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THE LONG TWENTIETH CENTURY IN AMERICAN SOCIOLOGY: A Semiautobiographical Survey*

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■ **Abstract** In this essay, I draw on a professional life history to suggest how sociological knowledge is generated by encounters with changing research opportunities, here called targets of opportunity. In my case, a study of rural communities led to unanticipated conclusions concerning buffering mechanisms that protected authorities by absorbing dissatisfactions and rebellions. Wartime research in a military setting identified sources of group solidarity and effective performance under stress. Major societal changes in racial/ethnic relations provided opportunities to develop new concepts and empirical findings. Synoptic studies of post–World War II American society led to extensive research on values and institutions. These macrosociological analyses of ethnicity and social systems, in turn, led me to a new sociology of war and interstate relations. I also offer here some critical reflections on recurrent issues and chronic controversies in American sociology. Final sections of the review deal with the continuing search for conceptual clarity and cumulative knowledge. I note the obstacles of disciplinary fragmentation, but my closing judgment is that sociology now has the base of substantial scientific knowledge and methodological expertise necessary for investigating crucial twenty-first century problems.

By virtue of the editors' generous guidelines, this essay is freed from restraints and can be partly autobiographical—a mode of discourse that all too easily can become both self-serving and misleading. In the present instance, however, some safeguards reside in the fact that the author's involvement in American sociology began in the 1930s and continues now some seven decades later. Thus, biography and history are thoroughly intertwined. Sociology today, of course, differs in many remarkable ways from what it was in the depression years just before World War II. In its organizational embodiment it has moved from a small scholarly organization (the American Sociological Society) to an extensive professional association [the

*Historically minded readers should be assured that no close parallelism with the Long Nineteenth Century is intended or implied.

American Sociological Association (ASA)] with a membership of over 14,000 and 26 full-time staff, organized into more than 40 specialized sections.

The 1950s name change, along with an increase in dues, were vigorously opposed at the time by sociologists who wanted the society to remain a collegial body with minimal part-time staff. The traditionalists feared what they saw as a change from a small village to a bureaucratized city (and I do not exaggerate). As a participant observer of this history, I try here to capitalize on an insider's perspective (and on the implied license due to the antiquity of the author) to illustrate how individual and societal contexts regularly provide cues—what I call targets of opportunity—for generating sociological knowledge. Thus, the autobiographical account is intended to provide a context of discovery while keeping primary attention focused on the context of justification—the scientifically justified claims that sociology can make (McClellan 2005, p. 11).

In his prefatory chapter for the *Annual Review of Sociology*, W. Richard Scott (2004, p. 1) gave a 50-year overview of the development of organizational sociology, noting “the broadening and deepening of theory and the widening flood of empirical studies.” That characterization appropriately serves as an overview of many other specialties within the wider discipline. In the present chapter, I try to describe a few research contributions and scientific and professional issues in the subject areas of my own involvement.

Although I was unaware of it at the time, my first professional experience (becoming a rural sociologist) illustrated a major pattern in the institutionalization of sociology. Rural sociology began as a diffusely defined subspecialty within departments of agricultural economics (early exceptions were the University of Wisconsin and Cornell). Similarly, general sociology tended to emerge from departments of economics, social welfare, social ethics (as in Harvard), and the like. At Cornell, for example, Walter Willcox initiated sociology by teaching both ethics and demography.

More dramatic, and quite successful, was the establishment of whole new departments of sociology. The field developed rapidly, from the first course in sociology given by William Graham Sumner at Yale in 1875 to the first Department of Sociology at the University of Chicago in 1893. The American Sociological Society was founded in 1905. Rapid institutionalization was favored by mass education, a decentralized university system, relative freedom of inquiry, and public interest in social issues arising from social change and cultural diversity. Sociology also developed, in part, because its practitioners were willing to deal with subjects that had been ignored or dismissed by established disciplines: social problems such as crime and delinquency, poverty, stratification, immigration, assimilation, ethnicity, family, and recreation in rural and urban communities.

The field also drew upon an intellectual heritage that included not only Comte and other Enlightenment thinkers but, importantly, also the early British demographers (John Graunt and Thomas Malthus), the Scottish moralists (Adam Ferguson and Adam Smith), and of course Herbert Spencer and Karl Marx—in short, the European pioneers. But the field quickly took on an empiricist cast as it sought

relevance in an urbanizing and industrializing society. The discipline initially met with considerable hostility and resistance as a new field, but acceptance in some major universities helped it to create the momentum to gain wider legitimacy. As I noted some time ago (Williams 1976, p. 97):

The cumulative development of the field occurred even though sharp separation of disciplines in American universities led very early to the removal of sociology from the close connection with law, philosophy, and history that existed in European universities. At the same time, the separate organization of schools of medicine, law, nursing, and social work militated against integration of sociology with professional and paraprofessional training. And the need to establish credentials as a science led many sociologists to reciprocate the rejection they experienced from the humanities and arts (cf. Reiss 1968, p. 4). Yet a permanent organization within the universities was achieved and was indispensable.

As of the year 2005, sociology in the United States has a century of development as an organized academic discipline. Now is thus an appropriate time to ask what that history tells us. Do we illustrate the dreary proverb that what one learns from history is that people do not learn from history, or rather do we learn that we learn entirely too much from history of inadequate or erroneous formulations? The account that follows will not be able to give satisfying answers, but the puzzling question surely deserves continued examination and dialogue. In that spirit, this review follows the unconventional dictum that if a thing is worth doing, it is worth doing poorly rather than not at all.

A first major question raised by the history of the discipline is, what changes have occurred in the Big Issues that have occupied center stage over the decades? Are there, in fact, consistent core interests or do dominant themes wax and wane, becoming submerged only to resurface at a later time? We may recall that Pitirim Sorokin famously decried what he called the Columbus Complex—the repeated rediscoveries of forgotten or neglected knowledge from the past, or the repetition of old errors in new guises. As the old quip in national political circles once had it: does the dragon slain on Friday breathe fire again on Monday? And a second query follows easily from the first: To what extent does sociology produce cumulative knowledge? Does each sociological generation know more and know it more reliably and systematically than preceding cohorts? If indeed there is cumulation, in what areas of interest has it occurred and how and when?

THE PROFESSIONAL AND THE PERSONAL

One of the great rewards of sociological curiosity is the recognition of pattern similarity among apparently disparate events in far-removed settings. Let us examine some, perhaps now commonplace, examples of such episodes of illumination. In his prefatory chapter for the *Annual Review of Sociology*, for example,

Robert Merton (1987, p. 1) pointed to the importance of what he called strategic research materials—strategic research sites, objects, or events that offer especially fruitful opportunities for uncovering important knowledge and posing new questions. My experience supports this observation and adds that strategic research materials often present themselves as targets of opportunity, strategic points where research for one set of purposes turns out to be productive for quite another use.

In my own work, the first of these targets of opportunity developed in a study of land-use planning committees that was carried out as a project in rural sociology at the University of Kentucky's Agricultural Experiment Station. The main purpose of the study was to identify factors in the successful or unsuccessful functioning of communities of local farmers, which had been activated by the Agricultural Extension Service as advisory bodies in agricultural programs. As part of surveys in several counties, information was gathered on farmers' attitudes toward the crop reduction programs of the Agricultural Adjustment Administration. Each grower of a crop (in this case, tobacco) was allotted a certain reduced acreage, based on past usage, and in return received government payments. Thus, the acreage allotments of a primary cash crop were crucial for the income of many farmers.

How were these allotments to be determined? At the local level, they were assigned by a committee of farmers under the leadership of the County Agricultural Agent. The program aroused resentment and protests from individuals who felt their allotments were unfair. Our interviews showed that blame was usually directed toward the local committee. But, in fact, the committees operated under strict guidelines set by the Agricultural Adjustment Administration in Washington. Quotas were assigned to state boards that, in turn, gave quotas to counties. Only rarely did aggrieved farmers blame the state or national officials or the federal government. Instead they vented their resentment against the known, visible, local target, "That Committee."

The committee indeed had the immediate responsibility for imposing differential sacrifices upon their fellows. In each case, the legal authority for this work came from a remote, national, bureaucratic organization. But the most tangible and visible source of unwanted decisions was the local committee or board, composed of locally known persons. Typically, resentment and outrage focused upon these people, who could justly claim that they were "only following regulations." If an aggrieved person wished to carry complaints to higher authorities, he or she found an intimidating array of intermediaries (county, state, federal) and an intricate, complex, and confusing set of definitions and rules. Few would have the resources or temerity to negotiate the mazes. It thus became clear that the local committees served as a buffer between the angry citizen and the vast impersonal bureaucracy at the national level.

I frequently observed the same phenomenon later during World War II, when the draft operated through local selective service boards that decided who would serve in the military, often literally a life-or-death matter. The same phenomenon also appeared in local committees that rationed scarce goods such as sugar and gasoline. These allocations were generally accepted as wartime necessities, but

individual distress and resentment were not uncommon. Who was blamed for perceived favoritism and other unfairness? Typically the visible and vulnerable local committees, or particular members thereof.

In this example I was able to identify an unanticipated social mechanism—the absorbing function or buffer role of co-opted local groups—as a result of a valuable target of opportunity. One had only to note this pattern to think immediately of the mechanism of indirect rule by classical empires—and so on to many new questions. In reflecting upon what was to me a new insight, I recalled similar arrangements in many other contexts; thus, the field offices of regulatory agencies, subject to the immediate rewards and penalties available to the objects of regulations, not only served as buffering mechanisms but were subject to capture by those whose activities were to be regulated—as shown in David Truman’s (1971 [1951]) study of the Chicago offices of the Department of Agriculture that were charged with regulating the meat-packing industry.

Once sensitized to this pattern, one is likely to note similar processes in other quite different realms. In the Cornell Studies in Intergroup Relations, Melvin Kohn and I observed numerous instances of what we called the exemption mechanism, which takes the form of exempting particular persons from racial/ethnic stereotypes—“some of my best friends are [X’s]”; “They are all lazy, but Jim is a hard worker” (Kohn & Williams 1956). Of course, it is now generally noted that such exceptionalism is manifest in the “token” woman CEO and in other arenas.

A review of these instances suggested that exemption and buffer patterns have curious affinities with scapegoat mechanisms. In the late 1930s, Wilbert Moore and I had coauthored an article in which we identified the overseer in Southern slavery as a key focus for blame (Moore & Williams 1942). Defenders of slavery often attributed the “evils of the system” to the bad behavior of the overseer. A similar pattern is found in the odium directed to the drill sergeant in the army, to the bad prison guard, to the atypical policeman, to the president’s evil advisers. Individual blame turns out to be a pervasive means of maintaining larger systems of authority, while providing outlets for criticism or reform.

THE SOCIOLOGY OF MILITARY AFFAIRS

Another substantive area of sociological research was called forth by the demands and opportunities of wartime, and my sociological explorations took a radically different direction with the advent of World War II. One of the perennial Big Issues within sociology has always been the role of applied research in its several forms. A salient case in point was the wartime research done by social scientists in military settings. In World War II, a large-scale research enterprise was carried out by the Research Branch of the Special Services Division of the U.S. War Department, an organization staffed by psychologists, sociologists, and other social scientists.

As the overseas work of the branch expanded, I was assigned to the European sector, working first on surveys of Air Force and Army personnel in Great Britain,

and then after D-Day moving to Normandy, from whence I accompanied infantry units through Belgium and into Germany as a combat observer. That close acquaintance with the hardships and dangers of frontline soldiers led to observations and insights that were recorded in part in Chapters 2 and 3 of *The American Soldier* (Stouffer et al. 1949a, pp. 59–101). Later critics who condemned that work for what they called disengaged statistical abstractions ignored the detailed direct involvement and observation that provided indispensable interpretive context for the survey findings (see Williams 1989).

The Research Branch's mission was to serve the U.S. Army by providing information to support its educational, training, and organizational objectives. It was under intense demands from day to day to facilitate the war effort, not to conduct scholarly studies. In that setting, research had to balance strong competing claims while dealing with bureaucratic complexities and experiencing considerable resistance from some military quarters. Applied research in this case consisted of immediate, quick and dirty data collection and analysis related to practical problems. The resulting findings provided only the foundations for later scholarly work. An unanticipated research site turned into a unique target of opportunity for gaining sociological knowledge.

From small beginnings the Research Branch eventually carried out over 250 studies, many of them in overseas and combat zones. In the overseas operations, research was often done under difficult and dangerous conditions and always under relentless time pressure. One survey in the European theater of operations during the winter of 1944–1945 gathered information from troops in four infantry divisions then engaged in combat and produced an analytical report within a period of a few weeks. No Ivory Tower there.

One sociological contribution of these efforts was to document and analyze in detail the importance for military morale and performance of particular organization structures and how these structures influenced interpersonal relations and group cohesion. Effective combat behavior was found to be sustained primarily by compliance with authoritative norms in a cohesive social organization. It was not official ideology or hatred of the enemy that proved most important in maintaining combat effectiveness, but firm organizational support and strong peer-group attachments.

Any inventory of what has been learned from studies of military affairs since the start of World War II would be encyclopedic in scope (Lang 1972, Caforio 2003). Obviously a full review cannot be undertaken here, but such research has made substantial contributions in nearly every major area of sociological interest. Examples include reference-group influences, relative deprivation, formal organizations, racial and ethnic relations, authority and leadership, military-political relations, informal organization, technological change, conformity processes, types of collective violence, conflict management, and peacekeeping.

Numerous popular stereotypes were challenged by the findings of World War II research, as noted in Paul Lazarsfeld's (1949) classic article on "The American Soldier." In response to the stereotype that "military organizations are rigid and

unchanging,” the research record proved otherwise. Likewise, many believed that “the military is politically reactionary,” but the U.S. military accepted and facilitated racial integration far beyond what civilian society at the time was willing to accept (see Lang 1972).

Military organizations—social formations engaged in the management of violence and the threat of violence—have to be capable of functioning under conditions of extreme instability and stress. To understand such organizations, one must see how they function under conditions of imperfect information, extreme unpredictability, depleted resources, lethal attacks, and other sources of severe strains upon group cohesion and individual endurance, resilience, and adaptability. “The fog of war” is a central reality.

The importance of specifying the context of sociological generalizations is widely accepted but not always made explicit. For example, a plausible generalization from World War II studies is that “small-group cohesion is an essential condition for sustained effectiveness of combat troops.” There is abundant evidence supporting this proposition. But inspection of the context of the evidence shows that it has been drawn primarily from studies of infantry, especially rifle companies. These are settings that highlight the positive effect of group cohesion. In other combat units, such as artillery crews, the weaponry requires group activity, so that default is not a real option.

Anything approaching a full account of combat behavior would have to include ideology and military policies (such as length of service and retention). The Vietnam experience (Moskos 1973) differed in considerable measure from that of infantry in World War II, and comparison of findings across quite different settings refined and expanded earlier work. The verdict is that cohesive primary groups in military formations can provide strong support for individual performance, but the influence may be either supportive of or in direct opposition to the goals and norms of higher authorities. No decisive predictions can be made until we specify the context of coercive authority, legitimacy, and technological and environmental constraints.

Sociological research on the military done since World War II has produced an impressive accumulation of such empirical specifications. In addition, comparative studies have identified major conditions favoring military intervention or dominance in national politics (Janowitz 1960) and have shown how extremely difficult it is for conventional military forces to overcome guerrilla activities when insurgents are embedded in a supportive civilian population. This lesson may not have been fully assimilated, even by 2001, by some political and military leaders.

A SOCIOLOGY OF ETHNICITY

The study of military affairs provided an unusual opportunity for basic sociological inquiry within another subfield not usually associated with applied research or military topics: race and ethnic relations. The results appeared as a chapter, “The

Negro Soldier,” in Volume I of *The American Soldier* (Stouffer et al. 1949b). Large-scale surveys of troops in training and combat and field observations documented black soldiers’ intense awareness of the contradiction between American racism and the official creed of a war for democracy. Their feelings of moral outrage were prevalent long before the Civil Rights movement of the 1960s.

Many sociological findings contradicted conventional beliefs about the willingness of black soldiers to fight. Neither acquiescence to segregation nor passive acceptance of discrimination increased the will to fight, but strong commitment to resistance against the System was the hallmark of those more highly committed to the fight against fascism. The more militant and antiracist that black soldiers were, the more willing they were to enter combat. The combat effectiveness of black volunteer infantrymen in the European theater thus contradicted prevailing stereotypes.

My experience with studies of black soldiers in World War II opened a career-long interest in race relations and ethnic conflict. The turning point was my assignment, under the auspices of the Social Science Research Council, to review research and theories on what was then called intergroup tensions. A search of the research literature together with my interviews of leaders of racial, religious, and ethnic organizations resulted in a monograph, *The Reduction of Intergroup Tensions* (Williams 1947). The centerpiece of that report was an inventory of some 101 propositions (hypothesis, empirical generalizations, research questions) that were intended to bring order to a scattered set of concepts and findings. Before then, research had used diverse definitions and was heavily weighted toward social-psychological studies of individual attitudes, focusing on prejudice, stereotyping, hostility, tension, and the like, with little attention to cultural and social-structural factors.

By the mid-1960s, however, a decisive shift within sociological research had occurred, and intergroup tensions came to be seen as outcomes of systemic political, legal, and economic structures as well as of cultural factors. Race relations thus became part of a more inclusive concept of ethnicity. As terms were defined more exactly and explanatory models were improved, new insights emerged. Tracking ethnic relations over time showed that the effects of independent variables, such as the size and number of ethnic groups within a given polity and their relative status ranking, depended on their place in a sequence of processes or events. Sequence could thus act as a major contextual factor.

Several decades of engagement with ethnic research has convinced me that different approaches are needed for different time spans and different levels of investigation. In the short-run and for microlevel contexts, the research methods that have proved most effective include participant and nonparticipation observation, sample surveys, program evaluation, and both field and laboratory experiments. For longer run, macrolevel problems, however, there is no way to avoid the necessity for comparative and historical studies, as the very meaning of race and ethnicity varies radically across different societies and across historical periods within the same society. Comparative historical studies of ethnicity have revealed its deep embeddedness in kinship, descent, and locality.

For a long time sociologists doing research on ethnicity had paid little attention to socio-biological or bio-sociological evidence and theorizing [cf. van den Berghe's (1987, 1990) incisive critiques], but cumulative knowledge showed this neglect could not be sustained. Ethnicity, rooted in kinship, is an enduring aspect of basic social structures, as are territorial communities. My recognition of this central condition later guided the studies summarized in *The Wars Within* (Williams 2003). Meanwhile, the broader context of changing events in America and the world insistently called for attention. The outcome was a new focus on the sociology of values and institutions—for me, a new target of opportunity.

A SOCIOLOGY OF VALUES AND INSTITUTIONS

My successive life course moves from rural sociology to military sociology to ethnic studies did not constitute an obviously natural or logical sequence. But the successive foci of attention are understandable under emerging societal conditions that produced different targets of opportunity. The constant aim was to understand important social processes that at different times played out in radically different contexts. Even as ethnic relations continued to engage a substantial part of my research, changes in the post-World War II United States stimulated a new set of interests in the comparative study of values and institutions. As this interest developed, it led directly to a macrosociology of societies.

The shift began with a fresh examination of basic concepts such as values and institutions. But the broader context for my interest—seen only in retrospect—was the postwar climate of optimism for the development of democratic values and a heightened sense of a new international world. From 1946 to the early 1950s, I taught a course on “American Society” at Cornell, which led me to reconsider such major concepts as role, status, norm, value, and institution. This conceptual work was intensified by participation in a multi-university program, sponsored by the Social Science Research Council, on individual values. The Cornell Values Study Project involved faculty members from philosophy, anthropology, economics, psychology, and sociology, and over many intensive sessions this group demonstrated the value of an interdisciplinary examination of the truth-claims and the complexity of meanings implicit in such an evocative concept as “value.”

A SOCIOLOGY OF INTERSTATE RELATIONS AND WAR

By the time of *Mutual Accommodation* (Williams 1977), many phenomena of intergroup cooperation and conflict could obviously be best understood by comparisons across periods and cultures. Many of the generalizations encountered in the literature of the 1960s and 1970s seemed to me overly localized and dated. Meanwhile, my participation in the Peace Studies Program at Cornell provided an opportunity to learn about international affairs from a diverse array of political scientists, economists, physicists, engineers, military strategists, and peace activists.

By 1981 I had published an article on “Resolving and Restricting International Conflicts” in *Armed Forces and Society* (Williams 1981). In the context of the rising tensions with the Soviets and the Reagan administration’s harsh rhetoric against “the evil empire,” I also tried to analyze the use of threats in the Cold War, which led to a 1986 chapter in *Research in Social Movements, Conflicts and Change* (Williams 1986). In it I showed the many ways that then-current political processes were increasing the stakes and arguably raising the likelihood of nuclear war.

Starting in 1981, I began to develop and teach a course entitled “Sociology of War and Peace,” which required my immersion in an immense body of writings on this subject—a literature dominated by historians and political scientists and marked by the relative absence of sociologists. That review helped a novice sociologist to become familiar with the sharp disagreements among experts on major issues. At one pole were the so-called Realists whose most fully developed schemata posited an interstate system composed of unitary national states represented by decision makers guided by national strategies that sought to increase or maintain power primarily by coercive means (Mearsheimer 1990, p. 4). This model was somewhat modified by neo-Realists who held that states were moved by the search for security, including economic considerations, in addition to the drive for military ascendancy.

Opposing the various versions of Realism were scholars who were prepared to admit ideas, culture, shared interests, and interstate organization into the analysis, and who often emphasized interstate networks of trade, migration, and cultural exchange (e.g., Keohane 1986). Scholars were attracted by the apparent clarity and rigor of the Realist model that rested upon a parsimonious set of assumptions and that generated deductions with a good fit to important historical cases. But how could a sociologist accept a scheme that ignored the sources of the drive for power, minimized the influence of beliefs and values, and denied the relevance of interstate norms and social institutions?

A massive array of questions stimulated me to develop conceptual orders and analytic schemes that could modify, extend, or partially refute the Realist claims. What of imperfect knowledge, lies, misinformation, ideological convictions, and contradictory goals of decision makers? What about unintended consequences and self-defeating prophecies? How to explain mutually disastrous arms races? To what extent were states unitary actors? Could revolutions and civil wars and intrastate ethnic conflicts be adequately analyzed in Realist terms? Could states be regarded as the only consequential actors in large-scale collective conflicts? Or was it more important to analyze the part played by networks of economic enterprises, religious collectivities, ethnic formations, and scientific or cultural organizations?

Armed with an awareness of contradictory theories of war and peace, I wondered how ethnicity could be brought into a field preoccupied with states and systems. A review of relevant research led to a 1994 chapter in *Annual Review of Sociology* (Williams 1994) that, in turn, led to work that finally produced *The Wars*

Within (Williams 2003). While this book was being developed, world events were illustrating its major themes, such as the prominence of civil wars and other ethno-political conflicts, the growing and often decisive importance of nonstate actors and interstate networks of ethnic and ethnoreligious contenders, the development of numerous organizations devoted to conflict management, and the massive toll of death and destruction within the claimed boundaries of national states.

BIG ISSUES AND CHRONIC CONTROVERSIES

While the decades of this story passed, what were some noteworthy developments in the wider field of American sociology? Sociologists are well aware of ebbs and flows of preferred topics and forms of discourse; such fluctuations have been prominent in the history reviewed here. A striking example is the recurrent attempt to bring a favored perspective back into primary focus. For example, Homans called for “bringing men back in” in response to an alleged overly abstract conceptualization of social actors; Skocpol eloquently urged “bringing the State back in,” implying that it had been missing or neglected; later, advocates of the Cultural Turn wished to bring culture front and center, implying its past neglect or inadequate conceptualization. There are many other examples of such favored factors—firms, technology, biology, geography, class, markets, gender, and so on.

Debates about these efforts raise important questions about scientific priorities—what processes and structures warrant major investments of sociological resources? But surely none of the allegedly missing factors actually went away by being wholly ignored. For example, the concept of culture was strongly influential for the generation of sociologists who welcomed Ralph Linton’s (1936) *The Study of Man*, as well as for their seniors who had used Park and Burgess’s popular introductory text. Many later debates would have seemed strange to those who were introduced to graduate study by Pitirim Sorokin’s (1928) *Contemporary Sociological Theories*, a wide-ranging work that described the characteristics of different schools of thought and which chose to explain social behavior in terms of different master factors or variables.

In their most far-reaching versions, one-factor theories represent proverbial tunnel vision: Total explanation is sought in genetic and evolutionary factors, in geographic and biological environments, in a rational choice theory “without black boxes” (cf. Boudon 2003), in culture, in symbolic and social construction, in social interaction (relationships, structures), in psychological process, and in other familiar master factors. An encyclopedic sociology would have to include the full range of factors, but in addition to the obvious limitations of individual sociologists, such an inclusive field is unlikely to have the conceptual rigor or focused empirical base that is necessary for coherent explanation. The risk of a drift toward vague ecumenicalism must be counterbalanced against the benefits of incorporating data and concepts from geography, biology, economics, and other sciences to develop increasingly comprehensive explanations of human social behavior.

Another of the much-debated Big Issues appears in perpetual discussions of research methodology. In my 1958 ASA Presidential Address (Williams 1958, p. 622), I optimistically suggested that with respect to research methods there was “a growing tendency to take a rationally pragmatic position” in choosing those methods best suited for particular problems. That appraisal was advanced in the hope that it would prove to be a self-fulfilling prophecy. Subsequent developments did not fulfill that anticipation, however, for sharp differences in methodological preferences and commitments still remain.

Room for optimism is nevertheless provided by twenty-first century advances in research methodology, empirically based theory, substantive findings, and conceptual developments. But my 1958 overview did not foresee the discipline’s later fragmentation and the reemergence of several basic theoretical controversies, an alleged lack of cumulation, and a new polarization around questions of advocacy and public policy. In these controversies, a core commitment to a genuine science of society became less evident and the central disciplinary focus grew less clearly defined than it was a half-century ago. I still believe, however, that some convergence of views may still be evolving.

Perhaps continuing specialization and the emergence of new clusters of interest are expectable and inevitable and therefore do not warrant the term “fragmentation.” Of the ASA’s 44 special interest sections, many surely are oriented to the advancement of a cumulative scientific field. Yet the question still remains: To what extent do the specialties contribute to a central, recognizable body of well-established sociological knowledge?

Still another persistent methodological debate is over the merits of direct observation and fieldwork compared with other research approaches. Plentiful examples suggest the virtues of observational and other qualitative studies in identifying objects of interest, inventing and modifying concepts, proposing hypotheses, suggesting research tactics, providing dense descriptive contexts, revealing anomalies and limitations in prior generalizations, and helping to guide theoretical developments. The history of the last century of sociology, nonetheless, demonstrates that powerful quantitative methods can be developed, applied to, and go beyond the initial findings of qualitative work. Several generations of intensive research have created a vast accumulation of increasingly effective statistical methods and study designs. Compared with the situation a generation ago, the array of advanced statistical methods now available represents a major growth in potential analytical power (for the case of panel models, see Halaby 2004).

Some of the unsung heroes of sociology have been researchers who located sources of information, compiled observations, organized and classified data, and assembled datasets in accessible form. Examples include the recovery of usable historical data from past census materials, as well as the critical assessment and organization of census data and other published information. Analysis of political affairs has been notably advanced by large datasets, ranging from voting to collective violence, revolutions, and wars. Newspaper event data have been compiled in comprehensive sets, and documents from the French Revolution have been fruitfully content analyzed by Shapiro & Markoff (1998).

In *The Wars Within* (Williams 2003, p. xii), I wrote that “the practical methodological motto is: use what works best for the problem at hand. By ‘works best’ I mean those concepts and procedures that most clearly produce reliable knowledge that can be checked and refined by criticism, replication, and new data.” That statement may seem bland eclecticism, but it answers to the long history of sharply opposed dicta in the fabled strife over methodology. The quotation reflects research experiences that have led to an inclusive but selective view of methodological choices. Skill in choosing methods and a practical knowledge of the options are the effective tactics in good research.

Many years of interviewing hundreds of diverse people—welfare clients, farm tenants and landlords, government officials, army officers and enlisted personnel—has taught me the merits and pitfalls of getting data by asking questions. Valid and reliable data can be generated, but methodological safeguards are essential. A case in point is the World War II experience of large-scale survey research, which was combined with interviews and participant observation and was rich with methodological problems and innovative solutions. Studies used military settings for the development and refinement of several research methods, such as attitude scaling, experimental designs, participant observation, questionnaire design and evaluation, and field experiments measuring interviewer effects. These studies developed a standard procedure of pretesting before the implementation of large-scale surveys, based on direct observation, intensive interviewing, and pilot testing of questionnaires.

A good example is the detailed field trials of personal interviews versus group-administered questionnaires that were done in studies of black and white soldiers. The design compared the effects of the two methods, cross-classified results by whether the interviewers or group supervisors were officers or enlisted men, civilians or military, and black or white. Racial identity produced large effects in face-to-face interviews but had little effect with group administration of the questionnaires. No significant differences were found between group sessions led by civilians or enlisted men. Consequently, one could use both white and black civilians to administer questionnaires to black troops (Williams 1989, p. 160). This multi-method approach was one of many practical solutions to methodological questions considered during the war, which was also the setting for Louis Guttman’s (1944) development of innovative methods for attitude scaling, work with substantial influence on later models for analyzing attribute data derived from survey research. Different research tasks called for new methods.

In contrast to these context-specific research tactics, comparative historical analysis (Mahoney & Rueschemeyer 2003) is uniquely suited for the investigation of large-scale and slow-moving processes (many of which are practically invisible in the short run), such as linguistic shifts, demographic fluctuations and cycles, and even geographical and climatic changes. Long-run technological changes have profound societal consequences, as do evolutionary sociobiological processes. We are earth-bound creatures, shaped by cosmic forces. Fortunately, there are always a few sociologists who take on the daunting tasks of analyzing long-range processes and large-scale events and structures—the rise and decline of civilizations and of

societies, the development of states and state systems, the causes and consequences of revolutions, wars, societal collapse, and massive cultural changes. Their analyses are an intrinsic part of the sociological enterprise.

THE CONTINUING SEARCH FOR CONCEPTUAL CLARITY¹

In common with other social sciences, sociology has often advanced by reason of developing sensitizing concepts, the value of which partly lies in the evocative effect of simple and vivid formulations. Many important phenomena can go unremarked and unreceiving of special attention until someone gives them a persuasive label, which stimulates new lines of thinking and investigation. Thus, the concept “marginal man” focused new attention on problems of ethnic identity, membership, and loyalty, and “relative deprivation” helped to identify and explain many otherwise paradoxical and puzzling social facts. Similarly fruitful was the idea of reference groups and reference categories, and the earlier coinage of the “definition of the situation” turned out to be a perennial source of productive insights.

Words taken from ordinary language carry “stars of meaning”—they radiate in many directions. We cannot do without them, but they often are unreliable tools for clear exposition. My first foray into the thicket of received concepts was an effort to unpack notions of marginality in rural population studies. One obvious signification came from economics: If one assumed that individuals were solely remunerated by the marginal product of their labors, disabled people and unproductive elders would simply starve to death; in fact, this sometimes does happen in extreme famines, but usually nonproducers are maintained through social ties. So, discussion of marginal rural populations needed a sociological dimension. A second important meaning was that of cultural marginality, as formulated in the classical concept of the marginal man. Still other meanings emerged from close inspection.

That excursion was a useful exercise for my later research on ethnic/racial relations. In the field surveys analyzed for *Strangers Next Door* (Williams et al. 1964), we found it necessary to subdivide the notion of prejudice into four main components: (a) negative or positive stereotyping of characteristics attributed to outgroups, (b) feelings of personal liking or disliking, (c) attitudes of social distance, (d) attitudes toward public policies (e.g., segregation). These distinctions clarified many problems of analysis. The global conception of discrimination similarly was found to cover an array of distinct patterns of behavior.

Consideration of ethnic relations required that we dissect the concept of conflict, a term variously used to refer to psychological dissonance or tension, incongruity of meanings, incompatibility of beliefs, opposition of interests, competition,

¹In an elegant appraisal of Robert K. Merton’s life work, Gerald Holton (2004, p. 506) has said “. . .one might well start with the observation that for the scholar in the social sciences or humanities, powerful concepts are necessary tools.”

controversy, rivalry, disagreement, debate, games, fights, wars. Once again, such diverse and elastic connotations called for careful specification. For purposes of describing and explaining social conflict, I settled upon this formulation: “[S]ocial conflict consists of interaction in which one party intends to deprive, control, injure, or eliminate another, against the will of other. Pure conflict is a fight . . .” (Williams 1970a, p. 218). Although still imprecise, this definition proved to be very useful for several decades.

Other examples of this kind are not hard to find. What is noteworthy is that such conceptual innovations are not mere neologisms or multiplications of jargon but useful tools in the search for new knowledge. So Granovetter’s (1973) imagery of “the strength of weak ties” highlighted a whole area of innovative research and reconceptualization, just as Charles Perrow’s (1984) concept of “normal accidents” became part of the general discourse on public policy.

Nonetheless, it is evident that some vague, ambiguous, and misleading concepts are so firmly established in social science as to survive repeated demolitions, rendering futile efforts to replace them. One conspicuous and curious example is that of nation-state, which I have elsewhere called a “semantic monstrosity” (Williams 1994). Clarification of the notion can be found in Charles Tilly’s (1992) distinctions among states, national states, and nation-states. Most large-scale national states do not have the cultural homogeneity and self-conscious identity of nations or nationalities. The claim that state must be coterminous with nation (exemplified in Hitler’s *Ein Staat, Ein Volk*) lent itself to political doctrines culminating in ethnic cleansing and genocide.

In a prolonged effort, beginning in the late 1940s, some sociologists attempted to explicate and assess critically the multi-referential concept of values. Although the concept had earlier been rejected by sociologists who regarded values either as epiphenomenal, unknowable by scientific means, or just impossibly vague, I found the concept quite helpful in my work on American society, but only when crucial distinctions were being made—between beliefs and values, and between the evaluation of objects of regard as opposed to the criteria used to make preferential judgments. These distinctions shaped my own definition of values as “those conceptions of desirable states of affairs that are utilized as criteria for preference and choice or as justifications for proposed or actual behavior” (Williams 1967, p. 23). That attempted clarification has not reached current political controversies that use and misuse the language of values.

In conceptual work of this kind, very simple devices can have powerful uses. The commonplace fourfold table deserves respect. For example, it helps to resolve scholarly arguments about whether or not a particular collective conflict is or is not ethnic. The debate confounds two distinct meanings: (a) whether the parties in the conflict were ethnic or (b) whether the issues or stakes in the conflict were ethnic. A fourfold table quickly came to the rescue (see Table 1).

Not surprisingly, many other global concepts consistently reveal multiple meanings that encourage divergent lines of analysis. One additional case in point is power. The literature of political sociology and political science contains many

TABLE 1 Ethnicity and collective conflict

The issues are	The contending parties are	
	Ethnies	Non-ethnies
Ethnic (language, religion, other cultural items)	Pure ethnic conflict	States and economic organizations in conflict
Non-ethnic (land, minerals, oil, forests, fishing grounds)	Many collective conflicts within states	Purely non-ethnic conflict

variations in use of the term, and discussions of international relations often involve the distinction between hard and soft power—hard power consisting of military force and other coercive means and soft power being composed of positive economic inducements, cultural contributions, national prestige, and other persuasive means. Analyzing the notion of power seems an obvious procedure, but it is necessary for assessing the extensive and complex literature that treats interstate relations and war produced by Realists, neo-Realists, institutionalists, game theorists, and other categories of scholarly analysis.

ON INVENTORIES, CUMULATION, DETOURS, AND CREATIVE DISORDER

Of necessity, sociology carries on all the main tasks that constitute the workload of science, identifying objects of regard, describing, classifying, measuring, correlating, predicting, offering causal explanations, interpreting, and developing descriptive and explanatory theoretical schemes. The workload of sociology illustrates all these tasks and their contributions. In attempting to establish empirical generalizations and seeking to develop well-supported theories, sociologists explored a line of activity that may be called propositional inventories. This line of work received a flurry of intense attention in the 1950s and 1960s before receding from favor. Part of its original impetus was to demonstrate that bodies of empirically supported knowledge actually did exist.

Thus Berelson & Steiner (1964) created a collection of findings from the behavioral and social sciences. Likewise, a massive inventory of propositions concerning kinship, family relations, and sexual behavior was developed by William J. Goode (1970). My own 1947 inventory drew upon several similar efforts by Arnold Rose and others. The list can be extended, but eventually professional attention turned to other pursuits. Why did this initially promising activity rapidly lose support? One reason was that the inventories stimulated few new inquiries or failed to open up new areas of research. Unlike interrupted tasks or incomplete games, these inventories seemed to be closed formulations that did not invite resolution.

In that respect, their fate bears a resemblance to the retreat from Grand Theory after the period in which Parsons's formulations occupied a central place. Many sociologists seemed to believe that codified research findings were no longer sufficiently interesting or productive of fresh insights and that comprehensive theoretical systems brought premature closure to a field still searching for theories of the middle range; or as Merton (1948) put it, sociology was not ready for an Einstein when it had still not found a Kepler. Nevertheless, both the effort to codify empirical knowledge and the effort to create unified theoretical schemes did derive part of their vigor from the vision of sociology as a cumulative science in which errors were exposed, false starts were rejected, valid knowledge was certified, and increasing fidelity was achieved between formulations and external reality.

That vision has been sharply challenged, of course, not just by deconstructionists and postmodernists but by a diverse roster of sociologists who reject the possibility of cumulation, prefer narrative accounts, regard knowledge as negotiated plausibility, or accept other forms of relativism. Sociology's treatment of cumulative knowledge reveals that contradictory views are an essential part of its character. At one extreme are those who hold social phenomena to be nonrepeatable or chaotic and science itself as a negotiated and rhetorical set of devices (cf. Turner & Kim 1999, pp. 8–9). At the opposite pole are those who hold that sociology should strive for propositions that have a certain and nonconditional relation to an objective external reality.

If all is merely a story, a text, open to an indefinitely large number of creative interpretations, that is the end of discussion. Science, then, is just a rationalized myth; without any stable uniformity, all thought of cumulative knowledge is illusory. If, however, we accept that there is a real social world that exhibits ascertainable uniformities, we can seek to establish empirically based knowledge that can indeed become cumulative. The remaining arguments then concern the extent (much or miniscule), and the scope—how restricted, narrow, or context-dependent.

An interesting aspect of the cumulation debate is that the evident fact of substantial cumulative knowledge is often socially unrecognized (Collins 1999, p. 41). Examples of the accumulation of well-founded generalizations, cited by Collins, exist in world-systems research, in studies of state breakdown and revolution, in experimental research on expectation states, in analyses of social control in organizations and in small groups, in studies of networks in social movements. It is not difficult to add other cases, such as studies of social conflict that have produced several hundred well-founded and nontrivial generalizations (Williams 1970b). Cumulative and rigorously derived findings have come from network analysis, an interdisciplinary field to which sociologists have made important contributions (Watts 2004, especially pp. 253–64).

There is no doubt that sociology continues to discover numerous nonobvious uniformities in the social world. An intriguing example is the remarkable number of social phenomena that exhibit substantively important curvilinear relationships and step functions. Three familiar cases serve as illustrations. An early example was the discovery of a curvilinear relation between negative ethnic prejudice and

religiosity (as indexed by membership and participation in organized religious activities). The curvilinear association resulted from the crisscrossing of influences on religious participation: the teaching of universalistic ethics is countered by the social conformity of participants who are embedded in congregations sharing traditional intergroup prejudices. In this way ethnic intolerance is maintained through the pressures of group conformity.

A second case comes from theories of social movements that link protest mobilization to the open or closed character of political opportunities, resulting in a curvilinear relation: low protest in a fully open structure and in a highly repressive system, with higher frequencies when dissatisfaction is joined with moderate opportunity. Under other specified conditions, then, open systems tend to direct protests into regularized political forms (Meyer 2004, pp. 128–29). Thirdly, lethal violence has a curvilinear relationship to the extent and kind of state centralization and intrusive societal control. Stateless societies and those with very weak states have high rates of deadly violence, but societies governed by highly centralized autocratic or totalitarian states have the very highest rates, whereas intermediate levels of state control are associated with low rates of lethal violence (Cooney 1997).

Such patterns emerge from examination of empirical generalizations. The same is true of the very different family of functions in which the relationship between variables abruptly changes when one of the factors reaches a certain critical level. A familiar example is the critical mass in social movements, observable when the number of adherents and the amount of resources reach a point at which the number of new participants rapidly increases as the costs of participation decrease and the likelihood of success rises (Williams 2003, pp. 172–76).

The study of tipping points has advanced the analysis of racial/ethnic segregation, e.g., showing that simple positive preferences can create massive clustering, even in the absence of negative prejudices. Threshold-effects are prominent in processes of social contagion when repeated exposures result in a rapid change in the probability of a new behavior (cf. Watts 2004, pp. 254–64).

CONCLUSION

In periods of rapid change in society and culture, some sociological theories might understandably picture the world out there as a kaleidoscopic flux in which no real structures exist, only momentary negotiated realities. Indeed, such views have had some real influence on the discipline. For this reason, retaining the basic idea of social structure is valuable. Social structures are not phantasmagoria, not fleeting, not ephemeral. They manifest their structural character by persistence through substantial periods and by resistance to attempted change. They have boundaries within which processes and arrangements of parts differ from those outside. There are, of course, varying degrees of boundary permeability, but social interaction is not homogeneous plasma. A defining idea of sociology from its early beginnings is

that symbolically mediated interaction builds relationships that become structures. The central focus has been upon social relations rather than entities or attributes. The specific task of research is to show how variations in structure arise and how structure is maintained, and with what consequence, thus moving inevitably into analysis of social processes and social change.

Sociology has had a major role among the social sciences in identifying and analyzing hitherto unrecognized collective consequences of individual actions. Familiar labels for such processes include: unanticipated consequences, negative and positive externalities, self-fulfilling and self-defeating prophecies, paradoxes of purposive action, side effects. A basic contribution has been the demonstration that the short-run rational maximizing behavior of individual persons and other social entities often produce unwanted negative collective consequences. The actors can be persons, firms, parties, ethnies, states, voluntary associations, and other collectivities. They act to achieve immediate goals of convenience, profit, prestige, security, material goods, and so on in ways that appear rationally suitable. The unanticipated and unwanted collective consequences can range from local frustrations to global catastrophes—from traffic jams to global warming, from extinction of species to destruction of the ozone layer.

Prototypical examples abound. When each person gains the convenience of driving a large, gasoline-hungry automobile, the freeways and streets become choked, air and water pollution increase, urban noise levels rise, and an entire society can become dependent on oil supplies from unstable foreign sources. Affluent individuals may buy caviar until the fish become extinct. In international affairs, the famed Inherent Security Dilemma arises when each state seeks its own security by increasing its military strength, thereby posing a potential threat to another state, which in turn increases its armed forces. The result is an arms race that brings increased risk of war. Seeking sustenance or profits, individuals and business firms overfish the oceans, drain the water supply, erode the soil, cut down the forests, overgraze the fragile grasslands, contaminate water and seas with chemicals. At the limit, depletions and pollutions can produce—and have produced—societal collapse.

Fascinated by the numerous seemingly diverse cases that seemed to have the same basic sources, I began in the 1970s and 1980s to explore these phenomena in papers and articles, e.g., “Relative Deprivation” (Williams 1975), “Individual Welfare and Collective Dilemmas” (Williams 1982). The upshot of these efforts was an image of four universal societal dilemmas, or problems: (a) the classical Hobbesian problem of order; (b) the tragedy of the commons; (c) the problem of collective action; (d) the problem of consensus. Each of these arises if and when social actors seek only to maximize their immediate self-interests through radically individualistic, myopic, rational action.

The problem of collective action arrives, therefore, because individual, short-range rationality cannot solve the dilemmas, which developed precisely because of such rationality. So, collective action is required. But collective action is threatened in turn by the free-rider problem – those who fail to sacrifice or contribute may still

enjoy the public good. Free riding can be controlled in small solidary collectivities where close interdependence and vulnerability to informal social sanctions combine to reduce the incentives for, and increase the costs of, violations of communal norms. In modern social systems, such controls for the most part are lacking or ineffective. Partial solutions sometimes are achieved when regulation can be imposed by groupings having different interests than those of would-be free riders. But even this apparent solution ultimately rests on the development of an effective consensus that permits and supports regulation and requisite sanctioning (each of which has its costs).

So, an unavoidable problem in any social formation is the development of consensus sufficient to support necessary limits, goals, procedures, rules, and sanctions. The specific sources of the needed consensus are complex and diverse; to properly deal with this problem, an intensive and extensive future research program will be required. [Yet a basic framework for analysis was formulated in the 1930s in a now largely neglected work, Parsons's (1938) *The Structure of Social Action*.²] The Hobbesian "war of each against all" postulates a total lack of shared goals and normative consensus, an abstract assumption that is empirically unsustainable in all societies in which stable social relationships and a common culture both prevent and control unrestrained greed and unlimited use of coercion and fraud.

The great virtues of parsimonious formal models of social processes reside in their specification of logical possibilities and in the rigorous analysis of the consequences of those possibilities. Neoclassical economics shows the power of such a simplified model, which permits predictions based on long chains of deduction. Realist theory of interstate behavior uses a few basic assumptions to deduce a Hobbesian world in which wars are restrained primarily by balance of power arrangements or by hegemonic dominations. Game theory (theory of strategic interactions) has brought enhanced clarity in a wide range of applications, and simulations based on similar foundations continually reveal new knowledge.

These virtues come with a price. Rigor often, although not always, means that the analysis is context-free, that is, variables that are not specified are treated as exogenous, as noise, as essentially irrelevant for the purposes at hand. So they represent "nothing-but" science. The difficulty comes when the choice of variables to include in the model omits other variables that can produce fatal errors in prediction. In fact, sociology often has been effective in identifying necessary modifications (or rejections) of overly narrow context-free or mis-specified models and hypotheses.

From my first acquaintance with sociology, I was captured by the vision of a discipline that could develop cumulative, well-tested knowledge. Sociology, I believed, could avoid the pitfall of being merely a chronicler of current issues and events. Although it could include interpretive and aesthetic components, its primary task should be to describe uniformities in the social world, to infer causes and consequences, to establish durable, empirically based generalizations, to develop

²The crucial section is "Hobbes and the problem of order," pp. 89–94.

explanatory theories. Even the present highly selective review exhibits the scientific yield of key concepts developed in research: buffering mechanisms, scapegoating, relative deprivation, group cohesion, ethnicity, values, institutions, conflict. The profession has had its share of debates over the resources and effort that should be devoted to the several divisions of sociology's primary tasks, whether descriptive, explanatory, heuristic, critical/advocacy, or humanistic-appreciative. But this backward look at sociology over the years should leave no doubt that in its scientific aspects the field has achieved genuine and important intellectual advances, both in theories or conceptual frameworks and in their methodological and empirical bases.

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