As the 1930s progressed, a few sociologists tried to arrest this development, at least with respect to the study of economic life. Bernard proposed that "more attention should be given in this country to the Sociology of Economic Relations" (Bernard 1934a, 167); Fairchild held that "wages, prices, trade unions, corporations, factories, the standard of living, all [were] fit subjects for sociological treatment"—adding that, were sociology to engage *these* topics and stop treating them as the natural preserve of economists, the discipline could finally help address "the needs of the contemporary world" (Fairchild 1934, 180; 1936, 8). But such voices were more and more in the minority. In the course of the Great Depression and New Deal, economic sociology, the sociology of the state, and the sociology of law were among the intellectual possibilities that sociologists bypassed as they stood before historically unprecedented changes in the American economy, the state, and the state's legal apparatus, seeking territory unclaimed by the political scientists, lawyers, and economists who had emerged as the professional owners of those great changes.

Acknowledgments

[EIGHT] Hot War, Cold War:
The Structures of Sociological Action, 1940–1955

Andrew Abbott & James T. Sparrow

Despite the immense impact of the Second World War on American society, its effect on American sociology has been little considered. This oversight reflects in part the difficulty of measuring—indeed, even conceptualizing—the war's impact. It is perhaps not surprising that so daunting an intellectual task has not been attempted.

In this chapter, we make a preliminary analysis of American sociology during the war and postwar period. We begin with a brief sketch of the broader social changes attending the wars—two hot and one cold—that defined the period 1940–55. Having thus set out what might loosely be imagined as our story's beginning and end, we evaluate how the events of the war period changed the demographic, institutional, and intellectual structure of sociology. We aim at the same time to analyze the role of sociology (as of the other social sciences) in shaping the new society that emerged after the war. We close with a discussion of the new stance of sociology during the postwar decade.

Society, War, and Sociology, 1940–55

The years 1940 to 1955 saw the United States emerge from the economic catastrophe of the Great Depression, transforming itself in a few years' time into a high-production, high-employment society of unprecedented affluence and global influence. Driving this transformation was a historically unprecedented centralized state, which even after postwar demobilization dwarfed the New Deal precursor that had seemed so large only half a decade before. It is difficult now to recall the dramatic nature of this change. In 1939, a mere 3.9 million Americans (3 percent of the population and 7 percent of the labor force) paid income tax. In 1955, nearly all working families paid income tax. The 44 million federal taxpayers were 27 percent of the population and 66 percent of the labor force. In 1939, at the high tide of New Deal spending, 9.77 percent of the GNP was government expenditure. In 1955, after the New Deal had been beaten back and a conservative presi-

dent occupied the White House, federal outlays were 17.2 percent of the GNP. In 1939 a tiny handful of American men served in the military. By 1955, 64 percent of men between ages 16 and 34 had served.

The social landscape also was transformed in the years during and after the war. The number of high school and college graduates more than doubled between 1940 and 1957. The percentage of American families who owned their homes went from 44 percent in 1940 to 55 percent in 1950, and then rose again to 62 percent in 1960. The median family income went from $1,231 in 1939 to $4,594 in 1957, an increase of 65 percent in real terms. The percentage of foreign born dropped from the low teens in the early part of the century to 8.8 percent in 1940, 6.9 percent in 1950, and 5.4 percent in 1960. The percentage of married women in the labor force rose steadily from 15 percent in 1940 to 24 percent in 1950, to 30 percent in 1960.

To be sure, most of these changes had begun before the war. Government expansion had begun in 1930s, as had married women's move into the labor force and the expansion of consumption-based leisure. The rise in mobility and the decline in immigration were also long-standing trends. But all of these were sharply accelerated by the war years. Even nationality, the least affected of the trends, was recast by a historic surge in naturalization driven by memories of the previous war's vigilante-style Americanism and by the desire to avoid the fate of the Japanese interned from the West Coast. More than 1.5 million people received U.S. citizenship between 1941 and 1945—the highest rate of naturalization for any five-year period up to that date (Ueda 1996, 202).

Driving this rapid change were great events. The Second World War was brief but apocalyptic. For the United States, the ordeal lasted only three years and nine months. But its 16.4 million soldiers included fully a third of all men aged 15 to 50 as of 1945. These men served an average of 33 months in the war, with 73 percent of them going overseas for an average of 16.2 months. Over 400,000 died, nearly three-fourths in battle, while more than 670,000 sustained nonfatal wounds or injuries. The war's cost—civilian and military—drove federal government expenditures to an astounding 46.4 percent of GNP in 1945, while federal debt soared to its all-time high of 129 percent of the GNP that year. Roughly half the total labor force worked directly for the military, the government, or war industry.

From the churning upheavals of total war issued ever-expanding ripples of postwar changes. Approximately 25 million Americans moved to another county or state between 1940 and 1947, a proportion of the population (21 percent) considerably larger than the proportion (13 percent) who had moved during the second half of the 1930s, and even greater than the pro-

portion who had moved during the first half of the Depression (Johnson 1993). Even while the housing markets and employers tried to absorb this mobility, war-induced programs produced new problems. The GI bill's housing loans provided critical impetus to suburbanization (and hypersegregation; see Jackson 1985; and Cohen 2002), while its educational provisions remade post-secondary and vocational campuses nationwide (Frydl 2000). Politically, the tenuous wartime deadlock between liberalism and conservatism broke into open conflict, producing a partisan battle to define the postwar fate of New Deal reform, as well as the mounting security mania that would lead to McCarthyism. The end of the war decade found the new geopolitics of the cold war firmly in place, as the "loss of China" focused American attention on the Far East and remade the Japanese from racialized enemies into respected allies. The Marshall Plan and NATO marked a comparable transformation of the West Germans. By the summer of 1950 the United States was again at war, this time against a proxy adversary whose sponsorship by the Soviet Union produced sufficient bipartisan agreement to underwrite the permanent erection of a national security state modeled heavily on its World War II precursor. Such were the trends and events of the years 1940 to 1955. It was a time of almost unimaginable domestic transformation, driven by global upheaval.

What were the equivalent changes in sociology? In 1940, sociology had itself just finished a transformative decade. The 1930s had seen the founding of the major regional associations as well as such specialty associations as the Population Association of America and the Rural Sociological Society. Also founded were a host of specialty journals—from *Public Opinion Quarterly* to *Rural Sociology* and *Journal of Marriage and the Family*. By now about half of the colleges and universities surveyed by the American Council on Education had freestanding departments of sociology, and nearly all the rest had sociological instruction (Marsh 1940). The Depression and the maturing of social work had driven most of the active reformers out of the American Sociological Society. (We use hereafter the modern name and the abbreviation ASA for this organization.) Coupled with the growth of alternative societies and the Depression itself, this academicization had shrunken ASA membership to 999 in 1940, down from 1,558 in 1930. The discipline had also shed its umbilical connection with the University of Chicago, whose personnel (as ASA secretaries and editors), journal (the *American Journal of Sociology* [*AJS*]), and money (the ASA subsidies provided by the University of Chicago Press) had translated into what many felt was almost feudal vassalage. The leaders of this revolt—whose greatest achievement was the transformation of the ASA quarterly *Proceedings* into the

bimonthly *American Sociological Review* (*ASR*) in 1934–35—found their victory somewhat Pyrrhic. This new elite, with its quantitative methods and foundation funding, quickly displaced the Chicago school. It took control of the *ASR* within two or three years of its founding and soon came to control the ASA as well.[1]

Intellectually, the 1930s had seen the waning of the Chicago ecological paradigm. Robert Park retired in 1934. His leadership of community studies was replaced by that of Everett Hughes and Lloyd Warner, who approached community quite differently than had Park, studying midsize towns rather than large cities and following a functionalist framework that came from anthropology (Warner's field of training). The decade also brought sociology new statistical rigor borrowed from agricultural and psychological research; the earlier, descriptive use of correlations and cross-tabulations tightened slowly into a more formal and rigorous methodology (Platt 1996; Turner and Turner 1990). The first election studies and survey-based market research date from the mid-1930s (Converse 1987), and even Chicagoans like Burgess turned to prediction—of parole, marital duration, and occupational success. The late 1930s also brought Parsons's *Structure of Social Action* (1937), with its resounding rejection of American social science in order to answer an English "problem of order" by a curious combination of French social absolutism and German historicism. Given the tendency of American intellectuals and reformers in the late 1930s and 1940s to obscure or ignore the European influences that had recently informed academic and reform circles (Rodgers 1998), Parsons's insistence on the insertion of Weber and Durkheim into the American social scientific canon is striking indeed.

The discipline in 1940 had thus recently weathered significant changes. What did this discipline look like in 1955? The ASA was five times the size it had been in 1940. It now met jointly with a new society founded by people who had rebelled against it, the Society for the Study of Social Problems (SSSP). An enormous generation of war-detained students had pushed through graduate school into a tertiary education sector already expanding to absorb veterans and women, and soon to face the baby boom. Sociology was enthroned in the popular imagination in books like *An American Dilemma* (1944), *The Lonely Crowd* (1950), and *The Organization Man* (1956), which the new multitudes of undergraduate sociology students were reading even while they aimed at the careers traditionally identified with sociology—teaching, social work, and ministry (Zetterberg 1956, 10). Sociological market researchers were peddling the concepts of brand image and market segmentation essential to the new consumerism, while pollsters were rapidly becoming electoral fixtures (on soft-core market research, see Karesh 1995 and Levy 2003; see Cohen 2003 on political advertising).

Intellectually, the discipline was now dominated by the odd marriage of survey analysis and Parsonian theory, symbolized by the pairings of Stouffer and Parsons at Harvard and Lazarsfeld and Merton at Columbia. Methodologically this self-proclaimed mainstream had rejected ethnographic and institutional analysis and had embraced big-project sociology (see the essays in Zetterberg [1956], and Lazarsfeld and Rosenberg [1955]), emulating the paradigm-shaping big science that had produced the atomic bomb, radar, and other techno-scientific fixtures of the already-forming World War II mythos. In the SSSP lurked a residue of reformists, ethnographers, radicals, and critics, all of them outside the elite circle of what had become an eastern sociological establishment rooted in Columbia, Harvard, and the New York foundations. Not surprisingly, SSSP-type research and the collections that conveyed it (undergraduate course readers) proved far more accessible and interesting to college students than did the high words and formal science of the elite, producing a structural disparity between undergraduate and graduate sociology that persisted for decades. Most important, by the mid-1950s large portions of sociological writing were not sociological at all in the older sense. They were not studies of social groups, group conflict, or group relations. Rather they were studies of atomized individuals characterized by variable properties and located in a larger and indefinite field—"the collectivity," "the social group," "the society." Given the influence of social psychology as an interdisciplinary platform for wartime social scientific expertise, this shift is perhaps not surprising (see Greenwood [2003] on the earlier decline of the genuinely social dimensions of social psychology), although the degree and timing of social psychology's influence require explanation.

In short, postwar sociology was bigger and more publicly successful than prewar. It was institutionally more solid. It had changed paradigm almost completely; the dominant sociologies of the 1920s having been shoved aside into the SSSP and the undergraduate world. To know how much these changes in sociology owed to the massive social changes and dramatic events of the war years and the early postwar period, we must consider the details of the discipline's wartime experience—its demography, institutions, and intellectual development.

1. Basic sources for the history of sociology in this period are Abbott (1999) and Turner and Turner (1990). On the founding of the *ASR* in particular, see also Lengermann (1979). On methods, see Platt (1996).

The Demographics of Disciplinary Experience during and after World War II

The career experience of sociologists in the war comprised two types of service—in war-related research and in the military. These involved, in effect, two different generations. Only a few sociologists combined military service with war-related research; Arnold Rose (a military fieldworker for the War Department's Research Branch) and Morris Janowitz (a soldier attached to OSS in London) were such rare exceptions. In general those working in war-related research were established sociologists who served as civilians, like Samuel Stouffer who ran the Research Branch and Herbert Blumer who worked at the Office of War Information and later the War Labor Board. Only a few were young people who served as civilian researchers in wartime agencies and then entered graduate study in sociology after the war. (For obvious reasons, these were mainly women—e.g., Gladys Lang.) In contrast, the sociologists who were veterans were mostly men of the younger generation, who either left school for the service and returned later to finish or took up sociology from scratch after the war.

Sociological service in war-related government research was relatively concentrated in certain agencies, although many agencies ended up with at least a few sociologists. This pattern probably grew out of the old-boy methods used to staff up agencies in periods that could be as short as one month. The Office of Strategic Services Research and Analysis (OSSR&A) Branch is the most famous case. The OSS founders tapped diplomatic historians James Phinney Baxter III and William Langer to organize and staff the R&A Branch (Katz 1989, 5ff.). Baxter's and Langer's disciplinary connections in turn guaranteed that OSSR&A would be particularly strong in historians and that it would attract historicist émigrés like Franz Neumann (who in turn recommended his colleagues at the Institute for Social Research). OSSR&A also became strong in economists, through Edward Mason, who moved in from the Office of Production Management to start the OSSR&A economics group. The Office of War Information (OWI), by contrast, was full of psychologists and anthropologists. Its Foreign Morale Analysis Division (FMAD), under psychiatrist-anthropologist Alexander Leighton, was typical in containing predominantly members of its director's anthropological specialty: 10 anthropologists to 5 sociologists overall and 8 to 1 in the OWI/FMAD staff proper. (OWI sociologists were mostly Research Branch loaners [Leighton 1949, 223–25].) By contrast, the Department of Agriculture, with long-standing survey and field study branches, had dozens of social psychologists and sociologists (Larson and Zimmerman 2003).

To gain a more detailed picture of wartime service, we have developed a biographical data set of sociologists in the 1940s by combing a wide variety of biographical sources for information. The data set includes 3,385 people, all of whom meet one of two criteria: (a) they published an article in the *American Journal of Sociology* before 1965 and were born after 1890; or (b) they were members of the ASA in at least two of the years between 1940 and 1949. Since there is no sharp edge to any academic field, our data set of course includes many people identifiably in other fields as well as transient members of the ASA—social reformers, students who didn't finish in sociology, and so on. It includes 1,885 individuals with PhD's whose field is known, of whom 1,314 had PhD's in sociology or rural sociology. This subgroup of 1,314 is what we mean by the word *sociologist* in the paragraphs that follow.[2]

Because of this very broad sampling strategy, the figures we present from this biographical data set are by no means exact. However, the difficulty of assembling data from diverse biographical sources means that the estimates of sociological participation are more likely underestimates than the reverse; false negatives are far more likely than false positives. Also, given our sampling strategy, the probability of our discovering individuals does not vary much from agency to agency, so the *relative* predominance in our data of sociologists in one agency as opposed to another is likely to be a reasonable estimate of the true ratio.[3]

The core of this data set, then, is a group of roughly 1,300 people who ultimately received PhD's in sociology. Of these, 9 percent saw government

2. The other PhD's with known fields are scattered: about 60 each in anthropology, economics, education, and psychology, and about 30 each in history and political science. Another 287 individuals had PhD's but in an unknown field. So far as we know, 1,500 individuals lacked PhD's. Many of these were transient members of the ASA or young reformist writers from *AJS* of the 1920s and 1930s.

3. Searching by agency is extremely difficult, as most agencies employed thousands of individuals and lack separate lists of social scientists or sociologists in their employ. In the few cases where we do have exact listings by agency, it seems that our underestimate within a given agency may be as much as 20 percent. For example, our data set misses about a quarter of the recognizable sociologists in the Research Branch of the War Department, all of whom are listed (along with many others—mostly psychologists and nonprofessional field staff) in the first volume of *The American Soldier*. On closer investigation, however, it turns out that we have data set knowledge of all but one of the "missing" people in government service elsewhere. They participated in the Research Branch as loaners or as military personnel detailed to work on the project as field surveyors. This underscores the fact that many people in government service moved between agencies and even within the military, both formally and informally. Note that by ending the ASA frame in 1949, we may have missed a number of veteran sociologists and younger people who worked in government service during the war.

service at some point in their careers, the vast majority of them during the war years. Another 5 percent saw government *and* military service, and 22 percent saw military service alone. In total, then, over a third of these sociologists did something outside their academic activities in the war, and over a quarter saw military service in particular, a topic to which we return later in the chapter. Here we are concerned with the 14 percent who saw agency service.

By far the most important location for sociologists in government during this entire period was the Department of Agriculture (USDA): 52 PhD's in rural sociology, sociology, or social psychology worked in USDA at some point, at least 42 of them during the war period. Already large in 1940, the USDA sociology program (discussed at length in Larson and Zimmerman 2003) expanded in wartime. Of particular importance for later sociology was the social psychology team under psychologist Rensis Likert, which developed many of the standard forms of survey analysis. The USDA was, however, not the only government location for sociologists before the war. The Census Bureau had employed sociologists for years, and 18 of our sociologists served in the census at some point. (Of the 10 who worked there during the war, several were seconded to other agencies at various times; many individuals served in two, three, or even four agencies during the war, often through informal rather than formal transfers.) The Labor Department also hired a few sociologists, some in the Bureau of Labor Statistics (BLS) and some elsewhere (8 total, mostly in the 1930s). Alongside these longtime employers of sociologists, a number of the new agencies employed sociologists. The Federal Emergency Relief Administration hired a few sociologists (8 in our data), as did the Farm Security Administration (9). However, the most important location for sociologists in the New Deal per se was the Works Progress Administration (WPA), in which 21 sociologists in our data set saw service at one point or another.

Of the war agencies proper, the Research Branch of the War Department hired the most sociologists, hardly surprising given Samuel Stouffer's position as director. Our data set locates 21 sociologists there, including Stouffer, John Clausen, John Dollard, Arnold Rose, Louis Guttman, Robin Williams, Shirley Star, and Leonard Cottrell, to give only the big names. Later in the chapter we consider the branch's great postwar product—the four-volume *American Soldier* (Stouffer et al. 1949–50).[4] Although often seen

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4. The studies commonly known as *The American Soldier* were in fact issued by Princeton University Press in four separate volumes whose official series title is Studies in Social Psychology in World War II. Of these four volumes, the first two share the main title *The American*

as sociological, the Research Branch projects were squarely on the boundary between sociology and psychology. The branch contributed almost as many major figures to psychology (e.g., Irving Janis and Carl Hovland) as it did to sociology.

Although the Office of Strategic Services—and in particular its R&A Branch as opposed to its cloak-and-dagger operations—has loomed large in the mythos of sociology, this eminence reflects mainly the accident of Neumann's having brought his soon-to-be famous Frankfurt cohorts Horkheimer, Marcuse, Adorno, and Lowenthal into the Washington office. In fact, the sociological presence in OSS was not very large—only 13 in our data set, although it includes such celebrated names as Morris Janowitz and Edward Shils (both in the London branch, after service with Harold Lasswell in his Library of Congress research operation), as well as Barrington Moore and Alex Inkeles. The Office of War Information actually had more sociologists (18), including Clarence Glick, Warren Dunham, Herbert Blumer, and Hans Speier.

Sociologists were scattered elsewhere in the government. The War Relocation Authority (or WRA, which ran the Japanese internment camps) had six sociologists in our data set, working within a "Bureau of Sociological Research" directed by Alexander Leighton at Poston, Arizona. More an anthropological than a sociological enterprise, this bureau was an extension of the anthropologists' long-standing involvement in the Bureau of Indian Affairs (Hayashi 2004, 24–25). (These researchers are not to be confused with the Nisei "participant observers" who studied the WRA's camps under the leadership of Dorothy Swaine Thomas, whose findings were reported in twin 1946 volumes, *The Salvage* and *The Spoilage*.) The War Production Board hired 4 data set sociologists, the War Labor Board 14, among them Herbert Blumer (who arbitrated a long Pennsylvania strike) and Delbert Miller. The Office of Price Administration hired 11 sociologists, and the National Resources Planning Board 9, among them Louis Wirth and August Hollingshead. Another 5 worked for the Selective Service System, among them William Sewell.

Sociologists also worked in special postwar service. Some served on the

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Soldier, the first being subtitled *Adjustment during Army Life* and the second *Combat and Its Aftermath*. Historical convention has generalized the main title of these first two volumes to the whole series. Volume 3 is actually titled *Experiments on Mass Communication* and volume 4 *Measurement and Prediction*. Even more confusingly, the four volumes have different author lists, although they involve many of the same people (just in different order). To save the reader confusion, we have cited these works throughout as *The American Soldier* (Stouffer et al. 1949–50), using volume numbers as usual with serial publication.

Strategic Bombing Survey—a multimethod investigation of the "effectiveness" of the saturation bombing that, in addition to its military mission, had incinerated over half a million German and nearly a million Japanese civilians, leaving another million or more injured and homeless. Occupational military government in Germany or Japan involved a much larger number of sociologists—20 in all. Sociologists also filtered into the State Department after the war, some through the temporary transfer of OSS to State Department jurisdiction, others through individual intergovernmental transfers.

Sociologists thus saw a wide variety of service in the government. Given that the ASA had about a thousand members during the war years, the fact that 17 percent of the organization (171 PhD-level sociologists in our data set) saw government service during the war may, however, seem surprisingly modest. But it should be recalled that higher education itself ran on skeleton schedules during the war because of the lack of students. Many of those who remained in college teaching were in fact teaching in ROTC programs and other such schemes for keeping colleges and universities in business during the lean years.[5] And many senior sociologists served as consultants to agencies without formal employment in them.

We shall return later to the institutional and intellectual impact of this wartime agency service and the research that grew out of it. Here, we turn to the other side of sociological participation in the war—military service. The extent of military service in the discipline is hard to capture. Of the sociologists in our biographical data set, more than a quarter served in the military. But veterans who finished degrees during the 1950s may have been missed by our sampling criteria. And there are no master lists of veterans, much less of veterans who were or became sociologists.[6] To provide an estimate, we have established exact figures for veterans in the immediate postwar era at the University of Chicago's sociology department, taking advantage of the requirement that all GI bill transcripts had to be stamped with the statement "Registered under Public Law 346." A total of 204 sociologists took PhD degrees from the department between 1945 and 1960. Of

5. The number of ROTC units jumped to 505 (at 352 schools) in 1952, up from 160 in 1941. In addition to ROTC courses, many schools hosted intensive classes for army ASTP and navy V-5, V-7, and V-12, with roughly half a million men in uniform taking such courses during World War II. Another federal source of military training on campus were the Engineering, Science Management, and War Training programs. See American Council on Education 1952, 6, 69–70.

6. A master list of veterans would be little use in any case, as it would not be electronic yet would contain 16.5 million names. We have looked for, but have not uncovered, evidence for the existence of an informal association or list of veterans who were sociologists.

these, 87 were veterans. Among the others, 26 were male foreigners (nearly half of them Canadians, several of whom had served in British forces), and 33 were women (both foreign and American; none served). Seventeen (15 whites and 2 blacks) of the 59 American male nonservers were too old (36 years old in 1941: in fact, men this old were virtually exempt from the draft) or too young (not 18 years old by August 1945) to serve. Ten American nonservers were known to be either conscientious objectors (COs), vital industry workers, interned, or physically handicapped. There remain, out of the 204 total degrees, 23 white and 8 black males of draftable age who did not serve and for whom we do not know the reason.[7]

Overall then, no less than 45 percent of all Chicago graduate students who finished in this fifteen-year period were veterans, and of those students who were white American males of draftable age, a *minimum* of 80 percent were veterans. Military experience was thus almost a universal for young American male sociologists. Most of these, however, had not seen combat. Given that the armywide percentage of combat service was around 25 percent (Stouffer et al. 1949–50, 1:165; Linderman [2000] suggests that no more than 10 percent of soldiers saw extended combat) and that the World War II military assigned military occupations with considerable attention to education and achievement (more so than the Vietnam military; see Flynn 1993, 234), the proportion of all graduate students who had seen combat was certainly not above 10 percent and was more likely 5 percent or less.[8]

On the basis of this estimate, it is likely that half or more of the sociologists coming out of American graduate schools in the decade immediately after the war were veterans. Did this commonality have intellectual effects?

7. The number of blacks is unsurprising, since the segregated military and racist draft boards usually did not want blacks, and many highly educated blacks in particular saw little reason to serve in a military that would constrain them to menial work. The nonserving whites may have had CO or vital industry status, or had physical deferments unknown to us (we know these only on an adventitious basis), and all but 8 of them were old enough to have married and possibly produced children in time to make their draft vulnerability low. Some veterans may not have used GI bill benefits. For example, veteran PhD candidates before service could write their dissertations away from Chicago, which often waived its requirement of registration in the quarter the degree was taken.

8. This measure of sociologists' (more properly protosociologists') service is probably an underestimate for the field as a whole, because of Chicago's relatively large contingent of foreign students, female students, and blacks. An SSRC survey found 72 percent of social science graduate students in 1946–47 drawing on GI bill funds, which provided over 50 percent of per capita monthly receipts across all graduate students (Sibley 1948, 116). However, the SSRC sample was heavily weighted toward the fields of economics and history, where alternative means of support may have been less.

After all, the veterans had shared experiences unlike those of any previous sociological generation: career disruption, heteronomous work for huge and often irrational organizations, erratic mobility in both geographic and social space, and exposure to the peculiar mixture of volunteerism, propaganda, and coercion that undergirded the citizen-soldier concept.

Although this common history may have unified the veterans in some ways, military life was not everywhere the same. The diversity of veteran experience is evident in the March 1946 *AJS* special issue ("Human Behavior in Military Society"), which featured 31 young social scientists whose graduate education was interrupted for war service. The issue combined an anonymous piece about the glories of the combat infantry with discussions of GI language and erotic behavior, examination of fighter squadron social hierarchies, a discussion of army delinquency (including black market trading and looting by U.S. GIs in both friendly and occupied Europe), and a portrayal of an embittered and cynical group of teacher volunteers ground down by army contempt and irrationality. The various papers make it clear that the social scientific impulse survived within the military experience but that the enormous diversity of that experience militated against any single impact.[9]

What about combat? As we noted, relatively few sociologists experienced combat. But combat drove some to sociology. Frank Westie's experience as a bomber pilot over Dresden made him believe "that one could make a greater contribution to world peace through sociological research and teaching than through any other occupation" (Westie 2004). But in the absence of more general data, we can only speculate about the impact of combat on what men later wrote. On the one hand, the bitterness of the combat soldier cries out from the tables of *The American Soldier*. On the other hand, it is common belief, both within the military and outside it, that combat soldiers do not like to talk or think about the experience. So its impact may have gone unrecognized or tacitly overlooked by this generation.

If the military and combat experiences had no clear intellectual impact on the discipline, the demographic cycles induced by the GI bill *did* have a very clear impact. The war in effect stopped graduate education altogether for about four years (Turner and Turner 1990, 87). When graduate school reopened, the GI bill flooded pent-up demand into the system and graduate departments ballooned. As a result, over three-quarters of the ASA mem-

9. During the war the *AJS* featured a number of special issues that provide a window into sociological thinking during the conflict. These issues were as follows: January 1941, "War" (note date); November 1941, "Morale"; May 1942, "Recent Social Change"; November 1942, "Impact of the War on American Life"; and March 1944, "Postwar Preparation."

bership in 1955 came from the post-1945 graduate school cohort; the discipline became extremely young. Moreover, tertiary education's continuing expansion through the 1950s and 1960s meant boundless job prospects not only for this cohort but also for several generations of its students. As a result, whatever was the sociological orthodoxy of the moment during the first postwar generation's training years would be spread without interruption for decades. To the extent that there was such an orthodoxy, it happened to be Parsonianism and survey analysis, and so it is not surprising that these dominated sociology until 1970 and, in the case of survey analysis, even beyond. This demographic account, however, does not address the antecedent question of *why* the dominant themes that the postwar demography so massively diffused should have been what they were. As we shall see, that is a more complicated question.[10]

The War and Sociological Institutions

The war and postwar period had a number of important effects on the institutional structure of sociology. In the first place, the imperative speed of the war mobilization of social scientists confirmed the dominance of sociology's new elite. There was, to be sure, an attempt to mobilize the discipline more generally. The ASA appointed a committee to oversee cooperation with war mobilization in 1940 (Queen 1941). Sociologists also signed up with the National Roster of Scientific and Technical Personnel, a voluntary listing of persons announcing themselves available for service. As of September 1, 1941, a total of 151,726 people had signed up with the National Roster, including 933 sociologists (see National Roster of Scientific and Technical Personnel 1942). The roster was rapidly folded into war mobilization agencies and from there into the civil service. Its records have disappeared, so there is little way of telling whether the 933 sociologists were mostly ASA members (at a time when the organization had only about a thousand members) or mostly laypeople—perhaps reformers and social welfare workers—seeking work at a time when unemployment was still high. (The roster opened for business in July 1940 [National Roster of Scientific and Technical Personnel 1942].) Nor is it evident whether the roster actually served as an important source for recruitment of sociologists into federal agencies. In fact, the lists of those who actually served in agencies

10. The splendid job prospects for the postwar generation make clear that the eclipse of the Chicago school of 1920-35 was in part demographic. Its graduates (and, even more, those they trained) faced the worst job market in American history.

make it plain that elite professional networks, and in particular those of the New York foundations and the Social Science Research Council (SSRC), mattered a great deal. Arnold Rose is a good example: he worked under Stouffer on the Carnegie Race Relations study in the early years of the war and then moved into the field staff of the Research Branch (under Stouffer) when he entered military service.

If, however, recruitment tended to privilege elite networks, far more important was the simple existence of a large and uncompetitive market for professional work. Interuniversity competition, geographic dispersion, and increasing specialization had all tended to move the discipline away from elite dominance in the 1930s. But the war changed that. In the first place, competition declined through sheer concentration. Many federal agencies had more PhD sociologists working for them than did the typical major university department. (Chicago, for example, had seven professors at all ranks in its sociology department for most of the 1940s. No more than five were ever in residence at the university at once. As we have seen, several federal agencies had two or three times that number.) Second, geographic dispersion declined. Transportation and time costs concentrated a large portion of wartime social scientists in the northeastern corner of the country, where social science funding had already concentrated. A substantial minority of the work was in Washington alone. Finally, specialization also declined. Agency service and the new large project model brought sociologists into intimate contact across specialties and indeed across disciplines, continuing a process of interdisciplinarity that had its roots in the movement for the SSRC in the 1920s. Indeed, war experience helped make social psychology—a completely interdisciplinary area staffed equally from both disciplines—one of the dominant subfields of sociology.[11]

In summary, the war created a large, publicly funded research sector within which personnel moved with considerable ease and which drew on university-sector consultants without regard for much more than research convenience. Such an environment fostered the coalescence of a new professional elite. Already emerging in the 1930s, this elite was rooted in the New York foundation community and the major East Coast universities, although drawing heavily on the great midwestern departments as well. After the war, it would locate securely on the East Coast, a move symbolized by Stouffer's move from Chicago to Harvard.

11. The term *social psychology*, however, took on hundreds of meanings. In Merton's influential paper "The Social Psychology of Housing" (1948), taking a social psychological approach meant, to all intents and purposes, looking at the housing situation with much greater subtlety, imagination, and rigor. Today we would say "Be more sociological!"

Wartime research also hardened the emerging belief—which came from this foundation-centered elite—that major sociological work should be centrally dominated, multidisciplinary, team-based research employing a deep division of labor. The template for this approach was the Race Relations Project (the *American Dilemma* project), many of whose dozens of subcontracted researchers were working with wartime agencies within a year or so of completing their work for the Carnegie Foundation. Myrdal's study was not, to be sure, the first such massive project. Warner's Yankee City project took this form, although on a smaller scale, and one could argue that the Chicago school's Local Community Research Committee—with its dozens of students making maps and writing vaguely interrelated dissertations—was a more loosely constructed version of the same thing. But during wartime this model became absolutely dominant, in part because the national scope of wartime agencies required research projects of a comparable scale. Likert's ability to marshal his national field staff within a week of Pearl Harbor earned him the immediate patronage of research directors within key civilian war agencies. Stouffer's comparable capacity to implement a "spot" survey using far-flung, theater-based research teams gave the Research Branch's work a timeliness that was absolutely essential for War Department support (Converse 1987; Stouffer et al. 1949–50, 1:31–53).

The new model for research practice had distinct intellectual implications. Theoretical and conceptual unification became extremely difficult. Neither Myrdal nor Stouffer could manage to produce a cohesive theoretical result despite great effort, for the wartime burst of research was not spawned by a clear set of theoretical questions or even hypotheses. Its overriding task was to monitor populations for bureaucratic compliance and tractability, or to check the pulse of the American people. Indeed, the sheer abundance of facts and findings issuing forth from the wartime government may have provided a heuristic advantage to structural-functionalist analysis, promising as it did to provide an operationalizing scythe that could harvest vast fields of data.

The political climate confronting sociologists in the postwar period also served to advance the new research approach at the cost of the old ones. As scientists basked in the glow of the Manhattan Project and other wartime wonders, Congress moved to fund a new National Science Foundation but did not include the social sciences in the new agency. In his 1946 report on the legislative developments that led to this exclusion, Talcott Parsons quickly identified the problem. Beyond the particular dynamics of the Office of Scientific Research and the Defense Department's insider politics, which had heavily shaped the contours of the final bill, there was the added

problem that social scientists as a group "have been associated with liberal causes and have absorbed reformist traditions in ways which are more or less closely associated with their professional work. . . . This tendency seems . . . to be associated with broad undercurrents of sentiment which are related to the differences of social status and function of the two groups" (i.e., "hard" and "social" scientists). But if liberalism and the associated failure to develop a "social technology" with promise comparable to that of natural technology hurt sociology's political fortunes in the immediate postwar years, Parsons felt that the "new social sciences" were moving in directions that warranted support. Pointing to "theoretical advances" and to improvements in the "range and accuracy" of new methodologies (e.g., "the analysis of opinions and attitudes"), Parsons underscored the promise of social science—indeed, its urgent necessity in the age of the "atomic bomb." Of particular promise was the advancing use of "statistical information," based on recent advances in sampling, to allow the social sciences to approach "the logical equivalent of the experimental method." "If," he closed, "we are to be moving more and more into a scientific age, and science is to help solve its social problems, it must be social science which does so" (Parsons 1946; see also Klausner and Lidz 1986).

Parsons's line of reasoning failed to persuade Congress. While the discipline as a whole waited for inclusion in the National Science Foundation, many individuals pursued contracts with research departments in the Office of Naval Research, the Defense Department, the State Department, and the organization that came to be known as RAND. These patrons placed the same burdens of pragmatism and scientism on their consultants as had been placed on them in World War II (Lyons 1969). As the mainstream of sociology shifted toward the eastern establishment that had charted this new course of research, a contingent of dissidents countered by forming the Society for the Study of Social Problems (SSSP) in 1951. Led by Alfred McLung Lee, who was outspoken in his criticism of the shift in priorities presided over by Stouffer, Lazarsfeld, and the East Coast fraternity, the SSSP spoke to fears that the discipline had gone from bad to worse, replacing "liberal rationalists of the status quo" with enthusiastic "instruments of those in power" (Lee and Lee 1976). The goal was to launch an alternative movement comparable to the rural sociologists' formation of the Rural Sociological Society in the 1930s (Skura 1976, 23, 24–27). Despite the security that Burgess provided as the first president and the active assistance of the major figures Arnold Rose, Louis Wirth, Reinhard Bendix, and Florian Znaniecki, the SSSP's focus on classic social problems such as juvenile delinquency and racial and ethnic conflict (Henslin and Roesti 1976, 57)

could not budge the mainstream literature. The fate of the SSSP showed that the new model of research not only flattened the social topography it studied but also marginalized alternative modes of study by sheer absorption of resources and scholarly attention during the years of its ascendancy.

The Intellectual Effects of the War

Like its effects on the discipline's institutional structure, the war's intellectual effect mainly consisted in hastening developments already underway. This hastening can be seen in each major element of the postwar mainstream: its methodological stance of scientism as articulated through large project surveys; its political stance of detachment; and its model of the social world as a mass of atomic individuals located in a larger "system" or "collectivity." Method, politics, and model all had their roots in the 1930s or even the 1920s. The war established their dominance.

SCIENTISM

Scientism obviously owed much to the war. Although it was the weapons systems that persuaded the public of science's importance, intellectual elites knew that the scientific contribution to military success was much broader—from code breaking to ballistics calculation to operations research. Social scientists were well aware of their own contributions to military performance and production. In sum, among both elites and the public, the prestige of "science" in the immediate postwar period was unparalleled in American history. It is hardly surprising that the new sociology claimed scientific legitimacy.

By this time, however, science had been the banner of sociology for many years. Robert Park had rejected reformism in the name of science in the 1920s. And Talcott Parsons's 1937 Structure of Social Action opened with a "scientific" analysis of action that could have come straight from Cohen and Nagel's (1934) enormously influential naturalist philosophy of science.[12] Nor can one ignore the empiricist scientism implicit in the new survey analysis of the 1930s, itself looking to the statistical revolution then being made by Fisher, Yule, Neyman, and Pearson. The 1930s were also the decade of the "social physics" of Lundberg, Dodd, Zipf, and others (Platt

12. Cohen and Nagel's book was the urtext of the positivism it became fashionable to reject in the 1980s. A purely rationalist theory of science, it was uninfluenced by the sociological subtleties of Fleck, the linguistic doubts of Wittgenstein, or the fatal logic of Gödel—all of them publicly available, if quite obscure, by the time Parsons wrote.

1996, 212ff.). Science in sociology was thus old news. To be sure wartime sociology certainly played the science card very forcefully, as Stouffer did in showing what sometimes degenerated into contempt for ethnography; general theory, and most of preexisting sociology in the opening chapter of *The American Soldier* (Stouffer et al. 1949–50). But his position was merely an extension of common earlier views.

On a less abstract level, the war had implications for sociological methodology in particular. The most evident effect came through sheer finance: wartime agencies invested enormous amounts of public money in sociology, nearly all of it for various forms of survey analysis—from the more than 176 individual studies done by Stouffer's War Department Research Branch to the endless USDA surveys of farmers and the many OWI studies of morale. Such support whetted sociologists' appetite for survey analysis, even as it created a body of results in which to triumph (see Merton and Lazarsfeld 1950). But the more important methodological implications were negative. Perhaps the most important alternatives to survey analysis were the ethnography, geography, and institutional analysis characteristic of the Chicago school and its era. These all identified their objects of research in a manner incompatible with national security, which in wartime extended not only to military installations but also to the war industries and their communities. Even in postwar publication of surveys, the military sometimes insisted on concealed sample sizes lest the actual figures betray military secrets (e.g., Wachtel and Fay 1946, 396). The ethnographic war industry studies—which made clear the chaos on many shop floors—were not publishable until after 1945. And Tamotsu Shibutani (1978) waited thirty years before publishing his amazing chronicle of the dissolution of a Nisei military unit. Even where secrecy was not an issue, the wartime need for unity and national devotion militated against the kinds of methodologies that would most easily uncover the dynamics of conflict and disorganization. In survey analysis such disunity could be sanitized as "problems of morale." And when studies did investigate such major "disunity" events as breakdown of morale, the race riots in Detroit, or the racial tensions in southern military camps and overseas posts, group conflict disappeared behind clashing white and black "attitudes" that were presumed to be tinder waiting for a spark—any spark—to ignite it.

Yet sociology stopped short of extreme scientism. Among the other results of the intelligentsia's experience in depression and wartime was a serious interest in scientific planning and social engineering. Indeed, the latter was a theme in works as diverse as *An American Dilemma* and Norbert Wiener's *The Human Use of Human Beings* (1950). A number of sociologists

flirted with social planning as a model—Louis Wirth, for example (Salerno 1983). Certainly social planning was pervasive in postwar Europe and indeed in the reconstruction of postwar Japan under American rule. But in the United States itself, social planning got a bad name after the war. In part this came from the spate of sociological wartime community studies, nearly all of them studies of the heroic problems in bureaucratically planned communities: the great shipyards in Oakland, California (Archibald 1947) and Seneca, Illinois (Havighurst and Morgan 1951), the 25,000-employee B-24 plant at Willow Run, Michigan (Carr and Stermer 1952), and above all the internment camps for Japanese Americans,[13] which produced a flurry of work from sociologists (e.g., Thomas and Nishimoto 1946), applied anthropologists (Leighton 1945), and political scientists (Grodzins 1949). (The most important planned communities—the many new army bases and such extraordinary places as Oak Ridge—were of course unstudiable for military reasons. But postwar research showed that Oak Ridge, for example, had been hastily erected by the Army Corps of Engineers as a cross between an encampment and a company town, without the advice or guidance of social scientists [Johnson and Jackson 1981, chap. 1]. Selznick's (1949) famous book on the TVA—another wartime study of bureaucracy published after the war's end—would make the same point about the failures of planning and bureaucracy.[14]

An even more important force against postwar planning was the example of Germany and the emerging belief that planning was a step toward totalitarian regimentation. (As the cold war deepened, fear of the USSR with its five-year plans also contributed to the notion that planning was totalitarian.) As Ido Oren (2003) has pointed out, although the Nazi government before 1936 was decried for its racial policies, it was nonetheless admired by some as a model for effective public administration. This respect for fascist "efficiency" dated back to Mussolini's successes in the American press, which came as early as 1925 (Alpers 2002, chap. 2). The postwar recognition that Nazi administrative efficiency had facilitated not only street cleaning but also human extermination created a rejection of public administration

13. We use the term *internment camps* because it was the phrase used at the time. The camps were, however, concentration camps in the literal sense of taking a dispersed population and concentrating it in one place.

14. Thomas and Nishimoto (1946) is the extreme example of this. Johnson and Jackson's 1981 retrospective analysis of Oak Ridge, based on unclassified documents, makes much the same argument, despite its apologetic tone. Given that these bureaucratic studies of failure fit into the growing postwar pessimism about administration, it is curious that they disappeared from the canon of the bureaucracy literature.

that not only destroyed the field in political science but also combined with the right's longtime revolt against interventionist government to produce a complete revulsion against social planning in the late 1940s. Only in some areas of urban studies did the notion of social planning survive the decisive wartime demise of the National Resources Planning Board and most of the New Deal public works projects (Brinkley 1995, 245–58; Porter 1980).

Planning and social engineering did not die away entirely in the postwar years but rather flowed within the narrower and more intensely concentrated channels of the national defense establishment. It is significant that the major experiments in social planning and engineering in this period were all conducted by the military or justified in the name of national defense. Domestically, this happened through the integration of the armed forces, the construction of an interstate highway system, and the bolstering of education through both the GI Bill of Rights (1944) and the National Defense Education Act of 1958. Internationally, it happened through the "reconstruction" of Europe, Japan, and the "third world" by means of foreign aid (the European Recovery Program, or Marshall Plan), the United Nations Educational, Scientific, and Cultural Organization (UNESCO), financial support (the World Bank and the International Monetary Fund), and the export of New Deal–style public works projects.[15]

In summary, the war furthered and perhaps consummated sociology's love affair with science. But it did not originate that affair, nor did it lead sociology to what might seem like the logical outcome—an applied science of openly recognized social planning based on esoteric social knowledge—even though the latter was extensively discussed both during the war and after. As we have seen, fear of just this possibility—because of the inherently political nature of social knowledge—had played a central role in the debate over a National Science Foundation directorate for social sciences.[16]

15. UNESCO exemplifies those sites for sociological activity where American sociologists—including such eminences as Frazier, Angell, and Wirth—would have encountered European social scientists and their ideas. Although the implications of this cross-national contact may have been considerable, the topic lies beyond the scope of our investigation. Similarly important were such international organizations as the ISA, which Wirth helped to found in 1949–50 (Salerno 1987, 29).

16. Even social scientists with strong interests in planning tiptoed around the "problem" that planning might be antidemocratic. Differentiating a possible American or democratic social engineering from totalitarianism was central for works like Mead (1942) and Leighton (1949, 205–18). Even Parsons made some of the same arguments in his (never accepted) SSRC brief for "nationalization" of the social sciences. All the same, social science–based manipulation of the public mind was practiced widely in the United States. And no one seemed to con-

THE SOCIOLOGY OF DETACHMENT AND THE INDIVIDUAL-COLLECTIVITY MODEL

The downfall of planning left sociology with a de facto ethic of detachment, which in turn implied an acceptance of the social status quo as something that did not need to be explained, at least in any focused way. Such an ethic was, of course, frankly expressed in wartime. Although Stouffer was in the abstract a relativist, in practice he identified with the command and scientific points of view:

The concept of personal adjustment is here viewed from the point of view of the Army command. One might have looked upon adjustment from other viewpoints. [As examples, he mentions adjustment defined as minimizing individual anxiety, maximizing democratic participation, or conforming to informal rather than formal Army rules.] But it seemed useful, both for the engineering task of serving the Army and for the analytic task of producing these chapters, to view adjustment in terms of adaptation as viewed by the Army command. (Stouffer et al. 1949–50, 1:82)

Indeed, the very subtitle of its most widely read volume tells us that The American Soldier is about the "adjustment" of the soldier to army life and to combat. After the war, sociology focused less on adjustment per se, taking a more detached view; in Robert Merton, an individual's problems with social structures became not maladjustment but simply "anomie" (Merton 1949a). Detachment was complete.

As the Merton citation suggests, the political stance of detachment was mixed up with another, more purely intellectual change, the move to what we may call the "individual-collectivity" (IC) model of social life. According to the IC model, social life is best conceived at two abstracted levels: individual and collective. The relation between these two levels was theorized in various ways: as microcosm/macrocosm (the individual contains a picture of the "larger" society or its values); as normative (society provides "rules" that govern or "integrate" individuals); and as purely aggregative (society is merely the appearance of individual attitudes or behaviors taken

sider social science–based advertising a form of undemocratic social engineering. The New York, New Haven, and Hartford's legendary "Kid in Upper 4" advertisement was actually part of a precisely engineered campaign to deflect complaints about the railroad's terrible service. The adman who devised it, Nelson Metcalf, proposed to create an ad that would "make everybody who read it feel real ashamed" and thereby defuse civilian discontent. Yet its gently sleeping soldier became one of the icons of war morale (Twichell 2000, 80–87; Fox 1975, 74–75).

as aggregates). But all three modes shared the concept of sharply separated "levels," related in ways that were abstract and timeless.[17]

The IC model and the concept of adjustment both loomed very large in wartime social science. American propaganda as crafted by the OWI, and the Advertising Council offered an especially individualistic vision of national purpose, consistently portraying the war effort as a natural extension of liberal self-interest rather than relying primarily on the heavy-handed appeals to ideology or jingoism that had discredited the Creel Committee after World War I (Fox 1975; Westbrook 2004; Winkler 1978). Even in the absence of this distinctively liberal conception of war aims, however, full mobilization demanded an unprecedented degree of national unity, at least on the surface of things, if only to advance beyond the politicized stalemates that had deeply divided society during the late New Deal.

Social scientists worked hard at this unification in such organizations as the Committee for National Morale, a private organization founded in June 1940 and dominated by prominent exponents of interventionism, including Margaret Mead, Ruth Benedict, Gregory Bateson, Gordon Allport, Hadley Cantril, Erich Fromm, and Robert Yerkes, all of whom sought an expedited mechanism to ensure that American preparedness was guided by the most advanced social science (Herman 1995, 49–50). In work after work—Gunnar Myrdal's *An American Dilemma* is only the most famous—the "American character" and "American creed" emerged as broad unifying concepts. The unity they portrayed—or wished to summon into existence—was not in any way a reality. We now know that the war years saw new levels of conflict, social dislocation, and disorganization on the "home front," as it was all too tellingly called. There was social chaos within the munitions plants and their communities. Work stoppages reached record numbers, cresting at 4,956 in 1944, only 29 short of the all-time high in 1946. Turnover among manufacturing employees more than doubled, leaping from a monthly rate of 4 percent in 1940 to over 9 percent in 1943. There were race riots in New York, Detroit, and Los Angeles, as well as lesser violent con-

17. For classics statements of the IC view, see Parson's review (1942) of Angell, *The Integration of American Society*, and Shils and Young's famous paper (1956) on the coronation. The IC view can be opposed to the notion (characteristic of the Chicago school both in social psychology and urban ecology) that both individual and collective phenomena emerge and reciprocally define each other in a social process in which overlapping and interconnected groups contact, compete, and accommodate. In such a view, all social phenomena flow in time, and levels are merely analytic appearances. Social problems are solved not by "adjusting" individuals to a procrustean social bed but by modifying the conflicts and interactions of groups so that socially destructive conflict is minimized.

flicts in dozens of other American cities, not to mention in numerous military camps and forts throughout the South and West. The U.S. divorce rate rose dramatically and steadily throughout the war, from 8.8 per thousand married women in 1940 to a peak of 17.9 in 1946, when many hastywartime unions were reconsidered. It did not settle back down to prewar levels until the second half of the 1950s. In the military, as Stouffer's Research Branch found, there was a very real and thoroughgoing lack of enthusiasm for the dangerous work of combat. In such a political setting, the focus on processes of conflict and accommodation that dominated much earlier sociology—particularly the Chicago school's concept of urban ecology—was simply unacceptable. This was America, and "we" were "all Americans." So wrote the distinguished cultural relativist Margaret Mead in her 1942 book *And Keep Your Powder Dry*, a book whose entire aim was precisely to conjure this "American character" out of the vast, inchoate diversity of the nation.[18]

If the IC model was a logical necessity of the very concept of national morale, it was even less surprising that adjustment was central to the applied social science of wartime. Once the collectivity's interest became the overwhelming one of war, it logically followed that individuals would have to "adjust" to social needs. The proximate groundwork for this approach had already been laid in a 1941 memorandum by Stouffer, Burgess, Cottrell, and other members of the prediction subcommittee of the SSRC's Committee on Social Adjustment. This memorandum framed both the civilian and military aspects of the defense program as a question of "efficient use of human resources," to be mined from the social soil much as other vital resources would be extracted from nature (Burgess et al. 1941).[19] The adjustment model meant that there were no competing groups in the *American Soldier* volumes, none of the particular units, and gangs, and squads, and brothels, and contending branches that would have filled the pages of a Chicago-style study of the army. Nor, indeed, is there any sense of the situatedness of social experience, either in particular social settings or in the particular life

18. Even so consistent a radical as Alfred McClung Lee felt it necessary, in his book on race riots (Lee and Humphrey 1943), to toe the line on the IC image as contained in the concept of Americanism: "Thirty-four Americans died in the Detroit race riots of the week of June 20, 1943. They died while their relatives were fighting in American Uniforms on the battlefields of a war for freedom. It is sincerely hoped that this book points to a few of the lessons we Americans must learn from this hysterical attack upon democracy and American morale" (Lee and Humphrey 1943: ix).

19. Many of the topics considered in this memorandum, such as "personnel placement in the military services," "selection of officers in the armed forces," "selection of aircraft pilots," and "prediction of adjustment to military life," read like a prescient chapter outline for the volumes of *The American Soldier* that would be so influential a decade later.

courses of individual soldiers. The soldiers are no more than sample points—arbitrary "representative" individuals. There is nothing linking those individuals to the collective (or to each other) other than their surveyed "attitudes" and, perhaps, the bureaucratic structure of the military.[20]

The practical meaning of the adjustment approach to war becomes clear if we read the two chapters of *American Soldier* on air corps morale (Stouffer et al. 1949–50, vol. 2, chaps. 7, 8) in parallel with the 1995 war novel *Ash Wednesday '45* by Frank Westie, a bomber pilot who became a sociologist studying intergroup conflict. The *American Soldier* chapters speak of high morale, even though a third or more of the flyers said they would not volunteer for combat flight if they had the choice to make over again. The chapters talk of "satisfaction" among flyers. But Westie's novel portrays profound demoralization, a black bitterness that leads his hero to refuse to firebomb Dresden a second time, making the choice Westie himself must have thought about. (He refused all military decorations after his own Dresden flights [Westie 2004].) Only once, at the very end of the *American Soldier* chapters, does Westie's world intrude into the Research Branch view of the world; there we read that the six-month casualty rate for the heavy bomber crews was 71 percent killed or MIA plus 18 percent wounded in action (Stouffer et al. 1949–50, 2:407). In all but name this was a suicide corps.

To such men, at such a time, the Research Branch and its questionnaires must have seemed literally incomprehensible. In reality, the aim of the *American Soldier* air corps morale investigations was to decide how long a tour should be—what number of sorties would get the maximum of skill and performance out of the flyers before they burned out to the point of collapse. *The American Soldier* was specifically *not* a conceptually driven inquiry into the sociology of military life, interesting as that would have been; it was a work of morale engineering and management using sociological methods (and drawing heavily on commercial market research [see Stouffer et al. 1949–50, 1:38]). The sociology was added as an afterthought or sideline (a criticism sometimes also made of Paul Lazarsfeld's later market research [Converse 1987, 270–72]).

20. The main exception to this statement is the reciprocal anger and distrust between combat and noncombat troops. In *The American Soldier* this conflict is a static attitude conflict between army types, even though it is clear that men rotated through both settings in their army careers. The organizational-level dynamics of this conflict, however, are never made clear. (Westie [1995] tells the interesting story of a combat flyer who sends a recalcitrant member of his noncombat ground crew off to France, combat, and probable death.) The *American Soldier* volumes do not consider the various "disorganizations" of army life—personal vendettas, looting, atrocities, goldbricking, subversion, and so on—other than as signs of weak morale. Nor does it consider interunit rivalry and conflict, even though any veteran knows this to be a crucial and often destructive factor in army life.

We see then that the IC model and adjustment were central in wartime social science. But they were not new wartime ideas. They dated from the 1920s. The idea of adjustment had originated in the vast expansion of psychiatry out of the mental hospitals after 1900.[21] As physician promotion slowed and mental illness proved intellectually intractable, psychiatrists flooded into criminology, industrial psychology, pastoral counseling, and dozens of other areas. They achieved institutional success in the juvenile court system and "child guidance" systems, and found public recognition in the shell shock controversy of the First World War and the "mental hygiene" movement. Adjustment was the core concept of all this "social psychiatry," which also developed the life-course concept (the "dynamic psychiatry" of Adolf Meyer) and indeed the very concept of personality.[22] By the late 1920s, the American Psychiatric Association was organizing joint meetings with the SSRC and the Laura Spelman Rockefeller Foundation about the possible contributions of psychiatry to social science. Organized by W. I. Thomas, these meetings brought together the Chicago sociologists, the leaders of American outpatient psychiatry, of child guidance and of delinquency studies, and a variety of luminaries from other social science disciplines.[23]

By the early 1930s, all this ferment about adjustment and personality had coalesced into two major streams. One was the strongly anthropological culture-and-personality movement centered at Yale (later Columbia), of

21. Psychiatry as of 1920 was not the low-status specialty it is today. Practitioners were a small (approximately 1,500) but powerful group deriving cultural authority from its nationwide control of about 250 mental institutions, which contained almost half as many patients as there were undergraduates in all of America's colleges and which cost the states about 10 percent of their total budgets. This discussion of the adjustment literature rests on Abbott (1982, chaps. 7, 11, and 12).

22. The idea of—and certainly the word—*adjustment* was also taken up by psychologists (Napoli 1981). Although in the child guidance clinics, the psychologists were subordinate to psychiatrists, the idea of mental and personality testing—psychology's main stock-in-trade at this point—spread far beyond the military and the guidance clinic, which were its first two areas of application. By the late 1920s, testing was nearly universal in industry, school, court, and society. The psychologists did "preventive" adjustment: they made sure square pegs went into square holes. The psychiatrists did the sanding when the pieces weren't quite square with the holes they happened to be stuck in. Together, these two things constituted a notion of adjustment that was absolutely pervasive in American culture in the 1920s.

23. Astonishingly, the Chicago sociology department's Society for Social Research focused its 1926 summer meeting on the subject of the relation between psychiatry, psychology, and sociology, even though in the standard histories this moment was the supposed apogee of the Chicago school's focus on urban sociology and ecology. Much of this history is based on materials identified and supplied to us by Rainer Egloff of the Collegium Helveticum. We thank Mr. Egloff for his generous sharing of this material, which challenges many presumptions about the history of the social sciences in the 1920s. Another source, excellent in its detail but not always careful about chronology, is Darnell (1990).

which the major figures were anthropologists Edward Sapir, Margaret Mead, Ruth Benedict, political scientist Harold Lasswell, psychiatrists Harry Stack Sullivan and Abram Kardiner, and the heavily psychoanalytic sociologist John Dollard. Out of this school came Benedict's popular success *Patterns of Culture* (1934) and the ensuing national character studies such as Mead's rhapsodic *And Keep Your Powder Dry* and Benedict's OWI-sponsored *The Chrysanthemum and the Sword*. For anthropologists, "culture and personality" had a natural fit, since at this time they were dedicated to studying small groups that were in themselves "whole societies," without clear internal subgroupings and intragroup conflict. The IC model came naturally.

The other stream was what we may call the "adjustment" school. Its clearest exemplar was Ernest Burgess, who spent most of the 1930s on large-scale studies of prediction of individual outcomes like probation violation and divorce. Here too the concept of personal continuity over time—the notion of personality—was central, but the focus was on *personal* adjustment in given social structures like marriage and everyday behavior, not the culture-and-personality school's loose configurational resemblance between the individual personality and the "larger culture." By the late 1930s, Burgess and Samuel Stouffer were running an SSRC subcommittee on prediction of personal adjustment. (Burgess was chairing the parent Committee on Social Adjustment [see Young 1941, 873; Horst 1941].) This work moved toward an IC model of the world, although sometimes retaining Thomas's insistence that "the moral good or evil of a wish depends on the social meaning or value of an activity which results from it" (Thomas 1923, 38).

The IC model was, however, not solely a product of the adjustment and culture/personality schools. The vision of "a national society" is also implicit in the celebrated *Recent Social Trends* volumes of the Hoover commission, which wrote, in 1932, with great prescience:

In times of war and imminent public calamity it has been possible to achieve a high degree of coordinated action, but in the intervals of which national life is largely made up, coordinated effort relaxes and under the heterogeneous forces of modern life a vast amount of disorganization has been possible in our economic, political and social affairs. It may indeed be said that the primary value of this report is to be found in the effort to interrelate the disjointed factors and elements in the social life of America, in the attempt to view the situation as a whole rather than as a cluster of parts. (President's Research Committee on Social Trends 1933, 1:xxii–xiii)

Indeed, much of the politics of the 1930s had searched for just such a national society and, significantly, used the analogy of war (specifically, the

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precedent of World War I) to tie New Deal reforms to patriotic values, as in Gen. Hugh Johnson's use of the Blue Eagle as a badge of compliance with the NRA (Leuchtenberg 1995, chap. 2). At such a national level, the view of social changes had to be segmentary and topical, rather than interwoven, and so in the Committee on Social Trends' volumes, as indeed in the contemporary community studies of the Lynd-Warner type, social life became a list of functions (education, family, childhood, consumers, and so forth) on the one hand and of problems (welfare activities, crime, health, and the like) on the other, all located within a giant, overarching "society" of which they were simply different aspects.

Thus the wartime focus on adjustment and insistence on a cohesive "American character" and "American culture" were not only necessities of mobilization but also logical developments of long-standing trends in social science more broadly. The major sociological classics throughout the war period and postwar follow the IC model almost without exception. Nothing could make this clearer than Myrdal's *An American Dilemma* (1944), for which the research strategy was designed well before the war. Although he came from a social engineering/social democracy background, Myrdal organized his analysis around a culture-and-personality argument. The roots of the American dilemma lay in the contradiction between the master values of the American creed and the particularities of racial prejudice. Although *An American Dilemma* discussed many of the social structures of racism, Myrdal ultimately viewed those structures as an expression of an attitude of bigotry rather than as the loci of intergroup conflicts in which racism was itself produced. Louis Wirth, by contrast, had objected to this aspect of the design from the beginning. (See his analysis of Myrdal's original project design for Carnegie's Frederick Keppel, in a letter of January 28, 1939.)[24]

As a widely celebrated study, *An American Dilemma* contributed much to the "disappearance of the middle ground" that was characteristic of the IC model of social life. Similar personality arguments became standard in the voluminous postwar literature on the origins of Nazism. The celebrated *Authoritarian Personality* (Adorno et al. 1950) and the enormous literature on prejudice took a personality-based view of prejudice in which actual social

24. Wirth papers, box 55, folder 12. Like other sociologists in the Parkian race relations tradition, Wirth preferred a processual approach to race conflict, in which racism like other social values was produced in the crucible of actual, ongoing interaction between social groups rather than being an ex ante personality or cultural quality. To Myrdal, however, Wirth's view was "pessimism." He did not see the strength of the processes that maintained what would later be called "institutional racism."

conflict and group relations played almost no role. Adorno himself had been highly dissatisfied with the study's failure to place the psychology of prejudice more firmly within a critically articulated social analysis, which suggests the power of the IC model and of survey methodology in that period, even for a veteran of the Frankfurt school's critical approach (Jay 1973). Not until the 1970s would there be serious attempts to seek the social dynamics of Nazism; it is striking in this connection that Neumann's *Behemoth* (1944) did not become one of the great texts of modern social science, while *The Authoritarian Personality* did.

Finally, the dominance of the IC model explains the otherwise enigmatic postwar marriage of the florid abstractions of Talcott Parsons with the dowdy concreteness of survey analysis. *The Structure of Social Action* clearly presupposed two levels of social life: the individual and the collectivity. In principle, the collectivity might be any size or shape of group, but in effect the very abstract framing of the book essentially opposed the individual and the total society, following the Durkheimian logic in which occupations, religious institutions, the family, and so on figure not as actual social groups with real qualities and conflictive existence but simply as conduits for "forces" between the actor and his "collectivity."

To be sure, the IC model had considerable empirical justification. As we noted earlier, American society really was transformed during the war era. Much of that transformation took the form of eradicating or overwhelming what had been stable intermediate structures of interwar society. The vast increases in geographic and occupational mobility, the move of women into the workplace, the dislocations and homogenization of military service, the new possibilities for housing and education, the massification of government, the consolidation of mass culture reinforced by rapid suburbanization and a national consumer market: all these things meant that those who talked of a larger "society" were indeed talking about something that was far more of a reality in 1950 than it had been in 1930. In such a context, the IC model made more sense than before. At the same time, the IC paradigm led mainstream sociology to miss the inevitable emergence of new middle-level structures in the mid- and late 1960s. It also led sociologists to ignore the role of quantitative sociology in *creating*, as much as finding, the new society. But all this was perhaps hard to see in 1955.²⁵

25. James Coleman (1978, 1980) argued this position at length, saying that a new society called for a new sociology. Coleman's argument ignored two other things: first, the problem of direction—sociologists played an important role in defining and constituting national society; and second, the extraordinary fecundity of social processes in producing new dimensions and groupings of differences within what appeared to be a highly individualized mass society. It is

Sociology in the Postwar Decade

MCCARTHYISM

Although the wartime and postwar flowering of applied scientistic research grew naturally out of past trends in sociology, the rising climate of anti-communist suspicion ensured that some aspects of the discipline—in particular the "liberal" propensities noted by Parsons in 1946—would be suppressed, if not on grounds of objectivity and scientific detachment then on grounds of loyalty. Yet so many sociologists had been involved in reform or meliorism that it is unclear what principle determined which scholars were singled out for judgment. And if, as seems likely, the process was in fact an arbitrary drive by anti-intellectuals and red baiters uninterested in fine distinctions, and intended simply to squelch all critical thought, then the intellectual impact of the purges remains unclear. (See Schrecker [1986] on the general climate of intellectual self-suppression and Lazarsfeld [1958] on the varied and nuanced responses of social scientists to anticommunist pressure.) Yet we can get a sense of the nature of anticommunism's impact on sociology by looking at the investigation of none other than Samuel Stouffer, a man whose lifelong Republican politics, extensive government service, and distanced methodology should have been above reproach in the new postwar climate.

In 1955, not long after McCarthy had been censured by the Senate, Stouffer had the temerity to publish a definitive national study of public attitudes toward the elephant in the living room. *Communism, Conformity, and Civil Liberties* investigated whether Americans really were anticommunist. Despite the unusually rigorous sampling methodology employed (dual independent national samples, one by Gallup, one by NORC), Stouffer was investigated and subsequently lost his security clearance. Stouffer's scientifically established reputation, his position in the Harvard Department of Social Relations, and the near-universal recognition accorded *The American Soldier* upon its publication in 1949 were not enough to protect him from suspicion. Nor did his avoidance of a "social problems" approach help him. The book overlooked the explosive class conflicts, racial antagonisms, and local power struggles that drove anticommunism: it rose above such lo-

the latter process that always defines the limits of survey analysis. The IC view eventually began to recognize the existence of intermediate structures, but these were always concentric: neighborhood, community, state, society. One can follow the methodologists' gradual unpacking of this series in the debate, started by Robinson's celebrated 1950 paper, on the ecological fallacy and the true "level" of variables. For a good contemporary overview, see Lazarsfeld (1993, chap. 8) and Menzel (1961).

cal events as particular unionization drives, or civil rights challenges, to the bloodless abstraction of "conformity." But all this did not suffice. Simply for investigating anticommunism in the context of conformity and civil liberties, one of America's best-positioned conservative sociologists lost for a time his position in the government funding game.[26]

Stouffer's plight suggests that McCarthyism may have operated as a general field effect rather than as a decisive agent favoring one sociologist over another. Yet the proliferation of such safe abstractions as "anomie," "conformity," "adjustment," and "mobility" during this period, tied so often to respectable and "objective" methodology, raises the possibility that fears of conservative pressure may have led many sociologists to take cover in the shade of the IC model.

NEW TOPICS

Postwar sociology turned to a number of new topics (for a general overview, see Kinloch 1988). The first of these was social mobility, which emerged in the 1950s as one of the field's central preoccupations. By 1954, study of mobility was on the cusp of transformation. Earlier mobility work (e.g., the Six Cities study reported in Palmer 1954) had been geographically based. Later work would all be national, partly because of the IC model and because of governmental and commercial efforts to monitor and shape the economic and political choices of the geographically mobile society that had emerged in the war years and after. Once government and industry regularly produced national data sets, which became the standard practice during the war and after, it would be difficult for social scientists to ignore the siren song of national generalizability.[27] The ideology of "national is better" was argued quietly but forcefully in Hebert Parnes's influential review of mobility studies. In Parnes's conceptual chapter (1954, chap. 2) one can see how the embedding of labor in geographical labor markets, in individual life cycles, and, ultimately, within particular employers or even employment sectors had become an inconvenience to the project of generalization, which would by the 1960s consider only the education, age, race, gender,

26. For an extended investigation of FBI surveillance of American sociology, see Keen (1999). Chapter 10 of the Keen volume discusses the case of Samuel Stouffer.

27. Of course national studies and data sets had been produced before, most notably the Census, but also data from the departments of Commerce and Labor in the 1920s, and various New Deal agencies such as the WPA. The shift here is to note that the war permanently increased the need for national data sets corresponding to the policy domains of military and civilian agencies.

and occupation (not employer) of workers. Codable and replicable nationally, these variables not only enabled the study of national level mobility; they also constituted it as a social phenomenon.

The second major new topic of the postwar era was the new mainstream's candidate for an intermediate institution between individual and society, its replacement for the acting groups and conflicting structures of the Chicago school. Not surprisingly, given wartime experience, the new candidate was bureaucracy. In 1944 some 3.3 million civilians worked for the now-enormous federal government, along with 12 million in the armed forces and some 19 million in defense production, the vast majority of it contracted to gigantic firms. In short, about half the total labor experience in wartime America was in large organizations. Moreover, federal bureaucracies had touched Americans in a thousand ways, from rationing to draft registration to the mass income tax.

This postwar importance of bureaucracy was utterly new in sociology. Neither the Chicago school nor its various competitors in the 1930s had taken the problems of bureaucracy seriously.[28] Most work on bureaucratic employees and employment during that period took place in the engineering and business schools, where the descendants and opponents of Taylorism studied the restless entrails of business. Other than psychology, the social sciences proper had little to do with this work.

Parsons, however, featured the Weber analysis of bureaucracy in *The Structure of Social Action* in 1937 and published a translation of the famous Weber essay on bureaucracy in 1947 (Weber 1947). The wartime community studies, as we noted, mostly concerned bureaucracies, as did Selznick's innovative study of the TVA. In 1952, Merton and others published a reader on bureaucracy (Merton et al. 1952) that drew together work from the "engineering" tradition of management (Simon, Veblen), the business studies tradition (Berle and Means, Gordon, Bernard), and historical sociology (Weber, Bendix, Michels). The book even took a crack at what had become the dominant social question of the day—why Nazism?—by including a section from Neumann's *Behemoth* and a commissioned paper giving a Weberian (rather than Neumann's more political and historicist) interpretation of National Socialist bureaucracy.[29]

28. Only seven articles on bureaucracy and bureaucratization appeared in the forty-five volumes of the *AJS* before 1940, while fifteen appeared in the next twenty-five volumes ("Cumulative Index" 1965).

29. Missing from the book, however, is the optimistic analysis of bureaucracy that had been characteristic of the political science subfield of public administration before the war; see Oren (2003).

After the war, community studies would become obsessed with the suburbs—Crestwood Heights, Levittown, "exurbia," Park Forest, and so on. But the theme of bureaucratic employment remained central to these studies as well, and a number of their subjects were themselves planned communities posing the same intellectual problem (how do "natural" communities form in bureaucratically structured settings) as had Seneca, Willow Run, and the internment camps during the war. The same bureaucratic theme emerged in such general interpretations as Riesman's *Lonely Crowd* and Whyte's *Organization Man*.

The concept of bureaucracy supplied the IC model of society with an intermediary institution between the individual and the collectivity. Bureaucracy—which in the guise of the military had been just such an intermediary institution for an entire generation of young American men—thus replaced the Chicago school's ethnicities, churches, voluntary and business associations, gangs, clubs, and so on as the basic "type" of social organization. Role theories abounded, but built not so much on the dramaturgical roles of Goffman as on the job-description roles of the social psychologists. Even communities, as we have seen, were often interpreted as planned (or at least "functional") structures, in which the "natural histories" of the Chicago school were mere eddies downstream from bureaucratic design. Thus bureaucracy's static, hierarchical systems of roles and rules became the model for social life, replacing the collective behavior, ecological competition, and interaction cycles of the Chicago school. Formal organization became the foreground, "informal organization" the background. In the distance lurked the notion of society as efficient, which had in fact been quite explicit in the wartime studies of munitions plants. Indeed, in Parsons's postwar writing, norms themselves became organized into quasi-military hierarchies of sub- and superordinate.

Weaving all these strands into a global system was modernization theory, the great paradigm of postwar social science and the most important bridge between academic work and national policy during the high cold war. With its immediate intellectual roots in Parsonian structural functionalism and the intellectual culture of the Harvard Department of Social Relations, modernization theory was quickly developed and applied to a wide range of social science and policy questions by the legions of graduate students issuing forth from Harvard and Columbia in the 1950s (Gilman 2003). Presenting the abstracted and decontextualized model of "modernity" as the end state of an ahistorical "process" of development, influential scholars such as Gabriel Almond, Lucien Pye, and Walt Rostow finally achieved the Washington influence that social scientists had sought since the earliest

years of Progressivism. Calling for massive social surveys to enable the technocratic management of personal and social adjustment required by the bureaucratic rationality of modern society, large-scale plans such as Project Camelot aimed to remake the "third world" in the idealized image of the United States, placing it on the fast track to "modernity." Only modern social science could so elegantly reduce the complexities of nonindustrialized societies to a manageable matrix of key factors in need of strategic manipulation before unleashing the full potential of the "free world." In the context of a global struggle against Communist seduction of "undeveloped" nations, such knowledge would be powerful indeed.

At this brink of technological mastery, cold war social science blinked. And in that moment, all its achievements came down like a house of cards, blown to pieces by the controversial winds of the 1960s. But that is another story.

Acknowledgments