

## Chapter 8

# A Serendipitous Career

Leon Grunberg



My sociological career began when, by chance, I met an American professor who was on sabbatical in London. We were both regulars at the Queens, a London pub, and after months of drink-enhanced conversation he somehow became convinced that I had the makings of a sociologist and persuaded Michigan State University to offer me a research fellowship. Although it might seem as if I came to sociology somewhat by accident, I believe that my experiences, temperament, and the way I was beginning to see the world, predisposed me to accept the fellowship offer and go to the United States. This chance meeting was one of several chance occurrences that shaped my life and career, giving them a somewhat serendipitous character.

### **Childhood and Student Years**

We are all at the mercy of large events and my young life was irrevocably altered in 1956, when Britain, France, and Israel attacked Egypt after Gamal Abdul Nasser nationalized the Suez Canal. As an eight-year-old boy I was excited by the sandbags and the anti-aircraft guns at the end of our street in Cairo and the sounds of British bombers flying above our blacked-out house. But I also felt my family's unease,

created by the daily visits of the police, the loss of my father's job, and my parents' scramble to sell our few possessions at "give away" prices. We were, along with several thousand other Jews, on our way out of Egypt, carrying with us, along with our suitcases, precious memories of a world and a Levantine culture that was about to disappear forever.<sup>1</sup>

It was also by a stroke of historical chance that it was a foggy and damp Victoria station in London that welcomed us to our new home rather than Paris, New York, Tel Aviv, or Sydney (each a destination for a few of our relatives). Although my father was stripped of his Egyptian nationality and was therefore stateless, my mother carried a British passport because her father was born in Gibraltar. So it was to Britain that we went and it was there that my parents started their lives again. Adapting to this new life was difficult for my parents, as they struggled to learn English and the strange ways of the British. But for my father none of the difficulties of adaptation could shake his appreciation for Britain's generosity to refugees or his great admiration for General Montgomery. Every time Montgomery appeared on television, my father would rise from his chair, approach the television until he was but a couple of feet from it and listen with rapt attention as the General expounded on his wondrous exploits. It was only later, when I had read the history of the war, that I learned that it was Montgomery and the British 8<sup>th</sup> army which had defeated Rommel's Africa corps on their march towards Cairo in 1942, and indirectly saved our family and made the rest of this story possible.

So it is in a very concrete sense that I understand what C. Wright Mills (1959) meant by his call for us to become "aware of the intricate connection between the patterns of [our] own lives and the course of world history". That awareness of how large historical events and trends affect individual lives has shaped my research focus and approach, as I will illustrate below.

But there was another effect on me of our sudden transplantation to England. Although we had been part of a very small Jewish minority in Egypt, I had never felt like an outsider, perhaps because Cairo in the fifties was a lively, cosmopolitan metropolis, home to a wide variety of religious and national groups. In any case, I was probably too young to have noticed any prejudice or discrimination. In the London of the fifties and sixties, being English was based on ancestry, and no matter how good my English and soccer skills became, my Egyptian birth was a source of ridicule and a marker of my separateness among my schoolmates. I grew up with a sense of shame about my accidental origins and with a powerful urge to gain entry and acceptance into my new world. Even as time and experience strengthened my feelings of Britishness (Englishness may have been asking too much), I never completely lost that sense of being different and an outsider.

This feeling was and still is particularly acute in the company of "true" Englishmen, as I assume that my name, or a question about my origins, will expose me as an impostor. But in a way this lingering sense of being an outsider, even as I fully assimilated, was useful conditioning for my future vocation. It produced an insider/outsider perspective that made it easier to observe things that others didn't

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1 For a wonderfully evocative description of one slice of this disappearing culture see Aciman (1996).

notice or took for granted. This sense of being different, perhaps along with an inborn temperamental rebelliousness, also acted as a break on a powerful and natural desire to identify closely with any one intellectual community. So although the desire to belong has remained powerful throughout my life, and occasionally produces a tinge of sadness that I cannot share in the comfort that belonging appears to confer, I seem to have a stronger, almost perverse, need to remain independent and an outsider. I have therefore never joined a political party and I have refused to accept the labels colleagues sometimes try to pin on me.

In high school I was always trying to pick holes in the teachers' arguments or in views that seemed to fit too comfortably with the conventional wisdom. Several of the young teachers at my grammar school encouraged this kind of sharp criticism and seemed genuinely pleased and stimulated by the lively, and often heated, intellectual to and fro in their classrooms. One teacher in a comment to me, which could have applied to several of my schoolmates, said I exhibited a kind of subversive intelligence. I increasingly think that having a contrarian spirit is very useful to scholars and researchers as it predisposes them to question and challenge what seem like settled findings or theories, and if they're fortunate, to discover something new or original. I also think such an attitude enlivens one's teaching. The hard but necessary extension of such an attitude is to apply it as rigorously to one's own work and ideas.

To my parents' great disappointment, I went to a "secondary modern" school, a place where those not destined for university were prepared for work in the nation's factories, offices, and shops. About eighty percent of all eleven-year-olds went to such schools; the other twenty percent went to grammar schools where they were prepared for university and careers in business and the professions. It was in secondary modern school that I got a first-hand look at the oppositional culture of working class boys so vividly portrayed by Paul Willis in *Learning to Labor: How Working Class Kids Get Working Class Jobs* (Willis 1982). Fighting, playing soccer, and "having a laff" were powerful attractions, and I did my fair share of all three. Still, although these norms permeated the school, there was a sizeable minority of pupils, mostly Jewish, who tried to negotiate a path between the oppositional culture of the "lads" and the almost obsessive belief in academic achievement deeply embedded in the psyche of the families of my Jewish friends, as well as my own. Like any smart human sorting system, this stratified British educational system had built in an escape hatch. Every year a handful of fifteen or sixteen-year-olds who had done well at "O" levels, a nation-wide exam taken at around the age of sixteen, were transferred to grammar school. I was one of these lucky few.

It was in secondary modern school, however, that I became conscious of social class divisions and developed a simmering sense of injustice at the unfairness of such an early selection process. I carried a "big chip on my shoulder" and was determined to expose the injustice of the system and, of course, to prove the faceless judges of my fate wrong. Ironically, as an adult this early failure became a badge I wore with an "in your face" pride, as did many of those angry, upwardly mobile working class young men who took Britain by storm in the fifties and sixties. But my five years at secondary modern did more than stoke my anger at the class system; they also taught me to respect the daily realities and choices of working class lives. Many,

but by no means all of my working class schoolmates found the academic part of school a boring rite of passage, one to get through as quickly as possible so they could get on with mating, playing soccer, and earning a living. They made these choices despite the best efforts of the teachers who tried to engage them in academic learning. Willis is right that such choices often ensured the reproduction of the class positions of these youngsters, but they were choices nonetheless, even if these young men were choosing from a highly constrained set of options. I went part way down that road with them and became a passionate devotee of soccer (my wife calls such devotion a kind of madness). Playing, watching, and talking soccer consumed hours of my time and that of many of my schoolmates. I came to understand the seductive quality of soccer and other team sports. There was an intensity and an unpredictability to those few minutes on the field that heightened one's senses, and the easy camaraderie off the field provided a haven from daily worries. In fact, I continued playing soccer well past the age when my body began to give out, partly for the joy of it, but also because it was one of the few activities where I could meet, on some basis of equality, individuals with all kinds of jobs and class backgrounds. The friendships I developed through sport have kept me connected to the aspirations and concerns of nonacademic men and their families and taught me to respect the lives these individuals made. That is why, even when I was most heavily influenced by Marxism, I have never seen working people as passive and helpless pawns pacified and distracted by clever elites.

I came of age, like millions of other baby boomers, in the turbulent sixties, entering Sussex University in 1967, at the very height of that swinging, rebellious era. Sussex University in those years was known as having one of the trendiest campuses in Britain. This was where some of the famous, and the not-quite-smart-enough-to-get-into-Oxford-and-Cambridge, children of politicians, businessmen, and professionals went. The late sixties, of course, seemed like heady revolutionary times to many students. Mixed in with the pot and the parties there was also a lot of radical political activity. There were marches, sit-ins, and endless meetings and resolutions. But to my eyes there was a certain unreality, perhaps even a kind of theatricality, to all this activity. Britain sat out the Vietnam War so we were not at risk of being called to fight as were students on U.S. campuses. I am certain some of the students acted out of a moral commitment to the principle of non-intervention and to an abhorrence of the brutality of that war, but it seemed to me that for many others the protests were a form of fashionable and risk-less rebellion. There was also a fair amount of inverted class snobbery at work on our campus. How one dressed and looked became signifiers of one's stance towards authority and class privilege. "Donkey" jackets, garments then often worn by laborers, became emblems of solidarity with the working class. Afghan coats, beads, and other paraphernalia were marks of one's anti-materialism. Pleading poverty as you ate slices of bread smeared with free ketchup demonstrated to all that you were not part of the privileged elite. To my eyes these displays seemed frivolous and false even as they were colorful. After all, neither their class solidarity nor their consciences stopped them from riding around in the Mini Coopers and Triumph sports cars that their middle and upper-middle class parents had given them.

I went to university with the intention of majoring in economics, but one day as I watched a tutor draw indifference curves on a blackboard, I realized I did not much care for this kind of economics, with its impoverished view of human psychology and social action. Even then I realized that *homo economus* was a convenient theoretical assumption and that the wellsprings of human action were far more complex and mysterious than could be captured by rational action models. And, since I was reading a lot of American literature and history at the time, I switched to an interdisciplinary major in American studies. I became fascinated by American society, by the images and stories of crime, riots, racial inequality, and brutal war but also by the physical and psychological space and openness that seemed to come through in the films I saw and the books I read. Bernard Malamud's (1961) novel, *A New Life*, was particularly appealing as it told the story of a down-on-his-luck Jewish instructor of English who leaves the East Coast to take a position at a college in the Pacific Northwest. His fumbling attempts to fashion a new life for himself in the middle of the lush, verdant forests of the northwest got lodged somewhere deep in my memory and seemed to foreshadow my later transplantation from Britain to the United States and then to Washington state.

After graduation and a brief interlude working in a bank, I taught working class youngsters as an instructor at one of the many technical colleges that were then providing education to some of the eighty percent of young men and women who had failed the "eleven plus" exam and not gone to university. My job was to try to inject a dose of liberal learning into the education of young apprentices training as draftsman, motor mechanics, and body shop repairmen. In their one or two days a week at college, I had one hour in which to broaden their minds and sharpen their critical thinking skills. During this time I also taught social theory, including Marx, to a small group of housewives, for that is what they were called at the time, under the auspices of the Workers' Educational Association, the largest voluntary provider of adult education in Britain. As a young teacher in my mid-twenties, I quickly realized that I could not spout abstract ideas to people who every day dealt with very practical and concrete problems. Ideas and theories about social life had to be grounded in empirical reality and had to be expressed in a language that was transparent and readily comprehensible. This lesson was of immense value to me as a research sociologist and university instructor.

## Influences

I did not grow up in an intellectual family. Academic achievement was important, but there was little concern in my family for the content of education, only for what worldly success it could lead to. However, though there were no more than a handful of books in the house, my father was an avid follower of current events and I would read the paper he brought home every night. As my three siblings grew and the house became too crowded a place in which to study, I spent more and more time in the local libraries. I would wander along the shelves, picking books to leaf through, often because of an unusual title or book cover. It was in this way in 1966 that I discovered the writings of the Italian holocaust survivor, Primo Levi (1987). His

account of his year in Auschwitz in *If This A Man*, and of his return journey home after liberation in *The Truce*, deeply moved me—partly because it was my first full exposure to what had happened, but also because of the understated way he told his story, without any trace of pathos. I was also taken with his careful sociological and psychological observations as he described how individuals sought to survive and to maintain a semblance of dignity and even humor in a cruel and capricious environment.

Browsing library shelves also led to the discovery of George Orwell and a life-long admiration for his writings. Like Levi, Orwell wrote in a straightforward, direct manner about things he had experienced first-hand. His observations were hard earned and he avoided the temptation to make the facts fit preconceived ideas. Orwell had no time for orthodoxies, whether political or ideological, and he rarely romanticized those whose plight he described. Orwell and Levi, with their keen observational skills and their independent minds, were powerful influences on my approach to doing research and, in my view, are worthy of emulation by all prospective sociologists. These fortuitous discoveries began a lifelong habit of browsing in libraries, something I find myself doing less and less these days and that alas few of our current students seem to do, tied as we all increasingly are to the internet.<sup>2</sup> It is no exaggeration to say that wandering and browsing in libraries encouraged me to read widely and to downplay disciplinary boundaries in my research and teaching.

My reading of fiction and memoirs has proved to be a powerful antidote to the often necessary categorization and classification that sociological analysis demands. By making the individual the fulcrum of the action, literature rebalances the weights that sociologists assign to deterministic structures over individual agency and reminds us of how complex and even contradictory individual lives are. Not surprisingly, I have found that nothing excites the undergraduate student's sociological imagination as much as a story of individuals grappling with powerful social and political forces and having to make choices in the face of uncertainty and obstacles. One of the joys of working in a liberal arts college is that I have been able to marry my sociological interests and my love of literature in a course, "Sociology through Literature", that I have taught for over two decades.

Theoretically, my orientation has been most heavily influenced by Marx's analysis of capitalism. In particular, by the central role he gave the conflicts and struggles that occur between labor and capital at the "frontier of control" (Goodrich 1975) and over the distribution of the surplus, and also by his understanding that it is the dynamic nature of competition among companies that gives capitalism its revolutionary energy and creates its profound insecurities. Focusing on these two relationships still seems to me a necessary if not sufficient basis for any cogent analysis of the factors that influence how economic organizations and workplaces operate. My encounter with the work of Tilly (1978) provided a framework and concepts to explain how theoretically expected conflicts of interests were transformed

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2 The internet, of course, also fosters a kind of browsing, though the speed and the multitude of connections may overwhelm the wanderer in cyberspace and inhibit the kind of careful discoveries that browsing in libraries encourages.

into dynamic struggles for power and control. To understand what happened in firms and in the political economy of the larger society it was necessary also to examine the ability of the contending parties to organize and mobilize their resources and to assess the opportunities for action created by particular historical conditions.

The intellectual excitement in graduate school was generated by the work of several neo-Marxists such as David Harvey, Harry Braverman, Paul Sweezy, James O'Connor, Nicos Poulantzas, Louis Althusser, and Bowles and Gintis, as well as by the writings of scholars such as Andre Gunter Frank and Immanuel Wallerstein, who advanced a more third world perspective. Although I shared in the excitement generated by the work of most of these scholars (I seem to be constitutionally incapable of making sense of the writings of Althusser and more recent French post-Marxists),<sup>3</sup> I was much more sympathetic to those who worked within the classical Marxist tradition. Capitalism, in this tradition, is understood as an economic and social system that is often cruel, but one that also is relentlessly dynamic, producing great technological and material advances. I suspected then, and history seems to have confirmed, that it was far more revolutionary and progressive in its accomplishments than the political revolutions that had been carried out in the name of Marx. Although most neo-Marxists had soured on the Soviet version of socialism by the mid-seventies, some briefly flirted with Maoism and his cultural revolution; others looked to the self-management experiments in Yugoslavia or to other socialist experiments like the Ujamaa program in Tanzania. Learning about the horrors and failures of these attempts to consciously remake the world clarified for me that the only sane political position for someone on the left was social democracy. What was necessary was for ordinary people to have the power and ability to shape how the system operated and to distribute the fruits of its dynamism more equitably.

Even as I fell under the sway of Marx and neo-Marxists, my dissertation advisor, Bill Faunce, was a quiet but insistent voice reminding me that many of the problems of capitalism, such as alienation, were actually problems of an industrial society (Faunce 1981). Moreover, he argued that many workers could escape much of the damage of what "objectively" seemed like alienating work by compartmentalizing their work and non-work lives and by seeking recognition and self-esteem outside the workplace (Faunce 2003). We had many intense conversations about his thesis over drinks in London, in East Lansing and at his cabin by Lake Superior. I argued with some conviction that this compartmentalization came at great psychological cost (a point made by Sennet and Cobb [1973] in *The Hidden Injuries of Class*) and that in any case it was extremely difficult for workers to avoid daily reminders of their low social ranking. However, I also sensed that if I pushed the point too far, it was but a short step to arguing that these workers not only didn't understand the causes of their alienation but, in their responses to it, were exhibiting false consciousness. I was never comfortable with the false consciousness concept as a way to explain away the lack of militancy or revolutionary fervor of workers. Yes,

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3 Apart from the opaque language, French structuralism remained too much at an abstract level, with few connections from concepts to actual individuals and groups, to appeal to my empiricist sensibilities. A witty and devastating critique of Althusserian structuralism is given in Thompson (1978).

workers could be poorly informed or naive, but what did it mean to claim that they did not know what was in their best interests? Naiveté was certainly not confined to workers and was it so abundantly clear that the enlightened few armed with Marxist theory knew better than the workers themselves what would improve their lives? What I learned from Bill Faunce was the importance of not letting theoretically derived expectations determine what I found but rather to listen to what people told me and to be open and flexible enough to let experience and the research evidence guide my interpretations and explanations. They say imitation is the best form of flattery and I have tried, quite imperfectly to be sure, to be the kind of calm, rational and gentle mentor and interlocutor to my students that he was to me. Bill and his lovely wife, Sheila, opened their home to me, included me in many of their family events and made my graduate years and my first few years in the U.S. among the happiest of my life.

### **Orientation**

My theoretical and methodological orientations are best revealed by a brief overview of my research: by the topics I choose to study; how I frame the questions I ask; how I design and conduct my research; and in how I interpret the results of my work. To simplify, I would say that, at heart, I am an empirical sociologist who freely borrows ideas from other disciplines in trying to understand how economic forces shape and affect our lives. I assume, though with somewhat less confidence now than in the past, that what happens in the workplaces in which we spend a large portion of our lives can and often does have powerful effects on our economic, social, and psychological well being. I am persuaded that the pursuit of economic output and efficiency inevitably entails social and human costs. I am persuaded as well that how we organize our economic activity is not immutable but is the result of human, and hence political, choices. That is why it is important to study economic organizations and to clarify the consequences of their decisions and activities for the millions of individuals who are affected by them.

As a graduate student in the mid-seventies, a great deal of academic buzz centered on the multi-national corporation (MNC). These were seen as the leading edge of a renewed process of internationalization (the term globalization only entered the popular and academic lexicon in the nineties). Indeed, MNCs seemed to be the driving force of this process and then, as now, their activities and what they portended for the future were the focus of intense debate and controversy. But as I read pretty much everything that was then written on the MNCs I found that the “storm over the multinational” had generated a lot more hot air than concrete insights. Dozens of books engaged in speculation and grandiose predictions about the future, painting either frightening scenarios of a world dominated by MNCs or, at the other extreme, bucolic visions of an integrated and prosperous world community. But there were very few studies then that provided an in-depth, close-up examination of how these companies operated and fewer still that investigated the effects of MNC activities on workers and the communities in which they lived.

A major reason for the absence of such studies was that MNCs had erected a very prickly shell around their operations, one that made it very difficult for outsiders to penetrate deeply into their decision-making process. It occurred to me, by analogy, that if individuals were likely to be more open when they were vulnerable and in crisis, that MNCs might also be more transparent when they were undergoing their own form of crisis. My instincts suggested they would be most exposed, despite their best efforts at maintaining secrecy, when they were undertaking divestments or closures of subsidiaries. I assumed and hoped that the controversial nature of closures and the likely associated conflicts would create openings that a dogged researcher could pursue. I wanted to get as deep inside these organizations as I could and to do that I took to the field in what I called the “long march” approach to collecting information (one of my dissertation advisers remarked that this approach was somewhat akin to “investigative journalism”, a descriptor that I was happy to accept). I began at the bottom and periphery of these organizations and gradually worked my way to the top and center, where ultimate decision-making power resided (Grunberg 1981). This involved traveling to the location of these threatened subsidiaries, in Milan, in Poissy outside Paris, and in Hull and Coventry in England to interview workers, union officials, local politicians, managers, and former managers of these subsidiaries. The former managers were particularly helpful informants as they had detailed knowledge of the financial and political bases for the divestment decision and were often quite willing to divulge crucial confidential information because of their bad feelings towards the MNC. It’s amazing how willing top executives are to meet you once you have done all this detailed legwork.

As Robert Merton (1956) has pointed out, serendipity is at the heart of the research process. Studying one thing can throw up an unexpected finding that leads you to develop a new hypothesis and a new line of research, provided one is able to see the theoretically rich possibility in the unexpected finding. One of the cases of threatened divestment I had examined concerned the United Kingdom subsidiary of the Chrysler Corporation. Naturally, the question arose as to why it was the U.K. subsidiary rather than the French one that was selected for closure. The official and very plausible answer was that in making the same car, Chrysler U.K. had a productivity performance that was much worse than Chrysler France. A great deal of internal company research had been done on this difference and it seemed clear to company officials and to me that a large reason for the difference was the absence of almost any strikes at Chrysler France and the numerous, sometimes numbering in the hundreds per year, short-duration strikes at Chrysler U.K. In discussing this productivity difference with one manager at Chrysler U.K. he casually mentioned in passing that he thought the French subsidiary had a much higher accident and injury rate. The question formed in my mind: could the higher productivity be related to the higher accident rates and could both be due to the weakness of the workers at Chrysler France?

This new line of investigation provided some support for this hypothesis (Grunberg 1983), although ensuring comparability of accident and injury data is fraught with difficulties, given how susceptible the production of such data is to manipulation. Fortuitously from my perspective as a researcher, major political changes occurred a few years later in Britain and France, with new governments coming to power

with significantly different policies from their predecessors (Thatcher became prime minister of the U.K. in 1979, and Mitterand became president of France in 1981). Here was an opportunity to retest the hypothesis. Would the coming to power of the socialists in France strengthen the largely immigrant workforce at Chrysler France and thus reduce their productivity and accident rates? And would Thatcher's assault on the power of British unions weaken the shop-floor militancy of the Chrysler U.K. workforce and result in higher productivity and accidents? Additional field work in France and Britain tended to provide affirmative answers to these questions (Grunberg 1986).

This research also taught me two important methodological lessons. One was about the value of what Merton calls the "strategic datum which exerts pressure upon the investigator for a new direction of inquiry which extends theory" (1956:105). In my case it was connecting two or three seemingly disparate sets of facts because my reading of Marx had already sensitized me to the reality that economic production is a social activity with attendant human costs as well as benefits. How much power workers had in the workplace (and in society) could influence their productivity and safety. The other was how rare yet how methodologically powerful natural experiments were, experiments where most variables in a comparison are similar except the one that is central to your theoretical argument. In the Chrysler comparison the political context changed, as did the relative power of the workers, but other important factors, like the technology, the product, and the location did not.

The underlying assumption of the Chrysler comparison was that conflictual capitalist relations between owners and workers lay behind the apparent trade-off between productivity, an indicator of our efficiency; and safety, a mark of our humanity. The logic of this perspective suggested that in workplaces with non-conflictual relations, where workers were also the owners, the trade-off would be eliminated or substantially reduced. Worker ownership and control would result in more efficiency and safer and more contented workers. Selecting cases that are at the extreme ends of a causal variable (in this case the variable is the power relations between workers and owners) can speak to whether the trade-off is confined to workplaces with capitalist social relations or whether the trade-off might have more universal applicability (Stinchcombe 2005). Fortune once again was kind to me as one of the most robust experiments in worker-ownership existed in the Pacific Northwest, with several plywood cooperatives located close to the university where I worked.

I spent two years visiting the coops, interviewing scores of worker-owners and other employees, and gathering data on productivity and injuries. I carried out extensive opinion surveys of the workforces and spent some time at each site observing the production process. I followed the same data collection process with conventionally organized plywood mills (which included union and non-union firms) and tried as best I could to match the mills on as many variables as I could, except the one deemed to be causal. The results of my research were surprising and disappointing to me. The cooperative mills were *less* productive than the conventional ones and possibly also more dangerous for the workers. Few of them exhibited the kind of positive social relations I expected; in fact several of them were quietly dying or close to disappearing (Grunberg 1991).

While studies on cooperative workplaces report mixed and sometimes disappointing results, I have been struck by how hard it was for me and for others doing research in this area to abandon or modify very deep rooted assumptions about the organization of work and its consequences. All researchers want very much to have their hypotheses confirmed, and most can live with seeing their hypotheses disproved, if only because they are forced to explain why their predictions were wrong. But when assumptions that are derived from one's values are challenged by the evidence, there is a powerful temptation to shore up the belief system by downplaying the evidence or undermining the design or methodology of the study that produced the disconfirming evidence. Such rigorous skepticism is an essential element in the scholarly process and it took additional research on worker owned firms (Greenberg and Grunberg 1995) to convince me that whatever benefits might flow from worker ownership and control, they were no panacea for the alienation and troubles that accompanied modern industrial work.

These disappointing results echoed those found by Ed Greenberg, a political scientist at the University of Colorado. Greenberg had studied these same plywood cooperatives a few years earlier and had written an important book on the promises and limitations of workplace democracy (Greenberg 1986). We quickly realized we shared an intellectual orientation and resolved to collaborate on a future research project. It was also at this time, by chance, that Terry Blum, a discussant on a paper I presented at a conference, and her husband, Paul Roman, both leading scholars in the area of alcohol and drug abuse in the workplace, encouraged us to apply for funding that the National Institute of Alcohol Abuse and Alcoholism was providing for studies that would examine alcohol abuse in the workplace. Although neither of us had experience in the alcohol or drug field, we saw this as an opportunity to carry out large multi-site and longitudinal studies on the workplace.

Thus began a long and fruitful collaboration with Ed Greenberg, first on a study that examined whether worker ownership and participation had a salutary effect on workers' alcohol use and abuse and more generally on their work and political attitudes (Greenberg and Grunberg 1995; Greenberg, Grunberg and Daniels 1994) and then on a study designed to investigate the consequences of the ongoing workplace changes that seemed to herald a new kind of economy. Large companies were now creating insecurity among workers not only by their international mobility, but also increasingly by reshaping and reorganizing themselves when they downsized and restructured. Once again, workers, including those with high-level skills, were subjected to the effects of remote, large, economic forces and organizational decisions. Working with Ed Greenberg and with Sarah Moore, a psychologist who joined our research team a few years into the project, has been a wonderful learning experience for me. Our collaboration has reinforced my belief in the artificiality of disciplinary boundaries and of how impossible it is to fully understand the lives of workers without examining the political and family contexts in which their lives unfold. It also confronted us with the temptation to justify our funding by bending over backwards to corroborate our guiding hypotheses and to find evidence of large effects of workplace change on workers' wellbeing. While modern statistical computer programs enable us to do some quite amazing statistical manipulations, there is an intractability to systematically collected data that resists the most ardent

wishes of the researcher. After tracking the responses of several hundred workers who had been subjected to repeated waves of downsizing and pervasive uncertainty for several years, we came to realize that while these organizational changes had scarred many of these workers, the deleterious effects tended to be small and seemed to have complicated etiologies (Grunberg, Anderson-Connolly and Greenberg 2000; Grunberg, Moore and Greenberg 2001).<sup>4</sup> From the vantage point of almost thirty years of research on workplaces and workers, I now appreciate more clearly that work is but one of many sources of pain (and joy) and that individuals can be remarkably resilient and psychologically resourceful in the face of chronic and acute sources of work stress. This in no way absolves us of the responsibility to expose the causes of these stressors or of the obligation to search for more humane ways to organize our work.

It is always tempting to see the current era as a revolutionary or transitional one, and sometimes scholars can magnify the significance of current changes when in fact change occurs continuously throughout history. But I don't think it's an exaggeration to say that economic organizations and workplaces are being significantly reshaped today. New technologies, the integration of China and India into the world economy, the breakdown of the implicit psychological contract in many advanced economies (that traded employee loyalty for life-long job security), the trend towards "hollowed out" companies (with less and less work being done in house and more being subcontracted out), as well as the massive entry of women into the labor force, all indicate a fundamental transformation in the world of work. These are therefore exciting times for young researchers willing to pursue studies of the middle ground, with research that is empirically grounded but not overly constrained or blinkered by current orthodoxies. When there are new facts on the ground, theoretical flexibility and an open mind are most likely to produce analytic breakthroughs.

Writing about myself and the factors that have shaped my interest in sociology has raised nagging questions in my mind. Have I done enough interesting work to merit such personal indulgence? Am I sufficiently self-aware to identify pivotal events or influences? Can any individual know with any degree of confidence what influenced his choices and his life course? Did I consciously or unconsciously elide unpleasant facts? Was my memory overly selective and inaccurate? Are there any useful lessons in my story for others contemplating careers in sociology? I also struggled with the temptation to fit all the pieces into one coherent and settled whole, to tell a tale that hangs together, with a few dominant themes that give shape, continuity and meaning to the story. But I don't think that would be an accurate representation of the course of my life or career, nor for that matter of most people's lives or careers. Chance, coincidences, accidents of family background, our geographic and sociological location and of course historical events, all create the constraints and structures that help form us and within which we exercise our will and make our choices.

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4 A complete list of our work can be accessed at <http://www.colorado.edu/ibs/PEC/workplacechange/>.

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