, and every year we would clean him out. On another occasion (perhaps in third grade) I played in a Garfield school marble contest as a preliminary to a city competition held later in a public park. I won at Garfield. My dad drove me to Verde Park for the city finals, and I was wiped out by an older a boy in the first round. I was despondent at the loss, but my dad took me forthwith to a soda fountain in a local drug store and treated me to an ice-cream sundae to celebrate my successes. I could not believe that my father could be so kind to such a worthless competitor.

Family vacations were also occasions for solidarity. With a modest income, we could not afford lavish travel or other forms of entertainment. But in every other summer in the 1930s and once at the end of World War II, we piled into the family car and drove the 1,500 miles, mainly on U.S. Highway 66, to the Midwest. We would spend a week or so at the beginning and ends of the summers at our grandparents' places. Between those visits we went to some university campus—Missouri, Wisconsin, Iowa, Minnesota—where my parents enrolled for “continuing education” in summer school. My dad used this medium to shift from teaching drama and speech to teaching philosophy—an unheard-of career-change nowadays—and my mother extended her studies in the humanities. We boys were on our own much of the time in these places, and invented diversions. We went to parks and played, or ventured down to the railroad tracks and horsed around in the empty train cars. We would traipse around the town to grocery stores and bars, begging used and waste bottle caps from the proprietors, who usually obliged. Then we would repair to an empty lot and play “baseball,” with one “pitcher” sailing bottle caps to a brother, who swatted at them with a broomstick. On one occasion in the 1990s Bill, Philip, and I took our families for a sentimental visit to Kahoka and the old farmhouse, by then in shambles. At one moment, when we were picnicking in the town square, we brought out a broomstick and some bottle caps and played a couple of sentimental innings. This caused great merriment among onlookers and some suspicions on the part of two policemen stationed at the edge of the park.

On the trips themselves we were often bored but inventive. Our favorite game was with slingshots we made out of wire clothes hangers and rubber bands, with shoe-leather for the pouch. The game was to shoot pebbles (usually collected in tourist cabins' driveways the night before) at road signs and billboards, trying to set records for consecutive hits. An occasional cur that had the misfortune of straying near the road also drew our fire. Interestingly, as in the competitive marble games, we did not compete with one another for hits. All three fired out of the car windows at the same time, and if one made a hit, it was a score for the little team. Solidarity again. I came to cherish every aspect of these trips, as well as briefer ones to the Grand Canyon and the Indian Country, as mementos of the best of our family and fraternal life. The Kahoka farm became a special shrine; I returned to it with my own family on several subsequent occasions.

Finally, the family was united around sports. My father, a northern Missourian, was a rabid fan of the St. Louis Cardinals and that loyalty spread to his sons. We followed their fortunes closely, even making detours on our summer vacations to take in a Sunday double-header at Sportsman's Park in St. Louis. (I continued to go to games when at Harvard, every time the Cardinals came to play the Boston Braves.). As a family we also attended basketball games at Phoenix College and professional women's softball games with the Phoenix Ramblers. The most important family athletic activity was golfing. My Dad was an average but dedicated golfer, and he involved his sons as each matured. I loved playing with him. The only negative note was my father's tendency to lose his temper after a bad shot or a poor score on a hole, often banging his club on the ground or into his golf bag. I didn't like this loss of control (much as I didn't like his political outbursts against businessmen, lawyers, and doctors at the dinner table). Though I didn't tell myself so consciously, I think I also applauded myself for being more levelheaded and controlled than he. Golf then became an independent passion for each of the boys, and during our youthful years we played Encanto public golf course in Phoenix as much as we could. Our interests continued into adulthood, when Bill and I played regularly with my son, Eric, and Philip, Bill and I played during family visits. All this worked to isolate my mother from the males in the family, and to some degree it did. She contended with this by joining the enemy, as it were, developing into an avid sports fan, a dedicated baseball statistician, and a partisan viewer of baseball, basketball, and football games when television came on the scene. She even took up golfing with my father in their later years.

On another sporting front my Dad and I did not fare so well. In his youth on the farm he had developed into an excellent hunter of birds, squirrels and other prey. He continued his interest in hunting deer, doves, and quail after moving to Phoenix. He involved his sons in some of his hunting, supplying a gun to each. I never warmed up. I was not very talented, partly because I had a "left eye lead," which meant that I had to lean my head over the butt of the gun to take proper aim, which in fact ruined my aim. Also, I didn't really care for the killing. This was not a principled expression of a pro-animal sentiment on my part, but mainly an unexamined indifference. My dad, I

presume, recognized my lack of ability and sensed my lack of interest. He didn't press things, and our ill-developed hunting culture gradually faded from the scene.

Bill was most special in my life. As I regard him in retrospect, he seemed to be very much like my mother in his saintly ways. Our age difference was significant. I often describe him as more like an uncle than a brother. He was caring of me, and remarkably supportive. We went to the same school (Emerson) when I was in first grade and he in seventh; during the lunch hour he came round and we ate our homemade lunches together. Most brothers would not have deserted their peers for that. He encouraged all my interests in sports, and praised me often. When I was in first grade or so we both played on a WPA neighborhood six-man football team, in a little league that competed in our schoolyard at Garfield School. Bill was fullback and captain, a superb runner and passer. I played center, the lowest status position on the team, destined to center the ball then block opposing players. But Bill insisted I be a receiver as well (passing to the center was legal but rare on a six-man team), and in that way he included me fully. He took an interest in my studies later on, and worked with me on math, which he had studied in the military. He included me in his life and fostered good feelings about myself. We almost never fought with one another. As for me, I became his willing subaltern and accepted his protection in the neighborhood.

Yet my relationship with Bill had its darker side, which I never acknowledged until later in my psychoanalytic explorations. I came to conclude that Bill was more like my mother with respect to self-denial, integrity of character, and altruism. I concluded further that I was lacking in these virtues, and was of lesser moral cloth—not really a bad boy but not as good as Bill. Because of this I concluded, finally, that he was probably my mother's favorite. Yet I could not really complain because both were so good. Bill seemed to have claimed the chair of virtue in the family.

Philip's arrival on the scene complicated matters greatly. As I said, I was three-and-one half at the time, not ready to handle this event very maturely. My mother bore Phil in a Phoenix hospital and brought him home after some days. My parents also hired a temporary home-visiting nurse to help out with the baby. One bit of family lore emerged from this scene. One day, I was told, I went up to my mother and said to her that I was convinced (and apprehensive) that the nurse was going to steal Philip and take

him away. My flimsy cover-up was immediately recognized by all, and much to my chagrin, became a bit of family humor that was repeated dozens of times during my childhood. But Philip remained, first taking the chair of adored baby and later the role of family jokester and entertainer, which I believe is a frequent path for the youngest child. I became very attached to Philip, but it was more fraught relationship than that with Bill, and had more teasing and fighting. The fact that Philip suffered two serious medical bouts, one in primary school and one in high school, only complicated the ambivalence. I should report, finally, that Bill, Philip and I continued into adulthood as best friends.

I suppose I am telling a story that is not unusual for a middle child. I was never the only child in the family, I sometimes told myself, the way Bill was before I was born and the way Philip was after Bill and I left home. In that context I found myself seeking some kind of identifying chair. Bill (and my mother) seemed to have a monopoly on the chair of saintliness and Philip had the chair of family entertainer. I remember, when I was about five years old, I experienced a cruel, painful reminder in an idle remark by a thoughtless woman neighbor who thought she was being funny (but I did not). She said to me, "Neil, Bill looks like your mother and Philip looks like your father. You must have come out of a garbage can!" What was my place, my chair? I resolved this question dramatically in adolescence, in ways I will reveal later in the next chapter.

Our Gang

I remember almost nothing about playmates and peers in my pre-school and early school years. My first memories were of our house at 1025 East Moreland Street, an adequate but modest dwelling we called the "25-dollar house" because that was the monthly rent. My kindergarten year was at Garfield elementary school several blocks away. When I was six my family moved to the north side of town for one year; I spent first grade at Emerson school. A decisive move came in 1937 when my parents apparently decided that they could afford their own home, and bought a newly constructed house at 1410 East Moreland (just outside the Phoenix city limits). This was a developing neighborhood, and the entire block across the street from us was vacant during our first years there. We lived in that house for ten years, nearly to the end of my high-school years.

I report here the move out of that Moreland house a little out of time sequence. My father had retained from his youth a kind of standing romance with farm life, which was revealed to the family in his entertaining story-telling and regaling. In mid-year of 1947 this sentimentality was radically re-activated when he heard of some acreage for sale in West Phoenix, complete with house and pasture. With the house came a cow (Molly) a calf (which we subsequently named Hubert), several cats, perhaps a dozen turkeys, and several dozen hens and chicks in poultry shelters. In a fit of sentimentality, I believe, he persuaded my mother to buy the house and move there, thereby realizing in a meaningful way his long-standing rural dream. I remember the moment well, because one afternoon, after school, when I was playing ping-pong with a schoolmate in a city park, my father tracked me down and informed me that we were moving and that I had to come immediately to the new place to begin to learn my new chores.

At first I did not welcome that move. It came in the middle of my junior year of high school, at which time I was up to my neck in studies and many extra-curricular activities, and beginning to think about if not prepare for college. It also involved a much longer bicycle commute to school. But my dad put the finger on me—now the oldest son since Bill was long gone from the household scene—and designated me as the official milker of Molly, the family slaughterer of chickens, and salesman (to local grocery stores) of eggs supplied by the chickens. These chores took a lot of time, but as I recall I took them on with some pride, as a fully responsible member of a “working farm family.” Though I lived this “rural” life for only one-and-one-half years before leaving home for college, it stands out as a very salient chapter in my youth, and the seat of very positive memories and sentiments.

To return to our home built in 1937. It was located in a neighborhood of families, largely middle-class, I suppose. It had two peculiar characteristics. First, most of the children living nearby were boys, who associated more or less exclusively with one another (and in the meantime excluded the few girls in the neighborhood). Second, a significant number of the boys were sons of policemen, in families probably more authoritarian in culture than ours. In all events, the neighborhood developed what I call a “gang,” though without the elements of fierce group loyalty, violence, and defiance of the law. We played games such as football, last tag, Red Rover, and kick-the-can. As a great

project we dug a complex cave in one of the vacant lots across the street from my house. In summers the gang would gather at night in the cave and have a potato roast, sitting around the fire, and breaking up when parents summoned the young members home. Bill, the oldest kid in the neighborhood, was the informal but acknowledged leader. I was, expectably, a favored lieutenant. We occasionally engaged in mischief, for example, smoking castor-bean stalks or crawling around in and sometimes damaging houses in the process of construction. On one occasion another member and I engaged in a clod-throwing contest, heaving them through open window-frames in a house under construction and dirtying the inside walls. We were caught red-handed by the housing contractor, who promptly turned me over to my father. Dad was furious, I am certain, but my only retribution was to go to the house, under his supervision, and scrub clean the soiled plaster. I was terrified and mortified at the time, but in retrospect my father's punishment seemed mild and fair.

The gang experience gave us a great deal of freedom. My parents generally knew where we were (usually in the cave-home in the nearby lot), and they never came around, expecting only that we would come home in time for bed. I don't think they worried about me, because they assumed that Bill would protect me and keep me out of trouble. I don't know what exact psychological significance to contribute to this gang life, but I associate my years with it as an experience of happy youth, a la Tom Sawyer. My brothers and I reminisced about the gang for decades after, and joked about how the house subsequently constructed on the site had collapsed into our cavern.

Early Schooling

It was taken as an unspoken given that the three sons would go through the Phoenix public schools. There were five over-determining reasons for this. First, nothing other than public schools had been available for my parents. Second, my father's teaching position and my mother's substitute teaching were in the public schools. Third, though my family was academically oriented and my parents had experienced "social mobility" by moving from farm to urban service occupations, the concept of social mobility, especially improving one's certification through private schooling, was not under their radar. Fourth, Phoenix was a modest town of 30,000 in the 1930s and had very little by way of private schooling of any description. Fifth, and rendering all of the

above moot, my family's income, though steady, was too modest for anything other than free schooling.

That issue settled, I commenced kindergarten in the early fall of 1935 in the nearest school, Garfield, located about eight city blocks from our 25 dollar house. Though I do not remember this, I was probably walked there by Bill, who was in its sixth grade (Garfield only went to sixth; other elementary schools went through eighth grade). I also do not remember much about the experience, though what I can bring no memories of work, but mainly singing and games. I do not recall being disciplined for any unruly behavior or having any warning notes sent home to my parents. I was apparently a well-enough behaved little scholar. I liked Miss White, the young teacher. I learned a year or so later that she died shortly after my school year with her. I do not remember any emotional reaction to this news on my part, but I do remember thinking that it should not have happened.

The transition from kindergarten to first grade was accompanied by moving to a new rental house about one mile north, and over the boundary into the Emerson school district on north 7th street. I do not know why we moved. I do not remember much about that year, either, beyond my lunchtimes with Bill, though I do remember being assigned to sit in a special place in the classroom—not a dunce-cap assignment but its opposite, for some kind of good academic performance. We read the Dick and Jane book, and I remember a certain effortlessness in getting it, even wondering what the fuss about reading was all about. Later, in fourth grade when I encountered the challenges of long division, I remember a little bit of initial difficulty, but one day everything fell into place, and long division suddenly became effortless.

The move into second grade also brought a residential change to the newly built house on 14th street and Moreland, and back to Garfield school, now in easy walking distance, about four blocks away. A dim memory places me initially in a class of mixed second graders, probably because I was a transfer from Emerson. With a matter of days I was summarily reassigned to a “regular” second grade class without explanation. I later learned that my initial class was a special one, in the sense that it was made up of both “regular” second graders and others who had had trouble with second grade the year before. Did I “perform” my way out of it into the regular grade? Did my parents

intervene and ask that I be transferred? I will never know, but I do remember that things were thought to have been set right by the shift.

Schoolwork generally came easy to me, and I was generally not a problem student in other ways. I did not, however, consciously experience feelings that I was a star student fighting to stay on top. By sixth grade I was recognized as such a star student, evidenced by the fact that I was made Captain of the Garfield Junior Police, that squad of uniformed kids who regulate traffic for street-crossing children before and after school. I learned that that high office was given to the boy with the best academic performance.

Lest I convey too smooth and rosy account of my school days, I should record a few items of delinquency and defiance in the primary grades. I have a most indistinct memory of altering a bad mark on my report card, perhaps in fourth grade. It was an amateur crime, easily discovered by my parents, and for it I received a humiliating paddling from my father. On another occasion, perhaps in the following year, I brought home a "C" mark in geography on one report card, and my father took me aside and told me soberly that I could do better. These two incidents give the lie to my general feeling that I did not experience conscious pressure for high academic performance from my parents, but it is also the case that this pressure was not salient in an everyday way, and that I was not in general preoccupied or anxious about it.

As for delinquency itself, I had several scrapes in later primary school. One day, in manual training class (fifth grade) the teacher had left the room temporarily, and the boys began to shout and raise mayhem in his absence. He returned and caught us and, in anger, yelled, "What do you kids think this is, a zoo for monkeys?" To this query I impulsively blurted out "Yes!" and earned some lashes on the back of my legs with the edge of a yardstick. (The taboo on physical punishment by teachers had not yet made its appearance in 1940.) In the same year my main teacher, Mrs. Strathey, whom I really liked, sent home a note informing my parents that I was showing off too much in class. I was chagrined and shaped up immediately. Finally, in sixth grade, when the teacher was again temporarily out of the room, the class broke into warfare, throwing paper wads at one another. It was my misfortune to be winding up for a heave when she came in the door and caught me in the act. That crime earned me a demotion from Captain to Lieutenant (number 2 leader) of the junior police squad, a minor sanction in retrospect,

but it humiliated me. Over time I came to regard these behavioral flaws with some pride, as evidence of Oa defiant streak in an otherwise well-behaved boy.

Sixth grade ended my days at Garfield, and it was time for me to transfer to another school for the last two years before beginning high school proper. (Phoenix had no junior high schools, elementary school going to eighth grade and high school from ninth through twelfth grades.) We lived in the Whittier school district, and that was my expected destination. I raised an objection to this, saying I wanted Emerson because Bill had gone there, and I wanted his teachers. (There was some mischief in this request, which I will relate in the next chapter.) In all events, my parents went to bat for me and requested that I be permitted to enroll at Emerson. This request was granted without opposition, I believe, in part because we lived so close to the Emerson district boundary. Entering Emerson also triggered a revolution in my academic outlook; I will begin Chapter 2 with an account of that great change.

Early Work

I have already revealed my parents' attitudes about work and rewards for work, as well as my minor and ill-paid labors in selling produce in the neighborhood and in tending neighbors' lawns for small wages. This picture also changed radically in seventh and eighth grades.

In his youth Bill had worked as package-boy at one of the A.J. Bayless stores (a small chain of supermarkets, such as they were, in Phoenix). His pay was seventeen-and-one-half cents per hour. His job was to pack groceries properly into paper bags or boxes and carry them to customers' cars. I seem to remember also that he was instructed by the store manager to run, not walk, back from the cars to the register station. Despite the menial nature of his job, I envied him. So, when by some circumstance I learned of an opening at another Bayless store (Lucky Market, different from Bill's and located some blocks north of Emerson school), I went after it. They hired me, but said I had to be fourteen years old to take it (in keeping with Arizona's child labor laws), and had to present evidence of a physical examination from a local physician. I lied about my age, thus breaking the law. They hired me, also breaking the law. I dawdled in getting the physical exam until after my fourteenth birthday, but nobody seemed to mind these irregularities.

So, at some moment in seventh grade I began after-school work, from 4 p.m. to 7 p.m. Monday through Thursday, bicycling from school to work and then from work to home for dinner. My wages were 35 cents per hour, doubled from Bill's depression rate. A couple of years later the Bayless employees were unionized by the retail clerks union; I joined the union at that time and my wages rocketed to \$1.45 per hour. My duties were much the same as Bill's had been, packaging and carrying groceries.

In this job I regarded myself simply as an employee, not special in any way. Yet evidence suggested that I performed well enough and merited trust. After some time as package boy, the manager of the store "promoted" me to vegetable clerk, which meant checking the quality of vegetables and fruit, preparing them for the store's shelves, and stocking the shelves. While doing those things I still packaged at the cash register in my spare time. I learned some valuable skills, and to this day I have been the trusted family authority in judging the ripeness and soundness of fruits and vegetables.

More decisive evidence of my trustworthiness came later. My early work years at the store coincided with the World War II years. Many items (meat, sugar, canned goods) were rationed, and many others (toilet paper, cigarettes) were often in short supply and subject to customer "rushes" to stock up on them. Cigarettes posed a special problem for management because regular customers complained loudly when they were unavailable. So management devised a distribution system and made me its agent. I would wrap up cigarettes in two-pack bundles and keep them in the back room away from the store's shelves. When a cashier recognized a good customer, he or she would call out to me to bring a bar of "Scat," a somewhat obscure wartime brand of soap. In response I would bring cigarettes from the back room and place them in their grocery carts. The cashiers would recognize the packets and charge the customers accordingly. On several occasions greedy customers offered to bribe me (50 cents, as I recall) if I would supply them with more than two packs. I don't know exactly what motivated me (my family's moral values? fear of getting caught?), but on these occasions I simply declined the bribes without reflection and not informing the management about them. I did not treat this cigarette-agent assignment by my superiors as an act of trust in me at the time (just part of my job, I thought), and I did not congratulate myself for behaving ethically, but I suppose I should have done so on both counts.

Another remarkable feature of my work at the store was that it seemed to open my style of interacting with other people—fellow workers and customers. I don't know how or why this happened, but I came to be more extroverted—talking eagerly, making clever remarks, joking, even some teasing—and other people seemed to like it. Prior to this time and prior to this experience I believe I regarded myself as a somewhat unobtrusive child if not a loner, and I experienced the new interpersonal style at the store very positively, as a kind of “new me.” This change obviously added to my enjoyment of the work environment and my self-confidence.

I must relate another dramatic incident that occurred about the same time, in late seventh grade. I had a teacher who was relatively new in the school during that year, and I recall that I had neither a strong like nor a dislike of her and that she didn't care about me very much one way or the other. I cannot even remember her name. However, one incident was so vivid that I have recalled it frequently throughout my life. One day she called me aside outside the class—after a recess, I think—and in a brief conversation she told me that I should become a diplomat in later life. This was a shock. First, it did not resonate with anything in my young self-image, and, second, I thought it strange and somewhat wonderful that she should care enough about me to say anything so personal and intimate as this. As I say, I have pondered this enigmatic moment many times in my life. I now speculate, as I have not done before, that perhaps she was sensing or picking up that dramatic change in interpersonal style, conversational wit, even charm that I witnessed in my work and workplace but not my school setting. Perhaps that change spilled over to my behavior at school. I also know that many have commented in my life on my peace-making style, both in academic exposition and in leadership roles.

I saved most of my modest wages, and liked the independence that they symbolized. I even expanded my work. Our local milkman asked me if I would be his helper with deliveries on Sunday mornings. One dollar per Sunday would be my reward. I took this on for a time, and enjoyed especially the end of the round late Sunday mornings when we would be treated to a large lunch at our last stop, a local Mexican restaurant.

Sometime late in eighth grade, my boss at Lucky Market asked if I would like to expand my hours, adding 4-7 p.m. on Fridays and a twelve-hour day, 8 a.m.-8 p.m. on

Saturdays, thus doubling both my workload and wages. (Another minor vote of confidence in me as a worker, I suppose, or perhaps some additional low-cost labor for him.) I took the proposal home and broached it to my parents. In a moving moment for me, my father took me aside and said, supportively and sympathetically, how much he liked it that I was working part time, but to add all these hours, “Well, that’s hitting the ball too hard,” and basically forbade it. I was ambivalent, but really knew he was right, and appreciate his wisdom to this day. As it was, I did shift from the weekday afternoons to the weekend shift some time later (again breaking the law, because children under sixteen were forbidden to work twelve hours a day, even one day a week). I worked in the Bayless grocery stores on that basis through high school. Toward the end of high school my part-time work shifted to journalism, and I will assess that adventure in the next chapter.

Summary Reflections on My Youth

One of the editorial commentators on my oral history at the Bancroft Library of the University of California referred to my childhood years in Phoenix as “sunny and idyllic.” They were certainly the first (in Arizona), but assuredly not the second (no childhood is fully idyllic), though I would regard them as generally positive and favorable. Like all families, mine had both its consistencies and its tensions. Its quiet academic culture constituted both a benign setting for development but also a scene of potential competition and conflict. The family had these latter elements for sure, but it was also remarkably inventive in fashioning a style of love and commitment that diverted conflict into cooperation and expressions of solidarity that were rituals but very meaningful ones. Every family member had both affectionate but anomalous relations to one another. Certainly I did not experience mine as a smothering childhood in any way. One day when I was in third or fourth grade, my teacher asked the students to say what they liked most about their family. Spontaneously I reported to her that what I liked most was that my parents gave me independence, letting me do what I wanted to do. It was an over-simple response, but I believed it and when I reported it to my parents they were evidently moved. My relations with my brothers were each fraught in their own ways, but were mainly harmonious. Above all, in that family setting I had a problem in finding a special, unique “chair,” independent of my brothers. In adolescence I found that chair,

an academic one, in which I sat in for life. The next chapter documents how I came to embrace that style.

Chapter 2

Later Schooling and Pre-professional Education, 1942-1954

Late Grammar School and High School Years

I experienced three simultaneous and decisive sequences of events in 1942. First, World War II began its course. The immediate family impact was on Bill, who turned eighteen, finished high school, and became eligible for the wartime draft. He decided to enlist rather than being drafted, and after basic training he was assigned to gain educational training to prepare him for a role as a U.S. Army meteorologist. He left home for basic training at Fort Ord, California, during which he contracted spinal meningitis and was saved, miraculously, by the newly developed sulfa drugs. Later he was trained in special courses at the University of Oregon and Harvard, after which he was assigned to a military weather bureau in Iran. Among other things he wrote me while at Harvard, describing its setting, buildings, and atmosphere positively.

The impact of Bill's departure on my life was immediate and dramatic, even though much of my adaptation to it was not conscious. I was now the oldest boy in the family home (at last). I reconstruct three responses to this decisive event. Within a matter of weeks after Bill left, I informed my mother that I wanted to switch from piano to violin lessons. She agreed without objection, happy that I was still with music. For years I had been a decent but semi-motivated piano student, but my desire to switch to "Bill's instrument" was unequivocal and strong. I did not consciously read it as taking over Bill's role, but surely it was a move in that direction. I had already initiated my campaign to go to Emerson, not Whittier school, which was "Bill's school" with "Bill's teachers" to impress. In the meantime I expressed great pride in Bill, buying a patriotic button that read "Brother in the Army," and demanding that he come back to Emerson with me on one of his furloughs to show himself off in uniform.

The second change was in my academic commitment. As indicated, I was a strong but not especially strongly motivated young scholar in my early years. But now it became a matter of conscious motivation and determination. I vowed to do well, hopefully exceptionally, as a student. This was not completely articulated but my behavior revealed it. I continued as a straight A student, but now I meant it. The teachers recognized my performance, and from time to time I was drawn aside and

complimented. At the same time, I did not broadcast my academic intentions or brag about them to friends and fellow-pupils. At the end of those two years at Emerson I was rewarded by being chosen as commencement speaker at the eighth-grade graduation ceremonies.

I brought my top grades and teachers' compliments home from school and thus informed my parents of my accomplishments and ambitions. The most vivid moment was when at school I came across a description of the Rhodes Scholarships at Oxford in some encyclopedia or other publication. I recognized it immediately as something special, and I promptly went home and announced to my mother that after I graduated from college I was going to get one of those scholarships. I cannot remember her exact expression on hearing this announcement, but it seemed to be a mixture of pride and disbelief, with a dash of amusement, but also supportive. At the same time, in these two final years of elementary school I also maintained a "normal" behavioral style in school, participating in all sports and engaging in early adolescent dating. I avoided, unconsciously, a style that would stamp me as a "brainy" young scholar among my peers. Furthermore, I underwent this little academic revolution in a spirit of conformity with, not rebellion against, my family culture. I did not have a stormy or oppositional adolescence. By and large my parents and teachers were proud of me and I was not rejected by my peers.

The third change was in my interpersonal style (described in Chapter 1), manifested at work but not consciously or openly at school. It was also a dramatic opening, though I did not experience or describe it as such at the time. Taken together, my not-too-subtle challenge to Bill's childhood reign, my responsibility at work, my new passion for studies, and my opening personality melded into an adolescent blossoming, only partially observed and acknowledged by me, and certainly acceptable to all of my relevant audiences. I read this pattern as an adaptive one at this age. In my navigation into adolescence I was assertively taking over Bill's role and in some sense replacing him, but simultaneously expressing pride and admiration for him in his service to the country in the war. I was moving to occupy the family's academic chair, an entirely praiseworthy move in my parents' eyes, given their strong academic commitments. At the same time I did not try to pull rank on and alienate my fellow pupils.

I continued the same pattern as I moved into high school in the fall of 1944. Even before I enrolled as a freshman at Phoenix Union High School, I took a summer school typing course there, with the conscious intent of acquiring a skill that would serve me well in my subsequent education and thereafter. The class was held from 6 a.m. to 9 a.m. to avoid Phoenix's unbearable summer heat. I bicycled to and from class, which was about a mile from home. I was the only boy in a class of girls bound for secretarial jobs. I threw myself into the class, being the first to reach 80 words per minute, which astonished the teacher.

I should say a few words about Phoenix Union. It was one of only two general high schools in Phoenix at the time. The other was North Phoenix, located in an affluent region of Phoenix not far from the Country Club. The class composition of the schools reflected their geographical location. North High was regarded, antagonistically by many, as the rich kids' school. Phoenix Union was a giant high school of 5,000 pupils, very rainbow in composition. Notably, it catered to many children from low-income families and to the substantial (about 15%) minority of Mexican-American migrants in the city's population at the time. (The black minority still went to segregated schools, integrating with others in 1952, several years after I graduated). The dropout rate was high; only slightly more than 700 were in my senior graduating class. At the same time the high school had a cadre of academically serious and talented faculty. My parents, who were teachers in the same system, and Bill, who had gone to Phoenix Union as well, knew about these teachers and guided me toward them.

From the freshman year onward, I took a path that reflected my parents' interests in the humanities and arts. As a foreign language (two years were required), I took Latin. I excelled in English, another of my mother's subjects. She had resumed teaching at this time, as her boys were growing up. Even as early in my freshman year, with encouragement from my father, I competed in the school's public speaking contest, delivering a speech identifying and condemning racial discrimination, with examples from the Phoenix scene. Public speaking, of course, was one of my father's subjects. He helped me prepare my speech. I was consciously ambivalent about this, appreciating his help on the one hand but wanting to be my own person on the other. My speech won at the freshman level, and I advanced to the school-wide competition. I came in third or

fourth at that level, but, interestingly, the votes of the judges (mainly prominent Phoenix citizens) were split, half at the very top, and half at the very bottom. I take it that this bifurcation resulted from the controversial nature of my speech. In fact, one of the judges, a local businessman, came up to me after the finals and chewed me out, telling me that I had no business delivering such a speech on such a topic. I was somewhat shaken by this attack and did not fight back, but inwardly held my ground. Throughout high school I entered all the speech contests, delivering addresses on serious and controversial topics (including an environmentalist one on preserving the national parks), never winning first prize.

Academically my performance in high school was at the top, gaining all A's without special effort from the beginning. Enjoying good health, I also had a perfect attendance record. I excelled not only in those classes I liked (English, speech, math, Latin, history), but also in those I found boring or taught by indifferent teachers (commercial law, physical geography). I also spread out into many extracurricular pursuits. Speech was one, drama another (my father's other interest). I joined the chess club my freshman year, but that didn't last. The school had an annual tradition of producing two farcical musical and dance events, Les Follies (all-girls) and Mor Follies (all-boys). In my sophomore year I stage-managed the former, and danced (as Rachael in a Ruben-and-Rachael skit) in the latter. I joined the high school orchestra as a violinist and later a violist. In my junior year I went out for the debate team, made it, competed against North High and went on one memorable trip to Southern California to debate teams in Compton and Pomona, with an exciting weekend in Los Angeles. In my junior year a physical-education teacher encouraged me to try out for the quarter-mile race on the track team and applauded my interest and skill in gymnastics. These athletic enterprises were cut short, however, by my successful tryouts for the Junior Class Play and the Senior Class Play the following year. My dad encouraged me and praised me for my roles in these dramas.

One of my "awards" in my senior year was to be named "student Rotarian," which meant going to a Rotary Club luncheon in a posh Phoenix hotel each week. I learned a lot about the culture of businessmen and bankers, who were the majority of local Rotarians. I was a well-behaved young Rotarian in a somewhat foreign culture, I

believe, except for one occasion. That was when I delivered a luncheon talk (a once-in-a-year reward and chore for the student Rotarian). I chose to talk about American economic imperialism, including the exploitation of Mexican oil (my Dad may have put me on that topic). The talk was something of a disaster, the last subject in the world that they wanted to hear from a smart upstart who had been rewarded by them. Several of them, including Howard Pyle, the future governor of Arizona, came up to me afterwards and gave me a tongue-lashing for my dangerous thoughts and irreverence.

Journalism was the greatest of my extracurricular interests. I did take one academic course in my sophomore year. On the basis of my performance, the teacher asked me to be sports editor of the Coyote Journal, the school newspaper, for the following year. My avid sports interests had continued into high school, so it was a welcome assignment. As sports editor I had a periodic column in which I spouted off on various sports subjects. I remember one in particular, written after a Big Ten team had squashed a Southern California team in the Rose Bowl. I ventured the blanket judgment that Midwest football was simply better than West Coast football. This reckless conclusion got me in hot water with one of the high-school coaches, a graduate from Stanford, who pulled me aside in physical education class and informed me that I didn't know what I was talking about and should keep my mouth shut. I didn't fight with him, but I didn't change my mind.

In my senior year I graduated to editor-in-chief of the newspaper, this time with a general column, in which I ventured to comment mainly on political subjects. This terrified my journalism teacher, who feared for the reputation of the newspaper, as well as his own, but I do not recall more than a few corrections and cautions. He also appointed me editor of the school yearbook, a less controversial position because its contents were largely determined in advance, but I cheerfully took on that position, too.

This journalistic experience, a helpful part of the development of my writing, served me well during the next few years. In the last year of high school I secured a job as proofreader for a new Phoenix newspaper, The Arizona Times, initiated by Anna Roosevelt Boettiger as a liberal voice to counter The Arizona Republic, the established conservative newspaper. I do not know how I came to get the job, but I sense I was recommended to one of the editorial staff members who knew my father. I quit my

grocery-store job to accept it. Proofreading was tedious work, but I learned a lot, and the proof desk was situated at the center of the print shop, so I became involved in the friendship group of printers and compositors. I felt a full success in that job when a group of the printers told me I should compete for a printing apprenticeship.

When I came home during the summer after my freshman year in college, the Times had folded financially, and I secured a summer job as proofreader at the Republic. All this was to supplement my fellowship and relieve my parents of supplementary support for my college years, but I found the culture of the little group of proofreaders to be rewarding. One fascinating aspect of the work was that a wide assortment of people in the community would telephone the proofreading room and its “experts” and ask for bits of information—a historical fact or the name of a play, for example. Evidently many of their queries arose in a discussion or argument at a party, and the call to us was to settle the matter.

During two subsequent summers of my college years I was promoted to cub reporter in the city room of the Republic, a job of much greater consequence. My job was to fill in on the beats of reporters who were on summer vacation—the police beat, the federal beat, the special-features beat. I was even asked to cover a few speeches by Barry Goldwater, who was making his first run for national office in 1952. I found him unimpressive at that time, and must have communicated that in my write-ups, because I was unceremoniously removed from that “beat” after a short time. The diversity of assignments was demanding but educational. It was enormously helpful for my writing style to be given a story to pursue and to write it up under a deadline in a style clear enough to be acceptable by my editors and to an imagined readership. My reputation as a sociologist has been, among other things, that of a clear writer in a murky field. If that appreciation has any truth, I attribute most of it to my journalistic background.

Despite this heavy involvement in journalism and despite my remote memory that journalism as a career had attracted my father in his young years, I did not make a commitment to it in thinking about my future. I cannot sort out the reasons for this, but two thoughts come to mind. First, and probably most important, by late high school I had already decided—though not in a fully articulate way—that my career would be an academic one. When my proofreading mates learned that I had been accepted at Harvard

they, especially one of them, urged me to go out for the Harvard Crimson, a natural move into a journalistic career. I balked at this, knowing that I was going to make academics my highest priority and that the time-commitment at the Crimson would undermine that intent. Second, during the summers I spent in the newsroom at the Arizona Republic, I came to perceive what I regarded as a cynical journalistic culture: the world is corrupt, most people are hiding things from you, and the job of the press is to expose them. That perception was surely in error, either because I was observing a small, biased sample of reporters or because I was exaggerating things, or both. Whatever the case, that perception turned me off.

Overall, my high school years read as a long season of burning the candle at both ends, of frantic involvement, the more so since I was also continuing my work at the grocery store, as well as pursuing my violin and later viola lessons. But I did not shun the informal peer culture of high school. I dated, went to dances, and socialized informally. But my pattern of interaction was perhaps unusual. I never became a full member any kind of high school clique, but hung around the edges of several groups and quasi-groups. I had one special friend, a Mormon boy in my year, with whom I double-dated for several years. I also regarded several girls as special friends, though I did not date any of them. I also socialized with a loosely defined quasi-group of talented, high performing students, among whom were several Jewish and Asian-American students. I also formed ties in the orchestra and in drama groups. By some strange circumstance I also came to socialize with a number of girls in a clique that regarded itself as socially elite (in social-class terms) above the mass of their schoolmates. I was not drawn to this subgroup and I regarded their pretentiousness as inane, but I was on the edge of it. Years later John Finlay, master of Eliot House at Harvard, told me that I was a half member of everything and a full member of nothing. I rejected his blanket assessment, but it carried some truth with regard to my friends and friendships in high school.

Collegiate Years at Harvard

And what about planning for college? The answer to this question is that I assumed I was college-bound but did very little about it. Without thinking I chose the Liberal Arts II curriculum, which was fashioned to cater to students preparing for college. Teachers and advisors in high school also urged me to go on to college on the basis of my

academic performance. But I did little if no systematic investigation of target colleges. Bill had communicated a sense of magic about Harvard during the time he was there, and that made it salient. I had also learned about Harvard's National Scholarship program, under which fifty outstanding high school students around the country (mostly from public schools, I believe) received full support throughout college. By circumstance I applied only to Harvard and the University of Arizona in Tucson, with a last-minute inclusion of Yale when I heard of one of its scholarship programs. Why was I so casual about college? Did I simply assume I would be placed? Was I ignorant? Was I unrealistically self-confident? Probably some indiscernible mix of all of these.

In all events, it was a risky application pattern I followed, even though I was to be named valedictorian (number one in a graduating class of 725) and received numerous other awards based on my high school performance. I note one of these in particular, the Goldwater Award, which included a gold wristwatch, given to an outstanding senior. I was proud of this, of course, and it came before Goldwater went into big-time politics. (The award caused him embarrassment in one of his subsequent political campaigns. A reporter noted that he gave the award at North High and Phoenix Union but not at Carver High School [the segregated institution for blacks]. Caught off guard, Goldwater responded naively that he hadn't noticed that, and reporters made a lot of that comment in light of his conservative politics.) I have mentioned that award with irony on many occasions, because my political values, even as a senior in high school, were, like my parents', a mirror of the liberal wing of the Democratic Party. The watch worked for only a few months, incidentally, but I kept it, my name inscribed on the back, as an unusual souvenir of my high school years.

There wasn't much preparation required to apply to Harvard. I had to sit for the College Board examinations, both the general math and verbal exams and a special one, for which I chose Latin. Harvard also had local alumni serving as interviewers for the National Scholarship competition, and I was interviewed in my home by one of these. When the announcement of my admission came (by mail) in May, I was of course delighted. The event was, however, tinged with ambivalence. The way I learned about it was as follows. I came home from school, and was presented with a note, I think from Philip. It said, "Look in the pocket of your tan sports coat in the closet." That note sent

me to search for another note in a pair of my shoes, and that directed me to a spot in a kitchen drawer. I searched through a trail of perhaps a dozen notes, the last of which read, “Congratulations! You are admitted to Harvard!” My parents had already opened the acceptance letter. It was an occasion for family happiness, but accompanied by a little ritual of frustration if not humiliation.

There were evident reasons for my parents’ ambivalence. They were happy to have their son receive such an honor. At the same time, it was an element foreign to the “all the boys are equal” family ideology. Also, they were losing a son to a locale 3,000 miles away, in all likelihood never to return to Phoenix to live. My father in particular, I presumed from knowing his prejudices, probably feared that my values might be compromised in the world of Eastern capitalism and high culture that I would be entering. All these aspects, however, only lurked behind the general feeling of family joy, but they remained as influences on how I reacted to my new Harvard environment.

Another note of ambivalence appeared at school. As expected, my past and present teachers, as well as my advisors, were pleased and proud of me for getting into Harvard, and they told me so. One advisor, however, raised a note of caution. He reminded me of Harvard’s greatness as an institution and in particular the high intellectual qualifications of the entering class. This was to tell me that I was going to be a small fish swimming in a big pond, and that I should expect to be a “B” student at best. I think he thought he was doing me a favor with this advice—that is, to help me come to terms with disappointment at not being academically at the top—but to me it came across as a judgment on my limitations as an aspiring young scholar. Interestingly, I got a similar message from Harvard during my first year. I earned A’s all my first semester. In fact, in the middle of that year I was informed that I was the co-recipient of the Detur prize, given to the most outstanding freshman. A little later, at a reception for freshmen who made the “Dean’s list” (basically all A’s), the freshman dean came up to me and asked, soberly, whether I was studying too hard and too much. I assured him that I was studying a lot but not in a debilitating way. Inwardly and secretly, I was delighted that I had apparently given the lie to both my advisor’s and Harvard’s expectations.

In September of 1948 I boarded a train for the long sentimental journey to Cambridge, replete with a mix of self-realization, self-doubt, and uncertainty. The arrival

at Harvard, through nobody's fault, proved to be frustrating in one respect. Selective service for young Americans was still in effect, in continuation of the World War II draft and destined to continue throughout the Korean War. Harvard, in anticipation that some of those admitted in the Class of 1952 would be drafted, admitted approximately 200 more students than it could house. But educational deferments were also extended, so that virtually all those admitted showed up. As a desperate measure, Harvard "housed" approximately 150 of us on cots on the basketball court in the Blockhouse, with the special exercise room set aside as a place to study. We lived in these primitive circumstances for a month while Harvard scoured for permanent places. This clouded the college scene for the arrivals, already carrying a burden of uncertainty and anxiety in leaving home and beginning college. I do not really remember the emotional impact of this frustrating situation on me. I do know that there was general dissatisfaction, and that I, along with one other emergent "leader," drew up a petition asking for a \$100 refund on our annual housing bill, on grounds that Harvard had not provided proper housing for us for a month. All but a few of the gym squatters signed the petition; a few others declined to sign, saying that they feared vulnerability to the draft if they signed. My co-leader and I went to the Dean's office and respectfully presented the petition, which Harvard apparently deemed reasonable, and the administration capitulated almost immediately. I do not know what drove me to lead even a quiet, reasonable protest—that kind of defiance was not in my blood—and risk being labeled as a troublemaker in this privileged setting. My apprehensions were unfounded, however, as Harvard accepted the request, and as the ghetto of blockhouse freshmen gradually dissolved as proper housing came available.

My first few months at Harvard were difficult ones, not from an academic but from an emotional point of view. I attribute this to the personal problems of direction-finding on the part of one who leaves a family setting from which one has never been absent for more than a few days and moves into a culturally somewhat foreign and extremely competitive environment. In any event, I experienced periods of loneliness and depression, and found myself obsessing so intensely and endlessly on philosophical dilemmas (for instance, free will vs. determination) that I have to conclude that this was mainly intellectualized fretting that concealed personal doubts and adjustment problems.

Interestingly, however, my psychological discomfort did not disrupt my academic work. I missed no classes, took copious notes, and—within a few hours after each class—typed up these notes and organized them, taking full advantage of those typing skills I had cultivated five years earlier. I scored at the top in every course I took, prompting the anxious questions about overwork that my dean put to me at the end of the first term.

The question of intellectual direction and the closely related one of declaring a concentration (Harvard's word for "major") were important, though they did not haunt me on a daily basis. My two major intellectual courses as a freshman were History of Philosophy (a clear gesture to my father) and the introductory course in Social Relations, a recently formed, pioneering department that encompassed anthropology, clinical psychology, social psychology, and sociology (a gesture, in part, to Bill). My other two courses were required Freshman English and French. It was my good fortune that Social Relations 1a was taught by Gordon Allport, the famous personality and social psychologist, an exciting teacher who brought his field alive. It was his analysis of Orson Welles' Invasion from Mars radio show that evoked an east coast panic in 1938 that generated an interest in collective behavior that was to become a major pre-occupation ten years later. Philosophy 1a also engaged me, but the course was marred by the departure (on account of illness) of its professor, Raphael Demos, another of Harvard's charismatic faculty members. In addition, I found myself wondering if Plato's and Aristotle's thought was sufficiently timely and important enough for me to invest my career. I imagine that this dialogue was not only with those great thinkers but also with my father. It was no struggle for me to decide on Social Relations as a major.

I did have a period of uncertainty as to which of the Department's four emphases to stress in my choice of courses. Intellectually I was drawn to the clinical psychology emphases, and took courses in dynamic psychology with Henry A. Murray and abnormal psychology from Robert W. White, both inspiring teachers. Murray recruited some students from his class, including me, to participate as subjects over the next couple of years in intensive, depth-psychology experiments at the Psychology Clinic, and even took a personal interest in me, inviting me to his home on several occasions. He encouraged me strongly to study to become a clinical psychologist and to take him on as my mentor. I will report later on how this relationship soured. In the meantime my brother Bill

lurked in the background; he was well into the clinical psychology graduate program at Berkeley. I did not pose this question openly to myself, but I am certain that at some level I was wondering whether I wanted to follow him into the same field. I was also drawn toward cultural and social anthropology, and took an introductory course called Anthropology and Modern Life from Clyde Kluckhohn in the spring of my freshman year after I had “tested out” of freshman English in the fall term. But Kluckhohn turned me off. He had recently finished his popular book, Mirror for Man, and his lectures were mainly a spoken version of that book. He was obviously not interested in teaching that course to undergraduates, and his lack of enthusiasm spread to the class, including me. That experience should not have soured me on anthropology as a dominant focus, but it did. George Homans taught Social Relations 1b in that spring term, bringing in several other sociology faculty as guest lecturers. I found Homans uninspiring, but I was electrified by Talcott Parsons’ three guest lectures, in which he systematically presented the outlines of what he was already calling the general theory of action.

Sometime in my second year at Harvard I decided definitely that I wanted to make my career in sociology, though I also continued the romance with interdisciplinary social science that Social Relations embodied. What I have written in the two foregoing paragraphs suggests that this decision was by default, and to a certain degree it was. But I was also genuinely excited by its subject matter, and in particular I took another full course on institutional analysis with Parsons and audited his course on the sociology of religion.

I report on another shift during my freshman year, which became more decisive over time. One of the political movements that grew in the years after World II was a campaign for a single, sovereign world government, and a more radical expression of the sentiment that fostered the creation of the United Nations. It was called United World Federalism, and constituted a challenge to the idea of individual nations-states as sovereign. In my last year of high school I was exposed to the movement, mainly by the sympathetic urgings from my father as well as one of my proofreading colleagues. I went to a few meetings of the Phoenix chapter and became a sympathizer, though not a passionate one, of the idea of a world government.

With this background I joined the Harvard chapter of the UWF early in my freshman year, a continuation of my attachment to the cause. I even became an officer—treasurer, I believe—in the chapter. But as the months moved on I found my convictions weakening. This distancing manifested itself in two ways. First, I came to sense that there was a kind of national UWF “line” that demanded adherence; I did not like that. (I also think that that sensitivity also expressed the general urge to distance myself from my father’s influence.) Second, I began to have doubts about the ideology of the movement, drifting toward the idea that it was promoting a program that was essentially utopian and unrealizable. By the end of my freshman year I had lost interest and was moving out of the chapter. I did not realize it at the time, but it signaled a general political orientation that involved a distrust of all utopian thinking, both right and left, both religious and secular. This orientation was to solidify over my lifetime, and has come to pervade my intellectual judgments in sociological writings (not always enunciated openly) and my personal politics. I regard myself as an anti-extremist and, more positively, as a pragmatic liberal.

I reconstruct the themes of my middle years as an undergraduate as follows. Taking the required courses to fill out my Social Relations concentration was one obvious direction. I took the required elementary statistics course with Samuel Stouffer, but I also decided to take advanced statistics as well with Frederick Mosteller, knowing that that would be a requirement in graduate school, to which I had already committed myself. I also took a memorable course from Barrington Moore, Jr. on political sociology that introduced me to Marxist and Leninist thinking. In that course I endeared myself to Moore by writing a term paper on Japanese economic development, on which he complimented me directly. On the basis of that paper alone he supported me thereafter. While intellectually stimulating, his style did not endear him to me; he seemed alternatively bored, opinionated, and hostile to the class. I audited a general course on civilizations and change given by Pitirim Sorokin, but by that time he had become markedly megalomaniacal and paranoid about his reception as a sociologist, and that was intellectually off-putting.

My reasons for choosing elective courses were to spread out to new areas and to a lesser degree to expose myself to Harvard’s great names—Zachariah Chafee, John

Fairbanks, Edwin Reischauer, Crane Brinton, and others. I had taken physics and chemistry but not biology in high school, and for that reason took a general education course in biology—to round myself out. I took introductory economics, not because I was especially interested in it but because I felt I should expose myself to the most theoretically advanced of the social sciences. I supplemented that with a course in economic sociology taught by James Duesenberry and Francis X. Sutton. Without fully articulating these directions, it is clear to me now that spreading out and crossing disciplinary lines were the main impulses. These were to remain salient during my entire career. My academic performance remained strong, winning me prizes for outstanding student in my sophomore and senior years, and admission to the Harvard's Junior Eight in Phi Beta Kappa.

In the summer of 1951—at the end my junior year—I had an experience that was most decisive in shaping my outlook and future. This had to do with my participation in the Salzburg Seminar in American Studies in Austria. Formed in 1947 by several idealistic young students, the seminar's purpose was to instruct promising young European scholars mainly about American culture, politics, and society. A larger purpose was to foster improved international relations in the era following the destructiveness of World War II. The seminar was held for six weeks each summer, drawing approximately fifty Europeans along with four undergraduates and four graduate students from Harvard (Harvard administered the program), and a number of American academics as faculty. Among the latter were notables such as Margaret Mead, Henry Steele Commager, and Talcott Parsons.

I did not apply for the Seminar, but was nominated and supported by the Social Relations Department, especially Florence Kluckhohn, and approved after an interview. I decided not to go home that summer, but to spend an additional month of the summer in solo travelling in Europe. So, in June I went to London, bought a used bicycle and took off, first around the south of England (including Oxford (in hope and anticipation) and then across France from Dieppe to Strasbourg. This was a completely solo journey, including several nights of sleeping in unfinished structures in a housing project in Paris. In the countryside I slept in farmers' fields and under bridges. A memorable, completely solitary experience.

Participating in the seminar later that summer was a great intellectual and personal experience (for a further account, see Smelser, 2009). As an American student participant my only real assignments were to attend some courses and to socialize with the other participants. I had anticipated that I would be overwhelmed intellectually by the European participants, but found myself holding my own. Perhaps more important, I participated fully in the collective life of the Seminar, experiencing all its emotional warmth. This was something new for me. I had never been swept away in a group setting before, and the experience added a new dimension to my self-image, which up to that period was dominantly one of semi-involvement. The Seminar experience was the most important intellectual and personal odyssey in my life, and it solidified a permanent attraction to European culture.

By the time I reached the end of my junior year, my academic record was such that there wasn't any question about my going for an honors degree. At Harvard at that time honors involved writing a substantial "undergraduate thesis" as evidence of ability to do research in one's area of concentration. A candidate could also forego taking one course in his senior year to prepare the thesis. In order to go for honors I had to choose a topic and a supervisor. I was at sea with respect to a topic, and was blessed when one fell into my lap. The administration of a local women's college had apparently got the idea that they would like to learn more about the informal aspects of social life of undergraduates at their college and had approached the psychology clinic to conduct some empirical research on this matter. The clinic turned the request over to one of its staff, Gardner Lindzey, also an assistant professor in Social Relations, whom I had befriended in my participation in the clinic's research. The other uncertainty involved choosing a faculty supervisor. Henry Murray let me know directly that he would like to have that role, but that very pressure turned me off; I feared he would be too directive. Instead, I asked Lindzey, a rigorous psychologist, but less directive and certainly supportive. Murray didn't like my decision, and expressed his disappointment. This was the beginning of my break with him, which was completed in graduate school, when I told him I had decided to continue in sociology rather than clinical psychology. This break with Murray disturbed and saddened me, but I learned later that it was something of a pattern with him—to befriend, then attempt to dominate, and then rupture.

The women's college had six dormitories, and I decided to investigate the determinants of students' popularity with other students, and, closely related, the determinants of friendship patterns. I made use of the sociometric method, somewhat in vogue at the time, which involved asking students to record their likes and dislikes of other students. With these data in hand, I was able to map the friendship patterns of a dorm, its "popularity structure," including its sociometric "stars" and "isolates." At the same time, I secured data on a number of background variables for each student—family income, father's occupation, family income, family's religious preference, ethnic background, and political attitudes, for instance—as well as students' areas of personal interest. My aim in the thesis was to sort out the latter as determinants of friendship and popularity. I predicted that the background variables would be most important in the early college years, but the "personal interest" variables would become stronger predictors in the later years as students sorted their friendships out.

During the middle of my senior year I analyzed all the data and wrote up the results. What took me somewhat by surprise was the dominance of religious identification. Protestant girls were uniformly most popular, Catholics in the middle, and the small minority of Jewish girls were at the bottom of the hierarchy in all the dorms, freshman through senior. Class variables were also predictors in the same direction, but weaker than religion. I found some evidence that personal attitudes did strengthen as determinants over the college years, but only moderately. My analysis was an extremely thorough one, and the results, along with my general academic record, earned me a summa cum laude degree at graduation. Lindzey urged me to prepare it as research report and submit it to a social-psychological journal. By that time, however, I had departed from Harvard, assumed my studies at Oxford, and had little interest in revising the thesis. I did so, however, somewhat desultorily, and sent it off to a recommended journal, which rejected it, on good grounds I believe, because my efforts were so half-hearted. Lindzey did summarize the thesis in one chapter of his Handbook of Social Psychology, so its results were not entirely lost.

I should report that the thesis, as an exercise in scientific social-psychological research, was as professional and non-judgmental as I could manage. Nevertheless, it nearly got me into trouble on three occasions. First, one of the conditions of granting

permission to conduct the study was that I would share its results with the administration of the college. So I visited its Dean and gave her a careful summary of the research results. That was a disaster. She was shocked to hear about the salience of religion in the dorms, especially with its overtones of anti-Semitism. She lost her temper with me, asserting that religious prejudice had no place in their school, and more or less dismissed my findings as wrong. Fortunately that episode did not spread. She had no reason to report my findings to her own school, and I was relieved that she did not complain to Harvard about this misguided young scholar.

Second, when I was competing for a Rhodes Scholarship in December of 1951, I was interviewed at the state level (Arizona) by a number of notables who constituted the selection committee. One of its members, a stranger to me, took the occasion to ask me about the thesis research, and then fell into a blistering attack on me and the project, dismissing it as unimportant, silly, and invasive of the students' privacy. I was very unsettled, and became immediately convinced that his assault had doomed my candidacy. It didn't, I suppose because the other members of the committee gave weight to my general academic record at Harvard and discounted this mischievous thesis.

Third, I learned of the following from one of my dons at Oxford. I had listed Magdalen College at the top of my choice of colleges, largely on the recommendation of several American professors who were knowledgeable about the colleges. At Magdalen, too, one or two members of the admissions committee attacked the thesis and the research on which it was based, saying that I couldn't possibly be taken seriously as a scholar on the basis my (possibly perverse) interest in girls' dormitories. The others, I was told, argued that my research, while possibly silly, did not outweigh my general academic qualifications. I resented all these criticisms, and would have really resented them if they had proven damaging in the end.

The Oxford Years

Though my seventh-grade fantasy about going to Oxford on a Rhodes Scholarship remained alive during my years at Harvard, it became salient only late in my junior year, when thinking about the post-collegiate years forced itself upon me and made that fantasy a reality. The competition was formidable. I had first to be "cleared" for application by Harvard College. I had to do nothing in this competition except declare my interest;

some weeks later I was informed I was eligible. The next step was at the state level. I applied in my home state, Arizona, rather than in Massachusetts, site of my college, largely because the competitive situation in the Western states was preferable (despite the gigantic presence of California) to New England, with its large number of top-flight private colleges. I almost came to regret this decision after that unsettling attack on my undergraduate thesis research in the Arizona interview.

The final regional interview (eight western states) in Pasadena was the most demanding, and I was uncertain and anxious. Most of the questions were the expected ones in a Rhodes interview—tell us about this odd major of Social Relations, tell us about your career ambitions, tell us about what sports you have participated in (I had done some boxing at Harvard and played for Adams House in intramural basketball). One line of questioning, however, merits notice because of its threatening character. Here is the story. Robert Gordon Sproul, long-time President of the University of California, was a member of the interviewing committee, along with other West Coast notables. At a certain point in the interview he asked me about any organizations I had joined as an undergraduate. I first mentioned the Social Relations Society, an undergraduate group sponsored by the Department, largely intellectual in orientation in that its major activity was scheduling presentations by local and visiting social scientists. I chaired that Society in my senior year. After I explained about the Society, Sproul broke in and asked if it had engaged in any political activities (a very relevant question in 1951 in the context of the McCarthyist years). It so happened that its sole political action was to have sent \$25 to the legal defense fund on behalf of faculty at the University of California who had been fired in the Loyalty Oath controversy. Sproul had supported the oath. The question was thus loaded politically as well as personally in this competitive situation. Without hesitation, however, I said, simply, that we had sent the \$25 to defend the non-signers. To declare that partisan act in that context could have been threatening to my chances. As it turned out, when I acknowledged it, every member of the selection committee burst into guffaws, in effect mocking Sproul for asking that question, and he had to join in the laughter. But as I reflected on the event after the competition, I wondered whether Sproul actually knew about the donation in advance, and asked the question to see if I

would tell the truth or lie about it. I remain uncertain about the whole episode, but it certainly reveals something about the political culture of those days.

After I was chosen for a Rhodes Scholarship at the Pasadena competition, another political situation almost prevented me from taking it. In the summer of 1952, right after my graduation from Harvard, I received a notice that I was classified 1-A and was to report for military service. Compulsory selective service remained in effect during and after the Korean War. At that time the federal regulations set aside Rhodes and Fulbright Scholars as deferrable on educational grounds. Local draft boards, however, could determine whether they would allow such deferments. In this context I applied for an educational deferment so I could take up the Rhodes. If denied, that meant that I would serve for two years and then take up the scholarship. My request for deferment carried no political baggage; I was not a conscientious objector and did not oppose the war in Korea. I simply wanted to take advantage of the deferment provision.

Responding to my appeal, the Phoenix draft board scheduled an interview. It went badly for me from the beginning. The board members were clearly hostile to my request. One member demanded to know why I had avoided taking ROTC in high school. I explained that students could choose ROTC or physical education, and I had chosen the latter. Another asked me to explain why I thought that going to Oxford was more important than serving my nation. A third demanded to know what kind of economics I was intending to study at Oxford—"Socialist economics" or "Churchill economics," as he put it—revealing both his ignorance and a political dimension. The local board rejected my appeal.

I had one remaining line of action. I could appeal their decision to the Arizona state draft board. I did that, still convinced that if federal draft policy permitted deferment, it was legitimate for me to request it. I was certain, however, that my appeal would be rejected. I did nothing more than appeal, but I learned that my father had urged the superintendent of schools in Phoenix to exert pressure on the state board on my behalf. He did so, but to what effect I do not know. Within a matter of weeks, however, I learned that the state board had overturned the local board's decision and granted me two years' extension of deferment to study abroad, but subject to serving after that time.

In October 1952, I sailed to Southampton on the liner *United States* with my fiancée. The rules of the Rhodes Scholarship, dictated by Rhodes himself, I learned, stated that if a Scholar married, he would forfeit his stipend. I could not afford that, so we assumed that she could find employment for two years; she did, with the United States Air Force.

What to study at Oxford posed an issue. I did know I wanted to read for a second undergraduate degree, because that would expose me to Oxford's tutorial system, its greatest strength. I was already committed in my mind to pursue sociology as a career. In line with its general academic conservatism, however, Oxford had never approved sociology as an undergraduate option. What to do (or read, in Oxford's language)? Two alternatives suggested themselves: Philosophy, Psychology, and Physiology (PPP) and Philosophy, Politics, and Economics (PPE). The decision was not a difficult one. I was not much interested in physiology, and psychology at Oxford was narrowly experimental in my estimation. PPE, on the other hand, seemed a "natural" for an intended sociologist with interdisciplinary leanings. I convinced myself it would help my intellectual career to become more than casually acquainted with these academic neighbors of sociology; this turned out to be true.

At the same time I assumed the Rhodes years would be something of an intellectual byway, and I was not as committed (or driven) to excel as at Harvard. In that context I decided to supplement my studies with athletic participation, choosing Oxford's preferred sport of rowing. I went out for crew at Magdalen College, learned quickly, and advanced up to the "varsity level" by the end of the first year. It was very demanding, however: four hours a day, six days a week of rowing (and no complaining). The time and the daily exhaustion were clearly diversions from my studies. On the other hand, my tutors, motivated mainly by the college's desire to attain as many First Class Degrees as possible for their students (as evidence of the college's excellence and competitiveness), had sized me up as a candidate for a First Class Degree in PPE, and directly encouraged me to have a go for a First.

At the end of my first year, I deliberately decided that I would take the academic line, even though I was asked to spend the summer competing to row in Oxford's varsity crew. I informed the President of the Magdalen College Boat Club that I was going to

discontinue rowing in my second year to concentrate on my studies. This was not a popular decision. When hearing of it, the President simply turned on his heel and would not speak with me. Magdalen College's rowing coach was heard to say, "Smelser's all right, but he didn't go to the right public schools." Even my scout (college servant who cleaned my room and polished my shoes daily) pulled me aside one day and told me, soberly, "Sir, a Blue is more important than a First." I knew, however, as an aspiring academic (rather than, say, an aspirant for the British civil service) that for me the opposite was true. Given all that, however, that decision was not an easy one. I had found my success in rowing to be very gratifying, and the camaraderie with fellow oarsmen a happy experience.

Another decisive event occurred in the summer after my first year. My economics tutor suggested to me that I apply for the George Webb Medley prize in economics, based on written examinations. I wasn't too confident, but he persuaded me. One of the exams was in economic history, about which I was completely ignorant and heretofore uninterested. I engaged myself in frantic reading of T.S. Ashton, John Clapham, and others. I didn't win the prize (my tutor told me I came close), but learned enough about (and came to be fascinated with) economic history that it later affected my choice of topic for the doctoral dissertation.

Another situation that developed during my second year was to prove most decisive for my career as a sociologist. Talcott Parsons was appointed Marshall lecturer at Cambridge University for the year 1953-54. As a topic for his lecture series he had chosen "The Integration of Economic and Sociological Theory." I heard that these lectures were a disaster because they were filled with Parsonian jargon understood by few in his audience (mainly Cambridge economists) and considered as heterodox by those economists who did understand. In all events, Parsons sent a copy of his lectures to me at Oxford and asked for my comments. He was coming to Oxford a couple of weeks later and wanted to meet with me. I was stunned and challenged by this because this sociological giant was approaching a "mere" undergraduate, and an apprentice at best. Also, though I had come to know Parsons a little in my undergraduate years, I was not certain he knew me at all. Apparently he remembered me and knew that I was studying economics at Oxford. I read the manuscript, and did have some reactions. He had

acquainted himself with Keynes only recently, and he was completely out of touch with postwar developments. I had some reservations about his sociological interpretations as well. I also showed the lectures and my comments to my college roommate, Bill Moffat, who found similar difficulties.

After our Oxford meeting I prepared a long memorandum for Parsons, listing all our reservations and criticisms. Because I was so frank, I dreaded his response. To my surprise, he was very receptive, and asked me to come to Cambridge later that spring to continue our conversations. We had a very fruitful weekend, at the end of which he asked me to come to Salzburg in June—where he would be teaching at the Seminar—to work further. Those meetings were also extremely exciting and fruitful. In particular we moved forward analytically in representing the economic and other social dimensions of society as sub-systems, and to fashion new ideas about exchanges among the subsystems. However, there was no reference to my ultimate role in his project, and no reference to anything like co-authorship. I imagined my role would win a note of thanks in the Preface. In our meetings Parsons also took an interest in my thinking about future research, especially a graduate dissertation, and urged me to return to Harvard for graduate work.

I decided on Harvard rather than Columbia for graduate school, and secured admission and a supporting graduate fellowship. Parsons and I continued work into the first semester, with more talks about the economics-and-sociology project. In one of these he suggested, almost offhandedly, that we co-author the book, to be called Economy and Society, because of my intellectual input and because I was better prepared to draft some of the material we wanted to include. I expressed my gratitude spontaneously and emotionally to Talcott (I had come to be on a first-name basis with him), because I knew how much that collaboration would mean for my future. But he, never one for much emotional display, seemed embarrassed by my burst of thanks, and wanted to get down to the brass-tack details of our division of labor forthwith. I will tell the story of that collaboration in the next chapter.

Back to Oxford. During all this time, I was preparing feverishly for my final examinations. One's final degree (First, Second, Third, or Pass) depended entirely on one's performance on eight three-hour written examinations (called Schools

Examinations) at the end of the two years (economic theory, economic institutions, economic statistics, general philosophy, moral philosophy, formal logic, British political history, and political institutions) as well as performance on an oral examination (viva) one month later, if that seemed necessary. I recall preparing for and taking the 24 hours of written exams (six hours a day for four days) with no nostalgia. How could one possibly study for them? And everything depended on them. I remember committing myself to go, nonstop, to movies for three or four days in advance of the exams to clear my brain.

After the exams my fiancée and I were married and took off for the continent for a honeymoon, to return one month later for the viva. I knew that if one had a certain result on the schools (for example, a clear Second Class degree), the committee merely asked a routine question and thanked the candidate. In more marginal cases (for example, between a First and a Second) the examining committee (nine dons in academic robes) examined the candidate more intensively. I appeared for my viva at 8:30 a.m., and they questioned me on several subjects. I was then dismissed, with instructions to return at 11:30. That told me I was on some kind of cusp. Then, after more questions at the second session, I was excused again and instructed to appear at 2:30. High anxiety time. At the third meeting I was questioned further. I remember only the last two questions. One of the philosophy dons asked me to analyze—as an exercise in linguistic philosophy—the differences between the utterances, “I promise” and “I love you.” I had been no great fan of Oxford ordinary-language philosophy, but I had become proficient at it. (This question also seemed apt for one immediately after his honeymoon). I remembered giving a textured, measured and confident response. Then another don asked me to compare and contrast the theories of causation of Hume and Aristotle. I panicked, because while I had Hume down pat, I drew a blank on Aristotle. However, just as I was about to reveal my ignorance, one of the dons, who had received a note from another, thanked me very much and excused me. Saved by the bell. I didn’t know the results of the exams until one month later, when as a matter of custom the Times published the results, and I read that I had got a First. However, about an hour after the viva, I met one of the examining dons on the street as I was running a last-minute errand before leaving town. We conversed somewhat uneasily for a few moments, after which

he said, “The third part of your examination was more interesting than the first two parts.” This was his way of telling me I had got a First! But I wasn’t able to confirm that until I read it in the Times. The fact that I landed on the favorable side of the First-Second cusp was to have a major consequence one year later, as I will detail in the next chapter.

I close the chapter with by reporting one final event (or, perhaps better, non-event) at Oxford. In the midst of our growing collaboration, Parsons had nominated me for a Junior Fellowship at Harvard. The Society of Fellows was an established, super-elite institution, appointing eight students a year at the graduate-study level, giving them three generously supported years of basically unsupervised research. Originally it expressed Harvard’s intellectual confidence by prohibiting the Junior Fellows, as they were called, from pursuing a Ph.D., confident that experience in the Society would gain them an appointment at Harvard or another elite institution. By 1954 this extreme policy was compromised, and a Junior Fellow could gain a Ph.D., but he could work only on a dissertation and was forbidden to take course work, orals examinations, and other requirements. As part of the evaluation process I was interviewed by one of the faculty Senior Fellows, the logician Willard van Orman Quine, who was spending the 1953-54 year as Eastman Professor at Oxford. Late in the spring I was informed that my candidacy was not successful. Unused to failure, I was very disappointed. However, as a backup I had applied for graduate study in Social Relations Department and was admitted, thus officially initiating the last phase of my apprenticeship as a social scientist.

Chapter 3

Graduate Training at Harvard, 1954-58

1954-55: An Impossible, Awful, and Wonderful Year

By all appearances, the fall of 1954—the year I returned to Harvard—presented a known path toward a doctorate in Social Relations—a journey of four to seven years of intensive study and research. The requirements included taking graduate courses in the four substantive areas of anthropology, clinical psychology, social psychology, and sociology in the first year, preparing for and taking orals examinations in several substantive areas of one's choice (hopefully in the second year), completing a project of field research, passing a foreign language examination, and conducting a serious research project to be written up as a doctoral dissertation. I was still officially draft-eligible, but the nation's military needs diminished significantly in the years after the Korean War, and my deferments continued throughout the graduate-school years.

My return to Harvard was marked by a certain ambivalence. On the positive side, I knew that the faculty was outstanding, that the department, along with Columbia, was the national seat of sociological theory, that I could contend with the intellectual demands of doctoral study, and, above all, that I had established a relationship with a mentor—Parsons—even before beginning graduate study. The negative side was also found in this familiarity. I knew the faculty and their interests. I did not know how much more and how much new would I learn in that known environment. As it turned out, an attractive alternative presented itself. I had applied and had been admitted to the PhD. program at Columbia. I received a very warm personal communication from Robert Merton, informing me that Columbia would offer generous fellowship support, and urging me to accept. With its obvious strengths, Columbia would afford me a new and expanded environment for my training. In a way, I was right to be tempted by this new setting. Subsequently I found that the four required courses at Harvard repeated much of what I had learned as an undergraduate—and were, as a result, less stimulating and sometimes boring. All this stewing about where I wanted to study, however, was rendered moot by the relationship I had established with Parsons in my second year at Oxford. To go anywhere other than Harvard would have sacrificed that great intellectual involvement and opportunity. To add to Harvard's attractiveness, the Social Relations Department

supplied me with fellowship support, and Parsons took me on as a research assistant during the final phases of his work with Robert F. Bales and others on family, socialization, and group analysis (Parsons and Bales, 1955).

My predicted life as a graduate student became more complicated within a matter of months. Parsons took the lead in re-nominating me to the Society of Fellows, and Barrington Moore, Jr.—remembering my great essay on Japanese development, no doubt—echoed the nomination, even though he was Parsons’ enemy-critic by that time. This time I was interviewed by the entire group of Senior Fellows, who grilled me on my plans for the dissertation research. This time I prevailed, and was granted a three-year Junior Fellowship, to take effect in the fall of 1955, one year after beginning graduate work.

As gratifying as it was, the Junior Fellowship presented me with an anomaly and a challenge. According to the Society’s rules, I could not take courses or meet the Department’s other graduate requirements while on the Fellowship. This meant that, if I were to gain a doctorate, I would have to take my qualifying oral examinations after completing my dissertation research and after the Fellowship ended. To me that was an impossibility, because I knew I would have to be seeking academic employment immediately after the Fellowship, without a full doctorate. I resolved this dilemma in a foolhardy way, deciding to take my orals at the end of one year of graduate study. That decision created the impossible year. In that first year I was to take all my course requirements and my orals, work for a couple of months as Parsons’ research assistant, and work with him as co-author on a major theoretical treatise, the deadline for which we had set for the summer of 1955.

Collaborating with Parsons.

Continuing the work on Economy and Society was by far the most exhilarating and demanding aspect of the year. Parsons himself had been enormously productive during the past decade. In the 1940s he had solidified his theoretical commitment to the theoretical scheme of “pattern variables,” which he regarded as universal choice-points (or dilemmas) that were encountered (and given solutions) as social action was structured and implemented at the personality, social, and cultural levels. The statement of the pattern-variable schema, which I regarded at the time as a creation of a genius, reached

its greatest maturity in an essay with Edward Shils that appeared in Toward a General Theory of Action (Parsons and Shils, 1951). Parsons advertised this book as a collective expression of the mission of the Social Relations Department. It also provided the major organizing scheme for his monumental The Social System (1951). At those very moments of fruition in 1951, however, Parsons, mainly under the influence of and in collaboration with Bales, turned to a major reformulation of his “general theory of action” as expressed in the pattern variables (Parsons, Bales, and Shils, 1953). This transition witnessed the formulation of the famous AGIL scheme (an acronym for the adaptive, goal-attainment, integration, and latent maintenance of the cultural system and handling its associated tensions). I had mastered the elaboration of the pattern-variable theory as an undergraduate at Harvard, and steeped myself in the AGIL scheme while collaborating with him, so I was intellectually prepared to work with him. My familiarity with the field of economics as of the mid-1950s brought resources that Parsons did not have to the project. In all events, using the basic AGIL scheme, we were able to bring insights to the nature of economic theory, exchanges between the economy and other societal subsystems, the role of institutions in structuring economic activity, economic processes such as trade cycles, and structural aspects of economic growth.

Throughout the academic year Parsons and I met frequently to explore and elaborate theoretical issues, to divide responsibilities for drafting, and to review one another’s drafting efforts critically. We also submitted ideas and drafts to an informal seminar that Parsons was leading, a group that included Ezra Vogel, Norman Bell, Edward Tiryakian, Jesse Pitts, and Christine Keyser, among others. The group was often baffled by the abstract material but, according to my memory, contributed much by way of critical comments. I still remember that process as an exhilarating one, and I thoroughly enjoyed mixing it up with this great mind. At the same time, I played an independent critical role, mainly attempting to bring consistency to our theoretical explorations and to locate and formulate empirical illustrations of all this airy stuff. This was also the main point of tension between Parsons’ style of thinking and mine, but the tension never spilled over into conflict. On the interpersonal side, Parsons treated me as an equal and over time included me fully in his social life, inviting me and my wife to social occasions and to his Vermont summer home. Our theoretical labors did not

include much humor, mainly because of Parsons' businesslike and reserved style, though I do remember moments. On one occasion, when I was helping him prune trees on his farm, we came across a large caterpillar, and Parsons made a number of playful comments on its limited adaptive subsystem.

The issue of writing style posed a major problem for us and the manuscript. A book of this composition was going to have problems of readability in any case, because it encompassed abstract theoretical formulations from two disciplines, each with its own language and each with its own problems of accessibility. In addition, Parsons' prose was an issue. His works were notoriously difficult, couched in his own special abstract jargon, and written with the apparent assumption that readers were familiar with all his past works. I was aware of all these stylistic issues at the time of our collaboration, but as a very junior collaborator I had difficulty coming right out and telling Parsons he was a bad writer and that I was a better writer—which I knew I was. In a somewhat brave move, I asked him in June 1955 if I could re-write Chapter 1 in an effort to give readers the clearest possible introduction to our interdisciplinary labors. Parsons assented. Furthermore, he liked the results of my re-writing efforts. Emboldened by his reaction, I proceed to do a re-writing of every sentence of the remaining chapters, still with apprehension, however. Parsons was very generous about all this, but I became aware that he was distinctly uncomfortable with my efforts to improve the final chapter on economic change. He didn't report his reactions to me, however, but gave the edited manuscript to his daughter, Ann, and reported that she thought my re-writing had gone "too far." I basically stuck with what I had done, however, and he didn't press for specific changes.

I have never answered the question of why, as a very junior collaborator, I made the potentially dangerous move of altering the language of Economy and Society (Parsons and Smelser, 1956) so radically when I really didn't have to. I suppose that, at a somewhat inaccessible psychological level, I was reacting to the trust that had developed between us. That was the decisive factor in permitting me to make that move. And even though some reviewers complained about the book's difficulty, I still believe that my efforts helped and that I made the right move in rewriting it.

A final ambiguity characterized my role as co-author. Parsons, who drafted the preface, referred to himself as the “senior author” and me as the “junior author,” even though I was listed alphabetically as co-author on the title page. I didn’t object to this or suggest that he change the language. I certainly was junior to Parsons; he was only a few years younger than my father, and he was a mighty force in American sociology at the time. I suppose that I also knew that co-authoring with him in any way would establish me prematurely, in my mid-20s, as a coming name in the field. Yet when I was being evaluated for a position at Berkeley in 1957, Marty Lipset contacted Parsons directly and asked him if I was really a co-author. It was reported to me (by Lipset) that Parsons angrily dismissed the question and affirmed that I was truly a co-author.

The reception of Economy and Society was less than we had hoped. It was reviewed several times by economists in economic journals, and most of their reactions were that the book was of little interest because it did not address technical issues that were pre-occupying economists. I also suspect that there was a jurisdictional dimension—resentment that these two outsiders were telling economists about the limitations of their field and how they ought to change their ways. Certainly they must have resented being told that economic theory was a “special case” in some larger theoretical enterprise; nobody likes to be downgraded to the status of a “special case.” Sociologist reviewers were more temperate and respectful, but my feeling was that the economic theory that we incorporated was inaccessible to them. More generally, I think that our book had less impact than it might have had over the years for two additional reasons: the fact that it fell between two disciplines going their own ways; and the fact that it was subsumed in the savage attacks on functionalist and Parsonian sociology in the 1960s and 1970s. So, while today the book is routinely described in references as “a classic,” I have to believe that it remains a “remote classic.”

Slogging Through Graduate School.

As indicted, the dictates of the Society of Fellows led to the bold decision to meet all the pre-dissertation requirements for a Ph.D. during my first year in graduate school. I did complete the four required Social Relations graduate courses with some ease, in large part because I had familiarized myself with so much of the several fields’ knowledge during my undergraduate years. I had already completed the graduate statistics

requirement as a junior in college. What remained was passing the oral qualification examination, the dissertation prospectus examination, the foreign language requirement, and the fieldwork requirement.

To be frank, I approached the orals examination not so much in the spirit of mastering new areas of knowledge as in a pragmatic spirit of getting it out of the way. As special subfields I chose economic sociology, an obvious selection because of my intensive reading and analyzing of its content and issues, and social stratification, not so much that it was a my greatest interest but more because it was a core field in which I had done very extensive reading during both my undergraduate years and my first year of graduate school. The examination itself was not memorable, but I do remember the questions put by the two senior faculty members of the committee, Parsons and Samuel Stouffer. I have the (unverified) suspicion that Parsons—with whom I was collaborating intensively—“fed” me an undemanding series of questions, having mainly to do with Durkheim’s and Weber’s contribution to economic sociology. Stouffer was not so kind. He asked one question on William Fielding Ogburn’s theory of cultural lag, a topic that was not very salient in sociological thinking in the 1950s. It was only a matter of luck that I had recently re-familiarized myself with it and could give a convincing representation of it. Stouffer then put a “killer” question to me and pursued it mercilessly. The question had to do with a hypothetical question about a grand banquet given by the mayor of Boston: which of the two should sit on the mayor’s right—the archbishop of the diocese of Boston, or Ted Williams, the iconic Boston Red Sox star? The reason it was a killer is that there was no correct answer to it, and it forced me into speculating about the different and perhaps incomparable dimensions of social status. I remember to this day fumbling around somewhat helplessly in the face of Stouffer’s dogged pursuit. My general feeling is that I performed passably on the orals, and that I deserved to pass but not more. I didn’t mind that feeling that I passed unceremoniously, because all I wanted to do was to pass. I also have the completely unverified sense that it would have been an embarrassment for the committee not to pass me, a young sociologist moving into the Society of Fellows.

I approached my dissertation prospectus examination in something of the same spirit as the orals. It is true that I had been working for some time on the intellectual

dimensions of the dissertation topic, which had to do with social-structural changes in the British economy and in the family of its working classes in the industrial revolution. I will discuss the evolution and meaning of that topic presently, but report at the moment that my preparation of materials for the exam was less than adequate, largely because I did not have the time in that year of over-commitment to prepare anything very systematic. I did submit materials to the prospectus committee and we had a long conversation about what questions should be central to the study. I remember that I did all right (again passably) but by no means brilliantly. As with the orals, that didn't matter all that much, because it, too, was forced upon me by the Society of Fellows' calendar, and my main intent was to get it behind me.

With respect to the remaining foreign-language and fieldwork requirements, I now confess a certain amount of fudging on the Society of Fellows' rules. On a given date in my first semester in the Society, I walked into the required departmental French examination, without preparation, and passed it. The Department of Social Relations recorded that I passed, but I believe that the Society of Fellows didn't hear about and certainly didn't do anything about this minor crime. The fieldwork requirement was more complicated. I think I could have met it by citing the enormous amount of work I had done in the historical archives in several American and British libraries for my dissertation research. For reasons that now escape me, I decided not to go this route, but rather to take the word "fieldwork" more seriously and do some quasi-anthropological investigation.

As it so happened, Stouffer had initiated a sizeable research project on community conflict, and found a small New England community—I called it "Beachtown" to disguise its identity—that was embroiled in conflict. The town had voted in referendum in 1956 to go "dry" and shut down some 40 bars and package liquor stores after an automobile accident, with drunk driving involved, that took the life of a local teen-age girl. In the ensuing period, the town experienced an unanticipated economic downturn associated with the decline of tourism and loss of the liquor business. A prolonged period of bitter conflict between "wets" and "drys" ensued, with hot feelings and many political-legal strategies, and in 1957 the town voted to reverse the original referendum and permit the sale of alcohol with, however, new regulatory machinery.

Stouffer and his colleagues secured permission from the town authorities to study the conflict, and I was the sole investigator. After several trips to Beachtown in late 1957 and early 1958, including interviews with town leaders and wet and dry activists, I discovered a complex economic symbiosis between “beach” (income-generating, mostly wet) and “town” (mostly middle-class residents, benefiting from income and taxes imposed on beach enterprises); a complex pattern of antagonism between “Old Yankee” and immigrant groups; class conflict; and a complex pattern of bribery and corruption involving commercial beach interests, town officials, and the police. All these cleavages surfaced and fed into the wet-dry conflict and provided a fascinating story of the dynamics of conflict and community turmoil. I wrote the material up for Stouffer, and later (Smelser, 1967a) published a more analytical account of the conflict in a chapter on social change in a text I edited. This touch with corruption also generated a side interest in the subject and produced, years later, in a theoretical statement on the larger significance of political corruption (Smelser, 1971c).

I found this study in the field very engaging, and a welcome interlude in the “real world” after my long season in the abstract theoretical heights with Parsons. But from the standpoint of the rules of the Society of Fellows, it violated the provision about meeting doctoral requirements other than the dissertation. As I recall, in December of 1957 I approached the chair of the Senior Fellows and asked if I could carry out this research. He found no objection; after all, I had finished my dissertation and it had just been approved, and I had already secured the appointment to an Assistant Professorship at Berkeley. The prohibitive Society rule was thus informally revoked, and my doctorate could be bestowed legitimately.

Formulating a Dissertation Topic

In my Oxford studies, I had learned a great deal about Great Britain’s constitutional and political history in the nineteenth century; picked up a substantial amount of its economic history during the industrial revolution; and gained a smattering of knowledge about its social history. Together, this knowledge had consolidated into a significant area of curiosity and some expertise. In a more pragmatic vein, I reflected that gaining this knowledge was a substantial intellectual investment on my part, and had supplied me with a great deal of knowledge that could be significant as “intellectual

capital” for subsequent scholarship. But until the spring of 1954 I carried that line of thinking no further.

At that time, however, I was forced to advance that thinking in response to Parsons’ interested inquiries about what line of research I might be undertaking for my doctoral dissertation. Under that pressure to specify, I provided him with two possibilities: (a) to apply not-very-well-articulated insights from the “general theory of action” to the understanding of the complex societal changes that unfolded during the industrial revolution in British society in the late eighteenth and early nineteenth centuries. Parsons was extremely interested and supportive, and we had a few exploratory discussions; (b) to apply some insights from “social system theory” to the world as a social system, with an eye to enlightening patterns of national interdependency and world conflict. I articulated this idea no further. Largely because of its non-specificity, I believe, Parsons showed little interest, even though he was to write later on the world as a social system (Parsons, 1969).

What really made the difference in my thinking on the industrial revolution were recent developments in the “general theory of action” and the recently vitalized literature on the sociology of economic development. One of the formulations developed by Parsons and Bales was called “structural differentiation,” a process by which a system moves from a less complex to more complex structure, and thus gains in functional capacity. The model was a kind of “problem-solving” one in the following sense: the system in question (personality, small group, organization, or social system) initially experiences dissatisfaction with its performance or functioning; the initial response to this is the expression of “symptoms of disturbance”, including anxiety, aggression, and fantasy solutions; these are brought under control through handling and channeling them toward more realistic and possible solutions; this process results in experimentation of various sorts; in the end, the processes produce new, more complex (differentiated) structures that are more effective in functioning than the outmoded one. Parsons and Bales (1955) had applied this model at the small group and personality levels, and Parsons and I had applied the model of differentiation selectively in Economy and Society (Chapter 5) to the process of separation of ownership and management described so famously by Berle and Means (1933). I also knew, from my general familiarity with

sociological analyses of the history of the family, formal education, and public welfare, that many of the features of their development could be characterized as processes of structural differentiation, and that economic and social growth involved the specialization of roles and institutions.

In thinking about a sociological analysis of the industrial revolution, I became more and more convinced that the model of structural differentiation was a powerful tool. I developed this idea in my thesis prospectus, and embellished it formally for presentation at the examination. I knew that this development was seriously incomplete, and that the committee could readily have suggested that the vague prospectus needed more specification and that a second meeting was necessary before it could be approved. However, the committee did approve it, not so much in itself but in large part as an act of faith in me as a promising young scholar who could be sent forward because of his recent collaboration with Parsons, who, moreover, was going to chair the dissertation committee.

In the months after the formal approval, I did in fact specify the project in two crucial ways. First, one of the committee members, Walt Whitman Rostow of MIT, a noted British economic historian and famous if controversial theorist of economic growth (Rostow, 1960) (and at a phase of his career earlier than that of national security advisor in the Kennedy and Johnson administrations), told me simply that to take on the British industrial revolution as a whole was an impossible research project. If I attempted that, he said, I would simply be swamped by the material and would deliver a product based on superficial research. He pointed out that I could say everything I wanted to say by choosing one salient industry. He suggested the cotton industry, the most important economic driver of change. I am forever grateful to him for that suggestion, which I followed. Second, I also came to realize on my own that, even in a case study involving a single industry, I could not possibly encompass all the processes of change in all the affected areas (family, community, class structure, political processes). As I read and thought more, I decided to focus on two lines of analysis: organizational change in the industry itself and changes the family structure of the working classes, along with the many social and political ramifications of these changes. In retrospect I believe that these two critical decisions to focus more narrowly saved my scholarly neck. Even after I had

made them, the amount of research required was gigantic. More important, if I had not made those decisions, I would have produced a much less worthy thesis and less influential book.

I constituted my thesis committee instrumentally. Rules called for three members, two within one's department and one outside. Parsons was the obvious choice as chair, and it would have been a blatant insult to him if I had chosen anyone else. The other obvious choice in the Social Relations Department was Homans, even though his brand of sociology, especially his subsequent embrace of social behaviorism, had little appeal for me. But he had written an excellent treatise on life in a thirteenth-century English village (Homans, 1941) and this made the case for his inclusion. As outside member I chose two: obviously Rostow, whose research in economic history covered the very period and episodes I had selected; and James Duesenberry, a very outstanding economist who had incorporated many "non-economic" variables and subjects into his theory of consumption (Duesenberry, 1949). Duesenberry also knew me from my undergraduate days in his course on economic sociology. I asked these other three members for little supervision and they offered equally little, being content with reading and approving the thesis after it was finished.

In reviewing the context of my chosen dissertation topic, I would today stress the risk if not the absurdity of that choice. Here was I, a virgin sociologist who at a young age had spread into psychology, philosophy, political science, and economics, was now declaring himself an economic and social historian as well. Furthermore, I was invading historical territory that was already occupied by established scholars who had defined and claimed areas such as industrial technology and working-class history. The latter topic had also been chewed up by controversies revolving around conservative, liberal, socialist and Marxist interpretations. What legitimate business did a sociology graduate student have in this arena? More on these issues later.

Life in the Society of Fellows

In most respects, becoming a Junior Fellow was a sheer gift to a young scholar. Here were three years of opportunity to do one's research without interference. The Senior Fellows offered benign interest and psychological support, but did not involve themselves as supervisors. It was assumed that their home department was carrying out

any supervision and control of these young geniuses. The level of financial support for Junior Fellows was at or higher than most assistant professors' salaries, and we had generous travel support and funds to buy any number of books we wanted (I remember pushing that privilege by purchasing Freud's complete psychological works on Society funds, stretching but not violating the limits). Perhaps more important, membership in the Society meant an exodus from the hothouse, competitive culture of graduate-student life. One's major group involvement was with the other Junior Fellows.

As it turned out, I was largely removed from supervision by the Department of Social Relations as well. Parsons was resident at Harvard during 1955-56, the first year of the Junior Fellowship, but during the second I was abroad in England, and during the third he was at the Center for Advanced Study in the Behavioral Sciences at Stanford. During those years we relied on correspondence, meeting face-to-face on only two occasions. All this meant that the "loner" side of my personality was perforce activated during the whole period of preparing my dissertation in the context of the Society.

The formal requirements of membership in the Society were two. The first was attendance at two lunches per week (Tuesdays and Fridays) with the other Junior Fellows in the Society's rooms at Eliot House. Conversations at these lunches were sophisticated and clever, but seldom developed into heated academic debates or political controversies. At these lunches some friendships, mostly casual, developed. I joined in as a full member of these luncheon sessions, but developed only two close friends, Henry Rosovsky, the economic historian, later a colleague at Berkeley and subsequently the Dean of Letters and Sciences at Harvard, and Geoffrey Bush in English literature. The second requirement was a weekly dinner (Mondays) with both the other Junior Fellows and the Senior Fellows. All the latter were famous academics of one description or another (a sample would include the philosopher Quine, the critic Harry Levin, and the historian Crane Brinton). Some past Junior Fellows would attend these dinners, and both Senior Fellows and Junior Fellows could invite guests. Sometimes these guests were superstars that one would only dream of meeting. I remember T.S. Eliot, Aaron Copeland, Thornton Wilder, and Hans Bethe among them. I recall inviting Parsons, Stouffer, Kluckhohn, and Murray as my own guests.

If I were to characterize the social atmosphere of the Society in one word, that word would have to be “precious.” We were generally but quietly felt to be an elite in an elite institution. The atmosphere of the Monday night dinners in particular was openly Anglophilic. These dinners were multi-course gourmet, preceded by sherry and followed by decanters of port wheeled around the table from person to person on an elegant little silver cart. I also discerned another feature that I had observed in full force in English college life and common-room conversations. I came to call this the “cult of cleverness,” with a high premium on conversational wit, and on discussing serious topics with the greatest levity and trivial topics with apparent emotional heat. It was also evident—but not advertised, for that would be bad form—that both Senior and Junior Fellows regarded membership and participation in the Society as an incomparable elite experience, much more special, for example, than appointment as a mere assistant professor on the Harvard faculty. Needless to say, these features excited all the ambivalence about elitism I experienced as an undergraduate at Harvard, making me something of a half-member, but I never advertised those feelings.

I also witnessed a darker side of life in the Society. Most of those appointed were ambitious, super-qualified, highly motivated, and continuing their pattern of high performance during their tenure. There were a few, however, who appeared to experience a crisis of confidence in this competitive, high-pressure atmosphere. The work of at least a couple fell into stagnation and accompanying bouts of self-doubt and paralysis. I attributed this to the Society’s supercharged atmosphere of brilliance and special status, which constituted a high motive to achieve, and at the same time, great internal psychological pressure. A few of us came to speak of this from time to time as “the J.F. crisis.” Subsequently I described this “syndrome” formally, generalized it, and built it into the social psychology of a sense of specialness that develops while participating in some kind of privileged odyssey (Smelser, 2009).

The Thesis, the Book, and its Reception

In the fall of 1955, I had a fairly well articulated thesis topic and three clear years of research time before me. This, I knew, was to be mostly solitary activity. I also anticipated (correctly) that the sheer volume of historical research required for this project would be monumental. Some of it, moreover, would involve a great deal of

detective work, since so many topics in the study (especially the family life of the working classes) were scattered and remotely accessible. I laid out my research strategy in the following way. I would spend the entire first year honing and sharpening my research questions and reading as voraciously as I could in Harvard's Widener Library, a treasure on almost all subjects. I planned to work through the available historical sources to discover the basic contours of economic and social change between 1770 and 1840, which I had decided were the decisive decades of revolutionary change in the British cotton industry and its working classes. I would spend the entire second year in various British libraries, mainly in the British Museum but also in more specialized libraries, to be determined. These turned out to be the British Library of Political and Economic Science, the Manchester Central Library, and the University of London Library, especially the Goldsmith's Library of Economic History. I did not know when I would be able to begin drafting, but I assumed it would be toward the end of the second year. I vowed that I would finish the work during the third year of the Junior Fellowship, so I would not have to drag an unfinished thesis into the first year of an academic appointment. I thought this a rational plan, even though at the beginning I knew that many new and unanticipated questions and research paths would appear.

The first year was one of basic investment in British industrial history and the history of the cotton industry and deciding on what topics would guide my further searches in British Libraries. It was clear enough that my home base in the second year would have to be London, but it became evident that I would surely have to travel to Manchester and Lancashire, the major sites of the historical story I would be telling. What I did not anticipate was the number and nature of new topics and new issues that would arise. I mention my discovery of how much of the work in the spinning factories involved the hiring of many family members; that the intensity of the campaign to regulate child and women's labor could be interpreted in the context of family changes; the anomalous historical fact that worker protest in the cotton industry was not present in the darkest periods of their exploitation and was greatest when their economic position was actually improving; and how changes in the family's structure—and ramifications of these changes—could throw light on the history of friendly societies, trade unions, savings banks, and consumer cooperatives during those years. As these various aspects

appeared as research surprises, I made them the object of aggressive investigation, and my analysis of them provided many of the most important objects of attention and controversy in the subsequent book.

My work in the library of the British Museum in Bloomsbury—it moved to the St. Pancras area as the British Library only later—was especially inspiring. I mainly sat in the giant, domed reading room, calling for books and having them delivered to my desk or to the typing room adjacent to the main reading room. I took all my notes on 4 x 6 inch cards in those pre-computer days, and I cannot today imagine how inefficient this was. I learned from numerous handwritten notices from the circulation desk that books with a certain range of call numbers were not available because of “enemy action” in World War II; I could almost reconstruct the pattern of bomb hits. I also imagined that I was actually sitting in Karl Marx’s seat in the reading room, and in fact I did read the same volumes of Parliamentary Papers that he had read in the state papers room. I also joked about imagined angry comments in German (surely Marx’s) in the margins of the Blue Book texts.

Reading the secondary materials in Widener Library and the British Museum also revealed the names of numerous British economic and social historians who had recently contributed to the understanding of my era. I decided to supplement my solo library research by arranging conversations with approximately a dozen of these in the London and Manchester areas. Among these were notables such as Eric J. Hobsbawm at Birkbeck College, Michael Postan and Peter Mathias at Cambridge University, and A. E. Musson at Manchester University. All these scholars, perhaps curious about this young American scholar roaming in their neighborhoods, were nonetheless welcoming and helpful in responding to questions I put to them.

About the middle of my year in England I began to experience a tension between continuing to accumulate the endless knowledge I was gathering and to begin writing it up. This was a matter of major concern for me, because I knew my time in the libraries was running out, but also knew that if I didn’t begin putting things together I might not finish writing the thesis in time. The “solution” to this tension involved a fortuitous historical accident. Before leaving for England, a Harvard colleague told me about a remote little institution in the south of France that hosted scholars, and he recommended

me to it. The institution was called Rustique Olivette, located in the hills above La Ciotat, a maritime port between Marseilles and Toulon. It had been founded by Daniel Guerin, a left-leaning French intellectual who had inherited some colonially generated wealth and invested it in a research haven for scholars. I applied for a month's stay in the spring of 1956, away from the libraries, to concentrate on drafting. I was accepted. It was a miraculous month. I took all my 4 x 6 cards and my research ideas with me and wrote furiously, drafting as many as twenty pages a day, with side trips along the coast for relaxation. I made the conscious decision to press ahead with this drafting, and when I came to a gap in my research knowledge, I would fabricate it, saving the remaining two months in the London and Manchester libraries to fill the gaps and set things right.

So, when I returned to Harvard for the third year as a Junior Fellow, I found myself ahead of schedule. I actually completed the drafting in the early winter of 1957, submitted it to the Department, and participating in the "graduation" ceremonies in December. The thesis was a huge product of 972 typed pages, entitled Revolution in Industry and Family: An Application of Social Theory to the British Cotton Industry, 1770-1840. Its length became immediately notorious, leading a couple of faculty members (not my committee members) to grumble and suggest an upper page limit on dissertations, and thereby not force dissertation-committee members to read such monsters. All the committee members approved the thesis. Parsons had followed the research at different stages, so his final approval was not an issue. Duesenberry approved it without comment, and Rostow wrote me a complimentary note containing no criticisms. Homans wrote a similar note of congratulations, noting as his only criticism the fact that the figures in one of the tables didn't add up correctly! Several years later, Homans, in his Presidential Address to the American Sociological Association (1964), referred to the thesis-based book as a "good, very good" piece of research, but added that it was good because it involved a break with useless (Parsonian) functionalist theory and used a superior scheme of interpretation. As an aside, I mention that when Homans delivered that paper as his Presidential Address, Talcott and I happened to be sitting next to one another in the audience, listening to his diatribe against him and his praise of me. We both sat quietly, expressing no emotion.

So, December 1957 was a memorable month of career and personal gratification. My thesis was finished and approved, and my doctorate was thereby finished; my first son was born on December 10; an appointment at Berkeley as Assistant Professor was guaranteed; and I had begun serious negotiations with the University of Chicago Press about the dissertation's joint publication with Routledge and Kegan Paul in London.

The story of its publication had some interesting twists. I knew that the manuscript had to be condensed in order to be published. I spent the spring months of 1958 slashing, rewriting, and making the exposition clearer. Peter Rossi, one of my Harvard teachers who had moved to the University of Chicago, approached its Press and pushed for its publication. The main critical reader was Robert Merton, an obvious choice because of his own historical research. He raved about it (he sent me his review at the time) and told the Press they would be fools not to publish it immediately. Chicago accepted it but wanted to enforce a major revision: to reduce the heavy theoretical exposition in the early chapters of the book. The logic of their thinking was that if I got right into the historical substance, the book would attract a larger readership. I objected to this, because I really believed the book should be regarded as an application of theory, and gutting the theoretical exposition at the beginning would compromise it as an intellectual product. The Chicago Press editors reacted to this conflict by sending the manuscript back to Merton for comment on the issue. Merton said simply, "the theory is necessary, don't disturb it," and the editors backed away. The book was published under the title, Social Change in the Industrial Revolution: An Application of Theory to the British Cotton Industry, 1770-1840 (Smelser, 1959).

I run a little ahead of my story, but will say a few words about the book's reception. In one of the first comments, The Economist hailed it as a brilliant, fresh, and novel contribution. The book was subsequently reviewed very favorably by several British economic historians; it was especially gratifying to me that I had apparently met their canons of historical research and interpretations, with no suggestion that I was an illegitimate outsider invading their territory. Reviews by sociologists were generally favorable, though Marion Levy, Jr., wrote a sniping review about its conclusions and exposition. I was somewhat hurt by this review, but wrote it off as possibly motivated by sibling rivalry; Levy was a very dedicated former student of Parsons, and had himself

engaged in serious historical research on East Asian development. Some years later I wrote a very critical review of Levy's treatise on modernization, so I suppose equal time was had by all. In all events, the book has been re-published several times, and also has gained reference as a "classic," which suggests that it is remembered and read about but probably not widely read.

Recruitment to Berkeley

I should begin the final episode of this chapter by taking note of the status of the academic market in the late 1950s and early 1960s. The college and university system had been given an enormous boost in the postwar years of the GI Bill for World War II veterans, and was readying itself for the onslaught of the baby boom on college admissions and attendance. The result was that those years produced a somewhat frantic seller's market for Ph.D.s., and to a lesser extent for pre-Ph.D.s. and non-Ph.D.s. Among the nation's elite universities the competition for top talent was intense, almost unreal. Sociology, in a very optimistic phase of development in those years, was very much a part of that pattern. During the three years after gaining my Ph.D., I received faculty offers from Berkeley, Harvard, Yale, Columbia, Michigan, Wisconsin, and the University of Chicago, all without solicitation or application on my part. One is tempted to flatter oneself and take all that personally, but I know better. It was mainly due to market forces. Subsequent periods of drought, especially the early 1970s and the first years of the 21st century, when many very qualified candidates went and go begging, drive home that point. I have jested ironically many times in my career that "everyone owes it to oneself to have been born in the Great Depression" as a way of illustrating the vivid career life-cycle impacts of that historic decade and subsequent ones.

The growth of Berkeley's sociology department coincided with the general market tendencies of that period. Though the University as a whole had developed into one of the nation's premier research universities, its growth in sociology had been stunted. Until mid-century it had a somewhat anomalous and sleepy Department of Social Institutions, associated mainly with the name of Frederick Teggart. Efforts to convert it into a conventional sociology department were strongly resisted by Teggart and some other powerful figures on campus, notably Alfred Louis Kroeber of the anthropology department. Around mid-century, however, with the strong support of

Clark Kerr, who became Chancellor in 1952, the department was changed to Sociology and Social Institutions (later quietly changed to Sociology) and given a mandate to build aggressively. Herbert Blumer, a leader in the field from the University of Chicago, was brought in to superintend the building, and over the next few years Reinhard Bendix, Kingsley Davis, Charles Y. Glock, Seymour Martin Lipset, Leo Lowenthal, and Philip Selznick were recruited. Subsequent senior appointments included Robert Bellah, John Clausen, Guy E. Swanson, and Harold Wilensky. Rising young stars such as Erving Goffman and Martin Trow were among the appointees in the 1950s. The department rose dramatically in national rankings of graduate-training centers, challenging institutions such as the University of Chicago, Columbia, Harvard, Michigan, and Wisconsin. It was into this Berkeley scene that I was invited to be an Assistant Professor in 1957.

To tell the truth, I would have been attracted to Berkeley as an institution even if it had not experienced its recent ascent in sociology. On the personal side, my brother Bill had settled into a career in Berkeley, practicing as a clinical psychologist and later to become a faculty member in the School of Social Welfare. The prospect of living in the same community with him was a powerful motive for me. Also, my parents were still living in Phoenix, as was my brother Philip and his family. Phoenix was nearer and easier to visit from Berkeley than from an Eastern or Midwestern University. I had also thought up another, somewhat intellectualized reason for liking Berkeley. I said to myself that Berkeley would be an ideal mix for me—a mix of my deep Western American roots, of which I was proud, on the one hand, and the cultured, cosmopolitan world of Harvard on the other. I say “intellectualized” because even if the point were valid, I didn’t think about that reason very much, and it didn’t have much to do with my subsequent experience at Berkeley. But in all events, when Marty Lipset approached me at the annual meetings of the American Sociological Association in August, 1957, and asked me if I had an interest in a position at Berkeley, my impulse was to blurt out, “Offer it to me and I’ll take it!” but my cooler self prevailed and I said, simply, “I would be interested.”

In October 1957 I was invited to pay a visit to Berkeley. It was not very elaborate—no “job talk”, which later became a standard feature of visits, but only an informal lunch with the senior members of the Department and a one-on-one half hour interview

with each of them. The lunch was congenial and it did not seem as though I was being evaluated. The interviews were also congenial, except the one with Kingsley Davis, who gave me something of an orals examination on the history of the British cotton textile industry. I suppose I passed, but I thought it an odd interview. The interview with Herbert Blumer was cordial enough, but I found him quite passive and remote, leaving me with the question of how aggressive he actually was in recruiting; Lipset was obviously the activist in the process. At the end of the final interview, with Leo Lowenthal, he leaned toward me in a friendly manner and said, "You've got the job if you want it, Neil." That moment was the beginning of our long, cordial friendship.

I had also combined the Berkeley interviews with a visit to Parsons at the Stanford Center, to go over some last drafts of my dissertation. I went there after the interviews. At a given moment during our conversations, Marty Lipset telephoned Parsons' study at the Center and asked to speak with me. He wanted to report that the Berkeley department had met shortly after my visit and formally voted to invite me to join their faculty. That call precipitated a comical scene. During the phone call Parsons discerned what was going on, and he commenced to whisper in an agitated manner, "Don't say yes! Don't say yes!" He knew that a Harvard offer of an assistant professorship was in the works, and he was beginning his campaign to keep me there.

As it turned out, Harvard did prepare an offer, and I got a feeler from the University of Michigan as well. Though I was already leaning strongly toward Berkeley, I didn't make a final decision until New Year's Day, 1958. I asked Talcott and his wife, Helen, over for champagne, and told him that I had accepted Berkeley's offer. The expression on his face told me he was crestfallen, and the mood was not relieved by Helen's effort to lighten the scene by blurting out, "Well, it's not so bad; you can now fly a polar route directly from San Francisco to London!" As it was, Parsons had been pressuring me from time to time to take the Harvard offer. In a moment of honesty I had told him that among other things I wanted to do was to set up my own shop. My comment was surely a relevant one, because he had continued to try to involve me in "his" group seminars during graduate school, and in my first couple of years at Berkeley continued to ask me to fly to Harvard to attend similar seminars. I declined all these invitations politely, usually on grounds of the competing demands of other commitments

in my life. It was easier to decline them from long distance than it would have been had I been at Harvard scene as junior faculty member. All this is to say that my decision to accept Berkeley was simultaneously a move, not entirely conscious, to remove myself quietly and peacefully from Parsons' influence without actually making a public break.

I report one final feature of Berkeley's offer. As one of the strategies of its aggressive growth and improvement, the Department had added a required upper-division theory course to "beef up" its major, and simultaneously to hire a faculty member with known strengths in theory to teach it. I was their choice for that position. Accordingly, during my period of negotiation with Berkeley, it became clear that the only demand from their side was that I develop and teach that course. I recall that they almost apologized for laying down such a requirement, because everything else about their offer was so open-ended with respect to teaching. They were at pains to leave the design and execution of the theory course completely up to me. As it turned out, I was in no way resentful of this condition, but welcomed it. It was certainly consistent with my self-image as a young theorist.

My situation in late winter and spring of 1958 was optimal. I had finished my thesis in December, and had six "clear months" ahead of me before moving to California. I filled up a part of this time by conducting the Bechtown research to fulfill Harvard's fieldwork requirement. In this period I also completed the shortening, rewriting, and otherwise improving my thesis for publication by the University of Chicago Press. But I also had ample time to design and develop that all-important theory course in advance of teaching it. Actually, I needed the time. I had never taught a course, except for a one-shot introductory course in sociology to US Air Force personnel in the UK enrolled in the University of Maryland's educational program for servicemen. In addition, there were many models of how to design a theory course: it could be a history-of-theory approach, assigning the works of the founders of the discipline; it could review contemporary theoretical developments; it could be an exposition of formal (including mathematical) models; it could be on the strategies of theory construction and testing.

As I labored with these issues, I fashioned a *mélange*. The readings would be in mainly "classics"; for the first year I assigned Durkheim's Suicide, much of Volume 1 of Marx's Capital; chapters from Parsons and Bales' Family, Socialization and Interaction

Process; and essays from Weber's political sociology. This would be rough going, and I do not know if I would today inflict such demanding works on juniors and seniors in college. (Willard van Orman Quine remarked over dinner at the Society of Fellows one evening that when a faculty member is young he is really tough and demanding of students because he labors under the illusion that he can really teach them something. Later on, with maturity, he gives up this fantasy and becomes a more reasonable if more cynical teacher. Perhaps I was illustrating that half-true principle.)

Yet I decided not to present these past masters exclusively on their own terms. I would employ a more formal method. I decided to make explicit at the beginning of the course the criteria by which one approaches and evaluates theory: What is a theoretical problem? What range of empirical data is called into question? What are the basic organizing concepts of the theorist's approach? How are these concepts organized logically? How are propositions generated? How are they tested or illustrated? Having laid out these questions more or less systematically, I decided to approach every one of the assigned theorists in the same way. First, I would summarize their theories sympathetically by attempting to identify the ways they addressed each of these questions; second, I would criticize their theories with respect to the ways they fell short of meeting all the criteria; and third, I would suggest various strategies by which their theories could be improved with respect to each criterion I had enumerated. So, in the end, my approach was both historical and formal. And for each theorist I asked the students to respond to a set question about each theorist in a five-page term paper. I should add that preparing the course was consistent with my style of thinking more generally: to treat the subject-matter or research problem as an object for analysis, that is, to objectify it rather than taking it entirely on its own terms.

I took advantage of my "free" time in the spring of 1958 to plan out the course and to write up, in advance, every lecture I would be presenting. I thereby avoided the unhappy experience of "chasing" the course and scrambling to prepare upcoming lectures. I taught that course, Sociology 109, modifying the list of theorists from year to year, from 1958 through 1965. It grew from 50 students and a sole teaching assistant to 350 students and 6 teaching assistants. Of course it was required for all sociology majors, but departments such as political science and social welfare began recommending

that their students take it. After a sabbatical in 1965-66, I never taught that course again; instead, I was recruited to teach, year after year, the required first-year graduate course in sociological theory. I still regard the teaching of that undergraduate course as one of the most gratifying experiences in my career. In 1971 I published the lectures, appropriately modified, as a brief general volume on formal sociological theory (Smelser, 1971a). Unfortunately, its publisher folded after a couple of years, so the book became virtually unavailable. It was republished recently (Smelser, 2011), more or less in its original form, and has enjoyed reasonable sales.

One more note on the course, though ahead of the main story of this chapter. Midway through the seven years of teaching, and as the size was increasing each year, I came to sense that the course was moving toward impersonality and coming to fit the stereotype of a large, faceless lecture course in a large, faceless public university. At that juncture there were about ten discussion sections. I had already taken up the practice of visiting each section once or twice to join in the discussions. That did not work very well, because I found the students and teaching assistants were treating me as the main audience. At one point the idea occurred to me to have each section to an evening in my home. It would not be on any topic in the course, but simply an informal social evening at which we would talk freely on any subject that arose. It would be a family situation, with my wife participating, and kids and dog wandering in and out as they chose. This was a very successful experiment. The students really welcomed the meetings. In ensuing years past students have encountered me and confessed remembering nothing of Marx or Weber, but certainly enjoying the evening at my house. I continued the practice in other undergraduate courses I taught subsequently.

So, well prepared and eager, in July 1958 I drove with my wife and infant son across the country, from Cambridge to Berkeley, stopping for a brief, sentimental look at my grandparents' razed farmhouse in Kahoka. Bill helped in arranging Berkeley housing, and, settled, I was eager to begin what would be my lifelong career at the University of California.

Chapter 4

Early Years at Berkeley, 1958-64

Dramatic Career Beginnings

My first experiences at Berkeley in the late summer of 1958 were welcoming ones. Almost all of my colleagues congratulated me on my appointment and some had me to their homes. Lipset and Reinhard Bendix (the new chair, replacing Blumer at the time I arrived) were especially friendly, as were Charles Glock and Hanan Selvin, the “methods” crew in the Department, and William Petersen, the Department’s specialist in demography (along with Kingsley Davis). My impressions on arrival and during the first few years were that this was a high-morale academic group, bound together by a culture of professional pride and committed to the Department, its academic quality, and its recent spectacular growth. Many, including myself, have referred the period of 1952-64 as the “golden age” of Berkeley sociology. That label is generally correct, but some have pointed out that there were no women and no minorities on the faculty as yet, and very few among our graduate students. I sensed some tensions among the faculty, which, however, were largely latent. For example, some had had youths of passionate commitment and activity with the far left in the depression years, and doctrinal rivalries (and their changes over time) still echoed. There were also some tensions among those representing different sociological styles—positivist-methodological vs. theoretical and politically neutral vs. politically committed, for example. There was some evidence of coolness among some colleagues, bred by differences of personality and interpersonal style. These were undercurrents, however, and the general tone was one of congeniality and professional commitment. I was also happy with what I sensed was the culture of the Berkeley faculty as a whole, which was characterized by institutional confidence, a spirit of aggressive building, affluence, and relative peace (despite some inherited scars and cleavages from the Loyalty Oath crisis of the early 1950s). My general feeling in the first months and years was that I had made the right decision and that I was in the right place.

A really dramatic incident occurred within a matter of days after my arrival in Berkeley. I got word that Robert Merton had telephoned me. I returned the call, and learned that the Columbia sociology faculty had voted to offer me an associate professorship with tenure in their department. I was staggered by this news, and agreed

to come to New York in the coming weeks on a courtship visit. The background for this dramatic move was Merton's very enthusiastic pre-publication review of my dissertation for the University of Chicago Press, and, I presume, his conversations with others and his impressions of me on the few occasions we had met. I reported the call to Bendix, but made no explicit demands on him or the department. Clearly, however, the move by Columbia put the department and the campus in a difficult position. Whoever heard of Berkeley, a proud university with most rigorous standards and procedures for advancement and promotion, offering a newcomer promotion to tenure immediately after his arrival? And with no single-author publications and his dissertation only "in press"? Nevertheless, Bendix brought the offer to the attention of the senior faculty of the department, who voted unanimously to promote me to Associate Professor with tenure. Needless to say, this augmented my sense of welcome at Berkeley.

Difficulties in responding to Columbia's offer remained, however. The Dean of the College of Letters and Science, Lincoln Constance, called me to his office and explained, painfully, that Berkeley could not, as a matter of institutional possibility, offer me tenure immediately upon arrival. However, he promised me that he would recommend tenure the following year, and in the meantime would authorize an advance within the ranks of the Assistant Professorship as a vote of confidence. He also begged me to stay at Berkeley. I did schedule a trip to Columbia a month or so later, and subjected myself to a campaign of persuasion by Paul Lazarsfeld (who knew me slightly from meetings when he had visited Harvard), Merton, and other faculty members. I did ponder a future in New York, but in the end decided not to leave Berkeley, trusting the institution to do what it promised the following year. It did, and that decision was further facilitated by the fact that in the following year (1959) both the University of Michigan and the University of Wisconsin had also extended offers of tenured positions, and I had visited those two institutions as well. As I mentioned, the seller's market for faculty was thriving. The University of Chicago came after me the following year, and in response Berkeley promoted me to full professor, taking effect in 1962. Can the reader believe, that in the context of this recognition and reward, in my first months and years at Berkeley I could still entertain occasional unrealistic and paranoid fantasies that I would fail professionally and that my Berkeley colleagues would turn against me? In my saner

moments I rejected such fantasies, but they drove home the point that, for me at least, such success cannot come without guilt and fear of retribution.

Through all this drama I was carrying out my teaching duties at Berkeley. The normal teaching load at that time was five courses (3-2 or 2-3) per year, heavy by current standards. One of these was my course in sociological theory, required of both me and of all sociology majors. I found this baptismal teaching experience rewarding, and in no way did I resent it as a requirement. The remainder of my teaching was in social change and economic sociology, both my choices and my chief intellectual interests at the time. I remember one additional teaching assignment, though do I not recall it as memorable. In my second year, Kingsley Davis initiated a request that I co-teach the required graduate course in sociological theory with him. It was the first of four such collaborations—the others with Philip Selznick, Arthur Stinchcombe, and Michael Burawoy. I was not certain why Davis initiated that request. Perhaps one element was that we were both “Parsons students” at different times, though we had different takes on our mentor, and Davis had moved radically from Parsons in an empiricist direction in his later years. But we had not befriended one another since my arrival, and I remember his “oral examination” of me in my recruitment visit with lingering distaste. Also, another colleague, David Schneider in anthropology, had co-taught the same course with Davis the year before, and had told me that it was a difficult experience, fraught with conflict. I had independently developed the impression (combined with some uneasiness) that Davis was a highly opinionated man prone to harsh judgments of others. Despite these misgivings, I agreed to join the teaching enterprise. As it turned out, it was more parallel play than collaboration. At his initiation, we divided the course more or less into half, I taking those theoretical topics closest to my interest, he taking those closest to his. We did come to one another’s lectures, but seemed to have a tacit understanding that we would not challenge one another or otherwise mix it up intellectually. We did not meet and plan outside of class. I suppose this is one way that colleagues with a chilly relationship collaborate—they non-collaborate. I heard from another colleague that Davis had said that he admired the content of my lectures. He never told me this, however, and I never conveyed my positive reactions to the many brilliant things that this brilliant man said.

In the meantime, Berkeley (mainly in the person of my chief supporter, Lipset) was working to reduce my teaching responsibilities. That worked out in the following way. As one of his innovations, Clark Kerr had authorized the creation of a special little “Center for Integrated Social Science Theory” on the Berkeley campus. The word “Center” actually dignified it; it was in reality a group of six or seven behavioral and social scientists who met to discuss and criticize one another’s theoretical work. Each appointee was relieved of half his teaching responsibilities for two years (in my case, 1959-60 and 1960-61). This was a great gift, facilitating my massive project to produce a major theoretical work on collective behavior and social movements. The group read emerging chapters of that work and helped me greatly in its progress and its quality. I will not detail its entire membership over those two years, but mention a number of colleagues who were especially helpful to me in their critical comments: Frederick Balderston (Business Administration); Richard Lazarus (Psychology); Erving Goffman and Leo Lowenthal (Sociology) and David Schneider (Anthropology). Marty Lipset’s role was to agitate for my appointment to the Center. That appointment was also tacitly understood as evidence that my colleagues were making life at Berkeley as comfortable for me as possible in the face of outside academic offers.

Work on Collective Behavior

Sometime in the spring of 1958, in that last period of “free time” during the Junior Fellowship, I formulated a plan for my next major research project and monograph. I envisioned that this would be a major, theoretical, and synthetic treatment of that large but chaotic territory covered by the terms “collective behavior” and “social movements,” heretofore treated sometimes as one subject and sometimes separately. I do not remember any conscious struggle on my part in choosing such a topic, but I have reconstructed the following known and not-really-known influences.

The first influence goes back to my freshman year at Harvard and Gordon Allport, whose lectures and demonstrations excited an abiding social and psychological interest in topics such as rumor, panic, and riot. Because of him I remained drawn to the whole range of phenomena covered by the term “collective outbursts,” and to include them in my own intellectual agenda of sociological research was an effortless decision.

The second influence derived from the research for my doctoral dissertation. As I had planned that project, I did not give much salience to group movements as part of the economic and social history of the cotton industry. As I moved into the research, however, I came to realize how important such movements were in that history. I refer in particular to working-class resistance to machinery, trade unions, agitation to limit working hours of children and women, Chartism, consumer cooperatives, and savings banks. Not only did I find these movements interesting in themselves but also I was able to relate them systematically to the larger institutional transitions in Britain in the nineteenth century. In doing so I expanded the understanding of those movements beyond the received explanations based on reactions to economic exploitation and economic interest. These interpretations proved to be the most novel and controversial aspects of my dissertation and cemented my more general interest in such phenomena.

Third, and less consciously, my dissertation and decision to move forward in research on those topics was part of my long-standing dialogue with Talcott Parsons. This process had begun in my dissertation research. Three criticisms of Parsonian functionalism had haunted me even before the onslaught of criticism in the 1960s: (a) his theories were unacceptably abstract and remote from empirical reality; (b) his theories were static and focused on equilibrating mechanisms, and could not analyze processes of social change; (c) his theories emphasized social integration, common values, and successful socialization, and ignored social conflict as a driving force in society. Though I insist to this day that my dissertation was not an act of rebellion against Parsons, it is true that my work was grounded solidly in historical data, was primarily oriented to social change, and—particularly in its focus on protest—built social conflict centrally into the analysis. Was I, mainly at an unconscious level, responding to these criticisms and doing Parsons one better?

Choosing collective behavior and social movements as a primary object of study extended these “responses” to criticisms of action theory, even though its early chapters were based on Parsonian categories and classifications. Furthermore, in that study I also “relaxed” one important assumption, namely that in processes of societal differentiation it is assumed that fundamental social values do not themselves change, but operate mainly as legitimizing influences. In radical revolutionary and religious movements, however,

values themselves are the objects of contention, and the assumption of their stability has to be changed if not dropped. This was still another modification of the fundamental assumptions of action theory.

Fourth, and completely outside my consciousness at the time, there was the person of Herbert Blumer, the doyen of the social-psychological studies of collective behavior, and the chair of my department-to-be. Was there some way that I was, in advance, threatening to de-throne him and his approach? At the time I had an attitude of innocence on this score and remember no such intent, though I did assume a critical stance toward all psychological explanations of collective behavior (including the University of Chicago tradition) in my efforts to generate a genuinely sociological interpretation.

Tensions with Blumer arose as I moved forward on the research. I am not certain why, but I sent him a completed draft chapter of what was to become Theory of Collective Behavior (Smelser, 1962a), and set up a lunch to discuss it. He obviously read it carefully. The lunch proved very uncomfortable for me. Basically he scolded me, saying that I was taking the wrong approach; that I was treating this behavior as people being pushed around by systems and structures; and that I paid no attention to the communication and strategies of leaders. I didn't learn too much from this lunch because it was so repetitive. I emerged from it with the feeling that I had been given a stern symbolic interactionist spanking. Subsequently I sent him a draft of the chapter on panic, but we did not schedule a lunch. I got back a strong letter, saying almost the same things I had heard before. I wrote him a detailed letter in response, trying to explain our difficulties. I sent him three or four more chapters, and his responses were still the predictable ones, as were my responses. I don't think he thought me stupid, only misguided. Subsequently, at a social gathering of sociologists where I was present, Blumer said, publicly and somewhat tastelessly, "Parsons told me that Neil Smelser was his best student." It was an obvious compliment from his point of view, but to me, knowing what I knew, it sounded as if he were saying I was the best wrong-headed sociologist in the United States. I thanked him for his critical readings in the Preface of the book, but our relationship leveled off into a civil but cool one.

I summarize the work on collective behavior briefly for the reader. I began with negative polemics, saying that I would not treat collective behavior as based on its physical or temporal characteristics but rather in terms of the types of belief that gave rise to an episode or movement; that an episode would not be defined by any special kind of communication or interaction (this is what Blumer objected to most); and that collective behavior would not be defined in terms of any special psychological state (this was an explicit critique of the LeBon-Freud “irrationalist” tradition). Instead, I defined collective behavior as “mobilization on the basis of belief which defines social action” (*ibid.*, p. 8) meant to change things because they are believed to be threatening or amiss. This led me to a very elaborate classification of what it is that can be regarded as amiss, and here I used the Parsonian scheme the components of social action—values, norms, structuring action into roles, and situational facilities. Finally, I classified the types of collective behavior as actions oriented according to a belief that would change one or more of these components. I came up with the major types: panic, craze, hostile outburst, norm-oriented (reform), and value-oriented (revolutionary, religious).

In treating the determinants, I shunned single-factor explanations, but insisted that an episode of collective behavior will occur (be determined) when a number of conditions combine in an order of increasing specificity, and each operating with the scope established by the former. I called this a “value-added” series, each determinant “adding its value” to the accumulation of determinants and to their causal combination.

I classified the determinants in the value-added scheme as follows:

- Structural conduciveness, or whether individual and groups can engage in the behavior at all; for example, if economic arrangements do not permit rapid transfer of property or money, a society cannot experience financial panic.
- Structural strain, or the existence or perception of malfunctioning, injustice, inefficiency, or relative deprivation in existing structural arrangements.
- Growth of a generalized belief, specifying some line of action that identifies the strain and formulates lines of action for its amelioration.
- A precipitating factor that aggravates the strain or confirms the generalized belief.
- Mobilization for action, either to effect change or persuade constituted agencies

to effect it.

- The reactions of agents of social control to the emerging behavior or movement.

Having set the theoretical stage, I then composed five detailed empirical chapters, each devoted, respectively, to one of my five types.

The book was widely noticed and generated a most diverse variety of reactions. Since these changed over the years, I will reserve discussion until the next chapter. When the book was published I felt generally satisfied with it as an intellectual accomplishment, taking special pride in the degree of discipline and system I was able to bring to very scattered and incoherent literatures, my solution to the problem of causality, and the originality of my discussions of structural conduciveness and the reactions of agents of social control as determining or shaping factors.

Other Scholarly Enterprises

As a scholar's career moves along, three temptations present themselves. First, he or she may be asked—or decide independently—to defend, elaborate, re-publish, or revise works that have made him or her known. In general I have let my works stand or fall on their own. I have not been prone, with few exceptions, to enter polemic contests with reviewers and critics of my works. Early I observed to myself that such battles bear little fruit, and often degenerate into ad hominem exchanges. I have also resisted writing second editions of things. For example, numerous people asked that I revise my book on collective behavior in the 1970s, when so many social movements of so many descriptions had shaken this country and the world. Such requests had merit, but I resisted them. I am not certain why, but the reasons I gave myself were that combing over already-published material lacked the excitement of the original enterprise. I discovered this when I assigned Theory of Collective Behavior to a graduate class on social movements, and became bored with teaching my own material. I also became bored with revising a popular text in introductory sociology in the 1980s and 1990s, even though I did see it through five editions at the insistence of the publisher (below, pp. 00-00).

Second, one is often influenced by the appearance of new situations, problems, and events in the surrounding society and the world. My attraction to writing about

economic development in the post-World War II period, my attraction to writing about higher education in its decades of turbulence (1960s and 1970s), and my decision to write a book on terrorism after 9/11 are examples. Further, if one is engaged in developments in one's own discipline and others, new formulations and developments (for example, social constructionism) inevitably become a positive or negative influence on one's thinking. This has also been an important theme in my choice of research topics.

Third, when a scholar becomes known, it frequently happens that other people with their own independent interests recruit him or her to new enterprises that they have launched. That has also happened to me often throughout my career. I have accepted some of these invitations and rejected others. To accept such invitations does not necessarily mean that one cannot say anything novel or important—most invitations are open-ended with respect to how one responds to and elaborates on them. The products of these invitations are thus inevitably a mix of constraints imposed by the inviter and the initiative of the invitee.

The Sociology of Economic Life. Prentice-Hall, a large and aggressive press in the post-war scene, was one of many that ventured into the publication of some scholarly monographs in the heady years of academic growth. (I will report later on my involvement as sociology series editor for them). In this enterprise, they recruited Alex Inkeles of Harvard to edit a major series called Foundations of Modern Sociology. It was a highly successful series, encompassing more than 20 volumes, including those by many “name” sociologists, such as Daniel Bell, Joseph Ben-David, Paul Lazarsfeld, Wilbert E. Moore, and Talcott Parsons. Most of the books were intended for use as high-level texts in college courses on different sub-fields of sociology, and the publisher invested more than the usual effort in producing clear, readable prose in the books.

Alex invited me to write the book on economic sociology. It was a reasonable request, because I had collaborated with Parsons on a major theoretical book on the subject. Also, my dissertation had been on economic development and I was developing a general interest in that subject. And I had taught an interdisciplinary course on the sociology of economic life (the title I ultimately chose for the book—Smelser, 1962b) in my first year at Berkeley. (There were personal ties as well. My first wife lived with the Inkeles family as a child minder while an undergraduate at Radcliffe, and Alex had been

a supervisor of my studies in social stratification at Harvard. Also, Marty Lipset, one of Alex's intimate friends, pressed me to take on the assignment.) I was attracted by the invitation at one level, for here was an opportunity for me to engage in an act of synthesis and "create," analytically, a subfield that was only nascent and scattered at the time. On another level, however, I dreaded the assignment, because I was so engulfed in writing Theory of Collective Behavior that it would be foolish to take on another major writing assignment in the middle of that. I was right in that apprehension; I remember many middle-of-the-night anxieties that I would never be able to finish the economic sociology volume and the collection of relevant readings to accompany the book (Smelser, 1964) anywhere near the publisher's deadline.

As it turned out, I was able to write the kind of book I wanted. It had a focus: how sociological knowledge can inform and revise economic thinking and research on economic behavior, processes and institutions, economic motivation, consumption, savings, organization of production, and growth. Furthermore, I was able to range widely and synthesize scattered literature from the history of economics, sociology, and anthropology, organizational sociology, industrial sociology, sociology of culture and ideology, and processes of growth. The book also fit my personal predilections and style; it was broadly accommodative of many lines of thinking and research; it contained criticisms, especially of micro-economics and rational-choice theory, but I believe I recorded these criticisms in a style that stressed synthesis and advancement of knowledge and did not give in to the overly-negative, overly-polemic style of so many social-science critics of economics. It was not the best of sellers in the Foundations series, but Prentice-Hall did commission a second edition (Smelser, 1973a). The book has been treated as one of the forerunners of the subfield of economic sociology, now one of the most vibrant in the discipline. That work also solidified economic sociology as one of my permanent research interests, to which I have returned periodically.

A Sociological Reader. Readers of this chapter will notice that Lipset was a great supporter of mine in my early years at Berkeley. Until preparing these autobiographical reflections, I did not realize how extensive that support was. Perhaps that slippage of memory results in part because I did not become very close to him personally. He was a shy man and reluctant to show personal feelings directly. He also came across sometimes

as crassly ambitious and this annoyed and alienated others. But I was very grateful to him.

In all events, when he approached me in late 1959 to collaborate with him in preparing a “reader” (collection) of journal articles reflecting the state of sociological knowledge in the preceding decade, I agreed without hesitation. Apparently he had discussed this idea with Prentice-Hall, the ultimate publishers, so there was no problem in finding a venue. Readers were “in” at that time, and this seemed a reasonable idea to all. I did not regard the volume as a great intellectual investment, because we were both au courant with the recent sociological literature, and the similarities in our intellectual judgments were greater than the differences.

As I re-visit that volume (Lipset and Smelser, 1961), it seems clear that we did, indeed, manage to include most of the major writers and the influential formulations of that decade. Almost all authors agreed to have their publications included; the single exception was C. Wright Mills, already no friend with Lipset, who demanded such a high honorarium that he drove himself and his article out of the volume (no other author asked for anything).

We organized the selections in a somewhat ad hoc, vacuum-sweeper, but serviceable manner. An initial section included articles on the status of the discipline, including its major theoretical and methodological conflicts. The middle section (reflecting a Parsonian influence) took the society as a social system as its starting point, and classified recent journal contributions as they contributed to knowledge concerning its cultural, personality, and physical-biological boundaries. A third part dealt with economic, political, and prestige-allocating processes, and a final section considered the balance between stability and change in society.

This encompassing intellectual canopy for the collection revealed a number of unacknowledged and unresolved tensions. My recent re-reading of our Introduction revealed the most important of these. On the one hand we used the term “progress” in the title, suggesting some kind of orderly accumulation toward a goal, presumably a scientific understanding of society. We said “. . . most important sociological works today attempt to locate their concerns in a body of developing theory” (*ibid.*, pp. 4-5). On the other hand, in the very first paragraph of our Introduction, we characterized

intellectual developments as “commonly plagued by . . . controversies, revolutions, secessions, and accusations of heresy and subversion associated with the dynamics of religion and politics” (*ibid.*, p. 1). In including both emphases, were we not referring—without knowing it or saying so—to the relative serenity and optimism of sociology and the other social sciences in the 1950s and the turmoil and conflict, if not chaos, that came to consume the discipline in the 1960s and 1970s?

Working with My Brother

Sometime in 1961, a number of lingering and some new things converged. The first was my ongoing relationship with William Gum, editor of John Wiley and Sons, who had befriended me as a promising author. The second was my long-standing interest in the articulations between sociology and psychology, inherited from my years in Social Relations at Harvard. The third was my reunion with my brother Bill as resident in Berkeley. He was a practicing clinical psychologist, a researcher in the University’s Institute of Human Development, and later a colleague who taught courses on child development in Berkeley’s School of Social Welfare. I had asked myself periodically whether we could collaborate on some scholarly enterprise. All these currents came together in one idea: why couldn’t we bring together, as systematically as possible, the scholarly literature on the articulation between sociological and psychological levels of analysis?

I approached Wiley with the idea and suggested a title, Personality and Social Systems. They liked the idea, and supposed that it was a good candidate for supplementary reading in large courses in social psychology. Bill and I forthwith went to work on the organization of the book, thinking about drafting an organizing framework, and beginning the needle-in-the-haystack search for what we might want to include (the literature on the topic we had proposed for the reader was vast and scattered). In general, Bill and I worked well together, capitalizing on a lifetime of closeness. We developed a division of labor in our searches. That made sense in that each of us was better prepared in different branches of the social and psychological literature. One feature of our collaboration, however, troubled me. Without planning or anticipating, it became clear that I was coming to exercise a leadership role, not so much in the detective work of locating and evaluating articles, but rather in organizing the intellectual

framework. I was responsible for formulating and drafting the “systems” approach to articulating the social and psychological materials (another Parsonian element that was still very much with me). I actually drafted most of the Introduction, Bill acting as reactor and editor. Neither of us acknowledged this emerging pattern but we both fell into it. What made me uncomfortable is that it violated two childhood-conditioned patterns. The first was that as younger brother I was the lieutenant and he was the captain, and the second was my family’s dogma that “the boys” were equal if different in their individual virtues. I cannot speak for Bill’s experience in the collaboration, but I found it mainly one of gratification to be thus rejoined with my brother combined with this abiding uneasiness over an apparent reversal of our childhood roles.

Wiley promoted the book (Smelser and Smelser, 1963) well. One of the ads, somewhat sensational and perhaps silly, read: “Smelser and Smelser—That’s Good News!” Bill and I thought this hilarious, and it became a fixture of family humor that we trotted out whenever anything good happened to either of our families. The book was well enough received and sold well enough so that Wiley asked for a second, revised edition (Smelser and Smelser, 1970). It is impossible for an author or an editor to evaluate, objectively, the quality and immediate and longer-term impact of his or her labors. But I regarded the organizing framework (personality and social systems, their articulations, and resulting outcomes) to be very fruitful (more so than the more ad hoc frameworks that Lipset and I employed). Finally, the work foreshadowed the efflorescence of the “micro-macro link” literature that became so visible in the 1980s and later.

Editing the American Sociological Review

One day in late 1961, I received a phone call from Parsons. It was totally unexpected. He was serving as Secretary of the American Sociological Association at the time, and in that capacity worked closely with the Association’s publication committee. He was calling to offer me the editorship-in-chief of the American Sociological Review, the official general journal of the Association and—in close competition with the American Journal of Sociology—the most prestigious journal publication in the profession. Though the offer was an official one from the publications committee, I learned that it was Parsons who brought my name forward and recommended me

strongly. Accepting meant a three-year term (1962-65) of editorial responsibility for publishing six issues per year, coordinating the review of manuscripts by an editorial board of approximately 20 sociologists around the country, making final editorial decisions, communicating with authors, and organizing the contents of each issue. The workload was heavy—processing approximately 300 manuscripts per year. The Editor was unpaid at the time, though the Association provided for a deputy editor, a secretary, and costs of editorial preparation and publication. The home sociology department of the Editor supplied an office and some relief from teaching (one course per year, as it turned out at Berkeley).

I was not only surprised by Parsons' call but also completely perplexed. The editorship, by tradition, was given to a seasoned, knowledgeable sociologist, hopefully broad in orientation. By contrast, I was a youngster (age 31 at the time of the invitation) barely beginning his academic career. It is true that I had some editorial experience. My journalistic background was relevant, though the publication committee surely was not aware of that. More directly, I was in my second year of service on the editorial board of the American Journal of Sociology, which was directly relevant. And, as detailed above, I had been Parsons' rewrite-man and editor, as well as co-author, for Economy and Society.

More fundamentally, I wondered why Parsons would be pressing me for the job—wonder accompanied by a tinge of paranoia. Parsons and I both knew perfectly well that by far the most important factor in the fate of a new, young faculty member is originality, creativity, and rigor in scholarship. Writing texts, editorial work, and service to the profession were important supplements in career assessments but definitely secondary. The Berkeley campus knew this as well, as manifested by its explicit policy of minimizing administrative work and Academic Senate assignments for its assistant professors, thus giving them time and room to maximize their scholarly work. Was not the assumption of a really major editorship, even the plum of editorships, a possibly harmful diversion from this priority? And if Parsons knew this, why was he recommending that I deliberately take it on? (That was the paranoid bit.) On the other hand, I had already been promoted to tenure at Berkeley, so there was no risk of a career

disaster of tenure denial. Also important for me, I had just finished Theory of Collective Behavior and The Sociology of Economic Life, major scholarly works in my mind.

Weighed down by these questions and doubts, I did not decide right away. Nor did I consult with anyone else about the decision. After a brief season of stewing, I decided to accept it, in full realization of the imagined risks, and frankly regarding that commitment as a matter of “for better or for worse.” Having made it, however, I did not hesitate further. I immediately scheduled a trip to Oregon to consult with the outgoing editor, Harry Alpert, and later discussed the editorship with Charles Page, an earlier editor. (Page, a man of superb humor, in greeting me, offered his congratulations and remarked, “There’s nothing as great as being an ex-editor!”). I immediately took first steps in negotiating details of my appointment with the American Sociological Association. Finally, I recruited my colleague, David Matza, as book review editor. (At the time, the ASR was a book-review journal as well as outlet for articles. (Contemporary Sociology had not yet been founded.) Subsequently I had a beautiful educational collaboration and friendship with him, damaged, however, in the subsequent period of political turmoil when he moved radically leftward and I did not.

The expectations for an editor of the American Sociological Review were clear enough, but there were also some ambiguities. On the one hand, it is essential that he or she be objective, honest, fair, and committed to the canons of social-science scholarship. Also, because the journal is an official organ of the field, the editor should strive for “balance” and “representation” of different subfields and emphases in the discipline. An editor should not make the position into a vehicle for advancing his or her disciplinary or professional preferences. Frankly “commissioned” articles—that is, inviting submission and more or less guaranteeing publication—are taboo. At the same time, some room is left for editorial maneuvering, following exciting themes, and taking a few bets on publishing articles known to be controversial. This whole complex of expectations is a formulation of “flexibility within constraints,” with a dose of ambiguity in specifying either of these.

In dealing with this range of policies and possibilities, I think I tried not only to live up to the general canons but also to stress innovative and imaginative directions (as I read them) more than most editors. Even before assuming editorial responsibilities, I

decided that I would “group” a number of—say, three—articles under a categorical heading, thus creating a “cluster” of contributions to a given topic or sub-field. Past practice had been simply to include nine or ten articles unconnected with one another in any individual issue of the Review. Examples of my headings in the first year were “Ecology-Demography,” “The Primary Group,” “Comparative Analysis” and “Foundations of Theory.” Adopting this policy meant that I had to hustle some authors in their revisions and disappoint others by delaying their publications, in order to guarantee that I would have a proper “cluster.” Almost all authors, glad to be published at all, did not complain about this manipulation of scheduling. A second innovation on my part, consciously made, was that I would publish imaginative or generally excellent articles on the fringes of the field, knowing that this might draw fire. Two examples of this were a major (and page-consuming) article on the psychoanalytic interpretation of group life, and a number of articles on social evolution, scarcely a mainstream topic in the field. I did receive some congratulations for exercising my imagination, but probably more groushings about my apparent sacrifice of worthier, core contributions by choosing these more marginal, “non-sociological” pieces.

At the time of my appointment, the general expectations regarding the editor’s performance emphasized his or her freedom and flexibility. I was never asked by any Association committee about my editorial expectations or “policies.” The whole tone was one of trust—“take the ball and run with it.” During my entire three years as editor, I was never interviewed, questioned, or blamed in any official way, indeed in any way whatsoever. I did hear some grumbling about some of my apparent policies and about the publication of particular articles, but these came from unhappy individual sociologists, not official sources. I thus consider myself blessed as an editor, whose main hope is always for freedom and discretion. In subsequent decades, particularly the 1990s, the American Sociological Review became something of a political football, with different groups in the association complaining of “bias,” “unrepresentativeness,” “establishmentarianism,” and “lack of diversity.” Furthermore, some called for interviews-in-advance of potential editors as to their definition of sociology and their priorities.

To stress these aspects of the editorship, however, is not to deny that the position requires a great deal of tact and diplomacy. (My seventh-grade teacher's comment that I should become a diplomat often came wistfully to mind during those three years.) Of course I had to maintain good relations with my stable of Editorial Board members, who naturally wanted me to take their opinions seriously, even when they were, in my mind, off the mark or otherwise unhelpful. My most important constituency, however, was those aspiring authors whose manuscripts were rejected. Year by year, the rejection rate of articles submitted was about 80 per cent. I joked frequently that an editor of ASR was guaranteed from the beginning to have 80 per cent enemies among his or her potential authors. How to communicate the fact of rejection to authors was a very major challenge. Of course, the brutal fact of rejection itself was the main message, and that was wounding, often enraging, and not easily softened. However, I did everything I could to ease the blow, without, however, slipping into dishonesty. I always tried to include associate editors' positive comments in a letter of rejection, whenever I could find them. When possible and without being dishonest, I would also mention "categorical" reasons for rejection—for example, that the manuscript was a generally a strong one but would find a more comfortable place in a more specialized journal, such as Journal of Marriage and the Family, or Sociometry. In general, I was very attentive to rejectees' imagined feelings, and became a kind of virtuoso in writing rejection letters. (In a moment of lightness I invented "the perfect letter of rejection," which read as follows: "We find your contribution to be so brilliant that to publish it would demean the pages of our journal." I dreamed about sending but never sent such a letter.) I learned from an informal source, after my editorship was over, that three rejected authors had submitted formal complaints about me to the Committee on Ethics of the American Sociological Association. The Committee never contacted me about these complaints, apparently feeling that they were not sufficiently serious. My source revealed to me that one of them was a charge of bias: that I rejected a manuscript on political grounds (in this case, my supposed anti-Cuban sentiments). A second complaint was that, as a matter of editorial laziness, I was delegating final editorial decisions to my deputy editor, an accusation absurd on its face. I do not know about the third complaint. I consider myself

fortunate not to have received more. Perhaps my efforts at diplomacy had a little to do with this result, but I will never know.

I mention, finally, one additional bit of unanticipated diplomacy. My appointment as editor was obviously noticed by my Berkeley colleagues. One aspect of that notice, moreover, was that there was now, in their midst as it were, a prestigious outlet for publication, at whose helm was an immediate colleague. This did not result in a flood of manuscripts for me, but two of my senior colleagues (who had hired me not too long ago) presented me with manuscripts that had been written but not yet published. It so happened that these two were Herbert Blumer and Kingsley Davis, the two senior members of the department with whom my personal relations were coolest. That circumstance didn't help matters. They exercised no pressure on me but the collegial dimension of these submissions was lurking on the sidelines. I was especially careful to have these manuscripts reviewed correctly by my editorial board. As it turned out, neither one received very strong reviews, and I felt that those reviews were on target. So I informed each of them, separately, that he had not been reviewed strongly enough to merit publication. This was a very difficult thing for me to do, and they did not help matters by communicating to me directly that I was a fool. But what could I do? I stuck by my decision, uncomfortable as it was. I suppose matters were somewhat relieved when they found other places to publish their manuscripts, but I confess that these were very difficult situations for me. My consolation was that I had behaved in a professionally correct way, personally uncomfortable as it was to do so.

As fraught as my editorship was with doubts about whether it was the right thing for me to do, and as demanding as it was on my time and my diplomacy, it turned out to be a very satisfying and profitable experience. Also, with the great flow of manuscripts that I had to process responsibly, I learned an enormous amount about the scholarly activity, useful and not-so-useful, going on in my discipline. It was also a three-year experience in honing my editorial judgment, and proved valuable in the dozens of future editorial assignments that I undertook. All my advance frettings about how taking on that assignment might impede my career as sociological scholar vanished like so many phantoms during and after that momentous editorial experience. I remember that era with few and minor misgivings and many major satisfactions.

Stirrings of Citizenship

In many respects my first half-dozen years on the Berkeley scene read like a familiar academic script: a young, ambitious academic establishing himself in the world of important scholarly research. Accordingly, my two most meaningful reference groups were my colleagues in Berkeley's department and the national and international communities of sociologists. The fact that the larger Berkeley campus was generally congenial and supportive—and it was very much so—was important but not something I thought about very much. I suppose I could say that I liked but did not yet love my university. With a couple of exceptions to be noted presently, I was uninvolved and generally unaware of the life and concerns of the world of foundations and federal government agencies.

My Department and Campus. According to script for a young faculty member, I was basically left alone. I was expected to attend departmental faculty meetings and vote responsibly, which I did. I remember only very light committee assignments, mainly a term of service on the committee on graduate admissions. Likewise, I was left alone by the leadership of Berkeley's Academic Senate—of which I was automatically a member because I was a ladder faculty member—serving on no committees whatsoever until more than a decade after I joined the faculty. The one exception to this script was my appointment for a couple of years to the Chancellor's Committee on Discrimination. Today this would be a role of major consequence, but in and around 1960 we were still far away from the world of affirmative action, and the topics of race and racial discrimination were not very salient. We investigated various campus problems and issues—for example, discrimination in fraternities and sororities, campus admissions policies, to some degree the social life of minorities—but all this was relatively low-key. We experienced some difficulty in our work because of lack of information; color-blind admissions policies left the campus not knowing who their minority students were, much less what their lives were like. We had to rely on other sources, such as campus surveys by the Survey Research Center, for the most basic information.

The Publishing World. My main relationship with publishers in these years was that of was of a young scholar pursuing opportunities for publication and publishers pursuing authors and editors. Beginning in 1961, however, I took on a major advising

role. In particular, Prentice-Hall had long retained “series editors,” that is, scholars in different disciplines who approved all books on their respective disciplinary “lists.” Herbert Blumer had served Prentice-Hall in this capacity for decades. When he stepped down, the publisher approached me to take his place. I accepted the position out of a mix of motives. It was a position of service to the profession and one of some influence; it was a source of my continuing education in the social sciences; and it was a significant source of supplementary income. I received a small royalty on every volume in “my” series. I remained in that role for nearly two decades, at which time the publisher asked me to resign, explaining frankly that I was costing them too much money in royalties. I told them that this was the only time in my life I had been fired.

The American Sociological Association. One of the “accidental” accompaniments of my ASR editorship was to be seated on the ASA Council (governing board) during my editorial years, 1962-65. At the time the Editor was automatically a member; this policy was subsequently discontinued. I was without any prior experience in the Association. As it happened, the ASA was engaged in very important matters at the time, especially the issue of whether to locate the national headquarters permanently in Washington, DC, where more and more of the discipline-relevant “action” was coming to be centered. As a result, my socialization into Association affairs (and service to the profession) was rapid and mandatory. It also set the stage for heavier future involvement, including three additional three-year terms on the Council.

Social Science Research Council. Even before arriving in Berkeley, I was invited to join the Committee on Economic Growth of the SSRC, an interdisciplinary group dedicated to advancing the committee’s work through projects and conferences. I know that the invitation came at the urging of Wilbert Moore, a sociologist from Princeton and a member of the Committee. He had become acquainted with my research on the British industrial revolution, and we shared broader interests in economic sociology and social change. (Moore also served as guardian angel and sponsor on several future occasions.) The committee was a formidable one intellectually. It was chaired by Simon Kuznets, the future Nobel Prize winner, with other eminent members such as Melville Herskovitz, Bert F. Hoselitz, and Joseph Spengler. I joined that group fully for several years, continuing my own interests in economic and social development. Moore invited me to

participate in an SSRC conference on industrialization and labor. He asked for a theoretical article on development, and I took the occasion to generalize from the framework that informed my thesis. I developed a model that regarded modernization as a contrapuntal dance among differentiation of structures, social disturbances, and re-integration. This article was reprinted many times, and on the basis of it I was cited as one of the theorists of modernization (see Smelser, 1963). I helped arrange some other conferences, including a major enterprise focusing on issues of social stratification and social mobility in economic development, which I organized with Marty Lipset (Smelser and Lipset, 1966). All this work predated a much more important role in the SSRC as well as several other foundations in future years.

Looking Backward and Forward. Summarizing these early years as a young professional, I regard them as unspectacular (even if frantic) in one sense: I was following aggressively a career line that was predictable in a known academic environment. As of 1964 and in the decade following, that career was shaken dramatically by personal, political, institutional, and academic turmoil. Above all it meant a diversion and sometimes interruption of my academic career, a redirection of research interests, an upward and outward bound in my citizenship roles, and a season of premature statesmanship in my professional life.

Chapter 5

Rupture, Recovery, Broadening, 1964-73

In reading the foregoing recollections on my first six years at Berkeley—which is as balanced an account as I can produce—one might conclude that those years were a golden age for me as well as my department and university. By many measures they were—personal career advancement, intellectual realization, success in undergraduate teaching, and collegial and university recognition. But both readers and I should be reminded that no phase of life is fully golden or fully dismal. I will mention some of darker sides as I move into the period that followed. In all events, that “age” came to an abrupt halt for me in the fall and winter of 1964, as I witnessed the mix of crumbling, conflict, and disarray in my personal life, my department, my university, and to some extent my profession. That decade could be labeled “chaotic” by all those measures, but they were also years of adaptation, expansion, new involvements, premature statesmanship, and new kinds of personal gratification.

A Psychoanalytic Decade

My intellectual interest in psychoanalysis traces to my Harvard years. While no single “culture” dominated that complex university, the psychoanalytic outlook was a very conspicuous component. It entered into the intellectual interpretations of historians and other humanists. It was fully evident in the Department of Social Relations in the persons of Henry Murray and Robert W. White, Clyde Kluckhohn, and Talcott Parsons and in the decisions of many of my mentors to seek the psychoanalytic training that was available through the Boston Psychoanalytic Institute at the time. That culture also infused cocktail-party talk, conversations, gossip, and jokes. My interest was further kindled in correspondence and conversations with my brother Bill, who was in training as a clinical psychologist during my Harvard years. Though I made no decisions about my further involvement, it can be said that by the time I left Harvard I had decided that I would take up psychoanalytic training sometime, somewhere in the future, but this decision remained latent and I had no timetable in mind.

The timetable was thrust upon my in 1963-64 by the disastrous ending of my first marriage. I remind readers that I am writing this book, by conscious intent, as an intellectual autobiography, and I have included personal aspects mainly as they have

affected that main theme. I should say, however, that that marriage began a downward cascade around 1960 and ended in separation in 1963, leaving me dispirited and depressed, haunted by a sense of personal failure, consumed with anxiety and care about my son (age 5 at the time of separation) and my daughter (age 3), and fully committed to continuing my love and care for them after the trauma of separation and divorce. As readers will discern from the preceding chapter, my intellectual and professional life did not collapse during those turbulent few years, though I am not certain why it did not.

A few months before separating, I applied for admission to the San Francisco Psychoanalytic Institute as a research training candidate, was interviewed, was accepted, and began my personal analysis. Of course my background exposure and lingering commitment to depth psychology played a role in my decision, and I chose the research-training candidacy rather than therapy alone because it was immersed in an academic program at the Institute. Ninety percent of the driving force, however, was my own despair and sense of personal failure.

Along with this need for help and a long-standing intellectual interest in depth psychology, I brought another orientation to the coming experience. I knew a great deal of the training was going to be dedicated to mastering the field of psychoanalysis as an intellectual system and a therapeutic practice. I also knew—or had heard or had imagined that I heard—that many of the psychoanalytic institutes, including San Francisco, were orthodox Freudian in their bent. I expected to find this, and I expected to resist it. It would be going too far to say that I was defiant or itching for a fight, but that element of ambivalence was there. In fact I turned out to be a good and non-threatening citizen of the Institute. Inwardly, however, I kept a spirit of independence, of selecting from different approaches and “schools” and refusing to be enslaved by any one of them.

At that time, recent changes in policies of the American Psychoanalytic Association permitted candidates without an M.D. to receive a full training (personal analysis, an intensive seminar program, supervised analysis of others, and graduation). The only restriction I had to accept was not to practice psychoanalysis after graduation. Even that prohibition was rescinded before I finished training in 1972, and at the time of my graduation most of my Institute teachers and colleagues were encouraging me to take up a practice. In 1979 the California legislature licensed non-medical graduates of

recognized psychoanalytic institutes to practice psychotherapy in that state. My training itself was an eight-year, no-turning-back commitment of 12-15 hours per week, a really major investment of time, intellect, and emotion. With all that, the whole journey was very rewarding in the end. I had a superb personal analyst, Stanley Goodman, and I profited greatly from the four-and-one-half years in analysis. I worked my way through the painful crisis of divorce. The analysis also enabled me to enter a second, happy, and permanent marriage in 1967, and it improved almost all my interpersonal relations with authorities, colleagues, students, and friends.

My seminars in the Institute were always with four other analysts-in-training admitted in my “class.” They were all psychiatrists with medical training and in full-time practice. All were extremely intelligent and talented, and we formed a solidary band. We read almost all of Freud’s work, as well as a great deal of contemporary psychoanalytic writing, and participated in many clinical case-study seminars. As the only non-physician in the group, I expected some marginalization. But as it worked out, my situation was quite the opposite. By virtue of my academic training and research, I was more advantaged in general scientific thinking. By virtue of their medical training, my compatriots had become wedded to the complex of diagnosis and clinical treatment of a single case. In fact, the whole culture was thinking about the “clinical case” and our seminars were forever being drawn in that direction. I myself was seduced into that magical mode of thinking as well, much as one might be seduced into a gripping novel or drama. But in addition, I had a philosophical, theoretical, and methodological advantage.

Despite the consuming nature of my participation in the Institute, the experience had a strange marginality about it. When I entered the program I had almost no support from my colleagues in sociology, some of whom thought me some kind of fool for taking on such a major diversion from my academic career. This set up a kind of permanent double-life for me. I made many new friends and joined several social circles in the psychoanalytic world and continued a professional and social life in the university, but with few exceptions these worlds remained separate. As I will iterate later, I made several intellectual efforts to build bridges between these two worlds, but there were not many social connections. A few psychoanalysts took an interest in my academic work,

and I later developed some relations with a group of psychoanalytically-minded university colleagues and students, but the rule of separation generally applied.

Because the prohibitions on practicing psychoanalysis had virtually ended by the time I graduated in 1972, the thought of a career-change and movement into psychoanalytic practice became an issue. I had plenty of experience as a therapist in those psychoanalytic years, first in clinical practice at Cowell hospital on the Berkeley campus—as “apprenticeship” required by the Institute. necessary before treating patients psychoanalytically—and a number of low-fee patients through the Institute in my later years as a trainee. I have to say that resigning from the University and taking up full-time analytic practice never really occurred to me as a serious possibility—my academic commitments were so deep that, in the end, that I couldn’t comfortably leave my academic home. (The same commitments kept me from assuming full-time administrative positions as well, as I will detail later). In fact, I also decided to shun part-time analytic practice with only a few patients, mainly because by that time my life was so taken up with professional travel that it would seriously disrupt therapeutic relationships. As a lesser compromise, I did re-join the Cowell staff for a time, and, later served for ten years as supervisor of clinical-psychology trainees on the Berkeley campus. These minimal, somewhat marginal attachments with therapeutic and supervising enterprises should not conceal that I found every aspect of those attachments personally rewarding, continuously involving me in the private dimensions of human existence that are obscured in one’s routine involvements and day-by-day copings in a large organization.

A second issue concerned how much I should try to integrate sociological and psychoanalytic themes into my academic research. Even before entering the analytic world, I was keenly aware of the substantial theoretical, substantive, and methodological difficulties in attempting such a marriage. In a way the two enterprises were fundamentally separate from one another from the standpoints of basic units of analysis, imagery of human nature, and the cultural “mind sets” of sociologists and psychoanalysts. I did undertake three writing exercises in the 1960s, however, which I will describe later in the chapter. Serious integrative work, however, began only in late 1970s in a collaboration with Erik Erikson, and culminated in my presidential address to

the American Sociological Association in 1997 (Smelser, 1998) and the publication of a book of essays on psychoanalysis and sociology (Smelser, 1999). All this will also be traced later.

The Free Speech Movement and Its Aftermath

The Berkeley campus has had a long history of political activism, and is regarded generally as a historical “leader” in that regard, along with other institutions such as the University of Wisconsin, Reed College, City College of New York, and, more recently, Brandeis University. This tradition was solidified by the loyalty oath controversy in the era of McCarthyism, and student political activism from the left accelerated in the early 1960s (Heirich and Kaplan, 1965). With hindsight, one can observe that administrators on the California campus should have paid more attention to this upswing than they did. In any event, in October 1964, in the midst of the Johnson-Goldwater presidential race, the Berkeley administration committed one of the greatest political blunders in the history of the university. It revoked certain political activities, including fund-raising, on a narrow strip of land between Bancroft Avenue and the campus proper. Such an act—taking away a long-established privilege that had evolved into a “right” on the part of those exercising it—is or should be listed in all handbooks on revolutions as one of the surest routes to radicalization. In this case it certainly had that effect. In the remainder of the fall and early winter, the campus experienced recurrent episodes of mass protest and administrative vacillation between punitiveness and collapse into weakness. A massive sit-in occurred in Sproul Hall, the main administrative building, on December 2, and on December 8 the faculty voted overwhelmingly in favor of relaxing restrictions on political activity (thus rebuking the administration). On January 2, 1965 the Chancellor, Edward Strong, decisively discredited, took leave from office, throwing the campus into further political disarray. The following day the Board of Regents appointed Martin Meyerson, Dean the School of Environmental Design, as Acting Chancellor.

On January 5 Meyerson summoned me into his office and asked me to take on a new position: Assistant to the Chancellor for Student Political Activity. I cannot recall another moment in my life when I was more stunned. I had absolutely no idea why he had put the finger on me. I had not been visibly partisan or active in the past four months (perhaps that was an asset, being “not known”). I was told that Marty Lipset, a confidant

of Meyerson, recommended me, but I never verified that information one way or the other. I was already known for my work on collective behavior (one headline, reporting my appointment, read “Meyerson Picks Expert on Mobs”), but I do not know if that had any significance, either. In all events, in an act of foolhardiness I accepted immediately. The ensuing eight months (Meyerson’s term, as it turned out) radically altered my future in ways I could not have anticipated.

In recent years I have written up those months from an institutional-biographical point of view (Smelser, 2010a), and it would be repetitive to reproduce that chaotic history here. I will limit my account to conveying a general flavor of the period, how my own predilections “meshed” with the experience, and, more speculatively, how it echoed into future directions as a part of my personal and administrative style.

The first thing to be noted is that those months in early 1965, though they did not match the Free Speech Movement days in 1964 in drama, were months of institutional electricity. They brought no diminution of bombardment, conflict, and uncertainty for the Berkeley administration. I rode herd over a campaign to revoke charges against the December sit-in arrestees in January-February, the “Filthy Speech Movement” in March and April (in which a sub-wing of the FSM made an acting-out attempt to assimilate free speech to public expressions of obscenity), and the dramatic first stages of the national protest against American military involvement in the Vietnam war. We were also under constant bombardment from the right: discontented Regents, conservative political spokespersons, and angry citizens. I was the lightning rod for that pressure as well.

In one sense, it was an ill-advised choice for me to accept Meyerson’s invitation. It created an almost unmanageable pressure on my time and energy. The job itself called on me to be available for meetings with angry constituents, especially student activists, to attend all critical meetings of the Chancellor’s administration, and to be on call when Martin needed me. One can set no time limits or schedule on that range of emergency activities in tense political times. On top of that, I taught my undergraduate theory course that spring (though was given relief from teaching the graduate theory course). I was also in my final semester as editor of the American Sociological Review. I had begun my personal psychoanalysis, one hour a day four days a week. And I was faithfully

committed to pick up my two young children and care for them every Wednesday afternoon and every weekend. I cannot imagine today how I managed.

The assignment also carried a certain career risk. From the beginning of campus turmoil in October 1964, I had witnessed the scolding of a number of administrators and faculty—with their careers marred—for taking the wrong political stand at the wrong time. Joining the Chancellor's staff in this role certainly exposed me to the same danger. I cannot say why, but in accepting the assignment I had no apprehensions on that score. This false confidence on my part was clearly unrealistic, but it probably served me well. As it turned out, I was publicly criticized only once, and that was in a letter by some sociology graduate students to the Daily Californian, criticizing me for an ill-advised venture I took on Sproul Plaza to try to convince sellers of an obscure, obscene publication called Spider to leave the campus. As I recall that incident, my heart wasn't in it, and I did it because Meyerson, embarrassed by the presence of the publication on campus, asked me to do so. The main effect of my effort was to be shouted down by a spontaneous crowd of activists for being a bureaucrat meddling their rights to free expression. That incident and the graduate students' criticisms disappeared from view and memory in a matter of days.

There was another risk of which I was unaware at the time. Alex Sherriffs, a psychologist at Berkeley, had served as Vice-Chancellor for Student Affairs for several years preceding the Free Speech movement, and continued through the crises of Fall and Winter, 1964. Though discredited, he continued to hold the Vice-Chancellorship when I was Meyerson's assistant. In the past few years Sherriffs had drifted to the right, regarding student activists alternatively as confused adolescents or tools of the Communist Party. He played a very repressive role in the FSM days. By the spring of 1965, however, he was isolated. Our relationship was distinctly cold (though civil) because I was in effect replacing him. What I did not know was that my secretary in Meyerson's office was having a love affair with Sherriffs, himself divorced. I learned about this only months after I began working for Meyerson. This was of course most threatening, because he knew about everything I did or said in the office in a matter of hours. What I didn't know is that Sherriffs had identified me as some kind of security risk to the FBI, for whom he was an informant. Among other things he reported was that I

was on a scrub basketball team at the time, and played with Leon Wofsy, the former communist now on the faculty! Apparently the FBI did not find out enough of interest about me to conduct a further investigation (Rosenfeld, 2012),

During those months in the Chancellor's office I experienced many moments of anxiety, as one would expect in an atmosphere of continuous crisis. I was treated abusively by individuals and delegations that came to me to express outrage, to threaten some embarrassing or disruptive action against the campus, or to make both legitimate and illegitimate requests. At the same time I found that the role that I came to play was oddly ego-syntonic. Here I was, simultaneously both above the fray and a man in the middle, remaining sane while others were losing their heads. As a matter of personal political attitude, I had found myself sympathetic to the student demands for greater freedom for political advocacy, but ambivalent in my feelings about their ideological excesses, as well as those of groups on the right, who attacked both activists for their activism and the University for catering to the activists. In these months my very strong loyalty, even love, for the University crystallized and has remained steadfast to this day. This love is not the same as the sentimental Old-Blue variety, but arises from a conviction that the University is a treasured institution, endowed with an invaluable role in a civilized society that is sacred and must be protected. At still another level, the role appealed to me as a peacemaker, bringing alive my diplomatic streak and solidifying skills that are assets in leadership generally.

This period in the Chancellor's office, finally, was a situation to which my recent academic research had direct relevance. My situation called for "applied social science" though in an indirect and restricted sense. In studying an endless variety of social movements, I had learned several lessons. One is that after a spectacular success, a movement often finds itself floundering, internally divided, and seeking new causes. I observed this directly with the Free Speech Movement in the spring of 1965. I also learned that one of the most fatal lines of behavior on the part of authorities in the face of demands is to vacillate between repressiveness and permissiveness, thus both victimizing and emboldening a movement. Finally, I came to appreciate the advisability of patient listening on the part of authorities, rather than duking it out with antagonists. I did not

consciously attempt to manipulate on the basis of these kinds of knowledge, but they provided me with a background of sanity and patience.

As might be expected, my maiden experience in the Chancellor's Office put me actively in the market for administrative positions, especially in the years immediately following, because the Berkeley campus and others were embroiled in more or less continuous bombardment, directly and indirectly, from the anti-war movement. Before my time with Meyerson was even completed, I received a feeler-offer to be the Executive Vice-Chancellor at the UC Santa Cruz campus. A couple of years later I was asked to be Dean of Letters and Sciences at Berkeley. I turned both down, knowing that both would surely entail deserting my life's love of academic research and teaching. As it turned out, however, from 1966 through 1972 I remained heavily involved in both formal and informal administrative roles associated with the university's political turmoil. I remark on these now, reserving until later changes in my organizational roles external to the university.

By the summer of 1965 my adventure with Myerson ended. He was not appointed Chancellor after his period as Acting Chancellor, and in any event I had a scheduled sabbatical leave that I was determined not to postpone. I did remain in Berkeley for 1965-66, mainly to continue to be near my children, and returned to on-hold research projects. However, being near at hand, I was periodically drawn into the Chancellor's office for advice during periods of urgency. I continued to be involved informally—though officially on leave—in urgent situations in a now-badly-split department of sociology. Most important, not many months after the new Chancellor, Roger Heyns, was appointed, he called me into his office with an offer and an appeal to join his administration in a new, formal administrative position, Assistant Chancellor for Educational Development. I possibly made a mistake in accepting it, for reasons I will now develop.

A recurrent but not the most salient theme in the student activist movement in the 1960s dealt with the quality of collegiate education. One concern was that classes had become large, impersonal, and ill-taught, as faculty were deserting teaching for research and the other demands of Clark Kerr's "multiversity" (Kerr, 1963). The anti-bureaucratic mantra was "I am a student; do not fold, spindle, or mutilate." Accordingly,

demands for smaller classes, including tutorials, were heard. More frankly political were attacks on the university for deserting its legitimate educational missions and “selling out” to a government that had become a war-machine. Acting Chancellor Meyerson had responded to these themes and created a special commission on educational reform, a faculty group that echoed much of the student rhetoric and called for bold reforms of university teaching. One of its recommendations in the commission report (Select Committee on Education, 1966) was to create special administrative machinery on the campus dedicated to educational reform.

The Heyns administration subsequently responded to the report by establishing a Board of Educational Development, a faculty body commissioned both to respond to others’ initiatives and to create unconventional and creative educational courses and curricula in keeping with the dissatisfactions and recommendations articulated by the Commission. As the administrative ingredient of this reform, the campus created a new Assistant Chancellor, who would sit on the BED and implement—through administrative action, recruitment, and budgeting—its decisions. That is the office that Heyns asked me to fill. I accepted, deciding in my own mind but not telling him that I would serve for a maximum of two years. In addition, the position included membership on the Chancellor’s staff, so that, though I was not in his informal “inner circle,” I was in on its decisions and was called into action when crisis situations broke out, which turned out to be very often, throughout Heyns’ entire term of office.

The BED lasted well beyond my two years of administrative service, but in my time at least, it had limited effectiveness. There were several reasons for this. First, being a part-time commitment on the part of its faculty members, it was mainly reactive to others’ proposals and initiated almost none of its own. On my own I undertook one administrative initiative and attempted to re-define “full time student” in order to facilitate the educational lives mainly of returning veterans and others, mainly women. I failed in this, largely through opposition of deans dedicated to existing rules governing full-time enrollment. I succeeded, however, in facilitating an educational program on religious studies that ultimately achieved coherence and permanent status. The main pattern, however, was that the BED mainly waited for, evaluated, and recommended on

others' initiatives, and then I would go to the relevant budgetary officials for funds to implement, if any were needed.

Second, as implied in these arrangements, the BED had no independent budget, but I had to clear each individual project with the campus budget office. I was generally successful in this, but I complained that I often felt like Nora in Ibsen's The Doll's House, coming to the powerful authority and begging for bits of spending money. With such arrangements, the impact of our limited, usually one-time innovations could not be very significant in the larger campus picture.

Third, the fact that the BED waited for outside initiatives guaranteed, at least indirectly, that its work would become politicized, along with so much else on the campus. Many of the projects moving through the Board had no special political agenda above and beyond improving instruction. Proposals to support group tutorials for writing instruction and course credit for fieldwork would be examples. Others, however, came from individuals and groups with larger social and political agendas on their minds. Most of these originated in the widespread opposition to the Vietnam war, and many were critical of American society on other grounds. In addition, numbers of proposals came from politically interested groups off-campus, with the added ingredient that they would be taught by individuals or groups not on the faculty and with no special academic qualifications. The numbers of such proposals were not the majority, but there were many. This practice tended to turn the BED into a censoring and policing agency, making judgments on whether a "course" was academic or propagandistic in character. Such judgments occasioned political debates and divisions on the Board. I became the major policeman, because several of the BED members were extremely permissive in their judgments of what should count as a course for credit. We did adopt a rule that instructors had to be faculty-approved, partially solving the problem of extra-campus "invasion" of the curriculum, but this did not solve the problem of politicization.

One course sanctioned by the Board exploded into conflict that spread well beyond the campus. In the late summer of 1968 the BED approved a course, called Social Analysis 139X. It was entitled "Dehumanization and Regeneration in the American Social Order," to be "supervised and taught" by two faculty members but featured ten lectures by Eldridge Cleaver, the Black Panther leader and candidate for President of the

United States for the Peace and Freedom Party. The action immediately provoked a denunciation of the course by the California State Superintendent of Public Instruction, and a clash over between Governor Ronald Reagan and Speaker of the House Jesse Unruh. The Regents ruled that the course could not be given for credit if Cleaver appeared more than once, an action that provoked cries of violation of academic freedom in the Berkeley Division of the Academic Senate. By chance this item appeared on the BED agenda just after I had finished my two years as Assistant Chancellor and was not involved in the decision. I have often wondered how I would have voted, and how I would have implemented or not implemented the vote. I will never know, but I think I would have voted against the proposal on grounds that it was not genuinely offered by faculty. After the episode died down, I sometimes jested to friends that the whole affair was a Western movie, and I had skipped town just in time.

The BED survived for a time after my departure, but its impact continued to be modest. In 1973 I wrote that “the movement for educational innovation must be regarded as a victim of politicization in large part; its impact on the entire campus was modest” (Smelser, 1973b, p. 71). A similar reservation can be made with respect to my own personal involvement. I felt I was part of a worthy but minor enterprise, one in which the campus did really have its heart. It was not the most gratifying chapter in my life’s work.

I will mention two final moments in this decade of crisis management on the campus. From 1970-72 I was member of the Policy Committee of the Berkeley Division of the Academic Senate (Chair, 1971-72). This committee was, in effect, the spokesbody for the Division on matters of campus-wide policy and politics. It was an outgrowth of the Emergency Executive Committee, formed during the heat of FSM crisis. Much of our work was not memorable, but I mention one especially meaningful situation for me. One of the last gasps of political activism on the Berkeley campus was the Reconstitution movement of 1971, a mobilization of faculty and students to “reconstitute” academic classes into political action groups that would go into the community and agitate against US military involvement in Southeast Asia. An incidental feature was that students would presumably not lose academic credit for this activity. That movement provoked Governor Reagan to close the campus for a day, and later to press for punitive budgetary measures against the University. The Policy Committee was naturally provoked into

action during this crisis, and I took the lead in urging and drafting a resolution that condemned the reconstitution movement for attempting to close the campus in the name of a political cause and condemned Governor Reagan for attempting to close the campus for purposes of political retribution. That resolution was the right one, I believe, and incidentally nestled into my now-solidified political self-image as a rational man of the middle, plague-on-both-your-houses leader.

After the reconstitution movement died, the campus still had to deal with “Reagan’s revenge” against the campus. The major response was for faculty to mobilize into a political organization that would give voice to faculty, mainly on budgetary issues. I, along with two other faculty members—David Feller in Law and Lloyd Ulman in Economics—took the lead in this movement, and fashioned a collective group. We named it the Faculty Association. It was a child of the Academic Senate, consisting of Senate members. We regarded it as a faculty voice, but short of a labor union in its activities. In fact the Academic Senate later voted not to involve it in collective bargaining. In consequence its activities drifted toward those of a political lobby; it established an office in Sacramento as a “voice” for the faculty. I served, off and on, on its Executive Board for nearly two decades. I will confess a certain ambivalence in this last role. On the positive side the political organization of the faculty into a quasi-professional, quasi-political body was consistent with my loyalty to the University and strong commitment to defending it from outside forces. On the other hand, for deep personal reasons I felt more comfortable in peace-making rather than fighting roles.

Teaching Graduate Sociology in the late 1960s and early 1970s

As of 1965, I ended my six-year stint of teaching Sociology 109, the required undergraduate theory course. Others picked up the teaching, and while they varied the content and emphases greatly, the format remained little changed from my model. Thereafter I continued as the department’s “theory person,” but switched to teaching the graduate theory course, Sociology 218, required for first-year students. I taught it almost every year I was not on leave or in some kind of university service that diminished my teaching load. It became a large course, up to 80 students, because graduate students from other departments attended (and I did nothing to discourage them) and our pre-orals

students in sociology sat in on it as a “refresher” course as they approached their orals examinations, one mandatory topic in which was sociological theory.

The late 1960s and the early 1970s were the years of intense student protest, and sociology students were among the most active. Some of the general protest, moreover, was aimed at the department and took the form of a generalized advocacy for making courses and education generally more “relevant”—mainly relevant to the social protest against societal injustices. There was also pressure for graduate students to become more centrally involved in the governance and policy-making of the department.

By and large the protest did not directly affect the teaching of specific courses in the classroom. Most students, including radical ones, continued their academic work on schedule. (That fact is consistent with my general interpretation of that activist period, namely that, except for a few, protesting students did little that would directly endanger their academic careers.) As a result, I mainly proceeded “normally” in offering my courses and the students proceeded “normally” in taking them. However, I mention two points that constitute exceptions to this general statement.

First, in my theory course, I continued to require readings and discussions of the works of Karl Marx. The activist students were most interested in that part of the course and became articulate and aggressive in discussion periods. One thing they did was to engage in incessant questions and point-making that would bring me to admit that I was sympathetic to this or that argument of Marx, or that I agreed with this point or that point. I found this irritating because I felt it an illegitimate politicization of the subject matter of the course. However, I always reacted with patience, defending my own interpretations, and never scolding the students for their partisan intrusiveness. In general, I found this feature to be disturbing, but I never experienced it as disruptive of the classroom in a strong sense. I never discouraged this feature, either; in some ways it made life in the classroom more interesting than if the students had been conformist and passive.

Second, I report one near “crisis.” This was in 1968-69, the year of the demand for an ethnic studies department, the Third World Liberation strike, and People’s Park episode—the most active year of protest, save for the Free Speech Movement of 1964-65. As background, the University of California had begun to admit larger numbers of minority students, and the sociology department was no exception. It was also in that

year that Arthur Stinchcombe and I were co-teaching a first-year graduate course that combined the graduate theory and methods requirements.

About two weeks into the course Stinchcombe and I were subjected to a militant demand on the part of about ten African American graduate students, who demanded that we change the locus of the course to an off-campus site. Their explanation was that, in the context of the Third World Liberation strike, they could not and would not cross the picket lines on Sproul Plaza. The demand had a menacing quality about it, though I did not feel personally threatened. They were willing to meet for negotiation in the Student Union, which was considered “liberated” territory. I agreed to a meeting with the agitating students; Stinchcombe was reluctant to join in on this and gave me freedom to represent him, so I went alone.

At that meeting the students militantly expressed their position and demands. I listened patiently for quite a while. I then explained that they were only ten in number, that there were 40 other students in the class that were not making the demand to move it, and that I was reluctant to coerce that majority. Then, in a spontaneous move, I made the suggestion that Stinchcombe and I hold the class with them separately at an off-campus site. I cannot explain why that idea occurred to me at that moment. It took the students completely off-guard, and after some moments of confusion, they said they could not respond to that offer, but had to “caucus.” They also agreed to send a couple of their representatives that evening to tell me the result. I agreed to meet in my office in Barrows Hall at 8 o’clock. At that meeting the students said they would accept my offer. I knew by that time that Stinchcombe was willing.

A menacing incident occurred shortly thereafter. I had finished work in my office, so I left the building with the students. As we were walking out of the building (between Barrows Hall and Sproul Hall), two campus police officers leapt from the shadows and confronted us, with guns drawn. Actually, it wasn’t “us” but it was the two black students they were confronting. I was very frightened, but managed to say that these were two of my students and that we were coming from an academic meeting. The police backed off immediately, but the incident told me everything about the dreadful atmosphere on campus at that moment.

So Stinchcombe and I taught the same course two times that semester, as it were, once at the scheduled time and place and once to the minority students at the Institute of Industrial Relations off campus. Things went OK, but I remember that Art and I had to spend quite a bit of time and effort in keeping the course “academic” and not joining into passionate discussions of the strike and the critical campus situation. As the course was nearing its end, a delegation of the minority students came to my office and demanded, militantly, that I give all of them “incomplete” (E) grades in the course, because they had been so deeply involved in the strike activities that they hadn’t and wouldn’t have time to read the materials and take the final exam. I responded by saying I wouldn’t do this, but I would postpone the final for two weeks, and that would still allow us to get the grades in on time. The students accepted this. However, two days before the delayed exam, two of them came to me and very belligerently announced that they were not coming to the exam and demanded that I give them an E grade. I felt physically intimidated at that moment, but something possessed me to say, “OK, you don’t have to show up, but if you don’t, you may expect an F for the course.” They caved, and showed up for the exam. The whole series of episodes was very scary for me, though I was helped a little when, six months later, one of the minority students came to my office and thanked me for the way I had handled the whole situation.

The Crumbling of the Sociology Department

Though conflict was latent in the Sociology Department in its “golden age,” the political turbulence of the late 1960s (the Free Speech Movement and the subsequent anti-war protest, the move to establish racial-ethnic studies in the university, and People’s Park) shattered its equilibrium. The Department became a deeply divided arena for political-academic conflict for at least two decades. The bases for conflict were overlapping: ideological “schools” of sociology; “scientific sociology” versus “softer” methodological approaches; aggressive implementation of affirmative action versus maintenance of “standards”; and involving graduate students in departmental governance. In principle these are separable issues, but during those years the department became severely polarized. That meant that whichever of these dimensions were salient at any given moment, the same groups of faculty members squared off against one another. I simplify things, but there was a “left” group that included Robert Blauner, David Matza,

William Kornhauser, and Troy Duster; a “middle” group that included Charles Glock, John Clausen, Leo Lowenthal, Robert Bellah, Harold Wilensky, Reinhard Bendix, Wolfram Eberhart, Franz Schurman, Seymour Martin Lipset, Nathan Glazer, Philip Selznick, Philippe Nonet, and Erving Goffman; and a smaller “right” group that included Kingsley Davis, Guy E. Swanson, and William Petersen. I was always in the middle. I say “simplify” because the division between “middle” and “right” was sometimes unclear, and the positions of some in these groups occasionally shifted over time. In all events, when conflicts erupted, they were bitter and often personal.

Over the years several (mainly moderate-right) departed for other universities: Lipset, Glazer, Petersen, and Goffman (the last not for political but for careerist motives), and others sought early retirement (Glock) or appointments in other Berkeley departments (Bendix, Wilensky, Swanson, and later Selznick and Nonet). The “left” remained in the sociology department. I myself received “feelers” from history, psychology, and political science, but I chose never to leave the department officially, withdrawing, however, in other ways to which I now turn.

In the remainder of the 1960s I participated in department affairs responsibly (teaching, committee service when requested, and attending department meetings), but most of my time was spent in the Chancellor’s Office or the Academic Senate, some of which involved reduced teaching. I was asked twice by the Dean to chair the department and declined twice. My departmental circumstances were most directly affected, however, by offers from outside the university. Harvard had re-offered me a position in 1965, and Berkeley countered with a generous counter-offer. I stayed, again mainly for “loyalty to Berkeley” and family reasons. Then, in 1970 I received simultaneous and very generous offers of professorships at the University of Pennsylvania, Yale, and Harvard again. The latter was of special significance for me, not only because of my enduring gratitude and loyalty to Harvard, but also because the offer coincided with the retirement of Talcott Parsons. Nobody came out and said that the meaning of the offer was to “succeed” Parsons as “Harvard’s theorist” but a few whisperers said that and the timing and symbolism suggested that. Talcott certainly made no secret of pressuring me to accept the Harvard offer.

Once again Berkeley responded generously. David Apter of the Institute of International Studies, a long-time and faithful supporter, was especially eager that I remain at Berkeley. In 1966 he asked me to chair an IIS group on comparative studies, which involved monthly meetings with a campus-wide group of scholars in the other social sciences (I chaired that group for almost three decades, and it became the seat of a major research effort on my part—below, pp. 00-00). In 1970 he arranged for me to join the IIS as a research professor, which meant paying half my salary from Institute funds for life, thus guaranteeing a half-time teaching load for the remainder of my career! Even more significantly, Roger Heyns, still Chancellor, invited me into his office and said, simply, “What can Berkeley do to keep you here?” I pondered that question for a few days, then came back to him with the possibility of appointing me to the position of University Professor of Sociology. I explained to him about my commitment to interdisciplinary work and how that appointment would permit me further to “spread out” intellectually. I also knew, but did not say, that it would be a way of extricating myself from the turbulence of my department.

That turned out to be a more complex request than I imagined. The University Professorship was regarded as the highest faculty position. It was a small club of natural scientists only, mostly Nobel Prize-winning figures such as Glen Seaborg, Edward Teller, and Charles Townes. Appointing me meant that an outsider, a social scientist, would be breaking into that club. Heyns decided to press for my appointment, and started the machinery rolling. Interestingly, he made his move a policy matter by simultaneously recommending two other candidates outside the natural sciences—Sheridan Washburn from anthropology and Josephine Miles from English, both on the Berkeley campus. My appointment required approval of the sociology departments and administrations of every University of California campus. In addition, it carried with it the expectation that a holder of such a chair would have the right and obligation to teach on any campus of the university, but left the initiative to the holder of the position, with details worked out by negotiation with other campuses. Significantly, this made my connection with the Berkeley department even frailer. Though half of my salary continued to be budgeted through the department (the other half with IIS), I basically became a free agent in

deciding on my teaching. In actuality, I taught on five campuses other than Berkeley during the remainder of my active career with the University.

It was not really so, but when the appointment to University Professor came through, it appeared as though I had prematurely reached the apex of my career at age 40. When hearing that the appointment had gone through, an advanced graduate student with whom I had become friends, said to me, “This must be difficult for you; where can you go from here?” This was not a meaningless question. However, its implications—including “dying on the vine”—did not materialize. In fact I re-engaged with the Department as its chair in the following decade, and continued to fan out in different research directions and institutional activities as I moved through the remainder of my academic career.

The National Scene: A Move into Premature Statesmanship

The editorship of the American Sociological Review was significant in two respects for my relations to the national scene of sociology—both intellectually in a social-science discipline and in an organizational embodiment in the American Sociological Association. It was meaningful for submitters of manuscripts and readers of the journal—certainly in an influential role, and perhaps regarded as a position in the discipline’s “establishment.” Perhaps more important, the editorship carried with it an appointment of three years to the Council of the ASA and its Executive Committee, as well as membership on the Publications Committee, which meant meeting periodically with its elected leadership in facing the organizational and political issues at hand. My term of appointment—1962-65, the years of the editorship—was an active period for the association. In all events that term of service on the Council increased my visibility in governing circles of sociology.

Whatever dynamics were involved, the late 1960s occasioned a series of invitations that brought me into a position of publicly representing the field and to some extent speaking for it. I mention four episodes.

Contextualizing Sociology. In 1966 Paul Lazarsfeld, recent President of the ASA, made his Presidential “mark” on the world by editing a book, The Uses of Sociology, with William H. Sewell and Harold L. Wilensky. Applied sociology was one of Lazarsfeld’s major preoccupations. It occurred to the editors that, without stretching the

idea of “uses” too far, sociology might be regarded as “useful” to the other social sciences. An essay on the topic would presumably refer to the intellectual and other benefits sociology brings or might bring to its sister disciplines. They asked me to write this “impossible” essay (Smelser, 1967b). To execute this task, I articulated a number of dimensions on which the various social sciences could be compared analytically with one another. For this purpose I chose four—favored dependent variables, favored independent variables, models of explanation, and research methods primarily employed—and gave concise expositions of sociology, economics, political science, anthropology, history, and psychology according to these dimensions. I extended the analysis and attempted to pinpoint specific kinds of “services” or articulations that sociology and the other sciences could provide one another, given their contrasting emphases. Even at the time I regarded this assignment as possibly pretentious and unwise, especially for one who was scarcely a veteran or a statesman.

Presenting Sociology. In 1966 two organizations in the center of the social-science establishment—the Social Science Research Council and the National Research Council of the National Academy of Sciences—joined in launching a major assessment of the social sciences. It was called the Behavioral and Social Science Survey (BASS). I remind the reader that these were heady days of support for the social sciences from the federal government and private foundations. These were in fact the de facto audiences for the survey. The survey was led by a Central Planning Committee (chaired by Ernest R. Hilgard and Henry W. Riecken). There were also panels for each discipline. William H. Sewell and Otis Dudley Duncan co-chaired the sociology panel, and chose me as a panel member.

A completely fortuitous event occurred early in 1967. Sewell was summoned to be Chancellor of the University of Wisconsin and resigned as leader of the sociology panel. At the same moment Duncan announced that without Sewell as chair he did not want to serve as co-chair. Within a matter of days Hilgard and Riecken asked me and James Davis of Dartmouth to be the new leaders of the panel. This pleased but also humbled me, because I truly felt I should not be catapulted in to this kind of spokesperson role at my age and with my experience. It turned out that Davis chose to take a somewhat passive leadership role, so I did most of the actual drafting of the

sociology report (Smelser and Davis, 1969). Working as a member of the Central Planning Committee also brought me to the attention of the National Research Council and possibly paved the way for my very intensive involvement with that body from the early 1980s into the twenty-first century. The BASS reports were well received at the time of their publication. As it turned out, however, that publication coincided with the transition from the era of largesse from external funders in the 1960s into the relative stagnation of support beginning around 1970. As a result, our many proposals for grand support and innovation for the social sciences necessarily fell on deaf ears.

Evaluating Sociology. Coinciding with the BASS activities was an independent survey and evaluation of sociology organized by the American Academy of Social and Political Science. A conference was held in late December of 1967 in Philadelphia (it also coincided with the honeymoon of my second marriage, a very happy moment). The conference was attended mainly by past presidents of the ASA and those likely destined to be presidents. The format was individual presentations by three sociologists regarded as young Turks at the time—James S. Coleman, Peter Blau, and myself—with comments by two wise seniors followed by general discussion.

My assigned topic, “The Optimum Scope of Sociology,” was in one sense a ridiculous one, asking for a normative evaluation of the field’s diverse facets, all of which had a history of contestation. Furthermore, the presentation was made on the eve of a period of turmoil, in which the whole field was thrown into an era of fundamental controversy about what sociology is and what it should be. At the time, I made a serious effort to lay out, more or less systematically (a) the five major frameworks that inform genuine sociological inquiry—the demographic-ecological, the psychological, the group, the social-structural and the cultural; (b) subdivisions of the field and what has produced them; and (c) the proper nature of sociological explanation (see Smelser, 1969). The commentary and discussion of my paper was brisk and serious, capped by Robert Nisbet’s fundamental challenge to the whole idea of evaluating sociological study normatively, suggesting that sociology ought to be regarded a “what sociologists do.”

Evaluating Educational Innovations. In the early 1960s the federal government invested considerable funds in centers for educational innovation around the country. Most of these involved innovations in pedagogy and educational strategies. In 1968 the

Office of Science and Technology established a commission to visit and evaluate these investments from the standpoint of their effectiveness. I was recruited to this commission as the only social scientist, as I recall. We were stern in our judgments, and concluded that many of the investments were mis-investments in terms of meaningful research and educational innovations. Over a period of years the federal government terminated funding for many of these kinds of centers.

Joining the Social Sciences Establishment Officially. In 1969 I was asked to join the Board of Directors of the Social Science Research Council. I was not unknown to the SSRC because of my years of membership on the Committee of Economic Growth. However, I regard my appointment as due directly to the recommendation of Gardner Lindzey, my undergraduate thesis advisor and lifetime supporter. Two years later I was asked to chair the Board—another premature assignment, I thought. These appointments involved several trips to New York City each year. They were important years for the Council, featuring deep internal conflict over a proposal by its President, Henry Riecken, that its headquarters from New York to Washington, DC. The idea activated fundamental divisions in the Council, and in the end the proposal was defeated and Riecken resigned. After that time I was centrally involved in the search for his successor, ultimately Eleanor Sheldon.

To myself I refer to these years of central involvement in the sociology and social science establishments a period of “premature statesmanship.” “Premature” connotes both my early age and my conviction that, despite the early accumulation of recognized scholarship, I was still early in my career. I lived with a lurking feeling that I had not yet proved myself. “Statesmanship” meant that I took on a role of speaking responsibly for my disciplines and the social sciences and that I had to be as wise and diplomatic as possible in “representing” them. Finally, I had to deal interpersonally with dozens of senior colleagues in the organizations and enterprises into which I had been drawn. As I recall, I experienced very little personal anxiety or difficulty in meeting these expectations. I approached them in a businesslike way. If I can judge this, I did not develop a “big head” or “pull rank” on others. The only negative feelings were the haunting ones that I didn’t really deserve this recognition, feelings derived from a mix of latent pride and latent guilt. Both of these were derived, I am convinced, from my origins

of ambition and desire to be special, combined with the idea that all the brothers were equal and shouldn't set themselves off as superior to one another.

Research and Publication in the Chaotic Decade

In following this account of organizational and political involvements in the 1960s, a reader cannot fail to ask, "What happened to scholarship, writing, and publication in the 1960s?" The question is a natural one, given the heavy demands on my time and the uncontrollability of deadlines, the travel involved, and the periods of emotional and physical exhaustion. Save for one project, to be detailed at the end of this section and in the next chapter, I did not initiate anything of the magnitude of Economy and Society or the two major monographs that followed. I began work on a general book on economic development and the family—generalizing the theme of my thesis—and received a grant to pursue it, but it fizzled through lack of attention. Most of my writings were shorter, on more limited topics, built to some degree on past research, written on demand or request, and somewhat unrelated to one another.

Psychoanalytic Themes. Three papers arose from my involvement in psychoanalysis and the psychoanalytic institute. One was explicitly on the role of psychoanalytic forces, especially motives and conflicts, in different kinds of collective behavior (Smelser, 1968a). It was written by request of the Ad Hoc Research Committee of the psychoanalytic institute as a supplement to my monograph. It was a paper limited in scope in that I fell short of developing anything like interactive models or synthetic explanations. I also concluded that the foreignness of sociological and psychoanalytic perspectives to one another basically forbids formal synthesis.

A second paper was submitted as a "thesis" required of all candidates for graduation from the Institute. It was supposed to be an article-length paper on an issue in psychoanalysis, as contrasted with a clinical report. I developed an idea that arose from my critical reaction to the psychoanalytic thinking about the mechanisms of defense. In my voluminous reading of the psychoanalytic literature, I noticed that writers on the subject treated these mechanisms—repression, rationalization, many types of acting out, for example—mainly as strategies of the ego to cope with instincts and conflicts internal to the personality. In reaction to this I asked whether the idea of defense mechanisms could equally well be applied to external threats. For my thesis I developed a new and

more systematic classification of the defenses (almost everything has been called a defense in the psychoanalytic literature) and supplemented it with a parallel classification of reactions to external threats. I experienced great satisfaction in constructing these arguments. My teachers and fellow candidates at the Institute declared it a very scholarly paper, though a couple of the more orthodox ones muttered criticisms about my heterodoxy, saying I was deserting psychoanalysis proper. I did not seek to publish this paper, and cannot account for my indifference. It sat around for more than a decade, and I brought it to life only in the 1980s, when I revised the essay and presented it at a conference on the micro-macro link (Smelser, 1987)

The third essay arose from a request from Robert W. Wallerstein, a senior training analyst and scholar at the psychoanalytic institute. He had been commissioned by the editors of the International Journal of Psycho-Analysis to write an essay on some broad topic, and he chose “psychoanalysis and sociology” as his theme, simultaneously asking me to collaborate. I was ready for this interdisciplinary enterprise, because I had just published my essay on sociology and the other social sciences and had also been reading intensively in psychoanalysis in the past several years. Wallerstein and I included a systematic comparison of the two fields as scientific endeavors, applied the notion of “complementary articulation” to tease out their relations to and usefulness to one another, and, finally, contrasted them with respect to their applicability to different kinds of social issues and social problems. This was an exciting intellectual adventure with Wallerstein, a broad-gauged psychoanalyst with a fine mind. What I brought to this essay were revised versions of insights I had developed in the essay on sociology and the other social sciences (Wallerstein and Smelser, 1969).

I note further that the three essays on psychoanalysis established the following pattern that has characterized my whole career. As my work became better known, I received increasing numbers of invitations from other scholars, asking me to contribute an essay on a given topic to a journal, an edited volume, or a symposium. I turned many of these down for reasons of lack of time or lack of interest. I accepted many, however, and much of my scholarship was in this sense reactive. However, because the solicitors wanted me to accept, they usually left my topic open-ended, and I could conveniently

write about what was on my mind at the time. This is a special kind of mutual opportunism, I suppose.

Scholarship on Higher Education. A second cluster of writings initiated a career of research on higher education. On the basis of my service with Meyerson in 1965, Clark Kerr came to notice me. After he was “fired with enthusiasm” from the Presidency of the University of California in 1967, he took over the leadership of the Carnegie Commission on Higher Education, which was to become one of the most influential bodies in the history of American higher education. He asked me to join the Commission’s “Technical Advisory Committee,” an informal advisory group composed mainly of Berkeley faculty. He met with us frequently in strategic sessions. I came to call my subgroup of the Committee “Clark’s boys.” I served as member of this advisory committee and its successor for almost a decade. Kerr and his colleagues persuaded me to write two essays. The first was an assessment of the Free Speech Movement a decade later, meant to analyze its causes and significance and to assess more generally how the campus had changed subsequently (Smelser, 1973b). The second was on a completely different topic: how best to represent and teach the social sciences in undergraduate curricula (Smelser, 1973c). That essay initiated yet another theme that echoed into my future: general education, its crises, and its reform.

Two more important projects on higher education emerged in the late 1960s. Talcott Parsons and Gerald Platt had turned to that subject and ultimately co-authored a major statement in a book entitled The American University (1973). It was a very important treatise, and, among other themes, analyzed many of the political disturbances in recent decades as reflecting tensions in the cultural value system of the university, which they described as “cognitive rationality.” The general theme was a somewhat benign one, treating many of the recent conflicts in higher education as transitional “adjustments.” Parsons and Platt asked me to join the enterprise as co-author because of my recent involvements on the Berkeley scene. Not really knowing their intellectual agenda in detail, and interested in another collaboration with Parsons, I accepted.

Disagreements emerged from an early moment in our collaboration. We found ourselves differing—though not completely at loggerheads—in the interpretation of recent crises and conflicts in higher education, including student protest. Their dominant

interpretations stressed cultural strains and adjustments, whereas I was convinced that deeper social-structural changes and associated group conflicts were much more fundamental. We argued a lot around these themes, and at a given moment I suggested to Parsons and Platt that our differences were sufficiently deep that I should not be a collaborator. This constituted a crisis of sorts—given my history of engagement-disengagement with Parsons. Platt came to the rescue and argued that the conflicts were not fundamental but were significant enough that I should not be a co-author but, instead, write an “Epilogue” (Smelser, 1973d) stressing my own interpretations. This seemed a happy compromise to both Parsons and me, and I am forever grateful to Platt for his diplomacy.

Matters did not end there, however. I drafted my epilogue and sent it to both of them. Parsons came back with a very long critical response, objecting to many points and much of my language, and suggesting many changes. His letter reached a kind of climax when he said I was “giving away the shop” (he should have said “his shop”) to his main critics—Lewis Coser, Ralf Dahrendorf, C. Wright Mills, and neo-Marxist writers—who had been battering Parsons for years for his benign, conservative sociology that was incapable of analyzing conflict. Years of criticism on this score had left Parsons bitter and defensive. Our long collaboration and friendship was also endangered by this clash. I responded to his critical memo, saying that I would make changes where I thought his suggestions were justified, but that I would not change my fundamental line of analysis. At that stare-down moment Parsons backed down, apparently willing to live with our differences rather than risk a rupture. The Epilogue appeared much as I had drafted it. Some reviewers noted the strain between our analyses. Happily, over the next couple of years Parsons and I re-established cordial relations, and our friendship and scholarly communications lasted right up to his death in 1979.

In connection with the Parsons-Platt project, I applied for and received a grant from the Ford Foundation that relieved me from teaching for 1970-71 to write a monograph on social change in the University of California and organize a conference on the topic under the auspices of Western Center of the American Academy of Arts and Sciences. I presented a draft of my monograph—about a half-book in length—to the

conference in February 1972, and each of a dozen academics and policy-involved others contributed a chapter (Smelser and Almond, 1974).

For my contribution I chose to make the monograph a fully academic work that would use of the set of principles of social change with which I had been dealing since my dissertation. I focused on the years 1950-70, the years of spectacular growth of the University of California, featuring a ballooning of graduate study and research emphases, the addition of three new University campuses, and the creation and solidification of the Master Plan in 1960. My focus was mainly on structural aspects of this growth on the one hand, and the kind of new groups or constituencies or estates that emerged (for example, teaching assistants, non-teaching research personnel), their changing place in the university system, and their dispositions to conflict. I was trying, among other things, to make change in the university system a special case of general principles of change, and throw light on the pattern, timing, and directions of conflicts of the era (including the second half of the 1960s). This kind of analysis is not frequent in the sociology of higher education, which tends to be more limited in theoretical scope and more policy-oriented. Three decades later Burton Clark commented that my analysis “has remained virtually one of a kind in the in the blending of rich empirical material with a highly-structured framework” (Clark, 2004). That is a very gratifying assessment, because that is what I was intending to produce at the time. Parsons wrote an Epilogue to the book (Parsons, 1974) in which he criticized my analysis of “functional overloading”—i.e., the continuous accumulation of functions and activities that made for inefficiencies and conflicts among groups—and he himself praised the “bundle” of functions as a positive feature in the evolution of the American university. His essay was written after I had written my Epilogue to the Parsons-Platt volume, so in the end I suppose that equal time was had by all.

I mention two final projects. In connection with a University Chicago Press project on the history of sociology, I agreed to edit a selection of Karl Marx’s writings (of my choice) that were the best sociological contributions (in my estimation). Searching for these was a time-consuming adventure, and on top of that I wrote an interpretative essay as an introduction (Smelser, 1973e). The introduction drew some flack from some Marxist and other left-leaning sociologists, who objected to my analysis of theoretical

continuities between Marx's view of society and the views of contemporary functionalist sociology. They didn't like that analysis because they regarded it as a sully of Marx by comparing his work with a thoroughly bourgeois (in their estimation) mode of theoretical thinking. The book enjoyed sizeable national and international sales, which continue.

The second piece was originally a commissioned essay for an organization called Human Sciences Research, Inc.—a beltway bandit, I believe, located in McLean, Virginia. Some federal agency preoccupied with civil defense in this period had commissioned a project on what sociological analysis might say about the short-term and long-term reactions to social catastrophes, including nuclear attack. As a student of collective behavior and social change, I was a logical candidate to prepare such an essay. I should also add that this was almost the only instance in which the fee for my writing was a determining factor. In 1965 I was in a temporary period of post-divorce poverty. I got a little criticism from the left for writing this essay in some letters to the Daily Californian. The critics argued that writing it was an illegitimate collaboration on my part with the nation's war machine. Later I reworked the essay as part of a more general and more ambitious analysis of short-term and long-term societal change (Smelser, 1968b).

Coda.

Readers of the last few pages may conclude that I am wrong in depicting the decade 1964-73 as a constricted period in my research life. Maybe my judgment of it as such is incorrect. I did in fact move in several new directions, but each item of writing was discrete and the quantity and drama of my work was more limited. More important, independent of actual evidence on this score, I felt I was cheating my research, and that depressed me. Looking back, I think that my impulse to publish a book of essays written in that decade (Smelser, 1968c) might have included a silent and irrational dread that I was deluding myself and others that I was still that wunderkind scholar. I realized these subterranean workings in my reaction to a comment by Morris Janowitz from the University of Chicago. When I mentioned the publication of that book of essays, he remarked, in a humorous-hostile way, "Ah, you are in that fallow stage of your career when you produce selections of your thinking already published." The comment was like a dagger, because it struck at the heart of my own silent dreads.

Another side of these subterranean forebodings is that, as the decade of the 1960s went along, I did begin to envision and formulate my thinking on a big project—a methodological treatise on comparative analysis. I became very excited about that project and impatient to throw myself into it. That excitement was another expression of growing internal dissatisfactions. Moreover, the excitement was made real by the fact that I had a sabbatical leave scheduled for 1973-74 and competed successfully for a Guggenheim Fellowship for that year, thus guaranteeing that I could execute the project in Europe. Being away that year, moreover, would remove me from both the demands and temptations to continue the heavy organizational and political involvements of the past decade. So when my new family and I departed for London in the summer of 1973, it was something like a rebirth for me. I will begin the following chapter with an account of that year.

Chapter 6

New Ventures at Home and Abroad—1973-80

The remainder of the 1970's brought a productive year abroad followed by two years as chair of the sociology department at Berkeley, two more years as director of the University of California Education Abroad program in the United Kingdom/Ireland, then a re-immersion in Academic Senate affairs at Berkeley. It was in many respects a disjointed decade with, however, some threads of coherence.

A Splendid Year in Europe

As indicated, I had grown hungry for a return to scholarship during the late 1960s, and the scheduled sabbatical year 1973-74 provided the occasion. It was an opportunity for my family as well. By then my second wife and I had two children who would be 5 and 3 years old during that year. We decided to give special priority to a family experience. It was necessary for me to spend some months in London's libraries, finishing the research for the book on comparative methods, but the actual writing could be done elsewhere. We determined to buy a camper for wandering and to settle for periods of writing and exploring in various undetermined places in continental Europe. I will tell the academic story first, then say some words about the family odyssey.

Scholarship on Comparative Methods. My preoccupation with the possibilities and puzzles of generating valid comparative knowledge traced back to my college days, when I was exposed to the philosophical and anthropological literature on cultural relativism. An important tenet of anthropological relativism was that it is an illegitimate exercise in ethnocentrism to assess and analyze other societies and cultures in the categories of one's own (or more generally, Western) values and preconceptions. Every society has its distinctive premises and preferences, and valid understandings or explanations of other societies must be framed in the context of these. It occurred to me early—and troubled me as a social-scientist in training—that if one carried this theoretical-methodological position further, one would end up in a state of scientific paralysis or nihilism, unable to generate knowledge that is valid across societal-cultural lines.

For a long time I set this troubling puzzle aside, but it re-emerged in connection with my thinking on modernization in the early 1960s. In particular, in my essay on the contrapuntal play between differentiation and integration as central to the process, I was in fact engaging in generalizations that transcended cultural-national lines. I mentioned that Clifford Geertz, at Berkeley at the time, and a faithful analyst of unique cultural configurations, chided me for my analysis, reminding me that every society and culture tells its unique story of social change and that these cannot be forced into a common pattern. Another contemporary expression of this issue was “configurational analysis,” representing societies as unique constellations or patterns of causal variables. The issue was also relevant in psychology in the tension between explanation on the basis of correlational analysis of discrete variables on the one hand and the “pattern” analysis of clinical reasoning on the other.

These issues became even more salient in my mind when the possibility of writing a methodological work on comparative studies with Marty Lipset emerged. I forget which of us initiated this project, but it came alive around 1964. We resolved to carry through with it. The salience of the project increased when I took on the leadership of the comparative studies group—of which Lipset was a member—in the Institute of International Studies. Expressing my intent, I wrote a critical essay on my own on the methodological problems that arise in comparing economic indicators and economic processes (Smelser, 1967c). However, Lipset was also engaged in many of his own projects, and was also personally preoccupied in fighting out political issues in the post-Free Speech Movement years. (He had been singled out as a kind of right-wing demon by the left on campus.) Ultimately he became alienated from the Berkeley scene and departed for Harvard. That effectively killed our collaborative project. However, I continued to be very engaged with the idea. I wrote a methodological essay on issues of comparative analysis in the work of Alexis de Tocqueville (Smelser, 1971b), and determined to write a general book on the subject during my coming sabbatical leave.

As with almost every one of my research projects, this one required me to spread. First, I knew there was a viable literature on comparative studies (including methods) in anthropology, political science, sociology, economics, history, and psychology. Wishing to make my analysis as comprehensive as possible, I felt it necessary to familiarize

myself with it. This kind of spreading out was legitimized by the conviction that comparative-methodological issues facing social scientists are generic and repeat themselves similarly without regard to disciplinary subject matter.

Second, I wanted to show that systematic comparative methodology was not a new invention in the social sciences but had been relevant ever since investigators had begun to define their studies as a species of scientific investigation. That assumption led me to include a methodological analysis of some classics as well as contemporary investigations. On this basis I decided to adapt my Tocqueville essay and treat his major works on France and the United States as a single comparative study, even though he did not conceive it as such. I also included both the methodological manifestos and much empirical work of Emile Durkheim and Max Weber, obvious choices because both had articulated different versions of comparative analysis. I could have included more classic thinkers as well, but I stopped with these three. Some readers of the book said I should have included Marx's historical analyses, and perhaps I should have. However, no new lines of analysis would have emerged if I had expanded the list of classics. Besides, I wanted to give full treatment to contemporary patterns of investigation.

Third, I took a broad view of methodology. To many that term connotes mainly a concern with measurement (usually quantitative), precise specification of independent and dependent variables, appropriate statistical techniques, and research design generally. I had long argued (see Smelser, 1971a) that there is a methodology at the theoretical level as well, and that issues of classification, logical consistency of argument, formal deduction of hypotheses, and conceptual clarity should come under the heading of methodology because they are an integral part of scientific analysis and explanation.

Fourth, and perhaps most important, I had a special analytic reason to spread. Early in my thinking I decided that there is no special method called comparative; instead it is a special case of scientific method in general. That method was that explanation involves the systematic manipulation of variables into those that are "held constant" and those that are "made to vary"—I called these parameters and operative variables—and by manipulating them to establish causal relations. Defining scientific inquiry thus, I was able to identify many types of investigation as variations of the same rubric. Thus I included (and found continuities among) experimental, statistical, "comparative" (many

variables, small N), case-study, as well as less obvious strategies such as exploring deviant cases, employing simplifying assumptions, and making use of “imaginary experiments” or counterfactual analysis. All these, I argued, have the same explanatory structure and strive toward scientific adequacy, but differ from one another with respect to the ways in which they can identify and handle parameters and operative variables.

Such was the overall framework that fell into place in the late 1960s. It was very important that I formulated it early, because it provided a series of guiding principles that permitted me to decide what issues were important in selecting from and interpreting the mountains of literature through which I was plowing in those years. I also refined my thinking by delivering lectures on Durkheim and Weber in my graduate theory classes. Having that analytic framework also facilitated the organization and writing of the final manuscript.

On the basis of the needed research, my wife and I decided on the following strategy. I covered as much as I could in Berkeley’s libraries from around 1969 to 1973. I knew that would not be fully sufficient, so we decided to spend the late summer and fall of 1973 in London, where I could make full use of the British Museum Library and the library of the London School of Economics and Political Science (where I had arranged a visiting research appointment). We lived in the Islington home of an LSE scholar on leave, and took in as much of London’s child and adult entertainment as we could. Then we camped for a month—outskirts of Paris, Avignon, Aix-on Provence, and then northern Spain—searching all the while for a place to settle for four months of intensive writing. We located our winter home in a villa called Ma Vie in Cagnes-sur-Mer, a magnificent spot for my work and for our explorations of the French Riviera. Then on for six weeks in an Italian villa on the Lago di Garda—trading the stay with my Italian student and colleague, Alberto Martinelli, who took up residence in our Berkeley home. Then two more weeks camping outside Vienna and elsewhere in Austria, before returning to two final months in Bloomsbury and drafting the text for the comparative book and finishing the second edition of The Sociology of Economic Life. If this year reads like a dream year, it should do so because it was.

The first thing I did after returning to Berkeley in the fall of 1974 was to put the completed draft on the agenda of my research group on comparative methods in the

Institute of International Studies, from which I had also taken leave during 1973-74. Members of the group read the manuscript assiduously, and we dedicated the first dinner meeting to it. The discussion was generally kind, but I should mention two lines of criticism that emerged. Charles Glock, our “methods man” in the sociology department, developed a criticism that my treatment was too “soft,” that is, not focused enough on standard quantitative indices (for example, demographic and survey data). In the same discussion Reinhard Bendix spoke out against a “positivistic” bias in the manuscript, with the result that I did not give enough play to unique historical patterns and, in effect, distorted the richness of history. In a strange way these lines of criticism, coming from opposite directions, pleased me. If I could draw fire from both sides, I must have succeeded, in one way or another, in my synthetic efforts.

The work on comparative analysis proved to be one of the most exciting episodes of my intellectual career (Smelser, 1976a). I still have the feeling that I got things right. Several comparative social scientists, however, criticized me for having an anti-comparativist bias. The accusation was that in grouping all the various methods together as sub-types of the scientific method of thinking, I was ennobling the laboratory experiment, and downgrading its imperfect cousins (comparative methods, the case study, etc.) to second-class citizenship. By extension, the critics saw comparative methodology as a distinct and special method that stood on its own. I defended myself by saying that I was not evaluating any of the methods normatively, but regarded each as a distinctive adaptation of research procedures to different research settings, problems, and possibilities. In all events, the book fared well in the market, and was republished recently (Smelser, 2013a), with gratifying sales. I never revised the volume, and attended to comparative-methodological issues only in a couple of invited lectures that were later published (Smelser, 2000a; Smelser, 2003a). More generally, I observed my working rule that I would let my major publications speak for themselves and refrain from publishing second editions, second thoughts, defenses against criticisms, and polemics.

A Strange Interlude

I report one most important episode—a completely fortuitous one—that worked to alter my general orientation toward the study of social change. Up to the early 1970s most of my focus had been on growth, though I had included a much wider range of

types, including the decline of civilizations, in my general theoretical treatment of change (Smelser, 1968b). The episode began with the OPEC crisis of 1973, which struck the world when we were living in splendid isolation in our villa in Cagnes-sur-Mer. One was compelled to follow the crisis, however, because the events hit the European economies very hard. In the midst of the confusion a number of Italian sociologists hastily gathered in Bologna. Their main preoccupation was: what would be the consequences if advanced industrial countries face a long-term situation of energy constriction? Furthermore, they mobilized a conference to take place within a matter of weeks and invited a group of available sociologists to assemble in Bologna and ponder that question.

At that time we were preparing to leave France and drive to our next villa on the Lago di Garda. One evening a neighbor (whom we did not know) came banging on our front door and said someone had telephoned his house, wanted to talk with me urgently, and was waiting on the phone at that moment. I was totally baffled, because nobody knew I was living Cagnes-sur-mer. By some stroke of ingenuity, however, the Italian sociologists had located my address, found a neighbor, and called! They wanted me to come to Bologna to the conference and to speak on their question in a week or so. I decided impulsively to do it, and set everything aside to ponder the question. I started with the impacts of changes in patterns of consumption occasioned by a decline of economic activity, and this led me to impacts on status spending and its extension to a society's stratification system. The general atrophy of economic activity, I thought, would fundamentally alter spending on status symbols. This led me to another, more general topic. I observed that in studies of growth—with which sociology and economics had been concerned throughout their history—a dominant motif is increasing complexity of society through the differentiation of social structures. What happens when the pattern is shrinkage? The existing complexity of structures and groups cannot be peeled back in the same way that it grew. The reason for that is that under conditions of indefinite shrinkage of societal wealth, groups above all fight to protect what they have. The dominant themes, then, are jealousies, jurisdictional rigidity, and embittered and repeated group conflicts over resources. I later wrote up an essay on this topic and published it in an obscure German journal (Smelser, 1979). The episode and my thinking heightened

the salience of crisis and disorderliness as social principles, and certainly crept into my subsequent analyses of social paralysis (Smelser, 1991a) and terrorism (Smelser, 2007a).

Two Tough Years as Chair

Returning from my sabbatical in Europe, I took on the chairmanship of the sociology department for two years. Readers may wonder why I did so at the time. I have never really sorted out the reasons. After all, I did not have to take the job. I knew already that it was an arduous assignment in a still conflicted department, and that Charles Glock had recently been wounded to the point of retiring from the university. I had turned it down twice before, and I regarded my appointment as University Professor as generally excusing me from such routine administrative posts. At the same time, the Dean and his colleagues were desperate for leadership and were prepared to strong-arm faculty members into the job. In fact, they conjured up a scheme that they hoped would bring some longer-term stability. The scheme was a six-year deal: Leo Lowenthal would chair for one year, I would chair for two, and John Clausen would then come in for a full three-year term. We all accepted this proposal. This group pressure influenced me, as did the prospect of two rather than three years. Finally, I believe I was already secretly a bit guilty about the coming, fantastic year abroad. I knew I was going on leave, and was something of a sitting duck for administrative assignment after a year of freedom. I did not delude myself into thinking that my colleagues were enthusiastic about my appointment—everyone was an enemy of someone in that department—but I suppose that being in the political middle and with a peacemaking style I was relatively acceptable. I also resolved in advance that I would attempt, whenever possible, to deal with individual faculty rather than groups or delegations. This proved impossible, of course, but the principle was sound enough.

I will not bore the reader with accounts of routine chairmanly duties—responding to the Dean's queries, keeping peace among the administrative staff, responding to colleagues' requests and occasional threats. (In a moment of frustration I thought up a definition of "chair" as "one who spends 80 per cent of his time running errands for disagreeable colleagues.>"). One situation that confirmed this definition arose immediately. Four senior colleagues had recently retired—Kingsley Davis, Herbert Blumer, Leo Lowenthal, and Wolfram Eberhard. At that time we had some funds to

recall emeriti to teach and pay them for it. All four approached me, independently, asking to be recalled. But a tightening budget yielded only one full-time position. All of them wanted it, and each made his wants known, insistently. I did the only thing I could do; I proposed (and enforced) a Solomonic solution, cutting the position into four, and giving one-quarter recall to each. They all accepted that, because it was the only thing they could do. At that moment I made up a mantra that lives to this day in departmental lore: “The key to being a successful chair is that you say to anybody who comes to you with a demand ‘You are absolutely right, and I’m going to do everything I can to support your position, but you must understand that there’s almost nothing I can do.’”

Strengthening the Major. One of the consequences of the student turbulence in the 1960s was that departments—and universities in general—had loosened departmental requirements for majors and relaxed grading standards to the point of serious grade inflation. By the mid-1970s a kind of backlash had set in, and a “movement” developed among deans and others for departments to beef up their requirements and restore some rigor. They did not dictate specific solutions, but there was a general feeling that if departments did not respond, the deans would be forced to raise the stakes.

The sociology department first responded by sorting its undergraduate courses into “core” and “other” and required that majors include a minimum number of core courses in their major. Core courses were in sub-fields that were thought to be central to the discipline and perhaps more rigorous. Evident examples would be demography, social stratification, and formal organizations. Sociology of literature and sociology of religion might not make it. This proposed reform was not extremely controversial, but it did generate some grouching by teachers of non-core subfields. After all, who wants to be labeled non-core, with its second-class-citizenship implications? This issue was sufficiently divisive that I had to intensify my persuasive efforts, most often using the argument that if we didn’t adopt such curricular change in our own ways, we would be risking more direct action from the College of Letters and Sciences.

A second, more intense pressure involved teaching research methods. The department already had a required upper-division course consisting mainly of instruction in quantitative and statistical methods. The pressure was to supplement this course with others. A logical candidate emerged: to add a required introductory methods course

(content not originally specified) at the lower division. The manifest justification for this was to give rigor to the major, but there also lurked a sense that it was a way of discouraging weak students from majoring in sociology and demonstrating that it was not a “gut” major. The department was divided over whether the course should emphasize quantitative methods course or include other methods of investigation such as fieldwork and historical-comparative analysis (to the numbers people, less rigorous and more permissive). Wrangling over this and related issues began to grow in department meetings.

For reasons I cannot fully explain, I responded to this conflict pro-actively as chair. This turned out to have both a pedagogical and a political aspect. I actually developed a specific kind of course. It would be called “Evaluation of Evidence.” It would be required of majors and intended as a kind of introduction to the field in the freshman or sophomore year. It would include perhaps a half-dozen books, each relying on a different method (experiment, survey, historical-comparative, ethnographic), with the instructor leading the way in identifying strengths and weakness of each. The course was to be non-technical, and I specified that it should not be taught only by the “methods people” but by anyone in the department. I volunteered to be the first faculty member to teach it (I had just finished my own methods book). I even described the hypothetical course to the Dean of Letters and Sciences, who gave it an informal vote of approval in advance. Armed with all this detail, I brought the whole package to the department, thinking (and hoping) that a structured, detailed discussion would be more fruitful than a free-for-all that excited so many jurisdictional jealousies. I told them about my discussion with the Dean, and again hinted darkly that if we didn’t do something along these lines, someone “upstairs” would likely do so. I secured the department’s approval—even of the course’s structure—and taught it the following year. The course became a departmental fixture that endured. On the personal side, I have never liked to think of myself as having a manipulative style. But this episode revealed just that style. If I said anything to myself on this point, it probably would have been that I was driven by circumstances to posture of aggressive intervention.

A Notable Personnel Action. Arlie Hochschild was a graduate student of mine in my early years at Berkeley. I chaired her Master’s essay (on the public role of the

ambassador's wife); sat on her orals examination committee; and chaired the committee for her Ph.D. dissertation (on the community and social interaction in a home for the elderly). She was one of our best, and the department over-rode the general expectation that it did not recruit our own recent doctoral students to the Berkeley faculty (based on a semi-articulated anti-inbreeding sentiment), and appointed her to a position of assistant professor.

During the course of that appointment she applied for making her position half time, and she was successful in that application. The two relevant circumstances underlying the request were, first, she and her husband wanted to start a family and she wanted to devote ample time to her children, and, second, their family resources were such that they could manage comfortably with her on a half-time salary. She served the department for several years on this half-time basis (expressed mainly in teaching half as many courses as her full-time colleagues).

By the ticking of the academic clock, Arlie came up for tenure during my years of chairmanship. This meant in turn that I, as chair, had to superintend the departmental assessment and vote, and represent the department to the higher administration. After going through its customary procedures, the department voted favorably on her promotion. I agreed without reservation with the vote. As her case went through the complicated, multi-stage review process embedded in Berkeley's personnel practices, her promotion was challenged, and indeed threatened by review committees and academic administrators, who asked (and doubted) whether she had been "productive" enough with respect to published research. When I, as chair, heard about these rumblings of opposition, I went immediately into action. I asked my administrative superiors if they really understood what half-time meant. Surely it meant, unequivocally, half-time teaching. But were they not thinking that that teaching was the whole story, and were judging her, erroneously and unfairly, full-time with respect to academic productivity and publication, to say nothing of departmental and public service? I asked them what it meant for the tenure clock to be clicking for a part-time appointee. And finally I asked if they were treating her unfairly on grounds of which they were unaware. For reasons I did not articulate to myself, I did not make this a larger political issue and duke it out as an issue of gender discrimination, though indeed it was and I believed it to be. I stressed the

outmodedness of the dominant image of the full-time faculty member in this case, and the injustice involved in imposing (perhaps unconsciously) full-time expectations on one whose contract was half time. Perhaps I sensed unconsciously that pursuing this procedural tactic would be more effective with my audiences upstairs than launching a heavy, ideologically loaded barrage of arguments based on gender discrimination in the university. In all events, my efforts prevailed and she was promoted. Her life career as one of the most successful and sensitive scholars of the sociology of family, gender, and emotion have unequivocally justified that result.

Expanding the Faculty. The travails of my second year as chair had to do with recruitment of new faculty, a perennial source of concern and conflict in academic departments. By virtue of a number of retirements and non-promotions in the department, positions for six assistant professors came available to be filled over two years, three of these in 1975-76. This was exceptional, because the mid-1970s were a period of market stagnation for sociology and for the university. Later in that year we were granted the possibility of a fourth position, joint with political science. That department was under affirmative-action pressure to hire minorities and women. The administration offered to provide a position—half time in sociology and half time in political science—for the appointment of a qualified woman scholar. Such positions were called “targets of opportunity.”

I will not relate the complicate saga of how these positions were filled. Subsequently I wrote that story, with the department’s top administrative staff officer, as an analysis of the academic labor market in general in those years and the story of our searches and appointments (Smelser and Content, 1980). Here I will tell the skeletal story, but will concentrate more on how I reacted to and coped with the ongoing drama.

Two salient features dominated departmental life in that moment of its history. First, its recruitment machinery was hopelessly inadequate for the recruitment work required. It had a standing personnel committee of five faculty, which was responsible for evaluating candidates and bringing recommendations to the department for a vote. This year we had four openings, an unprecedented number. In addition, faced with a buyer’s market and under constraint of affirmative-action requirements to advertise, we could anticipate (and did receive) between 75 and 100 applications for each position.

With that flood, no single committee could possibly give full and fair evaluation to such numbers. Second, the department still suffered the deep cleavages generated by the Free Speech Movement and anti-Vietnam war periods. The recent salience of affirmative-action pressures, in full swing in the mid-1970s, created another basis for conflict, as well as the issue of how much, in what ways, and with what power our graduate students would participate in the search. Adding new people to the department could be nothing other than ridden with conflict.

I dealt with the first issue by expanding the faculty's involvement beyond the personnel committee by a specific mechanism. I formed a new personnel subcommittee for each of the three new positions; the chair of each was a member of the larger personnel committee, and other members were added from the ranks of the rest of the faculty. This move was intended both to spread the work of evaluating candidates and to include, as a political strategy, most of the faculty in the process. Each subcommittee was to process "its" candidate, and recommend back to the larger personnel committee, which in turn would bring the favored candidate to the department for debate and approval. These innovations drew no opposition and appeared to function well. We also incorporated the available computer technology to track and systematize the process.

How to deal with the political cleavages in the department, which were certain to appear as the academic qualifications (and apparent political orientations) of each recommended candidate became known? This was the most difficult question, because one could not predict when and in what way conflicts might surface. One strategy I adopted was to follow the required search procedures fully, publicly, and religiously—procedures such as advertising and publicizing the positions, following and reporting faithfully on affirmative action procedures, and standardizing the criteria for evaluating candidates. To do this was not only right practice in itself, but it helped blunt any suggestion of procedural irregularities that might taint the process. I also felt it essential that I refrain from pressing, as chair, for one specific candidate or another, but rather to play the role of coordinator and facilitator, pressing the department to be certain that we followed all procedures and completed the searches. I felt it would be suicidal if the chair pressed his own views.

Two of the positions were filled without controversy—the joint appointment with political science, and one historical-comparative position by a scholar with strong interests in the Russian Revolution. Both were women, and gender played a role in the former, because political science was under the affirmative-action gun. For the other two positions the searches produced two candidates from the University of Chicago. Quite by coincidence, one happened to be in “mainstream” sociology—a rigorous methodologist and an organizational sociologist—and the other an avowed Marxist sociologist. To appoint both might have seemed rigged as horse-trading, one candidate for the left and one for the moderate-right, but I can assure readers it was not a deliberate ploy to bring people to consensus. The two candidates had surfaced independently from the work of two separate personnel subcommittees. On the critical day of the final departmental vote on these two candidates, both were approved but several faculty from the “right” spoke up against the Marxist candidate and proposed a young demographer instead (I did not anticipate this move), but they were voted down. All four candidates accepted the positions, so in the end we formally “succeeded” in completing this demanding search. I was relieved and proud, though I reported these feelings to no one.

Immediately after the searches were completed, my administrative associate, Robin Content, began agitating that I write up the whole process for publication, arguing that it was important for others to know how a major research university had dealt with the challenges of the time. I was initially not enthusiastic about this project, but in the end agreed, on condition that she would be a co-author, that we (I) would develop a general diagnosis of the academic market in the 1970s, and that we would cast the analysis in such a way that it be a supplement and corrective to the most influential book at the time—that of Caplow and McGee (1958). We finished the book over the next couple of years. A few publishers declined interest, largely because they feared that a case study would not generate large enough sales, but the University of California Press obliged (Smelser and Content, 1980).

I suppose that in general I reacted to those two years in the chairmanship the way most academics would—frustration with completing paperwork, coddling difficult colleagues and staff, dealing with central administrators, and feeling that I could have been doing more important things in my career than completing unrewarding chores. I

experienced almost no sense—or gratifications—that I was wielding power. Almost all my positive feelings were that I had steered the ship through stormy seas and had avoided catastrophe. I have often wished that I could be more positive, but I do not think that Berkeley sociology's academic scene at the time permitted that reaction. In all events, in the summer of 1976 I told myself that I had paid my dues and vowed not to serve as chair again. I broke that vow 15 years later, again for reasons somewhat beyond me.

A Brush with Medical Education.

The years in the chair were punctuated by an engaging assignment that began decades of involvement with the medical profession. The occasion was an initiative, taken by the state of California, to expand training in primary practice, including the establishment of new medical centers. Chancellor Albert Bowker of the Berkeley campus did not fail to notice this, and set in motion a campus effort to capitalize on this initiative in the most appropriate way.

The Berkeley campus had never had its own medical school, and the establishment of the San Francisco medical campus had probably captured that turf and assured that Berkeley would not have a major one. Certainly, many on the San Francisco campus embraced the idea that it was the state medical center of the Bay Area. Furthermore, many Berkeley faculty did not especially want one, on grounds that in institution after institution the medical school had become a huge budgetary tail that wagged the academic dog of the university. Taking these pressures into account, Chancellor Bowker appointed a planning committee to explore and recommend some kind of joint Berkeley-San Francisco training program that would take advantage of the strengths of both campuses. The committee consisted of faculty from both campuses, and I was asked to join. I was somewhat perplexed by the invitation, but I suppose my training in psychoanalysis and my work as therapist at Cowell Hospital might have had something to do with it.

The line of thinking that emerged was that students in such a joint program would complete a degree in some academic major at Berkeley (along with pre-med work) and then transfer to the San Francisco campus for clinical training. It was assumed that the Berkeley years would broaden the transfers' background and would emphasize courses in the humanities and social sciences and thus supplement in some degree the "straight-

arrow-nut” model of medical education. On that point there was consensus among the representatives of the two campuses. When it came to details, however, consensus broke down and wrangling commenced. Berkeley faculty argued that the non-medical component should be circumstantial, not cosmetic, whereas San Francisco faculty argued that pre-medical courses should in no way be sacrificed, and suggested that Berkeley’s version of pre-medical courses was sometimes too “soft” or “academic” or in some other way not adequate for a pre-medical agenda. I sensed the danger of paralysis in the planning group, and began to argue that if the goals of a joint program were to be realized, an additional, a full academic year had to be added. Some San Francisco faculty scoffed at the idea, saying that no prospective medical school student would apply to a program that added an extra year to an already very long apprenticeship. They pointed to the difficulty and sometimes the failure of some five-year engineering programs because they could not draw enough students. A few allies and I continued to agitate, however, and ultimately won the day.

I will not trace the fortunes of that experiment beyond saying it has been a small but very successful enterprise to the present. The prospect of an additional year did not appear to be a deterrent. Applicants were strong and many were “mature” students who had pursued other paths (such as service in the Peace Corps). Generally they have performed better academically and found more attractive residencies than San Francisco students on the more conventional track. All that is a source of pride on my part, and I thoroughly enjoyed winning my way in the face of opposition.

Enter Erik Erikson and Psychoanalysis Again

In the second year of my chairmanship a happy event occurred. Erik Erikson retired from Harvard and moved to Marin County. He had had a history in the Bay Area, both with the Institute for Human Development on the Berkeley Campus and with the San Francisco Psychoanalytic Institute. As one way of re-incorporating him into the area, Bob Wallerstein created an informal seminar of psychoanalysts and academics that met periodically in the evening in San Francisco. He invited both Erikson and me to attend. I believe I had not yet met Erikson, though I had read most of his works at the Institute and on my own. I had also developed a friendship his son, Kai, a sociologist at

Yale. Early in the Wallerstein seminar Erikson and I were drawn to one another, and drifted into private conversations in the break periods.

After a short while we began arranging meetings on our own. Most of these were for lunches at the Berkeley Faculty Club, after which we would amble around the Berkeley campus for extended conversations. I am not certain what drew us to one another. We were both trained in psychoanalysis, but both had some distance from it. His extensions of psychoanalysis in historical and social directions were both innovations in and breaks from that tradition. I regarded my attachment to psychoanalysis as selective, not total. But I have speculated that there was more at work. My own father had died recently. In the last part of his life and in my adult years he and I maintained a basically positive relationship with one another, but at some distance. One of the discoveries I made in my personal analysis was how deep my love for my father was. By that time, however, he was an old and aging man, and I could not easily express this closeness. In a way Erik filled an important role. He was nearly the same age as my father and was a kind and loving man himself. I welcomed and was not threatened by him. As for him, I was about the same age as Kai, and I can speculate that perhaps it was easier for him to express closeness and less ambivalence toward me than toward a real son.

Our relationship thrived, and he, especially, opened up. He revealed himself as a vain as well as a kind man, and indicated to me how wounded he was by what he regarded as both neglect and unfounded criticisms of him and his work. At times I almost became his therapist. Our relationship also expanded to include his wife, Joan, and my wife Sharin, and we sometimes visited as couples. A few months into our friendship, Erik began talking about some joint intellectual project that we might undertake. I responded positively. At first we didn't know what that collaboration might be, but we gravitated toward organizing a conference on the topic of psychodynamic development in adulthood. Of course his work on the stages of life put him in the center of that topic; in fact he was one of its innovators and leaders. Moreover, the topic of the life cycle had risen as an explicit interest for some demographers (cohort analysis, age stratification), psychologists, sociologists, and historians. So we went to work forthwith, organized our thinking, identified possible participants, and secured support from the

Western Center of the American Academy of Arts and Sciences for a meeting at the Center for Advanced Study at Stanford. It was held in the summer of 1977, just before I was about to depart for two years abroad. I agreed to take the leadership in putting the edited volume together and successfully arranged for its publication with Harvard University Press. Erik gladly gave me this leadership, claiming that in retirement he had “no infrastructure.” When the book appeared, under the title Themes of Work and Love in Adulthood (Smelser and Erikson, 1981), I experienced its publication as a happy ending to one of the happiest chapters of my life. Sometime later Erik returned to the East Coast and I lost contact with him, but continued to treasure his friendship.

An Intellectual Misadventure and Setting It Right

In terms of my scholarly research (such as it was) during the two years of my chairmanship and the year following (1974-77), three threads stood out:

The first was my intellectual focus on developmental and life-cycle issues. By way of background, my doctoral dissertation had been in part a treatment of the changing definitions of childhood and adolescence in Britain during the Industrial Revolution. Also, I had roamed through the psychoanalytic contributions on developmental issues in my many years in the psychoanalytic institute. And then there was Erikson, his work and his friendship, so important to me in the late seventies. One of my vows was to turn my next major phase of work into some applications of the life-cycle perspective.

Second, I was yearning again to immerse myself into a major research project resulting in a major book. My work on the methodology of comparative analysis had not satisfied this urge. In that context I decided to return to Victorian Britain, and to study the spread of mass education to the working classes. I had written on this topic briefly in my dissertation, noting the increased attention to the education of children and young people as their lives were being transformed by changes in work and family life. Now I would make this subject explicitly an age-grading issue by analyzing it within the framework of the changing institutionalization of childhood and formal education. The result would be another major historical monograph. In connection with this intellectual plan, I co-authored an article with a graduate student on how work, family and education changed their contours—in relation to one another—in nineteenth-century Britain (Smelser and Halpern, 1978). In addition, my contribution to the Erikson volume was a

survey of how the social structuring of intimacy and work had evolved differently in British and American society during the nineteenth century (Smelser, 1980).

Third, I had resolved—in the long shadow of our glorious sabbatical of 1973-4—to go abroad again. The opportunity that presented itself as the most attractive was to take a turn as director of one of the University of California's Education Abroad Program's centers. For a variety of family and language considerations—our children would be in their early elementary-school years, and the only conceivable language other than English that I could work in would be French, and that would be laborious. Such an appointment was for two years, but because it was within the University of California, those years counted as full service to the University, and I would not have to apply for independent funds or take sabbatical leave. The administrative side of the job involved supervising (with an associate director) the academic work of approximately 135 University of California students spending their junior year in one of a dozen universities in England, Scotland, or Ireland, and translating their work into UC credits. I knew this job entailed administering a small staff in the London Office, traveling from campus to campus to visit students twice a year, approving students' courses and acting as a friendly dean-therapist for students as the occasion demanded. I bravely assumed that I could do all this and also move ahead on my research trajectory as well. Late in 1976 I applied for and was accepted as director, for a term to begin in the summer of 1977.

In accord with this grand plan, I began reading as much as I could on the chosen topic in my years as chair and the following year. Mainly I tried to cover the literature on age grading, the life cycle, and the historical literature on working-class education. But as I proceeded, I came to realize that the frameworks of these literatures simply weren't fitting together. True, education was about childhood, but the British agenda for working-class education was being fought out on other grounds. The education of children was mainly a pawn in the dynamics of class domination and class conflict, as well as religious and regional warfare. The literature of socialization and age grading seemed remote from those adult-based conflict groups fighting for their own status and for the minds and hearts of the nation's children. In a word, my historical subject matter was turning its back on my interpretative framework. I did the only thing I could do. While sticking with working-class education as the main topic, I began to desert the age-

grading frameworks and to become a sociologist of class, religion, region, and political conflict and its management.

Abroad Again

In the late summer of 1977 my family took up residence on a charming Highgate street called The Grove, and enrolled our children in St. Michael's School (originally one of my working-class schools but long since a state institution in a wealthy suburb). I installed myself in the EAP headquarters on Strutton Ground, a small market street off Victoria Road not far from Westminster Abbey. This began two years of a different life. I did not return to Berkeley at all, and re-visited the United States only once on a trip to Princeton, where I had been invited to accept a joint appointment in the sociology department and the Woodrow Wilson School.

From the beginning the Directorship of the EAP (1977-79) brought a surprise. I will confess that in anticipating the role I put my official duties as supervisor of UC students as secondary, even though the University of California defined it as a full-time position. I did not intend to shirk my responsibilities, but my more important agenda was my own research, and I intended to give as much time as possible to it. The surprise was how much I came to enjoy the students themselves, and how my orientation toward them evolved from a sense of duty to a sense of positive fulfillment. The students themselves were in many ways special. By EAP policy they had to have a strong academic record in order to be selected, and, in addition, I came to believe that they were some kind of special class by virtue of taking that plunge of going abroad. I learned from their biographical statements that a large proportion had had an international motif in their family life—children of military parents stationed abroad, parents who worked abroad, or travel abroad. All these factors made them interesting, engaging young people.

In addition, the sojourn abroad seemed, in general, to bring out the best in the students. It was a liberating experience for most of them, a chance to be free and to grow. The most significant part of this was being away from their home collegiate routine and further away from parents. Many were emboldened by this, and though I did not have a quantitative count, I observed that many of them gained more definite life direction during their stay. Unconsciously I played into this, I believe, by taking on role of benevolent parent or uncle, not stern authority, even though I was evaluating their

academic experiences for their home campuses. Students seemed to like this style; I was sometimes teasingly called “daddy” on the occasions I took them out to dinner on my visits to their British campuses.

At the same time a darker side of the years abroad became evident. All the students were immersed in a new, foreign environment, much as they had been in leaving home for college. Such transitions involve leaving family, loved ones, and friends, and taking up a social life with strangers, encountering foreign ways, and forming new relationships. A minority of students experienced loneliness, social paralysis, and episodes of depression. A smaller minority experienced a tragedy back home—a family death or illness, for example, or a parental divorce. These came to my attention, because the typical response to these was to want to resign from the EAP program and return home to help or save someone. This urge was in part spurred by their guilt, and the fact that they were in a selected, elite program probably intensified those feelings. As a recently graduated psychoanalyst and with a history of student counseling I found I could play a helpful role. I never defined my role, either to myself or to troubled students, as a therapist; I had long-since learned how inadvisable it is to mix the roles of teacher and therapist. However, I lent a sympathetic ear and would occasionally attempt to introduce a reality check by asking the troubled student who and what they could realistically save by withdrawing from the program and suggesting, as an alternative, that they return home for a couple of weeks to help their family situation, but then return to their studies abroad.

As I reflected on the ambivalent nature of students’ participation in the education-abroad program, I began to think more generally about the process, and to compare it with other kinds of temporary and finite moratoria in life. Of course I had had my own memorable journey as a youth spending a season (my own truncated version of the “junior year abroad”) at the Salzburg Seminar. That experience, even so long in the past, was still very vivid for me. I began to wonder, in only partially articulated ways, if the year abroad was a special case of something more general. This line of thinking stayed with me after returning home, though it was latent most of the time. It came alive again in the 1990s, when I took over the directorship of the Center for Advanced Study in the Behavioral Sciences at Stanford. That institution provides a yearlong refuge—a kind of year abroad—for scholars in an atmosphere of maximum freedom to realize themselves

in their research. The dynamics seemed to be similar to the undergraduate experience. About the time of my retirement from the Center (2001), I decided to make this topic of the “odyssey experience” a major research endeavor and ultimately produced a comprehensive book on the topic (Smelser, 2009).

One of the high moments of my directorship—and I report this seriously—came shortly after Thanksgiving, 1979. The UK/I Center had a long tradition of inviting all 135 students in the program to the homes of the Director and Associate Director in London for Thanksgiving Dinner—a welcome event, as daylight hours were shortening into the winter months and as the euphoria of the year’s beginning was settling into realism and perhaps letdown. We paid the students’ way to London if they could make it. About 80-90 typically showed up. To prepare the dinner was an enormous enterprise of inviting, shopping, organizing help, cooking, welcoming, and entertaining. My family and that of my Associate Director, Alan Nelson, were traumatized by the preparation and execution of the dinner, happy as it was. As a kind of relief and catharsis, I sat down over the next several days and composed a long spoof-letter on the whole occasion, containing many truths and many exaggerations and near-lies, and written in a style satirizing staid academic-bureaucratic reports. I experienced creating this like something my father or one of my brothers would have written in a seizure of satiric humor. I sent this “report” to the Santa Barbara office, where it apparently created general hilarity. It has become a kind of historical document, shown to all incoming Directors and Associate Directors. When I was pulling together my collected reflections on the University of California (Smelser, 2010b), I persuaded the University of California Press to include it along with my more sober University reports (below, pp. 00-00).

Even though my role as Director of the EAP engaged me more than I anticipated, I also involved myself in the large research endeavor on British working-class education. I would do this as I could, bicycling from Strutton Ground to the British Museum Libraries in free afternoons, and burying myself in the mountains of literature. Mainly I spoke my research notes into a dictating machine, sending the tapes back to Berkeley where my secretary—retained on a research grant—could transcribe them. I did no actual writing, but had one category of dictation called “idea pages” that, when transcribed, became raw material for later drafting. Intellectually I found myself

evolving toward a revised theoretical perspective. In my dissertation I had treated the process of structural differentiation as a (relatively) smooth sequence of social change, and some readers of my thesis-based book pointed this out as a kind of criticism of my work—that is, that my interpretations were too benign. Whatever the merits of that line of criticism, the history of working-class education certainly did not appear to be smooth. It was rocky, with religious, political, and regional forces constituting the greatest array of obstacles that, as a rule, opposed educational expansion and reform and forced growth, in most cases, to take the form of minimal compromises. I did not formulate this model of change until much later, but it is what lay behind my ultimate choice of title—Social Paralysis and Social Change—for the monograph (Smelser, 1991a). In all events, I made significant progress on the research phase of the project, though I would not have predicted that it would be more than a decade before the book would see the light of day.

My sociological life was kept alive in another way. By the late 1970s my reputation as a sociologist had solidified among other sociologists. And because in my role as travelling director of EAP, I was fair (and free) game to lecture on the campuses where our students resided. There were ten such campuses, and I lectured on sociological topics on most of them, as well as others, such as Oxford, Cambridge, LSE, and Reading. In addition, my family and I would travel abroad (in another camper) during vacation periods, and on these trips I would also lecture, mainly in Germany and Italy. All this continued to remind me that I was still a sociologist in a world of sociologists.

The two years abroad were as magnificent for our family as 1973-74 was, though in a different way. Our children's education in the primary years was positive and memorable for them, and we ourselves became involved in an active social life in Highgate, largely with parents of our children's schoolmates at St. Michael's School, as well as some interaction with sociologists at the London School of Economics. Some of these became friends for life. We also had memorable travels and narrowboat canal adventures around the British Isles and camping adventures on the European continent. Visitors from Berkeley and the Education Abroad Office also kept us in touch with reality in California. Yet in some ways our return to Berkeley in 1979 was like the end of a two-year dream.

A Return to "Normalcy"

One little principle of my academic life is that when I have been away from Berkeley, the routines of research, teaching, supervising graduate students, and administering for a sustained period, I have become a sitting duck for assignments I might not otherwise have taken. I believe that I agreed to take my first term as chair because I knew how free I was going to be on the sabbatical year that preceded it. I later agreed to serve an additional year as chair just after a productive and enjoyable sabbatical year in New York City. I was also vulnerable after those two “free” years as Director of the EAP. When I was asked to chair the important Educational Policy Committee of the Berkeley Division of the Academic Senate, beginning in the fall of 1979, I took the bait.

Most of the work of that committee is important but not memorable, consisting of routine reviews and approval of projects and educational innovations on the Berkeley campus. It seldom rejects any of these, which in all events are generally well conceived and helpful to the campus. I would like to report on one issue of historical moment, however, that also reveals something about my educational philosophy.

For decades, dating back to the surge of nativistic, anti-Bolshevist fever in the 1920s, the Berkeley campus had two courses required for graduation: one in American History and one in American Institutions. For a long time a course given in the history department had met the first requirement and a kind of high-level civics course given in the political science department had met the latter. However, over the decades other departments had petitioned that they could offer courses that legitimately “counted”, and many of these were approved, one by one. For example, my own department offered an upper-division course in American Society and secured its approval as a means of meeting the American Institutions requirement. By 1980 the number of approved courses had reached almost 80 in number. Some of them were highly specialized. I heard one jest that a course on the history of the Big Band Era met the requirement. One recent development was that each of the Ethnic Studies programs had petitioned that two courses in the curriculum of each (e.g., African-American history; Asian-American society) count as ways of meeting the requirement.

During my term as chair this situation came to the attention of the Committee on Educational Policy. The Committee’s reaction to this was generally that the requirement had been watered down over time to the point of meaninglessness. It seemed apparent to

me, moreover, that it had become a mechanism for attracting students into courses that would meet the requirement and thereby increase the numbers of enrolled students (a relevant fact in calculating departmental budgets). I regarded this as a kind of corruption of the requirement. A number of us, noting that almost all high schools offered courses in American history and civics, proposed that those courses should normally meet the requirement; if a student had not completed these, however, then he or she would have to take a relevant course at the college level. I pressed the committee to approve this, and to present it to a general meeting of the Berkeley division for approval. As committee chair I did so at a spring meeting in 1980. It occasioned a brisk debate, with a coalition of members of the history department and the ethnic studies programs strongly opposing our reform. Our recommendation passed handily enough, but the next day the Daily Californian quoted a member of the ethnic studies program accusing me personally of being “racially insensitive.” The accusation led to no further consequence, I believe, but it told me something of the political dynamics of the situation. A decade later, I opposed the imposition of an American Cultures requirement on the same grounds—that it would become an arena of competing for students as well—and got into deeper trouble over that stand (below, pp. 00).

The second event on my re-entry was not anticipated. Some months after my return to Berkeley Ed Stanford of Prentice-Hall publishers paid me a visit. During my term as sociology series editor he and I had become good friends. His visit was to invite (beg, actually) me to prepare a major introductory sociology text. He also offered some attractions—the promise of significant royalties including a handsome advance, research assistance, and generous editorial contribution by the publisher. He also had a formidable list of arguments why I should do it. I think he was a bit disappointed that he could not use them, because I accepted immediately, almost impulsively. I cannot recapitulate why I did so, to this day. There were plenty of reasons for not taking it on, notably that I was very far away from completing my British primary education project.

I started work right away, assembling all the accumulated knowledge of sociology over my decades of reading, teaching and editing. The book was published in less than two years (Smelser, 1981). I mention four unique features. First, I dictated and had my secretary transcribe the first draft, then searched (and had my research assistant search)

for materials to fill known gaps. This was very efficient, but dictating the text of each chapter was exhausting, because I had to organize such masses of material in my head for each chapter. Second, I insisted from the beginning that I would not prepare the book for a really mass market, namely community college students. I dreaded watering down (worse, dumbing down), and made my position clear at the beginning, though I acknowledged that clarity of exposition was essential. Third, and in the same spirit, I declined much of the publisher's offer of editorial assistance. I learned they wanted this to be a "managed text," which I also considered dumbing-down. Fourth, in a gesture toward readability, I decided to begin each chapter with a colorful anecdote. For example, in introducing the chapter on stratification, I described the cultural and psychological difficulties I (as an American) had in dealing with my college servant at Oxford. I reported hiding my shoes from him because I couldn't tolerate the idea of his polishing them every morning, and his scolding me for hiding them from him.

The text sold well, and prompted the publisher to request a second edition almost immediately. In all, it enjoyed five editions, the first in 1981 and the last in 1995, and was translated/adapted into Italian, Russian, and Mongolian. (A Mongolian sociologist visited me recently, and told me that every Mongolian sociology student knows my name. In appreciation he gave me a trinket bearing an image of Chinggis Khaan, which adorns my key-chain.) By the third or fourth time around I was thoroughly bored with revising. After the fifth edition appeared I asked that the publisher either discontinue it or recruit another sociologist to revise it and be listed as co-author.

Four items were on my agenda, then, as we resettled in Berkeley: the Senate assignment, return to teaching, the text, and the historical monograph. These items seemed enough, at least for the next several years. But matters took several unanticipated new turns in the coming decade, all of which involved service to my university and to many external groups and agencies. I have come to call the 1980s my decade of service, and I devote the following chapter to its complicated journeys.

Chapter 7

A Decade of Service—1979-89

There is a principle in academia, and probably in much of the rest of the world. It reads as follows: If you want to be asked to do things and involve yourself in the wider world, the main way to do this is to have done well in some prior similar assignment—to have done the work; to have produced results; to have pleased or calmed, not offended others; and to have influenced an intended audience. If you do not want to do things, you refuse them, butcher them, offend people, or otherwise fail. My life in the 1980s followed a variant of this principle. I did not seek out assignments, and did not direct my efforts in committees and commissions in order to receive further invitations, but in sphere after sphere these invitations and responsibilities came my way. This history had its gratifying side, or else I would have said “no” more often. But at the same time, over the years of the decade I came to experience increasingly strong feelings, indeed desperation, that I was shunning my first love—academic scholarship—and I took a dramatic step (as in 1973-74) to set things right at the end of the decade.

“Mr. Report”

My return from sabbatical leave initiated a pattern that would continue for decades: when a major—often “impossible”—committee or kindred body was deemed necessary and was launched, I was asked not only to participate but also to chair it.

Graduate School of Education. The first of these assignments concerned the general status of the Graduate School of Education at the University of California, Berkeley. This had been a body in trouble and hounded for its troubles for some time. It had been reviewed in 1977, with many negative features cited and many reforms advanced, by a subcommittee of the campus’s Academic Planning and Program Review Board. Two additional dismal reviews appeared in 1978, one by a subcommittee of the Committee on Academic Planning and one from a subcommittee of the Graduate Council. Other Senate bodies had commented on these, including the Committee on Educational Policy, during my chairmanship. The tone and diagnoses were consistent. The school was found to be programmatically fragmented, lacking in intellectual mission, wanting in administrative leadership, checkered if not inadequate with respect to faculty quality, and suffering from neglect on the part of the central campus administration.

Over five years the school had had five acting deans, and most recently serious negotiations with a permanent dean had begun but foundered on disagreements about budgetary issues between the prospective dean and the Chancellor's Office. In some desperation the Chancellor's Office appointed a small commission, consisting of chairs of three Senate committees. As chair of the Committee on Educational Policy, I was asked to chair this body. We were given adequate funds and staff to proceed expeditiously.

Our deliberations confirmed the earlier review bodies' conclusions, if anything finding the school in even greater disarray than those investigations did. Toward the end of our report we systematically considered thirteen separate lines of reform of different levels of profundity. Defending our reasoning as we went along, we concluded that none of the thirteen, either alone or in combination, seemed to promise a productive way out of the school's troubled state. With a heavy heart we concluded that the Berkeley campus should make research and teaching in higher education a matter of high priority, but that the Graduate School of Education should not be a part of that effort. In a word, the school should be discontinued (see Smelser, 2010b [1981]). I was the one who pressed that final conclusion strongest, and I had to spend a great deal of time and effort persuading one of the other two members of the Commission to go along.

That recommendation was a bombshell. It is difficult to kill anything in a college or university, most of all a large, long-established body (Smelser, 2013b). The recommendation obviously threw the administration, faculty, students, and staff of the school into a state of consternation. When I went to Chancellor Mike Heyman, a day or two before the report's release and relayed our conclusions to him, he turned ashen and said immediately that he couldn't and wouldn't discontinue the school. His motives were frankly political: how can you kill—and get away with it in Sacramento—the only major public graduate school of education in Northern California? Other voices were friendlier to the report. In particular, the extremely powerful Committee on Budget and Interdepartmental Relations (the faculty's central personnel committee) endorsed it and began to look around the campus for places to re-locate the school's faculty members. In the end Heyman's response fell short of our recommendations. He launched an aggressive search for an aggressive dean, and found one. That did not solve the school's structural and educational problems. Later the new Dean told me that he did not agree

with our recommendations, but found our report central and valuable in his efforts. I was not popular in Tolman Hall, where the School of Education was housed. For years I jested that I would not go near that corner of the campus for fear of being headed off at the pass.

University of California Press. A few years later, I received a call from the Office of the President, a request to chair a systemwide committee to evaluate the University of California Press. Though a feature of the University of California from an early time in its history, it had never been reviewed since its establishment in 1893. I never learned why the Office of the President decided to review it at this moment. I do know why I was chosen to chair the committee. James Clark had been appointed its director a few years before. I had met him almost on my first day at Berkeley in 1958, when he was a publisher's representative from Prentice-Hall. He had heard of me and my scholarship. We became friends almost immediately—which is saying something, given the general relations between academics and publishers' agents. After he became director he leaned on me for advice frequently. I am certain he pressed vigorously for me as chair of the review committee. I suppose it was also symbolically significant to appoint a University Professor to head a University-wide review committee.

We wrote a very thorough report. We surveyed leading scholars, many of them UC Press authors, about the reputation of the Press; we interviewed members of the editorial staff, budgetary staff, and authors who had been published (and a few whose manuscripts had been rejected). I felt the work of the committee was very important. We analyzed and made recommendations on the balance between scholarly and trade and "coffee-table" books, warning of the seductiveness of the latter (and the attendant compromise of the Press's historic mission). We analyzed the composition and role of the Press's editorial committee; recommended expanding the "outside" members of the governing board; called for enhancing international operations; called for developing standards for journal publications; and reviewed and analyzed the problematic relations between the Berkeley and Los Angeles offices of the Press, an anomalous feature at the time.

I was determined to draft the report myself. My staff associate on the project, Lynne Withey from the Office of the President (later Director of the Press itself), was

stunned at this, thinking that it was “natural” that the staff should do the drafting. I learned later that Clark took the report very seriously, submitting every one of its recommendations to his staff and exploring as many ways of responding to them as they could.

On this issue of drafting, I ask readers to indulge me in one quotation from the report. After pointing out that market and other conditions were pushing the Press in the direction of greater reliance on trade publications (and less on research monographs), I entered the following prose:

. . . if we may use a theological metaphor, the trade side of this mix constitutes a potential temptation into a life of sin. The particular sins involved, moreover, are two of the deadly ones, covetousness and pride. One of the main motives for the move toward trade books is to generate revenues for a starving press, but if this proves successful in that regard, hunger is likely to change into greed. Furthermore, the “big books” generate more splash, more glamor, and a more immediate sense of self-importance and self-regard (Smelser, 1985a, p.5).

I then went on to stress the threat of this “greed complex” to the traditional mission of the press, namely to support knowledge and advance scholarship in the academic world. I do not claim that this flowery prose is typical of my writing, but I daresay it is rare in the volumes of dry committee reports that the academy churns out.

Lower-Division Education in the University of California. In the mid-1980s a dismal wave of criticism swept over American undergraduate education. This was not novel; the “decline” of both liberal and general education has been a perennial theme in the higher education literature, with much handwringing about academics’ desertion of their calling in favor of prestige-seeking through research and publication. I cannot explain the timing of the onslaught of the 1980s, but three separate reports appeared—one from the National Endowment of the Humanities, one from the National Institute of Education, and one from the Association of American Colleges. The language of the reports was strong, and phrases such as “unhappy disarray,” “loss of integrity,” “diminished vision,” “majoring in narrow specialties,” and “a vacuum of educational leadership” appeared. Their calls for reform were very different from one another. One

called for a revival of humanistic learning, another for greater involvement of students in the learning process, and the third for imparting specific skills and modes of inquiry.

Colleges and universities could ignore this outpouring of abuse only at their own peril, and the University of California was no exception. Its president, David Gardner, felt it necessary to respond affirmatively and aggressively. In 1985 he proposed a major, university-wide task force, to be dedicated to diagnosing problems and suggesting reforms in lower-division education (the first two years) for the entire UC System. His focus was the right one, for the first two years have long been regarded as the most problematical ones, marked by huge lecture courses, chaotic scheduling, unavailability of classes, and discussion sections and labs conducted by temporary faculty and teaching assistants. Yet budgetary limitations, faculty unavailability, and faculty unwillingness to give high priority to teaching large lower-division courses constituted resistances to reform. Other historically specific problems also faced such a task force, for example, the recent atrophy in transfer students from other segments of California's higher education system to the University.

When I received a call from Calvin Moore in the Office of the President asking me to chair the proposed task force, I was initially disposed to decline, mainly because of the magnitude of the undertaking and because I truly wondered what of value could come of such a commission. What made me accept in the end was my respect for and friendship with David Gardner. We had been in the student-protest trenches together in the 1960s—he at Santa Barbara and I at Berkeley, and he had interviewed me once or twice about the Free Speech Movement years in connection with research he was doing on that era. Gardner, I knew, badly wanted me to take on the task. It also seemed symbolically important that a University Professor chair that university-wide enterprise. Also, I had recently completed a term as chair of the Berkeley Division of the Academic Senate.

Fortunately I did not have to determine the membership of the Commission; it was handed to me. I knew what artistry had to go into its recruitment, and I had neither the knowledge nor the leverage to do it. Every campus had to be represented; there had to be a spread of members from the natural sciences, social sciences, and humanities; there had to be a mix of faculty, administrators, and student representatives; and all

members had to have had a history of some kind of concern with undergraduate education. All this meant a large committee membership—it turned out to be twenty—and this brought more problems of manageability for the chair. Happily the Office of the President supplied the task force with ample staff and clerical support.

By this time I had had enough experience with committees (see Smelser, 1993c) to know that this kind of assignment required more than the usual initiative and leadership on the part of the chair. I could not in any sense let the work of the task force “happen.” Accordingly, at the first meeting I explained that I had the following “understandings” about its work. I said I thought we had to have only a finite number of recommendations to have any impact (I was aware of the uselessness of “kitchen-sink” reports with dozens of recommendations); that we had to make the recommendations specific, not airy; that it would be foolhardy to recommend reforms that were not budgetarily or organizationally possible; that the report contain diagnoses, not just recommendations; and that our report had to be framed in an analysis and written in a readable style. Finally, I identified for the Commission the main substantive problems and possibilities that I perceived at this early stage of our work. I got explicit or qualified or nodding agreement from the group on most counts. At the end of the meeting Calvin Moore from the President’s Office took me aside and whispered, “You have this group in your hands.”

The task force worked hard in undertaking individual and group assignments and discussing issues soberly and responsibly. I was fortunate that all the members were civil in demeanor. Disagreements naturally appeared, but not wrangling. No sects or cliques or personal vendettas developed. I felt it necessary to keep contact not only with the whole group at meetings, but also, when needed, with individual members. Their suggestions for framing issues and setting the right tone for recommendations were almost always on target. The Office of the President had insisted that we submit an interim report in February 1986. I found that a nuisance, but we did so, summarizing the criticisms of undergraduate education that were abroad at the time, and pointing to programs and activities on different UC campuses that addressed these criticisms.

Then, one long weekend in the spring of 1986—our deadline for the final report was in June—I went with my family to our cabin in Twain Harte and, in a writing frenzy,

composed an entire final draft of more than 50 typewritten pages. I surprised myself with the easy flow of prose. The bulk of the report was analytical, and that set the stage for perhaps a dozen specific recommendations, under the following headings:

- Reforming curricula and programs, including a call for freshman and sophomore seminars, capstone courses at the upper division, and an increased emphasis on internationalization and globalization in the curriculum.
- Improving the quality of teaching, including assigning the most brilliant teachers to introductory courses, improving teaching evaluation, making more intelligent use of temporary faculty, improving the language capacities of teachers whose native language is not English, and improving the training of teaching assistants.
- Improving educational continuity, including better scheduling of courses and better coordination of courses with instruction taken by transfer students in other institutions.
- Reaffirming the general mission of the university as a haven for general scientific and intellectual enlightenment, mainly by giving proper balance to its multiple functions.

I was greatly relieved when, at its final meeting, the task force approved the draft, almost without change.

Unlike most reports of this sort, this one stirred immediate attention. President Gardner put it on the agenda of the Regents, where I appeared and carried out spirited dialogues on undergraduate teaching with Ernest Boyer, the national crusader for improving collegiate education, and with Bill Honig, California's Superintendent of Public Instruction (see Gardner, 2005). I traveled to several campuses to meet with groups of administrators and faculty, and lectured on the report at a couple of other universities. The Academic Senate gave it more than usual attention because I had just been elected Vice-President of the statewide Senate. Relevant legislators and executives in Sacramento took notice, since the University has had a long history of critical attention given to its undergraduate teaching responsibility in the state. One Senator, Tom Hayden, the 1960s Students for Democratic Action activist, praised the report and simultaneously lambasted the University of California for its decades of neglect and lack of reform.

Over time the impact of Task Force Report was mixed. In the short run the University moved to improve teaching assistants' language ability and training, and to introduce reforms that facilitated the transfer process. Over time the freshman-sophomore seminar has become a standard and positive feature of most campuses. Other recommendations appear to have moved nothing; examples are the deployment of the most brilliant scholars and teachers into large, introductory courses, and the improvement of the situation of temporary faculty. Speaking for myself, I found this assignment—and its results—a satisfying journey in troubled waters.

Two years later I began a similar journey, but it was short, troubled, and unsatisfying. The late 1980s was a turbulent period in the “multiculturalism” movement on the left. Its driving impulse was the idea that undergraduate general education in the United States—both historically and in contemporary times—was dominated in its coverage by a traditional Anglo-Saxon, Protestant, older male establishment. One of the targets was Stanford's history of Western Civilization course (“from Plato to NATO,” cynical undergraduates called it). On the Berkeley campus a movement developed to establish an “American Cultures” requirement that all undergraduates take on course from an approved list of courses that emphasized groups (minorities, mainly) whose histories and cultures had previously been slighted.

As part of this scene Chancellor Heyman established and appointed a campus commission on general education, somewhat parallel to my systemwide task force two years previously. He asked me to chair it, and because of my work with the previous body my appointment made sense. I didn't want the job but accepted it mainly out of friendship with Mike. He appointed a number of students to the commission as well.

Although “American Cultures” was not the main charge to the general education commission, it became clear that the item of highest priority in many members' minds (especially the students) was to recommend such a requirement. At the same time, the proposal for that requirement came up for debate and action in the Academic Senate. Without consulting anyone, I went to the Senate debate and spoke, as an individual faculty member, against such a requirement. I wasn't against diversification; as a matter of fact our previous task force had explicitly stressed internationalization and diversification of the curriculum as a whole. Instead I stressed is that an American

Cultures requirement was certain to go the way that the American History and Institutions had gone—namely corruption via the multiplication of courses and the competition for undergraduate bodies into courses that would meet the requirement.

In retrospect I am not certain why I made that statement in the Senate debate. Certainly it reflected a genuine opinion on my part, and I did not feel it compromised the work of the commission. In addition, however, I made a mistake in failing to diagnose the American Cultures issue as the serious—life and death, to some—matter was in that hot political context. Or, alternatively, at a deeper level, in effect I committed political suicide as the commission chair in making such a public statement. In all events the student members of the commission complained loudly to the Chancellor about my statement and the Chancellor complained to me and suggested I resign. I resigned in one minute, relieved to get out of this assignment, which I was reluctant to take in the first place. Because I resigned so quickly, a crisis was avoided. Heyman appointed a new, very good chair, and the fight over and ultimate victory of the American Cultures requirement proceeded without me. My prediction about the multiplication of courses in American Cultures has seemed to come true in the ensuing years, but I do not take any special pleasure in that fact. The whole episode was not one of which I am proud. I suppose I could have been more straightforward from the beginning and not taken on the assignment.

The University and the Self-Esteem Movement. In the middle of the 1980s an unusual social movement crystallized and caught the official attention of the California State Legislature. The movement was not totally uniform in aim and intent, but its central mission seemed to be this: if the self-esteem of citizens could be cultivated and elevated, this would work to reduce the incidence (and control the cost) of social problems such as child abuse, school failure, teenage pregnancy, crime and violence, chronic welfare dependency, and drug abuse. The “movement” gained special salience in California because it was embraced so fervently by Assemblyman John Vasconcellos, the powerful, long-time head of the Ways and Means (budget) Committee in the state assembly. He persuaded the legislature and Governor George Deukmejian to create a California Task Force to Promote Self-Esteem and Personal and Social Responsibility. That act and the movement in general were regarded with great ambivalence, ranging

from its welcome as a salvation to its dismissal as silly and ineffective. Many wondered whether government should be meddling so directly in citizens' private affairs. Be all that as it may, the creation of the Task Force gave the movement official sanction.

At one point Vasconcellos, the enthusiast, placed a telephone call to President Gardner, asking if there was some way the University of California might undertake activities relevant to the promotion of citizens' self-esteem. Gardner certainly didn't know how to respond, yet a request coming from such a powerful person in such a powerful place relative to the University could not be ignored. Gardner passed the request on to Vice-President William Fraser, a physicist and scarcely an expert on self-esteem. He didn't know what to do either and passed it on to Calvin Moore, a mathematician and even less likely an expert. In some kind of anguish, Moore called me, informed me of Vasconcellos' request, and asked me what the University might do. I said I didn't know, probably nothing, but I would think about it. I did, and came up with the following idea. The University can respond only by doing what it can do best: to take each of the social problems claimed to be caused by a deficit of citizens' self-esteem, and to make a serious, scholarly effort to assess the social and psychological research on causal role of diminished self-esteem in generating the social problem. I knew that a great deal of social-scientific research was relevant, and wouldn't it be appropriate if scholars undertook to assess it systematically and scientifically? I called Moore back and reported my thoughts, and within three seconds he said "Brilliant, Neil, you do it!" That moment for me was a personal confirmation of the organizational principle that no good deed goes unpunished.

I took on the assignment, mainly out of friendship with David Gardner and by way of expressing my dedication to the University, in this case helping it out of a politically very delicate situation. I did not promise to deliver anything specific, because I really didn't know what kind of relevant scholarly resources were available on the campuses. All I asked for was a budget to offer scholars a modest honorarium, to pay for an occasional meeting, and to provide me with some staff and editorial help. I was to be organizer of the project, editor of an intended volume, and author of an introduction to that volume. It took only three more seconds for Moore to grant that request.

I set to work, exploring faculty statements of interest in various academic units on all the campuses: departments, especially psychology and sociology; schools of medicine, social welfare and education; and relevant organized research units. I searched for scholars who had done or were doing research related to self-esteem and kindred psychological variables and their causal role in generating behavior that, when aggregated, has a role in producing the identified social problems. I was gratified to find significant numbers of qualified scholars. I brought a group together for an intellectual orientation, and for months thenceforward worked as editor to ensure quality, consistency, and comparability of their reports. We were able to put together a coherent volume, with chapters organized according to the types of social problems. I wrote an introductory essay for the volume, identifying the nature of the difficulties that arise in explaining social problems by reference to psychological variables and reviewing methodological problems of measurement, correlation, and causation. As might have been anticipated, the findings reported in the chapters, when considered from an applied social-science perspective, were modest. My key, summarizing words were “the news most consistently reported . . . is that the associations between self-esteem and its expected consequences are mixed, insignificant, or absent” (Smelser, 1989a, p. 15). I referred extensively to the findings reported in the individual chapters to document this generalization and to explicating the methodological reasons why that psychological variable, while important in social life, did not serve especially well in explaining complex phenomena with multiple causes. I regarded that conclusion as a scientifically responsible one, but it clearly did not bolster the claims of enthusiasts for the social importance of self-esteem.

One delicate diplomatic situation arose. I decided to ask Vasconcellos to write a “Preface” explaining the legislative history of the creation of the California Task Force to Promote Self-Esteem, and Andrew Mecca, the head of that Task Force, to write a “Foreword” about its work. I also decided, for reasons of state, to invite both of them to be listed as co-editors, even though I had done all the editorial work. They were both flattered. But both largely ignored the topics I asked them to write about. Vasconcellos wrote a rambling essay on his commitment to the self-esteem movement, and included a lot of his own personal history, which I regarded as irrelevant and bordering on

embarrassing. Mecca wrote an amateur essay on the importance of self-esteem in the history of Western thought. Both of them were inappropriate for the volume, in my estimation, and reviewers for our prospective publisher said the same. I wrote both of them politely asking each to submit another essay on the originally requested topic. Mecca obliged, but Vasconcellos returned a minimally edited paper.

At that moment I nearly panicked. I placed an urgent phone call to David Gardner, saying to him, “we have a problem,” and explained why. David, evidently not wanting the President of the University to become involved, said to me, “you have a problem” and asked me do the best I could. I did the only thing I thought an honest editor could do. A research assistant and I radically rewrote Vasconcellos’ paper, and I wrote a “Dear Vasco” letter accompanying it (we had been “Vasco” and “Neil” to one another for some time). It was the most diplomatic letter I have ever written. I explained everything painstakingly once again, but said, in the end, that we could not publish his version in the volume. I was stunned when he agreed, without reservation, to accept my version, because he had the reputation of being a strong and willful man. Why did he do this? Did he weary of my badgering? Did he have more important things than editorial fighting on his mind? Did he fall into a role of student being scolded by the professor? I will never know.

The University of California Press published the book late in 1989. I was by that time in New York City for the year on a Russell Sage Foundation grant, so I could not really follow reactions in California. I do know that Vasconcellos, in a press conference, hailed the report’s publication as a vindication for his cause. Reporters did not miss the discontinuity between that enthusiasm and the very qualified tone I took in my introduction, and a couple of them telephoned me and asked me about it. I begged off, saying that I was in New York, out of touch with things, and could not really comment. I think that reporters commented on the inconsistency in their write-ups. Gary Trudeau had one *Doonesbury* cartoon poking fun at an unnamed Vasconcellos for the contradictions between the book’s findings and his claims. From a personal standpoint this adventure into the politics of self-esteem was unique in my scholarly life, worth undertaking for my University’s sake, engaging, and occasionally comical.

I postpone telling about my report on intercollegiate athletics until next chapter, since it was written in the 1990s. But where did “Mr. Report” come from? In greeting me in public at a meeting in Irvine in the early 1990s, Jack Peltason, the Chancellor of that campus, announced, “here comes Mr. Report!” referring to all I had done. Since he and I were long-time friends, I took the remark for the compliment that it seemed to be, rather than the possible insult that I was spinning my wheels in the academic bureaucracy.

The Academic Senate

My stint with the Educational Policy Committee had scarcely ended (summer, 1980) when I was nominated and selected to be Chair of the Berkeley Division, to begin in the summer of 1982. This position also involves a number of fixed, largely routine and time-consuming functions—coordinating the activities of campus Senate committees, meeting routinely with the Chancellor, chairing the general meetings of the Berkeley Division, and representing Berkeley on the systemwide Academic Council and Academic Assembly. All these are important features of the University of California’s “shared governance” system, but produce few memorable moments. I report only two recollections.

The first has to do with managing the meetings of the representative assembly of the faculty on the Berkeley campus. This requires deft leadership at times, since the old Senate hands are masters of Robert’s rules of order, and know how to move effectively through tangled procedures to gain their ways. A second, almost opposite problem has to do with Senate sleepiness. My term of office (1982-84) was not an especially turbulent period in campus history, and on some occasions the general meetings failed to generate a quorum. This is always a challenge to leadership, because without a quorum the body cannot take actions, even routine ones such as approving committee reports. I invented a way of dealing with this. When a quorum did not materialize, I suggested to the gathered body of faculty that we make believe we had a quorum and take necessary actions, and bring them forward for approval at the next meeting at which a quorum was present. This mechanism was never challenged, and kept the Senate moving.

The second instance involved me in a major role in averting a campus crisis. In early 1983, Jeanne Kirkpatrick, US Ambassador to the United Nations in the Reagan

administration, was invited to deliver the campus' Jefferson Lectures. Her topic was "The U.S. Tradition of Human Rights." On February 15, at an early lecture, members from a group called SAINTES (Students Against the Intervention in El Salvador) stood up and shouted her down. The disruption was unanticipated, and police and campus administrators were unable to identify most of the disrupting individuals after the room had been cleared and the lecture resumed. The event led to an instant polarization of students and faculty on the legitimacy of the protest. I became immediately involved as chair of the Senate. I believed the disruption was an outrage, and politicked strongly with the chair of the Senate's Committee on Academic Freedom to declare the disruption as "prima facie evidence of a violation of freedom of speech." I did this not out of any partisan like or dislike of Kirkpatrick and the conservative policies she represented, but rather as one offended by the incivility of the disruption. My role was delicate and demanded a certain amount of strong-arming, because the Academic Freedom Committee was divided on the matter, and its two student members were opposed to the resolution.

The conflict immediately assumed an extra-campus, even national political dimension. The conservative chair of the board of regents, Glen Campbell, who was close to President Reagan (and Reagan was close to Ambassador Kirkpatrick), demanded that chancellor Heyman prepare a report on the event, including "an explanation of why this interference with free speech occurred, and what steps are being taken to impose appropriate punishment on the perpetrators of the Act." At a subsequent regents' meeting, Heyman spoke against Campbell's attempted interference and in effect invited the regents to remove him, not tell him what to do, if they were dissatisfied. The regents did not take that invited step, so an even greater crisis was averted. I had been invited by Heyman—as Division Chair and supporter—to accompany him to the regents' meeting. I sat next to him, and spent much of the meeting restraining this large and angry man.

One of my duties as Division Chair was to attend the systemwide Senate meetings as representative of the Berkeley campus. I became known in that setting, and subsequently was invited to serve a term of leadership—one year as vice-chair, and one year as chair—of the systemwide senate (1985-87). This was also demanding, and involved much travel to other campuses and to regents' meetings (the appointment brought two years of service as non-voting faculty member on the Board of Regents).

The first year (1985-86) coincided with the publication of my systemwide report on lower-division education, so I was especially well-positioned to coordinate Academic Senate action on some of its provisions, namely to rationalize and improve the transfer function of California Higher Education and to develop a core transfer curriculum in coordination with the other segments.

The chair and vice-chair also met monthly with the President and some other high officials in the systemwide administration immediately before the regents meeting. This was a comfortable role for me, mainly because of my already-close relationship with David Gardner. We saw eye-to-eye on most issues, and I had no difficulty in supporting his position on almost all items that came before the regents. One exception was my opposition to his initiative to move the Office of the President to downtown Oakland from its Berkeley campus location. I did not oppose moving from Berkeley as such, but thought that the Office of the President—a huge bureaucracy with a life of its own—should be located on or near one of the campuses as a reminder of the academic priorities of the University of California. More generally, I was quite active at the regents meetings, and individual regents increasingly directed questions to me as the “voice of the faculty” on important issues before them. I also became a kind of aesthetic darling of the board, because during the long and dreary periods of the meetings I would produce my signature, multi-colored, doodled-on Styrofoam cups, yielding complex patterns in Native-American artistic style. I made the mistake of presenting one of these cups in public view as a gift to my favorite regent, and this created an enduring queue of Regents waiting for a similar souvenir.

Two “Service” Episodes Derived from My Senate Activities

Academic Fraud. Academic dishonesty is not exactly common but it seems to be an ineradicable aspect of academic life. I became involved in this issue in the 1980s. A furtive enterprise located in Santa Monica had apparently been hiring graduate students to write term papers and selling them to undergraduates in California colleges and universities. On one occasion they became extra-aggressive and advertised their services by placing flyers on cars’ windshields in the parking lots of Long Beach State University. Since this activity was illegal, the University joined in a court action to prohibit the firm’s activities.

I was called into a Long Beach court during the trial as an “expert witness” who, as an established academic authority, would testify on behalf of the prosecution on how the marketing of term papers was a corrupting activity for students, for academic values, and for the educational system in general. I so testified before the jury. The lawyers representing the publishing outfit cross-examined me. Among other things, they wanted to discredit my expertise by demonstrating that I couldn’t tell the difference between a term paper, a research report, and a journal article. In that connection they handed me a written paper and challenge me to say which of these it was. By sheer coincidence I was familiar with the paper and its authors, having read it in a recent medical journal! I played dumb, and said I could not be absolutely certain but said that it appeared to be an article in a professional medical journal. So much for my moment of high courtroom drama. The proceedings, I learned later, resulted in a significant fine for the firm, but not outright prohibition.

As part of the “disclosure” aspects of the case, I was taken to the premises of the publishing “firm” and asked to read a sample of approximately three dozen “term papers” that the firm was marketing. By chance I came across one paper that was a commentary on my Theory of Collective Behavior. I experienced no pride in this discovery. It was a paper of ordinary quality, with some errors, a sort of paper that would have earned a grade of C or C- if I were grading it. This feature of mediocrity ran through most of the papers I scanned. From that fact I concluded that the firm did not cater to high-quality students but to mediocre and weak ones, perhaps out of apprehension that students submitting excellent pre-cooked papers might be called in by professor to discuss their work, an outcome that neither preparer nor purchaser of the term paper desired.

“Trying” a Chancellor. In 1986 Robert Huttenback, Chancellor of the UC Santa Barbara campus was removed from office for allegedly embezzling, with his wife, some \$170,000 of university funds. He was also brought to criminal trial. But matters did not end there. Protests continued to arrive at the Office of the President, objecting to the fact that he had not been discharged as a tenured professor of history on the UCSB campus. This continuing protest was a difficult one for the administration, because it involved breaking the expectation of tenure as employment with lifetime security.

Rather than act on the protest on its own, the Office of the President decided to convene a kind of “jury of peers”, i.e., an ad hoc Academic Senate committee to advise it on the suitability of continuing Huttenback’s professorial appointment. Apparently it was difficult to constitute such a committee of peers on the Santa Barbara campus. Vice-President Fraser asked me to chair a systemwide committee. We met in Santa Barbara, with Chancellor Huttenback present, along with his attorney, at all meetings. It was an unusually difficult assignment for me, but I took it on. One of the reasons for its difficulty was that I already knew Bob Huttenback socially. He and his wife had visited me as newly-appointed Chancellor of Santa Barbara when I was Director of the Education Abroad program in the UK, (we took them to a picnic lunch on Hampstead Heath) and I had visited with him a couple of times subsequently. I liked him as a person. To have him present when we were sitting in judgment on him was personally unsettling for me.

I will not recap the proceedings but report that in the end we recommended terminating his professorship. I personally felt his “crime” was sufficiently serious that it fell into the category of “gross moral turpitude” that is often invoked as legitimate grounds for discharging a tenured faculty member. I felt the committee’s decision was the correct line of action, but I also confess that I lost some sleep during the period of sitting in judgment on my peer.

A Re-link with Professional Medicine

In 1981 I received an unexpected call from Julius Krevans, Chancellor of the San Francisco campus of the University of California. He asked me to join with one or two non-medical academics in an enterprise of the American Board of Internal Medicine called the Subcommittee on Humanism. The Board had taken the initiative in re-defining the human and humane sides of medical practice—especially the doctor-patient relationship—as a kind late twentieth century reaffirmation of the “caring” side of the Hippocratic tradition. Some enlightened internists had expressed the view that the medical practice had evolved toward depersonalization in the past decades. They stressed team practice, new technologies, and the bureaucratization of care as causes. The subcommittee was charged to re-define the essential features of humanism in medical practice as precisely as possible. More novel and important, it was to specify

steps as to how to build these values concretely into the training of interns and residents and thus make this training “count” in the certification process. The subcommittee labored at collective meetings and ultimately produced a report that was adopted in many medical training programs. As a social scientist I was able to help identify systemic factors that played a role in the depersonalization of medical practice. I also argued that commitment to humanism was not only a personal and moral matter (which was mainly what the physicians stressed) but was undermined by systemic changes in the practice of medicine. The subcommittee terminated its work in 1984, but was re-activated in 1989 to revisit and revise the manifesto on humanism. For me the experience was an expanding one, and after an initial expectation that my role was going to that of a cosmetic outside member, I was gratified to have my voice respected by a proud and sometimes arrogant group of professionals. The episode began a period of more than two decades as consultant with ABIM.

A Policy Voice in the National Academy of Sciences

One of the initiatives taken by the administration of Ronald Reagan was an assault on government support of research in the social sciences. David Stockman was chief agent in this move. (I knew of Reagan’s sentiments about the social sciences; in an earlier California Press conference he had named “sociology and philosophy” students as central actors in the campus disruptions.) Soon after Reagan’s election as President in 1980, noises began emanating that cuts in social science funding as large as 75 per cent should be anticipated in the NSF and other agencies. The main justifying argument was that research in the behavioral and social sciences was not “useful” for the nation. This position generated a barrage of criticism in the national press, which pointed out how much the federal government depended on the data and research produced by these sciences. Nevertheless, the pressure continued, and the social sciences responded politically by creating the Commission of Social Sciences Associations (COSSA) as a national lobby of all the social sciences, even drawing in economics and psychology, which had traditionally been loathe to join in collective political enterprises with the others. That lobby continues its activities in Washington to this day.

The National Academy of Sciences shared in the alarm, and assumed an important initiative via the National Research Council’s Commission on Behavioral and

Social Sciences and Education (CBASSE). They launched a body called the Committee on Basic Research in the Behavioral and Social Sciences, and commissioned it to “assess the value, significance, and social utility of basic research in the behavioral and social sciences” (Adams, Smelser, and Treiman, 1982, v). The chair was Robert McCormack Adams, University of Chicago anthropologist; the study director was Donald Treiman, sociologist from UCLA, with a dozen eminent social and behavioral scientists as members. I was one of the sociologists appointed—recommended, I believe, by my lifetime angel, Gardner Lindzey.

The committee was, from the beginning, inclined to be a solidary body because of the hostile political environment in which it was born. An early dispute emerged, however, and that turned out to be a personal debate between me and Peter Rossi (another sociologist and, by coincidence the member of my undergraduate honors thesis committee who gave me such a rough time). At the initial meeting Rossi came on very aggressively and argued that the Committee’s report should be narrowly focused on the “applied science” emphasis of the behavioral and social sciences and deal with specific applications to specific policies. I argued for breadth in representing those sciences, with central emphasis on the importance of basic research. The debate was heated, and when it appeared that “my” position was emerging as the favored strategy, Rossi resigned from the committee in a huff.

We met several times and gradually worked out a report that contained an initial part on the nature, data, and methods of the relevant sciences, a second part on arenas of usefulness, and a third on different kinds of useful documentation provided by social science research. I argued from the beginning that it should be a partisan document on the usefulness of knowledge (we referred to our sciences as “a national resource”), but also that we take a “high road” of scientific discourse. Adams delegated drafting assignments to members of the Committee. I turned out to be such an ardent drafter and re-writer that Adams requested—through no supplication on my part—that I be listed one of the co-editors of the entire report, as well as co-editor of a supplementary volume of more specific, directed essays. I also turned out to be something of a peacemaker between the other two co-editors, who sometimes held conflicting views.

One never knows about the impact of such reports; one releases them on a given day and they disappear into a complex world of competing perspectives and arguments. However, as part of the collective efforts of the behavioral and social sciences, the early Reagan budgets evolved from draconian to merely damaging, and funding levels gradually began to revive in subsequent years. As for the committee itself, the National Research Council decided to extend its life, and commissioned me (and Dean Gerstein) to hold a conference at the Academy (and publish a volume) on selected past accomplishments of specific lines of social-science research (for example, on deterrence of crime), and write up accounts of these contributions from a historical point of view. It was meant to stress past contributions as a supplement to the present-oriented emphasis of the first committee report (Smelser and Gerstein, 1986). The publication was timed to coincide with the fiftieth anniversary of the publication of the important federal report, Recent Social Trends in the United States, and I wrote an account of the evolution of sociological thinking since William Fielding Ogburn pioneered that report (Smelser, 1986).

The Committee on Basic Research was to live a third life—this oriented toward the future—that was part of a larger enterprise of the National Academy of Sciences. Early in the 1980s, the National Science Foundation had asked the Academy to prepare a number of high-level, discipline-based reports identifying priorities for research for the coming decades. By the mid-1980s influential reports had been prepared for astronomy, mathematics, chemistry, and physics. At that time the Academy authorized a similar report—this one a more “impossible” one—covering all the behavioral and social sciences. As key actor in the previous two projects, I was asked to organize and chair this project, a much larger one. However, the powers-that-be recognized that I was stronger on the “macro” than the “micro” side of the social and behavioral sciences, and (I surmise) that the same powers wanted a co-chair who was a member of the National Academy of Sciences (I was not elected until 1993). They chose R. Duncan Luce, the brilliant quantitative psychologist from Harvard and later UC Irvine, whose work I respected but with whom I had never worked. Dean Gerstein, with whom I had become friends, was to continue as study director.

I was naturally gratified to be chosen to head this most important effort by the national social-science establishment, even though I knew the time and energy involved would be enormous. To carry out our work (and to symbolize our wider support, I think), we were also brought under the sponsorship of the Social Science Research Council and the Center for Advanced Study in the Behavioral and Social Sciences.

Two dilemmas faced the Committee from the beginning. The first was whether to treat priorities by academic discipline or by “problem areas”, many of which spread across disciplinary lines. The second was how literally to interpret the idea of “priority”. One model was to follow the astronomy report and submit a list of strictly ranked research priorities, to be undertaken in order, one after the other. I knew exactly where I stood on both issues. I wanted to make “problem areas” (hopefully interdisciplinary) central; to organize the report by discipline seemed to me to be too much like “horse trading” of research priorities among established political groupings. I also wanted, however, a discrete number of priorities (not listed in rank order but grouped), so we would avoid calling for supporting equally all things that behavioral and social scientists thought important to study.

At the first meeting of the Committee we had a long, complicated tussle that evoked, and ultimately solved, both dilemmas. Two members of the Committee, my dear friends Gardner Lindzey and Kenneth Prewitt, waded right in and said we should be very limited in our choices, and given highest priority to “recognized” and “scientific” topics, mainly survey research. (Their emphasis, though different in content, brought Rossi’s earlier intervention to mind.). I fought against this proposal, which I thought would limit the effectiveness of our work on account of its narrowness. Partly because I had thought through these issues, my arguments won the day, and helped define, early in its work, the mission and culture of the Committee.

Above all, the story of that Committee was one of hard work. It had expanded to approximately two dozen members, many of them academic stars. This demanded greater coordination and more dealing with large, delicate egos. We met as a whole committee perhaps six times. We sent out inquiries about “leading edges” of the behavioral and social sciences to approximately 150 journal editors and 600 researchers who provided suggestions for more than 1,000 lines of research. I reported periodically

on the work of the Committee to our sponsoring bodies and the Russell Sage Foundation. Systematizing the topics under almost 30 discrete research headings (e.g., markets and economic systems, family, and religion) was a lengthy and delicate process, and the farming out of drafting and revising proved a daunting enterprise. Finally, we formed a research committee for each of the chapter topics to prepare a more comprehensive account of research and problems in their areas. We coordinated their reports into a separate volume (Luce, Smelser, and Gerstein, 1989). At the time of publication of the final report, the National Research Council prepared a grand celebratory event at the National Academy of Sciences building in Washington, DC. The report came to the immediate attention of relevant funding agencies. It also received notice in the professional social science associations, several of whom scheduled sessions on it at their annual meetings. (I spoke on the Report at the annual sociology meetings.) As far as I could discern, the report did not ignite any turf wars among the academic disciplines.

By and large, the work of the committee was harmonious, despite the fact that attempting to establish priorities of any kind typically occasions jurisdictional struggles. My relations with Luce were generally cooperative, but we had a few rough patches. First, he was generally more prone to be dismissive of research that did not measure up to his stern standards of methodological rigor, whereas I leaned more to inclusiveness, which I justified by reference to the broad charge and mission of our Committee. I think he regarded me as scientifically “soft” at times. Second, at a late stage when we were framing recommendations, he seized upon the idea that we should make a major recommendation to separate biological funding from behavioral and social science funding in the National Science Foundation. He argued that this structural splitting would work to the advantage of the latter. Knowing this would be controversial, I tried to persuade him not to take that position, explaining that I feared that the political tail would wag the scientific dog in the report’s reception. I managed to talk him out of the idea. Finally, we had a jurisdictional bout with respect to order of editors’ names. I was for alphabetical order by last name of all the editors, which would have been Gerstein, Luce, Smelser, and Sperlich (the last was a most helpful staff member). Luce wanted the staff members to be last, which would have yielded a listing of Luce, Smelser, Gerstein and Sperlich. I didn’t care about the status listing of editors, and argued my case strongly

and, in the end, persuasively. Luce capitulated, but I don't think he liked the ultimate ordering.

Troubles at Harvard and Yale

Harvard. In 1980, Henry Rosovsky, my dear friend from the Society of Fellows and my early Berkeley days and now Dean of Letters and Sciences at Harvard, was on the West Coast and came to my home. He wanted to talk about the misfortunes of sociology at Harvard, and what to do about them. He ticked off the problems of the past decade or so—departing faculty, difficulty in recruiting new faculty, migration of faculty (and faculty loyalty) to other units on the campus, loss of a sense of community, and some internal conflict. He was clearly downcast, and was muttering about putting the department into receivership, even discontinuing it (some departments, for example, Washington University at St. Louis, had been shut down around the country, and Yale would make such an effort later). Henry and I had a long, somber talk, at the end of which I said that I believed it would not serve Harvard well to shut down sociology, and he shouldn't rush to something so radical. Why not create an external body to take over the most crucial function of recruiting top faculty. This should not be called “receivership”—a very punitive word—but something more benign such as “external advisory committee” to the Dean. Henry snapped it up and said, on the spot, “You chair that group.” Thus was born the External Advisory Committee on Sociology at Harvard University. Virtue rewarded by punishment again.

There was almost no way I could refuse this request. I have always known that it was Harvard, by taking me in as an undergraduate National Scholar with full support, that launched my career. I also knew the importance of the Junior Fellowship in further advancing it. I had turned down Harvard's invitation to join its faculty three times, and carried some guilt about that. And it was almost impossible to refuse the personal request from my friend Henry. I said I would do it, and I suggested three others to join me: Bob Merton of Columbia, Bill Sewell of Wisconsin, and Morris Zelditch, Jr., of Stanford, all of whom accepted.

During the years of the committee, 1981 through 1987, we established a pattern. We would come up with the best sociologists we could, with the aim of building a general, excellent, better-balanced department. For Harvard this meant a more

mainstream, methodologically strong cohort (the sociologists that had remained were more on the qualitative, historical, “intellectual” side). I was operating on the philosophy that a first-class department has to be a general department, capable of offering expanded coverage in theory and methods as well as general empirical and comparative sociology. There was nothing grand or ideological in my mind in this strategy. It was simply my view (and Henry and my colleagues agreed) on the best strategy to bring Harvard back to excellence. Others read more than that into our work. On one occasion a reporter of The New York Times aggressively demanded of me whether we were carrying out a quantitative, empiricist, hard-science crusade for Harvard. Mainly we continuously scanned the scene and identified the best people we could find. Because we were seeking those best (and the most competed-for), we maintained a modest batting average, but expanded Harvard’s faculty with four for five excellent appointments over several years. Henry accepted all our recommendations, and so did the department’s faculty members (they had to, of course, but they didn’t oppose them). I insisted that the external committee meet with the tenured members as well as the assistant professors of the Department to keep them in the loop and attempt to minimize the sense of powerlessness and alienation on the part of members of the Department.

One day I suggested to my other committee members that we had done our job and should fold up our tents, and Henry agreed to let us dissolve. That was the unceremonious end of that piece of history, though I served for two additional years on Harvard’s Visiting Committee for Sociology. A few years later I was consulted extensively during the founding years of the Radcliffe Institute for Advanced Study. Throughout the whole relationship with Harvard I lived with the happy feeling that I was paying my debt to that university in an incomplete but appropriate way.

Yale. I served in a similar role at Yale, though not as central. Late in the 1980s, facing a season of budgetary pressures, the university decided to take a specific path. Instead of the generally preferred strategy of shaving budgets across the board, the administration decided to pursue a more “rational” plan of discontinuing some of the university’s traditionally weak or troubled departments. Units such as sociology, philosophy, forestry, some parts of engineering, and statistics were targeted in different ways. This administrative strategy set off a faculty rebellion of sorts, the ultimate

outcome of which was the resignation of three of the initiators—a dean, a provost, and the president—and a resolve by a new president to build and augment rather than slash.

I fell into the middle of this cauldron by accident, because I had independently been recruited for a term (1988-93) as a member of Yale's President's Committee to review the Social Sciences (Behavioral) at Yale. I was selected as sociologist on that Committee, and my term coincided with Yale's tempest. I assumed the role as sociology person on the committee, and joined forces with a group that acknowledged the past weaknesses, mistakes, and some scapegoating of Yale's sociology department but called for aggressive rebuilding. I take no special credit during that conflict beyond arguing against discontinuing Yale sociology (I thought it would weaken the University to follow the policy of decapitation), and fighting for rebuilding aggressively. I take no credit for Yale's taking that path and strengthening its national position in sociology, even though I also served for five years on Yale's advisory committee on sociology between 1988 and 1993.

Lectures on Academic Cultures

By the 1980s my career had come to touch many parts of that elephant known as the University of California. Also, I had become well enough known around the campus to be invited to groups, centers, occasionally clubs, and other meetings. I first developed a lecture on "student cultures" at UC which I gave to diverse groups. This usually provoked extended discussions both because the lectures contained material and observations that members of the audience had not thought about—or thought about in my way. I subsequently developed corresponding lectures on graduate student culture and faculty culture, and delivered one of them when a speaking opportunity arose. On one occasion I gave my "faculty cultures" to a group at the home of the University-wide vice-president in Berkeley. Libby Gardner, wife of President David Gardner, was in the audience and after the session she came up to me and more or less demanded that I come and talk about "administrative culture" to an upcoming luncheon of Chancellors' spouses at University House. I had not developed such a lecture before, but I had hung around administrative settings enough to say some frank things about administrators' roles and outlooks. Because my audience was spouses, I developed a few speculative remarks about administrators' spouses' situations and "culture" as well. It was clear that the latter

opened some wounds, because they unleashed a passionate torrent of comments, many personal, on the part of those gathered. The lunch lasted well into the afternoon and evolved into a group therapy session.

I never wrote up these lectures, but I did incorporate some materials into academic writings I undertook subsequently. I like to think of them as service to my community, in the sense that I succeeded in raising a certain amount of consciousness in my audiences and marginally increased their involvement in their own university community by “objectifying” it in the way a good social scientist does.

The International Scene

Institute for International Studies. As will be recalled, from 1971 onward half of my academic salary at Berkeley came from the Institute of International Studies, one of largest and most active of the social-science organized research units on the campus. As was appropriate to that affiliation, I served as Associate Director for many years, 1969-70, 1972-73, and 1981-89. By tradition the Director was from Political Science, and I worked closely with Ernst Haas and Carl Rosberg from that department during those years. On one occasion, I forget exactly when, I was asked to be Director, but declined without much thought, because of my distaste for daily administrative routine. Instead, I carved out a role that certainly served me, and, I think, the Institute well. I did not have direct administrative duties, but was kept centrally involved in policy matters at the Institute, such as the place of area studies centers in its structure and the inclusion of comparative social science as well as international studies in the scope of the Institute’s activities. This role was a continuous one and kept alive my interest in the international studies wing of the social sciences, but did not involve a great investment of time.

Two Journeys to the Soviet Union. After my term of service with the Education Abroad Program (1977-79), I remained in periodic contact with the Santa Barbara office, the administrative home of the EAP. By virtue of this involvement—and by virtue of Bill Frazer’s continuing faith in me as university representative—I was chosen in 1987 to head a delegation of University of California administrators and faculty on a visit to Leningrad State University (I jokingly called it “LSU”), which had initiated an interest in an international exchange program. We hammered out a program, which lasted for a few years. I report only one experience that I found profoundly educational. The negotiating

table was laid out in the following contour: the Rector of Leningrad State University sat at the center of one side of a long table, with three or four of his colleagues on either side of him. I sat at the center of the other side of the table, with our delegates on either side of me. There was a little Soviet flag on the table, pointing at him, and a little American flag, pointing at me. He spoke with his colleagues and I spoke with my colleagues, but the operative rule was that only the Rector would speak with me and only I would speak to the Rector. Meantime a mysterious man (evidently a Party member) stood silently on the side, monitoring everything but not speaking. Being guests, we accepted and lived by these logistical arrangements, but did not fail to notice the authoritarian understandings they manifested. On that trip I was stunned by the beauties of Leningrad and its June midnight light.

Two years later, near the end of Glasnost and on the eve of the collapse of Soviet communism, I was one of a delegation of lecturers selected by the Social Science Research Council to speak at selected sites in the Soviet Union. I visited Moscow, Novosibirsk, and Tallinn in Estonia. In Moscow I presented lectures on American society and American sociology to a large audience of sociologists from the Moscow area. The venue was an auditorium in a Komsomol training center outside Moscow, and I lectured in front of a massive statue of Lenin, making me appear as a tiny lecturing bug. I recall bringing up the name of Karl Marx in my lectures, and this triggered a period of vicious, anti-Marxian outbursts from dozens of members of the audience. What a surprise! At the end I jested to the audience that I had concluded that the only remaining true Marxists in the entire world were a handful of graduate students at Columbia and Berkeley.

In Novosibirsk I was greeted by a very large audience of sociologists who already knew about me. The circumstances were these. Academic City was a hive of interest in economic sociology. Part of the scene was that my book, The Sociology of Economic Life, which had been on the censored list in the Soviet Union for a long time, but had been circulating underground at Novosibirsk for years. Another surprise was to find myself grilled on my economic sociology by a mass of eager students in the remoteness of Siberia.

The International Sociological Association. Throughout my career in the social sciences, I was a regular participant in the International Sociological Association, beginning with the Congress in Washington, DC, in 1962. At most of these meetings, held every four years, I presented a paper in an area of my interest. My most circumstantial involvement was in the 1980s, when Fernando Henriques Cardoso (future president of ISA and future President of Brazil) and I established and co-chaired a Research Group on Economy and Society. The catalyst for that development was Harry Makler of the University of Toronto, later at Stanford. Such a research group was long overdue, and during that decade it became solidified as one of ISA's largest and most successful research groups. For me it became the hub of my international network of sociologists.

In the summer of 1986 I received a phone call from an ISA official asking if I would serve on its Executive Committee. The call came from New Delhi, where the International Congress was held that year. I agreed. The duties were not arduous, involving attendance at meetings of the Committee between and at Congresses. I had always been actively but not officially involved in the ISA. Part of the reason for this was that I was alienated by its UN-like political jockeying, both during the Cold War when the Soviet bloc dominated by agenda with its claims of representation, and subsequently, when national, regional, and academic groups' political aspirations for recognition and power in that powerless organization moved front and center.

Without any knowledge or effort on my part, a small movement to nominate me for President of the ISA developed before the 1990 Congress in Madrid. I accepted the nomination but did not campaign. The other two candidates were T. K. Ooman from India (who was a personal friend of mine through past ISA meetings), and Salvatore Giner, a Spanish sociologist whose nomination was subsequently voided on procedural grounds because he had already served too many terms in ISA offices. In all events, several days before the election a campaign, both written and verbal, against me appeared. Its themes were that I was a functionalist (read: politically right-wing and retrograde) sociologist and that my sociology was parochially American, not truly international. The origins of the attacks were evidently from the left and from an overlapping group who resented what they felt was undue American influence in the ISA.

In the Council vote Ooman won with a coalition of third-world votes and leftish, anti-American country representatives. By way of compensation, I suppose, I was elected vice-president and chair of the program committee for the Congress at Bielefeld in 1994. Also by way of compensation, I surmise, my supporters in the ISA Executive Committee nominated me as the first recipient of the ISA's Mattei Dogan Prize for Distinguished Career Achievement, awarded in Brisbane in 2002. I liked the recognition, but was alienated by the symbolic politics of the Presidential episode that had little to do with academic sociology. When I was asked whether I wanted to be re-nominated for the Presidency in 1998, I declined.

The National Laboratories

Ever since World War II and the Manhattan Project days, the University of California had played a role in the governance of several national research laboratories: the Los Alamos National Laboratory, the Lawrence-Livermore National Laboratory, and the Lawrence Berkeley National Laboratory. Each lab is a gigantic enterprise, and the first two were the primary centers for developing nuclear weaponry. Funded mainly by the Departments of Defense and Energy, the labs were supervised in the 1980s by a Scientific and Academic Advisory Committee of the Office of the President of the University of California, and from 1992 by a President's Advisory Council. These groups were visiting committees for the laboratories, and were charged with supervising and testifying to the scientific quality of their research. I should add that the involvement in supervision was a controversial one within the university, with opposition based on the argument that the university had no business in allying itself with the nation's war machine. The ideological origins of this opposition were anti-war and more generally from the left. On an opposing side was the opinion, strongly represented in the board of regents and by some natural scientists, that the university's role was in the national interest and contributed to the scientific quality of research done in the labs. Every time the subject of the renewal of the federal contracts with the laboratories approached, there was faculty political action against renewal on many campuses, and sometimes Academic Senate votes against involvement passed.

Another side criticism of the labs, relatively minor, was that the SAAC was not a genuine supervisory body but was a club of university physicists who met periodically to

talk physics with a club of physicists in the labs. To make membership more comprehensive would presumably improve the supervisory role. It was in the context of that issue that I was invited by Vice-President William Frazer to join SAAC as a regular member. I was, of course, a social scientist. I had worked with Frazer in the Senate and on the Board of Regents, and he had become my sponsor and cheerleader for involvement in many systemwide assignments. As for myself, I was on neither ideological extreme with regard to university involvement in the labs, though I did wonder if the university's involvement was worth it from a budgetary point of view and from the standpoint of being drawn into political controversies about the labs when it was not responsible for them and had no capacity for dealing with them. I did get some flack from colleagues on my left for accepting the appointment, but that was minor.

Initially I served for only one year, taking leave to join the Russell Sage Foundation during my sabbatical leave in 1989-90. That first year was almost a dead loss. The meetings did turn out to be mainly physicists talking physics with other physicists. I understood about ten per cent of what was going on, though I could sometimes join in on general discussions about the star wars initiative and other policy issues. Because of all this, I have wondered why I rejoined SAAC the following year. However, everything changed with the fall of Berlin wall and the Soviet empire, and the end of the cold war as we had known it. In one respect those developments were a disaster for the weapons labs, because the cold-war economy also collapsed to some degree, and much of the nuclear and related work of the labs shrank radically, leaving them as monster enterprises facing cutbacks and changes in research emphasis. I jested with my physics colleagues that the labs had come to me, a sociologist of organizational crises, and I was now the expert. In all events, the content of the meetings of SAAC and later PAC changed radically, focusing on new policy changes and conflicts. I became a full member of discussions about new directions of scientific research, collaborations between the labs and the business sector, meddling in the labs by the Department of Energy, and the changing significance of security clearances. My contributions to such issues were much more informed and helpful than those in my first year.

Speaking of security, this issue became the most negative aspect of my personal experience with the labs. Actually, the troubles began with my role as Faculty

Representative to the Board of Regents, 1985-87. The Regents were officially involved in supervising the nuclear labs at Los Alamos and Livermore, so every Regent had to receive clearance at the Q-level, a very sensitive classification. As Faculty Representative I was considered a member of the board and had to be cleared, too. So the process was initiated. A slightly ridiculous aspect was that it was not completed for one-and-one-half years, not long before I departed from the Regents. As far as I could determine, the FBI talked to everybody—neighbors, past friends, colleagues, even my first wife. They called me in on one occasion for a long grilling, very hostile. They spent almost one-third of the time on my father's politics in Phoenix when I was young. I don't know what kind of information they were relying on, perhaps his membership in the American Federation of Teachers, perhaps on information passed on to them by others, who knows. They also spent a lot of time on my role in the Free Speech Movement period at Berkeley, feeding me a lot of misinformation about my activities, and completely misunderstanding my role as institutional leader at the time. Were they relying on "information" that Alex Sherriffs had supplied them in the 1960s? (above, p. 00). They wondered why I had travelled to Bulgaria to the congress of the International Sociological Association in 1970, with whom I had talked there, and what I would do if I were asked to divulge sensitive or classified information. The whole thing was so absurd and threatening that I telephoned UC President David Gardner and asked why I had to go through this abuse in order to give service to my University. He said I could call off the investigation at any time, and all that would mean was that when classified matters were discussed at the lab meetings I would leave the room. I said no, let them finish the investigation. About three weeks later I was notified that I was cleared. The whole thing smelled the same as my grilling in 1952 by the Phoenix draft board, which suspected that I was some kind of disloyal draft-dodger for seeking authorized deferment. I wondered how many other innocents had received such abuse by these kinds of investigative bodies.

The Stanford Center

Shortly after I returned from the UK in 1979, I received a telephone call from Gardner Lindzey, now Director of the Center for Advanced Studies in the Behavioral Sciences. He wanted me to join the Board of Trustees. In one way that was an unusual

request, because I had never been a Fellow of the Center (even though I had long been eligible). I agreed, and ultimately served for two six-year terms. Mainly it meant traveling to Stanford from Berkeley (a short commute) three times a year and helping to conduct business. Gardner also asked me to be a member and later chair of the Special Projects Committee, which evaluated collective projects that Fellows proposed to execute while at the Center. At one time in that period Gardner asked if I would like to be Associate Director under him, and implied that I would have the inside track to be chosen the next Director. I declined. In 1988 I was nominated for the Directorship and agreed to be interviewed by the Search Committee. Philip Converse won in that competition. I cannot explain fully why, but my main reaction to not getting the Directorship at that time one of was enormous relief. I was preparing myself for my year of research sabbatical in New York. To have taken the directorship would have killed that year as well my research project on British education. If I had been honest with myself I would not have accepted the nomination and would have declined to be interviewed. All this narrative about my connections with the Center in the 1980s should also indicate how those years fully “set me up” for the directorship five years later, at which time I was eager for the job.

The Center for Studies in Higher Education

Because I had written works on California higher education and the academic market, and because of my notorious report on Berkeley’s graduate school of education, I drifted toward regular participation in the Center for Studies in Higher Education on the Berkeley campus. I had also become close colleagues and friends with Martin Trow and Sheldon Rothblatt, both directors of the Center at different times. In 1987 I was asked to be Director for an interim term. I knew I could not take on that job with any entrepreneurial energy because of the myriad of Academic Senate and other involvements. So I insisted that I keep the title “Acting Director” during my two-year stint, and did not promise any major new directions of development for the Center. I kept the shop running by hosting visitors and sponsoring several conferences, but initiated little. My most significant accomplishment was to fight (and win) a budgetary battle with the Dean of the Graduate Division on the Berkeley campus, who made a crusade of cutting the Center’s budget. He was chemist, and believed that the proper organized

research unit was the home of big-science projects that brought in large overheads for the campus—and the CSHE certainly was not that. I thought I carried out a good enough holding operation for the Center, but I am not especially proud of my Directorship.

Squeezing in Academic Scholarship

It does not take a mighty act of induction to conclude that all of the extra-scholarly activities I pursued in the 1980s took a heavy toll on my scholarship and publications. They slowed greatly. In particular, my progress on the major monograph on British working-class education could only inch forward. I did pay a one-month visit in 1981 to Magdalen College and the Bodleian library to include known primary sources, and did draft what were to become major portions of the book, for example, on Welsh and Scottish educational changes. Basically, however, I was drastically slowed on the project and began to wonder (and punish myself over) whether I would ever finish it after all my investment.

I did pursue other lines of activity. I think the most important was to be a primary coordinator, author and editor in a massive project on sociological theory jointly sponsored by the theory sections of the German and American Sociological Associations. We raised funds to sponsor three separate joint conferences, two in Germany and one in the United States on the following topics: the micro-macro link in the social sciences, sociological modernity, and the theory of culture. In my estimation these were conducted at a very high level of discourse. I was American co-editor for all three volumes (Alexander, Giesen, Munch, and Smelser, 1987; Haferkamp and Smelser, 1992; Munch and Smelser, 1992), secured the publication of each by the University of California Press, contributed a theoretical essay as a chapter in each book, and solidified my relations with many German sociologists. All this was to yield fruit later in my invitation to deliver the Georg Simmel lectures in Berlin in 1994.

Another major project was to edit a handbook of sociology (Smelser, 1988). The most recent comprehensive work of the genre in sociology was by then 25 years in the past. I was originally scheduled to co-edit it with Richard Burt, one of “my” Berkeley appointees in 1976 who had subsequently gone to Columbia. In the early stages he faltered in carrying out his side of the editorship, so the publisher (Sage Publications) and I had to go through the delicate process of excusing him from the project. That meant

carrying the entire editorial workload myself, on which I had not counted. My contributing authors were of high quality and generally very cooperative, and the book, being so comprehensive and ambitious, received much attention. Contemporary Sociology gave it multi-reviewer space, with reviewers generally treating the book as canonical, but most complained that their own special sub-field of sociology was under-represented. In a “response” I gently chided these reviewers for their parochialism and complained in return that they did not properly review the entire volume.

I could but will not detail a number of other essays, mainly in economic sociology (penned largely as an outcome of my involvement in the founding of the ISA’s research committee on economic sociology), and a few essays on the status and problems of sociology as a discipline. Though I do not degrade these lines of scholarly work, I found myself at the end of the decade in a mood of frustration, even self-disgust, at not producing a really significant contribution. I had a full sabbatical coming up in 1989-90, and however and wherever we were to spend it, I determined above all to finish my monograph. Our youngest daughter was leaving for college at the beginning of that year; and we thought it might be a good idea for us to empty the nest as well. I applied for the sabbatical, and sought and won research appointments at the Russell Sage Foundation and the Woodrow Wilson center in Washington, DC. We thought a year in New York would probably be more engaging, so in the summer of 1989 we took off for a year in the Big Apple.

Chapter 8

Professional Confirmations—1989-1996Paralysis Finished, Paralysis Ended

The Russell Sage Foundation is an ideal spot to return to scholarship: a location on the East Side of Manhattan near Central Park; excellent housing two blocks away, provided at low cost; clerical, computer, and library assistance; intellectual fellowship with others; a pleasant, helpful staff; and all of New York's cultural attractions. I contributed my bit to the year, too. I left the University of California on sabbatical (but retained a secretary there for residual business). I resigned from all my "extracurricular" committees and assignments. I decided to work 9 to 5 at the Foundation every day, reserving evenings and weekends for entertainment. I knew invitations to speak in New York and elsewhere would arise, so I developed a party line: "I am dedicating this year to research and must decline; contact me in April to see where I am." Most inviters did not, but I was finished by that time and could oblige those who asked.

To be honest, I had some apprehensions about returning to full-time solitary research. Had I been "seduced" by invitations, demands, and extracurricular activity? Would I have the patience to read and write all day? Would my ideas on the historical project have gone stale? Happily, these frettings turned out to be phantoms, and I was able to plunge into the right kind of work right away. I experienced no significant thinking blocks or writing blocks during the entire year. The monograph I finished (Smelser, 1991a) was, in my opinion, as accomplished an intellectual product as my dissertation, though it did not create the interest that I had experienced earlier. The analysis in the dissertation had been, above all, schematic and systematic, creating a new kind of order and interrelationships among diverse historical changes and episodes. This later monograph was not as neat, though I tried to be systematic in my analysis. Furthermore, my treatment was more exclusively focused on vested group interests, group struggles, primordial clashes, and painful political compromises—in a word, Social Paralysis and Social Change. I believe my analysis caught a distinctively British style in the accommodation of social change—incremental muddling through while all the while proclaiming nothing was changing. Finally I was able to "apply" my principles of comparative analysis, even though this was a case study in historical change. I did so by

treating England, Wales, Scotland and Ireland—with a glimpse at New York—as special comparative cases “within” a case, and apply my favored variables of religion, class relations and conflict, and regional tensions to explain differences among these within-case variations. Above all, the book re-established me fully in the world of basic scholarship; I had not left it altogether in the past decade or so, but my work was much more scattered and limited.

Looking About at Age 60

We returned to our Berkeley home and “normality” in 1990. That created a bit of a void and a problem in a way, because I had in fact reached closure on my major monograph, and did not have another at hand. I had in mind undertaking a monograph on the history of American sociology, which was a project that attracted me enough that I began spending some research time on it. As it turned out, my return to Berkeley was for only four more years before going to the Center and the Stanford years, and each one of those four years had a very different flavor.

Another Year as Chair. Late in the year 1990 the Dean of Letters and Sciences approached me in desperation with a request that I chair the Department of Sociology for one year, on account of a serious shortage of candidates to begin a regular term. I did not want to hear this because of my mixed experiences as chair in the 1970s; I had more or less vowed not to do it again. But it was another one of “sitting duck” years after a luxury year of research away. The pressure from dean and colleagues was strong, so I agreed. I laid down one condition, however: that in May of 1991 I would be able to spend five weeks at the Rockefeller Center in Bellagio on the Lago di Como. I had been awarded a visitorship; I had been there twice before for conferences, and I was not going to miss that time of my dreams for anything. The Dean had no choice.

I expected the year as chair to be a holding operation for the department, an act of good citizenship on my part. But a really difficult crisis landed in my lap. The brief background was that the Department had been authorized to make a junior appointment in the area of race relations. After a regular search we settled virtually unanimously on a candidate, Loic Waquant, a French sociologist finishing up a Junior Fellowship at Harvard. In the meantime, a Chicano faculty member had not been promoted a year earlier, and the Chicano caucus and some supporters were arguing that the opening was a

designated “Chicano chair” and should be reserved for one. The department ignored that demand, and recommended Wacquant. This triggered an unanticipated protest and a semi-organized one-week student strike against the department. In the middle of it all someone discovered that Wacquant’s application for the position had arrived after the deadline date. On discovering this, and under pressure, the Chancellor’s Office voided Wacquant’s application and told the Department to search again next year.

It so happened that I knew about a number of cases in other departments that had had appointments approved after missed deadlines. I was furious, convinced that the administration had caved in the middle of the strike. I demanded that the administration produce such a rule about having to meet deadlines, and they couldn’t. But they wouldn’t reverse their decision. I was hamstrung, so my task was to persuade Wacquant to apply next year, which he agreed to do after a period of hesitations and reassurances. I instructed Wacquant and the next year’s chair to make certain he applied on time, and his appointment went through. I disliked the whole episode, in large part because as a very dedicated citizen of the campus I did not like fighting with my own administration, but in this case I felt they had blown it and had to be challenged.

On another matter I took an initiative in implementing university rules. Several years earlier, the campus established a rule that all faculty members (including tenured) had to be reviewed every five years. This was part of the “post-tenure review” movement that was adopted by many institutions in response to criticisms of academic tenure as inefficient and productive of “dead wood.” That five-year rule had been implemented only incompletely. As chair I noticed that our department had one member who had not been reviewed for 23 years! I decided to act on this case, because it seemed so egregious. I wrote and telephoned the faculty member repeatedly to secure the materials relevant for review, but he would not respond. After a period of frustration I wrote him, saying that if he didn’t produce documentation I was going to prepare a review on my own, without his cooperation. He did come in for an interview and produced some papers, but the record was as paltry and unproductive as I had suspected.

At that moment I prepared a review-report on this colleague, concluding it with the recommendation that he be demoted within the professorial steps and that his salary be reduced accordingly. I know this recommendation caused consternation in the

administration, because it was unprecedented; the existing practice was to “freeze” such cases at their existing step. The dean responded with a letter expressing concern about this colleague and requested that I have a conversation with him and to tell him, basically, to “shape up.” Given all the circumstances, I thought this a ludicrous response. Since the colleague in question was nearing retirement, however, both the administration and I let history take its course and, in doing so, watched the problem disappear. In the process I became a solid supporter of the institution of post-tenure review in colleges and universities.

Mr. Report Again: Intercollegiate Athletics. Intercollegiate athletics at the University of California, Berkeley—especially football and men’s basketball—has been a matter of contentiousness for decades. The two most visible mentalities are opposed: the Old Blue culture, on the one hand, for which athletic prowess is a high goal and a primary focus of institutional loyalty; and an orthodox view of a proportion of faculty that regards intercollegiate competition as anti-academic and corrupting (USC, Notre Dame, and Oklahoma are strong, negative symbols). Around these views crystallize many variants with different strengths of passion attached.

Chang-lin Tien was Chancellor of the Berkeley campus from 1990 to 1997. From the beginning he publicly identified himself as a “Go Bears” enthusiast, and his Chancellorship marked a radical change in orientation from his predecessor, Mike Heyman, who was notoriously unenthusiastic. In accord with his position, Chancellor Tien decided early in his term to appoint a “Chancellor’s Blue Ribbon Commission on Intercollegiate Athletics”—a commission, not a committee, and a blue ribbon one at that. Its charge was to address the general place of intercollegiate athletics at Berkeley, as well as issues of budget, student athletes’ academic experiences, gender equality, and sports facilities on campus. I was not acquainted with Tien, and I assume he appointed me as a known “Mr. Report.” I was a follower and fan of Cal’s major athletic teams, but not a wild one, and above all I brought my disposition to be balanced to the task. The membership of the Commission was faculty, some administration, and some alumni, but no maniacal jocks and no faculty curmudgeons. With respect to the Commission itself, the mood was civil and responsible, and I did not have any problems with stubborn cliques or fundamental conflicts.

That was not the case, however, with the athletic units and interests on campus and with alumni groups. The former could best be described as mainly territorial, the latter as mainly religious. One example: our commission thought it a “slam dunk” that we would recommend a single athletic director for men’s and women’s sports, because Berkeley was one of the retrograde campuses with respect to implementing some proposals of Title IX. Not so; I received a delegation of women’s coaches and others opposing that merger on grounds that such an AD would inevitably be a male, and that under present arrangements women did at least reign over their own part of the world. Another example: when we were considering reconfiguring the Strawberry Canyon athletic area we entertained a measure that would eliminate one tennis court; the next day a tennis lobby turned up in my office to protest. Different alums seemed to know for certain exactly where and when Cal athletics had gone right or wrong (mostly wrong), wanted things set right, and told us so.

Our report began with an observation that Cal had lived a kind of double-life historically. It remained in a top competitive conference (Pac 10, now Pac 12), but had consistently settled for mediocrity in the major sports. We took the position that this kind of ambivalent drift was unsatisfactory, and that it should take a path of taking these sports seriously at one extreme (UCLA-USC, for example), eliminating them at another extreme (University of Chicago, for example), or settling for some articulated intermediate position (the Ivy League, for example). Chancellor Tien never attempted to influence the commission or me at all, but it was no secret that he favored the first path. In the remainder of the report we recommended a new basketball arena, a more aggressive student-athlete education policy, more positive involvement of faculty, and budgeting policies such as endowing sports. I insisted that we walk the moderate, balanced path in our recommendations, protecting the academic purposes of the university. When I submitted the report to Chancellor Tien personally, I said I believed he should forthwith launch another commission, this one on “athletic excesses,” but that suggestion apparently died on the spot.

This became another “Smelser report.” After it was released I entertained several groups from other campuses around the country. Over time it became a kind of historical document for different groups to cite selectively in their strivings. When major campus

and community conflict broke out 20 years later over the renovation of Memorial Stadium, sacrifice of trees, and seismic dangers, the report was negatively cited by some as fostering athletic ambitions and their unhappy consequences. In the last page of my book on Effective Committee Service I made the remark that once a report is submitted the committee or commission that produced it loses control over its fate. The history of the report on intercollegiate athletics at is certainly a prime illustration of that principle.

A Phi Beta Kappa Interlude. The work of the Blue Ribbon Commission coincided with the year as departmental chair, providing both an extra burden and some relief from it. Another relief that year was my acceptance of a Phi Beta Kappa lectureship. That honor asks the recipient to take two national trips of two weeks each, visiting four institutions on each trip, spending three days per institution in lecturing and mixing it up with local faculty and students. The requirement is that the institutions be mainly “non-mainstream,” namely campuses that one would not be likely to visit otherwise. My wife and I made two such trips—one to the South, visiting Hampden-Sidney College and Randolph-Macon College in Virginia, and the universities of Kentucky and Florida; and one to New England, visiting Holy Cross and the universities of New Hampshire and Maine, with a stop at the University of Oklahoma on the way. These were very gratifying tours. I lectured and talked mainly with sociologists and economists, met with the local Phi Beta Kappa chapter and other students, and gave a public lecture. It was on trip that I perfected my talk on “The Myth of the Good Life in California,” which was a main draw everywhere.

One droll incident. When at the University of Florida, I paid a spontaneous visit to a new basketball arena on that campus. I was in the middle of our Blue Ribbon Commission’s work, and I wanted to do a little comparative analysis. I merely walked into the gym to look about, but a coach who happened to be in the building kindly showed me around. It was a dazzling, state-of-the-art arena, including moving banks of spectator seats, potted palms, and other luxuries. After the tour I sat with the coach for a while, and mentioned my commission. This set him off on a barrage of complaints about how cash-strapped the athletics program was at Florida—right in the middle of this new luxury super-gym. I concluded on the spot that budgets for intercollegiate athletics have no bottom.

The Bellagio Adventure. We had chosen May as the month to go to Bellagio. The weather is typically the best in that month, and it was. The setting is above the town, on the peninsula extending into the Lago di Garda, with views of the Italian Alps to the North. The site is owned by the Rockefeller Foundation and maintained in classical style. Visiting scholars live in the main villa or an adjacent one, and are granted complete freedom to study, save for common meals and reporting on one's research at an after-dinner meeting. We had one set of friends, Robert and Judy Wallerstein, in residence with us, and made several more enduring friends while there. I later wrote about this adventure as one of the great odysseys of my life (Smelser, 2009).

The road was not altogether smooth, however. I was enough involved in my early work on the history of American sociology that I mailed several cartons of journals to have on hand. (The Bellagio Center had no research library to speak of.) I had given plenty of advance time in mailing, but when we arrived the journals had not, and there was no indication of when they would. Basically, I had nothing on hand to do. During my inquiries about the mails, it was suggested to me that if I would pay \$100 it would "facilitate" the search. I smelled Italian politics, and didn't bite.

A coincidence bailed me out. About a year earlier I had been persuaded to contract to write an unusual book. It was for one volume of a set entitled "Survival Skills for Scholars," written by scholars, each on a practical aspect of academic life, such as classroom teaching, improving writing skills, and getting tenure. The publishers had approached me with the list, and it occurred to me that I could write a volume on committee service. I had certainly had enough of that, and had reflected amply on it. The publishers liked that idea. I laid down only two conditions: first, with my crowded life I would not agree to a strict deadline; and second, I would base the book on my personal experience, not the organizational or public-administration literature.

When the Italian mail system failed me, I told myself that the only real choice besides wasting my fellowship was to start on the committee book. Literally, I began writing that day, needing only my mind and memory as resources. I intended the book to have three parts: a somewhat scholarly exposition on what an academic committee is and what functions it serves; a middle section on the formal and informal processes that transpire; and a final set of practical chapters on how to be an effective member and an

effective chair, as well as guidelines for writing committee reports. I illustrated everything from my own experience. I completed the book in that five-week stay, even leaving ample time for exploration around the lake and for daily afternoon excursions to the gelato shops in town. Everyone should have such a respite during a year as chair. The book, called Effective Committee Service (Smelser, 1993c), had some success, and on occasion bodies such as the National Research Council and the UC Academic Senate purchased a bundle of copies for circulation among their committees.

The Office of the President

In 1992 David Gardner retired as UC President, and Jack Peltason was appointed as his successor. He was a respected veteran in higher education, advanced in his career. It was generally assumed that he would not have a long presidency. It was not long after his appointment, moreover, that the lean, punitive budgetary years of the early 1990s threw the university into a scrambling, survival mode. Peltason and his staff performed heroically during this unhappy phase in the history of the university, but the main themes of his presidency were defense and protection of the institution.

I do not know anything about the background of Peltason's phone call to me, in the spring of 1993, to join his staff in the Oakland office the following summer. Of course I had known him for years, and we liked one another. He described the position only as "advisor for long-term planning," which communicated little to me. He made it clear that I would be mainly working on a daily basis with the new Vice-president, Walter Massey, a physicist who had been Director of the National Science Foundation. I also knew that people in the Office of the President and in Sacramento were beginning to worry seriously about the long-term viability of the Master Plan for Higher Education in California, in light of current and anticipated changes in the demographics and economics of the state. In the way Peltason described it, the position was for a minister without portfolio, a systemwide trouble-shooter, and in no way a bureaucrat. Given my history of nervous flirting with and semi-avoidance of official administrative responsibility, this description, while vague, seemed to fit me perfectly. The only "condition" I laid down was to bring my personal secretary of many years to my office in the Kaiser building in downtown Oakland, and that request was granted immediately.

I was not a stranger to the Office of the President. I had known every President from Clark Kerr on in one capacity or another, but Charles Hitch and David Saxon not very well. My years in the systemwide Academic Senate (1982-87) had involved me in direct and close relationship with David Gardner, and I had come to know many of the systemwide staff. Many citizens of the University—campus administrators and faculty—develop a negative orientation toward systemwide as a huge, largely useless, meddling monster, but I was not in their ranks. In particular I appreciated its role of taking the political flack from Sacramento and thus protecting the campuses. I was from time to time apprehensive about the “corporate” character of that bureaucracy, and its tendency to develop a culture and life of its own and to remove itself from the academic trenches. That is why I opposed moving the Office of the President away from a campus. But on the whole I did not have an ideological “thing” about “allstate” and was comfortable in my anticipated helping role.

The job turned out pretty much as advertised. I had no “routine” assignments. I was called on early to plan and coordinate a conference of top campus and systemwide administrators on long-term planning, and write up the results. I was “on call” for Massey (with whom I developed a very warm relationship) and to a lesser extent for Peltason. I commented on policy drafts—for example, a circulated memo called “The Thirteen Commandments”, containing that number of proposals for long-term economies. I attended selected monthly meetings between Peltason and the Chancellors. I prepared a working memo and analysis of prospects for the Master Plan to guide a Regents’ policy discussion. I joined an informal “fire brigade” that was assembled in the President’s office after Peltason had made an apparently slurring comment about a state legislator—overheard but not intended to be overheard by a reporter—and the media and Sacramento were rumbling. I liked my role, and I think I was appreciated, because near the end of the year Peltason asked me to stay on. I didn’t accept, for reasons I will give presently.

Toward the end of that year, when it was clear that I would not be continuing in the President’s office, I got an impulse to write an essay on governance in the University of California for Peltason and Massey. This was completely unsolicited. I regarded it as a kind of gift for the rewarding year they had given me. I completed it in one of those three-day writing fits. It was nearly 100 pages long. It was an analytic piece, covering

the nature of governance in a systemwide entity, principles of academic organization, the role of formal and informal constituencies, the nature of core academic units such as departments and organized research units, and a long section on the Master Plan and its looming problems. I just handed it to Jack and Walter and left. A few years later the next President, Richard Atkinson, found it and wrote me a letter advising me to publish it. I didn't, but in 2009, when the University of California Press and I decided to publish a variety of (mostly unpublished) essays, reports, and reflections in a single volume, I included this essay in its original form (Smelser, 2010d).

VERIP, the Prospect of Retirement, and the Center

The late 1980s and 1990s were years of concern for universities with respect to faculty costs. The huge bulge of faculty hired at young ages in the explosive growth of the 1950s and 1960s was now a large, senior, tenured and highly paid cohort. In 1993 the federal “uncapping” legislation came to apply to higher education, so that those in that cohort could not be retired in any mandatory way. In this context various colleges and universities began to devise incentive plans that would induce faculty to retire voluntarily. The University of California was no exception. Its scheme was called the Voluntary Early Retirement Incentive Program (VERIP). There had been two early versions, but VERIP III, to apply in 1994, was the most generous. (And, I learned from my vantage point in the systemwide administration, VERIP III was to be the last one over the coming years.) The main mechanisms were to permit faculty to retire at younger-than-previous ages (as low as the very late 50s), to “add on” imaginary years of service to boost retirement pay, and to offer cash bonuses. In my own case, if I retired at age 64 (which I was at the time), I would have 36 years of service. The “add-on” would easily push me over 40 years of service, which meant I would retire at 100% of my highest-average three years of actual service. But I would have had to retire in 1994, not later. Nor could I “take it back” later, and there were prohibitions on teaching at the University of California after retiring. It would be a cold-turkey deal as far as I was concerned.

But I was not psychologically ready to retire at that time. I have sometimes wondered if in the spring of 1994, when I had to decide, what I would have done if it were simply a matter of “to VERIP or not to VERIP.” I didn't want to seek out another professorship in another institution after VERIP, though some—too many, it was

feared—would do so. I never got a chance to resolve that question because in the spring of 1994—just a couple of months before the decision-deadline for VERIP—I was offered the directorship of the Center for Advanced Study in the Behavioral Sciences. So the “opportunity structure,” as they say, changed dramatically. I was now presented with the prospect of retiring at full salary for the rest of my life, and simultaneously taking on a new full-time position at salary somewhat higher than that. (There were no restrictions in VERIP on taking employment outside the University after retiring.)

At this moment I should say that, during my entire life the aspiration for money—or more money—has not played a decisive role in my professional choices, though it has not been absent. Through the undergraduate years at Harvard and Oxford I was on more or less full scholarship support. The Society of Fellows’ stipends were higher than the salaries of most beginning assistant professorships. And in my early career ponderings I simply assumed that my life’s earnings would be modest in the academic profession and that I valued the other rewards of that life sufficiently that income wasn’t the major factor. At the same time, in my early years at Berkeley I was accelerated rapidly through the ranks, and this meant rapid increases in salary. I never felt that Berkeley was anything other than completely generous in responding to outside efforts to lure me away. At one moment in 1979 I was offered a generous joint appointment in the department of sociology and the Woodrow Wilson School at Princeton. This possibility interested me. However, in reporting this offer to Chancellor Mike Heyman, I told him that I had been treated so well by Berkeley that I didn’t want any increase in salary. I did say that I would like transferring my secretary’s salary to the systemwide level, in keeping with my own appointment as University Professor. (She had previously received her salary from my research grants). More generally, I never entertained outside offers for increased rank or salary if I was not willing to accept them in principle.

On some occasions, however, money was an issue. I was strapped for a couple of years during my divorce proceedings, and accepted a couple of well-paying writing assignments. Later, when I felt the pinch with both our children enrolled in private institutions, I looked around for assignments that would bring in a couple of months of summer salary. On most other occasions, however, I did not seek money. If I were asked to lecture at another university and no fee was offered, I never requested one. On

one occasion I found myself put off by a money offer. I received a letter from Columbia University law school, asking me to evaluate a candidate for tenure, and was offered \$300 when I submitted it. I did this, but only after thinking a while about it. In fact I felt uncomfortable in doing so, as though there was something vaguely corrupt about it. I suppose I could have written the letter and declined the fee, but that seemed too much. In all, I suppose my motivation in these matters was a vague mix of medieval-monkish and modern-monetary, never fully settled.

In all events, here in 1994 was a combination of circumstances that almost dictated that I should retire and take the Center job. There was an unprecedented gain in income—indeed, taking that position fully resolved any material issues I might have faced in my retirement years. But in addition, I really wanted the position at the Center and I knew I would thrive in it. I was moving along on somewhat familiar career tracks in the University of California since my return from New York, and I could envision no real change in that pattern if I remained at Berkeley. When the offer was in the works at Stanford, I said unequivocally to myself that seven years at the Center (I actually had that term in mind) would be an ideal way to finish my active years as an academic. I reflect on my term at the Center in the next chapter.

As a footnote I should note that VERIP III created its own tempest in the University of California. Many campuses welcomed it as a source of savings and an opportunity to bolster the quality of their faculty with new replacement appointments. However, Chancellor Tien regarded it as a potential disaster for Berkeley. If faculty could retire at age 58 and take other positions, they would do so in droves and decimate the quality of the senior Berkeley faculty. He actually threatened to resign if that age provision was not changed for Berkeley. I joined with Clark Kerr and a few other Berkeley colleagues in urging him not to resign on this issue. The crisis passed when the President's Office made a minor concession to Berkeley and Tien withdrew his threat. I myself received telephone calls from the Chronicle of Higher Education and The New York Times, asking me about my decision to retire, whether I had any qualms of conscience about leaving the Berkeley ship, and whether or not VERIP III was damaging to Berkeley. I didn't take on those baiting questions, but jested to reporters about everyone owing it to oneself to have been born in the cohort of the Great Depression.

Academic Work: Ambivalence Front and Center

Sometime in the early 1990s I noticed a recurring theme in my scholarly work. I had introduced the pages of my dissertation on the British industrial revolution by pointing out that terms like “development” had both positive and negative connotations. I had begun my treatise on collective behavior by noting that throughout history people had reacted both positively and negatively—and strongly—toward collective outbursts. I began my book on comparative methods by pointing out that “the other” or “the different” typically excites ambivalent reactions of hostility and envy. And I opened the pages of my work on British education by observing that elementary education in society often excited ambivalence on account of its mixed consequences. Oddly, I had not been aware of this convergence. Each time I made the observation it seemed a novel way to begin. Of course the idea of ambivalence had come before me many times, particularly in my involvement with the psychoanalytic tradition, in which the concept is still a core one. I also know—but not fully—that the idea of ambivalence infuses my own personal involvement in the world, as evidenced especially in the “half-member” theme I have mentioned.

I brought the idea of ambivalence fully into the open, as it were, in 1980. Jerome Oremland, a teacher and friend in the San Francisco Institute, asked me to deliver a plenary address to a national meeting of the American Psychoanalytic Association in San Francisco. I was pleased with the invitation, but had no idea what to talk about. As it so happened, I had been somewhat casually involved in a seminar on “California as a Society” in Berkeley’s Institute of Governmental Studies, and there I had come across some dark, pathological views of the state and its culture, especially in novels. At the same time, California has been celebrated repeatedly as the promised land, symbolized in the idea of the Golden State. It occurred to me to exploit this theme for the psychoanalysts. So, in the address, I pointed out the vicissitudes between gratification and guilt, indulgence and retribution, and other dialogues in the history, lore, and politics of the state. In many respects it was a playful analysis, and was evidently entertaining to the gathered psychoanalysts. Oddly, however, I did not publish it for several years, and when I did, it was in an obscure place, the Humboldt Journal of Social Relations (Smelser, 1983) a regional journal in northern California—which meant that it was still

hidden, in a way. However, I did adopt the piece as my “toastmaster’s club” speech, giving it at colleges and universities outside the state when I was travelling; audiences took glee in it, because they heard it as a Californian bashing California. I also gave it to the Fellows as a kind of welcoming at the beginning of their year at the Center for Advanced Study (below, pp. 00-00), a symbolic but unspoken reminder that their coming year was not going to be utopian.

Another opportunity to develop the theme of ambivalence came in 1991. Elinor Barber and Jonathan Cole of Columbia had been commissioned by the American Academy of Arts and Sciences to develop a conference and an issue of Daedalus to the theme of “The Research University in a Time of Discontent.” Originally the organizers asked me to deliver a paper on “the mission of the research university.” I accepted, but without much enthusiasm. I had written up such material several times for administrators at the University of California, but I felt that I had produced nothing original in these drafts. I also feared I could do little more than that for the coming conference. A couple of months later, however, before I had begun to write, the organizers asked me to change my topic to the hotter one of the bleeding conflicts of the 1980s—affirmative action, diversity, and multiculturalism. Others they had thought of asking had already revealed their partisan views on these topics; still others were unwilling to accept because the topics were too hot. I had not gone on record on these subjects. It occurred to me that I should try my hand at an “objective” analysis of these very emotional issues—that is, to ask why they had arisen in the way they had, and what were their peculiarities. It occurred to me further that partisans in all these struggles were experiencing ambivalence toward different values and principles in their struggles. So I entitled the paper “The Politics of Ambivalence” (Smelser, 1993a), which I thought much more exciting than mouthing out the purposes of the university once again.

A year or so later I was asked to lecture at the 250th anniversary of Princeton University. Much of that celebration was devoted to higher education, and the organizers asked me to speak on affirmative action. I believe my essay on the politics of ambivalence had come to their attention. Once again I decided to take distance and “objectify” the topic, taking no partisan stand on one side or the other, but to ask, instead, why this issue, unlike most other episode of social change, had not “settled down” into a

pattern of stable institutionalization. I explored this issue and analyzed it at the cultural and political levels, concluding that the abiding and deep national ambivalence about race could explain many of the peculiarities, vicissitudes, and stubbornness of the issue of affirmative action (Smelser, 1999c)

In 1996, I was elected President of the American Sociological Association (below, pp. 00-00), and faced immediately the question of the topic of my Presidential Address. I listed about six topics of conventional sociological interest—"the fate of functionalism," "theory of social change," for example, and then added "ambivalence." I showed the list to Christine Williams, a former student of mine and Fellow at the Center at the time. She looked at the list and within one minute said "ambivalence" and would hear of no other topic. I was leaning that way, but her enthusiasm sealed it. In mulling over the subject, it occurred to me that I could combine that theme with my life-long critique of motivation as represented by rational-choice thinkers, which I regarded as a univalent motivational scheme. It proved a good strategy for me, now able to root my theoretical observations more in mainstream social science, and to demonstrate the power of ambivalence as a principle (Smelser, 1998).

When, in the late 1990s, I put together all my psychoanalytically oriented essays (Smelser, 1999a), it was of special significance for to me to include, in a conspicuous center section of the book, the four essays on ambivalence. For me the concept, as I had extended it, had borne much intellectual fruit. It was also consistent with my "taking distance" and "standing above the fray" modes of thinking and analysis, a feature that has always been a source of special pride in my style.

Other Academic Work: Reflections on Sociology

Though I did not have an agenda by that name, in the late 1980s and early 1990s I wrote a number of essays and a book on the nature of sociological inquiry. I cover this period in this chapter, even though it extends into my years at the Center. I should add that this theme was not conscious in my mind at the time. I was responding to invitations one by one, and recognized the thematic accumulation only recently (Smelser, 2014).

I note the themes of the articles briefly. In 1989 I published a paper invited for Herbert Gans' Presidential Session of the ASA (Smelser, 1989b). Its title was "External Influences on Sociology," and singled out national-cultural influences and political forces

as external sources. This marked a deviation from my earlier writings that had treated sociology mainly as an autonomous enterprise. A year later, I wrote a long, complicated essay on “The Internationalization of Social Science Knowledge” (Smelser, 1991b) in which I explored the limitations and qualifications of arguments for cross-national and comparative (if not universal) social knowledge, and explored avenues for and obstacles to the pursuit and institutionalization of such knowledge. In that essay I was working through, perhaps unconsciously, my own recent perceptions and experiences in Madrid with the political forces involved in international sociology.

A couple of years later I wrote what I consider to be one of my most imaginative essays, in which I identified, as facets of sociology, the relevant values and norms of social-scientific inquiry, the fundamental features of humanism, and the artistic appreciation of the world. I argued that sociology, in the course of its evolution, had become a *mélange* of all three of these, and by understanding this unstable mix, one could read some systematic sense into the internal tensions and conflicts that characterize the discipline’s history and present state (Smelser, 1994). And finally, I tried my hand in defining “applied sociology,” in particular the relevance of sociological knowledge in the understanding and amelioration of social problems. In this essay I attempted what I hoped was a synthesis of positivist and social-constructivist approaches to issues of social policy (Smelser, 1995). In reflecting on these and other essays on sociology as they developed over time (Smelser, 2014), I interpreted this largely unconscious evolution of my thinking as a kind of mellowing, of permitting more contingency into my thinking about my chosen discipline without, however, sacrificing my passion for the field as a life’s calling.

This line of interpretation came to a head in the spring of 1995 when I gave the Georg Simmel lectures at Humboldt University in Berlin. The invitation to deliver these came in the fall of 1994, just after I arrived at the Center. I suppose if I had been sensible, I would not have accepted Hans-Peter Mueller’s invitation, because it meant preparing the equivalent of a short book during my maiden year of directing that institution, and a one-month’s absence the following June (more on that absence below, pp. 00-00). I did accept with enthusiasm, knowing, however, that I had once again brought myself to the brink of over-commitment. I do not recall the wanderings that led

me to my topic, but I decided that some general reflections on sociology would be in order. I took as an intellectual starting-point that the sociological tradition has been characterized by approaches to the subject matter at different levels of analysis, notably the social-psychological, group, institutional, societal, and most recently the international level. The vitality of all of these analytical levels has made for a special richness and complexity of sociological explanations, but has also occasioned a great many divisions in the field over which of these levels is decisive in explaining human society. This decision meant invigorating the idea of levels of reality that are analytically separable from one another but combine and mesh in any empirical explanation of human phenomena. In addition to employing this notion of analytically distinct but “complementary” relations among these levels as an explanatory sociological strategy, I also made central the idea of “problematics,” or the puzzles and riddles of sociological explanation. The four levels I identified were the microsociological (psychological and interactional), mesosociological (group-associational), macro-sociological (institutional and societal) and global-sociological (international), the most recent arrival on the scene. The Simmel appointment called for four lectures, one a week over a month’s period. This was a happy coincidence, for it meant that I could give equal time (one lecture each) to each analytic level. My wife and I stayed for a glorious month in Berlin in guest housing on the Spree River near the Reichstag and the former Wall, with a side trip to the European University Institute in Fiesole. I also lectured at two East German Universities (Halle and Dresden) that were in the throes of the transitions that accompanied German reunification.

I find it important, in reflecting on these lectures, that I dwelt on the “problematics” of sociology, as contrasted with “principles” of sociology, or simply “sociological analysis,” titles I might have used two decades earlier in my life. By “problematics,” I meant “those generic, recurrent, never-resolved and never completely resolvable issues that shape how we pursue our work, how we generate theoretical tensions and conflicts in that work, how we converse and debate with one another, and how we engage in that complex counterpoint of simultaneous advance, retreat, and repetition in our scholarship” (Smelser, 1997, p. 1). This formulation marked an important point on my trail toward increasing theoretical modesty, I suppose. It also

meant a major intellectual move on my part from representing sociology as an identifiable science and as set of principles—a thing, perhaps—toward representing it as a continuous process. I tried to represent recurrent conflicts and confusions, rather than to present “correct” versions of them. In reconsidering the lectures, I think that the most original one might have been that attempting to discern the principal issues in global sociology, an emphasis that has been emerging as a major focus in the discipline, but in a confused way. I also think the idea of distinct but complementary and interacting levels is one fruitful path to productive analysis, explanation, and theoretical synthesis in sociology.

Elections to the National Academy and the Presidency of the ASA

When I was winding up the work on the behavioral and social sciences with the National Research Council in the late 1980s, a colleague asked me directly and somewhat intrusively, “what honors haven’t you received?” I had to think about that and deemed it unanswerable, but two obvious cases came to mind: election to the National Academy of Sciences and to the Presidency of the American Sociological Associations. I had certainly been aware of these and mused about “why not”, but his query crystallized my thinking. I confess to readers that I thought I deserved both on grounds of merit, but that there were discernible reasons why neither had happened.

With respect to the Academy, its elections in the behavioral and social sciences had long been skewed, with significant exceptions, toward those who represented their most “scientific” variants: formal-quantitative economists, demographers and other “hard” social scientists, and experimental psychologists. I was also aware of a past battle in which Talcott Parsons was turned down largely because he was thought to be too “soft” or “philosophical”—that is, not sufficiently “scientific”—in his sociology. I had, without much reflection, thought I was not eligible on these categorical grounds. I knew my work was empirical enough, to be sure, and self-consciously embedded in a scientific mode of thinking, but I gave myself the reason that the dominant Academy style did not comprehend mine. That was not true, of course, for when I was nominated in 1992, the election went through. Later I was to play an active role in the Academy (below, pp. 00-00 and 00-00).

The story in the ASA was a different one. When I was young a number of senior sociologists in the Association told me that it was only a matter of time before I would be elected to the presidency. I did not dwell on this item, but I assumed it would happen some day, and it would happen on traditional grounds of scholarly merit. I was elected to the vice-presidency and a three-year term on the Council in 1972, but a couple of years earlier I had been nominated for that office and defeated by Matilda White Riley, a friend and colleague and a person I thought fully deserving. But with regard to the presidency, all was silent for years. In 1986 I was written in as a candidate, with four others, and the plurality went to Herbert Gans. In 1992, I was nominated to run against Amitai Etzioni, who ran a “campaign” of sorts, and I narrowly lost—which especially disappointed me, for I had always regarded him as an intellectual opportunist and less deserving for that reason. In the following year, the nominated candidates were three women—Maureen Hallinan, Cynthia Epstein, and Arlie Hochschild. Some irritated male sociologists approached me and asked me if I would be a write-in candidate in an election “rigged” for a woman. I said I would not; inwardly I knew it was not my style to fight this kind of fight (especially against Arlie, a beloved student of mine). At that moment I more or less concluded that the presidency was not to be mine. Then, however, in the following year I was nominated to run against a less known candidate and won by a large margin.

I am in all likelihood unable to analyze this bit of personal history, and perhaps there is nothing to analyze. I believe I merited the Presidency on “conventional” grounds of scholarly achievement, though I also believe that my advances in scholarship had decelerated in the 1970s and 1980s. A partial unknown was the issue of what political symbolism was attached to my name. Certainly my association with Talcott Parsons was a liability because it appeared to cast me as an agent for his disfavored “conservative” functionalism. My work on collective behavior was pushed to the right by the left because of its supposed origins in “functionalist” work and the accusations that I was a scholar with an “irrationalist bias.” I have always resented the over-simple labeling of my work as “functionalist” because, considered overall, it does not fit in that category. But one can scarcely affect one’s own labeling. More generally, the ASA became and remained politicized continuously for the remainder of the century—and still is—along diverse lines, including the dimensions of race and gender. Identity and identity politics

play both a real and a symbolic role in the politics of the Association. Let us say that office-nomination, office-seeking, and office-holding have become more complicated and politicized. I can also add that I was completely passive all this time with respect to “seeking office,” such as that is in the politically ill-organized ASA. This was perhaps odd behavior on my part, for if I desired the position I should have cultivated friends in the right places and made the right sounds. I never did this, out of a deep and only partially examined belief that this would have been alien to my self-image as an unwavering professional.

During my three years on the ASA Council (associated with being President), I behaved conscientiously enough as member and chair (for one year), moving business along in the way one should. One major issue that came before me while I was chair was the move for the Council to be more directive of editorial policies of the Association’s journals. This initiative arose from the criticism that these journals (especially the American Sociological Review) were “biased” in favor of the scientific “establishment” by favoring positivistic and quantitative emphases in their selection process. I thought there were other, unspoken items in that political agenda, calling for greater representation of groups presumably disfavored by this editorial process. At least people lined up that way. My parliamentary accomplishment during that year was basically to postpone the issue by referring it from the Council to the Publications Committee, a less politicized body. However, that served only to delay things, which blew up into a public fight in the following two years.

Chapter 9

The Center Years—1994-2001Joining the Center

The second of my two six-year terms on the Board of Trustees of the Center for Advanced Study in the Behavioral Sciences expired in the summer of 1993. I had planned that I would spend the year 1993-94 as a Fellow at the Center and was on the roster. I had even arranged housing near the Stanford campus for that year. But when I got the call from Jack Peltason to work with him for 1993-94, I more or less immediately decided that I would accept his invitation and delay my year as Fellow at the Center. I later learned that if I were in any way campaigning for the directorship of the Center, the last thing in the world I would have done would be to agree to a fellowship year and then to withdraw. When I did become Director, I learned how much the Center, especially the staff, resented people who said they would spend the year, then resigned, creating a vacancy on the roster, often difficult to fill at the last minute. I did not know this at the time, and I was certainly not campaigning for the directorship.

As it happened, Phil Converse decided to retire from the directorship after a five-year term, as of the summer of 1994. I did not press my name as a candidate to succeed him, but I was certainly in the running, having served for 12 years as trustee and having been the runner-up for the directorship the last time around. When I was invited to be interviewed and the appointment became a vivid possibility, I realized in all clarity how much I would like to have the job. This realization came as something of a surprise to me, because I had not entertained any active aspiration for the position during Converse's term. I think the realization had to do with the latent dissatisfaction with the prospect of living out my future years at Berkeley in much the same way I had lived my former ones.

The interview was a comfortable one. There was no hostile questioning, no controversial challenging. I remember vividly one question from Robert Solow, chair of the Board of Trustees. He asked in what ways I would change the Center if I were the Director. I hadn't thought about this, but I answered spontaneously, "not very much." This was an honest answer, because I had always regarded the Center as a really admirable institution.. I went on to say that I had one innovation especially in mind, and that was to increase the number of international fellows on the roster. Traditionally there

had been only a handful in any given year. I knew one reason for this, namely that they did not have access to resources and the Center had to carry the full burden of their support. So I added that I would try to raise funds from both European and American sources to achieve this augmentation (I had no idea what those sources were). But that was my only “campaign promise.” When, a few weeks later, I was telephoned by Solow, who said the job was mine if I wanted it, I was euphoric, and arranged a celebratory dinner in San Francisco with Gardner Lindzey, who I knew was my “campaign manager.” I did not bargain for salary with Solow, because I knew I would be receiving a full salary on top of a full salary, and a Director’s house on top of that. I told Jack Peltason the following day I was going to the Center and couldn’t work with him the following year. Long associated with the Center, Jack said he understood my decision completely, and did nothing to try to persuade me to stay in the President’s Office.

There was one definite but somewhat intangible dimension peculiar to my appointment. I was coming to the Stanford community (though not formally to Stanford University) and I was from the University of California, Berkeley. These two institutions have a long history of special rivalry, something on the Harvard-Yale model. Much of it is a magnified athletic competition, but striving for academic excellence and prestige is also there. More recently some Berkeley faculty have snorted at Stanford’s “commercial-corporate” culture and some Stanford faculty have snorted at Berkeley’s radical and counter-cultural excesses. While much of this competition is latent and did not apply to me, I was mock-criticized by some Berkeley colleagues for deserting the ship and reminded of Berkeley’s weaknesses by my new Stanford acquaintances. From time to time I joked that when I took the Center position, my lifetime colleagues at Berkeley regarded me as a Judas and my new Stanford neighbors regarded me as an alien. This Berkeley-Stanford thread was not a major one in my life, but it was there, and was the more so because I made no secret of the fact that we had not sold our Berkeley house and were going to be returning after I left the Center.

The Center as a Place. The Center is a simple institution, a remarkable invention. A yearly cohort of scholars (forty or fifty in my time) of high accomplishment or promise gather in a structure on an isolated hill near the Stanford University golf course for one year of uninterrupted study. Each has a study, and there is a cloister-like area in the

center for collective dining. The main object is to maximize the freedom of the Fellows to pursue their scholarly labors. Traditionally there were approximately three “special projects” each year, in which a small number of scholars collectively pursued a line of research approved by the Board. All research is the scholars’ own, not directed in any sense. “Accountability” consisted of a year-end report on research progress made along with comments on the year’s experience and how things might be improved. More proactively, the Center, in my time, provided the Fellows with a book- and article-delivery service from the Stanford University Library, as well as computer, statistical, and editorial services. Any exercise of authority was minimal. I think this is why I so cherished the promise and the reality of being Director. I was virtually one of the Fellows in that almost all of my interaction with them was intellectual. I frequently told others that my job as Director was “the best job in the Western World.” What I had in mind was this dimension of intellectual richness that I experienced.

Actually, I participated in the Fellows’ intellectual activities as much as I could. I joined them at lunch on the Center premises almost every day, went to almost all the Wednesday evening seminars at which Fellows presented their work-in-progress, and attended some meetings of special-project groups. In one year, 1997-98, I actually “became” a Fellow and joined a special project myself. I had collaborated with Jeffrey Alexander of UCLA, later Yale, my former student, in securing a sizeable research grant from the Hewlett Foundation. The project was a general one, on “values and social process.” One activity was to convene a special project at the Center. Its members were Alexander, Ron Eyerman, Bernhard Giesen, Piotr Sztompka, and Bjorn Wittrock. I joined that project and attended all its meetings. It was an extraordinary intellectual journey that led us focus on the topics of collective trauma and collective identity, subjects with vitality in the literature of the time. I truly relished becoming a full “fellow” that year.

The primary intellectual product of that project was a volume on cultural trauma and collective memory, co-authored by the participants (Alexander, *et al*, 2004), with theoretical chapters supplemented by case studies of American slavery, the German holocaust, and successive traumas in Poland in the twentieth century. I wrote a theoretical essay (Smelser, 2004a) on the essential features of cultural trauma—its

embeddedness in cultural values, its indelibility, and its compulsive characteristics, for example. My theoretical starting point that the idea of cultural trauma is an analogy, derived from physiological and psychological formulations. In that essay I explored all the implications of that kind of explanation, cautioned against reductionist reasoning, and identified the fundamental processes at work at the social and cultural levels of analysis—thus revisiting the major theoretical themes of “levels of analysis” in the Simmel lectures. As part of the larger project, Alexander and I edited a work on the many dimensions of the nation’s current preoccupation with diversity, postmodernism, and cultural conflict (Smelser and Alexander, 1999).

I report one other twist on the cultural trauma project. By the time the book entered the editorial process at the University of California Press, the terrorists’ attacks on September 11, 2001 had just occurred. The editors of the press contacted Alexander, who was coordinating things, and said that the book could not possibly be published without a chapter on 9/11 as a cultural trauma. By circumstance I had just joined the collective efforts of the National Research Council to prepare several major reports on defending the nation against terrorist attacks (below, pp. 00-00). I was the logical candidate to produce such a chapter for the book. So I wrote an Epilogue (Smelser, 2004b), trying my hand at interpreting the drama of public events in the post-9/11 months as they were unfolding. I would not have wished such a tragic coincidence on anyone, but undertaking that interpretation was an exciting adventure.

Steering the Center. Directing the Center also involved some administration in the more literal sense, as does any organization. The Director might best be described as a small-time CEO. The position meant being responsible for recruiting a class of Fellows each year and making eligible a reservoir of scholars for future years. It meant superintending each year’s cohort, which was not a demanding job because the understanding was that the main aim was to maximize freedom and minimize outside distractions. It meant supervising a staff of approximately 20 administrative and clerical employees. It meant “representing” the Center to the local Stanford community when necessary, and entertaining visitors. It meant accepting a courtesy appointment in the Department of Sociology at Stanford, but this did not involve me in teaching, attending meetings, or accepting a salary. It meant managing, mostly via a budgetary officer, an

annual budget of about \$5 million. It meant meeting with the Board of Trustees three times a year to review policy matters, to report on the state of the Center, and to hear if the Board had concerns or criticisms about my performance. In many of these activities I was assisted and supported by an experienced Associate Director, Robert Scott, who was superbly skilled, and with whom I got on beautifully as a colleague and friend.

I save one responsibility of office until the last, because it loomed as the only really problematical one I had on my mind. That had to do with fund-raising. The Center had a smallish endowment, and approximately \$3-4 million per year had to be raised, mainly from private foundations and federal funding agencies. Because the Center was such a small shop, and because its early decades were relatively amply and easily funded, fund-raising was the exclusive responsibility of the director.

I knew that if anything was going to be my Achilles heel at the Center, it would be fund-raising. First, I had limited experience, consisting mainly of finding support for my own research. I was successful at this, but it was very different from supporting an institution on an annual basis. Second, I knew that one driving impulse of funders was to make a distinctive mark in the philanthropic world, and this impulse was behind the tendencies for foundations to “target” their grants toward given purposes and (hopefully) meaningful outcomes. The Center did not lend itself to this kind of external direction tied to funding. Our operating philosophy was “give us the money, do not ask us to observe any criteria other than academic excellence in spending it, and trust us.” This philosophy was a genuine one, but not the greatest sales pitch in the philanthropic world.

Third, I will confess that I was averse to fund-raising as a type of activity. I remember one day as a child—around 12 or 13 as I recall—I engaged in long self-dialogue about what kind of occupation I would like to pursue as an adult. I was already committed to be an academic of some sort (see above, p. 00), but I went down the roster of possibilities as a kind of exercise in fantasy, imagining how much or how little I would be drawn to each and how well I would do in each. I remember vividly to this day that the occupation at the very bottom of my list was salesman. Despite my striking childhood success in marketing blackberries, gooseberries, and cantaloupes around the neighborhood, I felt that I was, or would be, no good as a salesman. I also felt I wouldn’t like the presumed phoniness, glad-handing, and fakery that sales pitches would involve.

From time to time in my life I have recalled this fantasy game, and have never really veered from its conclusion. So while I fully recognized and accepted that taking the directorship would mean selling the Center to potential supporters, I continued to live with the very real expectation that that selling would be ego-alien, and that I might well fail. It surprised me when I subsequently discovered that I was good enough at it and enjoyed some aspects of it, especially when I got a hit (gained a grant) in this low-batting-average endeavor.

With respect to all other aspects of the Center's responsibilities, I moved forward with the blithe and reckless assumption that I would succeed. I believed that I had an administrative style of permissiveness, understanding, and friendliness that fit the culture of the Center. I believed that my history of interdisciplinary research would enable me to comprehend and discuss the work of most Fellows. I assumed that the Fellows, all committed and high-quality academics, would like me, a committed and high-quality fellow academic. I assumed, slightly more shakily, that my trustees, almost all known to me, were friends, and that we would work together with common purpose and would not squabble. And, most unrealistic of all, I simply made the assumption, and told others, that the term I would serve would be seven years (that just seemed right to me), forgetting that the Board could give me the sack at any moment they became sufficiently unhappy. That is what "serving at the pleasure of the Board" means. But it never occurred to me that I would remain anything other than popular with the Board or come anywhere close to being discharged. I suppose these unrealistic fantasies served me well, because I never had a significant crisis or deep conflict with the Board.

I remember much of my first year as throwing myself deeply and responsibly into my new role. Within the first month I initiated an hour-or-so meeting with each employee of the Center, getting acquainted, exploring their interests, and gently inquiring if they had any reservations or problems in working at the Center. I also greeted and spent time with each incoming Fellow, finding out more about their interests, and, if necessary, reminding them of the Center's policy that Fellows could take on no teaching or other employment and that they would take the idea of a residential center seriously and would not be absent for long periods. I remember reminding myself that, while the

Center was dedicated to supporting individual, usually solitary scholarship, its greatest and most valuable strength was generating a remarkable community during each year.

The other noteworthy fact—surprising in many respects—is that I threw myself with the greatest energy into the fund-raising responsibilities of the Director. I acquainted myself with the officers and staff of our reliable, regular funding agencies. I also undertook the initiative in locating, becoming acquainted with the policies of, and contacting foundations and kindred agencies—mostly European, but some American as well—about the possibility of supporting non-American fellows at the Center. Over time a small number of these investigations paid off. I know that I gave priority to these “salesman-like” activities mainly because I was aware of my own vulnerabilities on the fund-raising score. The trustees obviously liked these entrepreneurial explorations. (I said in jest that a “CEO” of my type could fail in everything besides fund-raising, but if he succeeded in that, he was home free with his Board.)

With respect to my whole seven-year term as Director, I can say that I kept the Center on keel and solvent with respect to its finances. I take no credit for most of this. Whereas the early 1990s were bleak for investors, things turned around during my first years, and the portfolio—which was administered by a Boston financial firm—grew from some \$25 million at the time I arrived to nearly \$50 million late in the decade. Things began to turn sour before I left, and the Center was badly damaged by the 2002 collapse. (Another case of my skipping town just in time). In one of my last annual reports, I wrote a somewhat dire message about changing Foundation funding policies and the perils posed for the Center. However, with one exception to be noted, I was able to hold steady with our reliable donors (Mellon, Hewlett and Spencer Foundations, mainly), and to develop some new sources (augmented grants from the Hewlett Foundation, principally), and to secure a number of grants to bring Fellows from abroad (the Volkswagen Foundation, the Ford Foundation, and other funds from Swedish and Swiss sources).

The one funding disaster had to do with the National Science Foundation. In the 1970s Gardner Lindzey had master-minded an effort to persuade NSF to supply the Center with funds for stipends for a significant number of its Fellows each year (typically the Center would pay up to a certain percentage of a Fellow’s salary, with the expectation

that sabbatical leave pay and research grants would pick up the rest). That support had been in place for almost two decades when I arrived as Director. It was around \$1 million per year, a significant part of a \$5 million budget. I succeeded in securing its renewal for a few years, but midway through my term I learned that the NSF was going to discontinue Center funding. I was not privy to the reasons for this decision, but I knew that NSF, like most foundations, did not want to get into the business of simply subsidizing its beneficiaries annually. I also knew that the Center grant had had a history of being sniped at by other, discipline-based funders in the Foundation, on the grounds that the interdisciplinary grant for the Center was taking money “away” from “their” funding pools. Whatever the causes, the NSF was on course to cut us out. The whole episode was extremely unpleasant for me, for I had to go through all the time-consuming and futile exercise of re-applying for support when I knew the negative decision was already 99% final. I also had to calm the nerves of the Center staff, who naturally feared draconian cuts as a result of the termination. Finally, I had to scramble to make up at least partially for the loss. I made most headway with the Hewlett Foundation. Matters were helped in this effort by the fact that David Gardner, my comrade and friend as President of UC, had been named as President of Hewlett a few years before.

I had one tragic and sobering personal experience in the first year. I mentioned the Simmel lectures in the last chapter, and my possible mistake in taking them on during my first year. But I did, and this meant being away from the Center for the month of June. I discounted this absence by noting (to myself) that June was nearing the end of the Center year, and my absence would be less noticed. Two weeks before I left, I received a visit from one of the Fellows, requesting that he take leave from the Center for two or three weeks. He needed the break, he said, because he was psychologically exhausted, and his psychiatrist had endorsed it. I gave him permission to be absent but asked that he return for our “graduation” ceremonies. I left a few days later. When in Berlin I got the disastrous message that this Fellow had checked into a local motel and slit his wrists in his room. Even though most of the Fellows did not know him (he was basically a loner), a wave of shock went through their ranks. Bob Scott met with them frequently and helped in dealing with the collective grief. One sentiment they expressed to him was that I (as Director) should spend some time in my orienting remarks on the dark and

difficult side of this blissful life at the Center, forewarning of episodes of loneliness and depression. When I got back from Berlin Bob communicated this request to me. I thought it a bad idea, something like treating the fellows like children, telling them condescendingly what they are going to be feeling. However, at one moment I thought of the idea of delivering my “Myth of the Good Life in California” speech at the first weekly meeting of the Fellows, as an indirect way of incorporating the darker side of euphoria in their minds, but not mentioning the year at the Center. I’m not sure what this accomplished, but after one of my presentations, one Fellow came up to me and said, “Are you talking about the year at the Center?” so I suppose some message got through.

As might be expected, my first year was most frantic, largely because it involved so much more insecurity and learning on my part. In subsequent years we were able to undertake several innovations. We initiated some new lines of entertaining Fellows beyond the welcoming cocktail parties at the Director’s house. We established the practice of having groups of Fellows and their spouses/partners with similar interests to our home for late Sunday morning breakfasts. I also knew from years of association with the Center and like institutions that they tended to short-change Fellows’ spouses. Fellows would come with work projects in hand, and basically could begin work comfortably on the first day, whereas spouses did most of the “adjusting” by putting kids in school and acclimatizing to the routines of a new community. We tried to make modest inroads on the problem of accommodating spouses by posting their pictures at a central place in the Center (in previous years only photos of Fellows were exhibited). We encouraged Fellows’ spouses to come to Center lunches. We provided baby-sitters at social gatherings. And when we had extra, unoccupied rooms we gave first priority to providing scholar-spouses with space on the Center premises. And Sharin, my wife, escorted groups of spouses around the Bay Area on a regular basis. Here I might also cite her important role with the Board of Trustees. By tradition we hosted them at a dinner the evening after each triennial meeting. Though the dinner itself was catered, she herself, a master chef of sweet things, prepared three separate delicious deserts for every dinner. These desserts became a matter of Trustees’ lore, with guessing-games as to what they might be each time. I said many times that any Board tensions as well as

dissatisfactions with and criticisms of my role during the day's Board meeting completely evaporated during the dessert orgy that evening.

Keeping Up with the Social-Science Establishment

The Russell Sage Foundation. I joined the Board of Trustees of the Foundation in 1990 for a ten-year term. This was well before my appointment as Director of the Center, but it lasted nearly to the end. It kept me involved there, especially in their residential scholars' program similar to the Center's. A question of conflict of interest arose for discussion: Should I be on the RSF Board if I were simultaneously Director of the Center? The heat of that question rose because Eric Wanner, President of RSF, was also on my Board of Trustees. The Boards engaged in a certain amount of agonizing on that issue, but nothing was done about it during our terms of service.

Social Science Research Council. Later in the chapter I will give an account of my involvement in the production of the International Encyclopedia of the Social and Behavioral Sciences. My co-editor was Paul B. Baltes of the Max Planck Institute in Berlin. He also happened to be the chair of the Social Science Research Council in New York, with which I had also had a long history. Baltes insisted to Craig Calhoun, the Council president, that I be put on the Council. I did not particularly relish that assignment, but it was extremely difficult to refuse, given the pressure from my co-editor. I served conscientiously enough for two terms, but if it were not for the personal obligation to Baltes, I probably would not have taken it on.

The National Academies. Shortly after assuming the Center position, I was asked to be a member of the Committee on International Security and Arms Control (CISAC), a very important body within the National Academy and in the international community. I was a little perplexed by this invitation, because its interests and competencies were not exactly my bag. I had been appointed to the advisory body of the National Labs a few years before, and I had been a Berkeley colleague of CISAC's chair for years. My membership on that committee scarcely deserves mention, however, because I attended only a couple of meetings and, according to my memory, contributed little. The reason for this brevity was simple. At the end of one year I was asked to join the Committee on Behavioral and Social Sciences and Education (CBASSE) of the National Research Council. This was the parent body for all the activities on behalf of the social and

behavioral sciences, and it was definitely my bag. So I resigned from CISAC and joined CBASSE. I served one year as member, then was asked to chair when John Swets resigned that position. That appointment lasted for five years. At that time the National Academies initiated a constitutional change and made the “Committee” a “Division” (thus DBASSE), which meant that until the end of my term (2003) I was also a member of the governing board of the National Research Council.

CBASSE and DBASSE (I teased the establishment of NRC, saying it should become completely electronic and move up the alphabet to EBASSE) were advisory and superintending bodies, which meant approving, monitoring, and evaluating the work of the scores of projects undertaken annually under the umbrella of the behavioral and social sciences in the NRC. Many of these were initiated by Congress and other branches of government, some by foundations, and a few internally. Funding had to be sought for some of these, but this task fell mainly to the administrative staff of the Council. DBASSE is thus not very powerful, but is quite influential. It is a hard-working body, and can take limited initiative in spawning projects and areas of interest. I played my now-familiar chair’s role of identifying issues, occasionally taking initiative, giving expression to consensus when it appeared to be emerging, and keeping order in a basically orderly group.

I was centrally involved in one extremely important enterprise. In June 1997, shortly after his second-term inauguration as President, Bill Clinton launched the President’s Race Initiative. The origins of this initiative are beyond my knowledge, but it was suggested that up to that time Clinton had somewhat soft-pedaled issues of racial injustice and related matters on grounds that these were such hot issues that taking them on would be unpredictable from a political point of view. Elected to a second term, however, he was protected from short-term electoral vulnerability, and he was relatively free to act on his political concerns. Whatever the context, the initiative solidified at that time. One of its strategies was to involve the National Research Council in a major conference on racial trends in the United States and their consequences. As chair of CBASSE, I was asked to be involved in a leadership capacity in organizing a major Research Conference on Racial Trends in the United States on October 15-16, 1998. I was joined in this enterprise by William Julius Wilson, the eminent sociologist of race,

member of the National Academy of Sciences and former advisor on matters of race to President Clinton. I had long been an admirer and friend of Wilson—indeed had pressed for his appointment at Berkeley in the early 1970s. (He later tried, with equal lack of success, to persuade me to come to the University of Chicago). I found his orientation to racial issues to be an admirable mixture of deep commitment and fair-mindedness.

Everyone involved in the conference realized how important an event it was nationally, and the staff of the NRC was extremely thorough in its involvement in the planning, recruitment, and logistics of its organization. I defined my own role as working to the best of my limited capacity to realize the National Academy's commitment to scientific standards and non-partisanship on a topic that slides readily into ideological discourse. In the introduction to the two-volume proceedings of the conference, written with Bill Wilson, I made a special effort to identify definitional, methodological, and conceptual pitfalls in thinking and writing about race and race relations, and made as serious an effort as I could to resolve them. It was my effort to set a right tone. The books themselves (Smelser, Wilson, and Mitchell, 2001) included valuable discussions of cultural, economic, demographic, health and criminal-justice dimensions of race, presented by the nation's best scholars. I regard that enterprise as a fine example of the kind of value that is added to national discourse by the National Academies. Later (Smelser and Reed, 2012) I analyzed the special (and more effective) role of the Academies in relation to expert consultation and advice and contrasted their work with that of most "think tanks." The key strength of the National Research Council is its high level of expertise combined with its relative independence, incorruptibility, and remove from partisan struggle.

The Guggenheim Foundation. For many years in the 1980s I was an outside reader/referee in sociology in for the John Simon Guggenheim Foundation in New York. My appointment, I surmise, was made on the recommendation of Robert Merton and Harriet Zuckerman, both of whom were core members of the Foundation's Committee of Selection. This prestigious foundation awards approximately 75 Fellowships per year to support the research of outstanding scholars in all academic fields (as well as poets, writers of fiction and non-fiction, artists and performers). My role as referee was to read up to 50 applications from sociologists each year and rank them according to their

reputation as scholars and the quality of their intended projects. The final awards were made by the Committee of Selection, to which referees' recommendations were advisory. In 1995, my first year at the Center, Joel Conarroe, President of the Foundation, asked me to join the Committee of Selection as the "social science" member. This six-person group met in February and each member had heavy reading in the preceding weeks. I had the responsibility of reviewing the applications in sociology, economics, political science, law, education, and planning and making final recommendations. After the first year I was asked to chair the committee, and did so for sixteen years.

I can best describe my years with the Guggenheim Foundation as glorious. It meant keeping abreast of the best on-going research not only in my own field(s), but also in the academy in general. The other members of the Committee of Selection were all at the very top of their fields and each was an intellectual virtuoso. I was tempted, even as chair, simply to settle back and listen to the other members as an educational experience. Of course, I could not do this, because the volume of work was enormous, and from time to time vigorous disagreements and heated conflicts between committee members would erupt. I described the mentality of the Committee members as one of "pig-headed flexibility." As chair I developed something of a manipulative art of moving things along, proclaiming consensus when there really wasn't consensus, postponing fights "until after lunch" when things had cooled down and people were drowsy, and issuing mock threats that we must move on or else we were not going to finish the work. I developed this "non-pushy authoritarian" style over the years, and was always teased affectionately for it. With all this the morale of the committee was always high and mutual respect was the rule. I developed a little theory that committees that influence the fates of others' careers—personnel committees on campuses, editorial boards, and award committees like our Guggenheim one—develop a special high morale, camaraderie, and sense of specialness. That theory fits with my experience with such bodies, but I do not push it as a general truth.

I mention one final, unsuccessful aspect of my Guggenheim work. One of the distinctive features of the Fellowship is the small size of its grants. My own fellowship in 1973-74 was \$12,000, which, even when discounted for inflation, was less than one-third of my salary. During my time on the Committee of Selection stipends had

increased—\$45,000 to \$60,000—but still barely enough to supplement sabbatical pay, and woefully inadequate if the recipient had no other income. At the other end of the scale, the Guggenheim did not have much meaning (except some prestige) for physicists, chemists, life scientists, medical scholars, and perhaps some economists, whose opportunities for federal and large foundation grants made the Guggenheim seem puny. This was reflected in the limited size of the application pools in these fields. Over time I developed the argument—and its justification—that the Guggenheim should change its policies and direct more of its funds toward the humanities and arts where other sources of support were limited or non-existent and, for that reason, the numbers of Guggenheim applications were enormous. Accordingly, I argued, the Foundation should considering discontinuing support in the natural sciences and direct more to the humanities and arts, where it was clearly needed. I saw merit in my arguments, but stopped making them over time because they fell on deaf ears of trustees and staff, who remained steadfast in the conviction that Guggenheim fellowships should span the whole spectrum of scientific, academic, and artistic endeavors.

The length of my service as chair of the Committee of Selection reflects my commitment, my joy in serving, my affection for the other committee members and staff, and my closeness to the two Presidents with whom I worked—Joel Conarroe and Edward Hirsch. I stayed on and on after my retirement from the Center, retiring from the Guggenheim work in 2012 at the age of eighty-two, not because I wanted to but because I felt I should and felt that others felt the same. I still miss the work, the meetings, and the people.

At a moment in the late 1990s, a Fellow at the Stanford Center posed a question to me during a conversation at lunch: “Are you Mr. Social Science?” He was referring to the membership, responsibilities (and presumed power) of the positions I have described in this section. To be honest, the question surprised me, even though one might see reason in posing it. About once a year, when we all happened to be in the same place, I, as head of the Center met with the leaders of the Social Science Research Council, the Russell Sage Foundation, and the chief staff officer of CBASSE, to discuss general issues facing the social sciences. We thought of ourselves, but only seldom and in a semi-articulated way, as representing the “social-science establishment.” The reason that I was

surprised by the question is that I defined whatever roles I had at the time in more modest, piecemeal ways, and very seldom conceived of what I was doing as an exercise of power or prestige, or in guiding important things. Rather, I thought I was doing important things. Part of this self-conception is rational because I believe that there isn't such a thing as a social-science establishment in power terms; things are too fragmented and decentralized to permit that characterization. But part of that self-conception was my own doing and emanated from my reluctance to permit illusions of grandiosity as part of my self-definition, though I know that such illusions were surely latent.

Keeping a Finger in the Natural Sciences and in Medicine

I had re-joined the National Laboratories' Scientific and Academic Advisory Committee in 1990, after my sabbatical leave in New York. In 1992 it became the President's Advisory Council, and the Academic Senate became a more important presence as other non-science representatives joined this academic supervisory body. As I indicated, the issues before the lab changed as the nuclear armaments component of their work faded and they scrambled to adapt. In the several years that followed I played a full and active role in policy discussions. Participation continued to be very rewarding, and a part of the rewards was a trip to the Santa Fe-Los Alamos area each year with my wife to renew Southwestern ties. I had to have my security clearance renewed in the mid-1990s. This was more routine than the first one, though I remember a grilling on why I had maintained social contact with a Russian graduate student in sociology (when she was at Berkeley) who had been one of our guides in Moscow on the 1989 academic visit to Russia. She was also a dissenter in the late Soviet era. I thought that line of questioning inane ("Isn't the Cold War over?" I thought of asking). In 1998 I was asked to accept another term of appointment with the labs, but at that time the burdens of the Center and the Encyclopedia were so heavy that I declined to continue. Incidentally, that decision was yet another "get-out-of-town-in-the-nick-of-time" events in my life, as the Lee security case broke into the open and came to consume the Los Alamos laboratory and the University of California shortly after I left the PAC.

I also deepened my association with the American Board of Internal Medicine, joining its Advisory Board in 1992, and taking over as its chair for the period 1995-98. This meant attending all Board meetings and serving on a number of important task

forces. One of these was membership on Task Force on Examination Standards, 1995-97. The circumstances leading to its formation were the following. Different medical specialties had different “pass rates” for residents, and the American Board of Internal Medicine, traditionally priding itself on “standards”, was lower in pass-rates than many other specialties. In the meantime, various hospitals and medical centers around the country had taken to “advertising” themselves as having high percentages of board-certified physicians on their medical staffs. Some members of the Board asked whether ABIM’s relatively low pass-rates (i.e., higher standards) were not punishing internists in the market. This concern led to the formation of that Task Force. I was asked to be a member. It was a very delicate mission, and I was among those that pressed for refusing to alter standards in a way that would increase pass-rates. The whole exercise was an interesting episode in a contest between maintaining professional standards and promoting the market position of the specialty. I was gratified when the task force recommended no alteration of standards, and that decision added to my respect for the board of that specialty. Later I became centrally involved in two other important assignments, one a committee to study and recommend on issues of conflict of interest among physicians, and the second an international committee of internists to prepare the highly influential “physician’s charter” for the “the new millennium” (American Board of Internal Medicine, 2002).

Some Miscellaneous Academic Writing

In the last decade of the twentieth century my psychoanalytic pen came alive again. I have mentioned the theme of ambivalence, including its central place in my Presidential Address for the American Sociological Association. In addition, I had written a comprehensive analytic essay (Smelser, 1993b) on the strengths and weaknesses of psychoanalytic explanations in non-clinical settings (psychohistory, the analysis of formal organizations, community crises, and aesthetic products). I also wrote an essay on Erik Erikson as social scientist (Smelser, 1996) an invited paper at a conference remembering the man after his death. At a given moment I reviewed my psychoanalytic writings over four decades and concluded that they might add up to something coherent. I floated the idea of a book of collected essays with the University of California Press and they agreed immediately to publish them (Smelser, 1999a). I also wrote two

“statesmanlike” essays. The first was an analysis of a quarter-century of the Annual Review of Sociology and its representation of the discipline (Smelser, 1999b). I had been active in the founding and early stages of that publication. The second was a biographical essay on the vicissitudes of the disciplinary (sociological) and interdisciplinary themes in my social-science career (Smelser, 2000b).

I mention a final item because it was so important for me. Earlier (above, pp. 00) I wrote about my prolonged involvement with the Carnegie Commission for Higher Education. A major ingredient of this was a deepening of my friendship with Clark Kerr, the legendary Chancellor at Berkeley and President of the UC System. He came to seek my advice on various matters, and ultimately to help him with his memoirs (Kerr, 2001; Kerr, 2003). He would reminisce and run ideas by me. I remember a special moment. Clark was uncertain about how detailed he should be in his account of his role in the years of student activism (1964-67). He asked me what my advice on this issue would be. I told him that he had a distinctive role in that history and a distinctive personal story to tell, and he owed it to the world to be as exhaustive and honest as he could be. Then, I added, “be sure not to brood too much.” I said this because he had taken to brooding with me in our conversations, wondering whether he should or should not have done this or that on various occasions. When I said that, he blinked, his face went blank, and he said, simply, “brood?” It was a telling moment to me both because he was so preoccupied with his brooding that he couldn’t recognize it, and because he could not recognize the semi-therapeutic role I was playing.

As time went on Clark began sending me draft materials. I would comment as frankly as I could, sometimes at the risk of offending him. I told myself that being complimentary, and especially flattering, would be of little help to him, except possibly to nurse his vanity. One day, late in my term as Director, I received a call from Clark asking me to come to Berkeley for a “meeting.” I went, and after a warming-up conversation, he asked me to write the Forward to his memoirs (Smelser, 2001; Smelser, 2003b). I was stunned, because I knew there were scores of national educational leaders (of whom I was not one) who were more likely candidates and who would leap at the chance. I did leap at the chance, and wrote two serious introductory essays, analyzing Kerr’s times and travails, and commenting on his intellect and his personal style as well

as his historical significance. Kerr liked my introductions, and he asked me to omit only one thing. I had said, in assessing the vicissitudes of his life, that among his other sensitivities he had a “sense of the tragic.” Kerr asked me to omit those words, but he didn’t say why. I surmised that they rubbed against his deeply ingrained Quaker optimism.

After the books appeared, I still found myself wondering why Kerr had asked me to write an introduction over the more plausible others. On one occasion he and I were being chauffeured from San Francisco to Berkeley after an interview on his memoirs on Bay Area public radio. They had asked me along as an interviewee, I learned, because Kerr was very old by then, and they wanted a relief pitcher in the interviews. As we were crossing the Bay Bridge, I screwed up my courage and said to Clark that he did not have to answer this question if he didn’t want to, but why had he invited me to do the Foreword when there were so many more obvious candidates? He did not hesitate, but said that he admired my capacity for analysis and objectivity, and preferred that to a simply adulatory commentator. This pleased me, of course, but it was a bit ironic, for in the Forward I had singled out Clark’s capacity to “objectify” and “analyze” historical situations, even those about which he had strong feelings and by which he was personally threatened. We apparently had developed a little mutual respect society based on our proclivity to distance ourselves from things!

A Festschrift By Any Other Name . . .

Sometime in the late 1990s Jeffrey Alexander, one of my most noted students, came up with the idea of the idea of producing a Festschrift honoring me, and told me about his plans. About the same time I learned that two other favorite students, Gary Marx and Christine Williams, had been hatching a similar plan, but completely separate from Alexander’s efforts. They talked to me as well, so I suggested they join forces. They made a perfect editorial trio. Each was a generous, dedicated, and distinguished scholar, and they represented three different decades of students—Marx the 1960s, Alexander the 1970s, and Williams the 1980s—and they showed no “ego problems” in collaborating on the project. They asked me to suggest additional students who would be appropriate authors, and I obliged with names, though I was careful to assure them that the ultimate choice of authors was theirs.

Alexander approached the University of California Press as the favored publisher, appropriate because I had been at Berkeley for many decades and had authored and edited well over a dozen books for the Press. The press was interested in principle, but informed Alexander that they had discontinued publishing Festschriften. The reasons for this were budgetary; such books did not generate widespread interest among readers, but more tangibly, they produced little revenue for strapped university presses. The Press and Alexander agreed that the editors could submit the book, however, if it were put forward not as a Festschrift but as a collection of social-scientific essays on a theme. This agreement resulted in a kind of unholy understanding that the editors and authors would proceed as though it were a Festschrift for me—without ever mentioning that word in the text—and the Press would evaluate it simply as a scholarly product and more or less permit but ignore those parts of chapters that were Festschrift-like encomia for me. This proved a convenient formula for all.

The work proceeded apace, and in general I was not part of it. A couple of the authors, reverting to their graduate student days I suppose, sent me drafts of their chapters and asked for comments on (and possibly approval of) their efforts. I thanked them for their consideration but refused to comment critically, explaining that I wanted their contributions to be exclusively their own and not subject to any kind of endorsement or critical input from the figure to whom the work was dedicated. As the final product turned out (Alexander, Marx, and Williams, 2004), it was in my mind the perfect Festschrift, each chapter honoring me and some aspect of my work generously, but each also developing an engaging and high-quality essay that was a distinctive and independent statement about their own work. To me that Festschrift—plus the fact that four of my students have dedicated scholarly books to me—symbolizes the special pride I have taken in my career in cultivating and supporting, not directing those dozens of students whom I have taught.

Editing the International Encyclopedia of the Social and Behavioral Sciences

The publication of encyclopedias of the social sciences has a visible history in the twentieth century. The initial publication, executed in the early 1930s by Macmillan (Seligman, 1937) was a very influential document, and featured independent, important statements on the part of established figures in the social sciences. The second, published

in 1968 (Sills, 1968), also by Macmillan but of larger scope, was the editorial product of David Sills, but a strong influence was evident in the persons of Talcott Parsons, Robert Merton, and Edward Shils. I was asked by the editors of Social Forces to write a review essay for the entire 17 volumes. This was an absurd request in many respects (how could anything meaningful be said in four journal pages?). It was also absurd for me to accept it (how could I say anything meaningful?). Despite this, they did ask and I did accept. In that essay I traced how certain substantive areas (e.g., psychoanalysis) fared in the pages of the work. I also commented—appropriately enough in a sociological journal review—on how the editor had built in an impression of the dominance of “functionalist sociology” in its pages (Smelser, 1970). I thought this a somewhat casual observation at the time, but it had an implicit criticism that Sills’ editorial decisions constituted a kind of “bias.” That observation also contained a kernel of what became my operating philosophy of inclusiveness when I took on the editorial responsibilities for a new encyclopedia.

On the unspoken principle that a new Encyclopedia is needed every one-third of a century, some publishers began thinking of such an enterprise in the years around 1990. When I was at the Russell Sage foundation in 1989-90, I and a few other scholars, including Merton, met with representatives of the Free Press to discuss the desirability and feasibility of a new version. The intellectual consensus was positive on this score, but the publisher reminded the scholars of the large capital investment required to undertake the enterprise. Apparently that disincentive was enough to discourage the Free Press and other publishers from acting at that time. In fact it was a giant publisher with an adequate capital base—Elsevier, a Dutch publisher with offices in Oxford—that decided to move ahead with the project. It was envisioned as a massive, 26-volume set with 5,000 entries and double the number of words of the 1968 edition.

My Early Involvement. Elsevier contacted me shortly after I had assumed the Directorship of the Center. I assumed they approached me as a “representative” of the social sciences. They wanted me to convene a “feasibility conference” of social scientists from several disciplines. As I recall, they suggested that the Center pay for this conference, but I balked, saying I would convene it but not pay for it. They accepted that, and shortly thereafter I invited a number of “usual suspects” to join me in such a

conference. It took those gathered only about twenty minutes to come to consensus on the desirability of a new version, and we spent the entire remainder of the mini-conference debating themes, strategies, and issues (such as the inclusion of biographies). I pressed the argument that a second such conference should be held in Europe if the anticipated encyclopedia was to be considered a truly international enterprise. All agreed with this idea, including the Elsevier representatives, and Bjorn Wittrock of the Scandinavian Center for Advanced Study in the Social Sciences agreed to host such a meeting in Uppsala a few months later. The Elsevier staff asked me to come to that gathering for purposes of continuity of discussions, and I agreed.

Up to this point, nobody, including Elsevier, had raised any questions about the editorship or editors. However, one month before the Uppsala meeting, Barbara Barrett, the chief Elsevier representative, came to the Center and asked me outright to be editor-in-chief for the entire project. I was not completely surprised by the request, but I did not exactly anticipate it, either. I realized, however, how important a decision it was. For that reason I told her that I would not say yes and would not say no, and would wait until after the Uppsala meeting to let her know. She accepted this delay; she more or less had to or risk my saying no on the spot. One thing I did say on the spot was that it was absolutely essential that in order to be truly international, the Encyclopedia had to have a European co-editor-in-chief, though I did not go into any particulars.

I was very torn by the offer. On the positive side, the editorship would be an appropriate culmination of my interdisciplinary career; it would symbolize my leadership in the social sciences; my royalties would exceed one year's salary (though I did not know the exact sum at the time, and I was not in need of income); and it would be a feather in the cap of the Center as host institution for this canonical publication. There was only one negative: the editorial job was so huge that it might erode my performance as Director, including my fund-raising. This was so important that I thought the Center Board of Trustees might simply order me not to take on the editorship.

Accordingly, at the next Board meeting, I reported the offer to the Board, and asked for their advice. The Board was as torn as I was. On the one hand they saw the advantages for me and the Center; on the other hand, they knew that I was a new and partly unknown Director, and the demands of the editorship might indeed diminish my

performance and harm the Center. Two or three of the Directors came out and said, simply, that I shouldn't do it; two or three others said it would be a wonderful thing for me and the Center. And separately and privately, Gardner Lindzey, also a Board member, told me that that I should under no circumstances take on the editorship and that I should dedicate the last years of my career to the Center and only to the Center (as he had done, incidentally). At the end of a long and spirited discussion on the issue, Eric Wanner said, directly and definitively, that it had been an interesting Board discussion, but in the end, he concluded, "this decision is up to Neil, and if he feels he can do the job and be an effective Director, he should do it." That ended the discussion at that point, but it did not end the issue. True, no Board member complained subsequently that I was neglecting my obligations because of the editorship, but I can assure readers that at every Board meeting until the end of my term I reported carefully and in detail on the progress of the project, and, when the volumes were published, I donated my second complementary copy (which retailed for \$10,000) to the Center library.

Not long after the Uppsala planning meeting I decided to take the position, informed Elsevier, and immediately began thinking about a European co-editor. I was as knowledgeable as the next about the European social-science scene, but I felt it necessary to consult with others on the issue. I landed finally on the name of Paul B. Baltes, Director of the Max Planck Institute for Human Development in Berlin and an internationally known and respected psychologist. His interests were primarily micro, mine macro (though he insisted that I take responsibility for all psychoanalytic aspects of psychology). I did not know him well, but Gardner Lindzey did, and testified to his credentials, abilities, and diplomatic skills. The Elsevier people did not think his appointment a good idea, because he had declined to come to the planning meeting in Uppsala, and they believed that signaled a lack of interest in the Encyclopedia on his part. I discounted this reasoning and overruled them, saying that any one of us might have declined to come to such a meeting for any number of reasons, and that not coming did not mean anything more than that. They withdrew their objections, and I approached Baltes. He was as torn as I about such an assignment, and we talked about it several times before he agreed. He turned out to be a brilliant choice. He brought extraordinary skills and experience to the job. Our division of intellectual responsibility was a

comfortable one. In dealing with Elsevier we evolved, spontaneously, a kind of “good-cop-bad-cop” pattern; he was the tough guy who insisted on contractual provisions and timely payments of royalties and made certain Elsevier was going to live up to its editorial promises; I was the arbitrator, conciliator, and nice guy with the publisher. We collaborated well. In an exceptional initiative, he insisted early and graciously that my name be listed first on the title page—alphabetical logic said that Baltes should come before Smelser—on grounds that I was the entrepreneur for the project. He would hear of no objection to this listing. I was able to arrange that Baltes could come to the Center as a Fellow for the year of most intensive planning and decision-making (1997-98); our geographical co-presence was absolutely essential at that stage.

Architectural Work. The first issue facing Baltes and me was how to organize the Encyclopedia conceptually. This meant creating “sub-sections”, or the best conceptual categories to organize past and on-going knowledge in the behavioral and social sciences. Such a list was important in that it provided the categories for choosing “section editors,” responsible for each “section,” subdividing it into topics, and selecting individual contributors for each topic. Making these decisions determined the entire intellectual architecture of the encyclopedia, and once all those categorical decisions and selection of editors and authors were made, the rest was implementation.

The most convenient way to organize knowledge in the social sciences is to do so by “disciplines” as these have developed historically and become institutionalized in the academy. The obvious choices were anthropology, economics, political science, psychology, and sociology. But there are other disciplines that have pursue social-scientific topics—education, law, geography, history, and philosophy. Do we choose them, too? Baltes and I took an inclusive approach here and listed fifteen, naming them and choosing a section editor for each.

We knew we could not stop here, however, because of the development of other extra-disciplinary lines of work that were important and informed by social-science research. How to include them? First, we created section headings for endeavors that were “overarching,” reaching across disciplines. We included several methodological topics (statistics, research design), and sections on history of the behavioral and social science, ethics, and institutions and infrastructure of research. Another major

comprehensive category we called “integrative concepts and issues,” and listed mainly interdisciplinary fields such as area and international studies, environmental/ecological sciences, evolutionary sciences, gender studies, and religious studies. And finally, we listed a number of “applied social science” endeavors such as public policy, urban studies and planning, and organizational and management studies.

These efforts to comprehend the social and behavioral sciences (and section editors for each topic) were mainly Baltes’ and my doing. They yielded 40 sections and 40 section editors (plus a few co-editors). As in the whole encyclopedia exercise, we consulted continuously with knowledgeable colleagues. We also made a point of involving the Encyclopedia’s International Advisory Board. This was a list of nearly 100 international luminaries. Most encyclopedias have such a board, but typically it is a cosmetic list—lending prestige to the publication and a little more eminence to those listed—but not a working group. Baltes and I made an exception to this, and consulted with all members on our section categories and, even more decisively, on appropriate section editors. This was enormously helpful, and, we were convinced, added great value to our product. On occasion we got more advice than we wanted, though it turned out to be valuable. One example: in pondering the philosophy section and the section editor we named, one of our International Advisory Board members, Juergen Habermas, objected strongly, saying that that section was biased toward analytic philosophy and neglected many continental European traditions. He carried his objection to the point of saying that if that bias persisted, he would withdraw as Board member and complain publicly. This put significant pressure on Baltes and me—especially Baltes because of the German connection—significant enough that we suggested a philosophy co-editor, agreeable to the other section editor and to Habermas, thus calming the scene.

As part of the planning, we assembled all the section editors at the Center in April 1998 to deal with issues of comprehensiveness and overlapping. That meeting occasioned a few disciplinary spats that had to be quieted, but by and large it was a very harmonious gathering. Within a matter of a couple of hours all general issues seemed to be settled, and the meeting broke spontaneously into horse-trading, with couples or groups of section editors agreeing on who would handle what topic that might fit into more than one section. One might have expected some intellectual possessiveness to

develop in these negotiations, but as often as not section editors were happy to “get rid of” subtopics by handing them to another, so they could expand their own sections with their favorite topics. I think that collective meeting significantly reduced the inevitable overlapping in such a sprawling enterprise as that encyclopedia.

Biographies. Many encyclopedias include biographical entries on both important historical figures and contemporary leaders in their areas of concern. Biographies had a very important place in the 1935 encyclopedia, less so in the 1968 edition. From the beginning this was a topic of contestation. Even at the first planning meeting Elsevier said it didn’t want any, because the choice of “biographees” was such a matter of conflict, especially with respect to those still living. Even dead ones excited passion and lobbying, because choosing one over another meant a symbolic endorsement of a certain emphasis in a discipline or field. Under bombardment from various points of view, we chose to include only 150 entries, and only deceased figures. That decision stuck, but was not without controversy. Karl Ulrich Meyer, our most skillful biographies section editor, was bombarded with requests for additional entries and some criticisms of his chosen entries. He (and we) also got pressures from individuals and groups on to include living scholars as well; we resisted because we knew that would have opened a snake pit of agitation and competition. In the end, by way of compromise and conciliation, Meyer prepared a list of “borderline cases” of people who nearly made the list but could not because of the lid of 150 biographical entries. Baltes and I considered the biographical entries to be of value to the Encyclopedia, but the Elsevier people were right in predicting that this issue would be something of a running sore.

Representativeness. Baltes and I were committed to the principle that the Encyclopedia should be as international as possible in order to deserve that name. We also knew that its pages would be dominated by North American and West European authors, especially the former, because of the world concentration of the social and behavioral scientists in those regions. We knew that the social sciences had grown in other regions of the world, but that they would be under-represented in our processes of consultation and search. We also knew that, by different dynamics, women would be under-represented as well. Aware of these issues, at an early moment in process of building our contributor list, we contacted all the section editors and made a request. We

asked that if, in their search, they had topics which revealed “ties” between men and women nominees, or between a North American or European candidate and one from other areas, they choose the latter in both cases. We also asked that they take initiative in seeking out scientists in the latter categories.

I had a special little problem in recruiting the sociology section editor. If I chose an American, this would be regarded and perhaps commented on if critics thought that I was pressing “my kind” of sociologist; in any case some issue about my “bias” might arise. I gave some thought to this issue, and “resolved” it, I suppose, by choosing the French sociologist, Pierre Boudon, for the position. He was a highly-respected general sociologist, and somewhat removed from parochial French in-fighting among sociologists. To my knowledge, his nomination was not criticized, and, in addition, he was a very responsible and responsive colleague. I was able to fill him in on various developments in North American sociology as he was compiling his list of topics and authors.

Reviewing the Contributions. In the negotiating phases before we signed our contracts, Baltes and I made a number of important agreements with the publisher. While we would be ultimately responsible for the intellectual content and quality of the Encyclopedia, the publisher’s staff would be responsible for the logistics of contacting the authors, distributing contracts, enforcing deadlines, receiving manuscripts, and distributing them to us. Paul and I each received sums from Elsevier for administrative and clerical expenses. I hired members of the Center staff and a research assistant from Berkeley to help with reviewing manuscripts. From a quantitative point of view, the review of manuscripts was the single activity that consumed most of my time.

Paul and I divided responsibilities for the submitted entries according to our areas of expertise, and both of us read the biographical entries and those in the “overarching topics” areas such as research methodology and the history of the social and behavioral sciences. I made a point of reading over every one of the entries that fell into my bailiwick, and I estimate that I returned about 10 per cent of these to the section editors for improvement or augmentation. I rejected only two entries outright. One was an ideological diatribe against contemporary Western imperialism submitted by a Muslim scholar commissioned to prepare an entry on the history of Islam. I explained to him that

he had not written on the assigned topic and that that his essay was a partisan statement not suitable for an Encyclopedia entry. The author complained and even threatened a lawsuit, but the matter died. The second rejection was of a sketchy outline pretending to be an essay on the topic of planning, submitted by a Chinese scholar. I rejected it on grounds of its intellectual inadequacy and got no reaction from the author.

Finis. Two copies of the 26-volume Encyclopedia (Smelser and Baltes, 2001) were delivered to my Berkeley home a few weeks after I had finished the Center directorship. The postman was both perplexed and overburdened by the delivery, and I had to explain the editorship and the encyclopedia to him. The delivery was a very happy moment for me. The encyclopedia was an enormous intellectual enterprise, valuable for our social-science endeavors. In addition, I convinced myself that taking on that adventure had not realized others' or my apprehensions about damage to my directorship of the Center.

The story of the Encyclopedia was not quite finished, however. Sales of the volumes mainly to libraries, embassies, and individuals were brisk, despite the asking price. Elsevier decided, after a brief post-publication period, to prepare a second edition. They also asked Paul and me to be the editors-in-chief of that, in light of the relatively happy mutual relations we had had and, I presume, on the basis of the quality of our editorial work. This second edition was going to be a fully electronic one. This had already been anticipated by the publisher, who was arranging the electronic distribution of our first edition for a fee to libraries around the world, who would then make it available to their patrons online. It was that distribution that created a tempest, which I now report.

Paul and I had asked that a provision be entered in our contract that royalties on all forms of distribution be paid to us. We had received several advances along the way, which we welcomed. Some time after the publication, however, Elsevier began offering the online version to libraries for a fee. One of those libraries happened to be that of the Max Planck Institute that Bales directed. Naturally it came to his attention, and we raised an inquiry about royalties. Elsevier responded by saying that there were none, and there would be none, in large part because they "could not be calculated" in light of the fact that they were part of larger "package deals" with the libraries. Paul and I were

infuriated but got no satisfaction. By chance Paul's wife was a lawyer, and she took on the role of instructing us in how best we could abuse and threaten the publisher, and we followed her advice. However, Elsevier, which we assumed had a large legal staff, would not move. The only real weapon Paul and I had was to refuse to edit the second edition, and we held off letting them know about our decision, despite the fact that both of us were thoroughly sick of encyclopedia editing and would not take on the second edition under any circumstances. After months of wrangling Elsevier offered each of us \$50,000 to end the dispute, and Paul and I, exhausted, agreed that we should take the money and run. We also declined the offer to do any more editing.

Paul died tragically some years after the publication of the Encyclopedia, and the loss of this comrade and friend saddened me greatly. Elsevier did decide to go ahead with a second edition, and the editors of that enterprise invited me to accept the position of honorary chair of its international advisory board. I accepted with pleasure, and have also prepared an entry on Collective Behavior, to appear in 2015.

Chapter 10

Unemployed But Not Retired—2001-

A Thought on Retirement in General

As retirees go, university professors are a blessed class. For many other jobs and callings, retirement has an either-or, or “cold turkey” aspect—more or less total disengagement from an organization or practice—with some freedom to take up some standing or new avocations or fall into passivity. This pattern also applies to many teachers in elementary and secondary educational institutions, as well as community-college faculty. In the case of university professors, retirement can involve little, partial, or general withdrawal from “normal” career activities. Many institutions have provisions for continuing to teach on a part-time basis; they often provide for “associations” of retirees in which common activities are pursued; some academics can continue in “service” roles in professional societies and elsewhere; and the same academics can continue to engage part-time or full-time in the research aspects of their calling. The key word is “discretion” in the post-retirement phase of life. Given my whole life pattern, I assumed I would remain at the “activist,” “involved” end of this spectrum of choices.

At another level, the opportunities for choice diminish. Those who are still fully engaged have a way of “forgetting” those who retire, and for that reason fewer opportunities for active engagement arise. I recall a comment that Henry Rosovsky made decades ago. He had decided to leave the Berkeley faculty to take a position at Harvard. The timing of that decision, however, was such that he remained at Berkeley for almost a year before actually departing. Late in that “moratorium” period he remarked to me in some seriousness, “people here [at Berkeley] treat me as if I were dead!” This captured the meaning I have in mind about retirement. No matter how active one wishes to be, and no matter how much one remains engaged, others come to forget that one is still present, and that leads to a partially unconscious, variable, selective, and cumulative pressure toward isolation.

At an even deeper level, retirement has an identical meaning for all, whatever the continuity with life’s activities. It is a symbol of some kind of termination, and it imposes a meaning—no matter how one adapts to it—that life has reached an end phase, and that one is approaching death. The increased incidence of health problems in the

older years continues to remind one of this fateful fact. No matter how passive or active, how engaged or disengaged one is in the retirement, this meaning looms and demands some kind of psychological and spiritual adaptation. We all must come to terms with this universal meaning in the retirement years.

First Plans for Retirement

My scheduled date of retirement from the Center was September 1, 2001. Months before I had been given a generous retirement party by the Board, staff and friends of the Center in the Stanford community. The Board also had had given me a little extra gift and said I could depart in July, so we were settled back in Berkeley a month before the formal date. To temper the reality of final official retirement I had scheduled a trip to Norway, beginning September 12, for a formal lecture in Oslo and a week or two of travel across the country to Bergen. That trip did not happen. Instead, the September 11 attacks happened. We were as paralyzed and shocked as the rest. A personal footnote was that our San Francisco International Airport was closed for four days and there was no way we could make the trip abroad. I telephoned my hosts in Oslo and received expressions of concern and sympathy, as well as assurances that we could come anytime we wished in the future. Ultimately we went in June of 2002. I changed the topic of my lecture to the social psychology of terrorism. Though I did not know it, many of my activities—both practical and scholarly—would be devoted to this topic for several years.

In the meantime, as of September 12, we had ten days of completely open time before us; nothing had been planned for that period except the Norwegian trip. We were transfixed by the events on and after September 12, of course, and I knew that if we stayed at home we would be daily glued to the television, viewing re-runs, re-hashes, speculations, and more speculations (the “CNN syndrome”). Impulsively we jumped into our small camper and drove to Seattle to visit our son and then to wander around Washington and Oregon for a week. I felt the impulse to visit Mount St. Helens, which we did, to remind myself of the violence in this world. And of course we remained glued to the camper’s radio.

I had also thought about restructuring my life after leaving the Center. I had decided that I did not want to join the Berkeley Department of Sociology in any active teaching or administrative capacity (the “I’ve paid my dues” mentality), though I did

agree to supervise a “thesis-writing” seminar in the fall semester, 2001, for students who were at the dissertation stage of their graduate training. This was a minor but positive commitment, my only departmental activity, not to be repeated. I also kept alive some on-going activities, including a post-directorship term on the Board of Trustees of the Center and my work with DBASSE, my role with the Guggenheim Foundation, and re-affiliation with the Center for Studies in Higher Education on the Berkeley campus. The main thing I planned was to initiate research and write a book on the odyssey experience. I had been doing some thinking and formulating while still at the Center, but now was the time to begin the serious search-and-research phase of that work. Though my energy was not diminished, I also planned to slow my pace somewhat, spending more time in our country cabin on the Klamath River in Northern California and renewing our life with friends in Berkeley. My wife and I joined the Drama Section of the Berkeley campus. I took to reading parts in plays at their monthly meetings, and ultimately acting in plays presented by the Town and Gown Society in the Berkeley community.

The Social Sciences and Terrorism.

The whole nation was traumatized and to some extent mobilized in the period after 9/11. The National Academies—science, medicine, and engineering—were no exception. Within a short time after the attacks, the Presidents of the three wrote a letter to President Bush pledging to do what they could in helping to cope with this national crisis. A principal activity emerging from this resolve was to form a national Committee on Science and Technology for Countering Terrorism, mobilized quickly and charged to submit a report within a year. The main emphasis was to be on “countering” and defending against attacks, and within that an emphasis on the scientific and technological capabilities of the nation to maximize the effectiveness of those efforts. It was a large committee, numbering 24, described as “the nation’s leading scientific, engineering, medical, and policy experts” (Committee on Science and Technology for Countering Terrorism, 2002, p. xi). Two members were social scientists. The first was Thomas Schelling, the noted economist and game theorist, who was one of the pioneers of deterrence theory; the reasons for his selection to the Committee are self-evident. I was also selected, but wondered why. I had never done any research or writing on terrorism. It is true that my appointment as chair of DBASSE and member of the Academy’s

Council made me visible. More remotely, my experience with the national laboratories might have had some relevance. Finally, some aspects of my research—on collective behavior, social movements, disaster, and some types of social change—were relevant. But still I wondered.

The committee worked expeditiously. We met four times over the next eight months, decided on priorities, cooperated efficiently, drafted materials, and responded to reviewers. We organized the chapters primarily around types of threats—nuclear and radiological, health, toxic chemicals, explosive materials, energy systems, transportation, and urban infrastructure. Several others dealt with issues of complexity and interdependence in thinking about terrorism and, more practically, how best to equip the federal government to counter terrorism. If I were to evaluate the report, I would say that it ranked very high in quality of analysis, in objectivity, and (I am guessing here) in its impact on thinking about the multiple facets of defending against the threats of terrorism.

Approaching the assignment in advance, I thought my presence and influence would be marginalized in two ways. The first was a general expectation based on my knowledge that the National Academies are, above all, “scientific” in culture, and that the behavioral and social sciences have long been diminished in their regard either because they are not really “sciences” or because they are “soft,” or both. And given the general composition of the committee, I was certain this bias would carry over into its work. The second, closely related, was that the committee’s emphasis was predominantly technological, and my own interests leaned toward explaining terrorism as a socio-political phenomenon in the contemporary world. In the general culture of the committee that seemed to be taken as a historical “given” and the major problems were how to deal with (“counter”) international terrorism’s many manifestations.

I was wrong in both these anticipations. I found that my interventions on the “human” responses to terrorism were heard and responded to respectfully, and in many cases incorporated into the committee’s thinking. My consistent emphasis was that terrorism as a threat generates complicating psychological and social effects—fear of attacks, initial responses, political reactions, recovery, and enduring public expectations, and that these, as well as technical and technological considerations, shape the nation’s reactions and responses. I was asked to draft the chapter, “The Response of People to

Terrorism,” and other committee members and reviewers found little to suggest by way of changes. I concluded that I had been oversensitive in my initial expectations.

I did get involved in a political fight on another facet of the committee’s work. A few of us suggested that, as a way of balancing the primary “countering terrorism” emphasis, there ought to be a section in the introduction to the report that commented on the origins of terrorism as a type of warfare and, in a general way, on its political significance in the contemporary world. This would be a few pages at most, and would appear in the opening chapter as an orientation to readers. A first effort at drafting was made by a few members of the committee—some of the “policy” experts. It was a provocative little document, using the term “evil” freely and its “causal” analysis described contemporary terrorist attacks as the work of fanatics and was laced with references and analogies to the fascist movement in Nazi Germany. A number of us objected to this language on grounds that it was not a proper analysis of conditions and causes, but it was an ideologically loaded document that had no place in a science-based report. In the face of this criticism, the chair of the panel asked me to draft some alternative language, which I did. I did my best to make that language neutral, but did incorporate topics such as economic and cultural penetration of non-Western societies, the current international power situation, and demographic imbalances in terrorism-prone societies.

My alternative language “failed” in that it proved as objectionable to the members of the original drafting group as theirs had been to us. My language was labeled “ideological” and “sympathetic” to terrorist groups, and was dismissed as so much “social science fluff.” The objecting group went further, saying that if that kind of prose found its way into the report, they would disown it and resign publicly. This tempest was obviously a crisis for the leadership of the committee, and the co-chairs did the only thing they could. They rejected both drafts of the introductory material and left the report without that kind of orienting language. The larger issue, of course, is what it means to understand and explicate social “causes” and “conditions.” The original drafting group equated this with some kind of “soft thinking” at best and sympathy with terrorism as well as taking blame on ourselves at worst. “My” language implied, I hoped, that one could understand and explain an abhorrent phenomenon separately from one’s moral

evaluation of it. Neither side prevailed in this little war, and the ideological ambiguity of the relations among explanation, understanding, abhorrence, and sympathy has not disappeared from our political discourse about terrorism.

The “voice” of the behavioral and social sciences in considering terrorism—stifled in a fashion by this tempest in the larger committee—was, however, heard in a different way. As part of its operations the master committee commissioned the creation of eight separate sub-committees, each corresponding in a rough way to one of the chapters. A member of the larger committee chaired each group, and each was charged was to go into greater detail and circumstance than the parent committee could do. My sub-committee was designated to deal with “Behavioral, Social, and Institutional Issues.” The co-chairs of the parent committee also laid it down as a matter of policy that it would grant full freedom to each subcommittee and make no effort to alter its activities or report. So my committee—composed of ten leading behavioral and social scientists—could go about its work freely. We did, and addressed issues such as the colonial past, globalization, cultural conflict, the implication of “statelessness,” as well as the motivation and organization of terrorist attacks (see Smelser and Mitchell, 2002a).

In connection with the Academies’ activities on terrorism, I was given one more task. In the middle of its work the National Research Council was approached by DARPA (Defense Advanced Research Projects Agency), the research arm of the Department of Defense. The question that interested them was also countering terrorism, but their focus was on deterrence theory, that body of thought that had developed during the Cold War as a way of understanding and dealing with the issue of nuclear attack by the superpowers. Is such a theory relevant or not to the containment of contemporary terrorism? Our subcommittee, with somewhat altered composition, turned to this as well, and produced a brief, thoughtful, and qualified report, stressing the limited applicability of deterrence theory to recent terrorism (Smelser and Mitchell, 2002b).

I report two incidents that reveal tensions between government and academia. The first arose when one member of my social-science subcommittee (incidentally a Berkeley colleague) came to me when the DARPA arrangements were put into place. He explained that he had political reasons for not wanting to work in any way for the Defense Department, and wanted to withdraw from our subcommittee. Fearing an

“incident” of public criticism of our work on deterrence, I persuaded him to remain as a member of our general group, but simply not come to the sessions on deterrence. We would also remove his name from that group’s list of members. This proved a happy enough solution for him, and the issue died.

The second reflected two different “mentalities.” As a matter of diplomacy I had asked the chief DARPA representative to the first meeting of the group on deterrence, hoping to “socialize” him informally on how we were thinking. He agreed, but before the meeting began he took me aside and said, in all seriousness, that DARPA wanted a report of no more than one-and-one-half pages out of this group. I did not react to or mention this demand to the group, but we commenced and carried out free and rich explorations of the issues of deterrence of terrorism. I was especially gratified because our DARPA representative appeared to be “grabbed” by the quality of our deliberations, and even joined in excitedly from time to time. Then, about 3 p.m., he thanked us and announced that he had to leave for another meeting. On his way out he summoned me aside for a second and whispered in my ear, “remember, one-and-one-half pages.” So much for the convergence of academic and government mentalities, I thought. In the end we submitted and published a report of forty-odd pages. DARPA accepted it without reservation and it disappeared into the defense establishment.

I mentioned I had never worked on terrorism before, or on the more general topic of the sociology of war. However, during 2002 I became something of a scholar and expert on the former by virtue of interacting with experts, reading essential works on terrorism and drafting the text for two reports and a chapter of a third. The thought of pushing the work further never occurred to me. It was primarily a commitment dictated by the critical times, and I was grateful to provide my bit of constructive thinking. However, several members of the Academy staff argued that I had something independent to say, and should write a scholarly book on contemporary terrorism. I reconsidered, and remained firm in my resolve not to do so. I was deep into my “odyssey” research and didn’t want to be distracted. Next, however, I was approached by the National Academies Press (for which I had done some advisory work) to write a book on terrorism for its “Science Essentials,” a book series aimed at bringing science and scientific topics to an audience wider than “the experts.” I reconsidered once again and

this time agreed. This meant shelving the odyssey project temporarily, because I knew how much more research on terrorism I had to complete, especially the literature in political science, psychology, and history.

Consistent, I believe, with my social-scientific, political, and social “take” on terrorism—and contrary to many approaches to that appeared in the scholarly and popular literature—I chose the following emphases:

- To avoid blanket, good-evil approaches to understanding the phenomenon and stress dispassionate understanding;
- To stress the non-rational and irrational elements in the genesis of terrorism and reactions to it, and de-emphasize rational explanations;
- To appreciate the importance but also the limits of technical and technology-based defenses against terrorism (as shorthand, I labeled these first three approaches, taken together, as the “goodness, game-theory, and gadgets” emphases);
- To identify a number of “entrapments” in and dealing with terrorism, that is unproductive, repetitive, and non-useful snarls and predicaments, such as the tension between national security and civil liberties in responding to terrorism;
- To appreciate the misdirections of both a hawkish “war on terrorism” and the “liberal” idea of long-term amelioration as ways of countering contemporary terrorism, and to rely more on an alternative, more patient approach I called “patience by death by strangulation”;
- To emphasize and analyze the ideological bases of much terrorist activity.

I divided the book more or less equally between the causes and conditions giving rise to terrorism on the one hand, and the immediate, middle-term and long-term responses to terrorism on the other. The book (Smelser, 2007a) comes as close as anything I have written to the genre of policy analysis.

Back to the Odyssey

Engaging as it was, the project on terrorism was a diversion from—and meant postponing work and progress on—a project that I regarded as original and compelling—the systematic analysis, expansion, and consolidation of and thinking about periods of moratoria, which I came to call odysseys. Actually, the project had been germinating, if

one can call it that, for decades, ever since my years as director of the Education Abroad Program. In my thinking I had moved toward including more and more phenomena that resembled one another sufficiently so that I could treat them as a class with common characteristics. Ultimately I defined the odyssey experience as “a finite period of disengagement from the routines of life an immersion into a simpler, transitory, often collective, and often intense period of involvement that often culminates in some kind of regeneration” (Smelser, 2009, p. xi). Under this heading I included religious experiences such as pilgrimages and conversion, tourism, rites of passage both religious and secular, many ritual experiences such as childbirth and dying, and some coercive ordeals such as brainwashing. As of 2005, the remaining work called for more theoretical refinement, extensive research in many academic disciplines, and organization of the exposition.

Once more fortune smiled on me. One day in 2004 I received an open-ended invitation to be a Kluge Fellow at the Library of Congress for a completely free period of three to twelve months’ duration, to be dedicated to scholarship, and supported handsomely with an honorarium and staff assistance. Though I had been doing extensive research at my Berkeley base for some time, I knew that several months of intense and isolated study was just what I needed. Accordingly, in the fall of 2006 I spent several months in the confines of that magnificent institution, and basically completed the empirical research. What a joy to sit in my oak-paneled office, ordering a long list of books, all of which appeared within two hours, and then disappeared within an hour or two after I had finished them. My wife and I thrived in the cultural wonders of the nation’s capital, and, in a camper, renewed our attachment to the beaches of the Eastern seaboard and the beauties of Appalachia on weekend jaunts—odyssey-like experiences in themselves.

I had intended the book to be a straightforward scholarly one, that is, based on research and synthesis of academic research on the classes of phenomena I had decided to include in the project. In the early exposition, however, I included a couple of brief stories of odyssey-experiences of my own—the summer stay at the Salzburg Seminar in 1951, and the memorable leave of absence in Europe in 1973-74. I submitted some early draft material to the University of California Press in an effort to persuade them to publish the book. They accepted it readily, but Lynne Withey, the Director, more or less

ordered me to expand the biographical examples as much as I could and devote an entire chapter to them. I resisted this suggestion inwardly—believing that to do so would be too self-promotional, too immodest, too egotistical. (Could I never be cured, even in my seventies, of this dynamic of self-advertisement followed by self-degradation?). I followed their advice, however, and add that, in reviews and comments on the book by readers, this chapter received notable and positive acknowledgement.

From time so time, in reflecting and in talking to others, I have referred to the book on terrorism as my “unhappy” book because of its focus on death and destruction and the book on odysseys as my “happy” book because of its emphasis on disengagement from routines and on self-regeneration.

Adventitious Teaching.

As indicated, I planned no post-retirement teaching, but I was not adamant on that subject. The one-shot supervision of dissertation-level graduate students seemed just about right during my first year of retirement. Toward the end of that year, however, I was approached by Richard Schleffler of Berkeley’s School of Public Health. He wanted me to teach one course per year in that school’s post-doctoral program in social-science research on health and health policy, financed by the Robert Wood Johnson Foundation. Four post-doctoral students were selected each year for two-year terms. In their first year they were required to take an interdisciplinary seminar, offered by an economist, a political scientist, and a sociologist on the approach of each discipline to issues in health, health delivery, and health policy. I knew about the program, because I had been in on its design before departing for Stanford. It seemed just right for a retiree—one course a year, not excessively demanding of time, an unheard of student-faculty ratio of 4:3, and the brightest of the fresh doctorates in country. I seized the opportunity and co-taught in the course for more than a decade until the program was phased out in 2015.

Two other teaching adventures were frankly opportunistic. Our youngest daughter is a print-maker, and in 1999 had taken a faculty position in the University of West Virginia in Morgantown. She had given birth to our first grandchild in 2001. Quite by accident, a young sociology student was enrolled in one of her art classes, and one day asked if she was related to Neil Smelser. She acknowledged that I was her father. The student took the news to the sociology department, and they approached me

forthwith with an invitation to teach a term in 2002 at West Virginia. A happy coincidence. However, after I had accepted she received an offer from Illinois State University in Normal, Illinois, and departed for her new assignment one month before we were to arrive in Morgantown! The West Virginia people subsequently told me that they expected me to withdraw, but I felt strongly that I should honor my contract. I did, and I enjoyed teaching a research methodology course to a bright group of undergraduates, and a course on my own research to graduates and young faculty. I also had my classes scheduled mid-week so that we could acquaint ourselves with the beauties of West Virginia and elsewhere on weekend camping trips. (We had purchased a small Volkswagen camper at the moment of retirement from the Center.)

Undaunted by the frustration of missing our daughter's family in Morgantown, several years later I swindled a visiting appointment for one semester in the sociology department at Illinois State for the fall semester, 2005. We lived around the corner from our daughter (and by then two grandchildren), thus making up for our disappointment several years earlier. I taught economic sociology (the second edition of my Handbook of Economic Sociology, with Richard Swedberg, had appeared recently) and enjoyed a group of very able students again. Toward the end of our stay the Dean of the College of Arts and Sciences called me into his office, and invited me to join the faculty of Illinois State as Chair of the Sociology Department! I suppose he thought I was fair game because of the presence of my family there. I told him I was flattered to be invited at age 75, but I that I had already retired twice and couldn't quite bring myself to initiate a third career.

Periodic teaching and lecturing trips to Trento, Vienna, Berlin, Krakow, Istanbul, and Bern rounded out the teaching activities in my post-retirement years, and permitted the renewal of ties with European colleagues. A rewarding feature of these was that most of them were at the initiative of my former students now at those locations—a kind of “payback,” in my mind, for earlier congenial relations that I had with them in their Berkeley days.

A Return to General Education

In the late 1990s Michel Schudson, a sociologist at the University of California, San Diego, and Columbia University, and I sat on a national group called the Penn

Commission on Society, Culture and Community, a national task force commissioned to examine problems and prospects in American society. It ultimately published a major report (Rodin and Steinberg, 2003). During the course of the Commission's work, Schudson and I became much better acquainted and discovered that we were in agreement on almost all issues that came before the Commission.

Late in the Commission's work, Schudson gave me a call, wondering whether we might not undertake some initiative in bringing issues of general education and liberal education in America to the fore. I had been in this territory in the 1980s in my work on lower-division education in the University of California (above, pp. 00-00), and my commitment to the vitalization of that tradition was still very strong. My only hesitation was that for decades those aspects of American higher education had deteriorated, and that the dozens of reform initiatives on undergraduate education had also foundered in one way or another in competition with universities' commitment to research emphases, graduate training, student vocationalism, and diminished status granted to creative undergraduate teaching in universities. Could yet another effort on behalf of undergraduate education have any impact in the face of these other, massive developments?

Despite these misgivings Schudson and I decided to move ahead, but we didn't know exactly how. In the end we decided to try to persuade the Office of the President of the University of California to launch a major commission on the topic. We called on Jud King, Vice President and Provost at the time, to talk up the idea and gain institutional and financial support. We received enthusiastic moral support, but no funds, except for a modest \$10,000 to convene a feasibility meeting at the Center for Advanced Studies in Higher Education at Berkeley, which had also expressed interest in our proposal. We held that meeting, constituted by several major spokespersons inside and outside the University, and easily gained support, enthusiasm, and suggestions for a major effort.

In the meantime I began seeking for funds outside the University during 2003. I was well positioned to do this, because my years at the Center had generated both knowledge and personal contacts with many Foundations. I had little difficulty in persuading the Carnegie Corporation in New York and the Hewlett Foundation in Palo Alto to pony up approximately \$100,000 each to finance a commission, which proved an

ample sum. During that time, we also began searching for and recruiting the best cadre of potential members for such a commission among faculty and administrators on all the UC campuses, as well as a few outsiders. Almost everybody we contacted agreed to serve.

Over the next two years the commission met five times and hammered out a long report (which Schudson and I mainly drafted, along with Diane Harley of the Center). We included a historical account of the vicissitudes (mainly decline) of liberal education, attempted to identify the long-term trends responsible, made recommendations for curricular and program reform (including revamping the ideas of a “major” and a “minor”), and suggested bolstering administrative responsibilities to promote undergraduate programs. We developed a special statement on education for citizenship and civic responsibility (University of California Commission on General Education, 2006). My own insistence throughout the process was that we fashion out as many reforms as we thought valuable and feasible, but that we should make no effort at a wholesale “rolling back” or dismantling major historical changes, knowing that the latter effort would be touting at windmills, given the history of American higher education.

The Center gave the report as much publicity as it could, and several forums were held. The reception, such as it was, was favorable, but in my estimation the impact of our efforts was no more than that of many other such reports in the past. Certainly it generated no major wave of interest or raising of consciousness, to say nothing of a visible “movement.” It was of interest to us that an unusually high (in relation to our initial expectations) number of European universities expressed interest, not surprising in retrospect as we learned that concern with general education has grown there in recent years. Schudson and I concluded not only that the effort was worth it as an intellectual and social endeavor, but also that our initial low expectations about what we might achieve were confirmed.

John Reed and Usable Social Science

John Shepard Reed has fashioned one of the most successful and unusual careers in twentieth-century American banking. It includes youthful years in Argentina (his father was a businessman), a bachelor’s degree in economics at the Massachusetts Institute of Technology, a successful apprenticeship at Citibank, and directorship of that

organization during the last sixteen years of the twentieth century. More pertinent to my story is that through his life he has also cultivated a knowledge of, faith in, and active participation in American social science and its institutions. He read widely, actually commissioned social-science research on his own organization, and served on boards of trustees of many social-science organizations: the Russell Sage Foundation, the Center for Advanced Study in the Behavioral Sciences, the Spencer Foundation, and the National Bureau of Economic Research. After his retirement from Citibank he served as interim head of the New York Stock Exchange during a season of scandal and reform, and has spent several years as an activist chair of the Corporation of the Massachusetts Institute of Technology.

I became acquainted with Reed during my many years of association with the Russell Sage Foundation, including my membership on its Board, 1990-2000, and during my many years on the Board of Trustees of the Center for Advanced Study at Stanford (1981-1993) and directing the Center (1994-2001). He was a Board member during my entire directorship. We had a good relationship from the beginning, though I would describe the early years as respectful but somewhat remote. We did not always see eye to eye but I can recall no notable conflicts. We became closer during my years as Director, partly because our respective positions (I as Director and he as Board member) called for more circumstantial interaction, and partly because my wife and I hit it off very well with his second wife, Cynthia (Cindy). We came to count ourselves as personal friends. John was a dedicated, obviously smart, conscientious, and outspoken Board member, and I confess some moments of intimidation. On the very few occasions we came into conflict, however, I did not back down. I recall one example. In the middle years of my term, not long after the National Science Foundation had discontinued its funding of the Center, the Board was having a serious discussion of a possible budgetary crisis. In that debate John put forward, in all seriousness, the proposal that the Center consider moving to another, less expensive location than Palo Alto. (He had recently engineered a successful move of his bank's credit operations from New York to South Dakota.) This triggered a heated reaction from other Board members, and I strongly opposed John's proposal, largely because of my conviction that the proximity of the Stanford community to the Center was crucial to its success as an institution, despite its forbidding housing prices and high cost

of living. At a given moment I turned to him and said, “John, what you are suggesting is like proposing that the Rockefeller Center at Bellagio move to Dusseldorf!” That more or less terminated the discussion, and John backed away in good humor.

We continued to see the Reeds socially after my retirement from the Center, whenever we came to New York or when they were visiting or passing through the Bay Area. At a moment in one of these visits (I place it somewhere in 2005) John said he would like to support a major inquiry on the applications and uses of the social and behavioral sciences. He would support the endeavor with funds from a foundation that he and Cindy had formed at the time of his retirement. His faith in academic social science had not diminished. (I teased him from time to time for having more faith in the social sciences than social scientists themselves.) More to the point, he asked me to head up the enterprise. I was taken by surprise by the request. I knew John respected me and my work, but this was more than I expected. The request was not an unreasonable one. I had written a reflective essay on the application of social-science knowledge to social problems (Smelser, 1995) and recently lectured on the “sociology-of-knowledge” implications of the structure of the academy (Smelser, 2007b). But did not say yes. I was in the middle of writing the odyssey volume, and I knew how many new lines of research I would have to explore if my involvement were to be consequential. I was genuinely uncertain and stewed about the idea for months.

After those months, and when I was in the home stretch of the odyssey enterprise, I said yes to John, without knowing exactly what kind of endeavor this “usable social science” project would be. We decided first to invite a number of respected people to a speculative one-day planning meeting held at Citibank in November 2007. Besides us, the group included Susan Fiske (psychology, Princeton), Robert Jervis (political science, Columbia), Alan Kruger (economics, Princeton) James Peacock (anthropology, University of North Carolina, Chapel Hill), Richard Scott (sociology, Stanford), and Stephen Stigler (statistics, University of Chicago). The meeting was exceptionally helpful to us. We came to consensus within an hour that such a project was a worthy one, and spent the rest of the day bringing up and refining specific emphases that might constitute chapters in a volume on applied social science.

That helpful meeting did not, however, solve the problem of what kind of project this would be and what my role would be. The two options were either to commission an edited volume with chapters on selected topics or areas or to commission one author to write a comprehensive, synthetic volume. The former was attractive in that it would bring in more people with more expertise. However, I knew from my history of editing almost thirty scholarly volumes over my career that there is no conceivable way of guaranteeing analytic coherence and continuity among a dozen or so academics, no matter how dictatorial an editor endeavors to be. After more stewing, I agreed to undertake the enterprise as primary researcher and author, and John agreed to be involved in detail at every stage.

The work required was enormous, and I estimate that I spent two years, more or less full-time, exploring and analyzing relevant lines of research. We wanted to cover micro-areas (e.g., memory, decision-making), meso-areas (organizational dynamics) and some macro-areas (notably economic development). We wanted to cover decision-making, social policy, and social problems as areas of applicability. The division of labor between us was that I would do most of the reading and drafting and that John would review everything critically, discovering errors and misplaced emphases, and suggesting new directions. I also asked that he write brief essays on major decisions he had confronted in his career; I would re-write these and assemble insights as to how social science was and could have been helpful. These exercises appeared as “boxes” throughout the text. We also included analyses of the academy, of “think tanks,” and independent commissions on the “supply side” of knowledge. The most important element in our collaboration was our agreement on a fundamental assumption that we articulated as we went along: we were skeptical of both simplified formulae (principles of social engineering, ultra-rational schemes) and radical rejections of the applicability of knowledge. From very different backgrounds, we both ended up in the “middle”—systematic social science knowledge as a tremendous but always contingent resource in practical policy and decision-making. This convergence of views meant that John and I had no fundamental disagreements about what we were doing.

We didn’t decide on the details of authorship until near the end of the project. At one extreme I could have been listed as sole author, since I had done the lion’s share of

the research and almost all the writing. At the other it could have been simple, alphabetical co-authorship, Reed and Smelser. In one of our final conversations I brought up the idea that I would be listed first (violating the convention of listing authors in alphabetical order) but he would be listed simply as co-author, without any qualifying language. John immediately accepted this as appropriate. That easy solution symbolized the ease and mutual respect that this collaboration manifested from beginning to end. The book (Smelser and Reed, 2012) is so recent that it has not found its location in the literature as yet, but we both hope (and anticipate in our more optimistic moments) that it will take a significant place in the history of the purposive application of systematic knowledge in our struggle to improve the critical relations between knowledge and action in organized society.

A Return to Higher Education (“Reflections”).

At some moment during the work on the Reed project, it occurred to me that I had, in effect, written but never published a book on the University of California during my decades of teaching and service (both on the Berkeley campus and systemwide). The core materials for this project were the long essay on governance written in 1994 while in the President’s Office (above, pp. 00-00), the publicly-available but never published committee reports completed during my years as “Mr. Report,” and several articles on higher-education politics in higher education in general and on the Berkeley campus in particular. Two other essays also came to mind, one calling for an as-yet-unwritten statement on my post-FSM administrative role and another a written by-product of an episode of service with the Chancellor’s office after my return to Berkeley in 2001.

The first had to do with a long-latent promise I had made to myself to write up an autobiographical-institutional-political essay on my period of service in the Chancellor’s Office with Martin Meyerson in 1965, after the climax of the Free Speech Movement (above, pp. 00-00). These were hot and consequential political days in the history of the campus, but they had been neglected by comparison with the more dramatic months of conflict that preceded them. For years colleagues on the Berkeley campus had urged me to write up the experience for the historical record, and for years I had resolved to do so “one day” but had never put pen to paper. The idea of producing a general book of reflections reinvigorated this long-latent intention. Accordingly, I took temporary leave

from other projects and buried myself in the Chancellor's Office archives in the Bancroft Library, reading minutes of meetings and memos I had written, reviewing the Bay Area press coverage of events, and digging into my memories. I wrote all this up, including many personal reactions and assessments of events and actors in those heady months. It ultimately appeared as the first chapter of the book on reflections (Smelser, 2010a).

The second essay was another by-product of an episode of service. In 2002-3 the campus had experienced a minor political crisis associated with the SARS (Severe Acute Respiratory Syndrome) outbreak in East Asia. One of the apprehensions of the Berkeley administration and the public health officials was that the epidemic would be brought to Berkeley by Asian visitors and generate a health crisis that the campus could not contain. In connection with these fears the campus cancelled or postponed some seminars with Asian participants as measures of precaution. The "crisis" associated with this episode was that some Asians reacted negatively to these cancellations and regarded them as a cultural "slap" at Asians, as a matter of "face." This reaction spread to some Asian-American groups in California, important constituencies for the Berkeley campus, as well as financial donors. Accordingly the campus had to engage in a season of delicate diplomatic fence-mending in response to this "surprise" development.

After the scene had calmed, Chancellor Robert Berdahl constituted and convened an informal committee called "Committee X" and later the "Committee on Surprises" to look systematically at occasions on which the campus had been taken by surprise and found itself in an unanticipated situation of political conflict, and, perhaps more important, to scan the horizon for upcoming situations and events that might become "surprises" in the near future and explore ways of anticipating and dealing with them. Berdahl asked a number seasoned administrators and faculty to serve on Committee X, and I, a couple of years after my return to the campus, was among them. I thought the formation of such a committee was a splendid idea and joined without hesitation. We met approximately monthly for a semester, but did not continue into the term of Berdahl's successor.

Midway during the work of the Committee, it occurred to me that I might be able to contribute something to its work by writing a systematic essay on crises at Berkeley over a forty-year period. That essay would describe the nature of surprises, their course

of development, and how the campus responded to them. I broached the idea to John Cummins, the campus administration's "chief of staff" and member of the committee, and requested support in the form of two research assistants. John agreed immediately, and I set to work preparing an essay, called "Surprises at Berkeley," in which I tried to identify precisely what constitutes a crisis, to provide a sample of historical episodes, and to develop a "policy analysis" of how the campus could prepare itself to anticipate, contend with, and hopefully minimize the political damage of "surprises." The essay was the basis for discussion at the last meeting of the Committee on Surprises. I thought it would be helpful to disseminate this essay—of which, incidentally, I was very proud of as an intellectual accomplishment—by including it as one of the chapters of my "reflections on the University of California" (Smelser, 2010e).

I have to report some personal feelings I had about this "reflections" enterprise. Rationally I knew that I was bringing a new and unusual kind of book on higher education onto the scene—not a traditional "memoirs" book by a past administrator, not a straight "history of higher education" publication, but a new *mélange* of scholarly and personal work by a "non-positional leader." I also experienced very positive feedback on the book in the reviews I read and from colleagues who read it. Yet in characteristic fashion I found ways to denigrate it: Were not many of the essays old and out of date? Who would be interested in reading committee reports? Why would the University of California Press be interested in publishing it in the first place? (They were in fact very interested.) I had long known and lived with this tendency to doubt my accomplishments, but I could not prevent such doubts from surfacing.

More on Higher Education: The Kerr Lectures.

Since the beginning of the century the Center for Studies in Higher Education on the Berkeley campus had sponsored the Clark Kerr Lectures, given every two years by a leader in higher education and on a theme in higher education. The lectures were delivered primarily at Berkeley but one was typically given on another campus to underscore their significance for the University of California as a whole. The first four lecturers were all past university presidents: Harold T. Shapiro (Princeton), Charles Vest (MIT); Donald Kennedy (Stanford), and Hanna Holborn Gray (Chicago). Their selection

was the responsibility of a special committee of the Center, on which I had served for a number of years. The University of California Press published the lectures in book form.

At the time the selection committee was to meet to choose the fifth lecturer for 2012, I received no notice of the meeting and no invitation to attend. I made little of this, and did not inquire about it, thinking perhaps that there were difficulties in scheduling its meeting or that my term as member had expired. A few months later, however, Jud King, Director of the Center, informed me that my name had come up as a nominee for the lectures, that I had been removed from the selection committee that year for reasons of possible conflict of interest, and that the committee had chosen me. I was surprised at this action, given that ex-administrators had dominated the list of lectures in the past. In giving reasons to myself, I presumed (but never verified), that the publication of the Reflections volume was an important factor in my nomination, perhaps establishing me as some kind of spokesperson on higher education and eligible for this honor. In any event, I accepted the invitation immediately.

I wanted the lectures to be sociological in emphasis, and I wanted to stress the implications of the spectacular growth and success of American higher education from the beginning. Among these implications of this growth was what I called the phenomenon of “blistering” or the adding on of functions, structures and groups by institutions of higher education, as well as the rigidities and conflicts entailed that the fact that these institutions seldom shed such accretions. While I had articulated this idea relatively well, to deliver a series of lectures on the larger implications of the process necessarily involved me in a massive reading project in the history of American higher education, in memoirs of past leaders, in celebrators and denigrators of colleges and universities, in economic and statistical analyses, and in policy studies. I wanted the lectures to be a thorough, scholarly project.

The period of preparation (mainly the year 2011) was marked by a blessed episode. My wife is a photographer, specializing in constructing photographic quilts. She applied for and was awarded an artist’s residency for May, 2011 on the North Rim of the Grand Canyon, long one of our favorite camping spots. This meant living in a little cabin right on the rim for three weeks, she doing her photographic work and making a couple of presentations at the Lodge. I went along as a “trailing spouse”—in

the lingo of the residency program—a role that my wife really appreciated, given her years of “trailing” me on sabbaticals and other occasions. We took full advantage of the Canyon’s wonders, but I still had a lot of time. I filled our car with library books on higher education, read them while at the Canyon, and constructed arguments that would constitute my lectures. It was difficult to think about the grimmer sides of contemporary college and university life in that setting, but I managed to force myself to do so.

I delivered the lectures themselves in January and February of 2012—all three at Berkeley and the third one again at the University of California, Riverside. They drew large, appreciative audiences, all savvy about higher education, so the question periods were long and lively. I believe the lectures were well enough thought through to stimulate if not provoke the audiences. Actually, I treated the three as one organized lecture, laying out the fundamentals of my theory of structural accretion as a principle of growth in higher education, and, in the context of that exposition, discussing one consequence after another in the remaining lectures. I organized the subsequent book version (Smelser, 2013b) along the same lines, attempting to bring as much order as possible into the confused kaleidoscope of change that higher education manifests in its history and contemporary situation.

The third lecture on the Berkeley campus produced an episode of hilarity. It was scheduled for late afternoon on a Tuesday in February. Quite independently, I had accepted an invitation for the annual black tie dinner (immediately after the lecture) of the Berkeley Fellows, a society of esteemed administrators, faculty, alumni, and patrons of the campus. Jud King, master of ceremonies for the Kerr lectures, was also a member of the Berkeley Fellows. I suggested to him that we both wear tuxedos to the third lecture, and I would explain to the audience why we were so dressed at the beginning of the lecture. He agreed to the plan, but discovered that his tux had been misplaced, and he came in a mere suit. I explained all this to the audience. After I had duly delivered the lecture, I was pacing back and forth on the platform, answering questions, when the cummerbund fell off in my hand. I was totally nonplussed for an instant, but recovered. I held it aloft and announced to the audience that “my cummerbund has fallen,” then marched over and placed it, as a stripper might, on the podium, announcing to the audience, “this is not what it might seem!” It was a nice, unanticipated light note in an

otherwise staid academic occasion. In the foreword to the subsequent book I thanked Jud for his help, “both formal and informal,” in arranging the lectures.

An Unanticipated Joy: My Oral History Project

At some moment late in the first decade of this century, John Cummins, long-time chief of staff in the Chancellor’s Office my long-time comrade-in-arms, approached the Oral History office of the University of California Bancroft Library and sold them on the idea of organizing an oral history of me. He justified his campaign on the fact that I had been of exceptional service to the university and larger society, and that I was “approaching eighty.” The Bancroft staff accepted the idea, but explained to me that their acceptance was conditional on their raising some \$35,000 for the project, and could I be of help in any way? They did not ask me for a donation, and I would not have given one—too much like a vanity press, I thought. But I did provide some names, and in the end much of the funding was supplied by a grant from the Carnegie Corporation of New York.

I was happy (and honored) to undertake the experience, but I did not expect anything special from the oral history project. I was sorely mistaken in that anticipation. There were more than twenty two-hour interviews, conducted in my home over a period of several months in 2011-12, and yielding almost eight hundred pages of text. The two interviewers—Lisa Rubens of the Oral History Staff, and Jess McIntosh, a graduate student in American intellectual history—were intelligent, sensitive, thorough, and usually on target with their interview questions. At the same time I was given ample opportunity to elaborate on my responses and turn the conversation in new directions. Though more structured and in a completely different personal setting, these sessions recalled my psychoanalysis to mind, in which I delved into my personal history in a semi-structured way, made new discoveries and fashioned some new ordering of my life. Also, as with a psychoanalytic session, I ended each interview with a combination of feelings of some euphoria, some anxiety, and some exhaustion.

Over the course of the interviews I learned many new things about myself and my career. In particular, in pondering over a series of unconnected essays on sociology as a discipline, written in the 1980s and 1890s, I discovered a number of thematic convergences around themes that I had not recognized or articulated earlier (above, pp.

00-00). So striking was this realization to me that I decided to bring these writings together, along with a few others, and publish a book of essays expressing my new insights. I called this book, somewhat presumptuously, Getting Sociology Right (Smelser, 2014), but that expresses my continuous effort to clarify things in writing about my own calling.

These observations seem to strike the right note on which to end my biographical explorations in this book. It would be tempting to sum up with some kind of flourish, some effort to encapsulate a grand pattern of my life's work. That, however, would be both grandiose and wrong. Particular for a wanderlust, a life cannot be summarized in to a comprehensive, overall pattern without presumption and distortion. Instead, there are themes only partially related to one another and, more generally, life is acted out in different environments with different, sometimes contradictory demands. Life is a historical mix of activity and passivity, a mix of opportunism and accident, and above all a mix of gratifications and disappointments. Few authors like to conclude on such a note of indeterminacy, but it is the only honest thing to do.

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- 2004b "Epilogue: September 2001 as Cultural Trauma," in Cultural Trauma and Collective Identity, by Jeffrey C. Alexander, Ron Eyerman, Bernhard Giesen, Neil J. Smelser, and Piotr Sztompka. Berkeley, CA: University of California Press. Pp. 264-82.
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2010d "Governing the University of California," in Neil J. Smelser, Reflections on the University of California: From the Free Speech Movement to the Global University. Berkeley: University of California Press. Pp. 173-210.

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Press.
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Turning Points: Historical and Sociological Essays on the Family,
edited by John Demos and Sarane Spence Boocock. Chicago, IL:
University of Chicago Press. Pp. 278-315.
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- 1980 The Changing Academic Market: General Trends and a Berkeley Case
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Contemporary American Society. Princeton, NJ: Princeton University
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- 1969 "Psychoanalysis and Sociology: Articulations and Applications,"
International Journal of Psycho-Analysis. Vol. 50. Pp. 693-710.

Appendix A

Curriculum Vitae

Name: Neil Joseph Smelser

Birth: July 22, 1930, in Kahoka, Missouri

Home address: 109 Hillcrest Road, Berkeley, CA 94705

Professional address: Department of Sociology
410 Barrows Hall
University of California
Berkeley, CA 94720

Marital status: Married, 1954, to Helen Margolis (divorced 1965); married to Sharin Hubbert, 1967.

Family status: Four children: Eric Jonathan Smelser, born 1957; Tina Rachel Smelser, born 1959; Joseph Neil Smelser, born 1968; Sarah Joanne Smelser, born 1971.

Education:

1936-48: Primary and secondary public schools in Phoenix, Arizona.

1948-52: Harvard College. B.A. in Social Relations.

1952-54: Magdalen College, Oxford University. Final Honours School of Philosophy, Politics, and Economics. B.A. 1954; M.A. 1959.

1954-58: Harvard University. Ph.D., 1958.

1963-71: San Francisco Psychoanalytic Institute, Research Training Candidate. Graduate psychoanalyst, 1971.

Teaching positions:

1958-60: Assistant Professor of Sociology, University of California, Berkeley

1960-62: Associate Professor of Sociology, University of California, Berkeley.

1962-72: Professor of Sociology, University of California, Berkeley.

1972-94 University Professor of Sociology, University of California. Emeritus, 1994-Present.

Research appointments: 1955-58: Junior Fellow of the Society of Fellows, Harvard University.

1959-61: Center for the Integration of Theory in the Social Sciences, University of California, Berkeley.

1961-63: Faculty Research Fellow, Social Science Research Council.

1962: Auxiliary Research Award, Social Science Research Council.

1965-66: Research grant, National Science Foundation.

1970-71: Research grant, Ford Foundation and American Academy of Arts and Sciences.

1973-74: Guggenheim Fellowship.

1975-76: Research grant, Russell Sage Foundation.

1976-77: Research grant, Ford Foundation.

1978-79: Research grant, Russell Sage Foundation.

1980-82: Research grant, National Institute of Education.

1989-90: Visiting Fellowship, Russell Sage Foundation.

1992 Residential Scholar, Study and Conference Center, Bellagio.

2006 Kluge Fellow, Library of Congress

Administrative and related positions:

Assistant to the Chancellor for Student Political Activity, University of California, Berkeley, 1965.

Vice-Chair, Department of Sociology, University of California, Berkeley, 1965.

Assistant Chancellor for Educational Development, University of California, Berkeley, 1966-68.

Chair, Policy Committee of the Berkeley Division of the Academic Senate, 1971-72.

Member, Executive Board, Berkeley Faculty Association, 1972-73, 1974-76, 1979-83; 1991-92; Chair of the Board, 1975-76; 1980-81.

Associate Director, Institute of International Studies, University of California, Berkeley, 1969-70, 1972-73, 1981-89.

Chair, Department of Sociology, University of California, Berkeley, 1974-76, 1991-92.

Staff Member, Department of Psychiatry, Cowell Hospital, University of California, Berkeley, 1966-76, 1981-82.

Vice-Chair of the Governing Board, Joint Medical Program, Berkeley and San Francisco Campuses of the University of California, 1977-78.

Director, Education Abroad Program of the University of California, United Kingdom/Ireland, 1977-79.

Chair, Educational Policy Committee, University of California, Berkeley, and Member, University-wide Educational Policy Committee, 1979-80.

Member of Governing Board, Joint Health and Medical Sciences Program, Berkeley and San Francisco Campuses of the University of California, 1979-82.

Chair, Commission on the School of Education at the University of California, Berkeley, 1981.

Chair, Berkeley Division of the Academic Senate, 1982-84.

Chair, Task Force on Lower Division Education in the University of California, 1985-86.

Vice-Chair and Chair, Academic Council, Assembly of the Academic Senate, University of California, 1985-87.

Faculty Representative to the Board of Regents, University of California, 1985-87.

Clinical Supervisor, Psychology Clinic, University of California, Berkeley, 1984-93.

Member, Committee on Committees of the Berkeley Division of the Academic Senate, 1988-89.

Acting Director, Center for the Study of Higher Education, University of California, Berkeley, 1987-89.

Chair, Blue Ribbon Commission on Intercollegiate Athletics, University of California, Berkeley, 1991-92.

Special Advisor on Long-term Planning, Office of the President, University of California, 1993-94.

Director, Center for Advanced Study in the Behavioral Sciences, 1994-2001.

Co-Chair, Commission on General Education in the 21st Century, University of California, 2003-2007.

Professional activities and memberships

Member, Committee on Economic Growth, Social Science Research Council, 1961-65.

Member, Comparative Development Group, Institute of Industrial Relations, University of California, Berkeley, 1961-65.

Advisor Editor, American Journal of Sociology, 1961-62.

Editor, American Sociological Review, 1962-65.

Consulting Editor, Philosophy and Public Affairs, 1972-75.

Consulting Editor, Comparative Studies in Society and History, 1973-81.

Member, Council of the American Sociological Association, 1962-65, 1968-70, 1973-75, 1996-98.

Member, Executive Committee of the American Sociological Association, 1962-65.

Member, Publications Committee of the American Sociological Association, 1962-65.

Member, Pacific Sociological Association.

Member, American Association of Rhodes Scholars.

Member, International Sociological Association.

Chair, Group on Theory and Method of Comparative Studies, Institute of International Studies, University of California, 1966-94.

Member, Board of Directors, Social Science Research Council, 1969-73, 1995-2001. Chair of the Board, 1971-73.

Chair, Sociology Panel of the Behavioral and Social Sciences Survey, sponsored by the Social Science Research Council and the National Academy of Sciences, 1967-69.

Member, Committee on Problems and Policy, Social Science Research Council, 1975-77.

Member, Steering Committee of the Undergraduate Curriculum Development Group of the American Political Science Association, 1975-77.

Co-Chair, Nominations Committee, Social Science Research Council, 1979-80.

Member, Executive Board, Western Center of the American Academy of Arts and Sciences, 1976-77, 1979-84. Co-chair, 1976-77, 1979-84.

Member, Council of the American Academy of Arts and Sciences, 1981-94.

Member, Special Projects Committee, Center for Advanced Study in the Behavioral Sciences, 1979-92.

Co-Chair, Research Committee on Economy and Society, International Sociological Association, 1980-86.

Member, Board of Trustees, Center for Advanced Study in the Behavioral Sciences, 1980-86, 1987-93, Chair 1984-86, 1993-2008.

Member, Committee on Basic Research in the Behavioral and Social Sciences, National Research Council, 1980-88; Chair, 1982-84; Co-chair, 1984-

88.

Member, Committee on Nominations, American Academy of Arts and Sciences, 1981-84.

Member, Board of Trustees, Head Royce School, Oakland, California, 1980-86.

Chair, External Advisory Committee on Sociology, Harvard University, 1981-87.

Member, Visiting Committee for Sociology, Harvard University, 1988-90.

Member, Subcommittee on Humanism, American Board of Internal Medicine, 1981-84, 1989-90.

Chair, Search Committee for the President of the Social Science Research Council, 1985-86.

Member, San Francisco Psychoanalytic Institute, 1971-Present.

Member, Executive Committee, International Sociological Association, 1986-90, 1990-94.

Member, Program Committee, International Sociological Association, 1986-90, 1990-94.

Member, President's Committee to Review the Social Sciences (Behavioral) at Yale University, 1988-93.

Member, Advisory Committee on Sociology, Yale University, 1993-98.

Member, President's Advisory Committee on Undergraduate Education, University of California, 1987-89.

Member, Scientific and Academic Advisory Committee to the President of the University of California on the Energy Laboratories (Livermore and Los Alamos), 1988-92.

Member, President's Advisory Council (National Laboratories), University of California, 1992-98.

Member, Board of Trustees, Russell Sage Foundation, 1990-2000.

Member, Advisory Committee to the Board, American Board of Internal Medicine, 1992-99. Chair, 1995-99.

Member, Kuratorium, German-American Academic Council, 1994-98.

Member, Executive Committee, International Social Science Council, 1994-95.

Member, Committee on Techniques for the Enhancement of Human Performance (National Academy of Sciences), 1994-95.

Member, Committee on International Security and Arms Control (National Academy of Sciences), 1995.

Member, Commission on Social and Behavioral Sciences and Education (National Research Council/National Academy of Sciences), 1995-2001. Chair, 1996-2001.

Member, Committee on Executive Office and Budget, American Sociological Association, 1996-98.

Member, Committee of Selection, John Simon Guggenheim Foundation, 1995-2012. Present. Chair, 1996-2012.

Chair, Visiting Committee on the Institute for Social Research, University of Michigan, 1996.

Member, Task Force on Examination Standards, American Board of Internal Medicine, 1995-96.

Member, Strategic Planning Oversight Committee, American Board of Internal Medicine, 1996-98.

Member, German-American Academic Council, 1999-2001.

Member, Committee on Science and Technology to Counter Terrorism, National Research Council, 2001-2002.

Chair, Panel on Behavioral, Social, and Institutional Issues (Committee on Science and Technology for Countering Terrorism), National Research Council, 2001-2002.

Chair, Panel on Understanding Terrorists to Deter Terrorism. National Research Council, 2001-2002i.

Chair, Division of Behavioral and Social Sciences and Education (National Research Council), 2001-2003.

Member, Committee to Prepare A Physician Charter, a committee representing the American Board of Internal Medicine, the ACP-ASIM Foundation, and the European Federation of Internal Medicine, 2000-01.

Member, Committee on Managing Physicians' Conflict of Interest, American Board of Internal Medicine, 2003-04.

Member, Council of the American Philosophical Society, 2008-14.

Consultative activities:

Series Editor in Sociology for Prentice-Hall Publishers, Englewood Cliffs, N.J., 1961-79.

Consultant, National Advisory Commission on Civil Disorders, 1968.

Member, Task Group on Research and Development in Education, President's Science Advisory Committee, 1968.

Consultant and Senior Advisor, Presidential Advisory Commission on the Causes and Prevention of Violence, 1969.

Member, Technical Advisory Committee, Carnegie Commission on Higher Education, 1968-73.

Member, Advisory Committee, Carnegie Council on Higher Education Project on the Undergraduate Curriculum, 1976-77, 1979-80.

Consultant, California Task Force to Promote Self-Esteem and Personal and Social Responsibility, 1987-89.

Consultant, Nobel Prize Committee on Economics, 1989.

Academic and related awards:

Graduated summa cum laude, Harvard College, 1952.

Awarded a Rhodes Scholarship from Arizona for two years' study at Oxford University, 1952-54.

Gained First Class Honours in the Final Honours School of Philosophy, Politics, and Economics, Oxford University, 1954.

Elected Junior Fellow of the Society of Fellows, Harvard University, 1955

58.

Elected Fellow, American Academy of Arts and Sciences, 1968.

Named University Professor of Sociology in the University of California, 1971.

Elected Vice-President, American Sociological Association, 1973.

Elected Member, Sociological Research Association, 1974.

Elected Member, American Philosophical Society, 1976.

Elected Chair, Theory Section of the American Sociological Association, 1982.

Named Chancellor's Fellow, University of California, Berkeley, 1987.

Visiting Scholar, Russell Sage Foundation, 1989-90.

Elected Vice-President, International Sociological Association, 1990.

Resident, Bellagio Study and Conference Center, Rockefeller Foundation, 1992.

Elected Member, The Tocqueville Society, 1992.

Elected member, National Academy of Sciences, 1993.

Awarded the Berkeley Citation by the University of California, Berkeley, 1994.

Elected President, American Sociological Association, for 1997, 1995.

Georg Simmel Guest Professorship, Humboldt University, Berlin, 1995.

Elected to Society of Fellows, American Academy of Political and Social Sciences, 2000.

Recipient, Mattei Dogan Foundation Prize for Distinguished Career Achievement, International Sociological Association, 2002.

Named Kluge Chair, Library of Congress, 2006.

Named Foreign Member, Russian Academy of Sciences, 2006.

Appointed Clark Kerr Lecturer, University of California, Berkeley for January-February, 2012, 2011.

Appendix B

Bibliography

Authored books:

Economy and Society (with Talcott Parsons). London: Routledge and Kegan Paul, 1956. Translated into Italian and Japanese.

Social Change in the Industrial Revolution: An Application of Theory to the British Cotton Industry. London: Routledge and Kegan Paul, and Chicago: University of Chicago Press, 1959. Translated into Italian. Republished in the Modern Revivals in Economic and Social History, Gregg Revivals, 1994. Republished in the Taylor and Francis Economic History Series, 2006.

Theory of Collective Behavior. London: Routledge and Kegan Paul and New York: The Free Press of Glencoe, 1962. Translated into German, Italian, Japanese, and Spanish. Republished 2011 by Quid Pro Quo Press, Preface by Gary Marx and New Author Preface.

The Sociology of Economic Life. Published as a volume in the Foundations of Modern Sociology Series, edited by Alex Inkeles. Englewood Cliffs, N.J.: Prentice-Hall, 1963. Translated into Spanish, Portuguese, Italian, Dutch, Swedish, Japanese, German, Greek, Korean, Turkish, and Chinese. Second edition: Prentice-Hall, 1975. Second edition. Republished by Quid Pro Press, 2013.

Essays in Sociological Explanation. Englewood Cliffs, N.J.: Prentice-Hall, 1968. Translated into Japanese and Korean. Republished by Quid Pro Quo Press, 2013.

Sociological Theory: A Contemporary View. New York: General Learning Press, 1971. Republished 2011 by Quid Pro Quo Press, Preface by Arlie Russell Hochschild, and New Author's Preface.

Sociological Theory: Historical and Formal (with R. Stephen Warner). Morristown, N.J.: General Learning Press, 1976.

Comparative Methods in the Social Sciences. Englewood Cliffs, N.J.: Prentice-Hall, 1976. Translated into Japanese, 1976. Republished by Quid Pro Quo Press, 2013.

The Changing Academic Market: Institutional Context and a Case Study (with Robin Content). Berkeley: University of California Press, 1980. Republished by Quid Pro Quo Books, 2012.

Sociology. Englewood Cliffs, N.J.: Prentice-Hall, 1981. Second edition, 1984; Third edition, 1987; Fourth edition, 1990; Fifth edition, 1995. Translated into Italian, Russian, and Mongolian.

Social Paralysis and Social Change: British Working-Class Education in the Nineteenth Century. Berkeley: University of California Press, 1991.

Effective Committee Service. Newbury Park, CA: Sage Publications, 1993.

Sociology. Oxford: Blackwell Publishers, 1994 (Volume 1, UNESCO/Blackwell Social Science Series).

Problematics of Sociology: The Georg Simmel Lectures, 1995. Berkeley: University of California Press, 1997.

The Social Edges of Psychoanalysis. Berkeley, University of California Press, 1999.

The Faces of Terrorism: Social and Psychological Dimensions. Princeton: Princeton University Press, 2007.

The Odyssey Experience: Physical, Social, Psychological, and Spiritual Journeys. Berkeley: University of California Press, 2009.

Reflections on the University of California: From the Free Speech Movement to a Global University. Berkeley: University of California Press, 2010.

(with John S. Reed) Usable Social Science. Berkeley, CA: University of California Press, 2012.

Dynamics of the Contemporary University: Growth, Accretion, and Conflict (Clark Kerr Lectures, 2012). Berkeley, CA: University of California Press, 2013.

Getting Sociology Right: A Half-Century of Reflections. Berkeley, CA: University of California Press, 2014.

Edited books:

Sociology: The Progress of a Decade (co-edited with Seymour Martin Lipset). Englewood Cliffs, N.J.: Prentice-Hall, 1961.

Personality and Social Systems (co-edited with William T. Smelser}. New York: John Wiley and Sons, 1963. Second edition, 1970.

Readings on Economic Sociology. Englewood Cliffs, N.J.: Prentice-Hall, 1964.

Social Structure, Mobility, and Economic Development (co-edited with Seymour Martin Lipset). Chicago: Aldine Press, 1966.

Sociology: An Introduction. New York: John Wiley and Sons, 1967. Second edition, 1973.

Sociology: A Survey Report (co-edited with James Davis), published as a report of the Sociology Panel of the Behavioral and Social Sciences Survey, sponsored by the Social Science Research Council and the National Academy of Sciences. Englewood Cliffs, N.J.: Prentice-Hall, 1969.

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The New International Economy (co-edited with Harry M. Makler and Alberto Martinelli). Beverly Hills and London: Sage Publications, 1982.

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Contemporary Classics in the Social and Behavioral Sciences. Philadelphia: ISI Press, 1987.

The Micro-Macro Link (co-edited with Jeffrey Alexander, Bernard Giesen and Richard Munch). Berkeley: University of California Press, 1987.

Lower Division Education in the University of California. A Report of the Task Force on Lower Division Education. University of California, 1987.

The Behavioral and Social Sciences: Achievements and Opportunities (co-edited with R. Duncan Luce, Dean R. Gerstein, and Sonja Sperlich), published as a report of the Committee on Basic Research in the Behavioral and Social Sciences of the National Academy of Science. Washington, D.C.: National Academy Press, 1988.

Handbook of Sociology. Westbury Park, CA: Sage Publications, 1988.

The Social Importance of Self-Esteem (co-edited with Andrew M. Mecca and John Vasconcellos). Berkeley: University of California Press, 1989.

Leading Edges in Social and Behavioral Science (co-edited with R. Duncan Luce and Dean R. Gerstein). New York: Russell Sage Foundation, 1989.

Economy and Society: Overviews in Economic Sociology (co-edited with Alberto Martinelli). London: Sage Publications, 1990. Translated into Italian.

Social Change and Modernity (co-edited with Hans Haferkamp). Berkeley: University of California Press, 1992.

Theory of Culture (co-edited with Richard Munch). Berkeley and Los Angeles: University of California Press, 1992.

The Handbook of Economic Sociology (co-edited with Richard Swedberg). Princeton, NJ: Princeton University Press and New York: Russell Sage Foundation, 1994.

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Terrorism: Perspectives from the Behavioral and Social Sciences (co-edited with Faith Mitchell). Washington, DC: National Academy Press, 2002).

The Handbook of Economic Sociology, second edition (co-edited with Richard Swedberg). Princeton, NJ: Princeton University press and New York: Russell Sage Foundation, 2005.

General Education in the 21st Century (co-edited with Michael Schudson). A Report of the University of California Commission on General Education in the 21st Century. Berkeley: Center for Studies in Higher Education. 2007.

ARTICLES, ESSAYS, ETC.

"A Sociological Model of Economic Development" (with Talcott Parsons), Explorations in Entrepreneurial History, May, 1956, pp. 181-204.

"A Comparative View of Exchange Systems," Economic Development and Cultural Change, Vol. VII, No. 2 (January, 1959), pp. 273-82.

"Change and Controversy in Recent American Sociology" (with Seymour Martin Lipset), British Journal of Sociology, Vol. XII, No. 1 (March, 1961), pp. 41-51. Reprinted in Social Realities: Dynamic Perspectives (edited by George Ritzer), Boston: Allyn and Bacon, 1974), pp. 409-19. Reprinted in Contemporary Sociology: An Introductory Textbook of Readings (edited by Milton L. Barron). New York: Dodd, Mead and Co., 1964, pp. 49-66.

"Mechanism of Change and Adjustment to Change in Economic Development" in Industrialization and Society (edited by Wilbert E. Moore and Bert F. Hoselitz). The Hague: Mouton, 1963. Reprinted in Political Development and Social Change (edited by Jason L. Finkle and Richard W. Gable. New York: John Wiley and Sons, 1966, pp. 28-43. Reprinted in Economic Development and Social Change (edited by George Dalton). Garden City, N.Y.: The Natural History Press, 1971, pp. 352-74. Translated and reprinted in Il Nuovo Imprenditore (edited by Angelo Pagani). Milan: Franco Angeli Editore, 1967, pp. 385-414. Reprinted with revisions as "Toward a Theory of Modernization" in Social Change: Sources, Patterns

and Consequences (edited by Amitai Etzioni and Eva Etzioni). New York: Basic Books, 1964, pp. 258-74. Reprinted in Tribal and Peasant Economies: Readings in Economic Anthropology (edited by George Dalton). Garden City, NY: Natural History Press, 1967), pp. 29-50.

"Theoretical Issues of Scope and Problems of Collective Behavior," Sociological Quarterly (April, 1964), pp. 116-22. Reprinted in Readings in Collective Behavior (edited by Robert R. Evans). Chicago: Rand McNally, 1969, pp. 89-94. Reprinted in The Sociology of Dissent (edited by R. Serge Denisoff), New York: Harcourt Brace Jovanovich, 1974, pp. 36-40. Reprinted in Collective Behavior: A Source Book (edited by M. D. Pugh). St. Paul: West Publishing Company, 1980, pp. 7-11.

"Notes on Functionalism and Scientific Analysis," Transactions of the Fifth World Congress of Sociology. Louvain: International Sociological Association, 1964. Vol. IV, pp. 59-70.

"Introduction," to Values and Interests in Social Change, by Sister Marie Augusta Neal. Englewood Cliffs, N.J.: Prentice-Hall, 1965, pp. vii-ix.

"The Social Challenge to Parental Authority," in Man and Civilization: The Family's Search for Survival (edited by Seymour Martin Lipset, Piero Mustacchi and Roger H. L. Wilson). New York: McGraw Hill, 1965, pp. 67-73.

"The Modernization of Social Relations," in Modernization (edited by Myron Weiner). New York: Basic Books, 1966, pp. 110-121. Reprinted in Modernization: The Dynamics of Growth (edited by Myron Weiner). Voice of America Forum Lectures, 1966, pp. 119-32.

"The Sociology of Economic Life," in American Sociology (edited by Talcott Parsons). New York: Basic Books, 1967, pp. 143-55.

"Sociology and the Other Social Sciences," in The Uses of Sociology (edited by Paul F. Lazarsfeld, William H. Sewell and Harold L. Wilensky). New York: Basic Books, 1967, pp. 3-44.

"Sociological History: The Industrial Revolution and the Working-Class Family," The Journal of Labor History. Vol. I, No. 1 (1967), pp. 18-35. Reprinted in Essays in Social History (edited by M. W. Flinn and T. C. Smout). Oxford: Clarendon Press, 1974, pp. 23-38.

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INDEX

- academic cultures, lectures on, 175-6
- academic culture of family, 14-15
- academic motivation in adolescence, 29-30
- Academic Planning and Program Review Board, 151
- Academic Senate, service on policy committee of, 108-9; service on Committee on Educational Policy, 148-9; serving as Chair of Berkeley Division, 163; serving as chair of systemwide, 165
- Adams, Robert McCormack, 169
- Adams House (Harvard), 46
- administrative opportunities, 103-4
- adolescent adaptations, 29-31; academic, 30; peers, 30
- AGIL scheme (Parsons), 55
- Alexander, Jeffrey, 207; organizer of festshrift, 222
- Allport, Gordon, 39
- ambivalence as research theme, 197ff.; in affirmative action, 198-9; in universities, 198; theme of Presidential Address, ASA, 199
- American Board of Internal Medicine, 167; advice on Board certification standards, 220; serving on Subcommittee on Humanism, 167-8; advisor to Board, 1990s, 219-20
- American cultures, committee to consider, 158; demands for course, 158; resignation from committee, 159
- American identity, 10-11
- American Psychoanalytic Association, 92
- American Sociological Association, activities as President, 204; elected President 1995, 203; failure in earlier elections, 203; membership on council, 95-6; politics of presidential election, 203-4
- American Sociological Review, 88-89; editing, 90-1; innovations in editing, 90-1; invitation to edit, 89; value of editing, 93-94
- American Studies requirement, Berkeley, 148-9
- Annual Review of Sociology, essay on, 221

Anti-Vietnam War protest, 102

Arizona Republic, 33; proofreader at, 34; reporter at, 33-34

Arizona Times, 33

Ashton, T. S. 49

Assistant to the Chancellor for Student Political Activity, 101-3; as applied social science, 104-5; discomfort and comfort in role, 104; writing up the experience, 249-50

Associate Chancellor for Educational Development, 106-7

Association of American Colleges, 154

Balderston, Frederick, 79

Bales, Robert F, 54

Baltes, Paul, 214, 224ff.; as co-editor of International Encyclopedia, 226-7

Bancroft Library (University of California), 254

BASS report, 117-18

Bayless supermarket in Phoenix, “cigarette boy” at, 25-6; employment at, 24-5; personal transformation at, 26; vegetable clerk at, 16; working hours at, 26-7

Bell, Norman, 55

Bellah, Robert, 17, 113

Bendix, Reinhard, 71, 76, 113

bicycling in Europe, 42

birth 7, 13

birth-myth, 7, 8-9

Black Thursday, 8

Blau, Peter, 117

Blauner, Robert, 112

Blumer, Herbert, 71, 72, 76, 132; exchanges on collective behavior with, 81

Board of Educational Development, 106-8

Boston Psychoanalytic Institute, 97

Bowker, Albrt, 139

Boyer, Ernest, 157

Brandeis University, 101

Brinton, Crane, 41, 64

British education, 142 ff; change of research plans, 143-41 false start in research on, 142;
library work in UK, 146-47

British Museum Library, 67-8

Burawoy, Michael, 78

Bush, Geoffrey, 64

California, good life in, 197-8

California Task Force to Promote Self-Esteem, 159

Cambridge University, 147

Campbell, Glen, 164

Carnegie Corporation of New York, 244

Carver High School in Phoenix, 36

Center for Advanced Study in the Behavioral Sciences (Stanford), 142, 145;
administrative role as director, 208; administrative style at, 210; first year at, 211;
fund-raising for, 209; innovations at, 213; institutional character, 206-7; interview
for directorship, 1988, 182; interview for directorship, 1994, 205; offer of
directorship, 1994, 193, 196; service on Board of Trustees, 181-2

Center for Studies in Higher Education, 182-3; serving as acting director of,
1987-89, 182-83, as site of commission on general education, 245

Chafee, Zachariah, 41

chair, sociology department, 1974-96, 132 ff; efforts to strengthen the sociology major,
133-4; leadership in strengthening methods requirement, 134; making new
appointments, 136-8; second term 1991-92, 186-7

Chronicle of Higher Education, 196

City College of New York, 101

Clapham, John, 49

Clausen, John, 49, 113, 132

Cleaver, Eldridge, 101

CNN syndrome, 234

Coleman, James, 117

Columbia University, as alternative to Harvard, 58; offer from, 76-7; law school, 196

collective behavior, 80-85; background to research, 80-1; reception of, 130; research summarized, 82-3

collective trauma, 207-8; 9/11 as;

comparative methods, broad approach to, 127-9; collaboration plans with Lipset, 127; preoccupation with, 125-26; reception of, 130; research strategy, 129; review by comparative group, 130

Commager, Henry Steele, 42

Committee on Academic Planning, 151

Commission of Social Sciences Associations (COSSA), 168

Commission on Behavioral and Social Sciences and Education (National Research Council), 168-69

Commission on International Security and Arms Control, 214

Committee on Basic Research in the Behavioral and Social Sciences, 169; co-editing the committee's first report, 196; co-editing the committee's second report, 170; co-editing the committee's third report, 170-71

Committee on Behavioral and Social Sciences and Education (CBASSE), 214-5

Committee on Science and Technology for Countering Terrorism, 234-5;

Constance, Lincoln, 77

Converse, Philip, 205

Content, Robin, 138

Copeland, Aaron, 64

Coyote Journal, sports editor of, 33-34; editor-in-chief of, 34

Daily Californian, 103

DARPA (Defense Advanced Research Projects Agency), 238-9

Davis, James, 116

Davis, Kingsley, 71, 72, 76i, 132; teaching with, 78, 113

"delinquency" in childhood, 23-4

Demos, Raphael, 39

Dick and Jane reader, 22

“diplomat” at age 13, 26

Division on Behavioral and Social Sciences and Education (DBASSE), 215

divorce, 197

doctoral dissertation (Harvard), 60-63, as risky undertaking, 64; choosing the cotton industry, 62; organizing the research and writing, 65-7; prospectus examination, 60; theoretical background of, 60-1; writing phase, 67-8

Dogan Prize for Distinguished Career Achievement, International Sociological Association, 179; awarded, 2002, 179

Duesenberry, James, 42, 63, 68

Duncan, Otis Dudley, 116

Duster, Troy, 113

Eberhart, Wolfram, 113, 132

Economy and Society, 54-61; collaboration in writing, 55-6; listing as junior author to Parsons, 57; reception by economists and sociologists, 57-8; theoretical background, 54-5.

Education Abroad Program, University of California, 143; establishing program at Leningrad State University, 176-7; students’ difficulties with, 145; Thanksgiving Dinner and spoof report on, 146; work with students, 144-45

Effective Committee Service, 191

Eliot, T. S., 65

Emerson School (Phoenix), 18, 19, 22, 24, 29

Epstein, Cynthis, 203

Erikson, Erik, 140-42; essay on, as social scientist, 220

Erikson, Kai, 140

Eyerman, Ron, 107

Faculty Association, Berkeley campus, 109-10

Fairbanks, John, 41-2

family “farm” in Phoenix, 20

- family isolation, 13
- family solidarity, 15-6
- Farber, Missouri, 10
- FBI, 103-4
- Feller, David, 109
- Festschrift, 222-3
- Filthy Speech Movement, 102
- first memory, 9
- Fiske, Susan, 247
- Ford Foundation as funder of Center, 211
- Fort Ord, California, 29
- Fraser, William, 160
- Free Speech Movement, 101, 155

- “gang” in childhood, 20-1
- Gans, Herbert, 199, 203
- Gardner, David, 155, 160
- Gardner, Libby, 175
- Garfield School (Phoenix), 11, 18,19, 22-3
- Geertz, Clifford, 127
- general education, commission on, 244ff; gaining support for commission, 244-45;
 - reception of commission report, 245
- German ancestry, 9-11
- German Evangelical Church, 10, 11
- George Webb Medley Prize (Oxford), 49
- Georg Simmel lectures, 200-01
- Gerstein, Dean, 170, 172-3
- Getting Sociology Right, 255
- Giesen, Bernard, 207
- Giner, Salvatore, 178
- Glazer, Nathan, 114

- Goffman, Erving, 71, 79, 114
- Glock, Charles, 71, 73, 132
- Goldwater Award, 36
- golf interests, 27
- Goodman, Stanley, 99
- Grace Lutheran Church, 11
- Graduate School of Education, University of California, academic troubles, 151;
 reactions to committee report, 152-3; review and recommendation for
 discontinuation, 152.
- Gray, Hanna Holborn, 251
- Grand Canyon, 16, 252-3
- Guerin, Daniel, 68
- Guggenheim Foundation, 125, 216-7; member of Committee on Selection, 217;
 sociology reader for, 216-7; mission and suggested changes, 217-8;
 retirement from, 218
- Haas, Ernst, 176
- Habermas, Juergen, 228
- Handbook of Economic Sociology, 242
- handbook of sociology, editor of, 183-84
- Hallinan, Maureen, 203
- Harley, Diane, 245
- Harvard Crimson, 35
- Harvard University, 70; academic advice about, 37; academic choices as undergraduate;
 ambivalence about, 37-38; ambivalence toward graduate work, 52; applying to,
 3-7; brother Bill at, 29; chairing committee on sociology department, 173-5;
 difficulties as freshman, 38-40; dissertation prospectus examination; fieldwork
 requirement examination graduate studies in Social Relations, 53; language
 examination, 59; notification of admission, 36-37; offer in 1957, 72;;\ offer, in
 1970, 113; orals examination, 58; residence difficulties at, 37-38
- Hayden, Tom, 157

Herskovitz, Melville, 96

Hewlett Foundation as funder of Center, 211, as supporter of commission on general education, 241

Heyman, Mike, 152, 158, 164, 195

Heyns, Roger, 105

Hobsbawm, Eric, 67

Hochschild, Arlie, 203; promotion case, 134-6

Homans, George C., 40; dissertation committee member 63, 68

Honig, Bill, 157

Hoselitz, Bert, 95

Human Sciences, Research, Inc., 124.

Humboldt Journal of Social Relations, 197

hunting interests, 18-9

Huttenback, Robert, 166; testifying on his academic tenure, 167

Indian country, 16

Institute of International Studies (Berkeley), appointed to, 114; service as Associate Director, 176

intercollegiate athletics, 188 ff.; chair of blue-ribbon commission on, 188; politics of, 190; recommendations of, 189

International Encyclopedia of the Social and Behavioral Sciences, 223ff; architecture of, 227-28; biographies as issue, 229; consultation with Elsevier, 224; decision to publish, 224; doubts about editing, 225; entry on collective behavior, second edition, 232; fight with Elsevier over royalties, 231-2; invitation to edit, 224; meeting of section editors, 228-29; objections to editing from Center's Board of Trustees, 225-6; representativeness of authors, 229-30; reviewing the contributions, 230-1; review for the 1968 edition, 224

International Sociological Association, 176; participation in, 178; service on Executive Committee, 178; service as vice-president, 1788-9 unsuccessful nomination for Presidency, 178-9

jigsaw puzzles, 9

Jervis, Robert, 247

Journal of Marriage and the Family, 92

journalistic interests, as possible career, 34-5; in high school, 33-4

Kahoka, Missouri, 7, 10, 16

Kansas City, 8

Kennedy, Donald, 251

Kerr, Clark, 71, 121, 196; advice on memoirs, 221; writing “Forward” to memoirs, 221-2

Kerr Lectures, 251ff; themes of, 253

Keyser, Christine, 55

Kirkpatrick, Jeanne, 163

Kirksville State Teachers College, 12

kindergarten, 22

Kluge Fellow, 242

Kluckhohn, Clyde, 40, 64

Kluckhohn, Florence, 42

Krevans, Julius, 167

Kroeber, Alfred Lewis, 70

Kruger, Alan, 247

Kuznets, Simon, 95

Lago di Garda, 129

Lazarus, Richard, 79

Leningrad State University, 176-7

Levin, Harry, 64

Library of Congress, 201

Lindzey, Gardner, 43, 169, 171, 206, 226

Lipset, Seymour Martin, 71, 72, 79; collaborating with, 86, 96, 113

London School of Economics, 147

Long Beach State University, 165

Lowenthal, Leo, 71, 72, 79, 112, 132

Lower division education, academic problems with, 155; task force to review, 154;
recommendations in report, 157; reception of report, 156-7; reforms based on,
158; writing the report, 156-7

loyalty oath crisis, 76

Luce, R. Duncan, 170

marble games, 15

Marx, Gary, organizer of festschrift, 222

Marx, Karl, 67; edited book on, 123-4 teaching on, 110

Massey, Walter, 192-93

Master Plan for Higher Education, 192, 193

Matza, David, 90, 112

Mathias, Peter, 67

McIntosh, Jess, 254

Mead, Margaret, 42

Mecca, Andrew, 159, 162

Mellon Foundation as funder of Center, 211

Merton, Robert K, 53, 69, 76, 173, 224

Meyerson, Martin, 101, 105

middle child, life as, 19

Miles, Josephine, 114

miscarriage of “sister”, 8-9

Missouri club, 12

Moffatt, Bill, 40

money as motivation, 193-4

Moore, Barrington, Jr., 41, 54

Moore, Calvin, 155, 160

Moore, Wilbert E, 95

“Mosteller, Frederick, 41

“Mr. Report”, 151. 163

“Mr. Social Science”, 218

Murray, Henry A, 39 434, 64, 97

Musson, A. E., 67

National Academy of Sciences, 168; doubts about my eligibility, 202, elected to, 203

National Endowment of the Humanities, 154

National Institute of Education, 154

National Laboratories, University of California, 179-80; continuation of service on President’s Advisory Council, 1990s; criticism of University of California’s link to the labs, 1979; service on Scientific and Academic Advisory Committee, 1989-92; security clearance again, 219; security clearance troubles, 180; service on President’s Advisory Council, 1992-98, 179

National Research Council, 208; as an impartial agency, 216; work on terrorism, 208

National Science Foundation as funder of Center, 211-12

near-death, 7-8

New York Times, 174, 196

Nonet, Philippe, 113

North Phoenix High School, 31

odyssey experience, autobiographical examples, 241; education abroad as, 145-6; finishing the book, 241ff.

Ogburn, William Fielding, 58

Ooman, T. K., 178

OPEC crisis, 131

Oral history, 254-5; like a psychoanalysis, 54

Oxford University, 45; lecturing at 147; schools examination at, 50-1; viva at, 51-2.

Parsons, Talcott, 42, 68-9, 72, 81, 97; 203, 224; as Marshall Lecturer (Cambridge), 49-

50; difficulties in collaboration, 121-2; collaboration at Harvard, 54-57; failure of membership nomination in National Academy of Sciences 202; my critique of Marshall lectures, 50; orals examiner, 58, 89-90.

Peacock, James, 247

Pearl Harbor, 11

Peltason, Jack, 192-93, 206

People's Park, 101

Personality and Social Systems, 88

Petersen, William, 113

Phi Beta Kappa, election to, 42; appointed lecturer, 190

Philosophy, Politics and Economics (Oxford), 48

Philosophy, Psychology, and Physiology (Oxford), 48

Phoenix, Arizona, 7, 13

Phoenix Union High School, 31; extracurricular activities, 32; performance at, 52;
student Rotarian at, 32-3

piano lessons, 12, 29

Pitts, Jesse, 55

Platt, Gerald, 121-3

Poston, Michael, 67

Post-tenure review, 187-88

Prentice-Hall publishers, series editor of, 94-4

Prewitt, Kenneth, 171

psychoanalysis, double-life in , 99-100; possible practice of, 98-9; writings in
1960s, 120-1; writings on, 100-1

public schools in Phoenix, 21-2

public speaking in high school, 31-2

Pyle, Howard, 33

Quine Willard van Orman, 51, 64

Reagan, Ronald, 108, 109, 164, 168

reconstitution movement, 108-9
 Reed College, 101
 Reed, John Shepard, 245-46; co-authorship with, 248-9; relationship with, 246-7;
 collaborating with on applied social science, 247-8
 Reischauer, Edwin, 422
 religious background, 10-11
 Research Conference on Racial Trends in the United States, 215-26; introducing
 the results of the conference, 216
 retirement, 233-4; dramatic activities in, 235; plan for, 234; teaching in, 234, 242-3
Revolution in Industry and Family (doctoral dissertation), 68
 Rhodes Scholarship, early dreams of, 30; interviews for, 45-7
 Rockefeller Center, Bellagio, 186; five-week visit, 1992, 191
 Rosberg, Carl, 176
 Rosovsky, Henry, 64; 173
 Rossi, Peter, 169
 Rostow, Walt Whitman, 62, 63, 68
 rowing at Oxford, 48=50
 Rubens, Lisa, 254
 Russell Sage Foundation, visitor, 1989-90, 185; work schedule at, 185; service on
 Board of Trustees, 214
 Rustique Olivette, 68

 Sabbatical leave Europe, 126, 129
 Salzburg Seminar in American Studies, 42-3, 145; summer experience at, 43
 San Francisco Psychoanalytic Institute, 98, 140; skepticism about orthodoxy, 98-9
 Schelling, Thomas, 235
 Schudson, Michael, 243
 Schneider, David, 78, 79
 Scott, Richard, 247
 Scott, Robert, 209
 Seaborg, Glen, 115

Selective Service interview, 47

self-esteem movement, 159-60; my committee on the subject, 158-9; report on, 161;
 request to the University of California to help, 159

Selznick, Philip, 71, 78, 113

September 11, 2001, 234; as cultural trauma, 108

Sewell, William, 117, 173

Shapiro, Harold, 251

Sherriffs, Alex, 103

Sills, David 224

Smelser, Bill (brother), 8 11, 13, 18, 39; enlistment in US Army, 19; collaborating
 With, 87-88

Smelser, Joseph (father), 9, 13-4, 17; political activities, 181

Smelser, Philip (brother), 18-9

Smelser, Susie (mother), 10, 14

Social Analysis 139X

social life, grammar school, 29-30; high school, 35

Social Paralysis and Social Change, 147; themes, 185

Social Science Research Council, early involvement in, 95-6; member of council, 118;
 member of Council, 1990s, 214

Social Relations Department (Harvard), 39

Society of Fellows (Harvard), as competitive setting, 65; as “precious institution” 65; as
 setting for dissertation work, 63-4; requirements, 64-5 selection as Junior Fellow,
 52.

sociology, essays on, 199 ff.

Sociological theory course at Berkeley, 73-5; teaching in 1960s-1970s, 109-10.

Sociology of scarcity, work on, 131-2

sociology text, 149-50

Solow, Robert, 205

Sorokin, Pitirim, 41

Soviet Union, visits to, 176-77; lectures, 1989. 177-8

Spence Foundation as funder of Center, 211

sports in family, 17
 Sproul, Robert Gordon, 47
 St. Michael's School, London, 144, 147
 Stanford University, as setting for Center for Advanced Study, 206; competition
 with Berkeley, 206
 Stinchcombe, Arthur, 78, 111-2
 Stigler, Stephen, 247
 Stockman, David 168
 Stouffer, Samuel, 41, 58; as orals examiner, 59
 Strong, Edward, 101
 Sunday school, 11, 13
 Sutton, Francis X, 42
 Swanson, Guy E., 71, 113
 Swedberg, Richard, 243
 Sztompka, Piotr, 207

 Teggart, Frederick, 70
 Teller, Edward, 115
 term-paper selling, 166; testifying in court on subject, 166
 terrorism, 235ff; book on, political struggle in academy on nature of terrorism, 237-8;
 239-40; role on National Academy committees, 236-7; themes in book on, 240;
 subcommittees on, 238
The American University, 121
The Economist, 78
The Social System, 55
The Sociology of Economic Life, 85-6, 90; in Soviet Union, 177
The Uses of Sociology, 55
Themes of Work and Love in Adulthood, 142
Theory of Collective Behavior, 83. 85. 90, 166
 Third World Liberation Front, 110=22
 Treiman, Donald, 169

Tien, Chang-lin, 188, 196

Trow, Martin, 31

Trudeau, Gary, 162

typing classes, 31

Tyriakian, Edward, 55

Ullman, Lloyd, 109

undergraduate thesis at Harvard, 43-40; criticisms of 44-5

University of California, office of the president, 192ff; duties as advisor, 193; service as advisor to President Peltason, 192-3; writing essay on governance, 193-94; writing essay on surprises at Berkeley, 250-1

United World Federalism, 40-41; interest in, 41; disaffection with, 41

University of California, Berkeley, 6; citizenship in, 94-5; conflict in department of sociology, 112-3; golden age of sociology, 76; promotions at, 77; research on, 231 visit to 70-1.

University of California Press, review of, 153-54

University of California, San Francisco, joint medical program with, 139-3

University of Chicago, 70, 74

University of Chicago Press, 69, 77

University of Michigan, 70, 77

University of Oregon, 29

University of Reading, 147

University of West Virginia, teaching at, 242-3

University of Wisconsin, 70, 77

University Professorship, 113-5

Unruh, Jesse, 108

Vasconcellos, John, 159, 162

VERIP retirement scheme, 192-3; conflict over, 196

Vest, Charles, 251

violin lessons, 29; relations to Bill, 29

Vogel, Ezra, 55

Volkswagen Foundation as funder of Center, 211

Wacquant, Loic, 186-87

Wallerstein, Robert , 121, 140

Washburn, Sheridan 114

White, Robert W., 39, 98

Whittier School, 19

Wilder, Thornton, 64

Wilensky, Harold L, 71, 113

Williams, Christine, 199; organizer of festschrift , 222

Wilson, William Julius, 215-26

Withey, Lynn, 241

Wittrock, 207

Woodrow Wilson School, Princeton, 195

work ethic in childhood, 12

World War I, 13

World War II, 11

Yale University, 70, 172;; advising on sociology department, 174-75; offer from, 113

youth years, 27-8

Zelditch, Morris, Jr., 173