Blue-Ribbon Papers: Behind the Professional Mask: The Autobiographies of Leading Symbolic Interactionists

Norman K. Denzin

Editor
BLUE-RIBBON PAPERS: BEHIND THE PROFESSIONAL MASK: THE AUTOBIOGRAPHIES OF LEADING SYMBOLIC INTERACTIONISTS
STUDIES IN SYMBOLIC INTERACTION

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THE SELF-REVELATIONS OF 20TH AND 21ST CENTURY INTERACTIONISTS: BREAKING THE “ACADEMIC MOLD”

Lonnie Athens

ABSTRACT

Sociologists are notorious for writing antiseptic and self-serving autobiographies that hide more than they reveal about their authors. As the editor of a volume providing the autobiographies of 10 well-known and still living interactionists, I discuss the reasons for their writing of bland, formulaic autobiographies that allow them to hide behind their professional masks and the difficulties that were encountered in trying to find contributors who would break the academic mold by offering revelations about themselves. Personal revelations are filched by me from my contributors’ autobiographies for purposes of illustration. The chief reason given for sociologists writing autobiographies free of personal revelations is their abiding by the convention that “decorum trumps truth” in academic discourse and the fear that its violation would be injurious to their careers. I conclude that the convention “decorum trumps truth” should be changed to “truth trumps decorum” because the former represents a serious impediment to any discipline that has as its goal the advancement of truth.
When Norman Denzin asked me if I would be interested in serving as a guest editor for a special issue in his series, *Studies in Symbolic Interaction*, in which the life stories of noted contemporary symbolic interactionists would be published, I jumped at the chance. I thought it was important to reveal unknown facets of the people who have done notable work from this sociological approach over the last part of the 20th century and the first part of the 21st century.

In the sociological classic, *Social Process*, Cooley (1918, pp. 401–402), one of the originators of symbolic interactionism, opines that for a sociologist “all his work is, in a certain sense, autobiographical” because “it is all based on perceptions which he has won by actual living (See also Mead, 1936, p. 413).” In *The Sociological Imagination*, Mills (1959, p. 195), a famous radical from the 1950s and 1960s and a long-time professor at Columbia University, seconds Cooley on this vital point, advising students embarking on a sociological career that “the most admirable thinkers within the scholarly community you have chosen to join do not split their work from their lives. They seem to take both too seriously to allow such dissociation, and they want to use each for the enrichment of the other.”

I also thought on a more personal level that it would be fun to edit a volume that includes the life stories of people I have known, or, have been acquainted with, for years. While trying to collect my thoughts about the task that lay before me, I recalled Charles Tilly's review of Berger's (1990) *Authors of Their Own Lives: Intellectual Autobiographies by Twenty American Sociologists*, which appeared in the *Sociological Forum*. Berger’s book had sparked a great deal of curiosity among sociologists. It seemed that everybody was rushing out to get a copy to discover what juicy tidbits they could learn about the sociologists whose work they had long admired – or for years held in contempt. More importantly, I also recalled how Tilly (1993, pp. 502–503) artfully described what a “hypothetical graduate student” who was born in the 1960s would likely conclude about pursuing a career as an academic sociologist after reading Berger’s book.

Jezz, what am I getting into? ... Bennett Berger has gotten twenty eminent sociologists, including himself to write brief autobiographies that sound mostly like Sunday School testimonies of the born-again. These people, to hear them tell it, don’t know hate, envy, despair, rage, arrogance, disorder, or lust; maybe they once did, but now have become modest, reflective, reasonable, circumspect, hard-working, organized, responsible. Wow? Whether they mean it or not, these paragons are intimidating. Do I really want to spend my life with saints, self-deceivers, or liars? Uncle Louie said he might be able to get me a job in his insurance business. Maybe I better give him a call.
By the end of Tilly’s review, aptly titled *Blanding In*, however, he (1993, p. 504) had not only softened his critique of Berger’s contributors for providing only formulaic, antiseptic, and ultimately self-serving intellectual autobiographies that academics are notorious for writing, but also made this candid admission:

And if Bennett Berger had asked me to contribute a chapter? I too would have written prudent pieties, subtly crafted to make readers think me a diffident genius. I would have complained, boasted, or both about my marginality. I would have offered self-serving tributes to three sociologists … plus one political scientist.

In Tilly’s final sentence to his review, he (1993, p. 504) delivered his humorous punch line: “My undistinguished service in the United States Navy imprinted in me a maximum much honored in that organization, one the *Authors of Their Own Lives* have learned mostly without benefit of military service: *Don’t stick out your neck, cover your ass* (italic not in original).”

Some five decades earlier, in his essay, “The Social Role of the Intellectual,” Mills (1944, pp. 296–297) had pointedly observed that academics suffered from the fear their careers might be seriously tarnished or, worse yet, ruined if it became widely known among their colleagues they had engaged in conduct unbecoming a professor.

Although, in general, larger universities are still the freest of places in which to work, the trends which limit the independence of scholars are not absent there …. Yet the deepest problem of freedom for teachers is not the occasional ousting of a professor, but a vague general fear — sometimes politely known as “discretion,” “good taste,” or “balanced judgment.” It is a fear which leads to self-intimidation and finally becomes so habitual that the scholar is unaware of it. The real restraints are not so much external prohibitions as control of the insurgent by the agreements of academic gentlemen.

While Mills does not spell it out, one of the most important gentlemen (and now gentlewomen) agreements restraining academic insurgents is that *decorum always trumps truth*. Make no mistake about it, neither universities nor academic professional societies will tolerate for long people telling it like it is about anyone, including themselves. One of the ironic consequences of this is it prevents academic insurgents from heeding Foucault’s (1980, pp. 81–88, 133) call to “speak truth to power.” Even within the protective confines of academia, which is supposedly dedicated to uncovering the truth, they fear having the label “indiscrete,” “vulgar,” or, worse yet, “unbalanced” tattooed across their foreheads (see, e.g., *Isaacson*, 2007, pp. 58–63, 67–72). As far as an academic career is concerned, this can be
the proverbial kiss of death. The only people who can afford to speak perfectly candidly about others and themselves are those from powerless groups. Since members of the powerless groups are stuck at the bottom of the pecking order, they are as Goffman (1967, p. 212) says “well organized for disorganization” that such candidness on their part can cause in their web of social relations. Thus, the more steps that we take up the academic ladder, the more circumspect and timid we become, and thereby, the less rather than the more able or willing we are to call a spade a spade.

Despite all this, I refused to be deterred. I stubbornly set out to break the mold used in past anthologies of academic autobiographers by prodding my contributors to reveal intimate information to add a dash of spice and a dose of reality to their autobiographies. My goal was to get my autobiographers to give their readers a glimpse of who they were behind their professional masks. I also believed that I had a reasonably good chance of achieving this goal because, unlike in Berger’s case, all my contributors were not only sociologists (or a philosopher in one case), but also leading contemporary experts of the social science approach, popularly known as “symbolic interactionism.” In The Roots of Social Knowledge, Cooley (1926) forcibly argued that a sociologist is not interested in “material facts” for his or her sake alone, but for the sake of the people whose daily lives these facts impact. Since symbolic interactionism places an enormous premium on studying peoples’ most intimate thoughts, most heartfelt emotions, and most closely guarded secrets, I figured it would be easy for me to get symbolic interactionists to disclose some of their own such thoughts, emotions, and secrets in their autobiographies.

Unfortunately, shortly after starting my work on this project, I was surprised, and, sometimes shocked, to discover that my raised expectations for symbolic interactionists were not completely warranted. I was surprised by the number of noted symbolic interactionists who declined the invitation to write their autobiographies, and I was even more surprised by the number who originally accepted the invitation but later decided that they should not write one. Finally, I was shocked by the number of interactionists who wrote autobiographies in which they merely chronicled the information on their resumes, such as the various academic appointments they had held; the grants and fellowships they had received; the books and articles they had published; the academic awards they had won from universities or professional associations; and the various elected offices they had held in each. Part of the shock I experienced was due to my forewarning the invitees not to send me narratives that simply mimicked their resumes.
When I pressed some of the invited contributors to reveal more intimate details and less information from their resumes, I often felt like a dentist trying to extract a patient’s impacted wisdom teeth without the aid of a local anesthesia. Some of those invited to make submissions became so indignant and angry at my request that they outright refused to revise their statements preferring to have their autobiographies excluded rather than provide any real details about themselves. In hindsight, I realized I should have expected this response even from symbolic interactionists. After all who should know better than them, the potential harm that making such revelations could later do to their professional reputations.

Undoubtedly, this raises the question as to what I considered a personal revelation. Before addressing this, I should mention I never expected, much less demanded, that anyone reveal anything on the order of (1) someone else took their graduate record exam or wrote their doctoral dissertation; (2) they made millions selling insider information or worthless penny stocks on Wall Street before or after earning their graduate degrees; or (3) they once served as a leader of the Weathermen. For me, a revelation is merely something people disclose about their lives that is not generally known, but may be important to our better understanding of who they are as mortal human beings. It need not be something salacious. To the contrary, a personal revelation may also be something highly commendable that is not widely known. Of course, if autobiographers fail to reveal any negative information, it sends up a red flag that they are not spilling the beans, and their autobiographies can be dismissed as Pollyannaish or self-promotional pieces. Thus, like everything else in life, writing a credible autobiography exacts a price, which I now understand not everyone is willing to pay.

Perhaps the best way to convey my sense of a personal revelation is not by means of denotation, but connotation. To achieve this goal, and whet the readers’ appetite, I will illustrate what I consider a personal revelation with examples taken from some of the 10 brave, hearty, and erudite souls who accommodated the demands for information about the person behind the professional mask. A female contributor, who came of age in the Pacific Northwest, revealed how she had been duped early in her career during a job interview at a university in Northern California. An assistant professor in the department warned her that his colleagues never granted assistant professors tenure, and if she were offered the position she better think twice before accepting it. Based on his advice, she declined the job offer, although it was located in a highly desirable professional area. Later, she discovered that the professor in question was granted tenure and had married
the woman who took the job and with whom he apparently had a prior relationship.

A male autobiographer, who also came of age in the Pacific Northwest, revealed that his department had voted against granting him tenure. Apparently, he had offended many of his department’s tenured members by purchasing a home before the vote had taken place that they perceived as a presumptuous on his part. Without the intervention of an outraged, tenured departmental member and long-time friend, his career, which included receiving a distinguished professorship at that same university, without doubt would have been derailed.

A female autobiographer who grew up in the “Windy City” revealed what happened to her when she applied for promotion to full professor at a Big Ten university. While her application was under consideration, she suffered severe head injuries in a car accident. Fearing her brain had been seriously damaged and not wanting to be stuck with a mentally challenged full professor, her department withdrew her application without consulting her. After a lengthy convalescence, she resumed teaching and publishing notable scholarly works. Despite this, no steps were taken to promote her until a widely known public figure at the university became involved. Shortly thereafter, the university’s administration granted her promotion and she subsequently did some of the most creative work of her long, highly distinguished career.

Another female contributor from one of many small towns that dot the sides of the mountain ranges in the South admitted she quickly became a “flower child” after enrolling in college during the 1960s. While attending graduate school at a major university in the Northeast, she became romantically involved with a professor in her department, who was an “up-and-coming” interactionist. They not only married after she obtained her doctorate, but also remained devoted to one another until his death a few years later. Despite or perhaps because of this tragic experience early in her professional career, she has gone on to become a leader of the autoethnography movement and one of a handful of brilliant social scientists trying to erase the hard-and-fast line drawn between the humanities and the social sciences.

Finally, a male autobiographer from a small town in the Midwestern plains, who was president of his high school class and an all-state tennis player, later became an ensign in The United States Navy. While deployed on a ship in the South China Sea during the Vietnam War, he was assigned the duty of defending sailors charged with violating the Navy’s rules of conduct. Although initially apprehensive about performing a role that would put him at loggerheads with more senior naval officers, he apparently discharged his duties above and beyond the call of duty. After the ship’s
captain cautioned him about pursuing too vigorously his defense of the sailors, he was placed in the brig for insubordination. This eye-opening experience, together with his Quaker upbringing, largely sparked his lifelong interest in social justice issues. He became an early figure in the movement discrediting “official” statistical information released by governmental agencies, including the defense department.

I purposefully withheld the identities of the autobiographers from whom I filched the above personal revelations. So, if you want to know who revealed these juicy tidbits in their autobiographies, you will have to read all 10 of the autobiographies from beginning to end. My bet is you will be very glad you did. I know I found them extremely absorbing and very insightful as far as the strange twists and turns people’s lives and academic careers can take. I am confident you will, too.

If nothing else, reading these autobiographies may help alleviate your chronic depression based on thinking that all your better connected colleagues have had charmed careers while working at more prestigious academic institutions. Of course, my main concern here is not to make us feel better about ourselves or pique your interest in the circuitous routes that others’ academic careers have followed. It is to encourage more of us, despite the risk to our careers, to change by our actions the long-standing convention of academic discourse where “decorum trumps truth” to “truth trumps decorum.”

In my opinion, if we do this, then it will remove a major academic stumbling block to achieving the noble goals of the discovery and transmission of truth, however, nebulous a pursuit, it sometimes may be. After all, academics should not be a mutual admiration society where everyone has to go along with knowledge stifling conventions to get along with their careers (Foucault, 1980, pp. 79–133; Isaacson, 2007, p. 79). As Cooley (1918, p. 401) observed almost a century ago, “A sociologist must have the patient love of truth and the need to reduce it to the principles which all men of science require.” Unfortunately, in our present-day career-driven, academic environment where the rare true believer occasions snickers, it is easy to cast aside Cooley’s dictum as hopelessly quixotic (e.g., see Isaacson, 2007, p. 79).

**REFERENCES**


AUTHOR BIOGRAPHY

Lonnie Athens is a professor in Seton Hall University’s Criminal Justice Program. His life and research were made the subject of two books – one popular and the other academic. Pulitzer Prize winner, Richard Rhodes, wrote Why They Kill: The Discoveries of a Maverick Criminologists (Knopf, 1999), and two Italian criminologists, Adolfo Cerretti and Lorenzo Natali, wrote La Cosmoligia Degli Attori Violenti: L’ inedita prospecti Di Lonnie Athens [The Cosmology of Violent Actors: The Unknown Perspective of Lonnie Athens] (Rome: Aracne, 2004). Professor Athens, a past president of the Society for the Study of Symbolic Interactionism, received the Society’s highest honor, the George Herbert Mead Award for career achievements. He was also awarded Seton Hall University’s Researcher of the Year Award for Social and Natural Scientists. Over the last decade or so, he has been busily at work trying to develop a “radical interactionism” as an alternative to its conventional counterpart.
LATE INNINGS: REFLECTIONS ON AN ACADEMIC GAME

David L. Altheide

ABSTRACT

David Altheide reflects on his long career, noting the role of family, friends, colleagues, organizational culture, and luck.

The organization isn’t
My mind does
Dusty pictures reflect
The dream was

PREGAME

I wanted to be a ball player. Sociology sort of happened. My modest sociological efforts are a reflection of the games I played, many failures, and memories of self, especially identity. My work also reflects contingencies and emergence. Some of my most memorable experiences also include having a mentally retarded sister, which taught me a lot about stigma and resistance; the fundamentalist Pentecostal religion; then there is a lifetime in the academy. We’ll skip the significance – or not – of being born the day the U.S. bombed Nagasaki. We can start with baseball, my constant passion, so why not try to
mine a few nuggets for metaphors now? (I met the love of my life, Carla, while I was playing ball with her cousin!) My brother, Duane, and I spent countless hours playing catch, bemoaning rained-out games where we grew up in western Washington, and dreaming, imagining what it would be like to be in the “Show,” of the big leagues. Duane, a pitcher, was a bit wild and threw hard, hard enough that years later a “bonus-baby” from our area, who played in the “bigs,” remarked that he had more trouble batting against my brother than any major league pitcher. Decades later, the late Ira Robinson, a gifted social analyst and baseball fan, would remark to those attending the 1989 Stone Symbolic Interaction Conference in Tempe, AZ that every kid growing up wanted to play second base for the Dodgers.

I still play softball (“slowpitch”) with teammates of 30 years, but my best year was when I was 9 years old; we won the league, but were humbled when we played better teams “under the lights” in Tacoma. I should have quit then, but I continued to play into college and a two-year stint playing semi-pro ball for St. Regis Paper Company, where we would work in the woods for half a day and play ball the rest of the time. It was, without question, the best job that I ever had. That was when I first heard about Arizona State University (ASU); the baseball team was recruiting a teammate, Bill Hobert, a left-handed pitcher, who could throw a ball through a wall, and could hit it a country mile. Bill attracted many major league scouts to our games, and their frank opinions about my skill as a ball player caused me to reconsider my future. Without question, Hobert would have made the major leagues, but his dream died in Vietnam. I went to college. Like most young people about all we accomplished was establishing solid identities as potential ball players, and later, as failed ball players. But I learned about identity, and how important it was to be known as someone with potential, anything that audiences could point to, and see an aura of meaning around you. The exhilaration of hitting the gap with runners on became an emotional benchmark for life.

Baseball was sacred, even though we didn’t see the big league cathedrals until we were teenagers. The Seattle Rainiers minor league stadium was plenty divine for us, where social mobility of players going up and others coming down (from the Show, that is) taught us about the tradeoff between luck and skill – playing behind a great player meant that you’d never get to the Show. We learned that how you play the game mattered. In later years, my best sociological teachers and experiences were more about how to play – and not just work – at sociology. Greg Stone’s (whom I met when he visited ASU!) brilliant essay (Stone, 1970) on play is solid instruction, in my view, for all aspiring sociologists, especially qualitative researchers and symbolic interactionists. It is about fun and commitment and passion. This is different, my
teachers, Jack Douglas and Stan Lyman, would tell me, than simply having a “career” and using your research training as a mere stepping stone to becoming a professor and then an administrator, who made the big bucks. But I’ll get to the formal education material below.

**NOT A BONUS-BABY**

I told you about baseball in my life, but let me contextualize that a bit. My childhood gave me three things that a qualitative researcher needs to have: empathy, a sense of humor, and a passion to “do the work.” It has to be fun, too. All three were provided in our family’s economic and medical struggles, our fundamentalist religious background, and the care and love for my mentally retarded sister, Susan, who my parents took care of as long as they could, while dealing with me and my two brothers.

At the outset I wish to make clear that I was very much loved as a child and never experienced much insecurity, except for the fear of going to hell (see below); but as I told an audience when I was inducted as a Regents’ professor some 20 years ago, my mother and father had very little, but gave me everything they had. My mother and father were from the Midwest. My father came to the “West” at the age of 19 to work in a Civilian Conservation Corps (CCC) camp in Hebo, OR, on the Oregon coast. (I include a unit on the “Works Project Administration (WPA)/CCC” programs in many of my classes.) (I visited the ruins of this camp a few years ago, took pictures, and showed them to my elderly father. Various agriculture jobs took him to Sumner, WA, where he got a job working in a mill that made cardboard boxes. He worked there for some 30 years, alternating shifts every two weeks (e.g., days, evenings, nights), going deaf from the screaming machinery that dulled his thoughts and dirtied his clothes. He loved to fly airplanes, which he rented or co-owned, perhaps emulating his childhood hero, Charles Lindbergh. My brother and I planned to buy him an airplane when we made the big leagues; sociology never paid enough. He was bothered through much of his life by a speech stutter and never communicated much. He had been a cab driver in Quincy, IL, an amateur boxer, and I would learn later that he could party, too. Perhaps this is why he was always suspicious of my brothers and me when we would take the car and head for the “big city,” Tacoma, WA. He changed his ways when he married mom, got the old time religion, and became very righteous. My mother was more gregarious, and would be president of the PTA, work in a bank for several years, and dabble in selling products from our home.
She also taught Sunday school and became a very good teacher in her own right. She acquired status, respect, and built an identity while doing God’s work. There was a lot of talk about God in our lives.

God’s work, God’s will, and God’s plan? I think that my interest in the mass media and fear got its start from our fundamentalist religion, which was a constant in our lives; we got it on the radio, on TV, and several times a week at church. (Like many symbolic interactionists, my parents broke off from established churches in search of a purer truth!) Brother Ralph, Oral Roberts, First Mate Bob, and the whole crew of Bob’s good ship “Grace,” prayed for us, we put our hands on the radio, we sent them money – that’s right, 10% of everything that was earned. Hell, fire, and damnation were abundant. Righteous and proud we were, consistent we were not, and the butt of many jokes among our childhood friends. Our routine experiences included watching people speaking in tongues, seeing faith healers in action – but they could not heal my mentally retarded sister or my partially crippled mother – and at least one case of a woman dying in church, or “the Lord calling her home.”

My experience with absolutist thinking and worldviews would prove invaluable in guiding future field studies, the social construction of reality, and theoretical interpretations of the definition of the situation that would occupy my time in later years. I needed no convincing of the notion of “dissonance” when I read the classical study of how people accommodate reality When Prophecy Fails (Festinger, Riecken, & Schachter, 1956) as an undergraduate. Indeed, for fundamentalists, the issue of justice on earth is a secular manifestation of having little faith in an omnipotent and just God, who will set things right in the sweet by and by. Of course, there isn’t justice on earth! That’s part of the plan, the cosmology. I fully understood this point of view, even when I stopped believing it; it did prove helpful in sorting through religious, political, and ideological documents and statements in later research projects. And it helped me understand colleagues and friends who sought out all manner of religious footholds. I could never really get interested in serious sociological study of religion; I had lived it, saw it as the search for meaning and certainty, and preferred a secular ideology, even one that accepted the absurdity of human existence and our creative – indeed, desperate – search for meaning. Now, that was interesting!

The vessel for my sociological imagination was firmly in place by my teen years. After our first year in college, my brother, Duane, and I tried out our newly acquired sociological (and philosophical) concepts by engaging Christian businessmen at a local fair on whether or not God existed. We were not surprised by the retort, “If there ain’t a God, then how can a brown cow, eat
green grass, and make white milk.” Yup. It became apparent that people believe the damndest things, but it was also clear that my sense of right and wrong was engrained, including the narratives and iconography of fear that would occupy my time years later. Decades later my friend and colleague, Gary Marx (1988), analyzed how pervasive surveillance was in our culture, citing various religious songs and slogans that encouraged people to watch themselves for signs of giving into the devil and temptation, or “backsliding.” He called it “autosurveillance.” Well, I experienced this sense of guilt and holy control since God (and Jesus, etc.) could see and hear what we did all the time. My brother and I would not curse or say the word “fuck” because Jesus might not like it, so we would compromise when telling a dirty joke and avoid the “f-word” by simply saying “_uck.” Just a few months before my father died at age 92, he told me that he prayed for my grandchildren; he would hold their pictures and sing to them the surveillance spiritual, “… Oh, be careful little eyes, what you see. Oh, be careful little eyes, what you see. There’s a Father up above, looking down in tender love. So be careful little eyes, what you see.” As I cried, I realized how much he loved the little ones, but the sociological irony was not lost.

Our fundamentalist life helped me appreciate a sense of place and the definition of the situation: Movie theaters and taverns were the “Devil’s House” (decades later my ASU students would frolic in a real “Devil House” aka: Sun Devils). When we drove by the local theater, I looked closely expecting to see flames shooting out! (This awareness helped me later understand the war on terrorism and fear of “illegal immigrants.”) We later negotiated the definition of the situation with our parents when we wanted to attend an “educational movie,” even lying that our teachers recommended it. The strict rules led to my oldest brother’s resistance; Brison rebelled most of his life, and to the salvation of my other brother and me, led the way into college, encouraging us to try to improve our situation by getting an education. His perception of my parents is drastically different from mine, but this is the lesson about sibling and age differences.

Perhaps nothing so prepared me for the sociological imagination as living and loving my retarded sister, Susan, one of the few people who love me unconditionally. While the terms come and go with various interest groups (e.g., “developmentally disabled,” “mentally challenged,” etc.), we knew that Sue was different, and the label “retarded” worked fine for us, although we hoped that she would get a lot better, and mom and dad had many Elmer Gantry wannabees pray over her in tent and revival meetings. We normalized her different competencies, but we did notice the reactions of others in stores. We were more sensitized even as young children to “calling
people names,” staring at people who looked different, and were more willing to assist people who, in Goffman’s terms, were trying to manage their spoiled identity. Goffman might have paid more attention to how family members of the stigmatized also work at identity management. At any rate, this experience was helpful to me in several research projects, as well as working with a widely diverse student population.

My home education had some smooth and rough transitions to college and my formal educational and sociological experience. First, the smoother part: I mentioned that my mother was more gregarious and outgoing. This enabled her to thrive in various church groups, and she had great skill at teaching “Sunday school,” including rather advanced – for the time – use of multimedia in departing Bible stories, today known as master narratives. That, plus the constant reading and memorization of religious scriptures, gave us a solid foundation in language and retention skills, as well as a substantive bedrock in the Judeo-Christian literature and symbolism, which has served me well in my mass media work. (I also drew heavily on my Biblical knowledge in an undergraduate course, The Bible as Literature.)

The rougher part was that my parents, especially my father, were sensitive about the secular teachings of college, and the influence of the “atheistic professors.” (My parents’ sons all earned graduate degrees, with Duane, earning a PhD in psychology from the University of Innsbruck.) We were definitely influenced by our college experience, although I would come to realize that the intellectual and progressive influence of ideas needed practice lest the mundane day-to-day prejudices and antiintellectual reference groups recapture the language, meanings, and interpretations of everyday life. Call it intellectual backsliding. I saw in my own family how the meanings and significance of one’s “Socioeconomic Status” (SES) was less a variable and more of an emergent process of interaction with self and others, and the discourse being heard and practiced in daily affairs can trump that which was merely learned. These experiences helped me become a better observer and interviewer, on the one hand, while also becoming more distrustful of opinion poll results reporting the impact of SES, on the other.

**SPRING TRAINING**

My brothers and I benefitted from two pieces of social legislation: The massive funding of land-grant colleges and universities, and the National Defense Education Act (NDEA) loan. I followed my two brothers to Central Washington State College (Central Washington University) in
Ellensburg, WA in 1964. The NDEA loans were passed by the Eisenhower administration as a response to the Soviet space success (AKA Sputnik), which was regarded as a threat to U.S. national security. So, the NDEA was intended to support education and science students, especially if they would teach in the primary and secondary schools. These loans covered much of the tuition, fees, books, with a little left for room and board. (I include materials about the NDEA in a unit on the governmental response to crisis (however defined ….) in several of my classes.)

Central was a teacher’s college, but I intended to be an attorney, because our next-door neighbor was a lawyer, lived in a big house, and always had a nicer yard than we had. We got to know him a bit as we would retrieve baseballs from his property, and occasionally replace a broken window. This resonated with me when I studied about reference groups in my early sociology courses. Basically, I experienced what some researchers referred to as “negative reference groups”: If I couldn’t be a ball player, I knew much better what I did not want to be (e.g., a factory worker or lumberjack) than what I wanted to be. Fear drove my brother and me, who were roommates, to study hard, since it was common for half of the freshmen to flunk out in the first two quarters. We had terrific teachers and were drawn to sociology and philosophy. My minor in philosophy proved most beneficial over the years since I was able to learn about basic philosophical issues, particularly those pertaining to methodology, including epistemology, and the centuries old debate about realism and idealism. This foundation, along with exceptional courses in existentialism and phenomenology, fueled countless hours of discussion with a small, but fluid, group of faculty and students. Of course, we were competing, but our identities as campus intellectuals, supported by key faculty members, were very rewarding and jealously pursued.

My sociology professors, especially Dr. Virgil J. Olson, were tremendous. While he and most of my instructors did not publish very much – but we were thrilled when we saw their work cited! – he was a brilliant, if eclectic, instructor, who quickly engaged me and encouraged me to take advance courses, even as a freshmen and sophomore. Another teacher at Central was Dr. Charles Hawkins who earned a PhD from Chicago, where he worked on the classical jury studies. More withdrawn, Hawkins was very analytical and low key.

Dr. Olson arranged for me to attend the 1967 Pacific Sociological Association meetings in Long Beach, CA. He accompanied us (also Carla) on an auto trip, and showed us the sights on our first trip to San Francisco. I met Ralph Turner, who along with Lewis Killian, had authored a textbook that I had used in a collective behavior course. Five years later I would publish my first sociological paper affirming his theory about the public perception of
protest. We laughed with Jerry Olson about this trip 43 years later when Carla and I had dinner with him in Seattle. One of my last courses with him was a “Sociology of Knowledge” class in which we used books by Maurice Stein and Arthur Vidich, *Sociology on Trial* (1963); Edward Tiryakian’s *Sociologism and Existentialism* (1962). The latter book fitted well with my course work in philosophy, and fueled my interest in an interactionist and contextualized understanding of social order, which would be nurtured several years later when I attended University of California, San Diego (UCSD); the former book cast sociology in a broader social and political context. How ironic that Professor Vidich would write the introduction to my first book on television newswork (*Altheide*, 1976), and would become a friend and mentor, who, in addition to collaborating with a future mentor at UCSD, Stan Lyman, would also explain in print and personal conversations the legacy and linkages of Weber to his former teacher, Hans Gerth, and fascinating public – and private – accounts of the work and career strategies of C. Wright Mills!

I worked as a TA for Professor Olson, and he enabled me to give some lectures. I did not realize it at the time, but he was preparing me for graduate school, as well as hedging his bet on what he could say in a letter of recommendation. Most importantly for me at the time, Olson, along with a few others, was regarded as a campus intellectual and leader, who was also interested in social injustice. He was independent, gave me a glimpse of academic politics, and showed me how to play the professor game, not in the sense of “hustling” grants and making superficial contacts to further a career, but to engage colleagues, and even administrators on key issues. He was often on the losing side of issues with the administration, but he maintained his integrity. He was a role model. This, of course, was the period of the civil rights movement, and I was mesmerized by lectures and readings documenting discrimination, poverty, and oppression throughout the world, as well as far reaching social problems in our own country. I quickly changed from prelaw to sociology, and never looked back. I was able to write an undergraduate Honors Paper for Dr. Olson, on “Phenomenology in Sociology.” This project led me to examine key epistemological and philosophical assumptions and issues about causation, consciousness, and intentionality. I read Schutz’s collected works, as well as Merleau-Ponty, Scheler, Heidegger, Sartre, work by Tiryakian, and others. This background served me well when I returned to work on my PhD several years later at the UCSD. I remembered how many good students there were at Central, and other small, often under-appreciated schools, and drew on this perspective in recruiting graduate students for our programs at ASU. The next stop was the University of Washington (UW).
My experience as a graduate student at the UW was different, but still quite valuable. In 1967, I received a four-year National Defense Education Act Fellowship (NDEA Title IV). However, the raging buildup in the Vietnam War prompted my draft board to hound me mercilessly, so I had to leave the UW after one year to teach in Colorado. Sociology at the UW, as a reader might realize, has for decades been a bastion of positivism, but one could sense a few tolerable changes, as I note below. When I arrived on campus, the early “scientism” pioneer’s name, George Lundberg, was still on the faculty directory even though he had been dead for several years. His classic book *Can Science Save Us?* epitomized the positivistic, quantitative foundation on which that department was built. The courses were quite helpful and foundational. Clarence Schrag, who taught a course on a positivist version of “Theory Construction,” was very bright and supportive, and spoke kindly about a new (then ...) book by Tom Scheff on labeling theory. One of the best teachers that I ever had was Herbert Costner, a gentleman scholar, who lectured without notes on a two-way analysis of variance (when it first came on the scene!), and used colored chalk to draw overlapping normal curves, etc. A few decades later Professor Costner and I worked together on a departmental review at San Diego State University (SDSU). A terrific course, “Frontiers of Sociology,” was offered by Otto Larsen, an authority on the mass media. We used a massive *Handbook of Sociology*, edited by R. E. L. Faris. Other faculty would come to our seminars and discuss their work and research issues. Professor Larsen discussed the (then) recently published book about “grounded theory” (*Glaser & Strauss, 1967*). My MA thesis was based on survey data from a rather pedestrian study of small decision groups at the Boeing aircraft factory. I found the very popular “small groups literature” to be unimaginative and turgid. The project manager, Professor L. Wesley Wager, was a stickler for detail, and I learned the logic, method, and practical steps of working with data cards, key punch machines, and made countless trips to the building that housed the university computer. We crawled under tables looking for lost cards more than once. The only symbolic interactionist was Department Chair, S. Frank Miyamoto, who kindly agreed to be on my committee, and graciously engaged me in several conversations.

Sociological players were different at the UW than at Central. This was big time stuff, and I do not mean that in a sarcastic way. Big research grants and conventional academic professionalism were the order of the day. Protesters and faculty who spent their time on such matters were viewed, from what I saw, as tolerated, but a bit quaint. There was more important work to do. Several doctoral students befriended me and tolerated my naïveté about graduate school and becoming a professor, offering practical wisdom. (The most famous of my cohort of students was Jim Colborn, who was a minor league prospect at the
time, and would go on to throw a no-hitter for the Kansas City Royals in 1977, before rounding out his career as a top flight pitching coach for several big league clubs.) One man that I did receive considerable encouragement from was a visiting professor from University of California, Los Angeles (UCLA), Dr. Tony Ngubo, a black South African scholar, who was a member of the African National Congress when they were being imprisoned. But my encounter with Tony had nothing directly to do with race. I had written a paper for my thesis professor, Professor Wager, and he had absolutely destroyed it, criticized the substance, the writing, etc. The paper was a critique of H. Blalock’s “power model.” The paper was later presented at a professional conference, but I don’t think it was all that good. However, it did not warrant the devastation I experienced at the hands of my thesis chair. I sought out Tony, who had kindly talked with several graduate students from time to time. I told him about it, and despite being extremely busy, he was very helpful, cautioned me to not take it too seriously, too personally, that this was an opinion, and that I should rework the paper if I believed that it was good. I did so, and I was always grateful to Tony for those kind words. I have tried to practice the same conciliatory counsel with countless students devastated by other professors.

The Vietnam War was taking a toll in 1967. Unlike many draft boards around the country that were still honoring student deferments, my draft board was zealous in pursuing me. In those days, it was possible to actually get a position in a four-year institution, like Central, with an MA degree, or one nearly in hand. Accordingly, I began interviewing for teaching assignments. On a whim, and for the practice, I interviewed with the Dean of Southern Colorado State College (SCSC) (Pueblo, CO). The interview went well, but I was quite surprised when SCSC called in July 1968 and offered me a job. My MA was not yet completed, but that did not matter to them. Clearly, they wanted a “body” to fill classes. My wife, Carla, and I were delighted since we had made plans to go in the Peace Corps. Carla was a nurse, so they were eager to place her; as for me, there was talk of raising chickens in a third world country. We headed to Colorado, arriving in Pueblo, the night of the Chicago “police riot” against demonstrators at the Democratic National Convention. The grainy images on a bad TV in a cheap hotel in a run-of-the-mill steel town did not dampen my enthusiasm for a new job as a college professor.

**MINOR LEAGUES**

SCSC – now Colorado State University, Pueblo, CO – had just recently changed from being a community college to a four-year institution. It still
taught many vocational courses, so a colleague might be a professor of welding, professor of refrigeration, etc. There were two social science divisions because there were two powerful administrators who hated each other; so we had a division of social science – where sociology was located – and a division of behavioral science. After my first year, I served as Department Head (age 24), a position that I was able to avoid thereafter for some 30 years, when I served as Interim Director at ASU.

I became socialized into being a professor at SCSC. In retrospect it was one of the most collegial departments with which I was ever associated. There were six faculty members, including a newly minted PhD whose arrogance and insecurity taught me what to avoid in later years. There was my good friend, Robert Gilmore, with whom I collaborated on a research project. And there was also an amazing former probation officer, John Hargadine, whose work salvaging damaged young lives was recognized with a treatment center in Boulder, CO being named for him. His humor, wit, and wisdom taught me a lot.

Progressive faculty and students interacted across disciplines on the small campus. We fought the usual suspects: racist and reactionary administrations, zealous local cops – especially undercover agents on campus, and the “war machine” producing Vietnam – Kent State shootings, and civil rights catastrophes. My role was small, but important for my identity as a professor. I occasionally wore a red suit and, not surprisingly, became known as “David the Red,” spoke at numerous workshops and teach-ins, was asked to speak at an “alternative commencement,” and conducted some media interviews. I was learning the ropes.

I also learned sociology while teaching some 15 courses over a three-year period at SCSC. Some of these courses were taught in the evenings at the state prison in Canon City or at one or more military bases in the Colorado Springs area. It was exciting and challenging to be thrown into the curriculum, while also having the opportunity to design special topics courses, and also “team teach” courses with very capable colleagues in philosophy, anthropology, and political science. I taught several theory classes, including the “Sociology of Knowledge,” where I explored and attempted to integrate classical theorists – especially Marx and Weber – with the newly arrived, The Social Construction of Reality (Berger & Luckmann, 1967). I also learned about academic politics, power, police spying, social protests, and much more, especially about being a professor. The theory classes that I taught honed my background in Simmel’s formal sociology, which would be central to my modest efforts in developing, with Bob Snow, the concept of “media forms,” formats, and media logic, as well as the social construction of reality.
Three very fortunate things happened to me at SCSC. First – and toward the end of our stay there – my wife became pregnant with our daughter, Tasha. Second, the student protest movement had begun, particularly the awakening of Chicanos, fueled by the United Farm Workers movement in California. My colleagues and I had been closely working with students who were aware of historically egregious discrimination in the southwest. Coors brewing company in Golden, CO had discriminated in hiring practices, and they would not change. A national call went out to boycott Coors beer. Yet, a decision was made to begin selling Coors beer on the SCSC campus. The administration would not budge, so the students made a move; they had a sit-in to shut down the campus pub. A colleague, Robert Gilmore, and I were there and had been talking with the organizers. A minor confrontation occurred, and more was promised. This became a major event and received substantial news coverage. I saw that this would be a prime opportunity to test a thesis set forth by Professor Ralph Turner, whom I had met several years prior, on the factors that contribute to public interpretations and definitions of events. Turner argued that certain meanings are not clear cut, but rather, depend on contextual factors that inform how different audiences perceive events, including whether they deem an event a “real protest” or as “trouble,” etc. Based on our interviews and observations, we produced a survey – where I drew on my UW experience – collected and analyzed data, and drafted a paper that was accepted for publication in the *American Sociological Review* (Altheide & Gilmore, 1972). This would be one of my first publications.

Another fortuitous turn of events for me was when we hired Professor Roland Bower, who had taught for more than a decade at SDSU. One of the brightest and well-read scholars I ever encountered, Roland was also quite stubborn and simply would not finish his dissertation with Melville Dalton at UCLA. Dalton’s classic work, *Men Who Manage* (1959), was an exceptional qualitative study of two organizations, which challenged boundaries about fieldwork ethics. I actually tried to help Roland with the draft, but to no avail. Most importantly, he knew that I wanted to return to graduate school, and told me about a new program that had just begun at UCSD, under the guidance of Professors Joseph Gusfield and Jack Douglas. Roland wrote a letter supporting my application. And I got admitted! With all the enthusiasm, foolhardiness, and support from my wonderfully supportive – and quite pregnant – wife (and cat, “La Jolla”), we headed to California in September 1971. I was able to take a one-year leave of absence to protect my tenured position at SCSC.
Heading to UCSD was one of the best decisions that I ever made. Most important, our lovely daughter, Tasha, was born prematurely – but was strong and healthy – thanks to the help of the very capable medical people affiliated with the UCSD campus. We never dreamed that she would return to UCSD on a postdoc appointment to work in a medical school lab 30 years later. Not only was La Jolla a beautiful place to live, but the beautiful new campus – still under construction – had the backing and promise to become a great institution. Moreover, the unofficial charter was for the sociology department to be different than the other stalwarts in the UC system (e.g., UCLA, Cal, Santa Barbara). Joseph Gusfield had been hired to build the department, and the first person he hired was Jack D. Douglas, a brilliant scholar, whose book, *The Social Meanings of Suicide (1967)* was getting strong reviews. (John Johnson’s essay in this volume covers much of the important background material about UCSD and Jack Douglas, so I will not repeat it all here.) Stan Lyman was another exceptional faculty member with whom I worked closely. There was already a cohort of exceptional students in attendance, including John Johnson – my good friend and colleague for some 40 years, Carol Estes, and Carol Warren.

The atmosphere and organizational structure of the program enabled creativity and initiative. Supported by a National Institute of Mental Health (NIMH) fellowship, I was able to take courses while also being a TA for Joseph Gusfield, Stan Lyman, and Aaron Cicourel, as well as teaching off campus to support our new family. The program was lightly structured so that seminars and independent studies could fill most of the requirements. My MA from the UW and three years of teaching provided a solid sociological background, so I was able to take my oral exams during the second year, and move onto writing the dissertation. I learned when I left UCSD that several faculty objected to the speed of my progress and enacted program changes to prevent a recurrence. I have continued to oppose tightly structured PhD programs with numerous required courses.

The seminars, colloquia, and hours of discussions with students set the tone for my future work. Our strong engagement with existential sociology, intended as a critique of both symbolic interactionism and ethnomethodology, focused on emergence, situational/contextual meanings and problematic and usually negotiated social orders. Stan Lyman’s scope of knowledge, including cinema and popular culture, enlivened his unparalleled lectures and discussions of identity, race, and ethnicity. His
immersion in legal foundations of moral and sociological reasoning, particularly regarding WW II and the Japanese relocation camps, was brilliantly connected to issues involving student protests. Several of his lectures articulating the social control implications of organizing and restricting social interaction according to the time, place, and manner of social action intrigued me, and led me to play with various combinations, applying them to various social problems, including efforts to restrict and regulate what I (and perhaps others) would later term “othering.” I built much of my conceptual and theoretical work on the nature, use, and impact of communication formats on the triumvirate of time, place, and manner. I was very happy that I got to thank Professor Lyman several decades after I left UCSD.

Jack Douglas is without question the smartest person that I know. His breadth and depth of knowledge and integration is humbling. For me, his willingness to accept what I knew and had to offer was most gratifying. He was the most unthreatened intellectual that I have ever encountered, except for perhaps John Johnson. He encouraged undergraduate students to publish and worked to remove limiting status distinctions that would prevent the real work from getting done. Jack and Stan Lyman were not really against large grants, but they implored us to not pursue a grant just to get the money, unless it was something that was really worth pursuing, that was cutting edge, and about which we were passionate.

A huge break for me at UCSD came through our team field research project of plans to hold the 1972 Republican Convention in San Diego, although corruption scandals ultimately moved it to Miami. We had researchers studying all facets, including the media, police, planning groups, merchants, the military, and city planners. As noted above, I had an interest in the mass media and had some preliminary work with it. My task in the team project was to study a local TV station, forever known in my dissertation and first book as Channel A. Jack was instrumental – along with the help of a graduate student, who worked at the station – in gaining access. Key insights about the interaction of organizational and institutional contexts were developed during this fieldwork, which took me to several stations, including the Democratic and Republican conventions in 1972. Since I had to gain access to areas, for example, network booths above the convention floor, for which I did not have clearance, I also developed some ideas about the negotiation of security, including, “presence implies acceptance,” “presentational ‘with’ conveys legitimacy,” and other features of a phenomenology of security. This material was further developed when I joined John Johnson at ASU.
Now I discuss a bit about my initial experiences at ASU. This is relevant because I was able to join John Johnson in the Department of Sociology, and having at least one other person with whom to work seems to nurture creativity and productivity. Baseball players who do not have a permanent position but are kept around are called utility players. They are parttimers, nonpermanent, and in the language of the modern university, are nontenure track. One reason John Johnson was hired at ASU in 1972 was because he could be branded as an “ethnomethodologist-type” and ASU wanted one. I was given an opportunity to go there in January 1974 as a sabbatical replacement for Bud Pfuhl, Jr., who would later become a wonderful colleague and friend. The first choice for his replacement committed suicide, so there I was. I was very fortunate since many of our very capable colleagues would never gain a full time university position. I was sure that I would be at ASU for a year and a half; I was off by 35 years. Our son, Tod, completed our family in 1976, and would eventually be courted by the Los Angeles Dodgers as well as recruited to play baseball at ASU, and then at San Diego State. Like his sister, Tasha, Tod would bring countless hours of joy and opportunities for me to coach youth sports, which contributed to my understanding of mediated youth culture.

The best part of my experience at ASU was getting to work with John Johnson. Our collaboration extended from our time in the Department of Sociology until we transferred to what became the School of Justice Studies (SJS, … and various other names) in 1982. We worked on projects, articles, and books together, on the one hand, while John did everything that he could do to protect me from resentful senior colleagues, on the other hand. Space does not permit a full record here, but one example may capture his support. One person resented my purchase of a house before obtaining tenure. Stan Lyman had warned us about quirky academic rules regarding “rites of passage,” but I bought a house anyway. My substantial teaching, service, and research/publication records were downplayed by several of the senior faculty, and the department denied my tenure and promotion. John, barely tenured himself, wrote a scathing and very descriptive – and truthful – letter to the Dean of Liberal Arts and Sciences, explaining the “taboo” that I had broken and other relevant factors. Ultimately, the Dean’s office relented and I was granted tenure and promotion. That was one of the many experiences that led John and I to vow to NEVER become like “them” if we ever became senior professors. I think that we affirmed that vow in our careers.
(Note: I started running to deal with the stress of this ordeal, and have kept
go - ever more slowly!)

John would save me in other ways, too. Like many aspiring qualitative
researchers looking for the first “book” publication, my first book
(Altheide, 1976), a revision of my dissertation, was brokered by John’s
contacts and “good word” with Sage. John was also the key person in
getting the esteemed Arthur J. Vidich to write an Introduction. We drew on
our research interests and experiences with the university bureaucracy to
produce a defensible quantity and quality body of work.

Our common interests in qualitative research, especially ethnography,
seemed to hit ASU about the right time, as debates about the “qualitative
vs. quantitative” approaches burned hot across campuses and disciplines.
Our work with fellow-travelers in sociology, especially Bud Pfuhl (Erdwin
Pfuhl, Jr.) and Robert Snow, provided some political support to sustain us,
and provide an alternative to the conventional positivism that ensnared
most social science faculty at ASU. Indeed, the expansive interest in
qualitative methods in such diverse disciplines on campus as anthropology,
education, nursing, and even some glimmers in the school of business,
provided many opportunities for interdisciplinary lectures, courses, and
graduate committees. We were also fortunate to have outstanding graduate
students, like Joe Kotarba and Wendy Espeland, who pursued qualitative
work in a less than supportive environment.

An opportunity arose for John and me to transfer to the Center for the
Study of Justice, which was informally known on campus as the “other
sociology department.” We were grateful for this opportunity, particularly
because there was more acceptance of our interest in qualitative research.
The interest in qualitative work at ASU continued to grow to such an extent
that John Johnson and I spent considerable time and effort attempting to
establish a “Consortium for Qualitative Research,” which would become a
data repository for qualitative research, as well as integrate seminars and
qualitative teaching and scholarship, while merging into an international
center for post doctoral – and sabbatical leave – projects. Despite having the
enthusiastic support of some of the most productive faculty from some 13
academic units, internal politics derailed the effort, and the Graduate
College would not provide the very modest start-up costs. I continue to
believe that this was a major loss for ASU. Nevertheless, perhaps because
we were some of the early qualitative “types” on campus, John and I served
on dozens of theses and dissertations over several decades.
THE EMERGENCE OF MEDIA LOGIC

My modest contributions involving media logic and a methodology for the qualitative study of media work, content, and meanings emerged through research and conversations. My pursuit of media studies and theoretical connections with symbolic interaction grew out of teaching opportunities, small grant support, and above all, my good fortune to work with Robert Snow, whose excellent foundation in social psychology and symbolic interaction at the University of Minnesota, particularly dramaturgy, were aided by Don Martindale and Greg Stone, as well as a cadre of graduate student colleagues that included Dennis Brissett, Chuck Edgley, Harvey Farberman, Ray Oldenburg, and others. Our common interest in the mass media and baseball led to many conversations, ball games, and ski trips, in which I learned more about the communication order and symbolic interaction. Bob had a much deeper grasp of the different experiences of radio and TV audiences, yet we both recognized that Marshall McLuhan had it partly right, but his technological determinism did not accommodate contingency, culture, and the definition of the situation. Far more was theoretically cogent beyond “hot” and “cold” media. My recollection is that Bob appreciated the “grammar of media” and the “rhythm of everyday life” arguments better than I did at first, and together we saw how baseball, in particular, was changing to accommodate “audience ratings,” and technological intrusions.

The notion of “media logic” emerged from these experiences, which we articulated in our first collaborative effort, *Media Logic* (Altheide & Snow, 1979). We knew that the concept was a bit premature for the sociological and communication studies establishments, but we believed that the perspective and the theoretical integration with interactionist conceptions of the definition of the situation were compelling enough to warrant additional study and development. We started out to theoretically integrate audience engagement with media into everyday life practices and social meanings. The critical development was treating media as an institutional form rather than merely content (Snow, 1987). We ended up with an interactionist theory of mass communication and social order (Altheide, 1985). In later work, this conceptual foundation would be expanded as a theory of institutional communication control and social change (Altheide, 1995; Snow, 1983). This work would benefit from several sabbatical leaves that provided suggestions and insights from many scholars in the U.S.
(e.g., Lance Bennett), as well as those in Canada and throughout Europe and Scandinavia, including Karl Erik Rosengren (Sweden), Henri Peretz (France), Luigi Spedicato, Milly Buonanno, Giovanni Bechelloni (Italy), Keith Soothill, Frank Furedi (England), and Richard Ericson (Canada), whose application and extension of media logic and formats to clarify the institutional communication of control process demonstrated how routine organizational practices are reflexive of power and sustain dominant relations. Perhaps it is because we both took academic “lumps” for this theory that I have been most gratified by the gradual acceptance and international use and continued work on the development of media logic (Couldry, 2008; Lundby, 2009).

Much of the early efforts to develop the concept media logic and flesh out its applications were contingent on receiving small grant support from ASU. Summer grant support, now essentially nonexistent at ASU and most other universities, was the life-blood of creativity and innovation. While it was expected that the research would lead to a publication or perhaps a larger grant proposal, it was not so narrowly tied to obtaining substantial external funding as is now the case. Faculty could apply for – and usually receive – modest small grant support to pursue research interests. I conducted several projects that became part of the corpus of media logic and qualitative document analysis, including studies of sports journalism, TV newsgathering, TV journalism awards, amateur running, the Governmental Response to Crime, media coverage of the Iranian Hostage Crisis, visual representation of the Gulf War, etc. Also, receiving the Graduate College Distinguished Research Award in 1991 enabled me to bring Henri Peretz (University of Paris) and Richard Ericson (University of Toronto) to our campus for a semester, each.

**QUALITATIVE MEDIA ANALYSIS**

I had relied on documents and some news and governmental committee transcripts in work on the media coverage of mental illness history of Senator Thomas Eagleton, who was forced to resign as George McGovern’s vice presidential candidate, as well as the media blitz against Bert Lance, who resigned as President Carter’s director of the Office of Management and the Budget. My emerging research agenda to study documents, especially some methodological developments, were aided by small grants as well as what I will call a “conceptual challenge,” along with two projects, all occurring within a four-year period, roughly 1980–1983. These all involved research issues dealing with documents. Basically, I studied what
interested me. My qualitative approach to document analysis emerged from challenges of theoretically sampling and analyzing documents ranging from newspaper reports to Federal Aviation Authority documents about aircraft crashes. The conceptual challenge came from reading research in top sociology journals, as well as Science, about the role of news media reports contributing to suicide, in which the author argued that the publication of certain suicides (e.g., celebrity suicides, a homicide/suicide) was followed by an increase in suicides several days later. My subsequent work to check these claims by examining documents, including a piece with my ASU colleague, Pat Lauderdale, challenged the validity of several of these assertions involving single car traffic accidents and aircraft crashes as homicide–suicides (Altheide, 1981; Altheide & Lauderdale, 1987). As I reflected back on the process that I was using in several of those projects, it became apparent that I was developing a method of sampling, creating a protocol, and recording qualitative and descriptive data. These steps would be refined in the development of what I would call Ethnographic Content Analysis, and be the basis for qualitative media analysis (Altheide, 1987).

The development of a theoretical approach and methodology for analyzing documents was most strongly influenced by my study of the Iranian Hostage Crisis in 1979, when American hostages were captured in the Iranian embassy and held hostage for 444 days. The resulting news coverage of this “crisis” became the foundation for the public perception and official governmental policy that I documented in subsequent work in more than 30 years on news coverage of the Middle East, including two Gulf Wars with Iraq and the War in Afghanistan. There was extensive news coverage of the Iranian Hostage Crisis. The ABC late evening news/talk show, “Nightline,” began with this coverage. Several important news information bases were becoming more widely available during this period, including the Vanderbilt TV News Index and Archive – critical for TV news – and the now widely used Lexis/Nexis – until recently, mainly useful for print media – which had previously been used primarily by law firms and businesses. Both services were quite expensive to use. But the important point is that more news information was becoming available for researchers.

The task was how to use these information bases in the most productive way. I recorded some broadcasts and rented videotapes of hundreds of others from the major network newscasts from the Vanderbilt University information base. Altogether, I observed nearly 1,000 reports. The general process of conducting ethnographic content analysis emerged during this lengthy study, including visual and verbal statements. The general steps include: exploration, discovery, protocol construction, comparative
quantitative and qualitative analysis. This approach would be refined through several other projects over the next 15 years (Altheide, 1996).

Another project was The Governmental Response to Crime Project, coordinated by political scientists Herbert Jacob and Robert Lineberry at Northwestern University. The aim was to investigate how some 10 cities adjusted and changed in response to urban crime over approximately a 40-year period. My ASU colleague, John Hall, kindly asked me to collaborate with him as we undertook to study the Phoenix portion of the project. We had several team meetings in Evanston, IL as the general research agenda was set forth. Much of our work involved what Norm Denzin described as “triangulated” research, combining different investigators, different settings, different methods, and different data. We interviewed local “decision-makers,” but also investigated numerous documents, including newspaper reports, meeting transcripts, and other archived records. Again, there was a challenge of how to systematically collect, code, and analyze documents. This was accomplished by adjusting the general approach used in the research on the Iranian hostage crisis.

Several research opportunities contributed to media and document analysis after transferring to the SJS in 1982. Additional projects provided challenges to adapt a qualitative approach to systematic document analysis, as well as examine the direct and indirect influence of media themes and formats on social policies, particularly emerging public social definitions of social problems. Computer software and qualitative protocols were used in several qualitative evaluation projects of community action programs undertaken in the late 1980s and early 1990s. These projects included: analyses of juvenile crime policies; a national project on “The NonProfit Sector” with the Urban Institute; domestic violence policy; and in the 1990s, several major school evaluation projects undertaken with interactionist scholar, Dee Spencer. I am gratified that many scholars have found qualitative media analysis/ethnographic content analysis/tracking discourse to be useful in their work (Lewis-Beck, Bryman, & Liao, 2004).
John Johnson was editor.) I attended my first Stone Symbolic Interaction Symposium in Iowa City in 1986. This was a monumental experience because, in addition to renewing acquaintance with Norman Denzin and Peter Hall, I met Harvey Farberman, as well as several people associated with the “Iowa School” of Symbolic Interaction including Carl Couch, and several of his former students (e.g., Stan Saxton, Mike Katovich, Dan Miller, Joel Powell, and others). This small gathering was one of the best conferences that I had ever attended, and the welcoming atmosphere, along with the inclusion in planning future gatherings, was a welcome respite from the rather inconsistent involvement that we experienced after transferring to Justice Studies, with its main core of younger faculty cohering more around critiques of law, social policies, and social problems. This was the start of a 25-year relationship with SSSI and colleagues that included many conferences, including two that John Johnson and I cohosted in Tempe.

Carl Couch’s work on interaction process fit well with his opus on the role of various communication media in social order and change. His book, *Constructing Civilizations*, (Couch, 1984) provided ample insights and data about how important it was to contextualize information technology with political processes and dominant institutional controls. I learned from this as I undertook studies of fear and the media (Altheide, 2002, 2006) with outstanding graduate students (e.g., Dion Dennis, R. Sam Michalowski, Kristi Wimmer, Ray Maratea, Roy Janisch, Lindsey Korbin, Michael Coyle, Kimber Williams, Jennifer Grimes, Chris Schneider, and Tim Rowlands). Further refinement of qualitative media analysis to incorporate “tracking discourse,” or following theoretically sampled words and phrases over time and across media expanded what began as a quasi-case study approach to institutional and historical analysis that illuminated cultural sociological forms, for example, the “problem frame.” Conversations with colleagues Gray Cavender and Pat Lauderdale – and always, John Johnson – helped clarify the relevance of such notions as the “entertainment format” to crime policy, along with broader social control efforts.

Finally, I would be remiss in not crediting my wife, Carla, and our children, Tasha and Tod, for my good family fortune. I have been luckier than many friends, colleagues, and students whose lives – and work – have been touched by tragic marital and family situations. I do not live in a “Pollyanna” world, but my everyday life happiness, stability, and security freed me to join my children in various youth sports and activities, learn from these experiences, and not resent the time away from work. I am grateful that I have been able to watch them grow, excel, fall in love, marry, and have children – grandchildren to whom I can dedicate books and take to ball games!
EXTRA INNINGS

It is getting late in the game. I will be retiring in 2011 after 43 years of teaching (37 at ASU). There remain other projects, a few more questions to pursue, and few comments to share with colleagues, who have kindly supported my modest efforts and helped shape the player that I have become. I am most fortunate and have no regrets. I mean, if I couldn’t be a ball player, then being a sociologist and professor was a terrific option. It has been very gratifying to teach students, engage research questions, and interact with exceptional friends and colleagues like John Johnson and Bob Snow, while having some control over how we apportion time. There is some implicit advice in my comments, including the importance of cherishing family and friendships, staying physically active to deal with inevitable stresses and injustices associated with academic life, and following your research passion rather than dollars. Yet, as I write this in 2010, most universities demand that faculty members “get funded,” equating dollars with quality of ideas. Funded research in social science, of which I have had little – mainly small grants, fellowships, and contract work – strikes me in general as being controlled, contrived, and seldom conceptually innovative. I should also stress that pursuing paradigms, ideas, and topics that are not mainstream can be risky professionally, but in my view, that is essential for creativity and theoretical advancement. Some of these pursuits and projects will work out and others will not.

I have tried throughout this essay to capture some key contributions, moments, and processes that our work reflects. Of course, in keeping with fieldwork ethics, I have provided the most significant general points that I recall as being relevant to my approach and interests, although others may not fully agree with my rendition (Johnson, 1975). Whether you find this to be helpful to your own project, not entirely accurate, or not worth much, please remember that try as I can, I am limited by my perspective. After all, I wanted to be a ball player.

REFERENCES


David L. Altheide, PhD, is Emeritus Regents’ Professor on the Faculty of Justice and Social Inquiry in the School of Social Transformation at Arizona State University, where he taught for 36 years. His work has focused on the role of mass media and information technology in social control. His most recent books are *Qualitative Media Analysis* (2nd ed., Sage, 2012) and *Terror Post 9/11 and the Media* (Lang, 2009). He received the Cooley Award three times, given to the outstanding book in symbolic interaction, from the Society for the Study of Symbolic Interaction: in 2007 for *Terrorism and the Politics of Fear* (2006); in 2004 for *Creating Fear: News and the Construction of Crisis* (2002); and in 1986 for *Media Power* (1985). Altheide received the 2005 George Herbert Mead Award for lifetime contributions from the Society for the Study of Symbolic Interaction, and the society’s Mentor Achievement Award in 2007.
AN ACCIDENTAL ANTHROPOLOGIST, A SCEPTICAL SOCIOLOGIST, A RELUCTANT METHODOLOGIST

Paul Atkinson

ABSTRACT

I completed a degree in classics at Cambridge and entered the British Civil Service. After a moderately successful, if unremarkable career, I took an early retirement and now live in London, spending my now free time at the opera and the theatre … . No, that’s not right. Start again. Having decided to become a social anthropologist, I had my first experience of fieldwork on the island of Crete. I went on to specialise in the anthropology of modern Greece, and also wrote several popular books about people and places in Greece. I now live in one of the picturesque hilltowns of the Peloponnese . . . . No, not that either. Try once more. My father, who was a gifted amateur photographer, gave me a classic Rolleiflex camera for my 21st birthday, and I became a professional photographer, specialising in documentary photographic essays on social conditions in rural Europe. No, not that either . . . . I combined my first degree in social anthropology with a postgraduate training in linguistics.
I went on to research and publish on discourse and social interaction, bringing together interactionist sociology, anthropology and semiotics. Well, not quite . . . . As you will see, all of these – and other – lives might have been mine. The actual life seems no more coherent than those shreds of unrealised possibilities.

ROUTES AND DETOURS

I never intended to be a sociologist. Indeed, I am not perfectly sure that I have ever really been one. I stumbled into it by a series of knight’s moves, driven by impulse and accident as much as by deliberate decision. Reflecting on my own life and career shows me that there are quite different ways of telling and writing it. One version could represent it as a consistent and rational set of personal and intellectual decisions. The other, the one I think is the more faithful, reveals that the orderliness of a biography is normally found retrospectively, the product of a tidied-up account. Far from a coherent set of epiphanies, my career seems to have been based on chance and impulse.

I was born in the suburbs of London in 1947, an only child born shortly after the Second World War. I was the first in my family to receive an academic education. My parents had both attended the sort of vocational schooling aimed at producing clerical workers, and my father was a clerk in the newly nationalised railways. (How old-fashioned the term ‘clerk’ now seems.) My mother never had full-time paid employment after I was born. We cared deeply about being respectable. I have reacted against that sort of buttoned-up respectability ever since. Better to be respected than respectable.

Like many of my generation I was socially and geographically mobile because of the sort of educational opportunities that were denied to earlier generations. The selective grammar school system took bright youngsters and gave them a rigorously academic education. My boys’ school was academically excellent, and competitive. (It was very similar to the environment portrayed in Alan Bennett’s play *The History Boys*, the film of which was in fact shot on location at my old school.) It produced many academics and provided a route to upward mobility for post-war boys. Like most of my fellow pupils, I continued on to university. Mine was a fortunate generation – free university education was available, with a grant that covered one’s basic needs.

I went up to Cambridge in 1966, intending to be a classicist. I had a scholarship in classics, and I was very good at Latin and Greek.
My intellectual future looked assured, although I was unclear whether I wanted to be an academic or to pursue a career in public service (the diplomatic corps seemed an attractive prospect). Anyway, almost immediately on arriving at King’s College, Cambridge, I experienced an intellectual crisis. I realised that I could not face doing classics any more. Luckily King’s was extraordinarily understanding, and the Fellows were willing to support me in deciding on another degree subject. (They could, I suppose, have told me to go away and come back another year when I knew myself more sensibly.) Leaving classics was one thing, but settling on a new direction was quite another. Well-meant but conflicting advice from the College Fellows did not really help (it included mediaeval Italian poetry and Oriental Studies). I thought of myself possibly becoming an archaeologist. The Cambridge tripos (as degree schemes are called there) combined archaeology and anthropology (physical and social).1 And so I embarked on ‘Arch and Anth’, a few weeks late.

Within a few more weeks the teaching of archaeology had squashed my nascent enthusiasm. But social anthropology turned out to be much more fascinating. I tottered through my first year (thanks to some remedial classes in archaeology and some undistinguished work in physical anthropology), and was able to specialise in social anthropology. The teaching of anthropology was not uniformly high, but we attended lecture courses from some major figures. Edmund Leach, Audrey Richards, Marilyn Strathern and Stanley Tambiah were inspirational intellectuals, and having one-to-one supervisions with such world-leading people was a formative influence. If nothing else it made one all but fearless in holding a discussion with, and sometimes challenging, people who were undisputedly world-leading scholars. We were required to take an optional paper. I managed to choose the most difficult option – general linguistics, with which I became fascinated, though I was too idle to master the most technical aspects of the discipline. Dell Hymes, the linguistic anthropologist, was a visiting scholar. His lectures were barely audible and only intermittently comprehensible. They were, however, oddly inspiring.

I suppose I nearly became an anthropologist of modern Greece. I spoke a little modern Greek, having spent some months in Athens, teaching English and being thoroughly feckless between leaving school and going to university. In the summer of 1967, I teamed up with two student friends, and was given a tiny amount of money from my College, to go to Crete, to record folk music and take photographs in the island’s White Mountains. The notable thing about that ‘expedition’ was that one of my fellow students was Michael Herzfeld, who did go on to become one of the world’s leading anthropologists of Greece. He is now a professor of anthropology at Harvard. I have remained
a philhellene ever since. I had become a reasonable photographer, and I assembled a passable portfolio of Cretan photographs, many of them portraits of villagers and shepherds. I still have the negatives. I really ought to transfer them to digital files and work on them again.

Photography was a special interest. My father was a very good amateur, and could have turned professional I think. He gave me a treasured Rolleiflex twin-lens reflex camera for my 21st birthday. I have it still, nestling among a large collection of Rolleiflexes and other vintage twin-lens cameras. Photography, while I was a student, was a fashionable preoccupation. People like David Bailey and Terence Donovan were breaking new ground in the genre, and there was a new generation of professional photographers breaking free of genteel studio-based fashion and portraiture. I still have a darkroom, but it gathers dust. Like many amateurs, I have a great many cameras. I simply do not take enough photographs, and there have been various ethnographic research projects for which I have not invested time and effort in making a photographic record. A bit of a waste, really.

I gained some useful experience at Cambridge over the summer of 1969. There is a village called Elmdon, near to Cambridge, which was a sort of training-ground for young anthropologists. Every summer the poor villagers were afflicted with young anthropologists. I was one of them, pestering them with questions about housing conditions in the village. It was a good chance to experience how to do fieldwork, write fieldnotes and write a report under pressure. The collective work was subsequently collated and published by Strathern (1981).

Cambridge provided an escape from the suburban respectability I had been born into. My shift from classics to anthropology also pushed me away from respectability towards a more raffish intellectual style. It was a period of student rebellion, and like many others I flirted with radical politics. I was a stereotypical long-haired left-wing student of the 1960s. Sadly, nobody from either side ever approached me to become a spy in the classic Cambridge tradition. Cambridge (or more specifically, King’s College) gave me something. In contemporary sociological terms you might call it ‘cultural capital’, but that makes it sound too mechanistic. It included self-confidence coupled with a reluctance to be unduly earnest.

EDINBURGH

Being feckless, I had no proper plans for postgraduate research. By chance a funded doctoral studentship came up in Edinburgh, though not in linguistics.
or anthropology, but in educational research. A social psychologist called Liam Hudson had recently gone to the Chair of Education at Edinburgh, and was busy recruiting people like anthropologists and social psychologists, with the aim of creating an innovative research centre. I took the next available train to Edinburgh and somehow managed to talk my way into the studentship. I now had three years of funding, and virtually no idea of what to work on. I tried to attend classes for the masters in linguistics anyway, but, with characteristic lack of application, I dropped out of the classes.

My academic socialisation at Edinburgh was largely continued with and by my fellow postgraduate students. They were an interesting collection of people, several of whom went on to have very successful academic careers. As a group we were more or less feral. There was little mentoring or supervision from the academic staff, and we were physically out of sight on the topmost floor of an Edinburgh tenement; the professors had posh offices on the more favoured lower floors. It was like being the children in one of the great English country houses: out of sight and out of mind in the nursery for the most part, occasionally allowed to join the grown-ups downstairs.

It was an especially invigorating time to be a postgraduate student. Intellectual challenges appeared virtually every month and it was an era of public intellectuals: R. D. Laing, Herbert Marcuse, Regis Debray, Marshall McLuhan, Claude Lévi-Strauss, Roland Barthes and many others were transforming the academy. They all impinged on the social sciences, and we read them avidly. There were fascinating developments in sociology too. The work of American researchers was becoming available. Urban ethnography and deviance studies provided publications by people like Howard Becker, Ned Polsky, Jack Douglas and David Sudnow. Symbolic interactionist ideas came to us in the form of the anthologies edited by Manis and Meltzer (1967) and Rose (1962). We became aware of The Discovery of Grounded Theory (Glaser & Strauss, 1967), and discovered Harold Garfinkel’s ethnmethodology (1967). While our reading was only minimally guided by academic mentors, and barely coherent, we benefited from eclectic influences. We were free to find our own way. The net effect has been that my own academic interests include a bricolage of interactionist and phenomenological sociology, social anthropology and semiotics.

By a series of accidents and stumbles, I found myself studying medical students. I had a brief flirtation with pencil-and-paper social psychology (Twenty Statements Test, Semantic Differentials, that sort of thing), but I soon reverted to my anthropological roots and based my doctoral research on participant observation in the Edinburgh medical school. I spent two years observing clinical instruction in Medicine and Surgery. I did not
embark on this with the explicit intention of recapitulating Boys in White (Becker, Geer, Hughes, & Strauss, 1961), although that classic of the Second Chicago School clearly became a significant benchmark. (I was lucky enough to find a copy signed by Everett Hughes in a local second-hand bookshop; it is still a treasured possession.). In 1972, my studentship came to an end, and I had to raise my own meagre salary with a small grant from the Nuffield Foundation, and finished the fieldwork in the medical school. Like most of the work I have done since then, it combined interactionist ideas with other strands of thought, grounded in fieldwork in the style of the ‘Second Chicago school’ (Fine, 1995).

It gave me experience of ethnographic fieldwork. The latter has been a major theme in my career. People who write historical accounts of fields like the sociology of education or the sociology of medicine can find reasons why people like me undertook ethnographic fieldwork. It can be portrayed as part of an intellectual rebellion against former modes of sociological inquiry, functionalist and survey-based. But in my case there was no ‘rebellion’ at all. As far as I was concerned, as a product of Cambridge anthropology, ethnographic fieldwork was the obvious way of conducting research. I mention this because our biographies can demonstrate that retrospective accounts of ‘paradigm shifts’ and the like are very often (always?) exaggerated at best, and completely inaccurate at worst. We graduate students did not undergo intellectual conversions, although a conjunction of circumstances meant that we helped to import well-established ideas from one research field to another. I also became a sociologist of medicine just at a time when a number of others were doing the same. Retrospective accounts of the rise of medical sociology account for it in various ways that suggest a purposeful movement on our part. As far as I could see, it was largely coincidence that led a number of young British social scientists to converge on a broadly interactionist perspective on medical institutions and encounters.

One especially valuable experience at Edinburgh was the year I was the editor of the New Edinburgh Review, a quarterly of literary and social commentary, and new Scottish poetry. This was a recent reincarnation of the famous Edinburgh Review of the Scottish Enlightenment. It was run by the Student Publications Board of the University, along with the student newspaper and other publications. The Chair of my Board was an ambitious left-wing young undergraduate called Gordon Brown. Being the editor meant commissioning articles, getting them illustrated by young artists from the College of Art, booking each issue into the printers, physically delivering the copy, checking and marking up galley proofs: the entire process of publishing. I soon learned all I needed to know about defaulting, evasive
and untruthful authors, printers’ deadlines, print runs and advertising revenue. As my grandfather was a compositor and my mother a proof-reader, I like to think I already had some printers’ ink in my veins. I have edited journals and books since, and I have always been glad I had that early experience as a postgraduate student.

My everyday life and intellectual career were both enhanced by the fact that Sara Delamont and I became life-partners at Edinburgh, and have remained so for the past 40 years. Sara had also read anthropology at Cambridge, and we had known each other but were not close. We were both recruited to the Edinburgh research group by a similar route. Sara was working on a study of intellectual styles among school students, and it led her to conduct an ethnography of an elite girls’ school. Without her inspiration, encouragement and intellectual companionship my own academic career would probably have been stillborn, victim of my general fecklessness. When we were first together Sara and I made a very conscious decision to avoid too many joint publications. We saw too many couples working in the same field who published together, and who struggled to maintain separate academic identities. People often seemed to assume that one or other partner did all the writing, and we were determined not to fall into the trap. So we kept that up for a long time – publishing only the occasional piece together. Academic couples in anthropology and sociology are not uncommon. They reflect how personal and intellectual commitments are intimately related. Somebody ought to do a biographical piece of research on couples and their shared intellectual development. I know that Sara and I have always been each other’s first reader, first critic and the first to share ideas with. It calls for more than the usual sort of mutual affection and trust to do that for 40 years and more.

A FOOT ON THE LADDER

In 1973, I went to Stirling University on a one-year teaching contract. Sara had already taken up a lectureship at Leicester, so we saw little of each other for a while. I learned a lot again. It was my first experience of teaching, and I was thrown in at the deep end, trying to cope with three-hour seminars in Advanced Social Theory, tutorial groups in first-year anthropology and a mixed portfolio of other commitments. I lived in a staff apartment on the beautiful but somewhat isolated campus. With my contract nearing its end, I managed to get a permanent lectureship at Cardiff University. I went there in 1974 and have been there ever since.
Cardiff sociology in those days was undistinguished. I am always amused by British sociologists who look back nostalgically to a mythical golden age in the late 1960s and the 1970s. There is no comparison with the state of things today. There were few decent departments, and a lot of second-rate people teaching and doing little research or publishing. Student enrolments were relatively low. I taught sociology to the medical students, or at least to those medical students that turned up: I once managed to reduce a lecture audience from about 120 to 2 in the space of a few weeks. It was a thankless job. I went to Cardiff intending to stay a few years and then move on – possibly to Leicester to join Sara if that could be arranged. I gradually grew into Cardiff. Over the years the department improved. Sara was given a job in the same department, and so we settled into the city and the university. Cardiff proved an excellent microcosm for sociology. It would be presumptuous to compare our work with that of the Chicago school. But there have been remarkable transformations in the economy and social composition of the city while Sara and I have been there, and in the quality of the sociology. In the spirit of a ‘new Chicago’ we have consistently tried to make Cardiff a centre of excellence in ethnographic research methods.

I embarked on the second-most important collaboration of my career in 1977. Martyn Hammersley was (and still is) at the Open University (OU). The OU was founded to deliver degree courses by distance learning: published materials supported by BBC television and radio programmes. The OU were developing a major new course on research methods, and Martyn invited me to join the team. He and I worked together designing and writing the qualitative strand of the course. We referred to it as ethnographic methods. There were very few sources available for us to draw on, and once the course was launched it seemed a good idea to turn our joint thinking into a book (Hammersley & Atkinson, 1983). It was a good idea, although the actual writing of it was not perfectly smooth, and we delivered the manuscript seriously late. We did it for Gill Davies, a wonderful publisher, then working at Tavistock books (and later Professor of publishing at City University). She had to intervene forcefully to get us to complete the book. I am so glad she did. It was the right book at the right time. It is now in its third edition and sells as well as ever. I am proud of the fact the tens of thousands of students have used the book.

AMERICA

My next opportunity to do sustained ethnographic fieldwork came in the autumn of 1984. I had a year’s sabbatical. I decided to divide my time
between Boston and San Francisco. In Boston my gatekeeper was John Stoeckle, a remarkable physician and an energetic collaborator with social scientists. He managed my access to a hospital setting, and arranged a visiting position at Boston University. I had formed the plan to do a very focused ethnography on clinical pathology. I wanted to understand how members of a consulting specialty like pathology delivered authoritative adjudications about patient’s conditions, and how they worked with other specialists. In the event, I spent my time working with a small group of haematologists rather than pathologists, but the analytic focus remained the same. Extremely productive, it was intensive work, rather than extensive and long-term fieldwork. I remain forever indebted to John Stoeckle for making it possible. During that visit I also met Eliot Mishler and Catherine Riessman for the first time. Their work on narrative has continued to be important to me in various ways. I returned to Cardiff and followed up my Boston work with some parallel fieldwork among UK haematologists. There seems to be a pattern in my career: I take a long time to produce the monograph after the end of a project. My haematology-based book did not appear until 1995. I wanted to call it ‘Blood and Judgment’, but the publishers wanted something more descriptive and so it had to be Medical Talk and Medical Work (1995).

In 1985, I went to University of California, San Francisco. My main contact there was Virginia Olesen. I had first met Ginnie in the 1970s, having already read her classic monograph, co-authored with Elvi Whittaker, on the professional socialisation of nurses (Olesen & Whittaker, 1968). She came to Manchester to be a resident tutor on a summer school for postgrads and young researchers. As she has remained ever since, she was generous and enthusiastic. It is testimony to so many good things about Ginnie that she remained in touch with members of her tutor-group from then on. In San Francisco I did not try to do more fieldwork. What was more important to me then – and since – were the personal contacts I made and the conversations I enjoyed. It was the first time I had met Anselm Strauss, and our time together was precious. I sat in on some of his postdoctoral analysis classes, and so got some insight into Strauss’s practical version of ‘grounded theory’. It differed considerably from the formulaic versions to be found in subsequent publications (including some by Strauss himself).

I was to return to California quite soon. I had an invitation to spend six months in 1987, at the University of California, Davis, teaching the graduate class on fieldwork that Lyn and John Lofland normally taught. They had sabbatical leave. I knew the Loflands, and I liked the idea of
spending time with them. I had never taught in an American University, and I wanted to gain that experience. I gained a lot from my six months in Davis. I greatly enjoyed teaching the field methods sequence, and I got to know several of the graduate students very well. They were much more friends than students. I also led a graduate seminar, but because I had not been forewarned of this teaching duty, I was not prepared for it. I had to improvise. I had already done a bit of thinking and writing about the rhetoric of ethnographic writing, and so I hit on the idea of providing a seminar series on that topic. I had a very small but enthusiastic student group. In the event I learned a lot from their thinking. It gave me the opportunity to develop my own thinking further, and the resulting monograph appeared as *The Ethnographic Imagination* (1990). The Cardiff–Davis link bore further fruit some time later when we edited the Sage *Handbook of Ethnography* together (Atkinson, Coffey, Delamont, Lofland, & Lofland, 2001).

**CARDIFF AGAIN**

By the time I returned I found myself the Head of Department in a newly created institution, dealing with new senior managers, and coping with new financial constraints. I was Head of Department for 10 years, which was quite enough for anyone. It became apparent that if I were to provide that elusive thing called ‘academic leadership’ I needed to set an example, in particular by getting external research funding. The opportunity arose for Sara and me to do so when the Economic and Social Research Council (ESRC) funded a research initiative to study doctoral students and their supervisors in the social sciences. The Research Council was going through one of its periodic moral panics about the state of postgraduate training. Sara and I put together a proposal. We planned a multi-site qualitative study across a number of social-science disciplines. During the commissioning process, we had feedback that suggested that the ESRC would have welcomed something ‘more like *Boys in White*’. The co-ordinator of the research initiative asked us, in all innocence, if we were familiar with that study. He got a rather dusty answer. We responded: gleefully, and mischieviously worked out what a team of Hughes, Becker, Geer and Strauss would cost the ESRC for a project lasting several years. The answer was an eye-watering amount of money. Needless to say they were not persuaded. The end result was a compromise: a little fieldwork in some departments, but a greater reliance on interviews with doctoral students and
their supervisors. Having completed that project, with Odette Parry as the postdoc, the three of us followed it up with a project on doctoral students and academic staff in a series of natural-science departments (Delamont, Atkinson, & Parry, 2000).

Like many UK sociologists, I had rarely written explicitly about symbolic interactionism, even though I was greatly influenced by its best authors. Eventually, however, I had the opportunity to write about interactionism. The British Sociological Association and Sage got together to launch a book series, designed to mark British sociology at the turn of the millennium. As is often the way, the project seemed to get off the drawing board too late for any books to actually appear in 2000 or 2001. I was asked to write the volume on ‘Interactionism’. I was a bit reluctant at first. While symbolic interactionism has, of course, been an abiding strand in American sociology, it has never had a firm institutional base in the United Kingdom. It was not perfectly obvious how a book could be crafted: the materials seemed limited to say the least. In the event, I decided to take on the task. It seemed to me that a credible and interesting intellectual argument could be created. I also realised that if I was to do it justice, a younger co-author with a complementary mix of interests would enhance the book. William Housley and I agreed to tackle it together. We wanted to pursue a particular argument, which was captured in the titles of the first and last chapters: ‘We were never interactionists’ and ‘We are all interactionists now’. The first means that British sociology hardly ever included any purely interactionist authors, and interactionism never really established an institutional home in the United Kingdom. The latter means that many of the ideas that are now fashionable in social theory are entirely congruent with, and in some cases indistinguishable from, classic interactionist ideas. Clearly it does not mean that all sociologists now identify themselves as interactionists. That would be an absurd claim, although some authors seem to have accused us of being that naïve. (It is an object-lesson: never underestimate the ability of your readers to interpret something in a disappointingly literal way.) Moreover, we argued, research methods and strategies that were characteristic of the interactionist tradition have now become pervasive (under the generic label of ‘qualitative research’). The overall argument of the book (Atkinson & Housley, 2003) resembles that proposed from within American interactionism by Maines (2001).

My research activities have been driven by curiosity and personal interest. I have been an enthusiastic opera-goer since I was about 16, having benefited from a movement called Youth and Music that provided subsidised opera seats for schoolchildren. I made it my business to meet Anthony Freud, the general director of the Welsh National Opera. (He is now at Houston.) I was
extremely lucky. Anthony invited me to join them: ‘Be as intellectual about us as you like’ he said. I spent a sabbatical year, and then another year supported by money from Leverhulme, doing fieldwork in the opera company. I went on tour with them. I spent a great deal of time observing rehearsals and a few performances watching from the stage wings and observing back-stage activity. I hung out in the offices of some of the administrative staff. I got in the way in the props department. It was one of the best possible periods of field research. Some people thought it was just a lame excuse to watch opera for nothing. But it was a rare opportunity to study performances at first hand. There have been surprisingly few ethnographies of performing arts, despite the fact that many of our metaphors of everyday life are derived from the theatre (Goffman’s dramaturgical analysis being the most prominent example). It resulted in a monograph, Everyday Arias, commissioned by Mitch Allen when he had AltaMira books, but published after AltaMira had been acquired by Rowman and Littlefield (Atkinson, 2006). Mitch Allen had been at Sage and we had got to know him in the course of various American conferences. He is an outstanding commissioning editor. He and Gill Davies both taught me a great deal about the process of commissioning, writing and editing a book. We should all be blessed with such publishers.

Mitch Allen was also instrumental in two of the books I co-authored. The first, with Amanda Coffey, was based on the realisation that too many textbooks on research methods have plenty to say about the conduct of fieldwork itself, but disappointingly little to say to students about how to turn their data into analyses and into ideas. I thought it would be fun to use one data-set and to demonstrate several complementary analytic strategies in one book. We therefore wrote Making Sense of Qualitative Data (Coffey & Atkinson, 1996). Sara, Amanda and I founded a book series called ‘Cardiff Papers in Qualitative Research’, published by Ashgate. The three of us also wrote Key Themes in Qualitative Research (Atkinson, Coffey & Delamont, 2003), because we were collectively ambivalent about the increasingly strident claims for methodological novelty that were coming from various quarters – notably but not exclusively from colleagues in the United States.4

I had never lost my interest in the sociology and anthropology of medical knowledge, and a series of personal links and funding opportunities meant that I was able to develop a research group on genetics and society at Cardiff. This coincided with another of the ESRC’s major programmes: the social and economic impacts of the new genetics. The Cardiff research group thus became an ESRC Research Centre, eventually funded for a period of 10 years, and one of the Research Council’s biggest investments. I left the Centre shortly after it began its second five-year phase, but my involvement
with genetics was extremely productive. The main publications from that period included a co-authored monograph called *Risky Relations*, which synthesised some of our work on families with genetic medical conditions (Featherstone, Atkinson, Bharadwaj, & Clarke, 2005). Ever since my early fieldwork in the Edinburgh medical school, I have found medical knowledge absorbing.

My interest in opera also continues, having completed a part-time BA in opera studies during my 50s. I am now working on an analysis of master-classes for young opera singers. At the time of writing I am just beginning some fieldwork with leading craft-artists. I am working with a printer, a glass blower and a ceramicist (all in London). I hope to combine my interests in photography with this theme, by joining and studying master-classes in photographic printing. Having taken some lessons myself, I should also like to study master-classes in Argentine tango. I can go on adding people and arts. Above all, I intend to have fun. This evolving project also combines my academic work with my private passions: I have a collection of post-war British lithographs and screenprints, and the shelves in our house are crowded with ceramics. I used to be a better and keener photographer than I am now. Of course, the research also recapitulates many classic interactionist interests in work, socialisation and art worlds (cf. Becker, 1982), as well as some classic themes in anthropology. In some ways, it means a convergence with my old friend Michael Herzfeld, in terms of his work on artisans (Herzfeld, 2004).

Sometimes I feel as if, without any plans on my part, I am following the paths pioneered by people like Howard Becker and Anselm Strauss – from studies of medical students and others in university education, to studies of artists and performances (e.g. Becker, 1982; Becker, Faulkner & Kirshenblatt-Gimblett, 2006). I suppose that it cannot be perfect coincidence, but since each of my own projects has been born of a combination of opportunity, planning and chance, I cannot be accused of having deliberately aped those distinguished people (whose work I admire greatly and whose sociological longevity I admire equally). Clearly recurrent interests in knowledge, performances and socialisation are likely to draw us along similar paths. In the late 1990s, we were approached by Sage to launch a new journal. Sara and I agreed to do it. ‘Excellent idea’, we said. ‘We’ll start a journal called *Ethnography*’, we said. ‘Ah’, they said, ‘We’ve already asked Paul Willis to found a journal called *Ethnography*’. So in the end ours was called *Qualitative Research*, although that is not a term we ourselves are very keen on. The journal first appeared in 2000. Like many of these activities, it is invisible and often thankless labour.
There seems to be a de facto division of labour between ourselves and Norman Denzin’s journals, such as *Qualitative Inquiry*. While we aim to be fairly eclectic and catholic in our approach, we are not the journal of choice for practitioners of ethnographic fiction, poems, literary auto-ethnography, ethnodramas and similar forms of writing. In that sense, we have positioned ourselves as representatives of a moderate but ‘traditional’ approach to ethnography.

I have never been keen on being narrowly classified as an academic. I see no real intellectual difference between the best of anthropology and the best of sociology. (Unfortunately too many people contrive to find doctrinal disputes more engaging than getting on with real work.) Equally, I have never wanted to be a ‘sociologist of …’. A lot of my colleagues might think of me as a sociologist of medicine, but I have never been totally committed to work in that field. I have, for instance, always maintained an interest in the sociology of education; indeed, some colleagues in the United Kingdom think I am principally an unproductive sociologist of education. My new work on performance, craft and master-classes capitalises on some of those eclectic interests. I am glad that I am still planning new fieldwork. It will not be arduous, but without it there seems little point in claiming to be a sociologist or anthropologist of any stripe.

I have, of course, been best known for writing on ethnographic methods. I would much rather be known for the actual research I have conducted and published. But methods texts sell in much greater numbers, and nowadays everybody has to take research methods courses. I have no time for people who write endlessly about research methods but rarely, if ever, actually conduct real research. I do not want to see ‘methods’ fetishised. I certainly do not want to see methods taught divorced from real-world concerns of practical researchers. Recent years have, I think, seen ‘methods’ elevated to far too prominent a place in sociology and related disciplines. Indeed, some people seem to treat methods as if they constituted paradigms in their own right. I think that recent developments and debates put too much emphasis on ‘methods’, with the effect of minimising a great many other commitments in sociological thought. Equally, an obsessive preoccupation with novelty and methodological innovation has disrupted a sense of the discipline’s roots, and the continuities between generations. It has always seemed to me that the interactionist tradition’s greatest strength has been the integration of theory and method in field-based research on significant aspects of everyday life in a multitude of social settings. It is a shame when that tradition is dissipated in methodological squabbling.
REFLECTIONS

So what might one learn from reflecting on such a life? Key turning points are nearly always accidental or opportunistic. There are many possible lives not lived. We are defined as much by what we have not done as much as by what we have. If I had continued to be a classicist, and become a civil servant, I would have been a different (more respectable) person. If I had not had the unforeseen opportunity to take up the doctoral studentship at Edinburgh, then I might not have become an academic at all. If I had not blundered into fieldwork in the medical school, then I would not have been involved in the very earliest years of medical sociology in the United Kingdom. If Sara had not guided me, I would never have been productive. And so on.

It is far too easy to impose a spurious order on these things. I have been interviewed by young colleagues who wanted to write about the recent history of sociology, and who assumed a far greater coherence to it all. They wanted me to embody a rebellion in sociology and a turn to qualitative research as a conscious reaction to earlier work. They are disappointed to learn that I was almost completely ignorant of earlier sociology and there was no deliberate or rebellious move to ethnographic research. As I have said, it came ‘naturally’ to an anthropologist. Equally, as I have also suggested, my own and others’ involvement in medicine was as much happenstance as a reflection of significant intellectual concerns.

I know, of course that any autobiography like this one is a contrivance. Indeed, several of my recent papers have been devoted to debates about narratives and biographical research. I have consistently argued that we ought to be analysing life-histories and autobiographical narratives, examining their narrative forms and functions, and not celebrating them as privileged insights into private experience (e.g. Atkinson, 2009; Atkinson & Delamont, 2006b). A life, then, is not merely a selective view; it is one shaped by repertoires of cultural conventions. An autobiographical piece like this ‘ought’ to display the intersection of the personal, the public and the intellectual. It ought to show how early influences shaped later commitments. It also ought to display how personal relationships – such as inspirational teachers and collaborations – shape the life and the thought. It should probably be couched in terms that are suitably confessional and self-deprecating, reflecting a judicious mixture of modesty and self-awareness. These are all conventions of the genre. So while the autobiography is faithful to the life, it is also faithful to the conventions of the genre.
This confessional genre also calls for some regrets. What are mine? There have been plenty of missed opportunities. I wish I had done more fieldwork. It is fun and it is the bedrock of all else. I wish I could write my monographs faster. One day I would like to conduct research that is perfectly planned, perfectly executed and published without blemishes. The reality has always been a series of compromises, making the best of a bad job. One failure of nerve stands out. When I was young I conceived the idea for a project called ‘Bodywork’, which was going to be a multi-site ethnography on embodiment, from the use of prostheses, to the disciplines of dancers, to the training regimes of elite athletes and gym members. At the time there was no great interest in ‘the body’, nobody else seemed to be enthused, and I allowed myself to lose interest. If only ... I wish I had spent more time learning languages. Only in recent years have I added a lot of modern Greek and a little Italian to my rusty French and faltering German. Europe offers so many exciting opportunities for work and recreation. I certainly could have done more work in Greece. Sara and I visit when we can, but when I read scholarly and popular books about contemporary Greek life, I sometimes think ‘Could have been me’. I sometimes have similar feelings when I admire the work of a photographer. I know I could have been better at photography, but my darkroom gathers dust and is a repository for all sorts of domestic stuff. But then so many different things could have been me, and I still carry traces of unrealised ambitions.

NOTES

1. The English university system normally has three-year undergraduate degrees. One chooses a specialism from the outset.

2. In those days it was normal to proceed directly from one’s first degree to doctoral research, with no intervening Masters. Things are different now, and social scientists are normally expected to have completed a one-year masters in research methods.

3. Our collective view of Cardiff can be found in papers we prepared when the Couch–Stone symposium came to the United Kingdom for the first time. See our departmental working-paper An Open Exploratory Spirit (Delamont, Atkinson, Coffey, & Burgess, 2001).

4. A transatlantic dialogue about research methods has continued: see, for instance, Delamont, Coffey, and Atkinson (2000), Atkinson and Delamont (2006a) and see also Atkinson, Delamont, and Housley (2008).
REFERENCES


**AUTHOR BIOGRAPHY**

**Paul Atkinson** is distinguished Research Professor in the School of Social Sciences, Cardiff University. He has written or co-authored 20 books. He and Sara Delamont are the founding editors of the Sage journal *Qualitative Research*. He has a BA in social anthropology from Cambridge and a Manchester BA in opera studies, a PhD from Edinburgh and a DScEcon from Wales. He is an Academician of the Academy of Social Sciences. He has recently been a visiting professor at the University of Vienna. His most recent books include *Making Conditions* (co-authored with Katie Featherstone), *Everyday Arias: An Operatic Ethnography* and *Contours of Culture: Ethnography for Complexity* (co-authored with Sara Delamont and William Housley).
REFLECTIONS ON A
SOCIOLOGICAL JOURNEY

Kathy Charmaz

ABSTRACT

My story is one of becoming a sociologist by accident. Throughout this story, I emphasize the turning points I took and those thrust upon me during my sociological journey. The turning points in my path to becoming a sociologist began during my childhood, although I could not have foreseen them. Both of my parents had experienced downward social mobility yet had managed to receive university degrees despite enduring hardships and living through the Great Depression. Gender roles circumscribed opportunities for girls who grew up in the 1950s. Thus, I entered a women’s profession, occupational therapy. After a few years, my interest in teaching occupational therapy students led me back to graduate school to seek a master’s degree in sociology at San Francisco State College. I gained a new worldview, although I soon learned that quantification and esoteric theorizing, not social issues, commanded the attention of most mid-1960s sociologists. I also learned that women sociologists had scant opportunities despite their qualifications but decided to seek a doctoral degree in sociology anyway. Eventually the University of California, San Francisco, accepted me in their first cohort of doctoral students and subsequently I worked closely with Ansem...
Strauss, my dissertation chair, and Barney Glaser, from whom I learned grounded theory. After finishing my dissertation, a temporary appointment at Sonoma State College turned into a tenure-track appointment. Although many years at Sonoma have been tumultuous, I have also had the privilege of developing a writing program to assist the faculty in their research and writing.

EMERGING FROM THE SHADOWS OF A FAMILY PAST

I became a sociologist by accident. The turning points in my path to becoming a sociologist began during my childhood, although I could not have foreseen them. As a child, history and culture fascinated me. To my parents’ chagrin, I had dreamt of becoming an archeologist who discovered buried Egyptian pyramids. Not only would becoming an archeologist take far too long, but also my parents insisted that girls should not pursue such an inappropriate career goal. I grew up during the 1950s when girls could enter respectable women’s occupations before marriage but not “men’s” professions.

My parents did, however, insist that their two daughters obtain a college education. We must have “something to fall back on” just in case we needed a job. I grew up with parents who talked about, “When you go to the University of Wisconsin …,” not about “when you go to college.” They were both Wisconsin graduates during the Depression and both came from families that had experienced economic reversals before the 1929 stock market crash. My paternal grandmother had been widowed when my father was 2. Although she had been a lady trained to sing opera and serve tea, my grandmother went out to work as a personnel manager at Wrigley’s Gum in Chicago. Unfortunately, a train accident amputated her leg and paralyzed her right arm, leaving her unable to work and dependent on her three boys to support the family with odd jobs. She encouraged her sons to seek an education and later my uncles both graduated from the University of Chicago, in keeping with family traditions that had preceded their poverty.

Meanwhile, my maternal grandmother had married the spoiled son of her town’s leading family. Not many years later, she tired of his philandering and divorced him, an unseemly act for a woman of her station who had three small children. Having a big house but few skills, she took in boarders and soon married one. Before their marriage, he had studied law at the local
university but dropped out of school afterward to support his new family. His business flourished for a time but folded almost a decade before the 1929 stock market crash. My mother never failed to remind her children and grandchildren that her entire college wardrobe consisted of three dresses and that a custodial job and Masonic scholarship paid for my father’s education, despite his membership in a prestigious fraternity.

Coming from downwardly mobile disrupted families and living through the Great Depression marked my parents’ lives and affected mine. I learned early to see beyond the immediate situation. My experiences had given me an understanding of the economic precariousness of individuals and within American society. My father avoided potential risks in the private sector in favor of a stable but poorly paid government position. Thus, he had turned down promising jobs in the late 1940s, such as the vice presidency of the rising US Gypsum Company (which manufactured wallboard for the booming post WWII housing industry).

The McCarthy era intensified my parents’ fear of risk. McCarthy named my father and uncle as communists simply because their deceased older brother’s progressive politics had raised his suspicions. This uncle had served as a convention director, campaign manager, and speechwriter for the Progressive Party’s 1948 presidential candidate, Henry Wallace. He died of a sudden heart attack the night before their convention opened. My father and surviving uncle – communists? Hardly. I used to accuse my uncle of being a “card-carrying” Republican because of his staunch devotion to all things the Republican Party represented. In addition, my uncle and his family enjoyed an affluent lifestyle in a gated community with stringent selection criteria (only white, affluent Protestant and maybe a few wealthy Catholic families need apply) and an armed guard. That was decades before most people knew what gated communities were and nobody except gangsters had armed guards.

Neither uncle’s political views persuaded my father. Instead he had retained the dreams of his youth as a Roosevelt Democrat who avowed that the New Deal had saved the United States from certain disaster. My parents’ difficult lives stood in sharp contrast to that of my beloved uncle with whom I occasionally sparred. This contrast fueled my developing sociological consciousness although I knew nothing about sociology and discussions about intersections between race, gender, and class had neither emerged in the discipline nor in public discourse. In a brash moment in the late 1950s, I once rebutted my uncle’s claims to being a self-made man who had pulled himself up by his bootstraps. I retorted, “You are white, male, Protestant, and a University of Chicago graduate. Few people have all those advantages.”
My father’s frequent job transfers took us far afield from our Midwestern roots to the alien world of the Northeast. With each move, class positions and differences became more apparent. We lived wherever my parents could find affordable housing, once in the rectory of a self-righteous young minister who wanted the unpaid work of a wife (i.e., my mother, who loathed him) without the bother of having one, next in a tenant farmhouse. All the windows had been broken to make the house a chicken coop until my father transformed it into an attractive home. The house resided on a revolutionary general’s decaying estate. We stayed in 3 huge rooms of the 19-room mansion while my father toiled to make the tenant house livable. The owners of the horse farm next to us were good neighbors by day and frightening by night. I saw men on horseback wearing the white robes and masked hoods of the Ku Klux Klan while they thundered around the farm with glowing torches. The estate across the road belonged to the owners of a popular soft drink company. When their daughter married, my sister and I and our two foster sisters climbed up the pillars of our estate to see and wave to the endless procession of opulent vehicles entering theirs. Soon an official came over to order my mother to remove the urchins from the pillars because, to our delight, we had made a bad impression on the guests.

During all the moves, my sister and I attended nearby public schools, no matter how impoverished the district or poor the schooling. I began first grade in a two-room schoolhouse with a harried teacher who taught three grades simultaneously. My parents’ belief in public education and in our abilities shaped my schooling and lack of it. They eschewed their friends’ occasional urgings to send us away to private schools and took little note of the inferior education that we received. Besides, my father’s professional and financial concerns dominated family conversations until superseded by his failing health. As a soil conservationist, he made pessimistic projections such as looming mass starvation by 1984 because of poor land-use. When my mother had to return to teaching, their decision for her to work at my high school exemplified their naïve approach to our education.

Except for spending second grade in an upper-middle-class town, every school that I attended had few good teachers, scarce resources, and served to contain wayward children of the working class. Our courses may have lacked substance, but the high school counselor’s screeching refrain, “Work to the level of your ability or you'll end up at the mill,” caught my attention. I expected to leave our mill-town but had no desire to attend the local state teachers’ college and become a physical education teacher, the pinnacle of success at my high school. Lessons that I would enter a hard world with few opportunities did not escape me.
Ironically, I did have the opportunity to attend the University of Wisconsin without paying out-of-state tuition through my father's connections. I felt simultaneously reluctant and ill-prepared to pursue admission there. An opportunity emanating from privilege rather than achievement also concerned me. To my parents' relief, I had relinquished earlier hopes of becoming an archaeologist in favor of the "suitable" goal of becoming an occupational therapist. Wisconsin had more prerequisites in science for occupational therapy students than other universities and required taking physics at the college level. Physics? That ruled me out. I had weathered biology and chemistry from our bumbling high school science teacher but fled from physics to home economics. The risk of flunking out of college haunted me because most college students entered with a substantially stronger foundation than I had.

Undergraduate life excited but exhausted me. During my freshman year, mononucleosis prevented me from finishing several courses. By the time I was a junior, I had transferred to the University of Kansas to be closer to my parents' new home in Nebraska. A week before Thanksgiving break, I caught a bad cold that escalated into a deadly double pneumonia. The doctor called my parents to come quickly against my expressed wishes. Their ashen faces informed me of the gravity of my condition. Pneumonia gave me my second brush with death; the first had occurred at age six when I hemorrhaged after a tonsillectomy. I happened to live because my mother had stayed at my bedside after visiting hours and screamed for help. These episodes and my father and uncles' pervasive heart disease made me aware of the fragility of life. I began to think about doing something with my life beyond marriage and occupational therapy. But what?

Thinking that I might want to become a clinical psychologist, I took courses in psychology. Later, a seven-unit anatomy class consumed my interest. Of the five students assigned to our cadaver, only one physical therapy student and I conducted the exacting dissections. After working all day in the lab, we reeked of preservatives but inhaled a whiff of success. My instructor admired our work and invited me to become an anatomy graduate student. By this time I believed that I could surmount my weak background in math and science but questioned what a PhD in anatomy might mean. Our marvelous anatomy lab assistant planned to leave the PhD program in favor of medical school. He had discovered that a medical degree afforded him better access to jobs, research, and salaries than a PhD could. In 1960, medical school admission in parts of the Midwest and South...
was not as competitive as it soon became. PhD students, however, usually entered their programs with stronger qualifications. My thoughts turned to medical school—until I worked as a student occupational therapist and witnessed the treatment of the few women interns and residents. Their peers and attending physicians ridiculed and vilified them with degrading sexual slurs and constant gibes about their competence. Those observations left a lasting impression. At the same time, my father overturned age norms. Observing my father’s return to school taught me to look beyond traditional expectations. Too sick to work, too bored to stay home, he entered law school and fulfilled his boyhood dream of becoming an attorney that poverty had foreclosed.

**BECOMING AND BEING A GRADUATE STUDENT**

I never pursued medicine but I still saw clinical psychology as an inviting possibility. After receiving my BS in occupational therapy in 1962, I visited a department chair of an excellent clinical psychology program. He seemed pleasant enough until he learned that like his niece I had a degree in occupational therapy. Then he exploded in rage. He screamed that occupational therapy was a “perfectly good profession for a girl until she got married” and told me that I was pretty enough to get a husband, and ordered me out of his office forthwith. I left never to return to clinical psychology. I soon took a job in rehabilitation at the University of Washington Hospital and subsequently moved on to San Francisco to be near my fiancé.

This engagement ended but not my interest in graduate school. After two years of working as an occupational therapist in physical medicine, teaching occupational therapy students appealed to me. I aimed to impart a broader worldview than I had found in the health field. Mexican migrant workers in Colorado, impoverished African Americans in Dallas, TX, disabled veterans in Memphis, TN, and privileged patients at the Menninger Foundation in Topeka, KS, had taught me that patients usually lived in worlds far removed from those of the health professionals who treated them.

In the 1960s, a master’s degree in any related field sufficed for teaching occupational therapy and sociology appeared to address social issues that concerned me. But would San Francisco State College (SFSC) accept me? Did they accept women? Could I become a sociology graduate student without an undergraduate degree in the field? A meeting with Graduate Advisor Fred Thalheimer, who became my mentor, advocate, and dear friend, answered my
questions and bolstered my confidence. He reassured me that I could enter with a “conditional” acceptance, contingent on completing the core undergraduate courses. Yet some gender biases persisted.

When I inquired about teaching assistantships, Fred said that the department liked to give them to the male students because their wives had to work. With a teaching assistantship, the men contributed to the household income. Most of the women, however, were single and self-supporting. To my lasting gratitude, a small traineeship for occupational therapists supported two years of my master’s program. Many women faced obstacles in finding financial support for graduate education and doors easily closed on women like me.

With a scant five units of undergraduate sociology, I then embarked in 1965 on my journey through graduate education at SFSC. I swiftly discovered that quantification and esoteric theorizing, not first-hand involvement with social issues, commanded the attention of most mid-1960s sociologists. Nonetheless, the master’s program gave me a new worldview. Our social science epistemology course led me to read such luminaries as P. W. Bridgman and Thomas Kuhn. I came to articulate a relativist view of objectivity, truth, and science. I began to identify with sociology as my home discipline and major pursuit although I did not identify myself as a symbolic interactionist.

Completing the master’s degree meant numerous hurdles. The three-unit epistemology class required three 20-page papers, each with an annotated bibliography. Many students never finished the course or their degrees. The comprehensive master’s exams stretched for 12 hours over 2 days. Only 2 students of about 20 passed all their exams on the first round and we only had one second chance. During a graduate students’ meeting with Arlene Daniels, she disclosed the tip I needed. She said that one student had sharply criticized Talcott Parsons and thus offended the faculty. Our exams had occurred a few years before graduate students engaged in ritual criticism of Parsons. On the next round, I passed the exams without difficulty, but the young woman with whom I had studied did not. Of the 8–10 women who entered the program about the same time, only 3 of us finished. Most became discouraged and dropped out before the exams and a few never finished the lengthy master’s thesis that followed the exams.

The sociology courses and our interdisciplinary epistemology course sparked my imagination and led me to seek a doctoral degree in 1968. Biographical time met cultural time with this transition, which I realized might not be possible later. I was young, single, and healthy, but cultural norms did not support women who pursued advanced professional careers.
Completing a master’s gave me a bit of panache; pursuing a doctorate brought me mixed messages of admiration and disapproval.

In 1968, the popularity of sociology resulted in stiff competition for admission to graduate departments with star faculties. Women had just begun to enter the discipline in sizeable numbers although sociology continued to be a male bailiwick. Still, the men in my master’s program accepted me as a good friend and colleague. I saw myself in Bales’ (1950) terms as the socio-emotional leader of the tribe of graduate students. Patrick Biernacki’s loyal friendship and intellectual companionship sustained me until his untimely death in 1994 at 52. Fred Thalheimer had forewarned me that I might never get a job despite plentiful positions, which typically went to white men. I decided to risk having spotty employment later even though I had witnessed the struggles of well-qualified women who scraped together part-time teaching and research jobs.

At the time I applied for doctoral programs, the future of sociology looked bright. *Newsweek* magazine had proclaimed that sociologists would become the sages of society. With our wider view, we would replace psychiatrists as the High Priests of Reason, interpreters of the human condition. The Free Speech Movement, Civil Rights Movement, and Anti-War Movement all made critique of American society a compelling pursuit. The Women’s Liberation Movement had fought for women to enter men’s jobs and professions. Subsequently, 1968 marked a major movement of women into fields that white men had dominated, including sociology. The academy was expanding; sociology was immensely popular; and jobs were plentiful – for white men (Charmaz, 2008). Meanwhile, students attempted to survive the competition for admission to high-ranking doctoral programs in sociology.

Fortunately, I knew enough to apply to programs in which I had a genuine interest rather than those for which I only had network links. A graduate of a highly respected quantitative program had affirmed that his recommendation and connections would get me into this program with more funding than I could imagine. I declined to go forward with an application because the program would have been a poor choice for me. Political sociology intrigued me and I did receive a fabulous fellowship to pursue doctoral studies at another university with an excellent program in this area. Yet the works of Strauss (1959/1969, 1961; Strauss, Schatzman, Ehrlich, Bucher, & Sabshin, 1963; Glaser & Strauss, 1965, 1967), Goffman (1959, 1961, 1963), Fred Davis (1963), Roth (1963), and Scott (1969) inspired me to become a qualitative sociologist who specialized in social psychology, sociological theory, health, illness, and disability.
Despite the reigning gender bias in academia, every school to which I applied except the one I later attended. To my lasting good fortune, the faculty in the Graduate Program in Sociology at the University of California, San Francisco (UCSF) eventually accepted my application. We students heard that they had had around 170 applications for 7 student positions in the 1968 inaugural year of their graduate program. My SFSC friends, Patrick Biernacki and Richard Rizzo, had already received their acceptance letters from UCSF. When the selection committee first declined my application, I committed an uncharacteristic assertive act, which became a turning point. With the encouragement of one of my advisors, I pushed my shyness aside and called Fred Davis at UCSF to inquire whether any chance of being accepted was still possible. He said that he would see what he could do. Some weeks later, a student withdrew and I received an acceptance letter.

The considerable expertise of the UCSF faculty fit my budding interests perfectly, far better than a well-funded department whose graduate advisor had said, “Sure, you can come here but I don’t know why you’d want to.” He explained that I would be on my own because their department had no one with expertise in qualitative research. The doctoral program at UCSF surpassed my hopes about what graduate school could be. Although UCSF offered little campus life, our cohort created a supportive and stimulating colleagueship and we had an amazing faculty to work within small classes and tutorials.

The structure of the UCSF doctoral program reflected progressive ideas in graduate education. The faculty had created an innovative program with close mentoring, relatively egalitarian relationships, and brilliant teaching. Unlike other programs of the day that emphasized heavy coursework in received theory and substantive areas, qualitative research formed the core of the UCSF program. As the department chair, senior scholar, and creator of the program, Anselm Strauss’s influence permeated all aspects of students’ doctoral education during the early years of the program.

In our first cohort meeting with Anselm, he told us that course grades would not pose problems for us; we would all get A’s. Sponsorship on the job market and in the discipline remained quite another matter. Who the faculty sponsored would reflect how they ranked us as students. Anselm taught us a little about disciplinary norms that day and a great deal about departmental expectations. He delivered a clear message – although what students hear and remember may differ. Anselm expected us to write books – many books. We would gain a rich life from writing books but would receive a poor financial return for our work. Anselm may have dampened any hopes of financial
largess but he assured us that our ideas would establish strong academic reputations and ensure that we could obtain good jobs.

This first meeting foretold the kind of mentoring that Anselm would give me. He engaged each one of us about our interests. His tone reflected his curiosity, warmth, genuine interest, and obvious excitement. Anselm seemed as eager to meet us as we were to meet him. “What are your interests?” “What are you working on?” – he asked when he engaged each one of us. To my surprise, his list of questions grew longer as he talked with me. It surprised me because my SFSC professors had typically evinced more interest in the male graduate students and two of my most creative friends, Patrick Biernacki and Richard Rizzo, were sitting right there. What came across so strongly in that meeting was that our ideas intrigued Anselm. He gravitated toward ideas and their possibilities and then he looked at them this way and that. The conversation that Anselm and I began that fall day in 1968 continued until the night before he died in 1996.

At the end of our first seminar, Anselm said that we had worked hard, so we could choose whether or not to write a paper. Of the seven of us, I was the only one who wrote a paper. A couple of years later, one secretary told me that Anselm had tested us to see who took the initiative. Initiative? Not exactly. I sought his feedback. I recall thinking of course I’m going to write the paper; it’s an opportunity to receive feedback from Anselm Strauss in an area that interested both of us. The paper used my earlier ethnographic research and focused on how institutionalized patients experienced time.

I had come to UCSF with six months of ethnographic research that involved living in a residential medical institution that served impoverished elders and people with disabilities. This experience transformed my thinking and imprinted images of a reality on my consciousness far removed from academe. It left me with some ideas and insights and poor data collection skills. I had written about the research in the first seminar paper. Anselm’s critique consisted of a few comments on the first page: “You have some gems here. Develop them.” Encouragement and freedom distinguished his mentoring. He encouraged us to move forward and gave us the freedom to do it in our own way. Students who needed structure might not have appreciated the freedom Anselm gave us and indeed a number of students drifted away during the early years. But encouragement and freedom was precisely what I needed. I developed the paper myself. Actually I did not follow Anselm’s advice. He insisted that I had three articles in the making. I said, “No.” The three parts had to be integrated and that’s what I tried to do. After the paper appeared in Social Problems, Anselm said, “I didn’t think you could make it work, but you did.”
Another telling incident concerned the challenge of entering emotionally stressful empirical worlds. One day when I went into the sociology office, the receptionist announced, “Well, you passed the test.” I asked, “What test?” “Accepting the job. That was a test.” She disclosed that everyone with whom Anselm chose to work closely had to be involved with one of his many projects in death and dying. For this research project, Anselm had asked me if I would conduct interviews with caregivers of dying or deceased working-class elders. Of course I would.

Our faculty observed us closely and treated us generously. They were finding their way with us as we were finding our way with them. They had some preconceived notions about who we should be and become and what we should be doing – but they did not always share the same notions. And we had preconceived and varied notions about graduate education. Four of us had entered the program with master’s degrees in sociology. Anselm and Barney questioned whether our exposure to the discipline had been too extensive and thus made us more resistant to learning grounded theory than less advanced students. Simultaneously, we saw our disciplinary backgrounds as a strength that prompted us to raise incisive questions and sophisticated issues.

In those days, a considerable amount of our instruction derived from tutorials. Anselm served as my dissertation chair, but I learned grounded theory analysis from Barney Glaser. I took classes from Barney and conversed with Anselm about each step of my work. As it turned out, I am one of the few graduates of the UCSF program whose work shows the discernible influence of both Anselm and Barney. Anselm’s theoretical and substantive interests, Chicago School allegiances, and worldview all resonated with me. I also learned much from Barney’s analytic skills and inspired teaching of grounded theory. However, my earlier foundation in epistemology made me uneasy with both the unexamined assumptions and the rush to categorization that I sensed in Barney’s formulation of grounded theory.

During my coursework, Anselm and Barney worked as an analytic team that scrutinized my research and involved me in analytic discussions about it. They usually read and responded to my work within the same day. This intensive involvement spurred my intellectual development early in my doctoral program. As a result, I gained excellent tools to analyze my dissertation data before beginning to collect materials. Barney’s ability to come up with an analytic framework often left students awestruck. Some of them waited for him to do the analytic groundwork for them. I intended to write the dissertation myself, so I had much of it completed before showing them any chapters. Working independently symbolized reaching an important identity goal to me.
The story above about writing my first article speaks to the kind of experience my cohort enjoyed in graduate school. What I gained as a graduate student has informed my teaching style and substance throughout my career. The basic foundation I gained at SFSC had provided me with a broad understanding of the discipline and a solid knowledge of its major works. The specialized training I received at UCSF gave me skills that fostered developing interesting courses and allowed me to conduct limited research as well as to teach. I have taught courses in my areas of formal training and in areas that I have developed since graduate school.

All the professors in my doctoral program offered exciting courses and modeled outstanding teaching. By the time I finished coursework, I had had six quarters of qualitative analysis, a rare amount of training now, an unheard of amount then. Although I worked most closely with Anselm and Barney, I had the good fortune to take courses from the other faculty members including field research from Leonard Schatzman, epistemology from Fred Davis, professional socialization from Virginia Olesen, and multiple tutorials in classical theory from Walter Klink. Virginia Olesen's meticulous scholarship made a powerful counterpoint to Barney Glaser's casual dismissals of extant literatures. Walter Klink's tutorials in classical theory led to a job later when sociologists had become surplus labor. Leonard Schatzman's and Fred Davis's masterful prose inspired us to hone our writing skills.

After completing my degree in 1973, a commitment to stay in Northern California shrunk my job possibilities in sociology and eliminated them in occupational therapy. Governor Ronald Reagan's war against higher education had resulted in hiring freezes that few sociology departments could circumvent. At the time I entered the California job market, only a handful of prospective tenure-track positions had survived Reagan's attack. The private institutions also had scant openings. My one-sentence rejection letter from a well-known symbolic interactionist at a University of California (UC) campus read: “We have selected our short-list of candidates; your name was not among them.” A gracious rejection letter came from another committee chair at another UC campus who admired my scholarship. He apologized for making the unusual request of asking to keep the copies of my articles and papers for his personal files. I later learned that the woman who got the job had strong personal and professional connections with key departmental faculty. I fared better at a small liberal arts college where I had my only interview for a funded tenure-track position. However, the students warned me that accepting a position there would mean a perilous path ahead. The newest member of the department,
who did not expect to get tenure, underscored their warning. He began our meeting with an ominous pronouncement, “Don’t come here,” and proceeded to utter an impassioned plea that I decline an offer from the department because I could never get tenure there. Ironically enough, he stayed at the college, received tenure, and later married the young woman with an Ivy League degree who got the job.

The few temporary positions available in the California State University (CSU) looked even less hopeful. Anselm called several department chairs in my behalf. After each call, he told me that I would be hearing from the department chair. No follow-up calls occurred. A friend in one of these departments offered her own view. She said, “You know where your application will go, don’t you?” I didn’t know. “The round basket. Of course, X is delighted to chat with Anselm Strauss for half an hour but he’s never going to let an application from a qualitative candidate get anywhere.” Later, I received an offer for a one-year temporary position at a state university a couple hundred miles away. We decided the distance was too great to commute for a position that had no chance of continuing. Another department chair for an interdisciplinary program offered me a one-year full-time temporary position with assurances that it would become permanent. The written offer, however, stipulated that the job would consist of two courses, which was less than half-time, for one term.

Shortly afterward, I accepted a temporary position in sociology to teach classical theory at Sonoma State College, a small liberal arts institution in the CSU system, and eventually received a tenure-track appointment there. This turn marked the beginning of a long saga of weathering conflicts, cutbacks, and avoiding termination and layoff (see Charmaz, 2008).

**SURVIVING AT SONOMA**

Sonoma State’s numerous institutional troubles multiplied during my first two decades of teaching. In the 1970s, definitions of fair tenure requirements, “good” teaching, and the place of research sharply divided Sonoma State’s faculty and departments. Faculty throughout the college viewed research with profound ambivalence although most possessed first-rate degrees from excellent US universities. Were faculty to be teachers only? Were we to be teachers and scholars? If so, how could we handle both roles when we had such high teaching and service workloads and little support for research?

Several departments, including mine, had ambiguous policies about tenure and promotion requirements and/or applied them inconsistently.
A member of another department said that politics, personality, and cronyism constituted the real criteria for tenure or promotion at Sonoma (Charmaz, 2008). Our department’s overt battles raged over its hiring, retention, tenure, and promotion policies and practices, particularly whether it required completion of the PhD and publications. Covert battles also ensued about departmental loyalties, course content, and teaching styles. Some faculty throughout the college valued a curriculum based on group interaction and self-discovery, while others seethed with resentment over the erosion of academic standards and the college’s Granola University image. Our department had faculty representing both camps.

I had entered a department with deep divisions and found myself on the less powerful side. Despite some decided conflicts between professors in my doctoral program, they had created a cordial, collegial environment for students that contrasted with numerous contentious sociology departments in the 1970s. As a consequence, I arrived at Sonoma State unprepared for the departmental and institutional politics. Our department chair thought my research background and interests made me a poor fit for a teaching institution. Simultaneously, the only full professor in our department bemoaned the lack of research-oriented faculty among us. Yet the belief prevailed at Sonoma State that teaching and research involved mutually exclusive commitments. Simultaneously, many faculty throughout the college believed that they needed a few publications to be promoted. How can a new assistant professor navigate contradictory expectations inhering in these beliefs?

The department chair gave me a series of edicts that I had to meet such as moving to Sonoma County, excelling in teaching, attracting large numbers of students, and fulfilling multiple heavy service requirements. All such edicts occurred in the privacy of his office with no one else present and never showed up in writing. But their significance was not lost on me. The chair issued warnings that I had to do more than other departmental members because half of our faculty needed time to work on their unfinished dissertations. He also arranged for me to serve on the most volatile institutional committees to meet service requirements.

By my second year in a bona fide tenure-track position at Sonoma, I had 20 hours a week scheduled as the academic advisor to 444 majors and chaired the College Services Council that acted on the raging issue of the year: whether or not the campus security officers should be armed. Dealing with this issue schooled me in campus politics. Strong proponents of each position argued their case at the Council meeting. I was adamantly against arming the security officers and expressed my view but voiced it respectfully.
and conducted the tense meeting smoothly. A biologist aligned with my department chair had argued passionately for arming the officers but commended me on how well I conducted the meeting. My subsequent private meeting about this issue with the vice president, however, took a different turn. He ended up screaming and shaking with anger. Yet when he talked about our meeting at the Academic Senate, he presented it as though it had been the most rational, cordial discussion, “Kathy and I discussed this issue at great length … .” Impression management matters even when emotional control does not.

By 1975, it had become unclear whether I would be retained and could receive tenure at Sonoma. The full professor who admired my scholarship had been away when I received an unenthusiastic departmental evaluation. Fortunately, teaching had gone well for me although I taught demanding courses. If any department at SSU wanted to get rid of a faculty member, poor teaching evaluations provided a ready justification. At the time, many sociology students expected to work and to engage with the course materials. I formed a tacit agreement with students that served me well for two decades: if I worked hard on my courses then they would, too – or at least not condemn me for having high standards. The department also had sufficiently large numbers of students and classes that students who did not wish to meet my requirements could take other courses.

The faculty union advisor told me that I had to exercise my rights to know the specifics for the general criticism wielded in my evaluation and to meet with the all-campus committee about my situation. When I followed his advice, one senior member of the department told me, “Kathy that’s exactly what we don’t like about you.” My job was in jeopardy. Yet my situation seemed less grave than other past and present faculty members because I did have a PhD. One week before my appointment with the department committee, however, another colleague felt devastated by what ensued while meeting with this committee. That jarred me.

An unforeseen consequence of the earlier pressures I had had to participate in campus governance helped me to get through this daunting situation. Many among the faculty leadership knew me – and they liked me. To this day, I am grateful to the philosopher who spent three hours talking with me the day before my meeting to bolster my confidence.

The meeting proceeded with three committee members raising confrontational questions while three others remained silent. When several jumped in simultaneously and tensions mounted, I acknowledged each member and tried to take their questions separately. When one member implied that the tenured members all disliked me, our full professor blinked in surprise and
said, “I think she’s charming.” After listening to my responses for a time, the department chair said to the committee, “Let’s just let go of this and forget about it.” And so he did. Two members evinced displeasure about his proposal but did not argue further. A personal meeting with the college president concluded these events after she asked, “Kathy, do you really want to teach?”

My situation in the department improved but that of Sonoma State deteriorated, despite its new status as a university. The department had granted me tenure and promotion to associate professor in 1977. I may have grown in stature, but the department shrank in size. In the 1980s, Sonoma State downsized as we lost students and subsequently the university laid-off tenured faculty. The then president, Peter Diamandopoulous (whose later battles with the faculty at Adelphi University brought him national infamy), aimed to reduce what he called the social disciplines. The 12 permanent positions in the Sociology Department shrunk to 3.75. Seniority meant everything during the 1980s. My low departmental seniority precluded teaching a funded course in sociology even though the department developed clever strategies to help our situation. Senior members of the department taught in other programs to give more junior people an opportunity to teach and thus remain employed. For seven years, the University sent me a layoff notice each semester but each time sent it to the wrong address. I never retrieved these notices from the post office.

During those years, I held two-thirds of a temporary position in gerontology and applied to teach courses in other programs. Subsequently, I taught courses in nursing research, medical ethics, English composition, and critical analysis in speaking and writing. I loved teaching the writing courses and seeing students grow over the course of a semester. My workload, however, mushroomed far beyond the faculty union contract. Two-thirds of a position in Gerontology did not cover what it took to run the program and to teach the courses. I had wonderful, caring, committed students in the Gerontology Program who were a delight to teach. Their commitment to serving the elderly inspired me while I continued without success to argue for adequate funding for the program.

The funding for the Gerontology Program improved as I left it. An administrator had told me for years that they could find no money to augment its funding. But the prospect of advertising the position when I went on sabbatical changed his mind. By that time, he did not believe it looked good for the University to publicize asking so much of a faculty member for a part-time job. At his urging, I left the Gerontology Program after my sabbatical leave and returned to the Sociology Department, and have remained there. The
elaborate contract we wrote to define my continued participation in Gerontolgy disintegrated after one year, another victim of budget cuts.

The earlier contentiousness in the Sociology Department rapidly faded in the 1990s but University-wide crises did not. Talk of layoff arose again and faculty morale plummeted. New faculty members felt threatened. Old faculty members blanched when a senior administrator mentioned that he preferred to protect new faculty over old faculty (a clear violation of the union contract). I decided to do something positive for new faculty and formed a writing group to encourage their scholarly research and writing. For some members, the group became an anchor in a sea of gloom. I witnessed the positive effects of group membership and offered to expand the program in 1996, if the administration funded me to do it. They agreed to one course release for two separate bi-weekly faculty seminars, which soon expanded to three. For 15 years, I have directed the Faculty Writing Program, albeit with many obstacles in sustaining its funding.

This program brings faculty together from all over the university in working seminars to offer constructive critiques to advance each participant’s writing. I provide lengthy critiques and offer an editor’s view of the work and the writing. In addition, the participants each give authors a fresh view of their work and ideas for improving it. The Faculty Writing Program aids its members in remaining active in their fields and in engaging in vibrant multidisciplinary discussions about research and teaching. It has been an honor to work with Sonoma State faculty in this capacity and a joy to contribute to their success as teacher-scholars.

**BECOMING A PROFESSIONAL**

What did being a professional sociologist mean? At Sonoma, it meant having a PhD. But academics elsewhere contested this definition. I recall one prominent feminist sociologist proclaiming that only those who taught doctoral students could call themselves professional sociologists. The elitism in her claim gave me pause although it did not surprise me. And I saw myself as having a job, not a career.

By 1975, I had abandoned my hopes to conduct research and thought I should narrow my focus to teaching and service. Anselm may have sensed this shift. He invited me to lunch and told me that I wasn’t publishing enough. Enough? I wasn’t publishing anything. That changed when Lyn Lofland asked me to pull together several earlier memos into a paper for
submission to a special issue of *Urban Life*. The memos detailed how coroner’s deputies handled notifying family members of a relative’s death. Shortly thereafter, a young textbook salesman from Addison-Wesley visited my office late one afternoon. We talked about my areas of expertise and he said that his company would be interested in a new textbook in either aging or death and dying. I thought about it and decided to submit a proposal for textbook on death and dying. I realized that writing one in the field of aging would give me more recognition but the 1960s and 1970s literature on aging bored me. So I plunged into the literature on death and dying and eventually produced a sociological treatise of it in 1980.

From 1975 through the next 20 years or so, I benefited from mentoring that had an unusual twist. By 1980, no one in my department still maintained strong links to the discipline, but several of my friends did. Lyn Lofland became my most significant mentor about the discipline and surviving as an academic. Lyn may have been only two years ahead of me in earning the PhD at UCSF but stood decades beyond in knowing the unstated norms of the discipline. I did not always follow her advice, but I always listened.

My research and writing slowed during the layoff years, but I attended meetings and presented bits and pieces of my book project on experiencing chronic illness. One time a sociologist who worked for the federal government asked me how she could help me finish my work. Her question startled me because at Sonoma State most social scientists worked on their research in isolation, if they pursued it at all.

What induced me to keep writing, however slowly it went? What made a difference? I felt a responsibility to my research participants to finish the book. Unlike so many colleagues in teaching institutions, I continued to believe that I had something to say if I could find a way and the time to say it. Having collegial support makes an enormous difference when you work in a setting in which no one else shares your perspective. I received encouragement from friends and colleagues like Lyn, Anselm, and Patrick as well as from members of a Bay Area writing group that included Adele Clarke, Anna Hazan, and Leigh Star. They each read early drafts of papers and chapters and gave me comments that sparked my thinking.

My story also turns on what I gained from belonging to the Society for the Study of Symbolic Interaction (SSSI). How did SSSI affect my professional development? In countless ways, this society offered me friendship and fellowship over the years. Carl Couch and Norm Denzin each welcomed me into the inner-workings of SSSI 30 years ago. The Old Boys network of its early years had made the society a chilly place for women. Women had often
attended meetings but seldom served in important SSSI offices. In my new position as Publications Committee Chair, I first attended a SSSI Executive Council meeting in Carl’s smoke-filled hotel room in 1981. When I walked in, a stunned silence followed. Carl greeted me by name, engaged me in conversation, and acted like nothing unusual had happened. But the other men did not follow Carl’s gracious lead. From that point on, Carl became an important anchor in SSSI for me.

The strength of Anselm’s bonds with fellow Chicago school graduates showed me the importance of having an intellectual home. I realized at an early age that membership in SSSI meant much more than receiving a journal and a newsletter. SSSI was a community. A loose community? Yes, loose and changing, but a community nonetheless. Thus, a world of connections and attachments opened to me where enriching interactions occurred. This community gave me the intellectual home that a college or university could not. SSSI remained small enough to allow becoming acquainted with numerous members. It became large enough to meet other people with similar substantive, theoretical, and methodological interests. At first glance, symbolic interactionists may seem to have obvious points of convergence. Yet we are and have become quite diverse.

Faculty at teaching institutions often view organizational participation as more relevant for those in research universities. However, such participation may be more important for us although for different reasons. We may not be given career points at our institutions for attending meetings, presenting papers, or serving scholarly societies. Yet such involvements break our isolation and give us purposes beyond our immediate situations. Our intellectual horizons stretch and our personal and professional rewards expand. These involvements give us a chance to think and grow in ways that may not exist in our everyday worlds.

CONCLUDING REFLECTIONS

My sociological journey brought me to destinations where writing and research assumed major importance in my life. Neither the SSU Faculty Writing Program nor my research would have materialized had not colleagues elsewhere, particularly members of SSSI, encouraged me to keep thinking and writing. Not only does the relentless workload at Sonoma State undermine a commitment to scholarship but also the University’s constant budget crises fuel the ambivalent stance toward research that has pervaded the institution over the past four decades. During such crises, courses can
become separated from content. Expertise can become an external skill that becomes an optional add-on to a program rather than integral to it.

Bright new faculty in teaching institutions can become caught up in their extraordinarily heavy workloads and in institutional crises. Subsequently, both their currency in their fields and confidence in themselves wane. The distance grows between who they are becoming and who they showed promise of becoming in graduate school. In addition, their acquaintances at research universities seldom understand the kind or extent of labor that a teaching institution demands of its faculty. Hence, they lose credibility with previous peers and, moreover, may blame themselves for it.

I am indebted to SSSI colleagues for believing that I had something to say before I said it. My earlier struggles made receiving the Charles Horton Cooley award all the more poignant and meaningful. I hope my award inspired other teacher-scholars to reach beyond their locations and situations as Joel Best’s (who had been at CSU, Fresno) Cooley award a few years before had inspired me. Those of us at undergraduate institutions can do some research and writing although the structure of our lives limits our productivity. And we, too, can mentor. We can help new PhDs create a balance between teaching and research. I so appreciated receiving the Feminist Mentors award because it affirmed that social scientists like me could contribute to the lives of new members of our professions.

A marker event in my sociological journey was receiving the George Herbert Mead award for lifetime achievement in 2006. I felt deeply honored to receive this award and humbled to think of what it has signified to SSSI members throughout the years. I am grateful to the Society for the award and for creating a community in which its members can flourish. Through participating in this community, our professional identities emerge and develop, often in unforeseen ways.

Throughout the years, I have written empirical pieces on the social psychology of health and illness but now much of my writing focuses on grounded theory methods and scholarly writing for publication (see, e.g., Charmaz, 2006a, 2006b, 2009a, 2009b, 2012; Wertz et al., 2011). These areas reach scholars across the globe and thus have involved coauthorships with international colleagues from diverse fields such as informatics, psychology, education, and counseling (e.g., Bryant & Charmaz, 2007; Charmaz & Henwood, 2008; Thornberg & Charmaz, 2012; Tweed & Charmaz, 2011). For some years, I have been giving short professional development classes to doctoral students and faculty on qualitative methods and writing for publication and hope to give many more of these classes in the future.
At present, I stand on the threshold of retirement not knowing whether it will be a transition or turning point. Should the SSU Faculty Writing Program survive the draconian budget cuts for the next academic year, participating in our part-time retirement program will symbolize a transition. If not, another sharp turning point awaits me. Whichever occurs, I look forward to developing my ideas as a symbolic interactionist.

ACKNOWLEDGMENTS

I thank Lonnie Athens for the invitation to form these reflections and for his forbearance with my difficulties in completing them. I also appreciate Linda Liska Belgrave’s helpful comments on an earlier draft. This chapter develops a few points from an earlier presentation, “Curiosity, Coaching, and Compassion: Being Mentored by Anselm Strauss,” at the 2005 Anselm Strauss Memorial Colloquium, University of California, San Francisco, and draws on several earlier comments from a statement I made on receiving the SSSI Feminist Mentors award.

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**AUTHOR BIOGRAPHY**

**Kathy Charmaz** is professor of sociology and director of the Faculty Writing Program at Sonoma State University. She has written, coauthored, or coedited nine books including *Good Days, Bad Days: The Self in Chronic Illness and Time*, which received awards from the Pacific Sociological Association and the Society for the Study of Symbolic Interaction, and *Constructing Grounded Theory: A Practical Guide Through Qualitative Analysis*, which received a Critics’ Choice Award from the American Educational Studies Association and has been translated into Chinese, Japanese, Polish, and Portuguese. Her recent multiauthored books are *Five Ways of Doing Qualitative Analysis: Phenomenological Psychology*,...
Grounded Theory, Discourse Analysis, Narrative Research, and Intuitive Inquiry and Developing Grounded Theory: The Second Generation. She has received the Goldstein Award for scholarship from Sonoma State University, the George Herbert Mead Award for Lifetime Achievement and the Feminist Mentors Award from the Society for the Study of Symbolic Interaction. She gives professional development workshops on grounded theory methods, intensive interviewing, and writing for publication across the globe.
TURNING POINTS AND TRAJECTORIES IN A LATE-BLOOMING CAREER

Adele E. Clarke

ABSTRACT

My early life was punctuated by turning points and transformations that gradually led to a surprising and late-blooming academic career – my first “real” sociology position began when I was 44. Here I trace six different trajectories of scholarly work which have compelled me: feminist women’s health and technoscience studies; social worlds/arenas and the disciplinary emergence of reproductive sciences; the sociology of work and scientific practices; biomedicalization studies; grounded theory and situational analysis as qualitative research methods; and symbolic interaction-ists and -isms. I have circled back across them multiple times. Instead of seeing a beautifully folded origami of a life, it feels more like a crumpled wad of newspapers from various times. Upon opening and holding them up to the light in different ways, stories may be slowly discerned. I try to capture here some of the sweetness and fragility of these moments toward the end of an initially stuttering but later wondrously gratifying career.

Blue-Ribbon Papers: Behind the Professional Mask: The Autobiographies of Leading Symbolic Interactionists
Studies in Symbolic Interaction, Volume 38, 75–102
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The year 2010 evoked for me much reflexivity about what is life and especially what is a scholarly life. Losing Leigh Star, my closest colleague and friend of 30 years, and writing an obituary for her (Clarke, 2010) was and remains utterly wrenching. Celebrating the 39th anniversary of our first date with my husband Allan Regenstreif (without whom …) was infinitely comfortable and cherishing. Taking Ginnie Olesen to lunch for her 85th birthday and discovering that the day before we had both sent off papers for publication tripled the cause for celebration. Moving to the house of my dreams which hangs into a canyon through which coyotes stroll while red-tailed hawks soar overhead and yet amazingly is still in San Francisco has been utterly joyous. Yet sorting through the detritus of the past, deciding which lives and projects to keep on with and which to toss, is demanding deep identity work indeed, especially intersecting with my 65th birthday. Looking up from my keyboard, I see a picture of Anselm Strauss leaning on the front steps of Max Weber’s house. I noted his passing 14 years ago with a call to his wife Fran, both of us reliving that huge loss yet again. I try to capture here some of the bitter sweetness and fragility of these moments toward the end of an initially stuttering but later wondrously gratifying career and life.

For Anselm’s students, intellectual biography is always with us, making the invitation to write this autobiography an even more special honor. Ans often began doctoral seminars by querying us about how we came to be there – teaching us reflexivity-by-doing in considering intersections. And he, too, frequently reflected on how aspects of his personal life had generated his scholarly pursuits, how those pursuits had flowed one into another and not necessarily neatly. His own and Barney Glaser’s loss of their fathers had propelled them into the study of death in hospitals that became both the book *Awareness of Dying* (1965) and the research through which they produced *The Discovery of Grounded Theory* (1967).

My earlier decades are characterized by a series of what Anselm called “turning points and transformations,” occasions when life shifted in irrevocable ways, when “… in coming to new terms a person becomes something other than he or she once was” (Strauss, [1959]1997, p. 316). Anselm always stressed “the open-ended, tentative, exploratory, hypothetical, problematical, devious, changeable, and only partly unified character
of human courses of action” (Strauss, [1959]1997, p. 315). I routinely teach this chapter of his work because doctoral students need it so much in terms of handling their own changing identities – and it is good for me to be reminded as well.

My early transformations finally set me on a sociological path, however belated and unconventional. Then a series of “status passages” (Glaser & Strauss, 1971) moved me from student to postdoc to faculty and, at long last, to tenure. Gradually a series of different “trajectories and arcs of work” emerged (Strauss, 1993, pp. 52–57). Instead of seeing all this as a beautifully folded origami of a life, it feels more like a crumpled wad of newspapers from various times. Upon opening and holding them up to the light in different ways, some stories may be slowly discerned.

BEGINNINGS

I was born in Brooklyn, NY at the end of WWII. My mother had been a bookkeeper at the Dyker Heights Home for Blind Children. My father was an industrial chemist who worked on “bug bombs” – pesticides to use in the trenches. When I was two, he developed a melanoma on his foot, attributed by my uncle to his working in a chemical miasma. He died when I was six, my mother returned to work, and we scraped by until she remarried. When I was 10, my stepfather had a cerebral hemorrhage, was forced to retire, and could never drive again. My mother worked well into her 60s. Thus I always knew I needed to be able to take care of myself. My sigh of relief at tenure at a job with a pension was especially deep and heartfelt.

Growing up on Long Island, I had incredible public school teachers bursting with the energy of second generation immigrant children who had “made it” into education – the white-collar middle class. From one, I learned to use Kate Turabian’s Guide for Writers of Theses and Dissertations – in junior high. I still love footnotes with a special passion for the side- engagements and conversations they allow. From many of those teachers I learned to love learning and to consider teaching as a career.

Two experiences in high school were, in retrospect, significant for me as a sociologist. My high school in Port Washington was located near F. Scott Fitzgerald’s East Egg and I went to school with children of the super elite, children of their servants, children from very “comfortable” upscale neighborhoods, and children from postwar housing developments (where I lived). These new housing developments were, quite radically for that era, “integrated” – there were Protestants, Catholics, and Jews from varied
ethnic backgrounds – all white, of course. My somewhat racially integrated class at that smallish high school was ferociously (if awkwardly and self-consciously) democratic and antiracist. I think I had a course that included “sociology” there as well.

One summer, I fell in with a crowd called the La Dolce Vita Club of Beacon Hill Beach. These were kids from the very “comfortable” Waspy neighborhood, folks from the quite ethnically diverse white working class summer home enclave (closer to summer shacks) next to the private beach, and a few others like myself. Instead of typical 1960s rigid heteronormative dating, this crowd just partied, went to movies, and did things in groups. Everyone was welcome. I made lifelong friends, gay and straight, learned to move in more diverse spaces, there and in Manhattan, and to see and better understand differences and their consequences.

Because I won a New York State Regents Scholarship, I had to go to college in New York State. This was not good, as I wanted to go far far away from home. Buffalo, was further but I settled for Syracuse. In a freshman philosophy course, the paper I wrote on the La Dolce Vita Club was returned with A+++ at the top and a note: “But this is sociology, not philosophy!” I quickly found Sociology 101 and “came home” for the first time intellectually. The TA, who was from India, had me meet with him in office hours to talk sociology. I also talked about how unhappy I was at Syracuse where most females went to get MRS degrees. My dorm had over 3,000 “coeds” and perhaps 5 of us actually studied. The TA told me I could transfer. I said, “What’s that?”

Magically, six months later I was at Barnard College of Columbia University, an intellectual and, importantly, sociological nirvana at that time. It provided me with excellent theoretical and empirical foundations that remain palpable in my work today. I had courses with Renee Fox, a superb qualitative researcher and teacher whose own autobiography is just out (Fox, 2010). She had us read Laura Bohannon’s Return to Laughter (Bowen, 1954), an ethnography that amazed me. She also introduced me to the emerging specialty of medical sociology, at the heart of my life ever since. Mirra Komarovsky had us read Karl Marx as a social theorist c1965 (among others, of course). She also studied what we now call gender, with books titled Women in the Modern World (1953) and Dilemmas of Masculinity (1976). As I read a biography of her (Reinharz, 1989), a wave of memory struck about her embodying and transmitting simultaneous commitments to meso-level theory and empirical work. She was seriously engaged in theorizing, and I was rapt. Of course, we also read lots of Merton, and I was dissuaded by other students from taking courses with
Bernard Barber, a functionalist sociologist of science. Fox’s (2010) memories of the department are not all good – and now I understand why.

In Gladys Meyer’s course, we used her book *Minorities in American Society* (Marden & Meyer, 1962) and I learned of the Chicago School – both of which became central to my scholarship. In this women’s college, I also learned deeply that women could be professors, and superb ones, however rare at the time. I also took probably the first incarnation of Immanuel Wallerstein’s course on world systems at Columbia across the street, along with much art history. This has been foundational for my work in visual culture (e.g., Clarke, 2005; Clarke, Mamo, Fosket, Fishman, & Shim, 2010; Moore & Clarke, 2001) and a lifelong source of pleasure.

Deciding that I wanted to teach, I applied to London School of Economics planning to become an Africanist. I was accepted sans fellowship. New York University (NYU) offered an NIMH Fellowship and I moved down to the Village with glee and a stipend in 1966. Eliot Freidson was my advisor and ran the Fellows Program. There were about 30 of us, including David Karp (who won the Cooley Award in 1996), his wife Doreen, and my pal Scottie Embree. We read Berger and Luckman hot off the presses, Goffman and Garfinkle, and Howie Becker came and gave talks. Yet despite the fact that many of the faculty were doing interview-based sociology of the professions (Erwin Smigel on law, Freidson on medicine), there was no qualitative methods training. Survey research was all. I got quite good at it (as did Carolyn Ellis! Forthcoming) and was in the first generation to do computerized data analysis (rather than using a “card sorter”). I was studying sociology of law, emphasizing constitutional law. For a course of Richard Quinney’s, I wrote a paper comparing the draft constitutions proposed by the Kuomintang and Communist parties during the Chinese Revolution, using the wonderful East Asian Library at Columbia. I wish I had heeded his urgings to publish it.

At this time I was also learning Tai Chi Chuan and dancing (nonprofessionally) with two amazing choreographers now heralded as founders of postmodern dance, Deborah Hay and Yvonne Rainer. Deborah offered free classes to those who would perform her work without pay, and we appeared at Lincoln Center, the Guggenheim Museum, and other venues open to rethinking/doing movement. The New York art and hippie scenes were fairly fused, especially at “Be-Ins” and the like. I participated in the Jade Companions of the Flowered Dance, a short-lived informal bail bond group for which my sociology of law prepared me well.¹ My navy blue pseudo-Chanel suit, altered from its Lane Bryant “Chubettes” origins, did heavy lifting in terms of “presentation of self” at the courts. I had to keep
the jacket closed so that the pink poodle lining would not show and discredit me, as it was not then a retro-hip signifier.

One of the students in the NYU Fellowship Seminar found a referral for psychotherapy at sliding scale. Many of us leapt at the opportunity. I paid $7 per session for individual therapy, and was in return required to participate in group therapy, then emergent. Sam Janus, PhD, and the group were pivotal in my life. Beset as an only child by a stunningly invasive mother, he had me put the phone down when she called: walk around the room, pick it up, say yes, put it down again, walk, repeat the sequence a few times, then say, “Gotta go now” and hang up.

But I really did have to go. I visited California in 1968. My first view of San Francisco was riding across the Golden Gate Bridge on the back of a motorcycle in late afternoon, and I thought I had found nirvana. The city looked like a white sailing ship against the blue waters of the bay and ocean. I felt an incredible lightness of being here that I cherish to this day. There’s a spaciousness and sense of freedom that does not exist on the East Coast. I could finally breathe. Knowing I would return, I went back to New York to finish my masters and do some more therapy. The thesis derived from my work at an employment agency for the physically disabled, Just One Break, Inc., founded after WWII, supported by an annual ball at the Plaza Hotel. I was working on what was called “The Poverty Area Project,” surveying disabled clients of the agency who were also poor and often Black. Freidson (1966) had just published Disability as Social Deviance, and Goffman’s (1963) Stigma extended Becker’s and others’ labeling theory to disability as well. I studied the “life chances” of disabled minority people in terms of employment, income, and marriage. The most significant variable was “visibility of the disability” – not functional ability or education – a theme that remains central in disability studies today, albeit in considerably more sophisticated forms.

As I was finishing the thesis, a woman from my therapy group, Rebecca Woolis, announced she had a car to drive across country that summer and did I want to go with her. OH! YES! PLEASE! Deborah Howland, my old friend from the La Dolce Vita Club, and a new friend, Pam Mendelsohn, gave a going away party for me with a mystery gift box with a tag to pull on coming out of a slot. I pulled and there were many many dollars taped together from all my friends to send me on my way. M.A., duffel bag and much needed cash in hand, SOHO loft sublet to a cat sitter, I took off, only returning to move out and move on a year later. (One only gives up an apartment in NYC in extremis.)
BEING A HIPPIE IN THE WOODS AND BECOMING A TRANSDISCIPLINARY SOCIALIST FEMINIST INTERACTIONIST

Moving West was transformative in more ways than I probably am aware of. I moved to a tiny village in the Mendocino mountains called Comptche. After living on two communes, I was able to rent a cabin at an abandoned logging camp many miles up a dirt road far beyond electricity though with running cold water (that I hooked up). My neighbor, Patty, had lost a leg and her partner in a car wreck but, in the words of a now-dated disability framing, was a “super-crip” indeed. Our cabins were wood-heated. I hauled; she chopped.

I got a job teaching at College of the Redwoods Fort Bragg Branch Campus that held classes in the local high school at night. The students were an odd combination of Viet Nam vets going to school on the GI Bill, young people seeking a way out of town, locals and newcomers (hippies and retirees) bored to death and needing something interesting to do, and a smattering of Alcoholics Anonymous Twelve-Steppers trying to stay out of the many bars lining the streets of this small logging and mill town. Soon I also taught courses for local el-hi teachers for Sonoma State University Extension so that they wouldn’t have to drive for hours to get their needed update credits.

Oddly, over the next decade mostly in the woods, I became a seriously transdisciplinary social scientist. I taught 25 different courses over five years (in sociology, anthropology, and history) because the student population was small and I needed a set number of students for the course to “go.” One day in the bathroom of the Fort Bragg Public Library, I found a pile of uncatalogued sociology books on the floor. The inside of the covers had the name Jerry Mandel, who I had heard was the only sociologist other than me in the county (Google tells me he briefly taught sociology of drugs at Sonoma State, then left academia). I took a bunch of the books, including interactionist works on deviance, the start of my West Coast sociology library.

When I asked about teaching some of the Native American Studies Program curriculum developed at the Eureka campus by local tribal scholars, I was interviewed by the Program Director and was invited to a powwow. Then, authorized by them to teach in this area, I began intensive learning, staying a week or three ahead of the students but also drawing deeply on my training in minorities in American life. Some of the students
were Native American, both local and from elsewhere. Many had passed for white for much of their lives, locally claiming “Portagee” (Portuguese immigrant) identity to explain their darker skin (thank you, Ev Severi). Native American history was so endlessly brutal, week after week, that one very large ex-Marine finally threw the textbook across the room and burst into tears. He then came out as Native American, triggering a wave of “coming out” and “passing” stories. Of note, some of the best teaching materials I found during the 1970s on native peoples and imperialism were from the East/West Institute in Hawaii, where President Obama’s mother was affiliated.

During these early 1970s years, I was very involved in feminism, participating in a women’s consciousness raising group that met on the Mendocino Coast, one of two in the area. The other group was in Albion and published Country Women, a feminist journal for rural women, and had annual Women’s Festivals for a while. I published my first paper on contraception (which I still study) in Country Women, titled “Me and My Diaphragm: Love at Third Sight”. It focused on the comparative safety and awkwardness of using this method, especially with only cold running water. Many years later when I met the late Barbara Seaman (The Doctors’ Case Against the Pill), she remembered that paper vividly.

Interestingly, there was no group to authorize my teaching women’s studies courses which I also began to do, making women’s studies up as I went along. I taught intro, American women’s history, women and the law and, in 1973, my first course on women’s health, the very same year Ginnie Olesen offered her first course on this topic at University of California, San Francisco (UCSF). I would meet her soon. Somehow, in the middle of the woods, I heard that there would be a conference on women’s health in San Francisco in 1975, and I got myself there. Organized by Ginnie, Sheryl Ruzek, and Ellen Lewin, this was the very first federally sponsored conference on women’s health that was not exclusively clinical (Olesen, 1975). It was transdisciplinary and laid down some of the feminist women’s health and healing tracks I have moved along ever since, often with both Ginnie and Sheryl (more later).

By then I was living with Allan Regenstreif who had come to Comptche to visit, via a connection to the brother of my therapy-and-travel-mate, Rebecca. We had lived within 10 miles of each other most of our lives in New York, yet first met, and seven years later wed, among the California redwoods, moving from Comptche to Ft. Bragg to Ukiah in search of employment and stimulation. We were both very active in the antidomestic violence movement there, founding a shelter (me and local feminists) and
providing childcare so the women could have therapy and breaks (Allan and the local men’s movement). I kept teaching, adding courses in assertiveness training with Anne Brener, who has since gone on to become a rabbi and presided wondrously at my father-in-law’s funeral in 2008.

One day, I visited a sociologist at the junior college campus in Ukiah, and she handed me a flyer recruiting part-time faculty for the Women’s Studies Program at Sonoma State University. It had hand-done drawings, said the program was run by a faculty–student collective, and offered student- as well as faculty-taught courses. I applied with incredible excitement and remember agonizing over what to wear to the interview (coral top, denim wrap skirt, navy espadrilles). It took place in someone’s home with all of us sitting on the floor in a large circle, and reminded me of the powwow interview. I got the job which was another major turning point. It and the Co-Coordinator of Women’s Studies, Ruth Mahaney, changed my life forever. She was part of the West Coast Socialist Feminist Study Group and recruited me, seriously deepening my understandings of feminist political theory. There I met Donna Haraway, Gayle Rubin, Judy Stacy, and many other feminist scholars.

I was coming to realize that I would need the PhD to be able to stay in academia. My other amazing contact at Sonoma, Kathy Charmaz, opened that door. She generously invited me to stay over at her home in Santa Rosa once a week and we talked and talked about where I should go to grad school. I knew I wanted medical sociology, qualitative methods, and women’s health. Berkeley grad students (Stacy Oliker and Ruth Milkman) warned me against going there because of the sexism (which Dorothy Smith has documented vividly). Kathy handed me Discovering Grounded Theory and wrote me a letter of recommendation for UCSF. I was accepted. Turning point indeed!

**GRAD SCHOOL AND POSTDOC YEARS**

After a decade in the woods and memories of hard times in grad school at NYU, I entered UCSF warily at age 35. As best I remember, the only females to complete the PhD at NYU had been married or had affairs with faculty or both. But, as with my undergrad experience, my second try at grad school was successful and exhilarating. It was a “coming home” to interactionism and feminism and women’s health and actual training in qualitative approaches to empirical research that I could believe in (Clarke & Star, 1998). Anselm taught interactionist theories as I sat next to Leigh Star,
bonding for life with the theories and with both of them. Ginnie Olesen and Lenny Schatzman taught superb courses in field research. Brushing sexism aside in my glee, which Ginnie’s presence certainly helped us to do (Olesen, 2009), I jumped into the qual world with both feet. My cohort also met a number of times with Barney Glaser at his offices in Marin, as he was no longer affiliated with UCSF. Some of us continued with him, while others, myself included, did not. I was Anselmian even then.

While a student at UCSF in the early 1980s, with Leigh Star, Rachel Volberg, and Joan Fujimura, I also jumped into science and technology studies (STS), perhaps the major turning point of my career. It was the ideal time to do this as Latour and Woolgar’s (1979/1986) Laboratory Life and other new forms of STS scholarship were revitalizing the terrain, providing alternatives to the strictly bounded Mertonian functionalist STS dominant in the United States and the neo-Marxist approaches more common in Europe. I joined the international Society for Social Studies of Science (4S), and this became my second “tribe,” along with the Society for the Study of Symbolic Interaction (SSSI) to which Ginnie and Ans had quickly introduced me. I was also invited to be one of the founding scholars (with Sal Restivo, Tom Gieryn, and Susan Cussins) of a new section of the ASA focused on Science, Knowledge, and Technology. With a proud smile I can say that my former student Monica Casper is the current Section Chair.

Through Anselm’s teachings, Leigh, Rachel, Joan, and I brought the interactionist sociology of work to bear in our early STS writings (generated in our dissertation study group). This was remarkably congruent with the nascent focus on scientific practices and the production of knowledge in STS, and our efforts were immediately intelligible and warmly welcomed in STS (Clarke & Star, 2003, 2008).

After graduating in 1985, I worked on a project in women’s health with Ginnie and taught as adjunct faculty in both sociology and, with Guenter Risse, in the history of health sciences at UCSF. I had taken a slew of history courses from Gert Brieger and Dan Todes (now at Hopkins). They connected me to historians of biology as well (e.g., Jane Maienschein, Gar Allen, Gregg Mitman), with whom I was invited to collaborate on some terrific projects, including one focused on biology at the University of Chicago.

At this point, my mother became seriously ill and we abruptly had to move her across the country and into a hospital in San Francisco where she remained for several months. I emptied and sold her house with help from my elderly uncles while Allan kept watch over her. I was feeling increasingly marginal, untethered, and desperate that I would not be able to pull off an academic career. Then presto! I landed a NIMH postdoc at Stanford with
Dick Scott, among the most generous senior scholars I have ever known. It transformed my life yet again.

**COMING HOME AGAIN – UCSF REVISITED**

After I had been on the postdoc for a year, the person who had been hired to replace Anselm, when he was forced to retire at age 70, announced she was not coming to UCSF after all. They would need to search again. This time, I could and did toss my hat into the ring. However briefly, I had left home and made good. And it was good enough. I was hired by then chair Carroll Estes and began as tenure track faculty in 1989. Thanks to a stunning letter from Carroll that started by saying hiring me was the best thing she had done as Chair, I got tenure in 1992 – literally three years, three months, three weeks, and three days later. I have adored tenure – not only for its job security, but also for the intellectual freedom.

One distinctive freedom at UCSF is that it is just dandy to publish with other people – including students and alums – rather than having only single-authored works really “matter.” I have done so with fantastic students for decades – Monica Casper, Lisa Jean Moore, Theresa Montini, Janet Shim, Laura Mamo, Jennifer Fosket, Jennifer Fishman, Sara Shostak, and Carrie Friese to date. While overall, I would have preferred to be at a “normal” university where my transdisciplinarity would have been more nourished, the History of Health Sciences and Medical Anthropology Programs have provided major colleagues at UCSF (especially Vincanne Adams, Warwick Anderson, and Philippe Bourgois), students and stimulation.

The UCSF Sociology Program is also happily far from the mainstream, with both a deep interactionist tradition and more recent critical social justice approaches. I have also had the not inconsiderable privilege of teaching only graduate students since 1983. As Director of the Doctoral Program in Sociology for 20 years, I am stunned, proud, and gratified at the recent rankings that placed us circa 6th in the United States² I am also most fortunate in my department colleagues, sociologists Ginnie Olesen, Howard Pinderhughes, Shari Dworkin, and Janet Shim, and nursing health policy scholars Ruth Malone and Susan Chapman. Key staff members also have made life much much better over the years, including Barbara Paschke, Ray Rudolph, Linda Tracy, Brandee Woleslagle, Cynthia Mercado-Scott, and Regina Gudelunas. Ginnie has been my special friend, mentor, and role model for decades, and also my moral compass in the quagmires of academia. When I feel lost, I ask myself, “What would Ginnie do?”
In 1995, someone ran a stop sign and rammed into the side of my car. I stood on the brakes and saved my life, but spent eight months in a wheelchair with a severely shattered leg. Told that I might never walk again, I found another doctor, and subsequently walked, albeit a bit atilt, thanks to a high tech “electronic bone growth stimulation device.” Biomedicalization-r-moi! A decade and two more car accidents later, I had ruptured disks and was literally on the verge of paraplegia. I had very major spinal reconstruction that has preserved my mobility, with limits. However, I had to cease work on a major new transnational project, tracing the history of globalization of “things medical” through exchanges between two “sister hospitals” – Johns Hopkins and a hospital in Chaing Mai, Thailand. And I have increasingly had to reconfigure my scholarly and personal lives within frames of limited mobility and chronic pain. I think too often of Kathy Charmaz’s (1991) terrific book, *Good Days, Bad Days: The Self in Chronic Illness and Time*. But our wondrous new home with its peace, quiet, community garden, views, and Caribbean-style sunsets is now making aging feel like being on vacation.

**TRAJECTORIES OF WORK**

In thinking *across* my intellectual career for this autobiography, I find myself clinging to Anselm’s oh-so-useful concepts of trajectories and arcs of work. For Anselm, “trajectory was the central concept in my sociological, interactionist theory of action … . It refers to a course of action but also embraces the interaction of multiple actors and contingencies that may be unanticipated and not entirely manageable” that are part of it (Strauss, 1993, p. 53). Trajectories themselves are composed of shorter-term arcs of action or work (Strauss, 1993, p. 56).

My scholarship falls into six main trajectories, most initiated early on and sustained into the present moment, though often shape-shifting quite dramatically over the years. They include feminist women’s health and technoscience studies, social worlds/arenas studies including the disciplinary emergence of reproductive sciences, the sociology of work/scientific practices, biomedicalization studies, qualitative research methods, and projects on symbolic interaction (SI) per se. The rest of this autobiography presents the six trajectories, jumping back and forth in my life but following the intellectual problematics I pursued and the changing angles of vision through which I did so.

Each trajectory has had its own visible and/or invisible hands guiding it. Certainly my commitments to feminism, symbolic interactionism, STS, and
medical sociology as sites of my action as a researcher and theorizer are clear across them. But two other over-arching “hands” have also been shaping these trajectories. First is the profound commitment to empirical research and analysis inculcated in me by Anselm, Ginnie, and Lenny. This relentless empiricism was itself guided by an Anselmian interactionist focus on actions and social processes so vivid in his final books (Strauss, [1959]1997, 1993). “What were people actually doing in the domain of action I cared about?” “How were they accomplishing it?” These were my early questions. Gradually, as I came to see the world more and more through Strauss’s social worlds and arenas theory, my work has focused as well on vividly situating actions and processes. Last, since arriving at UCSF as faculty in 1989, I have sought to open up the work I pursued in each of these trajectories to interweave and integrate poststructuralist/postmodernist approaches from Foucault to Haraway and Chakrabarty, especially vis-à-vis feminisms, postcolonialities and beyond. Given the impoverished theory training I had in these domains, this has been deeply challenging but utterly worthwhile.

Feminist Women’s Health and Technoscience Studies

While this has been one of my longest and strongest areas of commitment, despite two edited books and a slew of articles, oddly enough I have never studied women per se. I did not and do not see women as the problem, but rather the sciences, professions, and institutions with which we must deal as both healthy women seeking enhanced control of our lives (e.g., via contraception) and as patients (e.g., getting Pap smears). I studied some of the sources of difficulty in hopes of changing them, and informing women of risks and problem areas. This included writing the cervical dysplasia section of Our Bodies, Ourselves with Monica Casper for a decade. Thus in this trajectory which centers on gendered practices, women are my primary audiences rather than my objects of research.

After moving to San Francisco in 1979, I became very active in the Committee to Defend Reproductive Rights and related women’s health organizations such as the Reproductive Rights National Network. These groups attended to issues of race and difference absent from many other feminist venues of the time. My focus was on sterilization abuse via both coercion and unknowing sterilization by physicians including those of Mexican women in Los Angeles, Native American women through the Indian Health Service, and African American women in the South who had “Mississippi appendectomies.” I planned a dissertation in this area, but
finding the most interesting questions already well-researched, “ended up” with one article on its subtler variations (Clarke, 1984) and another on the challenges of reproductive rights organizing for antiabuse regulations (Clarke & Wolfson, 1989).

Working with Ginnie Olesen and Sheryl Ruzek on a series of summer institutes led to the book *Women's Health: Complexities and Differences* (Ruzek, Olesen, & Clarke, 1997) that really took seriously the differences among American women in terms of race, class, sexuality, geopolitics, and their implications for health. It was/is widely used. Ginnie and I also wanted to pull feminist theory into women’s health much more ambitiously and explicitly, so we held a conference and produced *Revisioning Women, Health and Healing: Feminist, Cultural and Technoscience Perspectives* (Clarke & Olesen, 1999). To reframe the problematics of women’s health and embodiment, we drew on Haraway’s (1997, p. 273) use of the concept of “diffraction”: “Diffraction patterns record the history of interaction, interference, reinforcement, difference. Diffraction is about heterogeneous history, not about originals.” And we had truly amazing feminist theorists who contributed: Anne Balsamo, Ruth Behar, Patricia Hill Collins, Adela de la Torre, Marge DeVault, Donna Haraway, Valerie Hartouni, Patti Lather, Emily Martin, Rayna Rapp, Sheryl Ruzek, Denise Segura, Jennifer Terry, Sharon Traweek, Francoise Verges, and Nancy Fugate Woods. Few works have superseded these two books as feminist intellectual interventions in women’s health broadly conceived with the exception of introducing intersectionality theory (Schulz & Mullings, 2006).

The field of women’s health has long been focused primarily on reproduction, ranging from contraception and abortion to infertility, new reproductive technologies, prenatal diagnostics, and beyond. One of my major commitments has been to the history of contraceptive technologies. My study of the reproductive sciences and the politics that produce them (Clarke, 1998, 2000) revealed that we have the inadequate and often problematic (dangerous, potentially coercive) array of scientific contraceptives we do largely because women have been “implicated actors” (Clarke, 2005, pp. 46–48) in this arena. Our needs and desires have rarely been interrogated directly – no one asks “What do women want?” Instead, contraceptives have largely been produced to meet criteria pleasing to the culture of science (science for science’s sake) and/or development/population control movements (“What will women accept?”).

More recently, sustaining my processual focus, I analyzed how the reproductive sciences have shifted focus from control over to transformation of reproductive processes, generating babies where none were thought
possible, selecting fetuses genetically and by sex, and most dramatically, stem cell research at the heart of many contemporary biotechnologies. Such “postmodern approaches are centered on re/de/sign and transformation of reproductive bodies and processes to achieve a variety of goals,” but also still rely on the means of control over reproductive processes achieved in earlier eras (Clarke, 1995, p. 140). Obviously, interest in both reproductive control and transformation is transnational and much feminist scholarship focuses transnationally as well. One of my recent honors was to be recruited to an Advisory Board of EASTS: East Asian Science and Technology Studies: An International Journal, which focuses especially on Taiwan, Japan, and South Korea. With Chia-Ling Wu and Azumi Tsuge, I coedited a special issue on Reproductive Technologies and Gender in East Asia, situating those developments historically and transnationally (Clarke, 2008). I hope to do one final project around contraception.

Social Worlds/Arenas Theory and the Disciplinary Emergence of Reproductive Sciences

In the early 1980s, having abandoned plans for a dissertation on sterilization, I began conceptualizing a new proposal that was focused through STS and my interest in contemporary contraceptives. This quickly opened out into a major life project on the history of the American reproductive sciences focusing on the period 1910–1965 – where and when the primary development of contemporary contraceptives had occurred. Thinking big, the dissertation research included examining the development of reproductive sciences in biology, medicine, and agriculture. Believe me, only a hardy few utilized STS lenses on agriculture at the time. Why did I do so? Because of social worlds theory.

In brief, at about the same time, Anselm was developing social worlds/arenas theory (e.g., Strauss, 1978) and my early interest in meso-level conceptualizations was revitalized. I began to see the world as a multilayered mosaic of social worlds and arenas, the fundamental organizational modes of doing social life. Anselm was developing social worlds theory apart from his work on grounded theory (GT) and his work on medical topics. For me, the social worlds and arenas framework began infusing theory, methods, and substance – and my interest is sustained today.

I first used social worlds theory to understand the processes of disciplinary emergence in the case of the reproductive sciences (Clarke, 1998). Like sex,
reproduction was pretty much a “dirty word” in the sciences as elsewhere, and for the scientists I studied, pursuing this area of work was a radical act. It was pursued, I analyzed, because on the one hand, the new area of endocrinology was hot science and, on the other, there were multiple markets for knowledge of reproductive processes – in clinical obstetrics and gynecology, in animal agriculture, and in the then emergent birth control and population control movements. To boot, major private research funding was forthcoming after WWI. I used the elasticity of social worlds theory to analyze that (1) the reproductive sciences themselves were a social world with three main subworlds (in biology, medicine, and animal agricultural sciences); and (2) one could simultaneously study the broader arena in which the reproductive sciences were lively including the other major social worlds involved (funding sources, birth control, and population control movements). The book itself is organized to feature the other key social worlds for the reproductive sciences across the era of its emergence (funding sources) and the era of its coalescence (birth control and population control movements). The book won the Eileen Basker Memorial Prize from the Society for Medical Anthropology and the Ludwig Fleck Award from the 4S, precious scholarly honors.

Early in this trajectory, I began writing more theoretical papers. I was trying to be not only a researcher but, like Anselm, to also write more broadly. Building up (or perhaps better, abducting) from an empirical base or case study, I tried to see if the key processes I analyzed shaped action more broadly. An initial effort was my response to spending a two-year postdoc on organizations at Stanford, which I had thought would center on social organizational phenomena rather than business organizations. In it I compared social worlds/arenas theory with the major organizations theories of the day: resource dependency and mobilization, population ecology, areal field, functional field, and network models (Clarke, 1991). It was published in the lovely festschrift for Anselm edited by David Maines, Social Organization and Social Process. And it connected me to the handful of other symbolic interactionists equally riveted by meso-level concerns. A special honor in this regard was being invited to give the Peter Hall Lecture at the Midwest Sociological Association meetings in 2007. Thank you, Peter.

And in STS, Leigh, Joan, and I all were publishing work utilizing different concepts related to social worlds/arenas theory. Leigh was especially famous for her concept of boundary objects – things that dwelled at the intersection of multiple social worlds and, while interpreted differently within these worlds, retained coherence across them (Clarke, 2010; Star, 2010). I became especially known for a paper that did an arena analysis also showing
differences within worlds (Clarke & Montini, 1993). Leigh and I also reviewed how “The Social Worlds Framework: A Theory-Methods Package” has been taken up in STS (Clarke & Star, 2008).

In a “return of the repressed,” I recently circled back to the history of animal agriculture, leading off a conference at Cambridge titled *Between the Farm and the Clinic: Agriculture and Reproductive Technology in the Twentieth Century*. My paper compared reproductive agricultural institutionalization in the United States and United Kingdom historically (Clarke, 2007). I dug out notes and papers from 25 years earlier to discuss some of the roots of biotechnology research, including stem cells, in the transnational industrialization of agricultural animal production. And I remembered the wonderful sociologist Rue Bucher driving me from Chicago to Urbana despite being gravely ill. There I interviewed Andy Nalbandov, a reproductive agricultural scientist pal of Rue’s former partner, pioneering reproductive endocrinologist Neena Schwartz (2010).

**Sociology of Work and Scientific Practices**

My third major research trajectory is the sociology of work and scientific practices. My initial project here centered on the organization of scientific research materials and animal models. Across the first half of the twentieth century, American reproductive scientists truly had to “do it themselves,” from building fowl cages to accessing parts of organisms from abattoirs to organizing the first primate research colonies. The remarkable Princeton historian of science Gerry Geison, since sadly deceased, invited me to a conference and published my first paper in this area (Clarke, 1987). He lauded my work widely for empirically demonstrating that it was not only theory that provoked new scientific experiments, but also reliable access to research materials, a quite radical thesis at the time.

Bouncing off this work, Joan Fujimura and I organized sessions at the ISHKABIBBLE meetings (International Society for the History, Philosophy and Social Study of Biology) that led to our edited volume *The Right Tools for the Job: At Work in Twentieth Century Life Sciences* (1992). Our core argument was that the tools, the jobs, and their “rightness” for the work at hand were each and all constructed or, in today’s framing, co-constitutive. In short, the materials, theories, techniques, and the work of drawing them successfully together to produce science all had to be, in Anselmian terms, “articulated” (Strauss, 1988). The materiality of this work and our in-depth analyses of relations among the humans (scientists)
and the nonhumans (research materials and technologies) led Bruno Latour to organize a French translation. Some lovely visits to Paris ensued. I was welcomed at Bruno’s Center for the Study of Innovation at the Ecole des Mines and at CERMES, a research unit focused on history and sociology of medicine, including Isabelle Baszanger, Ilana Lowy, and Jean-Paul Gaudilliere. Especially lovely was being visiting faculty in the Group for the Study of Ethnography in Medicine at l’Ecole Normale Superieure.

Two other kinds of scientific practices became the focus of studies of the history of the Pap smear Monica Casper and I did. The Pap smear is one of the most successful screening techniques for the prevention of cancer, utilized transnationally since the mid-twentieth century. Despite its success, reading and classifying Pap smears is very difficult and time-consuming and a number of different classificatory systems featuring this or that facet have been utilized. One of our rather ghastly findings, reminiscent of my contraception studies, was that recent classification systems have been institutionalized not because of their clinical accuracy or diagnostic utility, but because they better serve the needs of research (Clarke & Casper, 1996).

Making the Pap smear into “the right tool for the job” of cervical cancer screening was our second focus (Casper & Clarke, 1998). Smears succeeded because of multiple efforts: gendering the division of labor by creating new cheaper largely female cytotechs; attempting to automate readings; juggling fees within labs so that actual smear costs were hidden by higher rates for other tests; promoting laboratory regulations; and settling for locally “negotiated orders” (Strauss, 1993, pp. 248–250) of clinical accuracy rather than global standardization. Sadly, as medicine itself is biomedicalized, locally “negotiated orders” for doing clinical work are rapidly disappearing – the precious art of fitting medicine to individual bodies.

With former student Carrie Friese, I recently returned to this trajectory. Carrie studied the cloning of endangered species for her dissertation and we had many conversations comparing the research materials practices of the reproductive scientists I had studied in the early/mid-twentieth century with those of the zoo and biotech scientists she was studying in the twenty-first century. As noted above, I had analyzed a shift in the life sciences from control over to transformation of reproductive processes, with postmodern approaches “centered on re/de/sign and transformation of reproductive bodies and processes” (Clarke, 1995, p. 140). This shift is precisely what we are seeing in our comparative work. Rather than study particular processes in particular species and then transpose the processes across species as in the past, the new focus of biomedical knowledge production is cloning rare and
endangered species using the bodies of domestic species for gestation. They are transforming the reproductive bodies and processes per se. Fascinat-
ingly, this “workaround” of using domestic species is provoking major debates about whether the new organisms can “count” as members of a species which did not gestate them (Friese & Clarke, 2012).

**Biomedicalization Studies**

In the late 1990s, a group dubbed by Ginnie Olesen “the gang of five” began to meet to discuss what became biomedicalization theory. Four were in a dissertation-writing group: Laura Mamo, Jennifer Ruth Fosket, Jennifer R. Fishman, and Janet K. Shim. I was chairing Mamo, Fosket, and Fishman’s committees and was a member of Shim’s. We all had current empirical research projects situated in late-twentieth-century biomedicine. What brought us together was individually and collectively grappling with the inadequacies of medicalization theory and the unnecessary and inappropriate disconnects among medical sociology, STS, history of medicine, feminist theory, body studies, cultural studies, and so on. The gang of five began collaboratively analyzing the shifting processes of biomedicine and the contours of what I (1998, p. 275) had framed as “the biomedicalization of life itself (human, plant, and animal)… often imaged as a juggernaut of technological imperatives.” We generated a dense historical chart to chronicle changes in biomedicine that became our “grounded” empirical database.

Again, I was trying to ratchet up the empirical work to generate and theorize its broader processual implications (thank you, Anselm). And we all wanted our audiences for this work to include but not be limited to medical sociology. We gulped and submitted our key paper, “Biomedicalization: Theorizing Technoscientific Transformations of Health, Illness, and U.S. Biomedicine”, to the American Sociological Review (ASR) where it appeared in 2003 after multiple huge revisions. The ASR article articulates biomedicalization theory as a synthesis and overview of changes in biomedicine and medicalization, discussing its five broader processes in depth: (1) privatization and commodification, (2) risk and surveillance, (3) expanding technoscientific practices, (4) the production and distribution of knowledges, and (5) transformations of bodies and subjectivities. Significantly, Ginnie (warmly) criticized a draft by asking whether there were any resistances, any moves to counter what she read as “a tsunami of biomedicalized power?” We then began to write about contingencies, stratifications, and the many forms of resistance as well.
The gang of five really did want to challenge the specialty of medical sociology to take sciences and technologies – and Big Pharma and the other formations of biocapital transforming the planet – much more seriously. It is simply not possible to understand the present and future of biomedicine without doing so. If we assess our success via the Medical Sociology Section meetings at the American Sociological Association, this has not occurred. Where it does happen extensively is at meetings of the 4S and in STS journals.

To showcase our grounded theorizing of biomedicalization, we have since published a book of empirical case studies (Clarke et al., 2010). It also situates biomedicalization vis-à-vis contemporary theorizations of “life itself” and biocapital. One of my chapters offers a visual cultural analysis of three eras (1890–present): (1) the rise of medicine, (2) medicalization, and (3) biomedicalization. I find visual culture as constitutive rather than reflective of social life and have tried to demonstrate that. I also wrote an epilogue about studying biomedicalization in its transnational travels. Here I was able to utilize the great gift I had of being a Residential Scholar focused on “medicines and globalization” at the Rockefeller Study Center in Bellagio, Italy with my inspirational colleague, historian Warwick Anderson.

Qualitative Research Methods

GT has been part of my life for over 30 years. I think through it. Yet as I regularly taught the doctoral course sequence in qualitative methods at UCSF in the 1990s, I began to want more. I had been haunted since grad school by the c1970s neo-Marxist critique of SI that it lacked an analysis of power (e.g., Athens, 2009), of particular importance to me because of my socialist feminism which included race and class concerns, and because of a great course on power with Carroll Estes. I also had a serious critique of Anselm’s conditional matrix, and sought a way to elucidate the conditions of possibility – the situation of research.

I was provoked especially by Norm Denzin’s (1970/1989) early efforts to “situate research” in The Research Act. There were two or three paragraphs where he pointed in this direction that I taught religiously and which deeply validated my efforts. Donna Haraway’s (1991) classic feminist theory paper on “situated knowledges” was also fundamental. But in retrospect it was Foucault’s (1972) concept of discourse that opened my mind and gave me the tool I needed. I had wanted that concept since the 1960s when McLuhan and Fiore (1967) drew our attention to how The Medium is the Message. I
knew it was not only the media, but could not move forward without the concept of discourse. For me it is the key social structural link between mass society/culture and interactions. Affect may be the processual link.

Then in 1995, I had the honor of being a Residential Research Fellow at the UC Humanities Research Institute with a group on “Feminist Epistemologies and Methodologies.” This incredible assemblage of scholars included Val Hartouni, Katie King, and the major feminist poststructural theorist of methodology today, Patti Lather. It put me over the edge. I knew I had to do something about GT. Ongoing conversations with Kathy Charmaz and Leigh Star helped get me going and I gradually generated situational analysis (SA). A great leap forward was accomplished in a small house high on a ridge overlooking the vast Saint Lawrence River valley in a tiny rural Quebec village called Les Eboulements where I stayed alone for a week in 2001. Me and my laptop in a place that was all about place – situatedness.

In SA, the situation of inquiry is central and empirically constructed by making three kinds of maps: (1) situational maps lay out the major human, nonhuman, discursive, and other elements in the research situation of inquiry and provoke analysis of relations among them; (2) social worlds/arenas maps lay out the collective actors and the arena(s) of commitment and discourse within which they are engaged in ongoing negotiations – meso-level interpretations of the situation; and (3) positional maps lay out the major positions taken, and not taken, in the data qua discourse vis-à-vis particular axes of difference, concern, and controversy around issues in the situation of inquiry. The key point is that in SA, the situation itself becomes the fundamental unit of analysis (Clarke, 2005, pp. 21–23, 71–73; http://www.situationalanalysis.com).

All three kinds of maps work well for solely interview-based to multisited research projects. They are intended as supplemental to traditional GT analyses centered on action. Instead, these maps center on elucidating the key elements, discourses, structures, and conditions of possibility that characterize the situation of inquiry. The maps can be used to study documents and discourses, visual and historical materials, and so on (merci, Michel). SA thus offers the possibility of analyzing policy and larger scale issues often considered beyond the purview of GT and (alas) of symbolic interactionism (though I never agreed).

Several lovely honors have been associated with this book. First and foremost, it won the 2006 Charles Horton Cooley Distinguished Book Award from the SSSI, a precious moment in my career as an interactionist. The rights to produce a modified Chinese translation were granted in 2008, and in 2011, a German translation will appear from Verlag für
Sozialwissenschaften. This was graciously organized by Professor Reiner Keller of the University of Augsburg, whose interests are especially in discourse analysis. The inexhaustible qualitative research organizer Jan Morse held a “Grounded Theory Bash” including a dialogue among the second generation that tracks and elucidates the different kinds of GT in circulation today (Morse et al., 2009). Norm Denzin (2007) describes them well: the Glaserian as objectivist, the Strauss and Corbin version as systematic, the Charmaz version as constructivist, and my own as situationist. And last though far from least, I regularly offer a workshop on SA at the Congress on Qualitative Inquiry organized by Norm. I promote this conference relentlessly as its transnationality is truly remarkable and politically significant for the future.

When I joined the faculty at UCSF, I joined an amazing qualitative research tradition and community of scholars. In addition to the old timers in sociology when I arrived (Anselm, Ginnie, and Lenny), remarkable nurse scholars included Patricia Benner, Afaf Meleis, Julienne Lipson, and Kit Chesla. A bunch of newer faculty now also teach in this area: in sociology, Howard Pinderhughes and Janet Shim; in nursing, Holly Kennedy, Susan Kools, Carol Dawson-Rose, Roberta Rehm, and Janice Humphreys. While we are certainly marginalized both in nursing and on the campus more broadly, we are also a very well-established and renowned tradition nationally and internationally – widely recognized in our little niche.

Next for me in this trajectory is a paper, “Taking the Nonhuman Explicitly into Account,” that follows through on earlier STS emphases on the nonhuman. Given increasing knowledge of the complexity of life forms, the human/nonhuman divide now also needs to be problematized. I am also working on a second edition of Situational Analysis. Last, Kathy Charmaz and I are going to coedit four volumes of previously published work on Grounded Theory and Situational Analysis (in press). This, among other things, will occupy both of us in our segues into retirement.

Symbolic Interaction-Ists and -isms

Episodically, I have pursued projects and published papers centered on symbolic interactionism and/or interactionists. In 1991, Ginnie and I hosted the Couch Stone Symposium at UCSF, an intellectual delight but logistical nightmare. Next were events and publications memorializing Anselm after his death in 1996, including coediting with Leigh a special issue of Symbolic
Interaction 21(4) with papers by scholars from four countries. I also organized an ambitious Anselm Strauss website (http://sbs.ucsf.edu/medsoc/anselmstrauss/index.html). Fritz Schutze, who recently retired from Magdeburg University in (the former East) Germany, organized a multiday conference in Magdeburg in honor of Anselm’s work in 1999, inviting Fran Strauss, Carolyn Wiener, and myself. Scholars from Germany, Austria, and Poland attended, including Professor Kris Koniecki who had been a postdoc at UCSF and has since started the online journal Qualitative Sociological Review. In Magdeburg, I presented a very early vision of SA.

We have had two Anselm Strauss Colloquia at UCSF to date, including international guests Annemarie Kesselring from Switzerland and Fritz Schutze. Norm Denzin suggested we publish some of these papers on the legacies of Anselm Strauss and grounded theory in Studies in Symbolic Interaction (2005, 31, pp. 61–174). My own essay was triggered by a request from French feminist scholars to reflect on gender in Anselm’s life and work for an edited book covering all the major social theorists. After considerable reflection, I wrote about both gender and race as these are intertwined in the United States and not easily teased apart analytically. I argued that it was both Barney’s and Anselm’s goals of writing formal theory within a 1950s American sociological imaginary that led them to assert that gender and race had to “earn” their way into a GT analysis. Neither was considered central to sociology in their day. But also neither Anselm nor Barney demonstrated openness to such ideas in their later years as the discipline itself changed.

Leigh and I also wrote on interactionist STS for the Handbook of Symbolic Interactionism, edited by Larry Reynolds and Nancy Herman, and on social worlds as a theory–methods package for the Handbook of Science and Technology Studies (Clarke & Star, 2003, 2008). And in 2002, I had the great honor of winning the Feminist Mentor Award of the SSSI.

My most recent work drawing upon interactionism has been part of the affective turn across the social sciences and humanities (Adams, Murphy, & Clarke, 2009). We argue that one defining quality of our current moment is its characteristic state of anticipation, of constantly thinking and living toward the future. One of the key dimensions is abduction, the requisite tacking back and forth between future, past, and present, and framing and managing templates for producing the future. We had a Workshop on Anticipation sponsored by the UC Humanities Research Institute in 2011 that will include famous interactionists such as Patricia Clough and Jackie Orr. My paper revisiting the work of pragmatist philosopher C. S. Peirce is titled The Injunction to Keep on Guessing. I am trying to see whether and how abduction
can work as a concept in a history and theory of change, drawing upon its processual looping back and forth and future-orientedness.

CIRCLING BACK AND RECLAIMING IDENTITIES

I have borrowed this subtitle from Carolyn Ellis’s thoughtful autobiography and it reminds me of organizing the website on Anselm. Like his, my pursuits had multiple trajectories sustained however episodically over time. Returning to a particular trajectory does feel like reclaiming an identity temporarily suspended while “doing other things.” I also agree with Carolyn (this volume) that “Organizing chaotic and contradictory details of a life ‘lived’ into a story ‘told’ sometimes feels like being in a dream state, halfway between sleep and wakefulness.” Parts of this autobiography simply jumped onto the page, others I have agonized over endlessly.

Since 1995, due to disability, I have had to limit my professional commitments. But thanks to email, serving on advisory boards for new initiatives such as the international journal EASTS: East Asian Science, Technology and Society has been a special pleasure. For many years I have also served on the editorial boards of Social Studies of Science, Science, Technology and Human Values, and Symbolic Interaction. Most recently, I became a coeditor of BioSocieties: An Interdisciplinary Journal for Social Studies of Life Sciences whose mission is to expand conversations between social and life sciences. While often invited to Europe, only lately have I worked with colleagues and students from Taiwan, South Korea, and Japan. I relish the increasing transnationality of SI, STS, and my career, and will continue to sow interactionist seeds in faraway places.

I am also experiencing discomforts. While very very proud of my accomplishments and even shocked by them in many ways, I find myself increasingly uneasy in my life vis-à-vis amour propre. Fred Neuhouser (2008), who was working at Bellagio on this topic when I was there, sensitized me to some issues, and being married to a Lacanian psychoanalyst continues to open my eyes to others. Having won teaching awards, I am less comfortable in the classroom. When lauded, I question such compliments. While I have especially enjoyed the weight being a senior scholar carries in terms of writing recommendations and shaping projects and events, I also sometimes feel like I take up too much space. I suspect not feeling fully deserving of such success or fully comfortable in it is deeply connected to class and gender expectations. Nothing from my background even hinted that I would become a transnationally recognized scholar. And there have also been times when
being an uppity feminist woman met with a chilly reception. I am sure these issues will remain lively in my consciousness as I segue into semi-retirement.

One of the things I have especially cherished is Anselm’s telling me, as I finished my PhD at 40, that he had not written anything worth reading until he was 40. He had so many ways of telling me to “Go for it!” I did, and it has been very, very good indeed. Interestingly, writing both Leigh’s obituary and this autobiography over the past months has been very helpful in coming to terms with my anticipated retirement. This lovely opportunity to reflect on my life and work toward their end feels exquisitely sweet – and also makes me experience the fragility and preciousness of it all afresh. Thank you.

NOTES


ACKNOWLEDGMENT

Special thanks to Norm Denzin and Lonnie Athens for inviting and coordinating this contribution, and to Carolyn Ellis for sharing her forthcoming work. I also much appreciate comments from Ginnie Olesen, Monica Casper, and Allan Regenstreif.

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BECOMING A MEAD SCHOLAR:
RECALLING MY INTELLECTUAL JOURNEY

Gary A. Cook

ABSTRACT

I have been asked by Professor Lonnie Athens to shed light upon those parts of my academic career that may be of interest to sociologists working within the tradition of symbolic interactionism. With this in mind, the present essay offers an account of how I became a scholar whose main focus has for many years been the philosophy and social psychology of George Herbert Mead (1863–1931).

The remembered “pasts” we carry around with ourselves, George Herbert Mead claimed in one of his published articles, “are in great part thought constructs of what the present by its nature involves, into which very slight material of memory imagery is fitted” (Mead, 1964, p. 348). Certainly this is true of the past I have tried to recall and portray in the essay that follows: the memory imagery involved here is slight indeed, but I have been aided by a present that contains a large file of letters and other documents accumulated over the years. These materials give me some measure of confidence that the brief portrayal of my past I set forth in this essay is
reasonably accurate. My task here is also made easier by the fact that the story I attempt to tell is directed toward a particular purpose: I have been asked by Professor Lonnie Athens to shed light upon those parts of my career that may be of interest to sociologists working within the tradition of symbolic interactionism. With this in mind, then, I offer the following account of how I became a scholar whose main focus has for many years been the philosophy and social psychology of George Herbert Mead.

EARLY EDUCATION

My earliest memories date from the mid-1940s, when I moved with my parents and younger brother to the small town of Postville in the northeastern corner of the state of Iowa. There my father was to serve for many years as Superintendent of the public school system and my mother as a librarian for the new elementary school – once it was built in the mid-1950s. I began second grade in the fall of 1946 and remained a student in the local schools until I graduated from high school in 1957. My recollections of these precollege days have mainly to do with early friendships, bicycling and camping, serving as a newspaper carrier for the Des Moines Register, earning Boy Scout merit badges, engaging in school sports, experimenting with amateur radio, learning to play the trumpet, becoming a member of the high school band, and singing in both school and church choirs.

This hometown of my youth, it is worth noting, has in recent years found itself the object of unwanted publicity in numerous newspaper articles and two books (Bloom, 2000; Grey, Devlin, & Goldsmith, 2009). The main causes of this phenomenon were the purchase of the local meat packing plant by a Brooklyn-based company specializing in the production of kosher meats, the hiring by this company of many undocumented immigrant workers, and a surprise 2008 raid on this plant by officials representing the federal departments of Homeland Security, Labor, and Justice. In the course of this raid a total of 389 immigrants working in the plant were arrested; many of these were subsequently convicted, sentenced to five-month prison sentences, and then deported. Moreover, the CEO of the plant was later convicted on various charges of fraud and sentenced to 27 years in prison.

I have followed the news stories about these events with great interest in the last 10 years or so, not only because they have focused upon my former hometown but also because many years ago I worked briefly in the hide cellar of the Postville packing plant when it was under other ownership.
During one of the summer vacations of my college years, I was occasionally employed as part of a Friday night crew that pulled a thousand or so cattle hides off a salt pack, shook off the salt, folded and tied each of them into a bundle, and then loaded them into a box car. This brief exposure to the odors and hard physical labor of working in a packing plant was one of many things that convinced me I should pursue an academic career!

After graduating from high school in 1957, I began my college studies at Drake University in Des Moines, IA. There I enrolled in the college of fine arts as a music major with vague ideas of becoming either a professional trumpet player or a high school music teacher. This plan lasted only a semester or two before I discovered that I was vulnerable to serious bouts of nervousness or “stage fright” when performing solo at required student recitals. Because of this problem, which at the time seemed insurmountable, I dropped my music major and enrolled in the college of liberal arts. But I continued to enjoy participating in both the college concert band and the college choir for a number of semesters during the remainder of my college career. Moreover, I spent two pleasant summers during this period serving as a choir director for the program called “A Christian Ministry in the National Parks” – first at the Old Faithful Lodge of Yellowstone National Park (1960) and then at Crater Lake National Park in Oregon (1961).

I have a hard time recalling exactly when and how it was that I ventured into philosophy as my new major field of study once I had given up a major in music. As nearly as I can now remember, however, I had come to Drake with at least some interests that could have been classified as “philosophical.” I had, for instance, often engaged in discussions with high school classmates about topics related to the philosophy of religion; furthermore, at some point during my last year of high school I had begun to enjoy the writings of Ralph Waldo Emerson in two old volumes of his essays that had been passed along to me by friends of my parents. But I had no clear understanding at that time of what philosophy as a discipline really involved. It was only once I had arrived at Drake and begun to have discussions about some of these matters with more mature students that I learned that a person with interests such as mine should take some courses in philosophy.

My formal study of philosophy at Drake began in the fall of my sophomore year when I enrolled in a course in Ethics. The next semester I took an Introduction to Philosophy course and a course in Logic; I also declared an academic major in philosophy. In my junior year I took courses in Ancient and Modern Philosophy along with seminars on Kierkegaard, Contemporary Analytical Philosophy, and Advanced Logic. Finally in my senior year I took courses in Contemporary European Philosophy and the Philosophy of
Science, along with a seminar on Ethical Theory. All of these philosophy courses were taught by Professor Donald Keyworth, to whom I give credit for exposing me to a good deal of philosophical literature and teaching me what it meant to think philosophically. In particular, Keyworth gave me a strong background in ethical theory and the philosophy of religion – two of his interests that were also my own at the time. The second of these interests was reinforced by the religion courses I took with Professor Dale Miller, who introduced me to the philosophical theology of Paul Tillich and Reinhold Niebuhr. It was due largely to the efforts of Keyworth and Miller that I decided during my junior year to pursue a graduate degree in philosophy with the aim of becoming a college teacher. Thanks to the background I had acquired in my undergraduate courses I was able to score well on the graduate record exam in philosophy during my senior year, and this encouraged me to apply to some of the better graduate programs in philosophy. I recall that I was accepted for admission by several of these schools, but only Yale University offered me a sizeable scholarship – so it was there that I chose to go in order to continue my philosophical education.

It may interest sociological readers of this essay to learn that I gained at least some exposure to their academic field during my days as an undergraduate student. I took an Introduction to Sociology course in the fall of my junior year and a course on Race and Minority Groups during the fall of my senior year. It was partly as a result of this latter course that I and several student friends engaged in our own small and unapproved sociological research project during late October of 1960. We had heard rumors for some time that black students attempting to rent privately owned housing near the University often encountered what they took to be discriminatory responses from prospective renters. In order to put this rumor to an empirical test, several of us devised the following experiment. First, two of us made a visit to the appropriate University office in order to obtain information about off-campus housing suitable for a pair of male students. We then made a list of nearby apartments that sounded as if they might be worth a visit. The owners of each of these apartments were called upon at different times during a two-day period by two pairs of students, each professing to be seeking a rental apartment for the coming semester. One of these pairs consisted of two well-dressed white students, Skip Andrew – a friend who later achieved some degree of notoriety as an activist lawyer in the city of Chicago (see Hass, 2010) – and me; the other pair consisted of two well-dressed African-American students, Roy King and Ed Luton – student friends whom Skip and I had met in one of the men’s dormitories while we were campaigning for election to the student
government earlier that year. Not surprisingly, there was a considerable difference between the responses given to the two pairs of students, and it was clear from these responses that a high proportion of the University-approved renters we interviewed were strongly disposed to engage in discrimination against the black students.

This experiment provided us with strong supporting evidence when two of us later argued, as part of our membership on Drake’s Student-Faculty Council, that the University should require some kind of nondiscriminatory pledge from private renters who wanted to be included on the off-campus housing list maintained by the University’s housing office. Our proposed requirement, however, turned out to be somewhat radical for the early 1960s, even at a Midwestern university that was presumably opposed to racial discrimination in student housing. Some members of the Drake faculty were hesitant to make such a demand of private housing owners, and many members (and even some national officers) of Drake’s social sororities and fraternities were concerned that their own housing units might become the next target of such a requirement. The upshot of this opposition was that we did not succeed in getting University approval for our nondiscriminatory proposal during the 1960–1961 school year; seven years later, however, this matter was resolved by the passage of the federal Fair Housing Act of 1968, which made it illegal to discriminate on the basis of race in the sale or rental of private housing.

I should also note at this point that it was in connection with the controversy just mentioned that I first became well-acquainted with my future spouse (Marilyn Treman). She, too, had come to Drake (a year after I did) to major in music – in her case as a pianist and prospective elementary music teacher; she, however, did not change her mind about this plan but stuck with it throughout her college career. Although I had earlier become acquainted with her through our common participation in the Drake Choir, it was not until our shared year on the Drake Student-Faculty Council (especially during the lengthy period of deliberation this body devoted to the issue of discrimination in off-campus housing) that I came to know her well. I eventually talked her into accepting a teaching job in the New Haven area following her graduation, and we were married in the late summer of 1963.

**YALE UNIVERSITY GRADUATE SCHOOL**

I began my graduate student days at Yale in the fall of 1961, after driving from Iowa to Connecticut with a classmate from Drake (Larry Herman) who had been accepted as a first-year graduate student in the Yale
Department of Sociology. Larry and I were roommates during our first year of graduate school, living in an apartment at 269 Dwight Street – a location within easy walking distance of the University but also on the edge of a deteriorating neighborhood in which the early evenings were often punctuated by loud arguments and occasional fights. I like to think that living in this apartment for a year with an aspiring sociologist contributed at least a little to my later interest in sociological theory – but that is perhaps just a case of “creative” remembering on my part. Larry, in fact, abandoned his sociology studies during his second year at Yale and later made a career for himself working for one of the departments of the federal government in Washington, DC.

My initial week or two at Yale involved a meeting with my fellow first-year graduate students in philosophy, all of whom I subsequently found to be quite impressive, and the opportunity to take qualifying exams in French, German, or Latin. I had spent a fair portion of the preceding summer brushing up on French (which I had studied for two semesters during my sophomore year at Drake) so that I would have a reasonable chance of passing my reading exam in that language. The French exam proved to be relatively easy, so I decided to try the German exam given a couple of days later. (I had taken two semesters of German during my senior year at Drake, but was not at all optimistic about my chances of passing this exam.) I was pleasantly surprised to learn in a week or so that I had passed both the French and the German exams, thereby getting all the language requirements for my advanced degree taken care of before regular classes began.

The other thing I recall from the beginning of my graduate school career was the discovery of the Chapel Choir at Yale’s Battell Chapel. I learned early in the fall that there would soon be tryouts for membership in this Choir; the Choir would be all male (this was in the years before Yale became coed), would consist of about 40 members, and would typically sing two anthems at the Chapel service every Sunday morning during the school year. Furthermore, all choir members would receive a modest monthly honorarium for their time and singing. Based upon the choral singing I had done in high school and during my years at Drake, I immediately decided to audition for this opportunity. I was rewarded by being accepted for membership in an outstanding musical group, which at that time was directed by Professor Charles R. Krigbaum, organist and member of the faculty at the Yale School of Music. The experience of singing great classical church music in this excellent choir for several years, together with the opportunity to hear Yale Chaplin William Sloan Coffin and a series of other outstanding preachers each month, was one of the high points of my time at Yale.
During my years at Yale I either took for credit or audited courses in Fundamentals of Deductive Logic (Professor Belnap), The Metaphysics of Aristotle (Professor Wells), Kant’s Theory of Knowledge and Metaphysics (Professor Smith), Meaning and Truth in Religion (Professor Christian), Intermediate Symbolic Logic (Professor Anderson), Plato: The *Philebus* and *Statesman* (Professor Brumbaugh), The Metaphysics of Leibniz (Professor Wells), Kant’s Ethics (Professor Schrader), The Logic of Entailment (Professor Anderson), Classical Problems in the Theory of Knowledge and Metaphysics (Professor Sellars), Philosophy of Religion (Professor Smith), Recent Ethical Theory (Professor Schneewind), Hegel: *The Phenomenology of Mind* (Professor Weiss), Ethics and Existence (Professor Schrader), Peirce (Professor Bernstein), The Function of Models in Science (Professor Hesse), and Kant’s Theory of Knowledge (Professor Sellars). In addition, I served as a teaching assistant during my third year for two different undergraduate philosophy courses. I found all of these experiences immensely stimulating, but by the time I passed my preliminary examinations for the PhD in May of 1963, I was more than ready to move on to the writing of a dissertation, the final stage of my career as a Yale graduate student.

I had by then decided that I wanted to do a dissertation related in some way to the history of American pragmatism, but on a topic that had not already been the object of a great deal of scholarly study. Beyond these considerations, however, my choice of an actual topic was in large part the result of a lucky accident. The accident took place at some point during my second year of graduate study when I was at home in Iowa on vacation. As I recall it, my younger brother and I were riding home with our parents at the end of a family day trip when we all stopped to check out the used books in a second-hand store. One of the items my brother (then an undergraduate student at the University of Iowa) purchased during this shopping spree was a used copy of George Herbert Mead’s *Mind, Self and Society* (1934). Later, I traded him one of my own acquisitions for the Mead book and read it to see what it was all about. I found Mead’s account of the social genesis of the self to be especially interesting, and subsequently did some research to see what else he had written and how much scholarly work had been done on his thought. It was this research that led me to formulate plans for a dissertation on Mead’s philosophical and social psychological work. The eventual title of this dissertation was “The Self as Moral Agent: A Study in the Philosophy of George Herbert Mead.”

My dissertation proposal was approved in early October of 1963, and I soon secured the services of Professor Robert Brumbaugh as my dissertation advisor. I asked Brumbaugh to serve in this capacity mainly because I had
enjoyed working with him in his Plato course, but it turned out that he was an appropriate choice also because of his own earlier relationship to the University of Chicago – where Mead had spent most of his professional career. Brumbaugh had been a preparatory student at the University’s Lab School (with which both Dewey and Mead had been involved during their early years at Chicago) and had earned his BA and PhD degrees at the University (although this was a number of years after Mead’s death).

Professor Brumbaugh proved to be an ideal advisor for my dissertation work. He kept me on a regular schedule, read my drafts of dissertation chapters in a timely manner, and always offered me counsel that was both supportive and helpfully critical. One of the chief bits of advice he gave me was that I should not expect the dissertation to be the best piece of research I would ever do; the main thing was just to finish it up so that I could then get on with the rest of my academic career. With this in mind, I completed the dissertation during the spring of 1965 and survived my oral examination on it during the early summer of that year. I was too late to graduate in June of 1965, but was technically granted my degree in December of that year. I received my actual PhD diploma at the Yale Commencement ceremonies held in June of 1966.

Meanwhile I entered the job market seeking a college or university teaching job during the final months of 1964. At that time teaching jobs were fairly plentiful and the whole system of hiring was much less formalized than it became in later years. The Yale Department of Philosophy maintained a bulletin board upon which were posted job advertisements it had received from various colleges and universities throughout the country. Those of us on the job market would regularly check this bulletin board and then write to the appropriate school to inquire about jobs we found of interest. I had by this time decided that I would seek a teaching job at a good liberal arts college rather than at a large university, and that I would prefer a job somewhere in the Midwest. Sometime during the late fall, I found on the departmental bulletin board a letter from Professor Joseph Barrell, Chairman of the Department of Philosophy at Beloit College in Wisconsin. Barrell was himself a graduate of Yale and his father had been a distinguished geologist on the Yale faculty. He made it clear in his letter that both he and his departmental colleague at Beloit (Professor Scott Crom) had received their PhD’s from Yale and that he would strongly prefer to hire another person from the same school. (Yes, faculty hiring worked differently in those days!) As soon as I saw this letter, I wrote to Professor Barrell indicating my interest in the new position opening up at Beloit. I also told him that I would be coming back to
the Midwest for the Christmas break. He immediately wrote back and invited me to stop in for an interview sometime after the Christmas holidays. I did so, taking along my wife for the visit, and the result was that I was given and subsequently accepted a job offer from Beloit during the early part of January 1965. I give much of the credit for this early success in finding an academic job to Joe Barrell’s bias in favor of Yalies, his obvious enjoyment upon meeting my wife, and the fact that the Dean of Beloit College at that time was Professor William L. Kolb, a sociologist who informed me during my job interview that his very first academic publication had been an article on the uses of the “I” and “me” concepts in George Herbert Mead’s theory of the self (Kolb, 1944).

BECOMING A FACULTY MEMBER AT BELOIT COLLEGE

When I began my teaching career at Beloit College in the fall of 1965, my only prior teaching experience was the two semesters I had served as a teaching assistant at Yale. My initial period at Beloit was thus a time in which I had to design my own courses, learn how to present course material and lead classroom discussions in an effective manner, function as an academic advisor, serve on faculty committees, and otherwise participate in faculty governance. I, like most other beginning faculty, found these to be demanding tasks that occupied a large portion of my available time for several years. Eventually, however, I developed a repertoire of courses I taught on a more or less regular basis. At the heart of this repertoire were Logic, Introduction to Philosophy, and History of Modern Philosophy. To these I gradually added courses in American Philosophy, Philosophy of Science, Biomedical Ethics, and more advanced seminars on such topics as The Philosophy of Kant, Pragmatism, and Nineteenth Century European Philosophy (Hegel, Marx, Kierkegaard, and Nietzsche). Occasionally, I even got to offer seminars on William James and George Herbert Mead.

None of these courses were graduate courses, since Beloit College (enrollment typically around 1,200 students) has always been a strictly undergraduate liberal arts college. On the other hand, almost all of my students – especially those in the more advanced courses – were able to digest fairly challenging philosophical material, and most of them could write good analytical papers on topics related to assigned course readings. The best students were also capable of carrying out independent research...
projects on specialized topics of their own choosing. Moreover, since some of the courses I taught regularly were interdisciplinary in character, I often enjoyed teaching very able students from departments other than my own. This was especially true of the psychology majors and the pre-medical students who signed up for the course on Biomedical Ethics or the science students who elected courses in Logic or the Philosophy of Science. Teaching philosophy at Beloit was thus intellectually satisfying even if it did not involve the supervision of graduate students working on advanced degrees.

A similar point can be made about my interaction with faculty colleagues. I was fortunate throughout my period of active teaching at Beloit College (1965–2004) to enjoy stimulating and cordial relationships with all the faculty members in my own department – both when it was a Department of Philosophy and when it later became a Department of Philosophy and Religious Studies. But this department (like most of those at Beloit) was always fairly small – ranging in size from three to five or six faculty members over the course of my teaching career. It was therefore natural for most faculty members to develop collegial relationships with those teaching in departments other than our own. In my case these relationships involved regular interaction with faculty from such departments as Political Science, Sociology, Geology, Economics, Education, Anthropology, and English. I was also privileged to serve on many occasions as an elected member of various all-college committees, the most important of which had to do either with the formulation of overall academic policies or with decisions regarding the awarding of faculty tenure and promotions. Service on these committees was a satisfying part of my academic life because of the significant role they played in the governance of the institution and because they brought me into close intellectual contact with faculty leaders from a variety of academic disciplines.

I should also mention here that my life at Beloit College and in the city of Beloit soon came to involve activities that had nothing directly to do with college teaching or service on faculty committees. First of all, my wife and I began raising a family not long after we arrived in Beloit: our daughter was born in 1968 and our son in 1972. Second, in the fall of 1972, I began a part-time job as the Minister of Music at the First Presbyterian Church of Beloit. This position gave me a chance to build upon the experiences I had gained as a church musician in high school, college, and graduate school: with the help of my spouse (the real musician in the family) I enjoyed directing the adult choir and arranging for other aspects of the Church’s music program for 37 years – until my retirement from this position in 2009. Finally, soon
after my arrival at Beloit College I began singing with the community chorus that rehearsed in the College Chapel and was directed by members of the Beloit College Music Department. This led to my meeting and becoming friends with Professor Eudora Shepherd, a member of the Beloit College Department of Music and a marvelous voice teacher. Eudie kindly offered to give me singing lessons sometime in the late 1960s, and ever since then I (like so many others who have come under her influence) have been greatly indebted to her because of what she managed to teach me about musicianship and the art of singing.

PURSUING A SCHOLARLY AGENDA

After several years of teaching, developing new courses, and learning the ropes of committee service, I began to feel the need to publish some scholarly articles based upon the dissertation research I had done as a graduate student. But whereas my dissertation had drawn heavily from Mead’s posthumously published books – *The Philosophy of the Present* (1932), *Mind, Self and Society* (1934), *Movements of Thought in the Nineteenth Century* (1936), and *The Philosophy of the Act* (1938) – I decided that this time around I would trace the development of Mead’s thought by examining the essays he had published in various journals throughout his lifetime. (My recollection is that this decision was inspired in part by the appearance in 1969 of Darnell Rucker’s excellent book on *The Chicago Pragmatists*.) Accordingly, I began by looking carefully at a series of essays Mead published between 1900 and 1913, essays in which he first set forth the reflections that led to his mature social psychological theory. This research eventually resulted in my first published article on Mead: “The Development of G. H. Mead’s Social Psychology” (1972). The publication of this essay in the *Transactions of the Charles S. Peirce Society (A Quarterly Journal in American Philosophy)* led in short order to an invitation to write a substantial review of Miller’s book on Mead (Miller, 1973) for that journal (Cook, 1974), and then also to an invitation from its editor, Peter Hare, to serve as a member of its team of consulting editors.

In this connection I want to acknowledge the wonderful support I was given by Peter Hare from the time of my first correspondence with him in the early 1970s until his death in January of 2008. Peter not only encouraged me with his kind and perceptive comments about various essays I had written on Mead’s thought, he also suggested journals to which I might submit them if they did not seem quite appropriate for the pages of
the Transactions. Furthermore, he enthusiastically urged me for many years to write a book-length study of Mead’s work, and when I finally managed to complete such a manuscript it was he who ultimately paved the way for its publication by putting me in touch with Richard Martin of the University of Illinois Press. Peter was, in my judgment, ideally suited by both his keen intellect and his warm personality to be the editor of the primary journal for the publication of scholarly essays on American philosophy.

At about the time I was working on my review of David Miller’s book for Peter Hare, I was selected to participate in an eight-week National Endowment of the Humanities Summer Seminar on the topic “Philosophical Analysis and Psychological Theories of Man” under the direction of Professor Theodore Mischel at the State University of New York campus in Binghamton, NY. My work for this stimulating seminar, held during the summer of 1974, involved reading and discussing with 11 other college teachers a number of works by such thinkers as Freud, Skinner, and Piaget. To meet one of the requirements of this seminar I composed a second essay on Mead’s social psychological theorizing – this one at first entitled “G. H. Mead’s Alleged Behaviorism” but subsequently toned down a bit and re-titled “G. H. Mead’s Social Behaviorism.” After a good deal of revision, this essay was finally published as the lead article in an issue of the Journal of the History of the Behavioral Sciences (Cook, 1977).

While waiting for the publication of the essay just mentioned, I turned to the task of composing another historically oriented article on Mead’s intellectual development, this one having to do with Alfred North Whitehead’s influence upon Mead’s thought – as seen in some of Mead’s later published essays and in such posthumous works as The Philosophy of the Present and The Philosophy of the Act. It was to better understand this influence that I began for the first time to investigate the many letters and other unpublished materials housed in the George Herbert Mead Papers at the Department of Special Collections of the Regenstein Library at the University of Chicago. The first results of this inquiry were published, after much analysis of these papers and of Whitehead’s philosophical writings, as “Whitehead’s Influence on the Thought of G. H. Mead” (Cook, 1979).

With the publication of this third article on Mead’s thought, I felt that I had mastered the development of both his social psychological theorizing and his later inquires related to the philosophy of nature. Furthermore, through my research at the University of Chicago I had started to become acquainted with most of the other dimensions of his life and philosophical work. It therefore seemed to me that it would not be a difficult task to continue this line of research, building upon what had already been done by
Darnell Rucker and David Miller, in order to write a complete book on the development of Mead’s thought and his activities as an important member of the Chicago School of pragmatism.

I would come to see later that I had seriously underestimated the difficulty of this task and the amount of time that would be required to complete it! I would also learn, however, that I found it immensely satisfying to engage in sustained archival research and thereby arrive at a well-documented interpretation of the life and intellectual development of an important pragmatic thinker.

One of my first tasks in this regard was to compose a long essay tracing the early development of Mead’s thought during his years as an undergraduate student at Oberlin College and Harvard University, a graduate student in the German University system at Leipzig and Berlin, and finally as a beginning university teacher at the University of Michigan. In this connection I made a research trip to the Bentley Historical Library at the University of Michigan, and subsequently secured the services of two former Beloit College students, Susan Cook and Randy Bell, to look up further materials for me at that library. In addition, I visited the Oberlin College Library and the Houghton Library of Harvard University to do relevant archival research. Finally, I spent a good deal of time looking through the early correspondence of Mead and his college friend, Henry Castle, at the Regenstein Library of the University of Chicago. All this archival research eventually led to the composition of the first two chapters of my book: “Early Life and Letters: Part 1” and “Early Life and Letters: Part 2.” At about the same time I also composed another substantial essay dealing with Mead’s persistent use of the concept of “taking the attitude of the other” as a fundamental notion in his social psychological theorizing. This essay, after numerous drafts and revisions, finally became Chapter 6 (“Taking the Attitude or the Role of the Other”) in my book.

Early in 1983, I was invited by Professor Hans Joas of the Max Planck Institute in Berlin to present a paper at a conference on Mead’s thought to be organized by the Research Committee on Sociological Theory of the German Sociological Association for the summer of 1984. Joas told me in his initial letter that he had read my PhD dissertation, as well as the several essays on Mead I had since published in various philosophical and sociological journals. He also informed me that he was the editor of the two-volume German edition of Mead’s collected essays and was the author of a recent German book on Mead’s work. He kindly informed me that although the language of the conference would be German, it would be
perfectly all right if I were to give my paper in English. I gladly accepted his invitation and prepared a paper that was presented in English, even though the conference program listed the title as “Mead’s Theorie der Moral.” Professor Joas not only served as a gracious host for me and my wife at this conference, taking us on a tour of East Berlin once the presentations were over, but also later translated a revised version of my paper (Cook, 1985) so that it could be published in German in a postconference anthology titled Das Problem der Intersubjektivit"at (Joas, 1985a).

In subsequent years I have occasionally met with Professor Joas when he has been in this country to teach part of the year at the University of Chicago or the University of Wisconsin. Joas is a prolific writer on sociological theory who has done much to promote interest in Mead’s ideas, both in Germany and in the United States. I was pleased to discover in 1985, when I was asked to write a review (Cook, 1986) of the English version of Joas’s book on Mead (translated under the title G. H. Mead: A Contemporary Re-examination of His Thought) that the book contained some very kind remarks about my own earlier writings on Mead’s thought (Joas, 1985b).

From 1985 onward I continued to do research and writing for various chapters of my Mead book whenever the demands of teaching would allow this. I spent a good deal of time, for instance, digging through relevant collections housed in the Regenstein Library, the Chicago Historical Society Library, the Newberry Library, and the Library of Health Sciences at the University of Illinois at Chicago in search of materials related to Mead’s involvement in various social reform activities during the first 20 years of his career at the University of Chicago. This work led eventually to Chapter 7 of my book: “Mead and the City of Chicago: Social and Educational Reform.” In addition, I revised and supplemented earlier versions of an essay on Mead’s ethical thought to arrive at Chapter 8: “Moral Reconstruction and the Social Self.” I also wrote a transitional chapter on Mead’s early move from his philosophical and psychological studies to an interest in social psychology (Chapter 3: “From Hegelianism to Social Psychology”) and a concluding chapter (Chapter 10: “Mead’s Social Pragmatism”) about the ways in which his emphasis on social conduct manifested itself in his pragmatic philosophy. Finally, I added a postscript about an epic battle that marred the final few years of his career at the University of Chicago (“Epilogue: Mead and the Hutchins Controversy”). A version of this postscript was also presented as a paper at the 1991 meeting of the Society for the Advancement of American Philosophy held on the campus of the University of California in Santa Cruz.
This trip to Santa Cruz is strongly associated in my memory with meeting Professor Richard Robin (long-time coeditor of the *Transactions of the Charles S. Peirce Society*) and his wife on a flight from Chicago to San Jose, and sharing the car ride from there to Santa Cruz. It turned out that the Robins were close friends from graduate school days of a couple whose daughter (Ellen Phillips) and granddaughter (Anne Marie Frohn-mayer) had both been outstanding singers at Beloit College. Moreover, just as Mrs. Robin had on at least one occasion served as the piano accompanist for a major performance given by Ellen, so my wife had served as accompanist for Anne Marie during her junior and senior year recitals at Beloit.

**RECENT AND PROJECTED RESEARCH PROJECTS**

During the years since the manuscript of my book, *George Herbert Mead: The Making of a Social Pragmatist* (*Cook, 1993*), was sent off to press, most of my research and writing has had to do either with the pursuit of projects that occurred to me while I was working on this volume or with topics I was invited to address for specific occasions or by editors with specific needs. In the first of these categories I would place the editing and publication of two Mead essays I ran across while doing my research at the Regenstein Library of the University of Chicago. These are entitled, respectively, “George Herbert Mead: An Unpublished Manuscript on Royce and James” (*Cook, 1992*) and “George Herbert Mead: An Unpublished Review of John Dewey’s Human nature and conduct” (*Cook, 1994*). I would also place in this category my essay on “George Herbert Mead and the Allen Controversy at the University of Wisconsin” (*Cook, 2007*). This paper deals with a little-known episode in which Mead was asked by the journal named *Survey* to examine and evaluate a dispute that arose as a result of a review of the University of Wisconsin mandated by the state legislature in 1913, and carried out under the leadership of William Harvey Allen during the summer and fall of 1914. This essay allowed me to get to the bottom of an often-overlooked facet of Mead’s career, while at the same time indulging my passion for historical research in academic archives (this time at the University of Wisconsin in Madison).

I would locate in the same category as the essays just mentioned an article entitled “G. H. Mead on Human and Animal Consciousness” (*Cook, 2002*), which I wrote at the invitation of a former student (Christopher Koy) for a conference held during the summer of 2002 in the Czech Republic. This
paper seeks to present and evaluate a contrast that is often made in Mead’s essays and lectures on social psychology: he repeatedly holds that the consciousness exhibited by nonhuman animals, unlike that of mature human beings, involves no self-awareness. This essay has been published, along with the other papers presented at this conference, in a volume edited by Professors Ivo Budil and Marta Ulrychova, and printed by the University of Pilsen. I have also written a paper entitled “Mending Mead’s ‘I’ and ‘Me’ Distinction,” versions of which I have presented at two conferences but not yet succeeded in putting into a form wholly acceptable for publication. This paper is motivated by an attempt to correct what I have come to regard as an error in my 1993 book’s assessment of what Mead has to say about this distinction in various lectures and essays.

Finally, a number of my recent essays have been provoked by invitations from various editors to write papers addressing Mead’s views on various topics. Into this category I would place three general essays I have written about Mead’s thought for anthologies entitled *Classical American Pragmatism: Its Contemporary Vitality* (1999), *The Dictionary of Modern American Philosophers* (2005), and *A Companion to Pragmatism* (2006). In addition I have written another essay entitled “Whitehead’s Impact on the Thought of G. H. Mead” (obviously inspired by Chapter 9 of my Mead book) for the *Handbook of Whiteheadian Process Thought* (2009). Most recently, I have prepared for publication an essay entitled “Revisiting the Mead-Blumer Controversy” (Cook, 2011). I here attempt to clarify and evaluate the much-debated relationship between the social psychological ideas of George Herbert Mead and those of his main sociological champion and interpreter, Herbert Blumer. This essay, which began as a paper presented (at the invitation of Professor Lonnie Athens) as the Distinguished Lecture for the August 2007 meeting of the Society for the Study of Symbolic Interaction, is my attempt to address a topic of interest to those sociologists whose understanding of human social behavior is grounded in the tradition of symbolic interaction as this is understood by Mead’s student Herbert Blumer.

My next major project, once I have written a shorter essay on Mead’s ethical philosophy that I have agreed to complete by the end of October 2010, will involve revisiting the controversy briefly examined in the final section of my Mead book. There is, I think, much more of interest to be said about the flap between Robert Maynard Hutchins and the University of Chicago philosophy department in the years immediately following 1927. At any rate, I intend to explore the ramifications of this dispute much more thoroughly than I did in my original Epilogue and to do so in a number of different directions. If I succeed in finding a sufficient amount of additional
information about this matter I shall seriously consider the possibility of composing a book-length manuscript about the infamous conflict between Hutchins and the Chicago Pragmatists.

**CONCLUDING REMARKS**

In May of 2004 I retired, after 39 years of teaching at Beloit College, to become a professor emeritus. But, as the immediately preceding pages suggest, I have continued to enjoy the pursuit of a reasonably active research agenda during the years since then. The summary of my recent scholarly work provided in these pages, however, still leaves a number of relevant questions unanswered – the most important of which have been urged upon me in a recent email from Lonnie Athens. First, he asks, “What accounts for your infatuation with philosophy, and, more specifically, your obsession with George Herbert Mead?” And, second, “As you look back on your career would you now do anything different if you could do it all over again – including becoming a philosopher and devoting all your scholarly attention to Mead?”

Left to my own devices I probably would not characterize my relationship to philosophy as one of “infatuation” or my approach to George Herbert Mead as a matter of “obsession,” but I do at least confess to a “long-standing fondness” for philosophy (“the love of wisdom”) and a “very persistent interest” in coming to an understanding of Mead’s life and work! With these slight amendments to Professor Athens’ choice of words, I want to conclude this essay by attempting to answer his questions.

As suggested earlier, I think that my interest in philosophy grew in part from my earlier exposure to questions related to religion. But my younger brother and only sibling (Monte Cook) had no serious interest in questions of religion (to the best of my knowledge); yet he also earned a PhD in philosophy (University of Iowa) and went on to become a professor of that subject at the University of Oklahoma. So perhaps there was something else in our shared background that might account for both his and my interest in philosophy. I must confess, however, that I do not know exactly what this might be – other than the fact that both of us clearly acquired from our parents a great interest in reading and the academic life. Suffice it to say that at some point during my years as a high school student I began to take an interest in basic questions about religion and ethics; then as a college student I discovered that there was a recognized discipline (philosophy) devoted to the exploration of these and related questions; later, during graduate school,
I learned from my reading of Charles Peirce and the other American pragmatists that one could think of philosophy as employing a method of radically self-critical inquiry – a method that sought to arrive at the most defensible, but still tentative and fallible, answers to such questions. It was for philosophy understood in this manner that I acquired a great fondness, one that lasts until the present day.

Could I have found satisfaction in some academic field other than philosophy? Well yes, perhaps. I have already mentioned that I began my college career in the field of music, but then dropped my music major in favor of philosophy. My subsequent experience suggests that I might well have found great satisfaction in music if I had stuck with it a bit longer, perhaps switching from trumpet performance to vocal performance and/or choral directing. I simply did not explore these options carefully during the early part of my undergraduate career. Nevertheless, I have subsequently enjoyed the opportunity to do a good deal of vocal performing and choral directing as an avocation, while pursuing the teaching and writing of philosophy as a profession.

My failure to explore alternatives carefully during my undergraduate years also applies to the subjects of sociology and American history. I am not sure that I would have enjoyed sustained empirical research in sociology, but I know that I do have a strong interest in at least some areas of sociological theory. I have also discovered through my research on Mead and Chicago pragmatism that I have a considerable interest in selected aspects of American intellectual history. But, again, the research career I have already outlined has given me at least some opportunity to satisfy these interests. So, in answer to one of the questions posed above, I would say that on balance I do not regret my decision to pursue a career in philosophy rather than in some other field.

But what do I now think about my decision to devote so much of my scholarly attention to George Herbert Mead? If I had it to do over again, would I change this in any way? I must confess that it was not my original intention to spend so much time digging into the life and thought of one thinker; I thought I would simply say what I had to say about Mead and then move on to other matters. But one thing led to another and I found that doing the kind of careful research and thinking I wanted to do about Mead took up almost all the time I had available for scholarly purposes not directly tied to my teaching. Luckily, as I have previously indicated, I found that I greatly enjoyed archival research at the University of Chicago and elsewhere; moreover, in order to understand Mead’s intellectual development I found that I had to dig deeply into the thought of those who had
influenced him most (e.g., John Dewey, Alfred North Whitehead, and others) and also look carefully at his extensive involvement in social reform activities in the city of Chicago. My pursuit of an informed view of Mead's life work, in other words, exposed me to a whole spectrum of intellectual history that I found immensely interesting. So, in answer to the question “Would you devote so much of your scholarly attention to George Herbert Mead if you had a chance to do your career over again?” I think I can honestly answer, “Yes, I would.” The next time around, however, I would try to get my thoughts about Mead in order soon enough to have meaningful discussions with some of his most important University of Chicago students – including Charles Morris (1901–1979) and Herbert Blumer (1900–1987) – while they were still alive and available for consultation!

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**AUTHOR BIOGRAPHY**

**Gary A. Cook** is Emeritus Professor of Philosophy at Beloit College in Wisconsin. He is the author of *George Herbert Mead: The Making of a Social Pragmatist* and, most recently, of an essay entitled “Revisiting the Mead-Blumer Controversy.”
REVISIONING AN ETHNOGRAPHIC LIFE: INTEGRATING A COMMUNICATIVE HEART WITH A SOCIOLOGICAL EYE

Carolyn Ellis

ABSTRACT

This story tells a version of my life as an ethnographer and symbolic interactionist. From an early age, I was intrigued by how people interacted and created meaningful worlds for themselves and by my own motives, actions, thoughts, and feelings. Later, as a student of sociology, my eyes were opened to the macro forces that constrained, liberated, and influenced actions, identities, and performances. Eventually, I located myself on the margins of sociology, as I experienced the constraints of mainstream sociology and how this perspective limited what and how I could study and write. I was drawn to a wider interdisciplinary community of scholars who examined experience more concretely and emotionally, and I began to work comfortably in the spaces between social science and literature, self and other, research and story. I now view myself more as a writer communicating heartfelt stories for the purpose of opening up and evoking conversations and emotional responses from readers than a reporter giving an account of what she has seen, heard, and analyzed from...
a distance, a researcher who works with others rather than one who collects data on them. In my current collaborative witnessing project with Holocaust survivors, I have come full circle, connecting macrohistory and structure with personal storytelling and integrating my sociological eye with a communicative heart.

I lived as an ethnographer who viewed the world and her own subjectivity through a sociological and interactionist lens, even before I knew what any of these terms meant. As a child, I constantly asked, “What is going on here?” To find out, I asked questions about other peoples’ lives, listened in on adult conversations, observed how people interacted, and reflected on how they created meaningful worlds for themselves. I also was intrigued by my own motives and intentions and often reflected on my actions, thoughts, and feelings. Later my education and work in formal sociology opened my eyes to the more macro, structural, and cultural forces that constrained, liberated, and influenced people’s actions, identities, and performances (including my own). Still, my interests lay more in the people who act back on culture – their agency – than on the macro forces themselves.

Later in my career, positioned on the margins of sociology, I was able to see more clearly the constraints of mainstream sociology and how this perspective limited what and how I could study and write (Richardson, 1990). Then I connected with a wider community of scholars interested in examining experience more concretely, holistically, and emotionally, including body, feelings, spirituality, and lived experience in addition to interaction, structure, culture, and society. In this process, I began to work comfortably in the spaces between social science and literature, self and other, research and story. I began to view myself more as a writer communicating heartfelt stories for the purpose of opening up and evoking conversations and emotional responses from readers than a reporter giving an account of what she has seen, heard, and analyzed from a distance (Bochner, 2002). My goal became integrating my sociological eye with a communicative heart.

Responding now to the generous offer of Lonnie Athens and Norman Denzin to think again about my life as an interactionist sociologist, I wonder whether I have “news” to share. I don’t want what I write here to be old hat, simply a retelling of the stories I have told before in other autobiographical accounts (Ellis, 1995a, 2004, 2009). What can I offer you that you might use to think about life, career, loyalty to professional
associations, and the academic lenses through which you view your scholarship? What might I learn from revisiting and adding to my autobiography as I begin my 30th year as a professor? I remind myself that revisioning a life (Ellis, 2009) is never finished and that each telling has the possibility of uncovering unspoken details, connections, and resonating with new readers.

Organizing chaotic and contradictory details of a life “lived” into a story “told” sometimes feels like being in a dream state, halfway between sleep and wakefulness. There, details float in and out, and I grab onto them to try to make some sense of what is happening. Each time I think I have the plotline figured out, I am aware that I am selecting some elements and ignoring others. Some things I can’t seem to fit together; other things are like identically shaped puzzle pieces that can be connected in a variety of patterns. Similarly, as I write, I often wonder how much I have forgotten; question why I choose to tell some stories and ignore others; reflect on why I put events together the way I do; wonder how much of the life I write is plucked from memory, how much is constrained by stories I already have written, how much is constructed now in the writing process, how much simply imagined. “There is no fixed truth of the past to which we can gain access; everything we say and mean and make of the past is a form of revision” (Bochner, 2007, p. 206; see also Ellis, 2009). That being said, I invite you to “dream-share” with me in the essay that follows (Nolan, 2010).

**A SMALL-TOWN RURAL SOUTHERN UPBRINGING, 1950–1969**

Born in 1950, I grew up in a farming area on the outskirts of Luray, Virginia, a small town of 3,000 people in the Shenandoah Valley, isolated from the rest of the world by the surrounding mountains. Ours was a small, homogeneous working-class community of farmers, piece-work factory employees, town and county employees, and those who serviced the tourists who came to admire and hike the Blue Ridge and Massanutten Mountains and walk through the Luray Caverns (primarily to see the “fried eggs” and hear “Oh, Shenandoah” played on the Stalacpipe Organ). Born and raised in this valley, my parents attended elementary school in one-room school houses and then took wage-labor jobs. After they married, my father started a small construction business and my mother kept his books. By the end of the booming 1960s, their construction business was considered the best in
the area, and we went from near poverty level to being able to afford many of the luxuries of life. Still we lived similarly to the working-class families that surrounded us – we bought the same things, only more and newer models, and our talk and stories continued to be about close-to-home events, day-to-day joys, and real-life tragedies.

I loved my life in this small-town community and there I developed an interest in examining and telling stories about rural communities on the periphery, small groups, interpersonal relationships, and emotions. These were the stories that would become the focus of much of my later sociological research. There wasn’t much to do in Luray except visit and talk with people, and gossip about what went on in our small town, and we did plenty of that. Telling stories provided an important way to pass the time and often our talk focused on concrete, mundane details. Illness and health issues always stimulated a lively conversation. Friends and acquaintances compared illness sites, doctors’ orders, prescriptions, and home remedies. Frequently we talked about the local environment, especially the weather – how it felt, what was predicted, especially if it called for rain or snow, or was unseasonably hot. Other times we talked about day-to-day concerns: what the neighbor was doing – where he placed the garbage, how he painted the house, how she dressed when going to town, and when the yard was mowed; who had her hair done this morning, was seen coming out of the liquor store, or driving a new car. Events signifying transitions or crises spread quickly through the community: who found a new love, got engaged, or started a new business; who died, separated or divorced, lost their minds, or went broke. Juicy gossip regarding violation of community norms – who was cheating on a spouse, ran off with another woman or man, got drunk and spent the night in jail, lost a job, or had a brawl – was shared with enthusiasm. We rarely, if ever, discussed abstract theoretical or philosophical issues, the political economy, culture, or society.

But growing up in Luray did not confine me completely to rural simplicity. Under the surface lay more sinister stories of race relations in the 1960s in small southern towns, a tension that led me later to focus on race relations in my studies and stories.

It’s 1969. She’s on her high school senior trip to Florida. She’s a white girl from a small, rural, southern town, attracted to Jesse, one of only two African American males on the journey. Prior to lights out the first night, students go for a walk on the beach. Immersed in talking about being Black in a White world, she and Jesse wander away from the others.

“You always remember you’re Black,” Jesse says. “People’s responses remind you.”

“What was it like growing up?” she asks.
“We had no money. My father left when I was a baby, so I was raised by my ma and grandma. Then my mother remarried. Once I woke up and my step-daddy had a butcher knife to my throat.”

“Oh, my God. What did you do?”

“I ran outside. In the freezing cold, with no shoes, in my underwear. I got frostbite on my toes.”

“What had you done to make him so angry?”

“Nothin’. He was drunk. And he was always jealous that my mother loved me more than him. That’s all. Just jealous.”

“We better go back in,” she says, noting that everyone else has disappeared. She wonders what people will think about their being out in the dark … together … alone.

She leads the way into the room where the other students have gathered. She feels she has nothing to be embarrassed about since her time with Jesse was so innocent. What she feels does not matter when all eyes turn on her and she experiences the deadly silence of all voices stopping – at precisely the same time. She has never felt such hostile attention before. Jesse, who has, hesitates before walking into the same treatment a few minutes later. In those few silent, enraged moments, she knows viscerally a little of what it feels like to be Black in a White world – just a little. (excerpted from Ellis, 1995a, pp. 152–153)

I continued to see Jessie, even after my father told me he would disown a daughter who ever had anything to do with a Black man.

FINDING SOCIOLOGY: 1967–1973

“You should go to college,” the guidance counselor said after I did well on an achievement test in the tenth grade. Excited, I went home to sell this idea to my mother, figuring that then she’d sell it to my father.

“How much would it cost?” my mother asked.

“A lot,” I said. “Maybe two thousand a year, including books, room, and food.”

“We’ll find a way. I’ll talk to your father.” She did, and it was settled: I was going to college.

A few days later I sought more information. A list of college majors on a form in the school guidance office piqued my interest. What would be my major in college? I recognized “social work” and thought that might be interesting since I cared about helping people in need. The word “sociology” followed. What’s that? I wondered. At home, I looked up “sociology” in my dictionary. Attracted by the emphasis on social behavior in face-to-face interaction, relationships, and groups, I decided that was what I wanted to study. (excerpted from Ellis, 2009, p. 61)

I left home in 1969 to go 150 miles – and a world away – to the College of William and Mary, where because of my experience with Jesse, I initially focused on race relations. In my first course in sociology of race, my professor, who was from Japan, taught the demography of Black–White relations with graphs and statistics. This plunge into demography – which ignored the heart, soul, and lived experience of race – led me away from studying race until several decades later when I began to write about my
experience with Jessie (Ellis, 1995b), reflect on my mixed-race neighborhood (Ellis, 2009), and teach a class on “Communicating Race and Emotions.”

Because of my interest in “helping people,” therapy, and the individual, I also took a few psychology classes. But I couldn’t find myself – indeed, I couldn’t find people in general – in the experimental labs, rat mazes, and behaviorist theory I was introduced to in psychology classes.

But many of the sociology classes spoke to me, and I took the maximum number allowed by the university. Early on, I found social psychology and was introduced to writers like Berger and Luckmann (1966), who showed me the interlinking of society and individual consciousness, helped me understand the social construction of reality, and how individuals create society and then internalize it and are constrained by their creations that become social facts. I also was mesmerized by Erving Goffman (1959, 1963, 1967), who excelled in describing the surface and deeper structures of what people do and say in everyday public interaction. I hoped I had a Goffmanian eye. His work gave me a new way of seeing both others and myself in terms of dramaturgical performances, where actors wear costumes, have props, and manage impressions for audiences. It wouldn’t be until much later that I would become critical of aspects of Goffman’s perspective, especially how his distance-observer focus privileging the view of the beholder of social life neglected the self-identification and subjectivity of the person being watched (Ellis, 1998).

Introduced to ethnographic study in my sociology classes (albeit briefly in a few sections in a methods course), I quickly decided I wanted to do field work. To be with people, watching and participating in their lived experiences, seemed preferable to a statistical or textual study. In search of a real-world situation in which to practice my sociological eye, I chose to study an isolated fishing community in the Chesapeake Bay area for my undergraduate honor’s work. There I concentrated on how isolation impacted community members’ lives and values. I continued this study for my MA thesis and PhD dissertation.

Before graduate school, I spent a year as a social worker in Virginia. I liked helping people and thought I was pretty good at it, but the bureaucratic structure and workload, plus the demands of being at work from eight to five, dampened my enthusiasm considerably. Missing academia, I applied to State University of New York (SUNY) at Stony Brook, hoping to study social psychology with Gene Weinstein, whose work I had been introduced to in my undergraduate courses. For me, Gene’s work on altercasting, presentation of self, and interpersonal competence epitomized a Goffmanian eye (Weinstein, 1969; Weinstein & Deutschberger, 1963).
BECOMING AN ACADEMIC: GRADUATE SCHOOL, 1974–1981

The contrast between the worldly experience of social work and the abstract discourse I encountered in my graduate classes at Stony Brook in 1974 was hard to reconcile. I learned early on that most sociologists rejected the “applied” and that my sentiment that sociology should “help people” was best left unexpressed publicly. My socialization “took,” and soon I shared the hierarchical notion that it was important to separate sociologists (the knowers) from social workers (the doers). Still I continued to wonder whether sociology shouldn’t strive to open people’s eyes and hearts to the world and make their lives better. Though I loved what I was learning, I could not, for the most part, connect my life to what was being taught in classes. How could those outside this academic tribe connect their lives to what we were doing?

By the time I got to graduate school at Stony Brook, I had made a break from small-town life. I became an “experience junkie,” wanting to try everything that had been unavailable to me in Luray. I took on the “hippie” life style in full force, dressing and acting the part. I did not fit then with the radical left or the conservative element in the department, but I felt comfortable with those who had liberal leanings. In the socialization process, as frequently happens when working-class youth go to college, I felt alienated from my conservative past and became someone my parents hardly recognized (Rodriquez, 1983; see also Casey, 2005). Upset by my changes – in religious values, the way I talked, my dress and appearance – my mother viewed my unshaven armpits and hairy legs as signs of my final demise. Most likely, my family felt similarly to the working-class mother of academic Carolyn Leste Law, who said, “Education destroys something” (Casey, 2005; Law, 1995, p. 1). I went home regularly to visit my parents and stayed in touch by phone, but I kept myself removed, more concerned about how I was feeling than about what they might be going through. My life revolved around graduate school, not my family and hometown community.

Still, as a working-class kid, graduate school in some ways continued to be a foreign environment to me (Lubrano, 2004). Abstract thinking and macropolitical knowledge were celebrated; feelings, storytelling, and everyday experience were not deemed important aspects of sociological analysis, and I learned to suppress those parts of myself in formal settings. I worked hard to speak (and think) like the professors I admired in this northeastern university setting, many of whom were Jewish intellectuals. I rehearsed speaking without the heavy southern accent I had carried from small-town
Virginia to Long Island. I practiced interrupting aggressively yet politely and speaking assertively and I worked hard to develop a voice that clearly articulated abstract ideas. It was only later that I realized that having a lisp in addition to a southern accent also contributed to the problems I had in speaking like an “intellectual” and fitting in (see Ellis, 1998). But eventually, just as I had at William and Mary, where I had graduated with honors, I made a place for myself in graduate school both intellectually and socially. I credit my achievement in both realms partly to my ethnographic sensibilities.

Theory and statistics were privileged over qualitative work in sociology, and Stony Brook was no exception. For a while I was fascinated by quantitative analysis, partly because I had a natural talent in math and statistics and did well in these areas. I became the graduate student assistant in graduate statistics, after having excelled in the department’s statistics course. But while I enjoyed solving puzzles and getting “answers,” I didn’t feel this approach let me address the questions of most interest to me, look at social life more holistically and concretely, or get close to the people I was studying. Thankfully there were qualitative folks on the sociology faculty at Stony Brook – such as Jerry Suttles and Rose Coser with whom I worked – and the faculty in general supported qualitative dissertations, unlike many mainstream sociology departments.

WRITING FISHER FOLK: 1974–1985

For my dissertation, I compared Fishneck, the community I studied as an undergraduate, to Crab Reef, a fishing community in the middle of the Chesapeake Bay (Ellis, 1986). Ethnographic participant observation allowed me to live with the people I studied and to participate in, observe, and describe their day-to-day lives. While in the communities, however, I often experienced conflict between remaining distant, as I had been taught in methods classes, and participating fully; between recording only my “objective” observations of the Fisher Folks’ actions and speech and noting my sense of their emotional lives, a process that required my engagement. Often distance won out over involvement because of my concern about meeting the requirements of neutrality and objectivity I had learned in my graduate education.

When I returned to the university to write my dissertation, I struggled with the constraints of detached social science prose and the demand to write theoretically in an authoritative and uninvolved voice. I thought I had
to write this way to be considered an academic. Yet this kind of thinking and writing did not come naturally to me. I struggled to organize my dissertation around “legitimate” sociological topics – social structure, family, work, and social change. Within this framework, I discussed “hard” sociological concepts, such as personal attachment, locus of social control, reciprocity, public conformity, civic status, individualism, communitarianism, center, and periphery. I found it difficult to capture the complexity of the lives of the Fisher Folk using these categories, and I often felt unsure of the distinctions I was forced to make.

Though I was probably more visible as a character in my study than were authors in most ethnographic texts of that time period, I still largely described “them,” the Fisher Folk, interacting with each other, as though I were off in a corner, invisible. In reality, most of what I learned came through my interactions with the people, especially their reactions to me. But those exchanges and the effects my presence might have had on what the Fisher Folk said and did took a back seat. I limited how much I spoke in the first person, because I felt I had to sound “professional,” unbiased, neutral, and objective (see Krieger, 1991).

In my dissertation, I rarely talked about how I felt. I was reluctant to admit how much my own emotional experiences in the communities influenced what I saw and how I framed my study theoretically in terms of “tight” and “loose” communities (Pelto, 1968). For example, I carefully watched how I comported myself in Crab Reef, which was under tight community control, and I felt much freer in Fishneck, which was managed more by family and personal loyalty.

Even during this research, however, I was drawn to stories for conveying lived experience and insisted on inserting vignettes showing specific incidents. In these stories, I could occasionally be present, though I rarely got to speak and almost never showed how I felt. But I knew, even then, that I wanted readers to hear the participants’ voices and see them acting. The vignettes breathed life into my more passive telling and categorizing of the Fisher Folk.

The next day I met Michael Paul and a thirteen-year-old boy named Jimmy James, who wanted to show me “a better way to clam – treading.”

Jimmy James said, “Git ya’ on me back, doll. I’ll tote you to the skiff so’s you don’t git yer feet wet.”

After Jimmy James deposited me into his skiff, he drove the boat to a shallow area and all of us jumped overboard (including Michael Paul, who was sixty-three years old) and held onto the side of the boat. We were in water about waist-deep and sometimes deeper (I didn’t call this shallow!), and dug into the bottom with our feet, hoping to find
the prized clams. (Actually I was hoping that I wouldn’t find a broken bottle.) It was a contest to see who could find the most and the “biggest one that ever has been.” The person who came up with a rock instead had to suffer laughter from others. They tried to teach me to toss the clams with my feet, but I finally decided I preferred bending over to retrieve them, since I was already wet.

We clammed about an hour. Jimmy James found 120 clams, Michael Paul retrieved 60, and I got 30. “Not bad for you,” they reassured me. We had 210 clams, which we could sell for 3 1/2 cents each or $7.35. Not much for three people, I thought. (excerpted from Ellis, 1986, pp. 80–81)

BRINGING IN EMOTIONS AND SOCIOLOGICAL INTROSPECTION: 1982–1986

While a graduate student at Stony Brook, I began a romantic relationship with Gene Weinstein, the sociology professor I had gone to graduate school to study with. Our relationship was complicated by his illness – he had emphysema – and our status differences, yet it lasted until he died nine years later (Ellis, 1995a). In most ways Gene was my mentor, though he was not formally on my dissertation committee. Still he and I talked often about all aspects of sociological inquiry, including my community study, and it was in these conversations that I began to appreciate the macro view of sociology. Indeed by the time I finished my dissertation in 1981, I saw the usefulness of a macroperspective and had begun to feel comfortable talking about these processes. But macrosociology never captured my heart. I always felt more comfortable returning to the interpersonal and emotional level of what was going on with the Fisher Folk. While I was immersed in ethnography and attached to the study of isolated communities and people on the margins, I felt my calling was in studying face-to-face relationships not macroprocesses. The ideal for me, I thought, would be to combine ethnography and social psychology and to be able to use my intuitive sense of people and the interactions I had with them.

One of the many attractions of my relationship with Gene was the discussions we had around his kitchen table about social psychology. He and I also spent many hours each week talking about his illness, our relationship, our emotions, and their intersection, as well as probing the interpersonal dynamics of our friends’ lives. I was captivated by the stories we shared and examined, and the insights I thought we had about emotions. But when I left the kitchen table and went to class, I encountered a different conversation. The graduate curriculum revolved around building theory, thinking abstractly, and synthesizing the results of empirical studies. I was
taught that sociology was not about personal stories and feelings, but rather it focused on theorizing, generalizing, manipulating variables, and predicting outcomes.

I finished my dissertation on the fishing communities in 1981, and that same year, I began an academic appointment at University of South Florida. In 1982, Gene came to Florida to live with me, first on a sabbatical, then on sick leave, and finally he retired there. We married on December 25, 1984, while he was in the hospital, where he died on February 8, 1985 during a return visit.

When Gene first arrived in Tampa, we began to conduct research on jealousy. Both of us desired to bring the study of emotion into sociology’s rational studies of human behavior, and we also sought to understand our own personal and relational experiences of jealousy. Our main source of data consisted of our personal episodes of jealousy buttressed by friends’ descriptions of their experiences. When we submitted our work for publication, we played down our introspective method and instead emphasized informal interviews and written descriptions that we had collected from students. Though our jealousy paper was based on people’s stories, the final version was written abstractly, camouflaging informants’ everyday experiences. When the reviewers rejected our paper saying we needed quantitative data, we inserted a few statistics from a survey of jealousy we had administered to 300 students, and the article was published (Ellis & Weinstein, 1986).

Gene and I began to talk about why introspective data had to be hidden in our published article. After all, we knew some things from our own jealousy experiences that we would never know from surveys or interviews of others, such as what it felt like when the jealousy flash took us over physically and emotionally in spite of our rational intentions. Why did social science have to be written in a way that made detailed lived experience secondary to abstraction and statistical data? Our experience with this work led us to ask at the end of the article about the importance of evocative detail, metaphor, and felt emotions in writing and reading about emotional experience. Though our ideas were couched in theoretical social scientific prose, the beginning rumblings of our desire to examine emotions in a more concrete, personal, and evocative way became apparent.

As we worked on this chapter, I was going through several traumatic life experiences that made me question the kind of work I wanted to do and moved me toward embracing storytelling and evocative social science. In January 1982, my younger brother was killed in an airplane crash on his way to visit me (see Ellis, 1993). In the summer of 1984, I tore the anterior
cruciate ligament and meniscus in my left knee while playing basketball, which demanded surgery. During this time, Gene entered the final stages of chronic emphysema. Flashbacks of my brother’s death and my ongoing inability to “get over” my loss were interrupted in real life by Gene gasping for breath while I hobbled as fast as I could to untangle his oxygen hose. In this context, the scientifically respectable survey of jealousy we were working on seemed insignificant.

Instead, I wanted to understand and cope with the intense emotion I felt about the sudden loss of my brother, the midlife crisis I experienced from my body failing me, and the emotional pain Gene and I both felt as he deteriorated. On July 14, 1984, I began keeping ethnographic field notes about these experiences, focusing on Gene’s deterioration, our relationship, and coping with illness and dying. These notes were therapeutic for me and, I thought, sociologically insightful as well. I wasn’t sure initially what to do with them; I just knew I had to write them. I continued writing notes about my personal experience for the next two years, including the year after Gene’s death.

In January 1985, I was promoted to associate professor and awarded tenure, primarily based on my work on the Fisher Folk. Now it felt less risky to write something other than traditional social science, something that would be engaging, therapeutic, and sociologically useful. Now I could better afford to challenge the boundaries of what counted as legitimate sociology, an endeavor that became my passion after Gene’s death a few weeks after my promotion.

**WRITING FINAL NEGOTIATIONS AS EMOTIONAL SOCIOLOGY: 1985–1995**

The deaths of my brother Rex and my partner Gene inspired me to study grief and loss. After trying a number of different approaches, I returned to the notes I had kept on my relationship with Gene and decided to analyze my own experience of loss. I wanted to write a story that conveyed the emotionality of this experience. The best way to do that seemed to be to write a narrative with scenes, action, plot, and developed characters who felt, thought, and spoke. I would be the main character.

Early in my writing process, I sent some narrative excerpts to friends and sociological colleagues. From the lay audience, people dealing with illness, those in the helping professions, and several friends in sociology, I received glowing evaluations about how well written and emotionally expressive
these stories were, and how much they were affected by what they read. One sociologist said, “I can never remember being so deeply and emotionally affected by something, time and again. Each time I read about your experiences I am blown away, amazed by how you were able to cope, and in deep admiration of your ability to live and care... You are correct in doggedly sticking to your guns, ignoring the chastising of your colleagues and respected ‘scholars,’ because you have something so much more powerful, so much more moving, so much more important.”

But some friends and colleagues in the academy admitted not knowing what to say, feeling “uneasy reading these materials,” embarrassed by my “emotional nakedness,” as though they had peered into a dimension of my personal life they were not supposed to see. “The writer’s calm has to quiet the reader’s embarrassment,” critiqued a colleague, and “Some things need to be written; they do not need to be read,” responded a second critical reader, both assuming that readers need protection. “If you have to do this for therapy, go ahead, but this is not sociology,” warned a friend, who felt sociology needed protection. “These kinds of things should not be revealed,” warned a sociologist, who assumed that I, the author, needed protection. “The greatest gift that we can give is the gift of our dispassionate analysis, our coolness, our marginality,” added another, in hopes of protecting all of us from the threat of emotional involvement. Even colleagues active in the movement to include emotions in sociology showed concern about what my work meant for the sociological enterprise: “How does what you are saying tell me anything about my own experience? How can this be generalized?” one sociologist asked.

The intense responses to my work, even the critical ones, signaled that I was on to something important. My vulnerabilities about this project were overshadowed by the challenge of convincing the academic world that introspective ethnography should be included in sociology and could meet the criteria of rigorous inquiry.

Determined to reach sociologists, I spent a year researching and writing a paper on systematic sociological introspection. I developed the idea that introspection was a scientific approach to social science research, and I included narrative excerpts from my unfinished manuscript to demonstrate its value. I claimed that introspection was a social as well as a psychological process, and though it had weaknesses as well as strengths – just like any other method – it deserved recognition. The majority of reviewers who responded formally to this work noted the excellence of my writing and seemed interested in my points of view and affected by my narratives. But, for the most part, sociological reviewers argued that introspection did not
produce sociology. One wrote, “Your writing reminds one a bit of the poems that some write while on drugs. I kept asking myself what is the significance to a sociological reader of your encounter with Gene’s doctor. Does a reader really care?” Some wanted the paper to be more scientific and analytic. A few claimed that to consider what I did as sociology might endanger the sociological enterprise. I wrote and rewrote this paper, responding to these critiques, for example, by substituting interviews with others for my autobiographic materials. I was disappointed with the responses, but suspected that these sentiments illustrated the power of what I was doing, rather than its irrelevance.

The review that caught my attention said that the manuscript was “schizophrenic.” “It makes a case for the method, and then turns hard science against it and ends up limply defending its case... The author is caught between two camps – hard psychological (sociological) science, and interpretive, imaginative, humanistic, phenomenological inquiry. You can’t have it both ways,” Norman Denzin wrote. This review cracked my sociological identity. For the first time, I thought of the possibility of seeing myself and my work connected to humanities as well as social science. I revised the paper again, omitting the focus on proving that what I was doing was science, and “Sociological Introspection and Emotional Experience” was published in Symbolic Interaction in 1991 (Ellis, 1991a).

Examining this published article now, I see the lingering remnants of my preoccupation with convincing orthodox social scientists on their own terms of the value of detailed, lived experience. Yet I also see a yearning to examine what could be learned from my project about how meaning is attached to human experience. Although some mainstream sociologists – especially women – were receptive to this work, my move away from orthodox sociology continued, and I eventually began to communicate with scholars across the disciplinary divides, who were interested in integrating humanities and social science and taking an interpretive stance toward understanding meaning and human life.

Nevertheless, in 1991, “Sociological Introspection and Emotional Experience” offered readers a persuasive sociological rationale for accepting introspection as a legitimate way of doing social science research and for examining what emotions feel like and how they are experienced, including the emotions of the researcher. Along with a second article on emotional sociology (Ellis, 1991b), this work provided justification for those who wanted to study emotions emotionally and suggested that introspective and interactive methods along with narrative writing might provide ways to accomplish this goal.
I was fortunate to be writing these essays at a time when postmodern, poststructuralist, and feminist writers were contesting issues of authority, representation, voice, and method. Anthropologists, such as Bruner (1986), Clifford and Marcus (1986), Geertz (1983), Marcus and Fisher (1986), and Rosaldo (1984) were deconstructing writing conventions, making arguments for introducing new forms for expressing lived experience, and demonstrating that literary and scientific genres of writing were situated within historical and linguistic practices that hid ideological interest and largely determined what could count as a legitimate contribution to knowledge. Feminists, sociologists, and other anthropologists, such as Belenky, Tarule, and Goldberger (1986), Cook and Fonow (1985), Denzin (1985), Jackson (1989), Johnson (1975), Kleinman (1991), Krieger (1983), Oakley (1981), Reinharz (1979), Richardson (1990), Rothman (1986), Tedlock (1991), and others too numerous to mention were creating new ideas about knowledge and methodological approaches, trying out new forms, studying emotions as lived experience, incorporating feelings in their research, getting close to those they study, and incorporating their personal experiences and standpoint in their research with others. I could not have been writing at a better time.

While I was working on these articles, I continued writing Final Negotiations. It took nine long years to construct and reconstruct the story of my relationship with Gene (Ellis, 1995a), to work out satisfactorily a version of what this relationship had been and had meant to me, and to tell a story that cohered both with what I remembered and what my life had become (Crites, 1971). Writing sociology as an intimate conversation about the intricacies of feeling, relating, and working continued to confront me with the deficiencies of traditional social science research practices and representational forms for dealing with day-to-day realities of chronic illness and relational processes.

In Final Negotiations, I violated many taken-for-granted notions in social science research: making myself the object of my research and writing in the first person infringed upon the separation of subject and researcher (Jackson, 1989); writing about a single case breached the traditional concerns of research with generalization across cases and focused instead on generalization within a case (Geertz, 1973); the mode of storytelling fractured the boundaries that normally separated social science from literature; the episodic portrayal of the ebb and flow of relationship experience dramatized the motion of connected lives across the curve of time and thus resisted the standard practice of portraying social life as a snapshot; and the disclosure of normally hidden details of private life.
highlighted emotional experience and thus challenged the cognitive model of social performance that dominates social science.

As I wrote and rewrote, I moved closer to telling an evocative and dramatic story and farther away from thinking of the text as realist ethnography. I reconstructed conversations Gene and I might have had, reading and rereading them aloud until I heard the ring of authenticity, continually questioning my mode of presentation and my motives. I showed interaction so that the reader might participate more fully in the emotional process, not merely observe the resolution. This meant moving from generalizing about a kind of event that took place to showing one event in particular, such as a doctor’s visit, often by condensing a number of scenes into evocative composites.

Visits to Dr. Silverman, Gene’s physician, provided occasions for confronting and evaluating Gene’s illness. The doctor’s office was on Park Avenue, yet he made us feel we were being visited by a rural doctor in a horse-drawn carriage. Sentimental and grandfatherly, he held Gene’s hand and teared when he had to tell us bad news; but his eyes brightened when immediately afterward he informed us of some new medicine to try or of a success story – a mayor who was working while hooked to a breathing machine – or when telling Gene how far he was above the normal curve given the extent of his emphysema.

Still, at every successive visit, Gene was worse. As Dr. Silverman compared the indices of tests measuring breathing rate and lung capacity for us, we could not ignore the downward progression.

During one visit when there is a dramatic drop, Dr. Silverman tries to be optimistic. “But look at what you can still do. And there are some developments; a new drug is being tested in Canada. Let’s see if we can figure out a way to obtain it.”

Gene listens attentively, hopefully. Then a cloud passes over his face. “But, Doc, it’s not a cure, is it?”

“No,” the doctor replies, holding eye-to-eye contact with Gene. “There is no cure. Maybe in the future, but not in your lifetime.”

Gene’s shoulders sag farther into his chair as Dr. Silverman looks away busying himself with altering Gene’s many medications. “I think changing your antibiotic will help. Try taking one, four times a day, instead of two twice a day.”

It took only a few visits to realize that new medications and rearrangements of old ones were of little sustained value. We simply substituted one steroid, one adrenalin, for others. The only time the change mattered was when Gene developed an allergy to an ingredient in one medication. Initially, Gene’s improved condition gave us false hope that this medication was a panacea only to let us down again when the allergy disappeared and the labored breathing continued. (excerpted from Ellis, 1995a, p. 55)

With each scene I wrote, I concentrated more on being true to the feelings that seemed to apply in each situation I described than to getting all the “facts” in the exact sequence. More and more I moved away from trying to make my tale a mirror representation of chronologically ordered events and
toward telling a story, where the events and feelings cohered, where questions of meaning and interpretation were emphasized, and where readers could grasp the main points and feel some of what I felt.

I also began to advocate research and scholarly writing as healing and to question more deeply the dichotomy of scholarship and therapeutic writing, a critical response from sociologists that I had accepted initially. I stopped being defensive when others accused my work of being therapeutic, and I began claiming the possibility of healing and insight about oneself as necessary and ethical parts of what I wrote. I made the case that what I had learned from my own struggles for meaning was unique enough to be interesting, yet typical enough to help others understand important aspects of their lives (Abrahams, 1986).

SEEKING AND FORMING ACADEMIC COMMUNITIES: 1985–2010

Soon after Gene died in 1985, I had a conversation with Candace Clark about starting a section in Emotions in the American Sociological Association (ASA). By 1986, we (along with others) had formed the section and attracted members. I participated as an officer or Council Member in the Emotions Section from 1986–1995, serving as Chair-elect and Chair from 1991–1995. The section helped legitimate the study of emotions as a proper arena of research. I now had an opportunity to broaden the scope of sociological writing. Still I was disappointed to see many colleagues omit a “lived experience” approach to emotions research, busily handing out surveys, counting and predicting emotional reactions, observing facial muscles contracting on videotapes, categorizing people, and abstracting generalizations. Emotion was in danger of becoming simply another variable to add to rational models for studying social life. What about emotion as lived experience and interaction? I vowed to resist the rationalist tendency to portray people exclusively as spiritless, empty husks with programmed, managed, predictable, and patterned emotions (Ellis, 1991b).

I also attended my first Society for the Study of Symbolic Interaction (SSSI) Annual Meeting in 1985. In 1987, I gave my first paper in this society at the Stone Symposium in Urbana, hosted by Norman Denzin. There, I became intrigued by the communication scholars on the program – their ethnographic and interactionist topics of study and their focus on language – but I knew little about communication as a discipline or program.
I loved the people I met in SSSI and the work they were doing on creating meaning, identity, role taking, socialization, interaction, and social construction. Unlike ASA where it was hard to find a session on qualitative methods, most of the sessions in SSSI focused on ethnography. I immediately felt at home. Though I had made a community for myself in the Emotions Section of the ASA, I felt tension there between empiricists and interpretivists and perceived that empiricists did not take my work seriously. But in SSSI I found myself surrounded by ethnographers, many of whom were interested in emotions and lived experience, and I felt welcomed, cared for, and caring in a way I never had in ASA. I made many wonderful friends there, such as Laurel Richardson and Norman Denzin and so many others, and their work inspired me to keep pushing at the boundaries of what ethnography could be and the many forms it could take. I threw myself into this organization wholeheartedly, attending the annual meeting and the mid-year symposium each year. I took an active role almost immediately, serving on committees and organizing sessions, eventually serving from 1996 until 1999 as president-elect, president, and past president.

I fantasized that it would be productive to join together the two organizations – the section on Emotions and SSSI – especially given some of the overlap in membership. I made several overtures in that regard but nothing really came from them. In 1990, Michael Flaherty and I organized a conference in St. Petersburg that was funded by SSSI and ASA on “Sociology of Subjectivity.” There was a lot of enthusiasm for the conference. I had high hopes for what we might be able to do from there, and the energy to put an agenda in place. But I knew that one obstacle to any kind of integration was the renegade identity of SSSI, an organization that began in the early 1970s as a rebellion against structural functionalism and the quantitative approaches of mainstream sociologists in control of the ASA; a second obstacle was that few of us interested in interpretive approaches to subjectivity were in PhD granting programs. Thus, although many graduate students were interested in what we were doing, attended the conference, and were active in our organizations, most did not have the support in their home departments or interested professors to work with on their projects and felt forced to do more mainstream work. It was hard for me to see a positive future in sociology for what we were doing.

And then… I met Art Bochner, who was chair of the Department of Communication on our campus. Actually I met him a few days before the January 1990 conference on subjectivity. Though he has been at University of South Florida (USF) for six years and I had been there for nine, our paths had not crossed. Partly that was due to the separation of the College
of Arts and Letters, of which Communication was a part, from the College of Social and Behavioral Sciences, of which Sociology was a part. On this fateful day, Art had come to a talk I was giving in the business school, titled “Systematic Sociological Introspection and Research on Emotions,” where I spoke about the writing of Final Negotiations. Among his intriguing comments and questions, he noted that I seemed to expend a lot of energy defending myself against critiques of orthodox sociologists and to accept their terms for evaluating my work. “Why not drop all the science talk?” he asked. “Just take it for granted that what you are doing is important. It only distracts you and your readers from the very human sense of suffering and loss you communicate so beautifully.” Now I had a lot to think about, and Art’s comments played right into the crack in my identity that first had opened when I received Norman Denzin’s review of my introspection paper. Perhaps it wasn’t so important to defend myself as a scientist after all. Perhaps it wasn’t the end of the world if what I did wasn’t seen as scientific sociology. Perhaps it was more important to do good work that I believed in, that I thought might contribute something that I thought I could do well, that had my heart, that used my sociological eye, and that touched people. Perhaps it was more important to talk to those who wanted to listen and talk back than be accepted by mainstream sociologists.

Art later would tell me that while listening to my lecture he had felt that I was giving his talk in terms of what needed to be done to resurrect the social sciences and make them meaningful. We both wanted a different kind of social science, a more artful one centered on meanings and stories. Shortly after that day we became partners in every sense of the word: we wrote together, edited each other’s work, mentored students, and in 1995 we married. Together we were able to accomplish much more than either of us would ever have been able to do alone.

In the first months of our relationship, Art read every page of Final Negotiations – several times. Not only was he significant in editing my prose and listening to me as I tried to figure out how to focus and end my story, he became part of my story, adding coherence and continuity to my life, and supporting me as I wrote and revised. He helped make it possible for me “to recover a living past” and “to believe again in the future” (Crites, 1971, p. 311). And he did all this without ever expressing jealousy about my relationship with Gene or pushing me to move beyond grief and immersion in writing until it was clear that I was ready to do so.

My words here cannot begin to tell the story about our relationship and what it has meant to me. This part would take a book to tell well – and someday I (or we) might write it. Suffice it to say now that Art and I found
kindred souls in each other dedicated to accomplishing the same goals – to join social science and humanities to make scholarship more human, useful, emotional, and evocative; to create a romantic partnership in which we supported each other and grew together; and to develop a research program in which we could mentor students in interpretive social science, work we believed contributed to the world in which we lived.

In 1995, I asked to move my faculty line from Sociology to the Communication Department, so that Art and I could work together in continuing to develop a PhD program there that would focus on ethnography, narrative, and autoethnography. I had hoped to build a curriculum focused on ethnography in the Sociology Department at USF, but for a number of reasons – no PhD degree at the time (though they now have one in Sustainable Communities in Global and Urban Environments), some political upheaval, and no doubt mistakes I made in trying to organize a program – it seemed that was not likely to happen. I was not happy in my department and I was attracted to the opportunity in Communication to educate graduate students who would teach other MA and PhD students about this perspective, something that always had been missing in sociology. I convinced myself that I could continue being involved in sociology as a discipline and that SSSI, which embraced sociologists and communication scholars, would serve as an integrative home. But, as I found out later, it is difficult to give your all to more than one professional identity and more than one professional organization.

Though I was immediately attracted to the possibilities in moving to communication and I already had moved psychically away from sociology, it was tough to actually “leave” sociology, which has been the source of my professional identity since I was an undergraduate. Then one day a communication journal came in the mail for Art. I leafed through it and started to cry because I didn’t know any of the authors. Nor did I care much about many of the topics. This isn’t who I am, I thought. Then I noticed the new ASA convention program also had arrived. Glancing through it, my tears turned into laughter. I didn’t know many of the people listed there either, nor was I interested in most of the topics. That realization made it a little easier to make the transition. Looking back now, I think everyone should change disciplines in the middle of their careers to broaden their educations and challenge the terms of their taken-for-granted way of coding and viewing the world.

With the transition, I became involved in my new department and in the National Communication Association (NCA). In Communication, I have had to learn a new vocabulary. I often joke that I now substitute the word
“communication” for “interaction,” but that they both basically mean the same. Thankfully I have had the freedom to research and teach what I wanted and my academic life feels similar, in most respects, to what it did in Sociology. It has, though, felt strange to be so old as a scholar yet so “young” in the field. When I entered Communication, I had never taken a communication course and I didn’t know much about other communication programs, nor did I recognize the names of some of the well-known scholars—and they didn’t recognize mine. Attending NCA became an important source of my knowledge about the field of communication and a place to make contacts and assist my students in pursing their careers and getting jobs.

Communication was ripe for developing qualitative methods when I became active in NCA. In the 1960s and 1970s the “ethnography of speaking” perspective used participant observation and interviews to emphasize rules for speaking (Philipsen, 1975). Then in the 1980s, more communication scholars began doing interpretive ethnographies (e.g., Goodall, 1989). In 1997, Art and I started an Ethnography Division in NCA, which today has almost 500 members and usually sponsors close to 25 programs at the annual meeting, many of which embrace the kind of work we do. Communication ethnographers do not seem as caught in the tension between science and humanities as sociological ethnographers tend to be. They are used to expressive arts and performance studies being part of the discipline. Thus it was relatively easy to make a new home in communication. I continue to feel appreciative of the welcome, acceptance, and recognition I have received in this field.

In spite of my good intentions, I gradually stopped attending ASA meetings, though I continue even now to pay my dues. The last paper I gave there was in 1999. Occasionally I put the words “ethnography,” “narrative,” “autoethnography,” “personal narrative,” “story,” and “qualitative methods” into ASA’s conference search engine and come up almost empty handed. Though not a scientific counting, in the 2010 ASA program I could locate only one session in ethnography/ethnographic studies, one ethnography roundtable, and no sessions in qualitative methods. Though I am still a member and served on the nominations committee as late as 2006, I have not otherwise participated in the Emotions Section since 1999.

I continued being involved in SSSI and attending meetings through 2003. In 2000, Art and I held a SSSI symposium in St. Petersburg on “Ethnographic Alternatives” and Art served as Vice President for SSSI during 2000 and organized the program. With the exception of 2009, when I received the Cooley award for my book, Revision: Autoethnographic Revisioning an Ethnographic Life
Reflections on Life and Work, I have not attended SSSI meetings since 2003. I was deeply honored to get this award. It was nostalgic to return to SSSI and see old friends there, though I could not understand why most of them now had gray hair.

Art and I and our students have been active in the International Congress of Qualitative Inquiry (ICQI) that Norman Denzin has held annually in Urbana since 2005. This conference features interdisciplinary interpretive work on issues of social justice, human rights, ethics, politics of evidence, socialization and identity, emotions, and the body, among other topics. Though many perspectives are represented, the bulk of the presentations use interpretive approaches and engage in nontraditional forms such as narrative, autoethnography, and performance. Around 1,000 people from more than 50 countries attend each year. Bringing together qualitative scholars from multiple disciplines and from around the world seems important at this point in our history. The Congress provides a caring place for many of us to gather.

Though SSSI held their symposium at ICQI a few times, they now have moved back to their own venue. I’d like to continue being involved as well in SSSI, but often I don’t feel I have the time, energy, inclination, or financial resources to attend more annual conferences. There also has been some dissension and disagreement between the more humanistic/narrative qualitative researchers, represented by many who are active in ICQI, and the more traditional social science scholars who are active in SSSI, though there is more overlap than separation. This difference in perspective has been played out in publications several times. (For example, see the review symposium in Journal of Contemporary Ethnography [2002] featuring a debate about the crisis of representation and several years later in the same journal [2006] featuring a debate about analytic autoethnography.) Though I have participated occasionally in these debates, I believe we all share more similarities and common goals than differences. Most of the time, I continue to want to show what my work can do and how it can help people rather than debate its sociological merits.

With Art, I have concentrated in the last decade on making our work available to others and providing outlets for scholars doing narrative and autoethnography. Along those lines, we have given keynote talks at international qualitative conferences in many different countries. Additionally, we have edited several collections (e.g., Ellis and Bochner, 1996) and two book series, all with Mitch Allen (now publisher at Left Coast Press), who always has been supportive of our endeavors. One turning point was the chapter we published in the 2nd edition of the Handbook of Qualitative
Methods (Ellis & Bochner, 2000), which seemed to provide legitimation for doing narrative and autoethnographic projects. Both of us have published a number of personal stories and theory-oriented pieces advocating for the work we do. In the last few years, I have published The Ethnographic I (Ellis, 2004), a methodological novel that serves as a textbook about the issues in writing autoethnography at the same time it shows the autoethnographic form and craft. I followed that with Revision (2009), which contains many of the stories I have written about loss, race, ethics, caregiving, and community, and layered meta-autoethnographic interpretations, reflections, responses, and vignettes to show how revisiting the stories from current perspectives can lead to new and deeper understandings.

Our PhD program in Communication continues to thrive, attracting many women, but also minority students and men, most who tend to be reflective, interpretive researchers, with vulnerable hearts. They have exceeded our expectations in their published scholarship and successful careers (e.g., see Adams, 2011; Ellingson, 2005; Foster, 2007; Tillmann-Healy, 2001). We now are meeting the students they mentor and this gives us hope that the work we advocate might live on for a while. Of course, much of that depends on the economy and the job market, both rather shaky at the moment I’m writing this essay.

CIRCLING BACK AND RECLAIMING IDENTITIES

I write this autobiography from our summer home in the mountains, where Art and I have become involved in the local culture of a small-town mountain community. Some years ago, I returned home in terms of reconnecting to and appreciating more fully my family of origin and the community in which I was raised. That reconnection occurred for me, as it does for others, as I grew older, experienced the aging and loss of my family members, in particular the early death of my brother and the aging and caregiving of my mother. Living part of the year now in a southern mountain community in the South and writing about my life there complexifies again all that I thought I knew about small-town values, race relations, and the politics of difference. It also gives me an opportunity to rethink again and again the ethics of gathering other peoples’ stories, telling my own, and all that implies.

I often think now about how my work fits with other interactionist and sociology of emotions scholarship. I work in the interactionist tradition and like to think that my emphasis on the “holder” of an experience adds to
the traditional emphasis on the “beholder.” I still hope I have a Goffmanian eye, though often I turn this eye on myself and my interactions rather than see myself as a distanced observer with privileged insight. I see the self, as did Mead (1934), as a soliloquy between the “I” and the “me.” The self I experience exhibits both fluidity (which comes from the changing voices in our heads) and consistency (which is related to the stability of real and phantom others with whom we interact) (Athens, 1994). I focus on the individual depicted in C. H. Cooley’s (1902) looking-glass self who often takes an active role in presenting a particular characterization of self to others rather than examine solely how we tend to see ourselves as we imagine others see and judge us. Except for the case of a one-way mirror, the looking-glass reflects two ways – others also tend to see us as they imagine we see and judge ourselves, and the self feeling that results from the looking-glass process in turn affects how others see and judge us.

I seek to expand Goffman’s discourse on emotions in terms of how people try to appear to feel (Hochschild, 1979), which he determines from their actions, gestures, looks, and words – in sum, their performances. I also seek to move beyond Hochschild (1979) who examines how people try to make themselves feel. I want to try to show how people experience their feelings and lives, add heart to eye and concrete story to theory, and evoke readers to feel with me rather than just try to see what I see.

In a new project on collaborative witnessing with Holocaust survivors, which I started in March 2009, I have returned to concentrating on studying the lives of others (though, of course, others have always been part of my autoethnographic examinations). Rather than ending with testimonies from videotaped oral history interviews, my students and I see them as a starting point for greater collaborative engagement by survivors and researchers. Interested in innovative ways that testimony can be produced, we hold follow-up sessions with a small number of survivors in an interview context that emphasizes relationality, where trust develops over time, stories are brought to memory, feedback is given immediately, meanings come to the forefront, chaos and rupture appear alongside narrative coherence, and deeper levels of discovery take place in self-reflection and cultural analysis (Rosenthal, 2003; Rubin & Greenspan, 2006).

Our Holocaust testimony project connects survivors to observers so that both come to know and tell with each other in mutual engagement of hearts and eyes together. The outcome – collaboratively produced stories and meanings rather than fragmented transcript excerpts and abstract analysis – reflects the goals of literature in its invitation to readers to enter and feel the experiences described, the method of ethnography in systematically and
collaboratively working with participants to coconstruct a version of their lives that rings true to them, and the mission of humanities in its aims to examine how life story testimonies can weave together history and memory and contribute to showing the full complexity and meaning of survivor identity, ordinary life, and coping with trauma (Kushner, 2006, p. 291).

In this work, I expand my long-standing interest in personal narratives of loss and trauma, health benefits of personal storytelling about sensitive and traumatic topics, and the ways emotions are experienced in mundane and extraordinary life situations. I use the ethnographic methods I (along with others) have developed that focus on how context and relationship affect the stories that are created in interaction during interviews (Ellis, Kiesinger, & Tillmann-Healy, 1997). And I continue integrating ethnographic, literary, and evocative writing through stories that make sense of unique lives in cultural context. In this project, I hope to emphasize care and concern, and respond to survivors who say that they want to experience attentive patience, gentleness, protection, care, even love when they give testimony (Greenspan & Bolkosky, 2006, p. 446).

I feel very appreciative that in this stage of my career I have found another project that makes me as excited as anything I already have done. I am honored to have received a National Endowment Humanities (NEH) Summer Stipend to do this work. I feel the immediacy of what needs to be accomplished as most of the survivors are in their eighties. But at the same time I don’t want to hurry through because each interview and each contact with a survivor alters and extends my understanding and makes me committed to providing an atmosphere in which telling their stories enhances their lives in some way as well. Survivors have so much to tell and teach all of us about coping with trauma, aging, and living with these memories for the last six decades. We owe it to them and to ourselves to seek and portray their stories in as many ways as possible.

I love teaching, especially graduate students, and am appreciative that I usually get to teach what I am working on. Last year I taught a course on the Holocaust and the class made up teams that interviewed, transcribed, and edited interviews. Next summer I will teach another class on the Holocaust with a historian, who also is Director of the USF Libraries Special and Digital Collections and the Holocaust and Genocide Studies Center, where our interviews of survivors are being digitized and made available to the public. In this class we will compare some of the testimonies videotaped in the 1990s by the Shoah Foundation with stories that we have gathered recently from the same people. This will allow us to examine memory over time and at different points in the life cycle. Now I am an
ethnographer and teacher who is deeply interested in history and macroprocesses as well as emotions and interaction.

As I start my 30th year of being a professor – a communication scholar and a sociologist, I have a new project that engages me fully, more academic paths to travel, and students to mentor and learn with. Our second home has given me a place to try on other identities, reflect on who I have been and how that affects who I am now and might become, and think critically about my academic life and the work I do. I have an engaging academic life and respite when I need it; friends and colleagues around the globe; a new garden to tend and an opportunity to live and walk in nature; two dogs – Zen and Buddha – who keep me in the here and now; and a partner who hikes the trails with me. Together we seek to blend sociological eyes and communicative hearts and to continue sharing our dreams.

I grab hold of my totem – my mouse – and, feeling content for now with the reality I have presented, I send this file to Lonnie and Norman.

NOTE

1. Portions of the text that follows are adapted from Ellis (1986, 1995, 2004; 2009).

ACKNOWLEDGMENT

Thanks to Art Bochner for reading and editing this chapter and to Lonnie Athens and Norman Denzin for providing this opportunity to publish this work.

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**AUTHOR BIOGRAPHY**

**Carolyn Ellis** is a professor of communication and sociology at the University of South Florida. Her most recent books include *The Ethnographic I: A Methodological Novel about Autoethnography; Revision: Autoethnographic Reflections on Life and Work*; and *Music Autoethnographies: Making Autoethnography Sing/Making Music Personal*. She has published numerous articles, chapters, and personal stories based on interpretive representations of qualitative research. Her current research focuses on interactive interviews and collaborative witnessing with Holocaust survivors. She also enjoys watching the sunset from the deck of her North Carolina mountain cabin, walking in the woods with her dogs Buddha and Zen, and engaging in intense relational conversations and traveling with her best friend Art.
AM I NOW, OR HAVE I EVER BEEN, A SYMBOLIC INTERACTIONIST? AUTOBIOGRAPHICAL REFLECTIONS

Martyn Hammersley

ABSTRACT

Assigning or claiming identities can be a dangerous business. Labels carry conflicting meanings and, even more importantly, what is a laudatory term to some will be grounds for condemnation by others. My immediate response to the invitation to write this piece about becoming a symbolic interactionist, aside from the pleasure of being asked, was that I was not sure that I could claim, or even that I would want to claim, this label. I have a visceral dislike of theoretical-cum-methodological camps, not least because over the years I have been accused of belonging to a variety of these, from positivism to post-modernism. Reflecting a little more on the invitation, however, I realized that I could not reasonably deny that in the past, particularly in the 1970s, I regarded myself and was seen by others as an interactionist. Moreover, while my ideas about sociological work are now somewhat different from what they were then, and the direction of travel might be viewed as 'un-interactionist', in fact much of my work is
still focused on issues coming out of the interactionist tradition: notably, Blumer’s views about methodology, Becker’s arguments about ‘Whose side are we on?’, and the notion of analytic induction.

I still believe that interactionism, as a broad tradition, represents an important counter-trend to much that is currently wrong with sociology, and with the social sciences more generally.¹ For me, the list of failings it challenges would include the prevalence of speculative theorizing in much qualitative research today, as well as in ‘social theory’; demands for scientific rigour that try to reduce this to calculative or logical procedures, or require reliance upon what is only purportedly quantifiable and/or observable; and rampant political partisanship. I also have a methodological preference for the kind of ethnographic studies that interactionists typically carry out. Yet, I probably see the character of the interactionist tradition rather differently from many currently influential advocates, and I do not regard the boundaries around it as fixed and impermeable.²

My title alludes to a time in the United Kingdom when, in some quarters, ‘interactionist’ was a stigmatized identity. In the early 1970s, interactionism had been a fashionable and influential position, notably in the study of deviance, education and health. However, by the end of that decade it was less well-regarded. Indeed, in 1978, David Hargreaves was asking ‘Whatever happened to symbolic interactionism?’, suggesting that in the sociology of education, the field in which I was then working, it had been eclipsed by other kinds of work, notably those inspired by Marxism (Hargreaves, 1978). Moreover, similar developments had occurred in other fields in the United Kingdom, notably in the sociology of deviance (Downes & Rock, 1979).

In the circles in which I moved interactionism was subject to ‘interrogation’ from a number of directions. Ethnomethodologists and conversation analysts challenged its alleged theoretical assumption that the coordination of action requires participants to share the same meanings, its treatment of motives and institutions as factors shaping actions and their outcomes, and its lack of rigour in using verbal data. Marxists accused it of being ‘macro-blind’, neglecting and obscuring social structural forces and inequalities, so that it implied that the world could be transformed simply through changing how people see it. They also denounced it for ignoring the way in which perceptions and beliefs are ideologically generated. And feminists and anti-racists charged interactionist work with perpetuating patriarchal and racist attitudes, through the choice of topics for investigation, its ‘voyeuristic’
tendencies, and its ‘failure’ to adopt an activist stance towards the world. Furthermore, much more recently, near the end of my career, interactionism, along with all kinds of qualitative research, has been subjected to attack by advocates of revived forms of ‘scientific research’, notably those championing the randomized controlled trial as capable of determining which policies and practices ‘work’, treating this as the proper task for social research.  

A BRIEF LIFE HISTORY

I was born in Manchester, UK, in the North of England, starting life in Moss Side, an inner-city area where both sets of grandparents lived. My parents later moved to another part of Manchester, to a semi-detached rather than a terraced house, signalling a move ‘up’ in the world. They were part of a generation that experienced expanded opportunities for social mobility out of the working class into higher-income jobs (Goldthorpe, 1980). And I benefited not just from their good fortune and hard work but also from the fact that my generation’s chances of getting to university were much greater than theirs, as a result of expansion of the higher education system during the 1960s. I was lucky to enter university given that, at age 11, I had been consigned to a school for those deemed ‘non-academic’, destined for manual and clerical jobs, only later graduating to one of the early ‘comprehensive’ schools from which going on to university was a possibility.

I became a sociologist largely as a result of taking a course in sixth form on ‘The Uses of English’, which encouraged the development of arguments about social issues. The teacher, one of several who shaped my life, also engaged in a lengthy written dialogue with me where I defended cultural relativism and she challenged this. This experience turned me from the study of history, previously my favourite subject, to an interest in understanding contemporary society. Around this time I took out Karl Mannheim’s book *Man and Society in an Age of Reconstruction* (1940) from the local public library, incorporating some of his neologisms into a history essay, much to the disgust of the teacher who marked it. Later, I borrowed Merton’s (1949) *Social Theory and Social Structure* from another teacher, who had himself studied sociology at university, and I became fascinated by his typology of societal adaptations and discussion of anomie. Taking economics at school – it was not possible to study sociology there at that time – was another important influence, particularly Lipsey’s (1963) *Introduction to Positive Economics*, which I suspect shaped my style of thinking in some important ways, notably in its application of an analytic approach to
substantive problems, and its insistence on the limits to what economics can offer. However, I soon came to the conclusion that one could not understand even market behaviour and economic institutions without adopting a broader social perspective, so I decided to study sociology at university (though I wavered a little in the face of Meghnad Desai’s powerful pitch for economics at the freshers’ conference I attended).

I became an interactionist when I was an undergraduate at the London School of Economics (LSE) in the late 1960s. What the label meant to me then was very much shaped by the local context, and in particular by the dominant kinds of work done at LSE. The main theoretical perspectives at the time were various forms of ‘conflict sociology’. This constituted a reaction against structural functionalism, drawing on the work of Marx and Weber but shorn of their philosophies and politics. As with functionalism, the focus was still on the social system, but there was an emphasis on structural contradictions and other sources of conflict. Key figures were David Lockwood (1956, 1964; see also 1992), Lewis Coser (1956), and Ralf Dahrendorf (1959; see also Collins, 1975). As regards methodology, what was taught, at least at the undergraduate level, was primarily the use of official statistics and survey research. There was a strong local tradition of ‘political arithmetic’ in the United Kingdom, and especially at LSE, this employing quantitative data to investigate social and educational inequalities, often guided by ‘ethical socialism’ (see Dennis & Halsey, 1988). While I didn’t know it at the time, Paul Rock was already teaching at LSE, championing a more interactionist approach, but I didn’t take any courses with him. He later went on to write what is, in my view, still among the best books on interactionism (Rock, 1979).

I arrived as an undergraduate at the LSE just in time for the student rebellion of 1968 – I took a minor part in occupying the building on the weekend of the demonstration against the Vietnam War outside the US Embassy in Grosvenor Square. Within LSE there were vociferous reactions against current forms of sociology by those, mainly junior staff and postgraduate students, who championed Marxism – this time with the philosophy and the radical leftist politics very much left in, albeit often twisted in an anarchist direction. The writings of Lukacs, and especially Gramsci, were promoted, but also the more recent structuralist work of Althusser and his students from across the Channel. Robin Blackburn was one of my tutors: he was later sacked from LSE for publicly supporting students’ removal of gates installed to prevent future occupations (see Dahrendorf 1995, pp. 469–470; Platt, 2003, pp. 109–111) and became an editor of New Left Review. I recall his being shocked when we, first-year undergraduate students, complained that
Heidegger’s *Being and Time* was on the reading list, *in the original German*, for his seminar. Even had we been able to read that language, and only one of us could, I doubt we would have been able to make much sense of this book; and had we known then of Heidegger’s involvement with the Nazis we would, no doubt, have objected even more strongly, such was the politicized spirit of the times.

In my third year at LSE, I discovered the work of the symbolic interactionists, especially Blumer and Becker, and also ‘phenomenological sociology’. *Blumer’s Symbolic Interactionism*, *Berger and Luckmann’s The Social Construction of Reality* and *Schutz’s Collected Papers* became my guiding texts, along with Cicourel’s book on *Method and Measurement in Sociology*. I pored over these materials, even though they weren’t on any of the reading lists for courses I took. Later, I explored the growing interactionist literature in the field of deviance, and of course the work of Goffman. *David Matza’s Becoming Deviant*, with his championing of what he called ‘naturalism’, also came to be added to the personal canon I was constructing.

When it came to thinking what I wanted to do for a PhD, I had already decided that this was the sort of approach I wanted to take. It appealed for a number of reasons:

1. **The emphasis on agency rather than on systems and structures.** I saw this as an essential complement to conflict sociology’s stress on dissensus and diversity, as against the presumption of structural coherence and value consensus that was central to some versions of functionalism. The forms of Marxism that were becoming influential at the time were rather ambiguous on this issue, it seemed to me. In many ways, Marx’s epigram ‘[people] make history, but not in circumstances of their own choosing’ captures the spirit of interactionism rather better than it does that of much Marxism. For Marx and many Marxists, agency and action are located within a teleological conception of History derived from Hegel (*Prokopczyk, 1980*), or alternatively within a system theory that is not very different in *form* from that assumed by functionalism (see *Cohen, 1978*).

2. **Agency and action were approached from a sociological rather than from an entirely psychological perspective**, with an emphasis on how people collectively interpret and respond to situations, the cultural resources that they use to do this, and so on. Moreover, as already noted, the fact that this was not done through reliance upon the notion of a collective subject, with Humanity treated as the agent of History, seemed to me to be an advance over Marxism.
3. The diversity that was recognized extended beyond commitment to diverse values to include variation in ways of experiencing and interpreting the world. The fact that people could and did ongoingly ‘construct’ reality in different ways, through interpretation and action, was perhaps the central and most exciting feature of what I took interactionism to stand for at this time. This was, of course, very much a phenomenological reading, but I do not believe that it is a major distortion.

4. Interactionist work seemed to involve a cosmopolitan perspective on the world rather than a dogmatic, partisan one. I was committed to the same sort of leftist politics as many of my peers but I had also been interested in existentialism and had read the work of Albert Camus; and, as a result, it seemed to me that utopian forms of politics needed to be resisted. I felt that Camus’ social and political thought represented a more realistic, tolerant, open-minded perspective on the world even than the kind of Marxism adopted by Sartre, where the idea of what is good and what is bad, how things ought to be, and so on, are taken for granted, much as they were by many leftist sociologists in the 1970s, and still are today. I regarded the view of the world implicit in Becker’s work, especially, as very similar to that of Camus.6

5. The insistence that we must not assume that the world is how some theory claims it to be, that we should explore and inspect it and try to describe and explain it in its own terms. This sort of ‘empiricism’, a common accusation, seemed to me to be of great value. Equally important was the argument that very different methods were required from those characteristic of much mainstream sociology, notably some form of ethnography.

6. Interactionism also avoided the naïve romanticism that treats the views, experience and feelings of the marginalized or oppressed as genuinely authentic, and their culture as superior to that of any elite. While the importance of understanding the perspectives of all parties involved in a situation was emphasized, so that any hierarchy of credibility that operates within it was to be resisted, at the same time there was no pretence that those at the bottom of the hierarchy have any epistemic privilege (Becker, 1967). This is an idea that has increasingly been abandoned in favour of what Becker refers to as ‘sentimentalism’; and, ironically, his article has often been misinterpreted as supporting this position (see Hammersley, 2000, Chapter 3).

7. There was a commitment to understanding but also a recognition of the limits operating on the sorts and degrees of knowledge that are possible. In its original form, interactionism combined recognition of the constructed and plural character of perspectives with an insistence on the importance,
and the possibility of discovering what people’s attitudes actually are and what is really going on. While this represents an unstable — perhaps even a contradictory — orientation, it nevertheless seemed to me to be productive in sociological terms.

I went to the University of Manchester to do my PhD largely because I had discovered that there were people in that department who did interactionist work. However, when I got there I found that what had previously been a predominantly interactionist department — albeit headed by Peter Worsley, an anthropologist whose work was strongly influenced by Marxism — was now split, with a strong contingent of converts to ethnomethodological conversation analysis. The influence of Garfinkel and Sacks had begun just before I arrived in the department, and grew very strong while I was there. Initially I knew little about their writings, but I learned a great deal from participating in sessions on conversation analysis. In particular, this experience persuaded me of the value of audio-recording and of careful and detailed analysis of verbal data. However, while I became sympathetic to this approach, I never abandoned the more broadly interactionist perspective I had already acquired. It seemed to me that conversation analysis had much to contribute to interactionist ethnography, but that some of its underlying assumptions were open to serious question.7

While my supervisor, Isabel Emmett, was neither a Marxist nor an ethnomethodologist, she was not really an interactionist either. She had carried out an anthropological community study near Blaenau Ffestiniog in Wales, and later returned to study the town itself, but when I was at Manchester she was engaged in a survey on youth and leisure in the Manchester area (Emmett, 1964, 1971, 1982a, 1982b). With hindsight, I greatly regret that at the time I paid little attention to her work, or to the whole community study tradition (see Frankenberg, 1970); something for which my preoccupation with the newly fashionable interactionism, phenomenology and conversation analysis was largely responsible, along with her intellectual modesty.

My doctoral work at Manchester involved carrying out what I initially thought was entirely novel: the application of interactionist and phenomenological ideas, along with some of the resources provided by conversation analysis, to the sociological study of education, in particular classroom interaction in secondary schools. In this, I also drew on the fundamental questioning of schooling characteristic of the radical education and deschooling movements of the late 1960s. In fact, the latter were very much my starting point and my source of motivation: I regarded dominant forms of education as reproducing an unequal and alienating society. I wrote a small number of
articles on the basis of this doctoral work, though it took me nearly 10 years to finish my thesis – not a good model for students today (Hammersley, 1974, 1976, 1977, 1980, 1989b; see also Hammersley, 1984).

Early on in this work, I discovered that there were others pursuing similar agendas to me. In 1970, a book titled *Knowledge and Control* was published, and soon after that an Open University (OU) course appeared called *E202 School and Society*, these coming to be treated as representing a ‘new sociology of education’ (Open University, 1971; Young, 1970). This movement had started at the London Institute of Education, under the influence of Basil Bernstein and Michael F. D. Young, and was later developed further at the OU. However, around the same time, there were several other researchers going in somewhat similar ‘new directions’, for example, coming out of anthropology and sociolinguistics (see Atkinson, Delamont, & Hammersley, 1988; Atkinson & Housley, 2003, pp. 90–101 and passim). At Manchester, there were regular departmental seminars, which were very lively, with speakers being vigorously challenged by both ethnomethodologists and Marxists. One speaker in my early years was Paul Atkinson, and talking together we discovered that we shared very similar views about sociology and methodology. I also met Sara Delamont around this time, who was almost single-handedly challenging the quantitative approach that then dominated the British Educational Research Association. I also became aware of what was going on in the sociology of deviance in the United Kingdom, the work of people like Stan Cohen, Jock Young, Paul Rock and others, by attending the National Deviancy Conferences (Cohen, 1971; Taylor & Taylor, 1973; see also Atkinson & Housley, 2003, pp. 46–47).

In 1975, I was invited to a conference organized by Peter Woods, who was working at the OU, and in that year I too obtained a job there. I found that Woods was also using interactionism to rethink the sociology of education (Woods, 1979), and he subsequently played an important role in coordinating a community of interactionists studying educational processes in the United Kingdom, notably through organizing what came to be called the St Hilda’s conference, named after the Oxford college where the first conferences were held. He also ran several influential research teams that applied an interactionist approach in the sociology of education (see Hammersley, 1999a). We published several books together that showcased interactionist work on education (Hammersley & Woods, 1976; Woods & Hammersley, 1977, 1993).

*Schooling and Society*, the course at the OU for which I had been recruited, was a re-make of the earlier course that had already been very influential in spreading interactionist ideas. However, just as the situation at
Manchester was not what I expected when I got there, so too things had moved on at the OU, this time in the direction of Marxism. The new course team was split, one side working broadly within interactionism while others promoted an approach influenced by, for example, Althusser’s (1971) article ‘Ideology and ideological state apparatuses’ and the work of Bowles and Gintis (1976). The Marxist character of the early parts of the new course attracted national attention, and began to be publicly criticized in newspapers and on the radio before we had even completed writing the whole course. The course team were accused of ‘Marxist bias’, and there was an internal inquiry within the University. Questions were raised in the UK parliament about the issue, and it was said that the Secretary of State for Education was reading the available course materials himself. The interactionists on the course team were caught in the cross-fire, having to defend the course against external attack by forces from the political Right.

However, perhaps the most significant event in my intellectual career at the OU was that, immediately after finishing work on *Schooling and Society*, I was invited to join the team working on a new course in research methods. As a result of the influence of interactionism, I had become interested in the relationship between theoretical ideas about society and the methods used to investigate it. I’d also read the then fairly small literature on qualitative method. But I was relatively ignorant about quantitative research, having previously largely rejected it – using the stock arguments common among qualitative researchers at the time, and still given too much credence today (Hammersley, 2008, Chapter 1). Now, as the only practising qualitative researcher on the course team, I had to engage with people committed to various kinds of quantitative work, and I found that the issues dividing us were more complex and uncertain than I had previously realized. Jeff Evans, who was responsible for bringing me on to the course in the first place, proved an important guide in exploring these issues, then and subsequently (Evans, 1983).

As a coordinator for the units in the course dealing with qualitative method, I commissioned Paul Atkinson to write two of these, while I wrote the others. We also made a couple of television programmes together, with Paul taking the lead role. Subsequent to this, we decided that we could build on what we’d written to produce an introductory text on ethnography (Hammersley & Atkinson, 1983). In doing this, we found that we agreed on many matters, and as far as I can remember there was little debate between us about the issues. He proved an extremely stimulating intellectual collaborator and provided support that was essential given my isolation on the course team. While, subsequently, our views have diverged a little, this has not been
a problem in preparing later editions of our book: pragmatism has long been the order of the day for both of us (Hammersley & Atkinson, 2007).

Another significant contact for me, a little later, was Phil Strong, one of the most important interactionist researchers in the United Kingdom, who worked in the field of health and drew particularly on the work of Goffman. For a short period of his career, he held a post at the OU, within another faculty, and we were able to meet up and share ideas. His broad interests and thoughtfulness, and marvellous essays, were illuminating, and his death at a young age was an intellectual tragedy as well as a personal one for those who knew him (see Bloor, 1996; Strong, 2006). His essay on sociological imperialism, in particular, was very influential for me. It pointed to the ways in which sociologists’ criticisms of medical imperialism in the health field could be turned back against them: as themselves designed to serve sociologists’ interests in claiming authority to speak about health issues in competition with doctors. This resonated with growing concerns I had about the ‘grand conception’ of sociology (Hammersley, 1999c): the tendency for some interpretations of the sociological mission not only to claim superiority over other social sciences but also to present sociological knowledge as a sound and sufficient basis for political and other kinds of practice. Examples of this include the sorts of “critical”, “public”, or “civic” sociology advocated by Mills, Gouldner, and more recently Burawoy and Wacquant (Hammersley, 2000b, 2004d, 2005b).

Working on the OU research methods course (Open University, 1979), and being forced to take the arguments behind quantitative work more seriously, led me to question some of the assumptions that I had initially adopted from interactionism and qualitative methodology (Hammersley, 1989a, 2010b). The differences between approaches were not as clear-cut as I had thought, nor were the positions I had adopted unproblematic. For example, I discovered that in the 1930s Blumer had been arguing against a form of positivism, represented especially clearly by Lundberg, whose character (despite major differences) bore some worrying resemblances to the constructionist strand of interactionism evident in nominalist readings of labelling theory that I had myself found appealing. Blumer countered this positivism with a form of realism, but his position itself came to be criticized as turning Mead’s stance in a nominalistic direction (Lewis & Smith, 1980). Around this time I began to read primary and secondary material about pragmatist philosophy, coming under the influence of Peirce. While his work is open to divergent interpretations, from being seen as a precursor of logical positivism to being regarded as an objective idealist, what was most important for me was his account of the nature of scientific inquiry (see Rescher, 1978),
and the epistemological and ontological arguments he deploys in explicating
this. Later, I discovered the work of Susan Haack, which develops this aspect
of Peirce’s thinking, and this too has been a very important influence on me

At this time, I came to the conclusion that a major problem with the
growing body of qualitative work, in education and in other fields too, was
a failure systematically to build and test theories. I explored what this
required in several articles (Hammersley, 1985, 1987a). This was integral to
the empirical research I did with two colleagues – John Scarth and Sue
Webb – looking at the influence of variations in assessment regime on modes
of teaching in secondary schools. We realized that this focus required us to
use quantitative as well as qualitative data, and to find ways of relating the
two; and in the course of this we became very critical of both sides of
the quantitative–qualitative divide (Hammersley, Scarth, & Webb, 1985;
Hammersley & Scarth, 1986a, 1986b, 1987). This experience led to articles
on quantitative method – dealing with reliability and validity, internal and
external validity – and also to a series of methodological assessments of
qualitative studies, resulting in a book on reading and assessing ethno-
graphic work (Hammersley, 1987b, 1991, 1997a). This work also led on to
an interest in analytic induction, which still continues (Hammersley, 1989a,
2010c, 2011b; Hammersley & Cooper, 2012).

In the 1990s, my work took another turn, being shaped by one of my
PhD students, Peter Foster. He carried out a study of a school that had a
reputation for its anti-racist policy, arguing that there was little evidence of
racial discrimination on the part of teachers in the school. He suggested,
instead, that the underachievement of black students at the regional, and
also at national, level stemmed as much from differences between schools as
from processes within them (Foster, 1990). His argument went against the
conclusions of much research on ethnic inequalities in English schools at the
time, which implied that most teachers were racist and that this was what
produced national ethnic inequalities in educational achievement. Subse-
quently, Foster criticized some of that research, and a major dispute
arose that was frequently acrimonious (see Hammersley, 2000, Chapter 4).
A colleague, Roger Gomm, and I wrote some articles in support of Foster’s
position, and all three of us were then accused of racism. This experience made
me reflect further on the naïve political assumptions that seemed to shape
a lot of social research, not just that concerned with ethnicity and ‘race’. So,
for example, I examined the claims made about feminist methodology
(Hammersley, 1992a), challenging amongst other things its commitment to a
political framework of analysis, and later I extended my critique to ‘critical’
approaches more generally (Hammersley, 1992b, 1995, 2000b). All this forced me to rethink my experience back to the politics of the 1960s, and the way I too had been ready to build political assumptions into my work. With Peter Foster and Roger Gomm, I moved towards a more Weberian position (Foster, Gomm, & Hammersley, 1996). However, Foster committed suicide in 1999, and this cast a long shadow over my subsequent work and life. I felt partly responsible for how he had been exposed to public vilification, and I lost any respect for those parts of the social science research community who had subjected him to this treatment.

In these ways, I became increasingly critical of dominant trends within the fields in which I worked, and this included methodology. Of course, that field was by no means standing still, in fact it was being pushed in directions even further from my own position. This was symbolized, for instance, by the sorts of approach represented in later editions of the Handbook of Qualitative Research (Denzin & Lincoln, 2000, 2005, 2011). Around this time I became involved in an informal trans-Atlantic correspondence with John K. Smith about criteria for assessing qualitative work; and this was productive for me even though it generated little agreement between us (Hammersley, 1998, 2004c, 2009; Smith & Hodkinson, 2009). Later, I tried to spell out the grounds for my disagreement with these increasingly influential trends within qualitative research methodology (Hammersley, 1999b, 2008).

Already at this point, qualitative research had started to come under attack from those who argued that the function of social research is to produce findings that facilitate evidence-based practice. This idea began with the evidence-based medicine movement of the 1980s and subsequently spread to other fields, including social work, education, social policy and criminology (see Reynolds & Trinder, 2000). It was argued that much professional practice in these fields was not based on rigorous empirical research but rather on outdated practitioner folklore, with the result that many treatments were ineffective and some even damaging. It was proposed therefore that practice should be based on the results of research regarding ‘what works’, with randomized controlled trials being treated as the gold standard, synthesized in the form of systematic reviews.

I was critical of this notion (Hammersley, 2001, 2004a, 2006a), and I became a target of criticism in turn, with one of the advocates of evidence-based medicine, Iain Chalmers, apparently suggesting in at least one talk that my attitude would change if I were lying seriously ill on a trolley in a hospital, and viewed matters as a patient needing the best treatment possible. The impact of the evidence-based practice movement generated a
major crisis in some areas of research, especially education, in the United Kingdom in the late 1990s, and subsequently in the United States. Interestingly, one of the major figures on the other side of this debate, David Hargreaves, had originally championed interactionism within the sociology of education, and had made important contributions to that field (Hammersley, 2000a; Hargreaves, 1996). I wrote a response to his influential call for research that demonstrates ‘what works’, albeit a response which found rather more common ground with him than did most other reactions to his lecture (Hammersley, 1997b). Several important issues arose in the course of this, in particular concerning what counts as the development of knowledge, how this can be facilitated, as well as how it is to be summarized and conveyed to lay audiences, and weighed by them against other sources of information in light of their value commitments.

These developments not only stimulated me to think more deeply about the relationship between research, policymaking and practice (Hammersley, 2002) but also to finish an empirical study that I had started earlier. This looked at how research is represented in the mass media, one of the main routes through which it is likely to have any major impact on policy and practice. I examined the ways in which a review of research on ethnic inequalities in education had been covered on television, on the radio and in the newspapers, exploring the complex relations between research as a source of knowledge and media representation of findings and their implications. My focus was on possible media bias, given that researchers frequently complain about how the media distort their work (Hammersley, 2006b). My conclusion was that while there were some examples of blatant misinterpretation in this case, a lot of the variation in coverage reflected the complex message contained in the source. There are, however, major issues here about what the function of the media ought to be, and how newspapers, television channels and the new media operate as regards the provision of news and knowledge about current affairs. This has crucial implications for ideas about the function of social science, for how social scientists should engage with the media, and for concepts of discursive democracy.

In the debates about research and evidence-based practice, as with the earlier Marxist bias controversy, I found myself caught in the middle between warring parties – seeking to resist what seemed to me to be very undesirable developments on both sides. I became increasingly concerned about the positions taken by many of the defenders of qualitative enquiry, especially in the United States, against arguments for ‘more scientific’ forms of social research (see Denzin & Giardina, 2008). These proposals were denounced in political terms, and qualitative work portrayed as arts-based rather than
scientific, often in effect as a kind of agitprop or political therapy (see Denzin, 2010). The new emphasis in some quarters on quantitative method and applied research had to be resisted, but my view was that this should be done on methodological not political grounds, that we needed to take proper account of the complexities of the issues involved, and that qualitative work had to be defended as a form of social science (Hammersley, 2008). Central here was a challenge to the caricature of science on which much dismissal of a scientific approach relies, and clarification of some of the difficult issues involved (Cooper, Glaesser, Gomm, & Hammersley, 2012). This was not a popular position to take, and was seen by some interactionists as a kind of betrayal (see for instance Denzin, 2009, 2010); a reaction, I suggest, that is symptomatic of the ideological mindset that prevails in some quarters today (Hammersley, 2010a).

Looking back on my career now, I see a decline in the fortunes of academic social science, in the face of politicized and arts-based approaches to qualitative work, on one side, and increasing ‘strategic management’ of research, within universities and beyond, that is designed to make it the servant of policymaking and practice, on the other (Hammersley, 2011a). It is hard not to despair.

**CONCLUSION**

So, am I still an interactionist? In some ways, writing this piece has shown me that I am, even more strongly than I had recognized before. But what I draw from the interactionist tradition is seriously at odds with the positions now adopted by many researchers who would place themselves in that camp. While I do not believe that battling over the label is worthwhile, the underlying issues are of great importance. Much hangs on how they are resolved.

These issues include whether or not we are engaged in social science; if we are, what the nature of the intended product is (description, explanation, theory, evocation, critique, social justice, personal expression, radical social change, etc.); and what is required in methodological terms if we are to produce this kind of product. I fear that there is little possibility of agreement about these matters. But we must consider the likely consequences of failing to reach agreement in the face of the challenges (political, institutional and economic) that now face social science.

In addition, of particular relevance here, are questions about the role of labels like ‘interactionism’ within social science today. What does, and should, the term ‘interactionism’ stand for, in relation to the issues that I have just
outlined? There is a historical dimension to this question, of course, and while I do not believe that the position we take on these issues should be determined by historical precedent, we can learn a lot from, and we lose much by ignoring, the past; or by viewing it through the distortions generated by current views. Whatever the failings of Lewis and Smith’s (1980) attempts to grasp the differences between versions of pragmatism and their implications for symbolic interactionism, their work had the virtue of highlighting how much had been forgotten about the views of G. H. Mead and other forerunners, and this forgetfulness continues in many quarters today. Notable here is the extent to which Mead’s work was located within the framework of evolutionary biology and functionalist, behaviouristic psychology, as well as his ‘positivistic’ conception of scientific methodology (see e.g. Mead, 1917, 1936, Chapter 13 onwards). I am not suggesting that we should go back to Mead and simply follow his guidance, but understanding his views would be a useful antidote to the kind of modernism, often dressing itself up as postmodernism, which claims to have seen the future and on this basis sneers at the past, insisting, like previous incarnations of modernism, that we live in unprecedented times. In fact, most of the problems we face are old ones, and we can learn from previous attempts to resolve them even though they do not provide us with ready-made solutions (Hammersley, 2004e). For me, the term ‘interactionism’ refers to one strand of thinking within sociology that still has much to teach us. It is no more than that, but it is important and valuable all the same.

NOTES

1. For arguments about its importance in this respect, see Atkinson and Housley (2003) and Martin and Dennis (2010).
2. Atkinson and Housley (2003, Chapters 2 and 6) have raised the general question of whether British sociology has ever contained an interactionist school, while also noting how interactionist ideas have had wide influence within it.
3. See, for example, Oakley (2000), but see also Oakley (1998) and Hammersley (2000a).
4. I remember the Socialist Society posting an open invitation to any member of the Sociology Department willing to attend a meeting to defend the idea that conflict has positive functions in current society, along the lines suggested by Coser (1956), building on the work of Simmel.
5. One of my tutors at LSE, Gabriel Newfield, set me an essay on Kuhn’s (1962) *The Structure of Scientific Revolutions*, and this too became a very important source for me.
6. I didn’t meet Howie Becker until quite recently, despite our having made a radio programme together (recorded in separate sections on either side of the Atlantic) in the late 1970s, and being in correspondence on several occasions subsequently.

7. It was 30 years before I finally managed to sort my ideas out about ethnomethodology and conversation analysis: see Hammersley (2003).

8. The Open University is a distance teaching institution, founded in 1969 by a Labour government, providing published course materials, these supported in the 1970s and 1980s by radio and television programmes on the BBC. Its undergraduate students are part-time, recruited on a first-come-first-served basis, with no qualification requirements other than being over 21 (now over 18). Students have access to tutorials designed to facilitate their work on the course materials. See http://www8.open.ac.uk/about/main/the-ou-explained/history-the-ou

9. These conferences are still going today though the name was later changed to the Oxford Ethnography Conference: see http://www.ethnographyandeducation.org/conferences.html


11. For his published critique, see Chalmers (2003), see also Hammersley (2005a), and Chalmers (2005).

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Am I Now, or Have I Ever Been, a Symbolic Interactionist?


**AUTHOR BIOGRAPHY**

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THIS REALLY ISN’T ABOUT ME: REFLECTIONS ON AN INTELLECTUAL AND ACTIVIST PATH

John Myrton Johnson

ABSTRACT

Reflecting on the contingencies and felicitous moments of life and career, a senior scholar celebrates the intellectual community and friends that inspired and sustained his efforts.

My Johnson ancestors emigrated from Caskabein (north of Aberdeen, Scotland) in 1616 to the Isle of Wight County, Virginia, and for the next 300 years they were small farmers and pious Quakers, twice losing family farms for “refusal to muster” (report for militia service). Laban’s widow Sarah and some others moved to small farms in central Indiana in 1832, where my father was born in 1906, near the Friends Meeting House at Rich Square in Henry County, the area popularized by Ross Lockridge’s famous 1948 novel, Raintree County. My lanky grandfather Myrton graduated from Earlham College in 1897, and played end on the football team. My father Wilmer graduated from Earlham College in 1928, majoring in history and
French. He won two national and one state singles tennis championships, and shortly thereafter moved to the city where I was raised, Fort Wayne, Indiana. During my childhood, it was a city of about 100,000 people, predominantly white, where no one locked their doors; if no one was at home, the milkman walked into the kitchen to inventory the refrigerator before deciding what dairy products he delivered.

My happy childhood was filled with family activities and visits, friends, sports, a cool sister, school success, and some minor troubles. It is possible to spin this childhood story in several ways, emphasizing the successes or the troubles. The former spin would note the sports championships, in five sports, including the state singles tennis championship, the various youth leadership roles, including the presidency of my high school class, and the school success, including a mathematics scholarship to Indiana University (IU). The other spin would note the six suspensions from school between 1954 and 1959, mostly for elaborate practical jokes directed at school or local political officials. The latter spin masks the criminality. The stories of these “practical jokes” still provided humor at the 50th high school reunion. My 400 classmates at Fort Wayne North Side High School voted me “the boy with the prettiest legs.”

EARLY EPIPHANIES

When I entered IU at Bloomington in 1959, I joined a Greek-letter fraternity, I admit with chagrin, and misspent the early years learning about alcohol and girls. Being marked as a potential fraternity leader, I attended the National Leadership Summer School, where I was invited to join secret meetings when the adult leader of the national fraternity, Dr. Glenn Nygreen, Dean of Students at Bowling Green State University, mapped out a strategy to subvert the fledgling civil rights efforts of the early 1960s, to keep the fraternity white. This incensed me, and I returned to school in the fall with a passion to counteract this strategy, and integrate the fraternity. I failed, completely. Moreover, many of the 107 members communicated that I was no longer welcome there. These sentiments were communicated to others outside the fraternity, so I began to learn first-hand about the Indiana Amish tradition of shunning. My first courses in sociology were taken during this time of identity change and increasing marginalization. I recall my Introduction to Sociology teacher saying that all fads and fashions begin on the U.S. coasts, and end in Indiana, as if we needed further proof of our Midwestern backwardness.
Existentialist themes were common fare in our late-night undergraduate discussions about the meaning of life, and in this context I encountered the works of Sartre, Camus, Nietzsche, Kierkegaard, Dostoyevski, and so on. I also read the Beatnik poetry and literature, and I was especially captured by Jack Kerouac’s messages about spontaneous freedom and adventure, as expressed in his books *On the Road* and *Dharma Bums*. Thus, in my third year I gave away all of my worldly possessions and left school, with only $20 in my pocket, and spent the next year hitchhiking, riding the rails, occasionally living in hobo jungles, outrunning the yard dicks, and working odd jobs across the United States. I worked as a construction worker in Boca Raton, as a garbage hauler and dishwasher in St. Petersburg, as an oyster-opener in New Orleans, and as a door-to-door salesman in Houston and Baytown, before heading south to Mexico for several months, living mostly at *La Casa de Juventina Campos* in Zihuantejeno, Guererro. Upon returning to the United States, I worked at several jobs on the West Coast, including a job at the 1962 World’s Faire in Seattle. I had many exciting and harrowing experiences during this interregnum, and when I returned to Bloomington I was a much different (and more mature) person. While I had sought a vague kind of spontaneous authenticity during my adventure, only many years later did I learn that the manic coast-to-coast sojourn reported by Kerouac in *On the Road* had taken but five days. Irony has been a close companion in my life.

**BACK HOME AGAIN IN INDIANA**

At IU Edwin Sutherland died in 1959, and the emerging intellectual star in sociology was Albert Cohen, who had received Dharma transmission from Parsons at Harvard. My other teachers included Melvin DeFleur, Allen Grimshaw, the old commie and nudnik Joseph Schneider, Richard Hall, Karl Schuessler, and my first tastes of symbolic interaction from the saturnine Sheldon Stryker and the avuncular Alfred Lindesmith. The latter was my favorite, and I had three courses with Lindy, even though by this time he did not so much “teach” as tell stories, but those stories of the life-long battle with his nemesis Bureau of Narcotics chief Harry Anslinger were truly riveting. I lived alone, worked full time as a waiter, and took my studies very seriously.

During my last year in Bloomington, I became very involved in political activism, not a popular activity for many of the 23,000 students. I joined and participated in N.A.A.C.P. and Congress of Racial Equality (CORE),
Before Stokley Carmichael kicked out the whites (in 1963). I joined the American Civil Liberties Union (ACLU), and participated in boycotts and civil rights demonstrations. I joined one demonstration of the Fair Trade for Cuba Committee, where the 15 demonstrators were met by over 5,000 counterdemonstrators, which gives some measure of the political climate in Indiana at the time. I developed some pride in becoming a “liberal,” which meant marching for civil rights and reading the Louisville Courier-Journal rather than the hated Indianapolis Star. Thus began a pattern which has continued for the rest of my life, joining my intellectual interests with social and political activism to make positive changes in the world. My loving parents were always supportive of my changing beliefs and actions, even though it cut against the grain of my politically conservative family.

After more than five decades, it is difficult to recall with great precision what so excited me about the promise of sociology, but I felt great passion about its potential to illuminate social life, while proving useful in efforts to work for greater justice. Since I retain much of that passion today, perhaps I am justifying my current passions and interests by projecting to the past the continuity of a bad habit!

**BECOMING A SPEAR CARRIER FOR THE EMPIRE**

When I lost my student deferment from the draft, I elected to enter the U.S. Navy as an officer, rather than being inducted as an Army grunt. Following Officers Candidate School at Newport, Rhode Island, I was assigned a three-year tour of duty aboard USS Taussig (DD 746), a World War II destroyer, the smallest combatant ship with a complement of about 300 men and 17 officers. At first my primary billet was Gunnery Officer (in charge of about 20 men in Second Division), and later Anti-Submarine Warfare Officer. I also had many secondary billets and collateral duties, the most significant being Legal Officer on the ship. I gained this latter job in an unusual manner. I once erred in successfully defending a young sailor at a court martial. Several months later three other sailors were prosecuted for negligent manslaughter, and various other charges, all related to the death of David George Darling aboard ship (at the beginning of a gunnery mission near North Vietnam). They asked me to defend them. I was successful in my defense of the three enlisted men (Abraham, Romancik, and Erwin), in separate court-martials. This produced two outcomes. First, the enraged Commanding Officer Samuel Lee Graveley said, “When I send someone to a court martial, they are guilty, and I want them found guilty.” He then put me in solitary confinement for
nine days, at Subic Bay, Philippines. I spent the nine days reading Gunnar Myrdal’s *An American Dilemma*. And second, he appointed me Legal Officer so that I would be the prosecutor for all subsequent cases aboard ship. I estimate that I prosecuted about 18 cases before I left the Navy in 1967, but about half of these were related to the same event.

During my 3 years aboard USS *Taussig*, we made two long deployments to the West Pacific, called Westpac, the first one being about 9 months in duration, and the second about 11 months. The first deployment involved some minor involvement in Vietnam, not as yet declared a war, but the second deployment involved actual wartime engagement, with firing missions in country down south, and on the shores up north. These events and experiences provided the basis for some of my early publications. Being in the Navy was not easy for me, and I was glad to be discharged, and head back to graduate school on the G.I. Bill.

**GRADUATE EDUCATION IN CALIFORNIA DURING THE 1960S**

I entered San Diego State University (SDSU) in 1967, to work on a master’s degree in sociology, and immediately encountered the handsome, athletic, charismatic Nicos Mouratides, one of Don Martindale’s 55 PhDs from Minnesota. Nicos had fought as a freedom fighter in the guerrilla forces seeking to liberate Greece and had represented Greece as an athlete in London at the 1948 Olympic Games. All of the better students were drawn to Nicos. He had quite a following, including some who remained in San Diego just to retake his graduate seminar over and over again, which is what I did as well. He called himself a “phenomenological Marxist,” hastening to self-identify with the early as opposed to the late Marx. Strangely, we read relatively little phenomenology in his seminar, and no Marx at all. What we did read, over and over again, was Talcott Parsons’ *The Structure of Social Action* (1937), which Nicos liked because of its “voluntaristic theory of action” (with strong affinities to symbolic interaction). Our high regard for Parsons was put askew when he visited SDSU for a talk, and fell off of the stage. We once spent an entire semester reading and discussing Edward A. Tiryakian’s *Sociologism and Existentialism* (1962), thus setting a link between my earlier and later intellectual interests.

At SDSU all of my close friends were “peaceniks,” and I happily joined with them in the antiwar movement, initially concealing my prior identity as a military officer. We bought grass by the kilo, and inhaled all of it.
During my second year at SDSU a new Department of Sociology was forming at the University of California, San Diego (UCSD), located in La Jolla. Joe Gusfield was hired as the founding Chair, but he remained at his position in Urbana until the next year. His first hire was Jack D. Douglas, a 1965 Princeton PhD who had previously taught at Johns Hopkins, Wellesley, and UCLA. Jack hired me to be his teaching assistant, until the Department was formed as an administrative unit. The plan was to begin a PhD program, and I was one of nine members of the first PhD cohort. I was in part motivated by my desire to continue attending Nicos Mouratides’ seminars.

Jack Douglas is a towering intellect, who has read widely in all disciplines, about everything. He had been at Harvard during the Parsons–Bales–Homans ascendancy, which were the latter years of Sorokin (Jack’s intellectual role model). He then moved to Princeton for his PhD. His dissertation was on suicide, significantly motivated by the mysterious circumstances of his older (by one year) brother Bill’s death. It was published in 1967 as *The Social Meanings of Suicide*. He was enormously productive during the late 1960s, and wrote or edited over 12 books between 1968 and 1972. He thought the early progenitors in sociology had taken very disastrous steps in their mechanistic, then later structural or social system directions, and he sought nothing less than creating new and better foundations for all of social science. This new foundation involved careful and rigorous observations of everyday life, in all of their myriad diversity and complexity, in Southern California at that tumultuous moment. He communicated a grand vision of a better science, and thus a better future, a future we could help create by being its humble but noble midwives or handmaidens. When we reported to him a casual remark overheard in the coffee shop, he saw direct linkages to what Thucydides had done to figure out the origins of the Peloponnesian Wars. If we reported a private or family indiscretion, he immediately saw the connections to St. Augustine’s *Confessions*. Our strolls with Jack among the eucalyptus trees on the UCSD campus were just like Plato’s peregrinations with his teacher Socrates at the Athenian acropolis.

There were exciting additions to UCSD Sociology during these early years, including Harold Garfinkel’s doppelganger Aaron Cicourel, who brought his paladins from Santa Barbara, Bud Mehan and Ken Jennings, and later Kenji Ima. Reinhard Bendix’ arrogant tulku Randall Collins joined the faculty, and the ebullient Stanford Lyman came over from Reno, bringing his brightest students Richard Brown and Tetsuden Kashima. I spent one year as a teaching assistant for Aaron Cicourel, when I learned about ethnomethodology from the horse’s mouth, and another working for Alvin W. Gouldner, just as his major book *The Coming Crisis of Western Sociology* (1970) was coming out, when I learned that Randall Collins really
wasn’t the most arrogant person in the galaxy after all. Since Joe Gusfield controlled the purse strings of soft money, he invited all of his Chicago School buddies as speakers, so we met David Reisman, Seymour Martin Lipset, Bob Habenstein, Anselm Strauss, Bennett Berger, Jerome Skolnick, Ralph Turner, Fred Davis, and so on. Joe wanted to create the postwar Chicago School all over again, literally, so many of these old buddies received job offers, and some of them actually accepted. While Joe’s vision (2003) never came to fruition, inviting all of these mid-career symbolic interactionists into this hot spot of Southern California existential-phenomenology-ethnomethodology produced a creative ferment among the graduate students. We argued, debated, and wrote papers about the ontological and epistemological nuances of science and everything else. Some of this fervor is captured in Douglas (1970) and Douglas et al. (1980). Those influenced by Douglas sought to create a new paradigm that fused existentialism, phenomenology, naturalistic field research, and later symbolic interaction (see Fontana & Kotarba, 1984). Those influenced by the ethnomethodologists sought to create a new paradigm later called conversational analysis.

Jack Douglas led us to expect that we could discover the font of all wisdom in a mustard seed, or perhaps more accurately, in the most commonplace and ordinary situations of everyday life. He encouraged all students to do naturalistic field research, much like in the earlier traditions of Franz Boaz or Robert Park. With this encouragement I conducted a field study of Child Protective Service workers, beginning in Los Angeles, and then one full year in San Diego. During that year I hardly missed one day of accompanying CPS workers on their routines, and I was scrupulous in recording my field notes. Jack had high praise for the methodological appendices of William Foote Whyte’s Street Corner Society and Melville Dalton’s Men Who Manage. He encouraged us to study the research process itself, and this is what led to my book on doing field research. Many people have told me that this book played some role in the reflexive turn in ethnography, but others said it reported more than they ever wanted to know. For over 35 years I have prepared myself to respond to a question of what my wife said when she learned that I had reported my marital infidelity in print, but so far no one has asked. It is a testament to Jack’s magnanimous and tolerant spirit that all of his students felt free to speak the truth.

Before leaving UCSD, I met several people who have become life-long friends, and important contributors and supporters of my work, including Andy Fontana and David Altheide at UCSD, Norman Denzin, a new PhD from Iowa, and Lonnie Athens, Herbert Blumer’s last PhD student at Berkeley. Shortly after that I met other life-long friends, Patti Adler, Peter
Adler, and Joe Kotarba. With Jack Douglas, these friendships have been the lifeblood for me, and I cannot overemphasize their importance. I’ve always thought of myself as a member of a team, as one small contributor to a transcendental project of advancing and teaching about our cultural legacy from all civilizations, all brothers and sisters, all thinkers.

**SOCIOMETRY IN ARIZONA**

In 1972, I began a tenure-track appointment in a department of sociology that suffered from a collective inferiority complex, which they sought to remedy by adopting the forms and mantras of Positive Science. Ethnography or qualitative research was barely tolerated, due mostly to liberal sentimentality. David Altheide joined the unit in 1974, and with our new friends Bob Snow and Bud Pfuhl we suffered the slings and arrows of the gerontocracy, or “The Wayne State Mafia” as we called them. It was in many respects a hostile work environment, but we were all young, we worked hard, and published more than the early retirees. We still have a lot of crazy stories and crazy (mimeographed) memos from that era. Whenever things got bad for us, Altheide would say, “At least this isn’t Weyerhauser,” a reference to the fact that young males in his home state of Washington had few options other than working for the dominant lumber industry giant.

As soon as I arrived in Arizona, I began my 30-year involvement in domestic violence, initially animated by my dissertation research of CPS. I was at first fascinated by child abuse and neglect, and especially the social movement and legal aspects. To learn more, I joined several organizations in this field, and the more I learned, the more my interests expanded beyond child abuse and neglect. I joined a small group of five activists to found a shelter for battered women, in 1978, ironically called *Friends of the Family*, and with this experience behind me I founded or helped found seven other nonprofit organizations in the coming years, most of them serving victims. One of the other cofounders of *Friends* was a graduate student Kathleen Ferraro, later my wife and mother of my two children, whom I married in order to follow a Johnson tradition begun by my father and grandfather, who also married one of their students; in today’s media age, we would likely star in some bizarre TV reality show on intergenerational perverts.

During these years, John Lofland called and asked me to play a role in the new journal *Urban Life* (later called *Urban Life and Culture* and then *Journal of Contemporary Ethnography*), which I did for 10 years, in order to facilitate the publication of qualitative work. And then Greg Stone called
one day from Minnesota to ask if I would join the new Society for the Study of Symbolic Interaction (SSSI), significantly animated by the desire to seek an alternative to the American Sociological Association (ASA), another hostile work environment that marginalized qualitative work. He made it clear that by soliciting my participation he wanted to expand the horizons of SSSI beyond the seminal group of founders from Midwestern universities and build bridges with all the intellectual ferment going on in California. And then Sara and George McCune (SA from Sara plus GE from George = SAGE), formerly editors at Macmillan where I published my book with The Free Press, asked me to edit a book series for Sage Publications, Inc., Sociological Observations, to publish ethnographic and qualitative monographs, which I did for about 20 years. Performing these roles put me in an important networking position for the promotion of qualitative work and filled me with a sense of purpose for larger, transcendent goals. They also extracted a price in terms of my own research, intellectual development, and personal life.

**THE TRANSITION TO JUSTICE STUDIES**

In 1982 Altheide, Ferraro, and I transferred to the School of Justice Studies, where two factions existed in an uneasy peace. One faction called themselves “The GOBS” (the Good Old Boys) and represented the older connections to criminal justice, whereas the second faction sought to build a new paradigm of justice where the definitional boundaries extended beyond the narrow confines of criminal justice, to include social and economic justice, human rights, environmental justice, peace, and so on. The members of this younger faction were all newly minted PhDs from social science disciplines, who combined social science research interests with normative or activist interests in justice. This younger group called themselves “the family,” which seemed logical because few of them had a family, and they conducted their lives in a manner so as to destroy the families of the remaining few who did.

The transition to Justice Studies proved relatively easy for me, because all of these young academics were activists of one kind or another, and now I had an intellectual justification for what I had been doing all along. I began another new course in Domestic Violence, this one with a justice spin, and to my new Justice Theory course I merely integrated the Holy Trinity from sociology (Marx, Weber, Durkheim) with an older Holy Trinity from justice (Plato, Aristotle, St. Thomas Acquinas), adding feminist, interactionist, and postmodern materials along the way. And Altheide and I were allowed to
teach courses on qualitative methods, which the gerontocracy never would have allowed in sociology.

The young turks in Justice Studies continuously debated the parameters of “justice,” and how these could or should be integrated into the curriculum. These debates produced a collectively edited volume with the title *New Directions in the Study of Justice, Law, and Social Control* (School of Justice Studies, 1990), and for me personally influenced my SSSI 1994 Distinguished Lecture “In Dispraise of Justice,” where I sought to integrate the domain of justice studies with the emphasis on emotions expressed in the early works of existential sociology.

During the early 1980s, I refocused my Domestic Violence interests, and spent several years running groups for violent men, and published articles on the battered women’s shelter movement, battered women, abusive men, the police responses, and the effectiveness of domestic violence intervention programs. I even spent one year as the first Administrator for the City of Phoenix Diversion of Violent Emotions (DOVE) program. I helped teach a class at the Arizona State Prison in Florence in 1980, and this began an involvement with the prison system which now spans more than 30 years.

After some early SSSI committee service, I was selected Editor of the SSSI journal *Symbolic Interaction*, for 1983–1986. I felt honored by this selection. I had met many of the Midwestern progenitors of SSSI, and I knew many of them interpreted my selection as a risky one. I began with an intention to integrate symbolic interaction with some of the empirical studies of everyday life influenced by existential phenomenology, but a review of the articles published during those years shows little evidence of such integration. What I discovered is that most of our heroic efforts were expended to provide quality editing and intellectual feedback for papers that were eventually rejected, authored by an amorphous community of interactionists stranded at small universities, trying to survive without kindred colleagues or a supportive network. I took this position very seriously, and worked very hard. I viewed this as a sacred stewardship, connecting our honored pragmatist ancestors with our future generations of students. While my name was atop the masthead, like many of my professional achievements this one was a true team performance involving scores of interactionists, but especially the Associate Editors David Altheide, Gary Alan Fine, and Jay Gubrium. In retrospect, my work habits and orientation played a role in the end of my marriage.
MIDLIFE CRISES AND TURNING POINTS

My marriage to Kathleen Ferraro ended in 1985. The triggering event was Kathleen falling in love with one of my Justice Studies colleagues, not knowing that he was having an affair with her best friend at the time. Her friendship ended very quickly, but my friendship with the man continues more than a quarter century later.

The breakup was traumatic for me, and the divorce has been a difficult one. The triggering event is not the cause, as I eventually learned from Dianne Vaughn’s (1986) elegant interactionist study of divorce. This traumatic crisis began my career as a single mother, then later as a single father. My daughter Kailey was only 5 at the time, and my son Kyle only 2. I threw myself into a wide range of youth activities, with much the same enthusiasm that I had shown in my earlier organizational and political involvements. At one point I was so involved in my leadership positions in the Girl Scouts of America that I considered leaving my academic position and going to work for the GSA on a full-time basis.

During this period I also experienced my own death. When this occurred, I was unaware that any other person had ever had such an experience. Fortunately, we happened to have at ASU one of the leading thanatologists in the world, Professor Robert Kastenbaum, and he directed me to the literature which afforded me some way to make sense of this troubling and problematic experience. Over several years the consequence of this experience was to eliminate any anxiety about my own physical death.

In this context, my life took a spiritual turn, and in 1987 I took refuge vows as a Buddhist. This path has taken several turns, as the Buddha teaches there are different paths to the top of the mountain. I have made many Buddhist pilgrimages, and about 15 of the 65 countries I have visited have been in Asia. I tried several practices, before eventually finding a compatible sangha (Buddhist community) at the White Cloud zendo. I also became very active at the 300-member Valley Unitarian Universalist Church in Arizona, serving as Board President and in many other positions.

Profound turning points in life may begin with one traumatic crisis, or they may develop incrementally, but discerning their meanings and consequences often takes years. Interactionist scholars and researchers have been at the vanguard in mapping out these complicated social and personal processes (Athens, 1994, 1995; Denzin, 1988, 1989a; Gubrium & Holstein, 2000; Holstein & Gubrium, 2000; Perinbanaygam, 2000; Weigert
& Gecas, 2003), elucidating the importance of how we talk to ourselves (Athens, 1994), how we begin to change our stories in talking to others (Denzin, 1988), the self-reflexive nature of these processes (Holstein & Gubrium, 2000; Perinbanaygam, 2000), and the critical role our significant others play (Gubrium & Holstein, 2000). While for me it is possible to identify an approximate beginning for some of my important life changes, it is also clear these processes continue for many decades. This is especially clear when thinking about our most important teachers, our children, and how they continue to educate us throughout the remainder of the life course.

**SYMBOLIC INTERACTION AND EXISTENTIAL SOCIOLOGY**

Jack Douglas sought to integrate certain themes from existentialism into the larger domain of the social sciences, and even though Jack and the students he influenced have published more than 90 books and over 700 articles and chapters, there is today little organized presence of an “Existential Sociology” in the United States. There are several reasons for this. First, Jack was very much against “schools of thought,” which he considered inimical to fundamental scholarly values and the commitment to seek truth. Second, from the earliest days those influenced by the existentialist ideas began joining forces with the kindred spirits within the ranks of symbolic interaction, a perspective which shares many basic ideas. Third, existential sociology and symbolic interaction are not best seen as general theories, but rather as perspectives that orient or sensitize observers what to look for in everyday life.

Central concepts from the tradition of symbolic interaction include self, act, situation, time, meaning, social interaction, gestures, emergence, situatedness, symbol, action, identity, language, process, interpretation, and many others. Central ideas that are found in many existential and phenomenological works include situation, object, emotions/feelings, meaning, interpretation, individual responsibility, irrationality, absurdity/absence of meaning, being, reflexivity, freedom, body, subjectivity, consciousness, intentionality, being/becoming, contextuality, lived experience, perception, empathic understand, and many others. These ideas and concepts have proven very useful for observers or researchers who wish to understand some human phenomena, to begin with concrete observations or interviews in everyday life, and to continually compare these to further observations.
or interviews, to work toward the development of truthful concepts or
generalizations, perhaps even theories of the phenomena investigated.

Feelings and emotions have always been a major preoccupation for
existentialists, and Jean Paul Sartre’s 1948 book *Emotions* underscores this
primordial concern. Existentialists generally feel that social science and its
conceptions are overly rational and deterministic (as shown by the role
played in failing to anticipate the current economic collapse), and in general
they insist on the primacy of emotions, whether along with rationality or
overwhelming rationality. The primacy of emotions is surely the major
theme of many of the essays collected in the 1977 volume *Existential Socio-
logy*, but today it is almost embarrassing to go back and re-read these
essays, because the main points they make have been so accepted by most
social scientists, including most symbolic interactionists. Studying feelings
and emotions became increasingly popular in the 1980s and 1990s, as shown
by the establishment of a section within the ASA, largely due to the enter-
prise of interactionists Candace Clark and Carolyn Ellis. Norman Denzin’s
standings of emotion have been profound.

The study of deviance has been one of the primary fields for interactionists of
all kinds. Eschewing official statistics and other forms of bureaucratically
generated information, interactionists venture out into the field to “get the seat
of their pants dirty,” to use Robert Park’s elegant phrase. After 50 years, the
results of these empirical researches by hundreds and hundreds of scholars are
impressive. All of my close professional friends have contributed studies on
deviance, which perhaps says something about who I choose to have as friends;
included here are David Altheide’s (1978) study of employee theft, Patti Adler’s
study of financial markets, and their combined studies of preadolescent drug
(2004), and self-mutilators or “cutters” (2010). Jack Douglas and Paul
Rasmussen (1977) published a well-known study of nude beaches, Andrea
Fontana (1980, 1984) included deviance in nursing homes, Rasmussen studied
massage parlors (1984, 1989), Kotarba studied (1993, 2009a, 2009b) drug use in
rave scenes, and Norman Denzin published a trilogy on the depths of
and Joel Best (2000) have tried to incorporate all of these empirical studies into a
more comprehensive theory of deviance, and this is also the mission of the
annual research series edited by Gale Miller and James Holstein, now in its third
decade. These studies (and hundreds more not cited here) share certain basic
characteristics; they focus on the meanings and perspectives of the activities for
the participants, show how these are related to the immediate social situation, how the meanings emerge and change over time, how they are related to the formal agents and institutions of social control, and the consequences for self, identity, and others. I feel proud to have been an insignificant handmaiden to the development of this larger intellectual enterprise.

Lonnie Athens’ dedicated and heroic scholarship deserves a special mention, because he has made major contributions to criminology with his studies of violence (1974, 1977, 1986, 1992, 1997), later culled for their theoretical nuances of the more general violentization processes (2003) and the implications for symbolic interaction theory (1994, 1995, 2002, 2007). Lonnie’s research received singular recognition when Pulitzer Prize winning author Richard Rhodes published a major trade book in 1999, Why They Kill: The Discoveries of a Maverick Sociologist, reviewed in over 70 media outlets, touting his research and scholarly achievements. In my view, based upon all of my experience in the field of domestic violence, one of the major contributions of Athens is to elucidate the elusively embedded early childhood experiences to later social and psychological development, even including violent behaviors and responses. His research embodies symbolic interaction at its best, when detailed empirical observations in concrete situations are linked to many others, and these to larger contexts and social processes, culminating in new conceptualizations and theoretical developments.

Mass media of all kinds are increasingly important in our world, and interactionist ideas have proven very relevant and productive in our new understandings. David Altheide’s productive career has documented and analyzed the significance of news sources (Altheide, 1976), the media logic of their formatting and representational practices (Altheide & Snow, 1978), how the emerging electronic forms alter former patterns of journalism (Altheide, 1985; Altheide & Snow, 1991), and how cultural formats of social control are used and modified in the contemporary media environment (Altheide, 1995). He has proposed new ways to study these phenomena (Altheide, 1996, 2003), and more recently he has used these innovative methods to study the creation and institutionalization of fear in the wake of the 9/11 terrorist bombings (Altheide, 2002, 2006, 2009). His research has significantly advanced our understandings of new forms of propaganda and information control in our current fascist milieu. Moreover, symbolic interactionist ideas have proven especially productive in contemporary studies of new mass media, cinema, cyberspace, and SIS systems. Having David by my side as a friend and colleague for over 40 years has been one of the most meaningful aspects of my scholarly path and development; we are as dissimilar as night and day, but we have gained strength, sustenance, and love in our differences.
Symbolic interaction has affinities with many intellectual traditions (see Joas, 1985; Plummer, 1996; Reynolds & Herman-Kinney, 2003; Rock, 1979), and according to Bob Prus these include our ancient Athenian progenitors (Prus, 1996, 2003). The ideas of existentialism and symbolic interaction have proven useful for many empirical investigations, arguably because both traditions see the self as an active and creative agent of human life. Both see the self as reflexive, as having the capacity to see itself as both subject and object; social selves make decisions, cooperate with others, try to manipulate and control others, destroy, create, make love, make war, trust, deceive, make commitments, avoid commitments, accept their responsibilities, deny or avoid their responsibilities, and so on, and so on. Reflexivity grounds self-freedom (or agency) and self-motives. This gives an evolutionary advantage over other species, but also brings the possibility of self-objectification and even self-destruction, whether through suicide or other less direct means (see Reynolds, 1999). Our biological and social conditions come together in immediate, concrete situations (see Athens, 1992, 2002), and how individuals and groups see and interpret these situations will play a decisive role in how they act. The old critique of symbolic interaction is that it focused on the daily minutia of life, and ignored larger political and institutional arrangements, but today this criticism is calumnious because power, dominance, and control have been integrated into the perspective (see the 10 chapters on institutions in Reynolds & Herman-Kinney, 2003; also see Athens, 2002). The openness of existentialism and symbolic interaction makes them (interpretively) radical, inviting the reconstruction and transformation of self and society, inviting new possibilities (and new sources of disaster).

**SYMBOLIC INTERACTION AND QUALITATIVE RESEARCH**

I feel the best way to build valid knowledge and theory is to begin with one’s cultural experience of everyday life, and then by using the more formal methods of interviewing and observation, work inductively to create valid concepts and generalizations. At the dissertations stage many of us did ethnographies or field research, akin to early anthropology or the Chicago School in sociology. Buford Junker’s 1960 text was arguably the first to codify these methods for sociologists, but much has happened in the last five decades, and there are now over 100 monographs and texts, scores of them.
in the Sage Publications series alone. Today there are many different kinds and types of qualitative methods, and their use has spread to many other disciplines, across the globe. In recent years, largely animated by Norman Denzin’s leadership and enterprise, there are over 1,000 who attend and participate in the Annual World Congresses of Qualitative Methodology at the University of Illinois. The program of Congress VI was 135 pages long, and included papers by scholars from 30 countries. One could easily argue that qualitative methods have been so successful that they have been colonized, in that other countries (especially Canada) and other disciplines (health sciences, nursing, communication, criminal justice, management) now have their own conferences on qualitative methods apart from the Congresses. Our Canadian brothers and sisters have sponsored “the Qualitatives” annually for over two decades now, involving hundreds of participants from all disciplines.

David Altheide and I began regularly teaching qualitative methods and mentoring doctoral students during the 1980s, and we also published a dozen papers on various issues related to “the postmodern turn.” These publications culminated in our contribution to the first *Handbook of Qualitative Methods*, where we sought to clarify what the concepts “validity” and “reliability” might mean for scholars who conducted qualitative or interpretive studies. For about one decade we participated in the SSSI “Cultural Studies” session at the annual meetings, along with the same set of participants, Andrea Fontana, Laurel Richardson, Joe Kotarba, and Patricia Clough, where we appeared to represent the curmudgeon wing of the interpretive party. Looking back, I think we felt some defensiveness about some of the new ideas, like “standpoint epistemology,” for example. But we always understood these conflicts as being “in house,” that is, among those who shared the interpretivist perspective.

When Altheide and I were asked to reprise our 1994 essay for the fourth edition of *Handbook of Qualitative Methods*, much had changed. The great growth in the interest and uses of qualitative methods had expanded to many other disciplines, and many other countries, where there was a burgeoning and robust literature on all important issues. But the field was also under attack, by various state agencies in England, United States, and New Zealand (see Denzin & Giardina, 2007, 2008; Lamont & White, 2009; Ragin, Nagel, & White, 2004), where state officials sought to standardized scientific procedures and acceptable scholarly performance in a manner consistent with their own bureaucratic ideologies and metrics. So the tone of our latter essay was entirely different from the first, and reflected our enthusiasm for the creative diversity and plurality of these new methodological developments.
Norman Denzin has shown remarkable enterprise in expanding the domain of qualitative methods, at first contributing seminal empirical researches about emotions (1984, 1985, 1987, 1988, 1991a), and original studies on how to use qualitative methods for studying cinema (1995, 2002), cultural studies (1991a, 2007, 2008), and race (2002, 2005, 2008). He has articulated new methodological approaches (1989a, 1989b, 1991b, 1997) and promoted the new arena of performance ethnography (2003, 2007, 2008), assisted significantly by Ellis (2004, 2009), Ellis and Bochner (1992), Bochner and Ellis (2002, 2006), Laurel Richardson (1997, 2007), Fontana (1998), Fontana and Frey (1994, 2000), Kotarba (2009a, 2009b, 2010, 2011), and many others. In addition to serving as the Editor (since 1977) for 35 annual volumes of *Studies in Symbolic Interaction*, arguably the main archive of interactionist studies for more than a generation, Denzin (2010) has recently challenged all social scientists to embrace the “qualitative mandate” to seek greater justice, human rights, and emancipation in our lives as we join with our students, colleagues, community members, and all others to seek greater justice in our world. Our researches and efforts always occur within a complicated matrix of social, economic, political, and cultural contexts, so it is important for us to accept political responsibility and make this explicit. Only in this manner can we build a critical, interpretive interactionism which is inclusive of everyone. This is not the time for methodological fetishism; we need to break our chains of bondage in order to forge creative solutions for our problematic future.

**EPILOGUE: NO REGRETS, NO FEAR**

Life is difficult. Sooner or later shit happens, perhaps many times, and you can choose to use it to fertilize the bountiful flowers in one’s life, or you can pack it behind the normal appearances and carry it with you to the grave. Growing old does not mean growing up, and hubris finds many compadres among academicians.

As I enter my eighth decade, I am working full-time, still teaching large undergraduate classes, doing research and writing, and mentoring PhD students. I focus on my health, and I work out every day because I plan to compete in the national racquetball championships in the over-70 division. I spend as much time with my family as possible, giving love and support to our grown children, and to my new (since 2005) wife, Susana Ibarra. I attend my advanced Spanish classes every day, and read widely outside the social sciences. I donate thousands of dollars to my chosen organizations and charities, and still participate in a wide range of organized actions, to
reverse the terrible political situation in the United States and the world. I regularly sit zazen at my local sangha. My goal is to perfect my Bodhisattva nature, which means that I seek to become the kind of person my dog Chico thinks I am. Not long ago I had a wonderful epiphany, namely, that in sitting zazen I was really doing no more and no less than my ancestors, those humble Quaker farmers sitting in silence at their central Indiana meeting houses. They believed that within each human heart there exists a divine spark of potential human creativity and love. That's what I believe.

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A partial list of John Johnson’s publications can be found on his web site at the School of Social Transformation, Arizona State University.


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Further Reading


AUTHOR BIOGRAPHY

MY LIFE AS A SOCIOLOGIST AND AN INTERACTIONIST

Joseph A. Kotarba

ABSTRACT

My intellectual journey as a sociologist and a symbolic interactionist began when I was a 13-year-old eighth-grader in Catholic School on the working-class, southwest side of Chicago. My eighth-grade nun pulled me aside after school one day and gently told me that, now that I should think about what to be when I grow up. She suggested I study to be “either a sociologist or a priest.” After some serious thought, I eliminated the option of becoming a priest – yet, the word sociologist was intriguing. I had no idea what it really meant, but it had a certain ring to it in 1960, when society was becoming a viable and visible orientation in terms of major events we were learning a little bit about from the good nuns and television – like civil rights, the cold war, and the space race. I took her advice and set out on a 50-year journey to become a sociologist. The map of the journey has been elusive, though, in that what it means to be a sociologist – especially an interactionist sociologist – has changed over the years as events in my life and the social world have evolved. This journey has had three segments: sociology as something to do; sociology as something to know; and sociology as something to be. The journey has been profound as well as fun because, as I continue to discover what it means to be an interactionist sociologist, I discover who I am.
As far back as I can remember, I’ve always wanted to be a sociologist. Well, I guess I may have had fleeting thoughts about being a fireman or a baseball player when I was a little kid, but I can remember the exact moment when being a sociologist became a serious option in my life. I was a 13-year-old eighth-grader at St. Turibius Catholic School on the working-class, southwest side of Chicago. I was doing well in school at a time and in a place when being good was at least as consequential to academic success as being smart – and I was a really good kid. So good, in fact, that my eighth-grade nun pulled me aside after school one day and said, “Joey (only nuns and my mother called me Joey back in those days), now that you will be moving up to high school, you need to think about what you should be when you grow up. I think you should be a sociologist or a priest.”

Sister Alicia was a formidable educator and, well, woman. My preadolescent peers and I did not take her advice or opinions lightly, since we figured her dictates probably had implications for our eternal salvation as well as our future careers. After some serious thought, I eliminated the option of becoming a priest and attending high school seminary. I was barely discovering who and what girls were and I was supposed to give them up so soon? The word sociologist was intriguing. I had no idea what it really meant, but it had a certain ring to it in 1960, when society was becoming a viable and visible orientation in terms of major events we were learning a little bit about from the good nuns and television – like civil rights, the cold war, the space race, and the increasing plausibility of having a Catholic elected president. Although I now think that she really meant to suggest I become a social worker – a professional good boy, if you will – I took her advice and set out on a 50-year journey to become a sociologist. The map of the journey has been elusive, though, in that what it means to be a sociologist – especially an interactionist sociologist – has changed over the years as events in my life and the social world have come and gone. The journey has been profound as well as fun because, as I continue to discover what it means to be a sociologist, I discover who I am.

As I tell students in my qualitative methods courses, I have a proclivity to organizing social phenomena into threes. (I would claim that this perception is a remnant of my Catholic upbringing, except that the trinity is a common archetype in secular adventures such as sociology, for example, in terms of Durkheim–Marx–Weber and consensus–conflict–symbolic interaction.) It helps me at least to make sense of my socio-biography to see how my career and self-identity as a sociologist have to date gone through three phases. The first phase was to experience sociology as something to do. The second phase was to experience sociology as something to know. The third and current phase is to actually experience sociology as something to be.
SOCIOLOGY AS SOMETHING TO DO

As soon as I left the constraining comfort of the good Felician Sisters, I entered a more liberating side of Catholicism: De La Salle High School, staffed by the Christian Brothers. The irony was that I now had to really work hard at being smart by wading through an honors program comprised of fun courses like calculus and Latin. Being good evolved into a more mature and purposeful program of doing good. The bridge between the two was that elusive thing Sister Alicia called sociology. I learned sociology formally in two ways at De La Salle. The first was the third-year course in French literature and culture. We read a textbook on French society, *La France actuelle*, by Camille Bauer (1962), that turned out to be a marvelous Exercise in comparative sociology. We learned that French society had its own distinctive minority groups, its own distinctive way of relating to its elderly, and of course its own distinctive way of being Catholic. The title page of the text displayed that marvelous quotation from Thomas Jefferson: “Tout homme a deux pays, le sien et puis la France.” (Every man has two countries, his own and France.)

We also had a formal course in sociology, which was quite unusual in secondary education in Chicago back then. The theme of that senior-level course was sociology as social activism. Our teacher, Mr. Robert Keeley, was a member of the somewhat radical Catholic Workers movement, inspired by Dorothy Day back in the depression era and WWII. We learned the moral value of pacifism that would inspire our thinking on the Viet Nam war several years later as we became draft eligible. Dorothy Day’s work was perhaps the earliest treatise I’ve read on the need for immigration reform, as she called for justice for all workers. We also learned the moral value of social activism through the writings of the Russian religious thinker and social philosopher, Nicholas Berdyaev, who emphasized that Christianity does not so much depend on the miraculous, but rather on the creative, even bold, activity of Christians in the world, in union with God’s grace. In one of his major works, *Freedom and Spirit*, Berdyaev (1935) wrote: “In every moral act, an act of love, compassion, sacrifice, begins the end of this world in which reign hatred, cruelty, and avarice. In every creative act begins the end of this world in which reign necessity, inertia, and limitation and arises a new world, the ‘other world . . .’.” As I think back to those adolescent days when we embarked upon uncharted intellectual journeys, I can now see how Berdyaev’s writings suggested a metaphysical view on life that served as the seed for my later propensity for existential thought and antithesis towards structuralism. On the meaning of history and progress, Berdyaev wrote: “A religion of progress based on this apotheosis of a future fortunate
generation is without compassion for either present or past; it addresses itself with infinite optimism to the future, with infinite pessimism to the past.” On freedom, he wrote:

(History’s) drama and tragedy are not only determined in the divine life itself, but also by the fact that they are based upon the mystery of freedom, which is not only a divine, but also a human revelation. Its drama and tragedy are not only determined in the divine life itself, but also by the fact that they are based upon the mystery of freedom, which is not only a divine, but also a human revelation – that longed for by God in the depths of the divine life. This freedom, which is absolutely irrational and inapprehensible to reason, offers a solution of the tragedy of world history, for it is the source and origin of movement, of process, of inner conflict and of inwardly experienced contradictions. An indissoluble tie exists therefore between freedom and the metaphysics of history.

Mr. Keeley did not let us be content to get good grades in sociology. He insisted we practice sociology in order to do good in the world. Of course, we engaged in simplistic yet good-hearted works such as assembling and personally delivering food baskets to black folks who we assumed were poor because they were black and lived in the massive Stalin-esque Stateway Gardens public housing projects on South State Street. Mr. Keeley also gave us a taste of true commitment to activism and social change by getting us invited to somewhat clandestine meetings of (then) strange yet intriguing groups such as Congress of Racial Equality (CORE); Student Non-Violent Coordinating Committee (SNCC); and Students for Democratic Society (SDS). This was 1963 and 1964, and we were getting a distinct sense that society was about to change. Sociology was in the streets, but also in lovely France. When I sat in the cafeteria with my best friends at De La Salle High School, Nolan Chambers and Rich Kalwa, and joked about our brief encounter with French existentialism in sociology class – “C’est absurd!” – little did I know my French connection would last.

SOCIOLOGY AS SOMETHING TO KNOW

In fall of 1965, I embarked upon my college career at St. Procopius College. “Proco,” as we affectionately referred to the institution, was a small liberal arts college run by the Benedictine monks and located in the (then) distant and idyllic suburb of Lisle, IL, with a student body of 350 young men – still no girls! We were engaged in a number of cool, required courses such as philosophy, metaphysics, and the Gregorian chant – a marvelous art form we were simply too young to appreciate as an ascetic experience. I majored in sociology and enrolled in the typical courses: theory, deviant behavior,
complex organizations, etc. What was clear to me was that sociology changed—it was now a discipline and course of study. We were physically, and to some extent affectively, removed from the social and political restlessness of the big city 40 miles east of us. I did well at St. Procopius, but decided to transfer to Illinois State University in Normal after my sophomore year. There, I could continue my major in sociology yet prepare for a career as a public school teacher. After a summer working as a teacher’s aid in an inner city special education school in Chicago, I realized that I enjoyed teaching a lot. I also strategically realized that a teaching credential in special education would provide me with a draft exemption from that awful war in Viet Nam. Oh, did I mention that there were girls at Illinois State? ... cool. Sociology was my major, but only a major—somewhat drowned out by the practical exigencies of everyday life in 1967.

When I graduated in 1969, my family’s modest political connections with the Democratic Party in Chicago—have you heard of Mayor Richard J. Daly?—resulted in my first teaching job at the McLaren High School. The student body was composed of EMH (i.e., educable mentally handicapped) kids, overwhelmingly black but not really very retarded in the commonsense meaning of the word. It seemed that, if you were a regular high school principal in Chicago, McLaren was a good place to “transfer” your academically marginal students who demonstrated behavioral problems. I had a great time working with these “bad” boys and girls. I taught social studies, which consisted very much of strategies for getting along with one’s friends and neighbors and showing respect for firemen and policemen. In point of fact, I spent a lot of time in the gym playing basketball with the boys. In the summer of 1971, I had a personal crisis that affected my life dramatically and brought me back to another version of sociology. While playing basketball one afternoon in the gym, I went up for a rebound and had my legs accidentally cut out from under me. I landed flat on my bottom and proceeded to experience severe back and leg pain. After several months of limping around, I submitted to back surgery to repair a ruptured disc. The surgery was no miracle cure for my back, but it solved my chronic problem with Selective Service: I was immediately ineligible for the draft for medical reasons. I had a big decision to make. I had a promising teaching career that would surely result in an administrative appointment. Yet, I was getting a bit bored with life—and winters—in Chicago and a bit chagrined over a failing relationship. Thus, I decided to do what many young people in my situation would do: I returned to school. An MA program in sociology sounded about right, and moving to warm and sunny Tempe to attend Arizona State University sounded even better.
When I arrived in Tempe in February 1972, I was assigned to serve as TA for undergraduate statistics. When I was an undergraduate student myself, I did well in my statistics course and also navigated successfully through micro- and macroeconomics, so kiddy stats was a gimme. Since the professor in charge of the course was fairly incompetent as a teacher, I virtually took over the course and felt true academic power: I knew how to teach and I knew how to be patient with students having trouble with the material. With my new TI calculator and my SPSS manual, I was on top of the world of regression.

After about a year of suffering from the sin of statistical pride, I decided I wanted to pursue a qualitative thesis related to my personal experience with and knowledge of chronic pain. The specific trigger was the emergence of acupuncture as the next “miracle” cure for chronic pain. A quick chat with my statistics professor diverted me to yet another sociological world, or shall I say universe. He told me bluntly that, if I really wanted to study something as bizarre as acupuncture, I should talk to the new guys in the department who do that kind of thing: David Altheide and John Johnson. I thus embarked on a thesis with two professors from whom I never took a course, yet who became two of the four most influential scholars in my career. They are also authors of two of the four most influential books I’ve read, the *Top Four Books*.

For a guy who was not far removed from carrying his slide rule in a belt holster as a mark of quantitative manhood, I felt that David and John were giving me some very unusual instructions. When I presented them with a proposal chock-full of hypotheses on health-seeking behavior, they urged me to put it down, go hang out at the physician’s office where acupuncture was practiced, talk to people, and see what was going on. They suggested I observe how the office was organized and how acupuncture was actually accomplished. Perhaps most importantly, they told me to talk to the dozens of people lined up outside the acupuncturist’s office to elicit their reasons for seeking acupuncture. David and John used words like “meaning” in describing the stuff of everyday social life. They told me to forget – at least for the moment – what the textbooks on medical sociology wrote about people who pursued alternative health care, that they are undersocialized if not deviant. Instead, they advised me to see them as people who are suffering and who are trying to make sense of their dilemma the best they can as a step in the path of hopefully finding relief.

David and John did not stop there. They invited me into the community of interactionist scholars by nominating my thesis for the Pacific Sociological Association student paper award – which I won – and encouraging me to submit my thesis for publication in the *Journal of Urban Life* – which
it was. They taught me to see social life through sensitizing concepts like self, identity, and situation. They showed me how sociology could be something meaningful and exciting to do. Their intellectual seduction continued as they pointed me in the direction of seeing sociology as something to be, and that direction was west to San Diego.

**SOCIOLOGY AS SOMETHING TO BE**

After I completed my MA in January 1975, my wife-to-be Polly and I were not sure at all what to do next. We weren’t ready to get married and we weren’t ready to continue on for more graduate school. John and David had a good idea, as they usually do. They suggested I drive out to San Diego and talk with their mentor, Jack Douglas, about working on a PhD at University of California, San Diego (UCSD). Before returning to Chicago to do some substitute teaching while trying to sort it all out, I did drive out to San Diego. I walked up to Jack’s door in La Jolla on a cloudy and misty afternoon, where his charming wife Beverly told me Jack was walking the beach as he always did in the late afternoon. If I wanted to talk to him now, I had to walk with him on the beach. I drove down to La Jolla Shores and caught up with this gentleman in a hoodie, sweats, and white tennis shoes – just as everyone described him to be. I was a bit scared, approaching a larger-than-life scholar and intellectual, as I was accidentally pretentious enough to be wearing sweats and white tennis myself. As I walked with Jack, he shocked me by saying that I might be best off returning to Chicago, and moving up the safe and predictable administrative career ladder in the public schools. His warning turned into a challenge for me, however, when he explained his advice by describing some of the dreadful conflict present in sociology at UCSD at that time, and noting just how difficult it was in general to forge a successful academic career. I took the bait and decided to apply for the graduate program at UCSD. For the first time, I could almost see myself as a sociologist, but I had about a year or so to stay busy before moving to San Diego in fall 1976.

That year was very productive, both personally and intellectually. Polly and I were married in the small town of Weston, MO in August of 1975. While house sitting for Polly’s aunt and uncle in Weston the summer before the wedding, I got a chance to read Jack Douglas’ (1971) seminal work, *American Social Order*. I recall the excitement I felt reading about the creative challenges the new sociologies posed to structuralist sociology. Jack synthesized ideas from Husserl, Merleau-Ponty, Sartre, Moore, Russell,
Wittgenstein, Poppers, and others into what he referred to as existential sociology. Jack saw *freedom* as the key feature of social order – a vision that for me began during my upbringing as a Catholic and that I continue to share with him to this day:

Society is only possible to the extent that its members *share* certain symbolic meanings, which they use in common to solve problems. Yet, man’s symbolic nature makes him free, for he can study reality and consciously choose from among alternative paths of action. The necessity of society creates the necessity of constraint, of shrewdness, of being like and acting with others; yet the necessity of meaning creates the necessity of freedom, of questioning and making choices ….

I saw myself in that intellectual world, and that world fit my life experiences and ideas, too. My summer in Weston provided context for that awareness. I interacted with midwestern farmers and small-town business people with a very pragmatic outlook on life. I attended Shriners’ lodge meetings, Christian Church Sunday services, tractor pulls, and Monday-through-Saturday 6:00 a.m. breakfast with men for whom the word “pragmatic” was a comprehensive strategy for relating to nature, politics, God, and their tightly knit community. Their everyday life circumstances gave meaning to Nicholas Berdyaev’s and Jack Douglas’ message that the best relationships with God and others result from the freedom to construct those relationship to fit the resources and needs of the immediate everyday life (social) world.

After we were wed, Polly and I moved to Chicago where I assumed an administrative position as a “team leader” at a new, open-classroom concept, public middle school. I had earlier applied for work with the system, expecting to be called in as a substitute on occasion, when I got a call in August 1975 from the principal of the new school. He hired me from the availability list because he felt that my MA in sociology and prior experience teaching special education would be useful tools for managing students in a school where 46 different dialects were spoken in a very diverse student body. I administered a staff of 8 teachers responsible for 160 sixth- and seventh-graders. The situation offered very tempting job and lifestyle security. The school was a state-of-the-art facility with all necessary resources, located in the gentrifying area on the north side of Chicago known as Uptown. Polly and I lived in an apartment in the pleasant community of Edgewater; we even had a view of Lake Michigan from our living room. Polly took the bus downtown to her job as a legal assistant at a very posh law firm, whereas I rode the subway two miles to my school. We could have even converted to being Cubs fans, we were so upwardly mobile.
For the most part, I really enjoyed my job as I gained the experience I would need to navigate up to the tempting world of higher administration in the robust world of Title I, urban education, if I so desired. I even taught sociology to a seventh-grade honors class by having them conduct a multi-media ethnography of the immediate community. (A highlight of the fieldwork was, of course, when we would stop in the local candy store on the way back to school.) Two events occurred later that school year that both altered my thinking on public education and pushed me more quickly back on the road to San Diego. The first involved my duties as head disciplinarian for my team. At the time, the Chicago Public Schools practiced corporal punishment. I could not bring myself to personally hitting a kid, but I was responsible for witnessing teachers who wanted to spank students. I had to ensure that excessive force was not being administered, and that an officially approved paddle was used. The paddle had to be constructed of smooth wood that could not have holes drilled through the part that hit the poor kid’s bottom. Such ventilation would cut down on wind resistance and thus increase impact and suffering. (I cannot believe I am writing this!) As I chronicled in an essay published in John Johnson and Jack Douglas’ (1978) edited collection on *Deviance at the Top*, seeing one tear fall down the cheek of the cutest little sixth-grade girl being spanked for some malfeasance I can’t even recall marked the beginning of the end of my public school career.

The second event occurred during the cold and icy month of January 1976. In order to escape winter for at least a moment and recall my intellectually invigorating respite in Arizona, I drove through the snow after school one day to the Loyola University bookstore near our apartment to pick up my special order copy of John Johnson’s (1975) recently published *Doing Field Research*. I vividly recall sitting near the massive fireplace in the student center at Loyola, overlooking the frozen lake just outside the windows, and dissolving into the second, super influential book in my career. John’s description of the fusion of feelings and rationality in fieldwork is exactly what it is like when engaging everyday life. Yet, what makes John’s treatise on field research truly special is his acknowledgment, no, celebration of the passion that engulfs the field research experience:

> What distinguishes those who complete a research project from those who do not is very likely a congeries of misadventitious events, circumstantial considerations, and other features of specific events . . . . But beyond any such trivial distinction lies the natural community of our self-interested curiosities, our passions for a more lucid self-understanding and understanding of others, of how and in what ways the twain meet, or don’t. These unite and divide us in everyday life. Very often they are what brought us in hope to the very first sociology class at the university. These passions and curiosities are
present whenever we entertain any thoughts of optimism or pessimism, joy or despair. Our feelings of membership in this natural community show themselves by our continued commitments to the institutions of talking and listening, reading and writing, questioning and answering, acting and reflecting.

After reading John’s book, I knew that I would only pursue becoming a sociologist of everyday life if I had the passion to do it. I think I did. I was pretty sure I was ready to go to San Diego to sculpt my identity as a sociologist, but for this stage of my education and training, I already had my girl with me.

**SOCIIOLOGY AS SOMEONE TO BE**

UCSD was a remarkable place to train to become a sociologist in fall 1976. The department was truly the epicenter of everyday life sociology. My professors included Jack Douglas for classical social theory; Joe Gusfield for deviance; Fred Davis for qualitative methods and the sociology of health; Bennett Berger for the sociology of youth; Aaron Cicourel for cognitive sociology; and Bud Mehan for ethnomethodology. With Beryl Bellman as instructor, we even organized an elective seminar to unpack, discuss, and enjoy Paul Ricoeur’s (1975) phenomenological masterpiece, *The Rule of Metaphor*. My funding for UCSD came from an NIAAA-funded predoc fellowship whose principal investigator was Tony Deutsch, a marvelous neuroscience researcher from the department of psychology who seemed to enjoy my adventurous accounts of street ethnography. Since the grant was intended to support alcohol-related research, my main obligation was to include at least one chapter in my dissertation on an alcohol-related topic. Early on, I decided to expand my earlier work on acupuncture into a comprehensive study of the chronic pain experience. A natural component of the study was an investigation of the strategies used by working-class men and women to manage pain problems related to working hard for a living. These strategies were designed, refined, communicated, and actualized in the working-class neighborhood tavern, with a shot-and-a-beer as a typical analgesic. Voila! I had my chapter.

Perhaps the most memorable experience of graduate school at UCSD was reading. After years of picking through textbooks and class notes as an undergraduate and masters student, I was expected to – and in fact was liberated to – read a wide-range of original, multidisciplinary intellectually breathtaking books. It might sound trite, but I relearned the pleasures of reading. I revisited the excitement I felt when, during my freshman year in
high school, I was hanging out at the neighborhood public library. Getting bored with my prescribed history assignment, I surfed the stacks and pulled out a book with an intriguing title, *A Neighborhood Finds Itself*. The author, Julia Abrahamson (1959), recounts her experience with the early, 1950s grass-root efforts in the University of Chicago/Hyde Park area to revitalize the community. That book made living in Chicago sociologically exciting. I suddenly saw Chicago as a laboratory, a case study of the most fascinating of objects: Chicagoans. Different reading environments sometimes call for different books to read. Living in idyllic La Jolla fit well with the philosophically oriented books informing the still nascent everyday life sociology movement centered at UCSD in the late 1970s. I will forever enjoy the picture postcard, summer Sundays Polly and I spent on the La Jolla Shores beach. The waves were sparkling yet loyal in their constant return to the sand. What better environment to delve into Edmund Husserl’s (1962) *Ideas: General Introduction to Pure Phenomenology* – kind of gave the word *transcendental* a whole new meaning.

Among my most memorable readings, however, were the final two books in my “Top Four Books” list. David Altheide, who along with John Johnson seduced – corrupted? – me into everyday life sociology, wrote perhaps the most significant epistemological work I’ve encountered. David’s seminal *Creating Reality* (1976) vividly describes the process of surrendering to a social setting, in this case the TV news room, in order to see how TV news is less a mirror of some social reality than the product of hard interactional, organizational, and technological work. The book reads like David’s constant advice to me over the years: study social things that are exciting as well as important, be passionate about your work, and be faithful to the social reality as experienced by your respondents. David’s dynamic approach to research informs his pragmatic position on the inevitability and value of mistakes and surprises in the field:

Unlike, most research manuals, which caution researchers against making mistakes, I find that mistakes are inevitable. Not to make mistakes implies that the researcher already understands the meanings, perspectives, and problems of those he studies, and since these are what field researchers seek to uncover, it is likely that they will make a variety of errors during the research journey to the world their subjects inhabit … recognizing that mistakes have been made is a test of how much the researcher is learning.

David’s wisdom helped me navigate through a potentially touchy situation during my dissertation research in 1978. I spent the better part of the summer in the locker room and dugout of the San Diego Padres, listening to stories about injuries and pain, and how baseball players and
trainers deal with them. I was hanging around pregame practice one day, listening to everyday baseball talk while trying to chew four pieces of Double Bubble gum at one time – going native? – when I casually referred to Roger Craig as coach. He immediately turned to me and sternly reminded me that: “Son, I’m not a coach, I’m the manager.” I wanted to disappear because my first thought was that my privileged access to the Padres and my dissertation were finished. Well, my faux pas turned out to be no big deal, but I learned two things from my mistake. First, do not take the status hierarchy for granted in a complex setting like a professional baseball locker room. Second, and perhaps most important, Roger’s admonishment was an indication that I was fitting in the scene, that I was someone to talk to directly. If Roger wanted to punish me or get rid of me for my mistake, baseball culture would have instructed him to simply ignore me, freeze me out of conversations, and tell the Director of Media Relations to cancel my credential. When my work that summer was completed, all the players including Bill Almon, Dave Kingman, Ozzie Smith, Rollie Fingers, and Dave Winfield – and all the coaches and Manager Craig – signed a baseball for me: a memento I still cherish to this day.

The fourth book on my list was written by my good friend Andy Fontana, who studied at UCSD several years before my arrival. In his *The Last Frontier* (1977), Andy describes in very elegant prose the ways people grow old. The particular setting he examines was one of the burgeoning retirement communities in the southwest in the early 1970s, but the main lesson his book taught me, both by example and by dictate, was the value of rich and lucid writing to the interactionist project:

> But if sociologists hope, and some of us do, in the wake of the great Max Weber, to demystify the world and present its social members with analytic insights about the invisible webs that hold it together, it is not enough to simply see the world stripped of its veils. Sociologists must strip their own language of the veils which hide the revelations from the members of society and present their sociological understanding with a new kind of sociological imagination, one that will guide the sociologist’s pen to paint a portrait of society which will stir empathy and understanding among readers.

I have stayed in contact with the authors of my “Top Four Books.” We have worked on numerous projects together, including *Introduction to the Sociologies of Everyday Life*, *Existential Sociology*, *The Existential Self in Society*, and *Postmodern Existential Sociology*. Their lasting impact on my life and my work, however, emanates from the fact that they have become four of my very best friends. They were there to help me make sense of the sometimes treacherous but always annoying conflicts in the sociology departments of which I’ve been a member. They were there to remind me
that there is integrity and love in our vocation, when careerists I have encountered at UCSD and other places have unscrupulously attempted to commodify my research and ideas and rip them off. I feel sorry for my peers who have not been as fortunate as me to be shown a better path.

Life at UCSD was rich in many ways. I took a half-year off from my NIAAA predoc to work with Joe Gusfield and one of the most creative ethnographers I’ve known, Paul Rasmussen, on an NSF-funded study of drinking-driving behavior. I spent numerous hours talking theoretical trash with the two closest grad school peers in my cohort – John Hund and Armando Arias – as we naively attempted to arrive at the definitive understanding of mysteries such as reflexivity and social fact. I’ll never forget my last summer in La Jolla, in 1979 when I wrote my dissertation. My typical workday was as follows: I would get up early, say at 7:00 a.m., and write until noon. I would then hitchhike down La Jolla Shores Drive to Mission Bay where I was enrolled in a for-credit, Red Cross certification “course” in sailing. I recall floating around in my little sabot, asking myself rhetorically if life could be any better. I would then hitchhike back to the YMCA in La Jolla to attend and observe a class in Siddha Yoga meditation that was populated largely by people seeking relief from back pain and suffering. I would then walk through the tall grass fields to Jack Douglas’ house on La Jolla Scenic Drive, where I sat in Jack’s office, talked a little and listened a lot. I then walked home to our student apartment – with Pacific Ocean views from two rooms – had dinner with Polly, wrote again until 11:00 p.m., and capped the day with a Honeymooners rerun on TV.

Given that majestic scenario, Polly and I did the unthinkable. After only three years in graduate school, we traded our paradise home for a university appointment. Polly was ready to start a family, and I felt that I might as well get paid to conduct the research and teaching for which I was so well prepared. My arrival at the University of Houston (UH) as an assistant professor, however, occasioned a review of my personal and professional identity. My new colleagues thought of me variously as a symbolic interactionist, a field researcher, a medical sociologist, or one of those new-fangled ethnographers from Southern California. In the end, it made much less difference to them who I was than what I could do. They hired me to create a research and publishing agenda in medical sociology, and that I did. I completed my book and several well-placed articles on the chronic pain experience. I initiated and completed an ethnographic study of the delivery of health care services to astronauts and their families at NASA’s Johnson Space Center. I also planted the seeds for seeking a sociological understanding of the mysterious new disease apparently striking young gay men that we now know as HIV/AIDS.
The university awarded me early tenure and promotion and, thus, the opportunity to develop my identity as, you got it, a symbolic interactionist. Over the course of my career, I’ve come to appreciate symbolic interaction (SI) for what it really is and what it can do for its adherents. I think of SI as a base of scholarly operations, a liberating intellectual home. Like thoughtful liberal parents, SI trusts the progeny it has so carefully nurtured to go out into the (social) world to discover that world – knowing, of course, that they will – no, want to – come home for Christmas. In my case, I did not have to abandon my scholarly agenda in health and illness studies to investigate the social aspects of music. Instead, I do both since all substantive areas can inform the interactionist project by highlighting the principles of the magical ways people work very hard together to assemble social life. Examining both the organizational processes by which translational biomedical research is conducted at the University of Texas Medical Branch in Galveston and the musical activities of aging baby boomer rock and roll fans, my two current major projects, makes great sense to me.

I cannot overstate the magic and warmth of the community of interactionists known formally as the Society for the Study of Symbolic Interaction (SSSI). My colleagues at the UH were always aware of and perhaps a bit envious of a reference group in academia that was friendly, supportive, intellectually challenging and, well, fun to be part of. I can recall the numerous poker games we would have during the Couch–Stone meetings or the annual SSSI meetings. Picture this typical scenario and all-star line-up: Andy Fontana hosting the game in his hotel room and providing the deck of playing cards from a Las Vegas casino; Carl Couch complaining in his inimitably grouchy way that the cards are probably fixed; Norm Denzin insisting we play some new game called “Texas Hold ‘em”; Mike Katovich and Stan Saxon swapping Greg Stone yarns; and Carolyn Ellis insisting that we desegregate and let her play, whereupon she ends up winning all the nickels and dimes.

The past three or four years of my life have brought my career as a sociologist and an interactionist full circle. My colleagues at the UH elected me chair in 2008. They no longer saw me as one of those new-fangled ethnomethodologists from Southern California they hired 30 years ago. They now saw me as a senior scholar whose take on sociology was of value to the department both intellectually and in terms of the applicability of interactionist thinking to practical research and policy formation. In 2009, I received the George Herbert Mead Award for lifetime service to SI, in all honesty, for doing what I simply love to do. In 2010, I received the Mentor’s
award from the SSSI, in all honesty, for simply passing along the intellectual riches my mentors were kind enough to afford me along the way. Among interactionists, these awards do not mark either completion of or retirement from our project. SI is literally a lifestyle. As is commonly the case among interactionists, our families and our personal lives become holistic with our work. Our three children – Chris, Jessie, and Andrew – have always been amazed that daddy’s job was cool because he wrote about popular music, until they read with caution my accounts of the place rock ‘n’ roll culture has in our family dynamic.

The spark of intellectual and historical energy and curiosity that has sustained my work over time has also come full circle. Several years ago, my wife and I had an opportunity to spend a weekend in Paris on our way to Krakow, Poland to lecture. This was our third trip to Paris, but special because we set out to discover the sidewalk café where Jean-Paul Sartre argued about absurdity with Simone de Beauvoir and Albert Camus. We found and had coffee at the Café de Flore, whose tourist-oriented menu claimed the place to be the birthplace of existentialism. I felt great: the American pragmatist in the spiritual presence of a French intellectual giant who wrote about freedom. For a moment, I was Sartre’s Mathieu.

In 2010, I was fortunate enough to continue my intellectual development and workstyle in the pristine hills and streams of Central Texas, when I was hired to establish a Center for Social Inquiry in the growing department of sociology at Texas State University (TxSt) in San Marcos. In an ironic way, my interpretive, qualitative, and individualistic identities as a symbolic interactionist prepared me well for this formal, organizational, and managerial position. Interactionism is very much a holistic enterprise that melds theory, methods, policy, application, and communication into a unified and pragmatic framework for seeing the social world. This is, I believe, exactly the orientation needed to create a center that will be scholarly as well as applied, intellectual as well as sensitive to the everyday life world, a research home that will see research methods as tools for discovery not weapons for achievement.

During the inaugural year of the Center, we engaged in several activities that demonstrated well the value of integrating symbolic interactionism into a comprehensive research enterprise. Our first event was a symposium on “Sociology, the Environment, and Sustainability: The Humanistic and Scientific Relevance of Water” (April 13, 2011). Water is of great relevance to Central Texas, where our rivers and streams provide a critical resource for recreation and commerce yet where periodic drought is common. TxSt scholars in geography, engineering, environmental studies, and land
management approach water policy issues with scientific and economic tools and assumptions. We organized our symposium to address these technologically oriented concerns, but we made a strong point that sociology in general – and SI in particular – allow us to focus on the mundane, everyday life aspects of water as a fundamental feature of our lives and our culture. I presented a multimedia discussion of the way water acts as a fundamental trope for defining who we are as residents of the Central Texas hill country. For example, I talked about the ways water creates general moods and images, as accomplished by the name of a quaint, laid-back small town near Austin, TX called “Dripping Springs.” Water can serve as a trope for evil, as illustrated by photos of groves of malevolent cedar trees, along the valleys and hills, that drink massive amounts of water from the otherwise fragile limestone aquifers that underlie the area. Water can serve as a symbol of upper middle-class status for those with the resources to install rain water collection systems on their homes. Our successful symposium propelled our department’s efforts to pursue and engage in a marvelous array of multitheoretical and multimethodological projects including self-identity work among female chefs, the effectiveness of NIH-funded translational biomedical research, and the influence of Mexican-American Tejano music star, Selena Quintanilla-Perez, on the evolution of Latino/a music and culture. In the midst of all the excitement and activity surrounding the institution of the Center for Social Inquiry, I was able to complete and publish my book on baby boomer rock ‘n’ roll fans (Kotarba, 2012). This book marks the fruition of my many years of research and writing in the sociology of popular music – attending all those concerts was tough work, but someone had to do it …

As I look back over my career as a sociologist and a symbolic interactionist, I can see just how important interaction has become for sociology, beyond the contributions of its theoretical riches. Interactionism’s pragmatic orientation is a powerful tool for managing and administering the contemporary university – from the department up through the research center and beyond! – whereas its love for the everyday life social world continues to infuse a sense of excitement into the sociological project. When my students at TxSt ask me about SI and whether they should become interactionists, I tell them the same thing I told my students and mentees over 30 years at the UH when I was asked the same question: If you have a passion for understanding other people’s everyday lives and dreams, and you have a passion for hanging around and even experiencing to some degree their social worlds, consider studying interactionism. Otherwise, count ‘em and crunch ‘em.
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AUTHOR BIOGRAPHY

Joseph A. Kotarba, Ph.D., is professor of sociology and director of the Center for Social Inquiry at Texas State University – San Marcos. He received his doctorate from the University of California at San Diego. His scholarly focus is on the sociology of everyday life, and he works primarily in the areas of health, culture, education, and existential social theory. His current projects include a study of the culture of translational scientific research, funded by the National Institutes of Health, and a study of the delivery of emergency medical care to musicians and their fans. He is the author or editor of 7 books, and approximately 90 articles and book chapters. He is the 2009 recipient of the Society for the Study of Symbolic Interaction’s George Herbert Mead Award for Lifetime Achievement. He is also the 2010 recipient of the Society for the Study of Symbolic Interaction’s Mentor’s Excellence Award. His forthcoming book is Baby Boomer Rock ‘n’ Roll Fans (see http://www.soci.txstate.edu/csi/).
MY STORY AND I’M STICKING TO IT – UNTIL I REVISE IT

Laurel Richardson

ABSTRACT

Laurel Richardson’s academic autobiography from preschool to Professor Emerita.

I could not have been more than four when it happened. We were playing hide-and-seek, and I had found the perfect place to hide my skinny little body. I waited. No one found me. I waited longer. Maybe they weren’t looking for me. Maybe they didn’t even want to find me! I panicked: Who would I be if I weren’t found? Hiding, and not found, in the bottom drawer of my father’s dresser, I had latched onto a kind of natural symbolic interactionism, and I have never let it go.

Sometimes, I think that my life’s course in academia has been over-determined. That I would have thought at age four that maybe no one wanted to find me was not a fantasy. No, it arose from the microdynamics of my family life, and their embeddedness in larger cultural and historical realities of prejudice, androcentrism, and power imbalances, although I would not have these words to describe my childhood until much later.
My father’s English–Irish family came to America before the Revolutionary War. His namesake Lord Tyrrell, having set a bomb off in the Tower, needed a fast getaway. My mother’s Russian Jewish family came in 1908 from a shtetl near Kiev. She was eight years old, a survivor of the unspeakable horrors of the pogrom. Her family needed a fast getaway, too. Father met Mother’s brother Mike in Chicago’s Bughouse Square, a site of intellectual bombasity in the 1920s. Mike brought Tyrrell home; Tyrrell and Rose, my mother, got engaged; and Rose’s mother had a “nervous breakdown.” To appease her, Father promised to raise the children Jewish, a fate surely leading to suffering, my Mother thought, insisting that the children also be reared as Christians. We went to Anshe Emet Synagogue during the school year, where I was confirmed, and to Presbyterian “family” camp, during the summer. At the synagogue, I was singled out as an example of “not looking Jewish”; at family camp, I was singled out for refusing to pray to Jesus, to sing his name.

The “Jewish–Gentile” issue was intensified because I grew up during World War II. At the synagogue, I learned that little Jewish girls’ skins were turned into lampshades by Nazis, and that no Gentiles were to be trusted. In my back alley, I learned from Gentile children that I had “killed Christ” and that was reason enough for the Germans to “come here and kill all the Jews.”

At the start of the school year, I would count the number of Jews and Gentiles in my classroom. Every year there were equal numbers, and with me “50–50,” the balance was maintained. In a misplaced megalomania, I thought it was my task in life to keep the demographic balance. I must have thought that if the external balance could be maintained, then my internal balance would be maintained too, and I would survive the onslaughts to my identity by both Jews and non-Jews.

Although World War II focused me on religious differences, I had a gut understanding that I came from two different cultural worlds in which the religious differences were embedded. My mother’s family spoke little English, ate kosher food, socialized only with family members; they did not drive, vacation, go to movies, or eat out. My father’s associates drank Scotch, had box seats at the Cub stadium, ate at Berghoff’s, went to stage plays, ran for political offices, and kept “mistresses.” My father was an anti-racist American Civil Liberties Union (ACLU) Republican and a criminal attorney. My mother had an eighth grade education. When I asked my father why he had married my mother, he said, “I’m always for the underdog.” Because father’s law and political career was premised on helping the “underdog,” only as I write this do I realize that father was talking ironically. Mother ran the house. As Karl Mannheim would say, if he had known me, “Why Laurel, you have a foot in two different worlds. You are positioned to be a sociologist.”
“Well, Karl,” I would answer, “You haven’t even heard about the micro-dynamics of my family life. And, I am not sure you would ‘get’ it. You’d think of it as natural – the special place the son has in the heart of a Jewish mother. You’d never consider the consequences of that upon the daughters.” Karl had no feminist consciousness.

The firstborn was my sister whose very gender disappointed my parents. She was also later than average in walking and talking. Whenever there was any illness in our house, my sister went away to live with one of my aunts. I don’t know why. She attended 15 different elementary schools. Seven years later my brother was born, then two and one-half years later, me.

Mother made pies for my brother every day, because he liked them. She looked the other way when he threw a hammer at me, pushed me down sewer grates, terrified me with his Boris Karloff imitations, bilked me out of my allowance, sent me up on rooftops for the entertainment of his friends, bound me up, hypnotized me, and accused me of doing the destructive things he had actually done. When I complained to my attorney father, he said, “Life is not fair.” This seemed like a very “unfair” answer, as it was me who was being “victimized” by my brother; he, never by me.

That is, until, I found I could “best” him in anything academic – spelling tests, reading, and math. So, I set upon an academic life course before I graduated from kindergarten. With my academic ability, why I could right so many wrongs – so many miscarries of justice!

When I was 13, I joined my brother, Barrie, working part-time in the notions department of Goldblatt’s Department Store on Lincoln Avenue. Father thought work would be “good” for us, that we should know about the work-lives of people, not be removed or arrogant. We were hired because Father doctored our birth certificates, and because he had created naturalization papers for the notions manager, Olga, a Russian Jewish refugee. Barrie was a stock boy; I was a sales clerk. He made $.39 an hour; I made $.33 an hour. However, because I was unwilling to report the old women who stole little things like spools of thread, needles, or packages of straight pins, Olga transferred me to the parakeet department. No one stole birds. I learned to “sex” the birds and earned commissions, more for male birds. They were prettier. When the parakeet department closed, I was transferred to the optical department. No one stole glasses. I sat in front of a wall of mirrors and behind a long low glass counter that held eyeglass frames. My tasks were to make appointments with the optometrist, measure the optical placement of the lens, determine the frame size, help the customer select a frame, write-up and process the order, and adjust frames whenever asked. I was effectively an optician. I was 14. My pay jumped to $.41 an hour. I liked feeling important,
independent, and in control of my little world between the mirrors and
the counter. For 15 hours a week, I felt grown-up, residing in a protected little
world. And I liked wearing lipstick and falsies. Being young and pretty gave
me the job but being capable let me keep it.

Barrie was two years ahead of me at Senn High School. He was a star
basketball player and all-around BMOC (big man on campus). I rode his
coattails my freshman year into student government, the school paper’s
gossip column, and Alpha, the only social club that accepted both Jewish
and non-Jewish members. I wrote his English papers; sometimes he fixed me
up with his basketball buddies for dances. At one Friday night “dateless”
social, I was crowned Queen, which gave me the right to chose my dance
partner, a right normally reserved for the boys. I chose to jitterbug with an
African-American boy. He tossed me out and in, spun me over his head and
slid me between his legs. Students encircled us, clapping and shimmying. I
felt like a movie star. Miss Baxter, the chaperone, stopped the dancing,
publically excoriated me for my choice of partner, and expelled us both from
the social. I wore my crown home on the streetcar. I never saw the boy
again.

Barrie had two grade cards. He gave himself good grades in the one he
brought home for Mother to sign; on the real one he forged Mother’s
signature. His basketball prowess got him into Carleton College. I worked the
academic system a different way. I wanted to be valedictorian; to guarantee
that I planned to enroll only in those classes where I felt confident I would get
an “A.” Latin was one of those, and I fell totally head over heels in love with
the language.

“I want to transfer to Girls’ Latin High School,” I told my father. I was
no longer interested in being valedictorian at Senn.

“No,” he said. “I believe in public education. I won’t pay for private
school.”

“If I get a scholarship, can I go?” I asked.

“Well … yes,” he said, a little too quickly.

“Promise?” I asked. My parents never broke promises.

Father smiled and nodded.

Immediately, I phoned Girls’ Latin, and explained my situation.

“Sorry,” the registrar said. “Our scholarships are only for those without
financial resources.”

Father smiled.

“Here, look at this newspaper article,” I said to my parents. “The Ford
Foundation is offering scholarships for ‘Early Entrants’ at the University of
Chicago … May I take the qualifying test?”
Father nodded.

“And if I pass and get a scholarship, can I go?”

In 1952, at the height of the “cold war” and McCarthyism, my “conservative” father allowed me to matriculate at the “commie-red” University of Chicago. I was 15. I had qualified for a Ford scholarship. I moved into Green Dormitory on campus. For the first time in my life, I felt physically and emotionally safe. I felt normal. I felt average. I loved the University of Chicago, and I have never recovered.

Most of my friends were from religiously mixed marriages, although this commonality was not a focus of our conversations. Books, theater, politics, and philosophical ideas were. On the placement tests, I placed out of two courses – Latin and mathematics. As there was no advanced undergraduate Latin course nor any other language I was interested in studying, I took no foreign language. My career plans to be a Latin teacher were demolished.

There was, though, an advanced mathematics course; it was wonderfully abstract and I came to understand the beauty of mathematics.

As a student in David Riesman’s Social Science II class, I learned to put sociological understandings to political use. I put my body where my mouth was. I chained myself to Frank Lloyd Wright’s Robie House, protesting plans for a wrecking ball. I sat in at Walgreen’s, a drugstore that refused sit-down service to “Negroes.” I canvassed realtors, “renting” apartments that claimed “no vacancy” when “Negroes” applied. Along with 25,000 other people, I heard Paul Robeson sing in Chicago’s Washington Park. My notions about “justice” were widening.

Having been a “child actress” at Goodman Theater in Chicago, I was drawn to University Theater. There, Viola Spolin’s ideas about improvisation of scenes and stories were implemented by our director, Roger Bowen. Many of us – to name-drop a bit, Severn Darden, Barbara Harris, Mike Nichols, Andrew Duncan, Alex Haselev, Elaine May, Ed Asner – went on to start Compass Players, an improvisational troupe that performed at Jimmy’s Tavern, and became the forerunner of “Second City.” What I learned from this experience were three things: (1) I wasn’t hungry for applause; (2) doing improvisational theater was not much different than doing “life”; and (3) to do theater or “life” well, one needed a sense of “how people think” tempered with a sense of the ironic.

Social action and Compass Players gave way to a new love – Chicago’s undergraduate capstone seminar, “The Organization, Methods, and Principles of the Sciences.” I learned – I should say, took to heart, whether that was or was not the intention of the seminar – that there was not one right method to make sense of the world, and that the tack one took affected
what one found. Although the seminar was about science, it seemed to me it was about everything I had ever experienced or thought about. Everyone sees from a particular position; nobody sees it all. How people think became my burning question. But, I really did not want to think about it just now.

In 1956, I left Chicago with two bachelor degrees and $500 – money earned from life guarding at Bruno Bettelheim’s Orthogenic School. I was 19, and burnt-out from proving myself. Go West, young woman, Go West!

My college roommate, Maggie Nash, and I did go west. We were hired as the summer theater staff for Camp Cibola in the San de Christo Mountains, north of Taos, and an easy jaunt down to D.H. Lawrence’s house, where his widow would sometimes welcome us, she being a friend of Maggie’s father, the U.S. director of Indian Affairs, and a friend of Sandy, a teacher at Georgetown Day School and the camp’s director. (Much, much later the camp was sold to Ram Dass as a retreat center.)

Our campers were teenagers of the very rich and the very famous and very powerful from Washington, DC. Their parents ran the government and its agencies, the banks, and the media. We all traveled out west together in rattley school buses. Everyone had but one duffel bag, lunched on peanut-butter and jelly sandwiches, and slept on the ground. It was my first time for all this, plus the first time I had been in close quarters with the Truly Entitled. By the time we had reached New Mexico, the teenagers stopped complaining and were as bristled and tattered as old cowhands. At Cibola, they stomped in cow manure and handily shaped adobe bricks for the swimming pool that they would never get to enjoy. They went on long hikes and perilous bus trips to experience for hours (and sometimes days) Native American ceremonies in respectful silence. They worked with Sandy on an archeological dig. They had camp chores, like laundry, dishes, and yard and kitchen duty, where Thomas and Seymour, a long-time interracial couple prepared meals.

For their theater experience, Maggie and I dramatized Thurber’s *The Thirteen Clocks*. It was the first time I had altered the genre of someone else’s writing, and the first time I had cast a play, directed the performers, designed costumes, chosen music and lighting – and all those “firsts” were shared with a good friend. Life could not get better: creativity as a value had replaced scholarship.

At the end of the summer, Sandy gave each staff member their salary plus a percentage bonus of the profits. How unexpected and nice that was! It meant I could take a bus, rather than depend upon my thumb or an ex-boyfriend to get to California (where *Everyone!* wanted to go). There I had a scholarship to the Actor’s Studio in San Francisco.
little income, I taught fourth grade in Decoto to Mexican children, who were often not in school, but in the fields picking crops. Most of them could neither speak nor read English. I translated (poorly, I am sure) some Dr. Seuss books into Spanish, and I asked the children to teach me Spanish. Their parents’ gratitude came in “care” packages of burritos and tacos. They were my teachers about the everyday consequences of poverty. My nemesis was a Castilian teacher from Spain. If the children violated any rule during recess, he stood them against the brick wall with their arms over their heads until the bell rang for classes. Rule 1, he said, was no talking in Spanish. I reported him; he reported me. My lecherous principal railed, “You are not dressed properly! You are not wearing nylon hose.” “How do you know?” I asked. “Why are you looking at my legs?” My days as a public school teacher were numbered.

The side-by-side experiences of mentoring the Truly Entitled at Camp Cibola and the Truly Impoverished at Decoto School when I was myself unmoored (no longer a student, a Midwesterner or economically secure) has had lasting consequences on my scholarly life, although I did not recognize the impact until I wrote this autobiography. Through those real-life experiences, I learned that people of all social classes can and do transcend their entitlements – or lack thereof. Unlike my Marxist and demographically inclined colleagues, social class did not become the determinant in my sociological world.

I had a boyfriend who was heading to the University of Colorado. I followed him. I enrolled in the psychology department so I could answer the question, “How do people think?” My psych advisor told me I’d be studying brain chemistry, physiology, and Skinnerian theory. I withdrew from the department before classes started, and through a bit of serendipity met Alex Garber, a Chicago graduate and professor in the sociology department. He convinced me that I could make myself at home there. And I did.

Not immediately, though. “You are not accepted, yet. You’ll have to prove yourself,” the chair told me because I was their first woman graduate student. This was 1958. These words rankled my competitive spirit, and I was off and running – well, reading. Apart from a first-semester directed study on the family (a specialty thought appropriate for me), where the professor gave me a list of eight books at our first meeting, and, me having read them by the second meeting (not knowing they were the semester’s reading list), and me being told by that professor, “Stop reading! Find a husband,” the department was a near perfect fit for me.

Most of the faculty and students were maverick, creative, and smart, each different from the other, none careerist. Each year the department hosted
a “United Nations Week,” where intellectuals, politicians, scientists, and artists of different persuasions shared the podium, modeling conversation across differences. Imagine Norman Thomas, Pierce Butler III, and James Truitt discussing “Should Cows Vote?” Or Harold Garfinkel, Buckminster Fuller, David Lodge, and Edward Teller addressing “Reality.” Best of all was the inclusion of the graduate students in the hosting of the visiting dignitaries. My mind was expanded not just through content, but through expansive interactions, where my (humble) opinion was sought and valued. I began to identify as a sociologist, and the idea of my becoming a “public intellectual” was fomenting.

But even more important than the yearly UN wingding, was the department’s academic program. Each semester I registered for a theory seminar, where we read the oeuvre of one or two theorists; a research practicum, where we produced work suitable for presentation at conferences; the proseminar, where faculty and advanced students presented their research; an interdisciplinary seminar, such as one on atomic energy; and an independent study. The seminar format replicated the format I was familiar with from Chicago, open discussion and meaningful writing assignments. In our study of Talcott Parsons, for example, we were asked to create a lexicon, labeling each concept as “new,” “new-name for old idea,” or “old name for an old idea,” and to rewrite *The Social System* in two pages or less. Through these exercises (and others), I made the theorists my own. I had grappled, synthesized, and summarized. And, once again, I fell in love. Writings by Durkheim, Weber, Goffman, Cooley, G.H. Mead, and Simmel – especially Simmel – were my bedtime, breakfast, lunch, and dinner companions. Their writings filled my board and brick bookcases. In the summer, I taught myself to read French and German well enough to pass the reading exams requisite for a PhD back then. And, I broke up with my boyfriend.

But most important to my future academic life was Professor Edward Rose’s yearlong graduate seminar on “Natural Languages,” a theoretical and empirical predecessor to Harold Garfinkle’s ethnomethodology – a name Rose rejected, preferring the more direct and simpler, “Natural Sociology.”

“The Werald is beautiful,” said Edward Rose, characteristically dressed all in white, and characteristically pronouncing “world” as the Anglo-Saxon “Werald,” lest we forget that we are always “wording the world” into being. A shoebox, silvered and textured, lay on the seminar table. Each of us looked through a peephole into a “small werald” Rose had created within the box. I do not remember what was “in” the world, but I do remember the vividness of the colors, the luminescent blues, especially, and sparkles.
Then, Rose divided us into two groups. “Imagine each group is a small, closed society inhabiting this small werald,” he said. “You only know about and can only talk to members of your own small society. Invent a living language to talk about your werald,” he instructed us.

“What’s a living language?” an ex-divinity student wanted to know.

Without a moment’s hesitation, smiling, Rose answered, “A living language is one in which you can write poetry.”

By the end of the semester, each group could converse, and I was writing awful poetry in my group’s awful Germanic language, “Fislongzuzakzurk.” At the start of the second semester, Rose told us, “You do inhabit the same werald, but you have worded it into being differently. Try now to translate from one language to the other; to communicate.” We spent weeks trying to do so. Via a courier, my group sent lots of information – our alphabet, our grammar rule book, my “poetry”; the other group only sent the same one-word message. Over and over again. No matter how we responded, they would send back a one-letter “word” followed by the original word. Over and over again. When I was feeling generous, I thought they couldn’t help it if they were dense; when I was feeling less generous, I thought the little demons were purposefully sabotaging the experiment.

I’m not sure who made the discovery – but it seemed to be all the Fislongzukzakers at once: the “others” were neither dense nor demonic saboteurs: They were “The Slsl” – and the word they were sending us was their name! The second “word” was their sign for the negative – “No.” Now, the two groups could civilly address each other and talk.

In this seminar, I learned through direct experiment and experience the centricity of symbolic interaction to the construction of group life, identity, and the functioning (yes!) of social and cultural systems, and how social separation and isolation created ethnocentrism. These were deeper learnings than any book-learning had been. It was as if I had gone abroad and been immersed in another language and culture – only to learn that it was “any” language, “any” culture that could be known about through implementing a “natural sociological” approach. And, if that was not enough, I learned through Rose’s modeling that the arts – writing poetry, creating lovely peephole worlds – were legitimate ways to know about the world. Science, social science, and the humanities were now happily nested together – the organization, methods and principles of – well – anything. Everything.

So, my research question changed from “how do people think” to “How do we claim to know something, and how do we defend that claim?” I became entranced by the sociology of knowledge, and the newly developing sociology of science.
During my second year of graduate school I married the University of Colorado’s top-ranked mathematics graduate student, Herb Walum. My father, one of Al Capone’s lawyers, had told me, “You can get away with murder, if you polish your shoes.” The “murder” I wanted to get away with was having children and a full-fledged academic career, a near unthinkable desire for a woman, a prima facie crime in 1959. So, I’d have to “polish my shoes” – that is, get married. Herb was the only man I dated who agreed that his wife could have both a career and children. Our son Ben was born 18 months later. We split care-giving times. I had the mornings free, Herb the afternoons. We never had a babysitter or “day care.” Without the near equality in parenting, I doubt I would have completed graduate school as soon as I did. It was most unusual, then, to have a father caring for a baby. When Herb carried a sleeping Ben on his shoulder under a blanket, people would ask if they could have a peek at the “puppy.”

Herb and I continuously debated the nature of universal knowledge. He could not imagine intelligent life existing without mathematics, as I could not imagine intelligent life existing without social organization, hierarchy, power relationships.

“Any intelligent life in the universe would recognize the prime numbers,” Herb insisted, defending the government’s decision to radio-broadcast the primes day and night into the heavens.

“Don’t be absurd,” I said. “Any intelligent life in the universe would have norms for social interaction. The government should radio-broadcast our name!”

With Ed Rose’s advice and a grant from the National Science Foundation, I wrote a dissertation in the sociology of knowledge. It was a sociocultural analysis of the conditions that fostered the development of pure mathematics from Pythagorean Greece to its multinational locations in the twentieth century. No slacker I. The dissertation was a qualitative historical and quantitative statistical one, the first sociology dissertation to use the university’s newly installed megasized UNIVAC computer. I thought that if I could show historically and contemporaneously how the most abstract of all knowledge systems – pure mathematics – was socioculturally located, I could prove by induction that all knowledge was similarly located.

Perhaps my dissertation topic was a clever way of simultaneously combining marriage and career. As a respectful wife, I would be dependent upon Herb for his superior knowledge of mathematics. His knowledge would help me win the argument, prove the primacy of the social over the mathematical. My dissertation topic could create a marriage in which our basic incompatibility, which I was denying, could be displaced into
intellectual differences. My dissertation could elevate me in sociology while proving Herb wrong. Academic achievement was the way I had won out over my brother, and I was doing it again, but this time with my husband. My life was a symbolic-interaction exemplar on speed!

Because I had married a mathematician and because I was fond of mathematics, as well, I was given a teaching assignment in nonparametric statistics, although I had never taken the class. I would stay one week ahead of my students, a teaching model I would replicate often over the years. Herb and I would laugh about the statistical research studies I was reading in the *American Sociological Review* that violated the assumptions of the tests they were applying.

Colorado had an “open” dissertation defense system. Every sociology faculty member attended mine, plus a scattering from history, physics, mathematics, psychology, and philosophy.

I was the only woman in the room. Edward Rose began the questioning: “You are about to become a doctor of philosophy. Philosophers know everything. How do the three laws of thermodynamics relate to sociology?” (I was glad I had been educated at Chicago and knew the laws!) At the time, I thought he was “testing” me; many years later, he told me he was “showing me off.”

I was 25 years old, Phd’ed, married, and a mother of a year-old son. I was resigned. Nothing new would ever happen in my life. My ignorance about life still astounds me.

1963. Herb had a position at Harvey Mudd College in Claremont, California, and I had a husband and a son. I thought I wanted a “time-out” to “make curtains,” but before three months had passed, I was climbing the curtains. Edward Rose found me a position at California State University – Los Angeles. That position, like all the positions I have ever had in academia, began as “Visiting.” I was hired not because I had published while still in graduate school or because I knew theory or the laws of thermodynamics, but because I could teach statistics.

Teaching five classes a semester at California State University was the easiest teaching I have ever done. I drove an hour in from Claremont on Tuesdays and Thursdays (Herb’s days off teaching), and made “friends” with other drivers, each in our own car-cocoon, “conversing” with each other with flashing lights, horn-blowing, waving, and kiss-blowing. The driving “community” opened my sociological self to the reframing of everyday situations and the rethinking of what constitutes meaningful interaction.

Most of my students at California State University were much older than I, and most were the first in their families to go to college. They were eager
to learn; I was eager to teach. Not only did I teach statistics and theory, I taught social psychology, urban life, American culture, crime and punishment, social stratification, research methods, marriage and the family, and the sociology of religion. I loved it. I loved the challenge of teaching material that was new to me and I loved learning about the lives my students led, most as full-time blue-collar workers.

I liked my colleagues, too, and found them intellectually challenging — Franz Adler, a Jew who escaped Germany and worried about anti-Semitism in America, Gil Geiss, a criminologist, who gently critiqued my writing, and Professor M., married to an undertaker’s daughter, and who taught that there were two social strata, the living and the dead, and Professor T., who kept a loaded pistol in our shared desk, and Professor L., who left the department for Oakland because his light-skinned children were becoming racists and somehow didn’t think that they were black, too, and Professor S., a statistician who read and discussed every paper I wrote, and Professor H., who had joined Alcoholics Anonymous (A.A.), and was “living one day at a time.” I was the only woman in the department — but, everyone else in the department, I felt, was the “only one” of their “kind” too. I never felt dissed, left out, or odd.

Similarly, I felt accepted within the southern California Committee on Racial Equality (CORE). Through my membership I developed a visceral understanding of the internexus between race and gender. The only white woman in the organization, I was elected secretary. Besides keeping minutes of meetings, my job grew into writing press releases, demands, letters, and accounts of our sit-ins and boycotts. Once, Herb and I took our son, Ben, to a Civil Rights march in Los Angeles. I never felt frightened; my “whiteness,” and my “womanhood/motherhood,” I felt, protected me from targeted violence, two entitlements in this protest world that were detriments in the academic one.

“Women in Science: Why so Few?,” my first post-PhD paper, analyzed the norms and practices of science that led to the exclusion of women. I submitted it to the American Sociological Review in 1963. The editor returned it with a one-sentence critique: “This paper was obviously written by a woman, because no one but a woman would be interested.” I took the critique to heart, buried the paper in a file, and published statistical work about the production of mathematics. These writings helped me find work when my husband in 1964 took a math position at The Ohio State University (OSU).

“I have a three-year-old son,” I told the “personnel director” of the sociology department at OSU, when I was asked about my family life. “And I hope to have another one.”
“Then, you should not be working,” he told me. “My wife went back to teaching... and well – our son – our son – well – he is not married – it is her fault that he is ... well, he is ... well, he lives in Greenwich Village. Morally, I cannot consider employing you.”

“Perhaps there is something for me in the department,” I asked the chair, Russell Dynes. He took my vita, noted my statistics ability, and said he thought there might be.

The next day, Saad Nagi phoned. He had a substantial grant from the National Institute of Mental Health to research disability and vocational rehabilitation at Dodd Hall. He could use a postdoctoral fellow, but only if I could design and execute my own research. A greater blessing I could not have imagined. I designed a social-psychological study that used statistical, ethnographic, and interview data: How do social skills impact recovery from traumatic injury? (cf. 1968). The birth of my second son, Josh, dissuaded me from renewing my postdoc. Spending time with paraplegic and quadriplegic young men – most injured through sports or car accidents – was too frightening for me as a mother of two rambunctious boys, a mother who had a little penchant for the mystic. I did not want to bring into my family life what I saw at Dodd Hall. Having a salaried professor-husband, of course, allowed me to be choosy.

Russ Dynes told me about a Visiting position at Denison University in Granville an hour’s drive away. They hired me to teach statistics in the math department, and a smorgasbord of courses in sociology. This was 1965. I hired a student to babysit Josh in my office while four-year-old Ben came with me to classes, astounding his “fellow” students by raising his hand and answering (often correctly) my questions. I don’t know how he did it.

The most difficult thing about Denison was the lack of support for research. I had to purchase my own reams of paper, hire a typist, and pony up postage. Publications did not lead to tenure. The best thing was a yearlong honors class for non-social science majors. I called it “The Sociology of the Sixties.” I assigned maybe three books and then set the class loose to “do” the sixties. We met everywhere: classrooms, theater, chapel, observatory, dorms, student union, grassy knolls, bars, eateries, bookstores, street corners. And we took field trips: art museums, theater, ice-cream factory, haunted house, earthworks, and pop events, like The Moody Blues (I loved them) and The Grateful Dead. Our last class was a feast that would have shamed Babette. Students chose topics and chose how they would study and present them – research papers, ethno-dramas, operas, multimedia shows.

This kind of teaching was quite unheard of. And quite wonderful. The students’ previous low opinions of sociology were toppled; they were
interacting and analyzing their worlds. More than two-thirds of those students went on to graduate degrees in social science, philosophy, or the helping professions.

Not surprising, my department chair – who happened to be a retired marine captain and unretired sheriff and Baptist minister – was none too happy with my unconventional teaching and my preschoolers. He invoked a hiring rule that required me to move to Granville, an impossibility for me as a married woman. I thought of suing Denison – I’m not sure on what grounds – when Hans Zetterberg, the new chairperson of OSU’s sociology department phoned and offered me a Visiting Assistant Professorship. In Scandinavian-accented English, he told me he had read my work, and he wanted me to teach theory. That I was a woman seemed irrelevant to this progressive man. That Hans Zetterberg had read my work was like – Wow! Three years later I learned that my salary was one-third less than the salaries of the men in my cohort – guys hired into tenure-track positions, guys fresh out of grad school, guys unencumbered with publications.

“You’re a scab!” an assistant professor said to me, as I moved into his old basement office in the summer of 1969.

“I’m a what?” I said. I’d been called many things, but never that.

He continued to berate me. I had no idea why, until later, when Zetterberg called me into his office. “I have created three categories of professors in my department,” he said. “The ‘chosen,’ the ‘invited,’ and the ‘frozen.’ I have frozen seven under-performing assistant professors and six underperforming associates. They will be leaving. That is why I hired you. For now, you are only invited, not chosen.” Thus, began my long – and mutually chosen – career at OSU.

My early years at OSU were heady and hailstorm ones. Social movements – anti-Vietnam War, Civil Rights, Black Power, Feminism, Sexual revolution, and Pychedelicism – dominated the cultural life of the country, campuses, and my department. I became a founding member of Sociologists for Women in Society (SWS), and met Betty Frankle Kirschner, a Kent State University professor, who became my closest friend. I wanted SWS to push for an American Sociological Association (ASA) sponsored gender section and journal, knowing such sponsorship would give our research legitimacy and our members a path to tenure-track positions. Others wanted to focus on national politics. The “others” won. Until later.

In my department, the untenured faculty bonded, socially and intellectually (cf. Franklin & Richardson 1972). Applying Marxist theory, we “bargained” for greater control over the conditions of our work, such as our teaching. I introduced the first undergraduate course on “The Sociology of
Women,’’ a graduate sequence on gender, and an advanced theory seminar that asked the question, ‘‘How do you know you know?’’ Speakers on civil rights, the Vietnam War, gay rights, and women’s issues were invited to introductory classes. New social movement organizations emerged. I became the faculty advisor for ‘‘Women’s Liberation,’’ and a member of the ‘‘Green Ribbon Committee,’’ an all university faculty committee dedicated to mediating between the administration and students, and to stilling violence on campus. We wore green-ribbon armbands and ‘‘patrolled’’ the campus. My husband, Herb, and I took turns as the threat seemed great enough that one of us needed to live to take care of our sons.

We were not being overdramatic. On April 29, 1970, students stormed my building, and I was trapped in my basement office, there being no exit other than the stairs the students controlled. Police were called in; they maced. Students threw Molotov cocktails and bricks (tearing up the walkways on the Oval). On April 30, Governor Rhodes called in the National Guard. They arrived in jeeps and tanks. They – or the police, I’m not sure – threw tear gas and fired ammunition. Students shouted ‘‘F… g Pigs!’’ The guard stood at one side of the Oval, the students at the other. The distance lessened. Hundreds were arrested. Many were hospitalized. Seven were shot.

On May 4, the United States invaded Cambodia, and guardsmen shot and killed four protestors at Kent State University, two hours north of us. ‘‘Not a good idea,’’ I intoned to the members of Women’s Liberation with whom I was meeting in a large lecture hall. Someone – maybe a student, maybe an ‘‘agitator’’ or ‘‘FBI instigator’’ – proposed that one of us volunteer to be killed for the ‘‘cause.’’ Not the person who suggested it, though. I prevailed, maybe because the students respected my authority in their ‘‘antiauthority’’ organization. Women’s Liberation members left the lecture hall and passed out flyers about their next meeting to protestors and guards. This was the scariest day of my life. I never again put my body on the line.

The university was closed on May 6 – the first time in its history (I think). Classes resumed on May 19. All students were given an automatic pass in all their classes. And, my department chair, Hans Zetterberg, resigned, leaving the department both leaderless and free to restructure itself.

My marriage ‘‘restructured’’ itself, too. ‘‘A divorce must be very heavy,’’ my six-year-old son Josh said, ‘‘for you to need other people to come and get it with you.’’ ‘‘Very heavy,’’ I said. I spent that Good Friday night taking a hammer, wrench, and screwdriver to the green corduroy couch that Herb had built. I had never used a hand tool before – or since. Destroying that ugly couch released the pent-up energy I would need to become an
untenured single mother of young boys. The nifty sharing of child-rearing responsibilities that I had enjoyed with Herb had terminated. I planned my teaching and office hours during my sons’ school hours, and held graduate seminars at my house. My sons respected my need to work on my research projects at home. To this day, I do not understand professors who spend from 8 to 5 or longer, five days a week at their universities when they can – with their computers, texting, and Skype – work some from home. I find my semitraditionalism ironic, as I fought for day care on my campus – but I guess I never thought that faculty would act as if they were on the staff’s time-punch schedules.

My name changed, too – back to my “unmarried” name, my father’s name, “Richardson.” But to do that, I had to get notarized acceptances of “name change” from my barely literate sons, and agree to keep their father’s name, “Walum,” as my legal “middle” name, lest someone (like the judge) think the children were “illegitimate.” So some of my research is under Walum and some under Richardson. Talk about symbolic interaction! I’ll never change my name again.

The fervor of the Women’s Movement over the next decade brought gifted graduate students to the department and to my seminars, – Diane Vaughn, Verta Taylor, Penny Maza, Judith Cook, Mary Margaret Fonow, Carla Corroto, Elizabethe St. Pierre, Wanda Pillow, and Amber Ault. That same fervor brought together feminist faculty such as Patti Lather, Nancy Johnson, Amy Shuman, Marilyn Johnstone, Patricia Stuhr, Nancy Campbell, and Suzanne Damarin into a feminist-postmodernist theory reading group (PMS). We still meet, read “hard” things through interdisciplinary lenses, support each other’s research projects – and have parties.

Ben and Josh learned about the accident on the 10 o’clock television news: the VW, rear-ended by a Buick, pushed into the trunk of a Chevy, squashed like a beetle bug between two full-sized cars; the medics pulling me out of the car, unconscious and bleeding; the television reporter announcing my name and predicting my condition: critical. Then, there was the coma, lasting a few days, maybe a week. Ten days, I think.

I remember little about the hospital, but I do remember that a kind-faced doctor, flanked by faceless, speechless men in white coats, frequented my bedside, tested my reflexes, and asked me embarrassing, unanswerable, questions. “What year is this?” I shook my head. “1972,” he said. “What is your address?” “How much is 3 × 7?” “Seven times nine?” Once the kind-faced doctor, tried cheering me. “You’ve only lost about 10–15% of your IQ,” he said. “There’s plenty left.” I had not the slightest idea what “10–15%” meant, but I could hear, feel the word, lost.
What had I lost? Much more than IQ points. I had lost access to my brain. I had lost language: my sword and my shield. My habitual routes for naming things were torn up, blocked off; paths to words and formulae were gone. I could not locate where anything was stored in my brain. I could feel my mind searching – this way, no, that way, up here, try there – searching, searching in my brain, as if it were a computer, searching for words, thoughts, connections, searching for memory, endless searches. Sometimes I could sense the place where a word was “hiding,” but I couldn’t make it come out, be recognized, be spoken. When I could find the word’s first letter, I felt grateful. I still do.

A trio of colleagues from the sociology department, bringing me caramels unchewable with my wired jaw, came to remind me of the upcoming “candidates’ meeting.” I was up for tenure and promotion. By departmental custom, a candidate appeared before the tenured to answer questions. Because I could not attend, the department dropped me from tenure considerations. Later, I learned that my department, believing I might be permanently brain-damaged, was protecting the university.

In 1973, I was still untenured, despite having been full-time academically employed for 10 years and having recovered, if not my statistical ability, my ethnographic ones. “Too soon,” said the promotion and tenure committee. “You have to wear short skirts – show your legs,” advised my mentor. I never again wore a skirt.

But as luck would have it (or hard work), my ethnomethodological study of “The Changing Door Ceremony” was featured on the front page of the Sunday Op-Ed The New York Times, along with pictures of me going in and out of doors (academic publication, 1974). The New York Times was the arbiter of “all the news that’s fit to print” – everywhere in the world back then. A member of the OSU Board of Trustees happened to see the article in Japan – in Japanese. He called Al Kuhn, the provost, to ask about me. Kuhn called my department chair. Before the day was over (or so I like to think), I was approved for promotion and tenure. Woody Hayes, the football couch, knew before I did. He notified me – offering better football tickets, commensurate with my associate rank.

Writing was a method through which I constituted the world and reconstituted myself. I was writing for my life; I was writing so I would have a life. Ever since the car accident I have been applying what I had learned as an undergraduate and graduate: what you need to know is how to know. Writing was and is how I come to know.

My mother was always working, doing something or other. In the decade or so after the car accident, I think I became my mother, but in
academic drag, rather than in a housedress. I published the first textbook on the sociology of gender, *The Dynamics of Sex and Gender: A Sociological Perspective* (1977). By writing it, I relearned sociological basics and helped create a new specialty, one which did not depend on advanced statistics, but privileged the interactionist perspective. I coedited (with Verta Taylor) several editions of *Feminist Frontiers: Rethinking Sex and Gender* (1983, still a bestselling anthology); began the National Institute of Education’s funded research with Anne Statham and Judith Cook that would result in *Gender and University Teaching: A Negotiated Difference*, a mixed-methods symbolic interactionist text written for administrators and the untenured (1991); I published 22 referred articles and I gave I don’t know how many conference papers. And, I wrote *The New Other Woman: Single Women Involved with Married Men* (1985), an “exemplar” of how symbolic interaction can be written for lay audiences and can be used as a tool for social awareness. That book was translated into Japanese, German, and Portuguese. I went on a book tour – national radio and television talk shows. It was grunge work, tiring, and not really glamorous. My chair froze my salary, falsely believing that my public success translated into financial fodder.

I was able to write and travel so much for interlocking professional, psychological, and domestic reasons. I had been promoted to Full Professor. I felt I was at last fully accepted into the academic world. I had made it. No more steps to climb. I felt different; I was treated differently. No more need to “compete” with the boys; I was “one” of them, even invited to lunch with them. I had moved past my childish personalized “feminism” into a politicized understanding of gender, finding community support and social movement resources. On the home front, my sons had gotten older and more independent. And I had gotten remarried.

My husband, novelist and Professor of English Ernest Lockridge, coparented my boys and supported my research and writing endeavors. He was (is) my first reader, scrupulous and kind in his critiques. Plus, he was (is) funny.

In 1988, I was elected president of the North Central Sociological Association, which was quite a nice honor, and would have been completely dandy if it weren’t for the niggling problem of having to give a presidential speech when I had been stricken by the postmodern bug. The ethical, theoretical, and values issues near paralyzed me. I had no choice, really; I would have to write my way out of my paralysis. And the only way I could imagine doing that was thinking of writing as method of discovery, a way of knowing what was going on with me – and with the social sciences, being
willing to write before I knew what I was going to say, letting go of the perfectly smart and perfectly competent student I once had been.

And thus began my halcyon days – months – years! While my department was going through its “Sociology = Demography” phase, I found colleagueship in the Society for the Study of Symbolic Interaction (SSSI) with Norman Denzin, Carolyn Ellis, The Adlers, Patricia Clough, John Johnston, Andy Fontana, David Altheide, Michael Flaherty, Art Bochner, Kathy Charmaz, Ginny Olsen, Joseph Kotarba, Gary Fine, Lonnie Athens, and many others.

I joined my penchant for the sociology of knowledge with feminist poststructuralism. This led me to ask a watershed question: How does the form in which social science is written contribute to its authority claims? At one SSSI meeting, I questioned the prose trope by presenting my interview with an unwed mother, “Louisa May,” as a narrative poem. All manner of four-letter words broke out: I was accused of fabricating the research. That shocked me; still does. I took ethnographic notes of the mayhem, and used them to write an ethnographic drama, “The Case of the Skipped Line.” I cast my family into the “drama,” recorded us – leaving space-time for me to speak out loud the thoughts I was too flummoxed to speak then, but could speak now. I presented that drama at the next SSSI meetings to an overpacked room. The eagerness of the audience to experiment with alternatives to the staid “science writing” model for qualitative researchers was alive and vibrant in the room. Many have commented to me that my “performances” gave them permission to do their own performances.

How you can know and what you can tell is limited by the format chosen. Writing in alternative ways is like adding another language to your repertoire; you do not lose your first research language, any more than you would lose it by adding any new language. But using alternative ways of knowing/telling has made welcome people – minorities, women, gay, lesbian, bisexual and transgender (GLBT) – who had felt diminished and excluded because the “data” they wanted to share didn’t fit into the standard dissertation or journal format. Giving permission to write and to publish in respected venues has enriched our SSSI world with not only new “data” but with embodied collegial diversity. In this way, I think, the pen (computer) has been mightier than the sword (disciplinary discipline).

Over the years, I have written research as lyric poetry, essays, dialogue, multimedia, altered books, conversations, dreams, memoir, and autoethnography. During this journey into the far reaches of the written word I have had the pleasure of many fellow-travelers – especially Carolyn Ellis and Norman Denzin. Writing Strategies: Reaching Diverse Audiences (1990), a
little Sage methods book (and still one of my favorites), provides a plethora of practical information for both “standard” and alternative writing. “Writing as a Method of Discovery,” a chapter in Norman Denzin’s and Yvonna Lincoln’s *Handbook of Qualitative Research*, introduces many of the inhabitants on this new “werald” (1993). When I finished the chapter, I was amazed at how much exciting and productive work was going on in so many different fields, and I knew that this was just the beginning.

About 10 years into this marathon of alternative representations of material (it’s malleable, that’s why it’s called *material*), I put together some of my articles into a kind of anthology, *Fields of Play: Constructing an Academic Life* (1988). *Fields* was different than other anthologies, though, in that I introduced each article with a “writing story,” a narrative that put the published research into a social-political and/or personal context. That is, my research/writing became “subjects” for analysis; another layer had been added. *Fields* has become something of a cult book, as, unfortunately, women and minorities continue to experience awfulnesses in academia similar to the ones I recounted in that book. I think that my becoming a Full Professor at a major research university gives the reader hope. I hope so.

A fortuitous turn in 1990. My husband, the novelist Ernest Lockridge, began coming to SSSI conferences with me. I welcomed his entry into my academic world, but what I really wanted was to write with him. Our highly different styles, disciplines, biographies, and sensibilities made that an impossible dream – until we hit upon an alternative format, a collaborative effort that honored each of our voices as distinct and separate, neither more important or in “control” than the others. Indeed, it is a format open to everyone and a practical way to avoid the hierarchy of researcher/researched.

Each of us wrote a narrative account inspired by a shared travel; then, we read each other’s narrative and had a wide ranging conversation across disciplinary boundaries. We conversed about writing matters that matter to us – ethics, authorship, collaboration, witnessing, fact/fiction, audiences, observation, and imagination – and we conversed about relationships that matter to us – ours, our blended family, our biographies, the world around us. The resulting book, *Travels with Ernest: Crossing the Literary-Sociological Divide* (2004), models writing as a method of discovery, turning theory into practice: writing that models the issues it considers. How can you craft your materials in different ways? How do you write yourself into your text without becoming the center? How can one incorporate autoethnography into cultural studies? How can one collaborate across disciplines? Gender? Sensibilities? How can one honor/retain independence of “voice”? What constitutes “witnessing”? Memory?
And then, the unthinkable happened. 2006. My dearest friend (Professor of Sociology, Kent State University), Betty Kirschner, was dying from emphysema. I did not know how I could live without her. For 35 years she had been my confidante, supporter, reader, and noble adversary on so many issues—like her smoking and “knee-jerk” liberalism. For the 10 months before Betty died, I made a near daily entry in my journal about her terrible illness and my mixed emotions—sadness, anger, despair, fear, loss—and my general (and evolving) attitudes towards death, dying, the health-care system, and friendship. A few months into the journal-keeping I began to question my motives: Was I still trying to deal with our losses or had I unconsciously begun a new book—a book about the death of a friend? Had I become a circling vulture?

Last Writes: A Daybook for a Dying Friend (2007) crafts my journal entries into a new (for me) format, the daybook. That format mimics the day-by-day process of dying and grief. The entries tell two interlocking stories, the story of a deep and lasting friendship between two women and the story of the writing of the book, particularly its ethics. Last Writes welds together my academic, personal, and political activist lives. The threads of my life are entangled—always have been, always will be. I lost my friend, but the book has given her an “afterlife.”

The last entry in the book—July 15, my birthday, a month after Betty’s death—is a dream in which Betty is dressed in white reclining on a white chaise. I show her a travel map of California, printed on 8½ × 11 inch writing paper.

I say, “I guess we won’t be driving down the coast road together, down Route 1.”

Betty says, “Don’t be so sure of that, Laurel.”

She was right. Mitch Allen (always a supporter of SSSI projects, and an editor of three of my books) chose to publish Last Writes in his newest venture, just off Route 1 in California, Left Coast Press!

Between a Protestant ethic-type father and a compulsively ethical Jewish mother, it has been near impossible for me to let go of “working.” My father intoned, “I should always be kind and use my talents so that when I leave this world it is better than how I found it.” Father worked pro bono for African-Americans in Chicago before there was an active Civil Rights movement. He believed everyone, regardless of race, creed, religion or crime, was entitled to the best defense lawyer possible. Him. (He never lost a case.) His stances on justice and ecology were global. When I was three and gleefully stamping on outside ants, he called me in and said, “All living things have a right to live.” My mother, I learned at my 50th school reunion, sent home-cooked food to my classmate’s families who were in dire straits.
She was Room Mother, and “mothered” any child in need. I did know that
she “fostered” very ill “blue” babies; knit sweaters for the orphanage;
accepted into her clean house “rescued” dogs; shared her “allowance” with
my near destitute Gentile relatives; rolled bandages for soldiers; and
overcame her deep fear of water, probably instilled from riding steerage
across the Atlantic Ocean to Ellis Island when she was eight. She learned to
swim at age 40 – a requirement back then for a Chicago high school diploma.
She kept everyone’s secrets and never said a bad word about anyone. I have
never written so positively about my parents before.

So, although I have been officially retired since 2004, I am not “retired.” I
continue to do my “work.” I write, research, mentor, teach, and do
workshops – nationally and internationally. I’ve had a great good fortune to
be invited to such wonderful places as Denmark, England, Ireland, Iceland –
where I taught gender to all 45 graduate students in the humanities and social
sciences – and to Australia where I was the first woman to become Miegunyah
Distinguished Fellow at the University of Melbourne, a feat masterminded by
Julie White. New Zealand, Italy, and France beckon. I only work pro bono,
now. I do not want to be monetarily paid. And, I don’t want to feel beholden.
That’s my father speaking. I am grateful that I do have a pension – and a
husband with a pension. I appreciate being my own security-net.

I am in the midst of writing two books – *Seven Minutes from Home* and
*Recovering from Death* – or maybe they will be one interwoven book. I’m
getting the insides and outsides of my house rejuvenated in warm natural
colors, and experimenting with the visual arts, making altered books and art
books, writing poetry, playing the piano, and feeling gratitude that my
mentor, Ed Rose, modeled a multifaceted life. “Erose’s” art rendition of
William Blake’s, “The Fallen Man takes his repose,” – his gift to me on my
university retirement – is in my view as I type.

I’m spending time with friends and family, happy that my competitiveness
with my brother has abated. He’s had his own academic career as an
economist and Dean of a Business School, and he is a world-class Mentalist, a
member of the prestigious “Magic Circle.” Twice a year Ernest and I take a
jaunt to Sedona where I buy my “Life is Good” T-shirts. My sister grew up to
become a teacher of and advocate for developmentally delayed preschoolers.
Two years ago she “went away” forever. And, I’m working through what she
told me on her deathbed: I was born a twin. My twin did not survive. My twin
got lost and was never found. (I told you my mother kept secrets.)

And, just this month we’ve adopted young Papillon dogs, Lily and Bashi.
They will join me, if it turns out that way, in my (volunteer) work with
hospice and bereaved children.
I have been honored to serve SSSI in various capacities, and I am appreciative of the honors that organization has bestowed upon me. SSSI honored *Fields of Play* with the SSSI Cooley Award for the best book of 1997. In 1998, I received the first SSSI Feminist Mentor award. In 2004, *Studies in Symbolic Interaction* honored me with mini-Festschrift, “Laurel Richardson’s Contributions to Symbolic Interaction and Communication.” And, now I am honored again to be invited into the *Studies* “Blue Ribbon” self-portraits of interactionists edited by Lonnie Athens.

To write a “self-portrait” is to be a witness to one’s self. Writing about the self gives one the opportunity to tell one’s story – over and over again, to see it on the computer screen, to alter it, delete it, over and over again, altering one’s sense of self, because one is reading about a newly emergent self, changing on the screen before one’s very eyes. One witnesses again and again one’s life, not as a foretold story, but as an evolving one – evolving the writer/witnesser as well, creating a more complex and, hopefully, more appreciative personage.

Symbolically interacting with my evolving self is transformative. Yet, another gift of SI.

**REFERENCES**


AUTHOR BIOGRAPHY

Laurel Richardson (The Ohio State University, Sociology) is an internationally renowned symbolic-interactionist with specialties in arts-based research, writing issues, gender, and contemporary theory. She has modeled alternative representation of research through her publications as essay, poetry, ethnodrama, and autoethnography. She has been honored with awards for her writing, mentoring, teaching, and community outreach. She is engaged now with a series of two interlaced ethnographic books: Seven Minutes from Home and Recovering from Death. On good days, she writes, walks her Papillion dogs, and writes some more.