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“Traditional Working-Class Neighborhoods”: An Inquiry into the Emergence of a Sociological Model in the 1950s and 1960s

By *Christian Topalov**

ABSTRACT

City districts that had been commonly called “slums” or “disorganized areas” came to be described as “communities” by a number of sociologists in the 1950s and early 1960s. The paper identifies the descriptive model that then took shape: “the traditional working-class neighborhood.” In spite of variations in scientific vocabulary and style, a surprisingly similar view emerged from inquiries by Willmott and Young in London (1957), Gans in Boston (1962), and Coing in Paris (1965). The paper takes this convergence as an issue and explores the cognitive and social conditions that may account for it. Stress is placed on the common distance of the inquirers from the academic sociological establishment, their concern for making sociology both critical and useful to planning, and a similar situation of wholesale destruction of popular neighborhoods by urban renewal. “Working-class neighborhoods” were thus “discovered” at the very time they were about to vanish.

INTRODUCTION

“How the other half lives”¹ in cities has long concerned reporters, reformers, and social scientists—and still does. Scientific languages, however, alter according to time as well as place, and the sociology of scientific knowledge can help make sense of those interesting variations. The change this article examines occurred about half a century ago. City districts called “slums” by late-nineteenth-century social science or “disorganized areas” by 1920s urban sociology came to be described as “neighborhoods” or “communities” by a number of sociologists in the 1950s and early 1960s. A strong descriptive model took shape in that period, one I would name “the traditional working-class neighborhood.” This wording was new then and deserves attention, since it was to become commonsense in various quarters of the social sciences for a long time.

Social sciences have a definite national bent, and the theme studied here represents

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¹ Jacob A. Riis, *How the Other Half Lives* (New York: Sampson Low, 1890).

no exception. That makes all the more intriguing the fact that in the span of less than one decade, a broadly similar model emerged to describe "traditional neighborhoods" in quite different cities, located in different countries. To make sense of this we cannot merely relate the history of some ideas and spare detailed historical-sociological research into the social (including intellectual or cognitive) conditions of scientific practices and findings. An inquiry has to be conducted on the right scale, from the local to the international, taking into account the actual boundaries and organization of scientific fields. The sound method, then, is to carefully inquire into each case and its context first, and then raise the broader issue of what made possible any similarities and peculiarities found among cases. Let us at once discard the lazy answer that sociologists in these different places said about the same thing because what they observed was similar: if that were true, sociologists would have described "working-class neighborhoods" twenty or fifty years earlier. If similarity there was indeed, we shall see it was not what one might expect.

BOOKS

Three books, recording inquiries made in London, Boston, and Paris, can provide the case studies we need. All of them had a wide circulation and eventually became "classics" of sociology in their respective linguistic spheres and sometimes beyond.

Michael Young and Peter Willmott studied Bethnal Green, an East End London borough, between 1953 and 1955, and published *Family and Kinship in East London* in 1957, in Britain and the United States.² A number of reviews in British newspapers and journals immediately followed, a Penguin edition came out in 1962, a French translation in 1983, and another Routledge edition in 1986, which the University of California Press republished in 1992. Half a million copies in English have been sold.³

Herbert J. Gans inquired into the Boston West End from October 1957 to May 1958, then wrote a report that became a 1962 book, *The Urban Villagers*.⁴ This work, too, proved to be a publishing success: a paperback was printed in 1965, and a new edition in 1982. All together, 160,000 copies of the 1962 and 1965 volumes were sold, and 22,000 of the 1982 edition.⁵

Henri Coing began his study of one large block in the thirteenth arrondissement of Paris in November 1963 and presented it as a doctoral dissertation in November 1965. He published *Rénovation urbaine et changement social* in 1966; a new edition came out in 1973 and a new printing in 1976.⁶ By the early 1980s, the book had sold 9,000

² Michael Young and Peter Willmott, *Family and Kinship in East London*, new rev. ed. (Baltimore: Penguin Books, 1962) (hereafter cited as *FK*). Unless otherwise noted, references in this paper are to the 1962 edition. Other editions: London: Routledge & Kegan Paul/Glencoe, Ill.: Free Press, 1957; and new ed., London: Routledge & Kegan Paul, 1986/Berkeley: Univ. of California Press, 1992. French translation: *Le Village dans la ville* (Paris: CCI, 1983).

³ Nick Tiratsoo and Mark Clapson, "The Ford Foundation and Social Planning in Britain," in *American Foundations and Large-Scale Research*, ed. G. Gemelli (Bologna: Clueb, 2001), 201–17. I thank the authors for kind communication of an earlier unpublished version of this excellent study.

⁴ Herbert J. Gans, *The Urban Villagers: Group and Class in the Life of Italian-Americans*, new paperback ed. (New York: Free Press, 1965) (hereafter cited as *UV*). References in this paper are to the 1965 edition. Other editions: New York: Free Press, 1962; and new ed., New York: Free Press, 1982.

⁵ Figures (as of November 1999) kindly provided by Herbert J. Gans.

⁶ Henri Coing, *Rénovation urbaine et changement social: L'ilot no. 4 (Paris 13e)* (Paris: Editions ouvrières, 1966) (hereafter cited as *RU*). Unless otherwise noted, references in this paper are to the 1966 edition. Other editions: new. ed., Paris: Editions ouvrières, 1973; and new printing, Paris: Editions ouvrières, 1976.

copies.⁷ Though modest in comparison with the English and American figures, this is rather impressive by French standards for a book in sociology.

The topics of the three works have an obvious common feature. All deal with urban districts located close to the city center, populated mostly by wage-earning and low-income inhabitants—and, most important, undergoing a process of radical urban renewal. In other words, when the surveys took place, their subject matter was about to disappear. The places did differ in size. The large borough of Bethnal Green had a population that had decreased from 108,000 in 1931 to 54,000 in 1955, mainly as a consequence of a consistent policy of demolition and rehousing by the London County Council. Between those years, 40,000 people were removed to council estates, most of them located outside the county of London. When sociologists stepped in, clearance was at its height, and families were being actively displaced to a remote estate, called “Greenleigh” in the book for anonymity. The West End was “an inner city Boston neighborhood” (*UV*, 6) with no administrative boundaries and a population that had decreased from 23,000 in 1910 to 7,000 in 1957. Official classification as a “slum area” had taken place in 1951 and a redevelopment plan had been approved in 1956. Two years later, a private firm started compulsory purchases and demolition. Within the next eighteen months, the district was emptied and its inhabitants scattered over the Boston area, except some 10 percent who accepted a move to a suburban public housing project. Finally, the “Ilot 4” upon which Coing focused had only 6,200 inhabitants but was located in an *arrondissement* where 80,000 out of 165,000 inhabitants dwelled in *îlots insalubres* (clearance areas), supposedly bound for demolition. Families being removed from Ilot 4 were offered rehousing in new council flats built on a neighboring site, but eventually most of them would not or could not move there and found accommodation in other “slums” nearby or in the periphery of Paris.

WORDS

What kind of places were those districts? The official answer was unequivocally “slums,” or the current legal euphemism. The designation of certain areas as slums and the path for urban renewal were not improvised by local authorities: a host of charity workers, sanitarians, planners, and reform-minded politicians had been developing the cognitive and statistical background for representing “slums” and identifying clearance areas since the beginning of the century. Legislation for renewal and public funds to carry it out had followed at unequal paces in each of the three countries, delaying plans, but by the early 1950s both were in place. In Boston, pressures from developers sped up the process. In such a context, the words sociologists chose to describe places were of special importance.

For Young and Willmott, Bethnal Green was “a working-class quarter” (*FK*, 93). Most of the people who lived in the borough, they argued, “belonged to the ‘working class,’ in the sense that they were predominantly employed in manual work, in such locally important industries as furniture, clothing, transport, docks, and engineering” (*FK*, 13). Culture also was involved: “The tone of the district is set by the working class” (*FK*, 94). Bethnal Green was loosely divided into various units, which sometimes amounted to a few streets: these were “the villages of the borough” (*FK*, 110). The authors did not bother to make further spatial distinctions, and their material—

⁷ Information kindly provided by Patrick Merrant, Editions de l’Atelier.

samples of families surveyed with a questionnaire, and firsthand observations of daily life—was not located precisely, as the same patterns were supposed to prevail in all parts of the borough.

Gans's approach differed slightly: he first described the West End as "an urban village," an area "in which European immigrants—and more recently Negro and Puerto Rican ones—were trying to adapt their nonurban institutions and cultures to the urban milieu" (*UV*, 4). But when it came to deciding whether it was better to describe "the way of life" of the group on which he focused—second and third generation Italian Americans who had moved from the North End to the district in the 1930s—by "ethnicity" or "class," he concluded: "the West Enders' way of life resembles that of other working-class populations" (*UV*, xi). Consequently, the author described them as "a working-class group." Such a decision, which seemed self-evident in Young and Willmott's intellectual environment, obviously had to be justified for Gans's audience.⁸

In that respect, the British sociologists were closer to their French colleague Coing than to the American Gans, in spite of the differing resources that natural languages provide to social scientists. In his study, Coing dealt with "an old *quartier populaire*, very characteristic and compact" (*RU*, 24). Those words need a brief comment. According to the context, *quartier* can mean either a rather large official district of Paris or the neighborhood where one lives, with a possible connotation of "community." *Populaire* is the adjective form of *peuple*, which here means workers of any description as opposed to *les bourgeois* and eschews *la classe ouvrière* and the political connotations involved. Nevertheless, "one dominant group"—*les ouvriers*—would put its mark on the area (*RU*, 73), which was "a natural unit of social life" (*RU*, 18), "a cultural area [*une aire culturelle*]" (*RU*, 80), where "a comprehensive and consistent way of life has developed" (*RU*, 24–5). The inhabitants themselves would say: "it is a true village" (*RU*, 73). Neither Coing nor Willmott and Young needed to justify this view to their readers.

Two terms then are crucial to the description: "village" and "working class"—alternatively *peuple*. It was obvious to the sociologists that they were dealing with natural social units closely knit by internal social ties, as hidden in the vast metropolis as tiny settlements in the countryside. Those villages had been there a long time, which allowed a set of customs to take shape over several generations. There dwelled ordinary people, workers and their families: the culture of the village, traditional by definition, was that of the working class. Such a description was a matter of immediate perception, and the only conceivable agenda was to research how those communities held together and reacted to their planned erasure.

INNOVATION

This vocabulary and vision were rather new in the social sciences. Willmott and Young stressed that novelty: "[What we found] was rather different from a popular view of what modern metropolis is like. Bethnal Green is not so much a crowd of individuals—restless, lonely, rootless—as an orderly community based on family and

⁸ One can argue that Gans's discussion of "ethnicity" and "class" was mostly a matter of rhetoric. He says today: "My tie to the Chicago school was almost nonexistent; I studied with Lloyd Warner and began with his notion of class, and of class culture." Gans, letter to author, 11 July 2001.

neighborhood groupings.”⁹ The many earlier monographs on, and surveys of, poor, deteriorated, ethnic, or working-class inner-city areas did not word their findings in that manner. Though reporters and other observers had sometimes used the village metaphor, concern with personal demoralization and urban deterioration had prevented scientists from seeing such places as “neighborhoods,” “communities,” *quartiers*, or “villages.”

For instance, the 1921–1929 research program run by Ernest W. Burgess in the University of Chicago’s sociology department, with funds from the Local Community Research Committee, was referred to as Chicago Local Communities and was designed to discover or illustrate the laws that ruled the birth, life, and death of possible communities in the “natural areas” composing the big city. Most of the cases Robert E. Park and Burgess’s students researched were chosen in the “zone of transition” or “deterioration area” that had been defined by the model Burgess published in 1924. Only two of these research projects produced a book. Louis Wirth’s *The Ghetto* (1928) showed that the tightly knit community transplanted from eastern Europe inevitably melted away as younger generations moved out from Maxwell and Halsted Streets and Americanized in the suburb of Lawndale. Harvey Zorbaugh’s *Gold Coast and Slum* (1929) demonstrated that as large a district as the Near North Side, because of its ecological and social diversity, could not possibly be described as a community. If something like a community existed anywhere in the city, it was among the wealthy in the apartment blocks along Lake Michigan, certainly not in the cheap hotel district, where isolated individuals constantly drifted, nor in Little Sicily, where social disorganization prevailed.¹⁰ In spite of Burgess’s once close ties with social work and his current interest in the “community organization movement,” he saw powerful ecological and cultural forces at play in the city constantly doing away with communities. The sense of community and neighborliness thought to be the basic feature of the Old World villages from which the immigrants came and the midwestern towns in which their academic observers were born could only be found in cities among the affluent suburban neighborhoods such as those Roderick D. McKenzie observed in Columbus, Ohio, and Seattle, Washington. The language of the neighborhood was then discarded by 1920s Chicago sociologists in their account of inner city districts, not to speak of the term “working class” which had no meaning in a society supposedly made up of Negroes and Jews, Germans and Italians, and any description of “ethnic” or “racial” group.

For their parts, British and French social scientists did not in general hesitate to identify the poorer districts in cities as “working class” or *ouvriers*, following common language usage in this respect. Charles Booth, the shipowner, conservative reformer, and amateur sociologist who surveyed the people of London for seventeen years, noted that in Bethnal Green, “artisans take the leading place,” in spite of the “semi-criminal class” in the black spots of his poverty map (1889). Maurice Halbwachs, a high school teacher in philosophy who belonged to the Durkheimian school and was a soft socialist, spoke of the large slum later to be included in the Ilot 4 as “a working-class block” (*une cité ouvrière*) with “true proletarians in the street” (*de vrais*

⁹ Michael Willmott and Peter Young, *Family and Class in a London Suburb* (London: Routledge & Kegan Paul, 1960), vii.

¹⁰ Louis Wirth, *The Ghetto* (Chicago: Univ. of Chicago Press, 1928); Harvey W. Zorbaugh, *Gold Coast and Slum* (Chicago: Univ. of Chicago Press, 1929).

prolétaires dans la rue).¹¹ But if in both cases workers were in the picture, community was definitely out of it. Poverty and isolation for the English observer, lack of social ties and “collective representations” for the French one, were supposed to characterize those places and people. “Working class” and “community” did not fit together on the urban scene.

If innovation in vision and vocabulary is granted to our authors, how to make sense of it?

SITUATIONS

Books on studies in the social sciences usually are not as closed as the black boxes of theorems and experimental results in the natural sciences. Authors of the former often explicitly state the questions they aim to answer and, by the same token, relate the conversations in which they are involved and the persons or groups to whom they speak. They refer to sources of information and inspiration and put forward the authorities on which they back their empirical statements and interpretations. Contexts, in short, show through the surface of the text. But what can be found within these books is only a clue to a broader situation that needs to be investigated by other means.

One of the main tasks of the sociology of science is to reconstruct the questions a scientific text intended to answer. Some questions are explicit, though we should not presume they are as plain as they seem, but many others may be lost to us. In seeking to reconstruct those questions, one can think of them as “the brief” the scholar has accepted for investigation,¹² a notion pointing to the social relationships that give birth and meaning to any technical, artistic, or scientific program. The brief is mostly defined independently from the author: it takes shape in institutions, scientific or mundane, and personal interactions, and includes a host of references to the living and the dead. Those elements define issues and words, working conditions and time schedules, evaluation processes and audiences—the many constraints binding any work. However, even when talent is bought by money, the relationship should not be seen as one-sided. Most of the time, scientists—like painters or engineers—collectively act upon what worldly authorities ask of them. Besides, an individual author fine tunes the definition of the brief. He or she may reword a commission, use the job as a means to personal ends, and when fulfilling a task, choose among the available resources. A scientific brief thus is the result of a series of interactions, which has to be as fully described as possible.

YOUNG, WILLMOTT, AND THE INSTITUTE OF COMMUNITY STUDIES

Family and Kinship was one output of a research program supported by the Ford Foundation in the Institute of Community Studies (ICS) during the late 1950s.¹³ The institute had been created in January 1954 at the initiative of Michael Young, a former head of the Labour Party Research Department. The ICS had no formal ties with any

¹¹ See Christian Topalov, “Maurice Halbwachs, photographe des taudis parisiens (1908),” *Genèses* 28 (1997): 128–45.

¹² I have borrowed this useful notion from art historian Michael Baxandall. Baxandall, *Patterns of Intention* (New Haven: Yale Univ. Press, 1985), 32–6.

¹³ See Tiratsoo and Clapson, “Ford Foundation and Social Planning” (cit. n. 3); and Peter Willmott, “The Institute of Community Studies,” in *Essays on the History of British Sociological Research*, ed. M. Bulmer (Cambridge: Cambridge Univ. Press, 1985), 137–50.

academic institution, but on the advisory committee were Richard Titmuss, professor of social administration at the London School of Economics, John Bowlby, director of child guidance at the Tavistock Institute of Human Relations, and Edward Shils, an American sociologist who had been trained at Chicago University and was instrumental in making its sociology known in Britain. The appointment of Shils to the committee may have been related to his being an adviser to the Ford Foundation, then keen to promote useful empirical social science in postwar Europe. It was to the foundation's Behavioral Sciences Program that Young and Titmuss formally applied for support for the ICS; in September 1955, the institute received an award of \$70,000, to be paid over three and a half years, for "studies of contemporary British society." It was the proposal for the Bethnal Green project that had convinced the Ford Foundation to provide support, and now researchers were ready to begin it. Peter Willmott, who had been hired the previous year, embarked with Young on the study of kinship, while Peter Townsend focused on old people, Peter Marris on widows, and Enid Mills, later, on the mentally ill. Within an impressively short span of time, their findings appeared as ICS reports, published by Routledge and Kegan Paul.¹⁴

Neither Young nor Willmott had the usual academic background and credentials. Young (1915–2002)¹⁵ studied law at the University of London and was admitted to the bar. A committed socialist of the Fabian type, he became the director of Political and Economic Planning—a major think tank during World War II—and in 1945 was appointed secretary of the Research Department of the Labour Party, where he helped shape and assess the welfare state and full-employment policies. He left this position in 1951, worked at the Tavistock Institute on the social consequences of social housing, and then launched his own research institution. Willmott (1923–2000),¹⁶ the son of the owner of a small motor-repair workshop and garage, left school at the outbreak of the war, when he was sixteen, apprenticed as a motor mechanic for a while, and then spent the rest of the war as a miner's helper in the Rhondda Valley. Later he briefly attended Ruskin College, Oxford, which provided an adult education curriculum. Temporary clerical jobs in London followed until Willmott approached Young. Appointed a research assistant in Young's department in 1948, Willmott wrote and edited a weekly briefing for party workers there. In 1954, Willmott enrolled at the London School of Economics as an external student and left his salaried position in the Labour Party to join Young's newly created ICS. Thus began a lasting collaboration.

GANS AND THE CENTER FOR COMMUNITY STUDIES

The Urban Villagers, like *Family and Kinship*, was the outcome of a larger project. The National Institute of Mental Health (NIMH) had commissioned a psychiatrist,

¹⁴ Young and Willmott, *FK* (1957 edition) (cit. n. 2); Peter Townsend, *The Family Life of Old People* (Glencoe, Ill.: Free Press, 1957); Peter Marris, *Widows and Their Families* (London: Routledge & Kegan Paul, 1958); Enid Mills, *Living with Mental Illness* (London: Routledge & Kegan Paul, 1962). For an assessment of the project, see Jennifer Platt, *Social Research in Bethnal Green* (London: Macmillan, 1971).

¹⁵ See *International Who's Who 1996–97* (London: Europa Publications, 1996), 1705; Geoff Dench, Tony Flower, and Kate Gavron, eds., *Young at Eighty* (Manchester: Carcanet, 1995); Tiratsoo and Clapson, "Ford Foundation and Social Planning" (cit. n. 3).

¹⁶ See *Who's Who 1999* (London: A&C Black, 1999), 2177; Jeremy Mitchell's obituary, *The Independent*, 14 April 2000; Phyllis Willmott, letter to Gérard Hery, 7 March 1990 (kindly provided by Phyllis Willmott).

Dr. Erich Lindemann, to run a survey on "Relocation and Mental Health: Adaptation under Stress," to be conducted at the Center for Community Studies (CCS) of the Harvard Medical School and Massachusetts General Hospital. Most of the staff involved were physicians interested in "the hazards to health and emotional well-being involved in the forced relocation of families with different types of ethnic and cultural origins"—and convinced that knowledge of community life was part of "a scientific foundation for health and social planning."¹⁷ Therefore, a sociologist was needed. Leonard Duhl, the psychiatrist at the NIMH, suggested that Herbert J. Gans be hired, on the recommendation of David Reisman, who had taught Gans and appreciated his recent work on a Chicago suburb.¹⁸ Lindemann and Marc Fried appointed Gans an "assistant sociologist" to undertake the inquiry in the Boston West End with minimal guidance. Even though Gans spent only eight months in the area, he had the opportunity to talk with members of the center—notably Lois Paul and Laura Morris, a specialist in child welfare problems, and other social scientists in the Boston area, such as Martin Meyerson, David Riesman, John R. Seeley, and Ezra F. Vogel. He also consulted with William F. Whyte, whose already "classic" study of the North End (1943) gave Gans a special interest in the neighboring district.¹⁹

Herb Gans (born 1927) came from Germany to Chicago with his family when he was a child.²⁰ He graduated from the University of Chicago in 1947, earned a master's degree in the social sciences in 1950, and later joined the University of Pennsylvania's Department of City Planning, as a research associate in the Institute for Urban Studies (1953–1957) and a department lecturer (1956–1957). Just after completing his Ph.D. in 1957, he spent a whole summer in "a grand sociological tour of Europe," which led him to London—where he met with Young and Willmott at the ICS—Glasgow, and Manchester, and outside Britain to Germany and Denmark. Upon his return, he settled in Boston to do the West End survey in 1957–1958, then returned to the University of Pennsylvania as an assistant professor of city planning (1958–1961) and completed his report there in November 1959.²¹ By 1962, it had been transformed into a book; two years later, Gans moved to Columbia University's Teachers College as an associate professor of sociology and education.

When Gans took the Boston job, in terms of institutional affiliations and interests he was more of a planner than a sociologist—or, as he phrases it, a "sociologist in planning."²² When he had discovered that his favorite topic—the sociology of knowledge—would not help him earn a living, he had chosen for his master's thesis to study Park Forest, Illinois, a new Chicago suburb mainly populated by families with a Jewish background.²³ After earning his master's, Gans had taken a series of short-term positions in various planning agencies: the American Society of Planners (1950), the

¹⁷ Erich Lindeman, foreword to *UV* (cit. n. 4), v, vi.

¹⁸ Gans, letter to author, 8 Nov. 1999.

¹⁹ Gans, "The Urban Villagers: A Study of the Second Generation Italians in the West End of Boston" (Institute for Urban Studies, University of Pennsylvania, Center for Community Studies, November 1959, draft not for publication, 256 mimeo, iii; and Gans, letter to author, 6 Aug. 2001.

²⁰ Gans, interviews with author, New York, 31 Oct. and 1 Nov. 1999; and "Bibliography and List of Publications—Herbert J. Gans," resumé provided to author, [fall 1999], 26. See also Gans, "Relativism, Equality, and Popular Culture," in *Authors of Their Own Lives*, ed. B. M. Berger (Berkeley: Univ. of California Press, 1990), 432–51.

²¹ Gans, "The Urban Villagers" (cit. n. 19).

²² Gans, letter to author, 11 June 2001.

²³ Gans, "Political Participation in a Suburban New Town: Park Forest, Illinois" (master's thesis, Univ. of Chicago, 1950).

Chicago Housing Authority (1950–1951), the planning firm PACE Associates (1951–1952), and the Division of Slum Clearance of the U.S. Housing and Home Finance Agency in Washington, D.C. (1952–1953), at a time when a huge national program of clearance and urban redevelopment had been reaching its greatest momentum. As for Gans's doctoral dissertation, it dealt with planning for recreation in urban settings,²⁴ an aspect of mass consumption and suburban culture in which he had long been interested.

COING AND THE CENTRE D'ETHNOLOGIE SOCIALE ET PSYCHOSOCIOLOGIE

Rénovation urbaine et changement social did not result from any direct commission from outside the university. It was written as a doctoral dissertation by a student of Paul-Henry Chombart de Lauwe's and defended at the Faculté des Lettres et Sciences Humaines of Paris—the Sorbonne—where a sociology curriculum had been added in 1958. Coing did his work as students in French universities often did, and still do—with minimal guidance from his tutor and few contacts with other scholars. However, he did work in relation to Chombart's group, which was then fully mobilized to work for French public authorities in urban planning and borrowed a lot of its concerns and vocabulary from them. Chombart de Lauwe (1913–1998), born to a northern aristocratic family, had initially worked with minimal training in ethnology at the new Centre d'Etudes Sociologiques of the Centre National de la Recherche Scientifique, which was mostly staffed with scholars who, like Chombart, had unconventional backgrounds. Here empirical sociology developed while university professors still ignored and usually despised it. Chombart de Lauwe was involved in social networks rooted in catholic and bourgeois milieus, which, after the experience of the Résistance, diversely invested in social reform. He was interested in the behavior of the *classe ouvrière* and started investigating working-class neighborhoods in Paris and collecting data on the city at large in 1949–1950, a time when no planning body in charge of the whole metropolis existed. Chombart's personal relationship with Robert Auzelle, an architect, planner, and top official at the Ministère de la Construction et de l'Urbanisme, who would teach at the Institut d'urbanisme and promote the neighborhood unit idea, had been instrumental in establishing working contacts with town planning authorities. A series of grants from various government agencies over the next fifteen years helped Chombart and his team develop research on urban topics in two institutional settings: the Centre d'Ethnologie Sociale et Psychosociologie (CESP), established in 1959, and the Centre d'Etude des Groupes Sociaux, which had been created five years earlier. The former was in the CNRS—from 1960 on also in the Ecole Pratique des Hautes Etudes, following Chombart's appointment as *directeur d'études* there—and dedicated to *recherche fondamentale*; the latter center was a private nonprofit association doing *recherche appliquée*—even though individuals, projects, and money rather freely moved from one to the other. Students such as Coing were attached only to the CESP and worked with no funding; their monographs on working-class neighborhoods belonged to a series mostly researched and written by paid staff.

Henri Coing (born 1936) was then a young Jesuit who wished to step back for a

²⁴ Gans, "Recreation Planning for Leisure Behavior: A Goal-Oriented Approach" (Ph.D. diss., Univ. of Pennsylvania, 1957).

while since Rome had recently put an end to the French experiment of working priests (1954) and suppressed any left-wing leaning within the Society of Jesus (1954–1957).²⁵ Apart from his church training, Coing had a *licence* (a bachelor's degree) in philosophy (Montpellier, 1957–1959) and had taken a course in psychology (1963); despite his lean reading in sociology, he decided to do doctoral research in that discipline. This option and the choice of the thirteenth arrondissement of Paris were made upon advice from a Jesuit friend who lived in the neighborhood and worked there with trade unions and tenant associations in which members of the Communist Party were active. Coing had thus chosen for his fieldwork, without knowing it, a district Chombart's team had been studying for years in close relation with a notably progressive parish at the Porte de Choisy, a group that included a number of former—or inconspicuously active—working priests Chombart had known through his acquaintance with the Mission de France. As for the theme to be studied, it was in line with Chombart's concern with urban planning. The purpose was to both understand what was happening to a working-class neighborhood undergoing a process of clearance and draw useful lessons for improved planning—even though neither the senior nor the junior scholar had complete confidence in the readiness of redevelopment authorities to take his conclusions into account.

THE SHAPING OF BRIEFS

The three scientific briefs I am trying to make explicit stemmed from conditions that while diverse nevertheless had striking features in common. In each case the study was part of a project that had been defined with some nonacademic funding agency—even if Coing's work was more of an independent doctoral endeavor. Though the wording of the larger program was rather imprecise, research was wholly committed to the enlightenment of public policies. The relation was indirect in several ways. Local redevelopment agencies were not involved—which was rather sensible since they obviously had not the slightest intention of making any changes to projects already underway. Private and public funding agencies were remote from the field and had rather broad goals (all related to justifying their spending to their own upper authorities), among them the redirection of social scientists toward useful aims and the production of data and ideas that could be used by national decision makers. Scientists—social scientists, public health specialists, and urban planners—held positions on both sides of the commissioning process: as members of the Ford Foundation Committee and the National Institute of Mental Health and as informal advisers to the Ministry of Housing. Funding agencies did not dictate projects; they selected them. They expected applicants to go most of the way and define goals that fit with the agencies' concerns as well as methods that were in line with their vision of science. Furthermore, each special inquiry was usually part of a broader program from which it got most of its acceptability and behind which it was partly hidden. Those features of the interaction between funders and researchers did much to define the profile of the applicants and at the same time left the latter a great deal of independence in the wording and execution of the commission.

None of the institutions in which research was conducted was part of a university and all were brand new. The ICS in Bethnal Green was a private nonprofit body

²⁵ H. Coing, interview with author, Paris, 14 Dec. 2000.

dedicated to policy-oriented research, CCS in Boston a tiny appendix for the social sciences in medical research institutions, the CESP in Paris a small unit in a then-peripheral school of postgraduate teaching. The relation of those institutions to established academia was slight and potentially conflictual. When they condescended to give an opinion on those they saw as well-funded outsiders, certified sociologists usually were highly critical. British academic referees to the Ford Foundation for Young's project did what they could to sink it. Meyer Fortes, professor in social anthropology at Cambridge, damningly praised Young as "an enthusiastic, even idealistic, though slightly sentimental amateur" and "an ideal teacher and organiser in a Department of Adult Education."²⁶ Even Talcott Parsons, who supported the project, saw it as "a maverick attempt outside the regular framework," which, for that very reason, might help British sociology to shake Oxbridge drowsiness.²⁷ As expected in such a situation, the illegitimate newcomer for his part criticized the "deplorable jargon" of established sociologists and planned to "make social science intelligible to the interested layman."²⁸

Interestingly, the authors shared some biographical features. When they started their surveys, they were rather young: while the institution builder Young was thirty-eight, Willmott was thirty-one, Gans thirty, and Coing twenty-seven. Most of them had only limited or informal training in the discipline of sociology, and none held any academic position. With his master's, Gans had the highest degree in sociology of the set but had been pursuing the calling and curriculum of a planner. Coing had set himself to become a doctor in sociology but was still better trained in theology and philosophy. Young held a degree not in sociology but in law, while Willmott had not yet earned a degree of any kind (not until after he had published his first book would he receive a bachelor's in sociology from the University of London). Practical policy makers, Young and Willmott knew much of Fabian literature but almost nothing of the sociological tradition. As a consequence, our authors were unaware of, or little concerned with, any of the manners required for admission to the ranks of academia. Yet at the same time they were keen to use the scientific authorities with whom they had to become quickly acquainted to back their own work. To a large extent, the authors' systems of reference result from this tension.

Defining our sociologists negatively by their distance from legitimate science is not enough, however, since they had either previous professional experience or one other main professional involvement. Furthermore, all of them had definite commitments on the left in various spheres of social-political action. Both Young and Willmott seemed disappointed by active politics from behind the scene and were concerned with the indifference institutions, including those of the fought-for and recently birthed welfare state, showed to ordinary people. As early as 1954, Young published a piece in the journal of the British urban planners to underscore the scarce attention "the planners" gave to "the planned."²⁹ He and Willmott would speak to planners for

²⁶ Fortes to Bernard Berelson, 13 July 1955, Ford Foundation Archives, quoted by Tiratsoo and Clapson in "Ford Foundation and Social Planning" (cit. n. 3).

²⁷ Parsons to Francis X. Sutton, 23 March 1955, Ford Foundation Archives, quoted by Tiratsoo and Clapson in "Ford Foundation and Social Planning" (cit. n. 3).

²⁸ "Application to the Ford Foundation by the Institute of Community Studies, London, England," Ford Foundation Archives, quoted by Tiratsoo and Clapson in "Ford Foundation and Social Planning" (cit. n. 3).

²⁹ Young, "The Planners and the Planned: The Family," *Journal of the Town Planning Institute* 60 (1954): 134-42.

many years to come, as would Gans. A young sociologist and would-be urban planner himself, Gans intensively discussed planning and democracy in liberal journals such as *Commentary*, *Phylon*, and *Dissent*. One of the purposes of his master's thesis was to rehabilitate a suburban way of life and culture despised by high culture critics; he later studied leisure, popular culture, and the West Enders with the same intent. In 1953, he argued in the *Journal of the American Institute of Planners* for political participation in planning and noted that the people more likely to participate when it came to urban renewal were "those who suffer from it by having to leave their homes and neighborhoods."³⁰

Our sociologists' briefs resulted from a double bind. They were asked to, and indeed did, believe in the necessity of planning and its capacity to improve. Yet at the same time, they felt that planners and decision makers were not hearing the demands of the voiceless groups. Hence the rationale for sociology: going out to study ordinary people, learning about their genuine needs and aspirations, and making those known to planners—in other words, fighting the class prejudices of people who think they know what is good for others without asking them.

Gans was the most explicit and outspoken: "I have tried to describe the way of life of lower level people as they might describe it themselves if they were sociologists. In a sense, I am reporting to the upper level for them and urging that they be given more consideration when policy decisions are made" (*UV*, x). The sociologist should be—"at least from the liberal perspective"—"an informal spokesman for groups who themselves lack the power to voice their demands in the larger society" (*UV*, 343). Though less vocal, Young and Willmott implied the same role for themselves when they insisted that "the purpose of rehousing is to meet human needs, not as they are judged by others but as people themselves assess their own" (*FK*, 198)—an assessment recorded by objective inquiry. Coing seemed less willing to be the voice of the voiceless. He argued that the sociologist should have no opinion of his own but should try "to bring to light the representation of social life or of the city that [each option] implies and the consequences of that option" and "to establish a dialogue and to find some common language" (*RU*, 227). A few years earlier, Chombart de Lauwe had written: "It is necessary to bring closer together those who act and those who are acted upon. Disinterested research can help greatly. . . . Very often it is enough for scholars to give expression to the aspirations of the population among which they live."³¹

The three books have a concluding chapter showing that at the very center of the briefs lay the discussion with planners. Beliefs strongly come into play here, as do the specific features of the redevelopment process and expectations of being heard or not by decisions makers. Gans's conclusions were clear-cut: "I quickly became convinced that the redevelopment of the area was unjustified, and that the planning was poorly handled" (*UV*, 343). The West End was not a slum, but a low-rent district. Total clearance would have been justified if the inhabitants had been rehoused on the spot in modern buildings "in such a way that the existing social structure could have been preserved" (*UV*, 316–7). But renewal as planned sought to get rid of a population whose behavior was considered undesirable and put a highly central location to a

³⁰ Gans, "Planning and Political Participation: A Study of Political Participation in a Planned Town," *Journal of the American Institute of Planners* 19 (1953): 3–9, on 8.

³¹ Chombart de Lauwe, "L'Etude de l'espace social," in *Paris et l'agglomération parisienne* (Paris: PUF, 1952), 1:25.

more profitable use. By contrast, neither Young and Willmott nor Coing ever questioned the legitimacy of erasing slums and rehousing their inhabitants—with public housing in both cases. Still, while they shared the progressive common view of the time on that point, they dissented about means and procedures; their respective views on the viability of old neighborhood communities shaped the solutions they suggested. To Young and Willmott—as to Gans—such communities were strong and sustainable. Only unwise planning broke them up by displacing people to remote estates and scattering nuclear families. Neighborliness did not need planned “neighborhood units”: “In such a district [as Bethnal Green], community spirit has not to be fostered, it is already there” (*FK*, 199). If the preferences of people were to be met, planners ought to remove “street and kinship groupings as a whole . . . without squandering the fruits of social cohesion” (*FK*, 198), or even better, they should rehouse people in their own neighborhood. To Coing, on the other hand, urban renewal only accelerated an unavoidable process of change bound to dismantle local communities: “The *quartier* as self-contained community belongs to bygone days” (*RU*, 231–2). His position seems uneasy, however, as he strongly supported rehousing on the spot, which “does not break all at once the threads which now link [the inhabitants] to society” (*RU*, 249), and he also suggested there should be planned “small units of basic social life” that would allow people to invent new forms of sociability (*RU*, 233). This uneasiness may be explained by the fact that those planning recommendations were not his own—one cannot find them in the doctoral dissertation; they were added to the book at Chombart de Lauwe’s insistence and threat that it would not otherwise be published.³²

In retrospect, it is apparent that our sociologists were waging an uphill battle. At that time, the profession of urban planning had an assertive rationalizing and modernizing vision, elaborate procedures, and reasonable funding. Through agreements with local authorities, planners could pursue their own goals of social cleansing and improvement, not to mention open new opportunities for private developers. As a consequence, our authors did not seem to be highly optimistic about what could result from their findings. Why then should they have tried to influence planners? The rationale is that among planners, architects, officials, and intellectuals interested in urban issues there were some groups thinking along the same lines as the sociologists. A clue to such possible alliances can be found in the fact that none of our authors was content with publishing a book; all followed suit in journals that would publicize their views to larger audiences, including professional ones. Young and Willmott made their findings and opinions widely known through the media—notably the BBC; through the *Town Planning Review*, they specifically addressed the planning profession. Gans was already well acquainted with “a handful of progressive planners.” Before he completed the report on the West End, he published his practical conclusions in the *Journal of the American Institute of Planners* (1959), as Marc Fried did later (1961). Their fight soon led to the establishment of Planners for Equal Opportunity, a professional association seeking alternative methods to those of the conservative American Institute of Planners. Coing, by contrast, had no previous interest in planning nor knowledge of planners. In the process of his study, though, he met with a number of middle-rank urban renewal officers who were working in redevelopment agencies in Paris, its suburbs, and the northern cities and were concerned with the social consequences of their actions. Some of them belonged to a small leftist political

³² Coing, interview (cit. n. 25).

party (Parti Socialiste Unifié) and had built a network around a journal dedicated to local issues (*Correspondance municipale*); Coing became actively involved in their conversation.³³ He and Chombart de Lauwe used journals widely circulated among Catholic intellectuals leaning to the left; unlike his academic patron, Coing was sharply critical of the current belief among planners in the power of neighborhood units to create villagelike local communities.³⁴ Whatever their particular relationships with planners may have been, all of our authors had found the means to publicize their conclusions among the general public and in some circles of the planning profession. All of them, too, would have the bittersweet satisfaction of having been right too early.

OBSERVING

With this kind of study, fieldwork comes first. When I asked Herb Gans about his previous readings, he insisted: "I learnt fieldwork with Everett Hughes, I could not stand the theory. . . . You know, I am very interested in theory, but I am not interested in ancestor worship. . . . Theory comes out of the data, or comes out of your interaction with the data. . . . Obviously you are influenced by everything you have read, but the study comes out of what you see."³⁵ Similarly, Henri Coing recalls: "I started head-on with my fieldwork. I discovered literature on the way."³⁶ We are dealing with authors who claim that above all else, they reported what they saw.

Due consideration was given to methodology, however. Those uncertified sociologists had to prove that their statements had some "representativeness," and the three books include a methodological appendix. Young and Willmott, as well as Coing, used some figures to back their conclusions. In spite of such gestures toward putting hard science in the foreground, the heart of the findings came from going to places and talking with people. The issue then is the categories in which observation happened to fall—in other words, the glasses through which people and things were seen or, to phrase it differently, the available cognitive resources used to fulfill the briefs. Evidence can be found by placing the three works in a series with those explicitly referred to in footnotes and bibliographies. The status of references is indeterminate: they can be only formal invocations of supposedly accepted authorities or more. Beyond the mere listing of quotations, we have then to try to describe the direct or indirect interactions actually taking place between our authors and the invisible college they mobilized in their work.

Strong similarities between the authors' descriptions of "working-class neighborhoods" exist, but a difference in emphasis is noticeable. Coing perceived the local group as a whole: he underscored the fact that people spoke of themselves as "us," and he wanted to make sense of the "collective representation" and memory that saturated the discourse of his informants. Gans put the stress on peer groups as the main feature of local sociability. Defined by gender and age, they resulted from the internal organization of the family, which basically shaped the community. Young and Willmott made the family the starting point of their study and discovered that extended

³³ Ibid.

³⁴ Coing, "Le 13e aujourd'hui," *Informations sociales* (June 1964); Chombart de Lauwe, "L'avenir de Paris," *Esprit* 32 (Oct. 1964); Coing, "Quartiers anciens et ville moderne," *Projet*, no. 9 (Nov. 1966).

³⁵ Gans, interview (Oct.) (cit. n. 20).

³⁶ Coing, "La Ville des sciences sociales," (paper presented at the EHESS Seminar, Paris, 9 Feb. 1993).

families and kinship networks were the organizing forces in the borough. Does that mean peer groups were unknown in Bethnal Green or family ties weak in the 13th arrondissement? It seems safer to relate the record of what was “seen” in fieldwork to the specific cognitive tools available to the sociologists in their different institutional and intellectual settings.

THE FAMILY

Concerns about the urban family and the conditions in which family life could be sustained and improved through adequate policies pervaded the environment of the three studies. If commonsense definitions of “family issues” were part of the brief, various brands of psychology and anthropology were embedded in the responses to it, as they provided both questions and answers.

To describe the family—especially the mother-daughter relationship and matrilocality—in learned terms, Young and Willmott mobilized Margaret Mead (1901–1978), who had explained sexual behavior in Samoa using a Freudian vocabulary, and A. R. Radcliffe-Brown (1881–1955), the Oxford social anthropologist who had studied kinship systems comparatively. They found other models of family structure in E. Franklin Frazier’s 1931 study of the black family—their only reference to a Chicago sociologist—and in current research on kinship in London by Firth and Bott (both inspirators and competitors). Solidly centered on British authors, the British Isles and Commonwealth, Young and Willmott’s readings left Americans aside, not to mention Continentals. The wave of compulsory quotation from American sociology had not come yet.

In the mid-1950s, the marriage of psychology and social anthropology was the order of the day at the Institute of Community Studies and related research institutions in London. The Tavistock Institute formed one of the nodes of the network. Young had worked there for a time and had placed John Bowlby (1907–1990)—a renowned psychologist and expert on childcare—on the ICS Advisory Committee. In 1952, Elizabeth Bott began studying twenty London families at Tavistock, and in 1955, she published her central hypothesis on the relation between conjugal roles and social networks.³⁷ An anthropologist, she cooperated in her inquiry with social psychologists and psychoanalysts. A number of family specialists advised her; in her acknowledgements, she mentioned Young and Willmott and underlined the roles of Bowlby and Firth.³⁸ The other node was the London School of Economics (LSE). Raymond Firth (born 1901), recently returned from Polynesia and Malaysia, was teaching anthropology at the school in Bronislaw Malinowski’s chair. As early as 1947, Firth had started to investigate kinship in London. Partial publication of his findings took place in 1956 as a social anthropology monograph of the LSE,³⁹ and a grand British-American comparative project followed in 1959. Over the next ten years, research developed on a more manageable scale in London middle-class suburbs. The LSE, still a den for Fabian socialists at that time, provided closer intellectual support to the ICS through Richard Titmuss (1907–1973), who had also been interested in the family—

³⁷ Bott, “Conjugal Roles and Social Networks,” *Human Relations* 8 (1955): 345–84. Final results in idem, *Family and Social Network* (London: Tavistock, 1957).

³⁸ Bott, acknowledgements in *Family and Social Network*, ix.

³⁹ Firth, ed., *Two Studies of Kinship in London* (London: Athlone Press, 1956).

from an economic and demographic standpoint—before he became an authoritative advocate and theoretician of the welfare state in the early 1950s.

Gans made less use of anthropological references and much more of those belonging to the “Culture and Personality” debate going full speed in American sociology at the time, resorting to a mix of anthropological, sociological, and psychological vocabularies to explore what was called “the American national character.” The social psychology of George H. Mead (1863–1931) formed part of the intellectual landscape, but even more so did the work of “such brave adventurers” as Margaret Mead and fellow anthropologist Ruth Benedict and those who sought “the application of a socially oriented psychoanalytic characterology to problems of historical change”—notably Erich Fromm.⁴⁰ The taming of psychoanalysis by American psychiatry—which had recently taken control of the International Psychoanalytical Association—was giving birth to new brands of progressive social psychiatry, ones fully represented in the Boston survey. Leonard Duhl (born 1926) was interested in the relationship of mental health to urban social conditions, and Erich Lindemann (1900–1974) worked at new therapy methods in social psychiatry. Young Gans was in contact with David Riesman (1909–2002), who had been, since the publication of *The Lonely Crowd* in 1950, on the cutting edge of the study of national character with loosely psychoanalytical concepts.

Such notions as “personality structure” and “family types” proved crucial to Gans’s interpretation of his observations. He suggested an ideal-typical opposition between “object-oriented” and “person-oriented” modes of behavior, personality types going hand in hand with family structures and the “subcultures” of classes. “The middle-class subculture is built around the nuclear family and its desire to make its way in the larger society” (*UV*, 246), while “the working-class subculture is distinguished by the dominant role of the family circle” (*UV*, 244). There, the segregation of sexes was the rule, hence the importance of peer groups. The opposition Gans drew was akin to a series of other then-current dichotomies, which he mentioned in his study: “ascription” and “universalistic groups” versus “achievement” and “particularistic groups” with Talcott Parsons, “locals” versus “cosmopolitans” with Robert K. Merton, “inner-direction” versus “other-direction” with David Riesman. Sociologists and others endlessly debated such concepts in the 1950s and 1960s and tested them in scores of empirical studies. Whatever nuances those various pairs of notions may imply, all of them matched up exactly with an opposition between the middle class—which most authors strongly believed to embody the true American character—and the class(es) below.

Coing’s approach to the family showed no awareness of what was being discussed in the United States or Britain. He took families as social units whose members did not need to be much differentiated, and he mostly referred to the statistical notion of *ménages* (households) and the nomenclature of *catégories socio-professionnelles* (social-occupational groupings), which only takes into account the occupation of the (usually male) head of family. Coing used data that the redevelopment agency had collected on rehousing to discuss family incomes and expenditures (*RU*, chap. 8), following a long tradition he learned about through Maurice Halbwachs’s book of 1933.⁴¹ Family responses to clearance were described typologically—“traditional behavior,” “frustrated ambition,” “tipsiness,” “controlled innovation” (*RU*, 143–

⁴⁰ David Riesman, preface to *The Lonely Crowd*, 2d ed. (New Haven: Yale Univ. Press, 1961), xii, xiv.

⁴¹ Halbwachs, *L’Evolution des besoins dans les classes ouvrières* (Paris: Alcan, 1933).

59)—but types were related to levels of skill and income rather than any social-psychological theory. The current French sociological and political debate in which Coing was actually interested concerned an emerging differentiation within the working class under the impact of technological change and mass consumption: a “new working class” was being born, one that was losing the traditional one’s specificity and isolation from society at large. In his minimal concern with the internal functioning of the family, Coing deviated from the work being done by Chombart de Lauwe and his wife, Marie-José, a psychosociologist who had given much attention to the impact of housing conditions on the psychological welfare of families. Nevertheless, when Coing described the families that were open to change, their social-psychological profile was surprisingly similar to the types characterized as “object-oriented” or by “a new partnership” in the two other studies: they were rather young and had stable employment, the wife played an active part in family decisions, and children’s education was a central concern for both parents (*RU*, 229–30).

Families were involved in various manners in the sociologists’ stories. Our “participant observers” who had a spouse and children lived with them in the neighborhood. When Willmott and Gans acknowledged the role their wives had played in their inquiry, they were not merely ritual statements.⁴² The men had needed their wives’ agreement and help to settle in and confront a rather new social environment, and without the women’s participation in observation, the authors knew they would have been entirely enclosed in male circles of sociability. Phyllis Willmott was not a silent companion but an ICS staff member who eventually became a social researcher and author in her own right. While living in Bethnal Green, she noted in her diary a wealth of observations on the daily lives of local housewives, and her husband and friend often used her comments in their book.⁴³ By contrast, Coing, being a bachelor, derived no interest in family roles from his personal experience. He lived in a suburban Jesuit institution, and his “access problem” had to be solved without the help of any female partner. He had been introduced to his informants through institutional networks—trade unions, social workers, the parish—and intensively used the main public space of male sociability: the local *bistro*.

The sociologists’ own marriages epitomized the “partnership” or “joint relationship that characterizes the middle-class family,”⁴⁴ according to the social-psychological theories with which Bott, Young and Willmott, Gans, and many others worked. Apparently, the families of the working class they observed were the exact opposite, with their “segregated conjugal pattern”: “Husbands and wives,” stated Gans, “come together for procreation and sexual gratification, but less so for the mutual satisfaction of emotional needs or problem solving” (*UV*, 51). Young and Willmott quoted Mayhew, Booth, Rowntree, and others, according to whom “the working-class man is a sort of absentee husband, sharing with his wife neither responsibility nor affection, partner only in the bed.” The only corrective to early urban explorers’ views was an

⁴² Gans acknowledged: “Iris Lezac, to whom I was then married, helped in the fieldwork and a number of the observations in the book are hers” (*UV* [cit. n. 6], xii). This was key to solving the “entry problem”: “My wife and I became friendly to our neighbors and other West Enders, spending much time with them in social activities and conversations that provided useful data” (*ibid*, 337). Willmott insisted on the role of his living in the borough with his “marriage family” in his grasping differences with the locals (*FK* [cit. n. 2], 13–4).

⁴³ “Bethnal Green Journal, 1954–1955,” unpublished manuscript kindly lent to author by Phyllis Willmott, who took direct part in the inquiry (see, e.g., 105–7).

⁴⁴ Respectively: Young and Willmott, *FK* (cit. n. 2), 25; and Gans, *UV* (cit. n. 4), 50.

acknowledgement that though the pattern still existed, things had greatly improved since then (*FK*, 19). By the 1950s, sociologists, psychologists, and social workers claimed a professional attitude of ethical neutrality; our authors, furthermore, were decidedly inclined to be advocates of the people they studied. Nevertheless, the feeling that their own families were not only different but far removed from the workers' on some line of evolution—or progress—seems to me to seep through the most objective statements.

THE COMMUNITY

Like the approach to the family, that to the community fell into recognizable national sociological patterns; it also led to very similar descriptions. The basic unit of community life had a spatial definition, a markedly narrow one. In Bethnal Green, inhabitants would say: "In our turning we . . . do this, that or the other." "Turnings" were the name by which the streets were known; in each of these "villages" lived some one hundred to two hundred people, who had relatives nearby (*FK*, 109). Sociability in Boston's West End also was limited to one street and the places where the peer group would meet (*UV*, 105); inhabitants of Ilot 4 were "at home" only in rue Nationale (*RU*, 82). In the three cases, the larger surrounding district represented more of an abstraction, except that people knew it comprised villages similar to theirs and was opposed in the same way to the other world, the city beyond. Most of the men seemed to work in the neighborhood. Respectable female inhabitants of the East End were not supposed to frequent "the Other End"—an immoral place (*FK*, 112 n. 1). Coing quotes an old lady saying: "[Going to] Paris? You know, I don't travel much" (*RU*, 45). "Us" and "them" coincided with "here" and "there." Spatial and cultural isolation from both the larger society and the rest of the city was one defining feature of the neighborhood community, stability another. People were supposed to have been there all their lives, even for several generations. Social homogeneity and the passing of time had developed a complex of social norms and feelings that defined the local culture. Shared memory also was involved. Young and Willmott underlined that "local history does not have to be learnt from books: it is passed on by word of mouth from parents to children" (*FK*, 113), and Coing insisted on the part that "collective memory" played in the building up of a consistent, if reconstructed, image of the neighborhood (*RU*, 23).

Were these closed neighborhoods "communities," and what future would they have had had there been no planned destruction? On this, our observers differed. Coing saw redevelopment as accelerating an inevitable process: for him, old working-class neighborhoods belonged to the past. By contrast, Young and Willmott as well as Gans believed that these neighborhoods would have been strong enough to hold had redevelopment been properly handled. These differing perspectives did not coincide with views on what defined a community. To Young and Willmott and to Coing, it was defined by social cohesion, shared experience and feelings. Gans, for his part, saw the peer group society as lacking the unifying forces and institutions that would have made it a community: "Indeed, only when the outside world discovered the West End and made plans to tear it down did its inhabitants begin to talk about the West End as a neighborhood, although, of course, they never used this term" (*UV*, 105). Coing also suggested that the prospect of clearance much revived community feelings and memories of the good old days, but he took this as a clue to "the true nature [of the

quartier], i.e., a ‘purpose’ for its own social life that [it] has tried to give expression to over its entire history” (*RU*, 24).

These different nuances aside, we observe here the shaping of a broadly similar model for describing the “traditional working-class neighborhood” in big cities. The authors did not build it up in isolation and could use contemporary literature to back their descriptions and offer some generalities about the working-class communities. Young and Willmott referred to recent surveys of the East End and similar places in Britain, but also of English, Welsh, and Irish villages. Gans, who had studied with F. L. Warner at Chicago, referred to his work on Yankee City and also to a few American neighborhood monographs (in particular Whyte’s study on the Boston North End, which “was probably [his] model for the Urban Villagers research more than anyone else”),⁴⁵ to anthropological literature on peasant societies, especially that by Robert Redfield and Oscar Lewis, to British books on working-class communities, including Young and Willmott’s. Gans borrowed the “us” versus “them” theme from the West Enders; he had also read R. Hoggart’s book, and during his 1957 British trip, Gans talked with the author.⁴⁶ Coing copiously used Chombart de Lauwe’s work on Paris neighborhoods as well as the survey of a “proletarian *quartier*” in Rouen by a Dominican reverend from *Economie et Humanisme*—the Roman Catholic research and reform group created by Père Lebreton—and Oscar Lewis’s description of the “culture of poverty” in the recently translated *The Children of Sanchez*. It is apparent, however, that Coing’s preferred author was Halbwegs, a highly legitimate source of inspiration who had written on “collective memory” and “workers’ needs.”

Sociologists and other researchers thus “discovered” working-class neighborhoods at the very time they were about to vanish. Like the anthropologists who hurriedly observed primitive peoples before “civilization” overran them, sociologists sought to study these neighborhoods before they disappeared. This was one reason why researchers thought of the culture of those communities as “traditional.” There was another one. Past and present were constantly contrasted in the books with which we are dealing. Young and Willmott followed former Bethnal Green inhabitants to their new suburban environment at “Greenleigh,” and Coing documented what happened with families rehoused in new council flats. Furthermore, our authors had scarcely completed their surveys of old neighborhoods when they began to inquire into the new suburbs. Willmott and Young worked on Woodford, a middle-class eastern suburb of London; Willmott revisited Dagenham, a council estate built in the 1920s and already surveyed in the 1930s by T. Young. Gans, who had previously inquired into a new suburb of Chicago, chose to research one “Levittown” in rural New Jersey.⁴⁷ Coing comparatively discussed *quartiers anciens* and *grands ensembles* (housing estates) and studied some of the latter in Lyons when he joined a consulting team in planning in 1966.⁴⁸ The authors’ interest in suburban life came from the widely shared belief that a new society was emerging in this new environment. Models of modernity—be it “mass society,” “the lonely crowd” or *la nouvelle classe ouvrière*—and models of tradition were built up simultaneously. This suggests that the various traits observed in

⁴⁵ Gans, letter to author, 11 July 2001.

⁴⁶ Gans, letter to author, 5 Aug. 2001.

⁴⁷ Willmott and Young, *Family and Class* (cit. n. 9); Willmott, *The Evolution of a Community: A Study of Dagenham after Forty Years* (London: Routledge & Kegan Paul, 1963); Gans, *The Levittowners: Ways of Life and Politics in a New Suburban Community* (New York: Pantheon Books, 1967).

⁴⁸ Coing, interview (cit. n. 25).

traditional working-class communities were singled out because they supposedly contrasted with those of a new society taking shape in other locations. Traditional communities represented the negative mirror image of the future.

WHAT DID THE SOCIOLOGISTS SEE?

The three case studies I have selected are not the only ones of their kind. They used references—almost all very recent—that make their own books part of a series, and they have been in turn used by other authors up to the present. I cannot explore the many-sided reception of their books here. I only wish to point out a moment of change in the manner in which the social sciences have described cities, without presuming that our sociologists have “influenced” what followed—“influence” seems to me the worst word for making sense of responses to past authors in intellectual history. In the 1950s and 1960s, the “traditional working-class neighborhood” replaced the former description of poor urban districts as “slums” or “disorganized areas.” On the whole, the shift was toward a symbolic rehabilitation of people at the bottom, and the new model has been mobilized along such lines in many quarters, with many variations, with or without reference to the books we have just studied. Sociologists have used the model to describe blue-collar, poor, and minority communities threatened by urban or industrial change. Anthropologists, who began to come back home in the 1970s and have since developed the urban anthropology of Western cities, have done the same. Descriptions of lower-class districts as communities have constantly been used by students of the developing world metropolises who debated first on “marginality” and later on “urban social movements.” Social historians who wanted to implement the “history from below” program E. P. Thompson defined in the 1960s discovered that nineteenth-century big cities were actually made of local communities such as those observed by contemporary sociologists.

The mood of some social scientists has changed, however, and what I describe here as a model has been somewhat weakened over the past fifteen years—markedly in France, where studying “the working class” is not as fashionable as before. Social science approaches have changed on two aspects of more general import. First, attention is given to the mobility of people in such a way that the permanence-and-isolation hypothesis is not sustainable any longer. By definition, those who can be interviewed in an old neighborhood are those who have stayed there, but many other people have only passed through. Documenting individual variations and their possible patterns implies questioning the very notion of “community,” which tends to be redefined as a performative artifact of the memory of the more stable inhabitants interacting with the observer and society at large. Second, identity may not be as unique and compact as one usually thinks. If analyzed relationally, identities of individuals and groups can vary according to the situations in which they are found, and once again, the relationship between the observer and the observed should be taken into account. To put it briefly, various current trends in sociology, anthropology, and history converge to produce an upheaval in the approach of what was formerly seen as communities, and this probably is what makes it possible to look in a historicist manner at the books I am considering here.

We have at length analyzed the answers sociologists gave to their briefs. It is time to come back to the brief itself and the social conditions that gave it birth. Scientific innovation in this case took place in a dispute over ends and means of slum clearance.

Urban renewal was under way on a big scale, and social scientists were called in to study and evaluate its consequences. Their assessment was somehow uneasy. On the one hand, they could not disregard the asserted goals of slum clearance. A broad consensus of people, including social scientists, supported improving the housing conditions of the economically weak. On the other hand, sociologists sharply criticized the manner in which the programs were conducted and entered into a controversy with two intellectual traditions and professional groups.⁴⁹ To sanitarians and public health authorities, the sociologists explained that “slum dwellers” were decent people with a culture of their own that should be respected. To urban planners dreaming of “organizing communities” with brick and mortar, they asserted that “communities” were already there and only needed to be handled with care. No doubt the leftist political leanings of the sociologists we are studying had some weight in the critical stance they took. But a mere “ideological” reading of it would be too short. Though diverse reform groups and political parties supported urban renewal programs, on the whole it was the political left that stood solidly behind them in the 1950s and 1960s. Dissent from that quarter was rather unexpected and should be accounted for.

At this point, the sociology of sociologists and their interaction with those defining their briefs can help. Institutionally and biographically, our authors were at the margin of established sociology and among the pioneers of research commissioned by nonacademic authorities. Those who defined our sociologists’ briefs were not directly involved in practical planning, and the knowledge they wanted to develop was not directly operational. The Ford Foundation and the U.S. National Institute of Mental Health heralded the emergence of specialized administrations of social science programs, conducted in part by scientists. The French Ministry of Housing was a big bureaucracy with niches that allowed tiny think tanks to develop. In all those institutions, the personnel in charge of commissioning social science surveys had to prove that intellectual independence paid and make no push toward practical conclusions. This made it possible for the sociologists to claim that their knowledge should not be instrumentalized if it were to be truly useful. One of the safest ways to back such an assertion was to seek recognition by academic science, but success in that respect came late. I would argue that our authors’ ambiguous profile was just what was needed, as one could expect from them the independent knowledge on policy matters that practical professionals were not able to—and established sociologists would not condescend to—provide.

Such scientists were placed in the position of witnessing a very special moment in urban history—the large-scale destruction of the poor districts in their respective cities through a consistent policy of urban renewal. They had no choice but to try to capture those vanishing places. Gans insists: “There were lots of other conceivable agendas, but what I did was basically what I was being paid to do—describe the West End before it was torn down.”⁵⁰ Coing just wanted to observe how families reacted to change, but the result proved the same. He discovered that he was compelled to expand the few pages he had planned to dedicate to the old neighborhood into a full first part of the book titled “The Weight of the Past”⁵¹ Whether planned or not with

⁴⁹ Gans had an additional argument with private real estate interests, which at that time were still absent from the London and Paris urban renewal programs.

⁵⁰ Gans, letter to author, 11 July 2001.

⁵¹ Coing, interview (cit. n. 25).

this intent, the three inquiries had to make sense of a common experience—the “neighborhood” or “community” was taking shape under the eyes of the sociologist as an answer to a threat. Local people actively mobilized memories for reconstructing the present as an enduring past on the verge of being lost for an uncertain future.⁵² Indeed, our authors told what they saw—although with slightly different scientific languages and unequal belief in the viability of the model—and what they saw was the same; this sameness sprang, however, from the situation in which the observers and the observed were involved. Inhabitants reconstructed reality under pressure of a global threat and in a specific interaction with strangers who came to study them. Carefully taking note of the resulting new meanings, sociologists transformed those meanings into certified knowledge that could be transferred and used on the scene where the sociologists’ own interaction with planners was taking place.

This case study has some general significance, for it sheds light on the relationship science may have with its historical conditions. These conditions determine—in Barry Barnes’s terms⁵³—the “interests” scientists pursue and make sense of the very content of their findings. Scientists fulfill a brief by using available resources in their own individual manner, but the brief, the resources, and the manners can be usefully interpreted as resulting from concrete social interactions. Objectivity is indeed situational.

⁵² See Gans, *UV* (cit. n. 4), 11; and Coing, *RU* (cit. n. 6), 84.

⁵³ Barnes, *Interests and the Growth of Knowledge* (London: Routledge & Kegan Paul, 1977).