Street Corner Society

THE SOCIAL STRUCTURE OF AN ITALIAN SLUM

FOURTH EDITION

William Foote Whyte

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To

THE CORNER BOYS OF CORNERVILLE

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PREFACE TO THE FOURTH EDITION

THE field work for this study was done on a Junior Fellowship from Harvard University (1936 to 1940). From February 1937 until May 1938, I lived with an Italian-American family which operated a restaurant at 7 Parmenter Street. When I married Kathleen King, we moved to our own flat at 477 Hanover Street in the same district, the North End of Boston, which I called Cornerville. Before leaving Boston in July 1940, I had written the first draft of *Street Corner Society*. While in graduate work at the University of Chicago, 1940 to 1942, I rewrote and condensed the Boston draft but made no changes in my analysis and theoretical orientations. The first edition of *Street Corner Society* was published in 1943.

Without the support and the complete freedom afforded me by the Society of Fellows, I should have never been able to undertake the study of Cornerville. I owe a great personal debt to social anthropologist Conrad M. Arensberg, whose period as a Junior Fellow overlapped mine. I discussed my plans with him before I began my study and had the benefit of his advice and criticism every step of the way. Eliot D. Chapple, in collaboration with Arensberg, worked out the conceptual scheme for the study of interactions which I used throughout this book.

Regarding my first research plans, I received painful but critically important advice from Lawrence J. Henderson, Chairman of the Society of Fellows. Elton Mayo of the Harvard Business School guided me in learning the techniques of interviewing used in my research.

While a Junior Fellow with me, John Howard did field work over two years in Cornerville. He was the first one to suggest that an analysis of leadership could provide a means of integrating the study.

Kathleen King Whyte shared my last two years in Cornerville. She did the charts and criticized the manuscript in every

stage of its preparation. Having designed book jackets for New York publishers, she volunteered to do the jacket for the first edition.

At the University of Chicago, I received helpful suggestions for revising my manuscript from social anthropologist W. Lloyd Warner and sociologist Everett C. Hughes. Beyond *Street Corner Society,* they helped to make graduate work at the University of Chicago an exciting learning experience.

For the second (1955) edition, I wrote Appendix A describing the methods I used and my personal experiences while living and working in the North End.

For the third (1981) edition, I expanded a section on "Cornerville Revisited," following the careers of some of the chief characters up through 1980 and briefly noting changes in the district itself. For the first time, in this edition, I identified the site of the study as Boston's North End and identified some of the leading characters by their real names.

Appendix B, "The Whyte Impact on an Underdog," was presented by my first research assistant, Angelo Ralph Orlandella (Sam Franco), at a Cornell conference marking my retirement. Here Orlandella describes eloquently how we worked together and how the methods we developed served him in leadership roles in military and civilian life.

In somewhat revised form, all of these materials are included in this fiftieth anniversary edition. I have added to Appendix A "Revisiting *Street Corner Society* after Fifty Years." The impetus for this edition was provided by the sudden revival of scholarly interest in the book. In 1991 a book edited by Peter Frost and colleagues, *Reframing Organizational Culture,* devoted a major section to *Street Corner Society.* This included reprinting part of my methodological and experiential appendix as "an exemplar" of research on organizational cultures. This was followed by essays on *Street Corner Society* by four behavioral scientists and my responses to the critics. The April 1992 issue of *Journal of Contemporary Ethnography* is entirely devoted to *"Street Corner Society* Revisited." Following an introduction by the editors, the issue leads off with an essay by W. A. Marianne Boelen, who made several visits to the North End thirty to forty-five years after I left it in 1940 and interviewed some of the people I had studied and some others. On the basis of these data, she argues that I presented a distorted portrait of the North end and committed violations of professional ethics. That is followed by my own rebuttal and an essay by Angelo Ralph Orlandella, the former corner boy, who worked with me on the study. The issue concludes with essays on the controversy by three behavioral scientists.

In those two publications, all seven of the behavioral scientists recognized *Street Corner Society* as a landmark study. Nevertheless, they raised issues worth further discussion regarding changes in critical standards of judging sociological research over the last fifty years. Furthermore, those issues involve the basic questions as to whether sociology is a science or whether sociologists should strive to make it a science.

I discuss those issues in "Revisiting *Street Corner Society* after Fifty Years," the final section of Appendix A.

INTRODUCTION: CORNERVILLE AND ITS PEOPLE

IN THE heart of "Eastern City" there is a slum district known as Cornerville, which is inhabited almost exclusively by Italian immigrants and their children. To the rest of the city it is a mysterious, dangerous, and depressing area. Cornerville is only a few minutes' walk from fashionable High Street, but the High Street inhabitant who takes that walk passes from the familiar to the unknown.

For years Cornerville has been known as a problem area, and, while we were at war with Italy, outsiders became increasingly concerned with that problem. They feared that the Italian slum dweller might be more devoted to fascism and Italy than to democracy and the United States. They have long felt that Cornerville was at odds with the rest of the community. They think of it as the home of racketeers and corrupt politicians, of poverty and crime, of subversive beliefs and activities.

Respectable people have access to a limited body of information upon Cornerville. They may learn that it is one of the most congested areas in the United States. It is one of the chief points of interest in any tour organized to show upper-class people the bad housing conditions in which lower-class people live. Through sight-seeing or statistics one may discover that bathtubs are rare, that children overrun the narrow and neglected streets, that the juvenile delinquency rate is high, that crime is prevalent among adults, and that a large proportion of the population was on home relief or W.P.A. during the depression.

In this view, Cornerville people appear as social work clients, as defendants in criminal cases, or as undifferentiated members of "the masses." There is one thing wrong with such a picture: no human beings are in it. Those who are concerned with Cornerville seek through a general survey to answer questions that require the most intimate knowledge of local life. The only way to gain such knowledge is to live in Cornerville and participate in

the activities of its people. One who does that finds that the district reveals itself to him in an entirely different light. The buildings, streets, and alleys that formerly represented dilapidation and physical congestion recede to form a familiar background for the actors upon the Cornerville scene.

One may enter Cornerville already equipped with newspaper information upon some of its racketeers and politicians, but the newspaper presents a very specialized picture. If a racketeer commits murder, that is news. If he proceeds quietly with the daily routines of his business, that is not news. If the politician is indicted for accepting graft, that is news. If he goes about doing the usual personal favors for his constituents, that is not news. The newspaper concentrates upon the crisis—the spectacular event. In a crisis the "big shot" becomes public property. He is removed from the society in which he functions and is judged by standards different from those of his own group. This may be the most effective way to prosecute the lawbreaker. It is not a good way to understand him. For that purpose, the individual must be put back into his social setting and observed in his daily activities. In order to understand the spectacular event, it is necessary to see it in relation to the everyday pattern of life—for there is a pattern to Cornerville life. The middle-class person looks upon the slum district as a formidable mass of confusion, a social chaos. The insider finds in Cornerville a highly organized and integrated social system.

It follows, therefore, that no immediate and direct solution to the problems posed for Cornerville can be given. It is only when the structure of the society and its patterns of action have been worked out that particular questions can be answered. This requires an exploration of new territory. In order to see how the present organization grew up, we may review the history of the local Italian settlement. When this is done, it will be time to go in and meet the people in order to discover from them the nature of the society in which they live.

For the Cornerville of today, history began in the 1860's, when a small group of Genoese settled together on an alley in one corner of what was then an Irish district. The stream of Italian immigration expanded slowly in the seventies and eighties and grew to a great flood in the nineties and in the first decade of the new century. The North Italians were the first to arrive, but the great wave of immigration came from the south, particularly from the vicinity of Naples and from Sicily. By the time that the southern immigration was at its height, most of the early Genoese settlers had moved to other sections of Eastern City or to suburban towns.

As early as 1915 the racial composition of Cornerville was practically the same as it is today. All but a few Irish families had moved out. The Jews, who came in at the same time as the Italians, had also been superseded, though many of them retained Cornerville business interests, particularly in the retail dry-goods line.

The Italian settlers brought over with them not only their language and customs but also a large proportion of their fellow-townsmen. The immigrants attracted relatives and friends. People from the same town, *paesani*, settled together, formed mutual aid societies, and each year celebrated the *Festa* of their patron saint as they had in Italy. The *paesani* made up little communities within a community, and even today one can mark out sections of Cornerville according to the town of origin of the immigrants, although these lines are fading with the growth of the younger generation.

First-generation immigrant society was organized primarily around the family and secondarily along the lines of *paesani*. Ties between families were cemented by the establishment of godparent-godchild relationships. Relatives by blood and ceremonial ties, as well as friends of the family, were linked together in an intricate network of reciprocal obligations. The individual who suffered misfortune was aided by his relatives and friends, and, when he had re-established himself, he shared his good fortune with those who had helped him.

The general region from which the immigrant came was also important in the organization of Cornerville life. The North Italians, who had had greater economic and educational opportunities, always looked down upon the southerners, and the Sicilians occupied the lowest position of all. Since many North and Cen-

tral Italians were able to establish themselves before the southerners arrived, these distinctions were accentuated in the period of settlement, and they have not yet completely died out.

As the American-born generation has grown to maturity, the pattern of Cornerville life has undergone far-reaching changes. The ties of loyalty to *paesani* do not bind the son as they do the father. Even the Italian family has been broken into two separate generations. The Italian-born are known to the younger generation as "greasers." The children are often strongly attached to their parents, and yet they look down upon them. A few of the older people hold respected positions, but on the whole they do not have the authority that is characteristic of the older generation in most societies.

The younger generation has built up its own society relatively independent of the influence of its elders. Within the ranks of the younger men there are two main divisions: corner boys and college boys. Corner boys are groups of men who center their social activities upon particular street corners, with their adjoining barbershops, lunchrooms, poolrooms, or clubrooms. They constitute the bottom level of society within their age group, and at the same time they make up the great majority of the young men of Cornerville. During the depression most of them were unemployed or had only irregular employment. Few had completed high school, and many of them had left school before finishing the eighth grade. The college boys are a small group of young men who have risen above the corner-boy level through higher education. As they try to make places for themselves as professional men, they are still moving socially upward.

In a society such as ours, in which it is possible for men to begin life at the bottom and move up, it is important to discover who the people are who are advancing and how they are doing it. This gives perspective upon Cornerville society, and, at the same time, it shows what the world outside Cornerville has to offer to local people. The stories of Doc and his corner-boy gang and of Chick and his college-boy club present the contrast between the two groups and explain the different careers of the individual members.

While Doc and his boys and Chick and his club members are representative of a large part of local society, they are all "little guys" in Cornerville. In order to understand them, it is necessary to discover who the "big shots" are and see how they function. In Cornerville the big shots are racketeers and politicians.

With the South Side and Welport, Cornerville makes up Eastern City's Fourth Ward. Until recently the ward was dominated by the Cleveland Club, an Irish Democratic political organization located on the South Side. When the Italians first settled in Cornerville and began displacing the Irish population, there were sharp clashes between the races. As the Irish moved out, the hostilities were transferred to the political arena. Italian politicians organized Cornerville to overthrow the Irish domination of the ward.

Illegal activities during the prohibition era centered around the liquor traffic. With repeal the racketeer built his career upon the control of gambling activities. Cornerville men have played prominent roles in this field, although their Irish and Jewish colleagues share in the direction of the Eastern City rackets.

The racket and political organizations extend from the bottom to the top of Cornerville society, mesh with one another, and integrate a large part of the life of the district. They provide a general framework for the understanding of the actions of both "little guys" and "big shots."

In this exploration of Cornerville we shall be little concerned with people in general. We shall encounter particular people and observe the particular things that they do. The general pattern of life is important, but it can be constructed only through observing the individuals whose actions make up that pattern.

The "little guys" will be first on the scene (Part I). We shall see how they organize the activities of their own groups, and then, to place those groups in the social structure, we shall move up and observe the "big shots." The description of the racket and political organizations (in Part II) will give a general picture, but we are still concerned with particular people. The question is: What makes a man a big shot and by what means is he able to dominate the little guys? To answer that question, let us watch Tony Ca-

taldo. He is a prominent racketeer, and he is concerned, among other things, with controlling the corner boys. How does he go about it? And let us watch George Ravello, Cornerville's state senator, as he organizes his political campaign. He needs the support of the corner boys. How does he get it? We know in general that the heads of political and racket organizations in Cornerville co-operate with one another. But what is the nature of that co-operation, upon what is it based, and how is it established? In order to answer that question, let us look at the particular people again and see how they act in relation to one another in the various situations that confront them in their careers.

If we can get to know these people intimately and understand the relations between little guy and little guy, big shot and little guy, and big shot and big shot, then we know how Cornerville society is organized. On the basis of that knowledge it becomes possible to explain people's loyalties and the significance of political and racket activities.

PART I

CORNER BOYS AND COLLEGE BOYS

APPENDIX A

ON THE EVOLUTION OF *STREET CORNER SOCIETY*

In the years since completing *Street Corner Society* I have several times sought to teach students the research methods needed for field studies of communities or organizations. Like other instructors in this field, I have been severely handicapped by the paucity of reading matter that I can assign to students.

There are now many good published studies of communities or organizations, but generally the published report gives little attention to the actual process whereby the research was carried out. There have also been some useful statements on methods of research, but, with a few exceptions, they place the discussion entirely on a logical-intellectual basis. They fail to note that the researcher, like his informants, is a social animal. He has a role to play, and he has his own personality needs that must be met in some degree if he is to function successfully. Where the researcher operates out of a university, just going into the field for a few hours at a time, he can keep his personal social life separate from field activity. His problem of role is not quite so complicated. If, on the other hand, the researcher is living for an extended period in the community he is studying, his personal life is inextricably mixed with his research. A real explanation, then, of how the research was done necessarily involves a rather personal account of how the researcher lived during the period of study.

This account of living in the community may help also to explain the process of analysis of the data. The ideas that we have in research are only in part a logical product growing out of a careful weighing of evidence. We do not generally think problems through in a straight line. Often we have the experience of being immersed in a mass of confusing data. We study the data carefully, bringing all our powers of logical analysis to bear upon them. We come up with an idea or two. But still the data do not fall in any coherent pattern. Then we go on living with the

data—and with the people—until perhaps some chance occurrence casts a totally different light upon the data, and we begin to see a pattern that we have not seen before. This pattern is not purely an artistic creation. Once we think we see it, we must reexamine our notes and perhaps set out to gather new data in order to determine whether the pattern adequately represents the life we are observing or is simply a product of our imagination. Logic, then, plays an important part. But I am convinced that the actual evolution of research ideas does not take place in accord with the formal statements we read on research methods. The ideas grow up in part out of our immersion in the data and out of the whole process of living. Since so much of this process of analysis proceeds on the unconscious level, I am sure that we can never present a full account of it. However, an account of the way the research was done may help to explain how the pattern *of Street Corner Society* gradually emerged.

I am not suggesting that my approach to *Street Corner Society* should be followed by other researchers. To some extent my approach must be unique to myself, to the particular situation, and to the state of knowledge existing when I began research. On the other hand, there must be some common elements of the field research process. Only as we accumulate a series of accounts of how research was actually done will we be able to go beyond the logical-intellectual picture and learn to describe the actual research process. What follows, then, is simply one contribution toward that end.

1. PERSONAL BACKGROUND

I come from a very consistent upper-middle-class background. One grandfather was a doctor; the other, a superintendent of schools. My father was a college professor. My upbringing, therefore, was very far removed from the life I have described in Cornerville.

At Swarthmore College I had two strong interests: economics (mixed with social reform) and writing. In college I wrote a number of short stories and one-act plays. During the summer after college I made an attempt at a novel. This writing was valuable to me largely in what it taught me about myself. Several of the stories appeared in the college literary magazine, and one was accepted for publication (but never published) in *Story* magazine. Three of the one-act plays were produced at Swarthmore in the annual one-act playwriting contest. Not a bad start for someone who had hopes, as I did then, for a writing career. But yet I felt uneasy and dissatisfied. The plays and stories were all fictionalized accounts of events and situations I had experienced or observed myself. When I attempted to go beyond my experience and tackle a novel on a political theme, the result was a complete bust. Even as I wrote the concluding chapters, I realized that the manuscript was worthless. I finished it, I suppose, just so that I could say to myself that I had written a novel.

Now I had read the often-given advice to young writers that they should write out of their own experience, so I had no reason to be ashamed of this limitation. On the other hand, it was when I reflected upon my experience that I became uneasy and dissatisfied. My home life had been very happy and intellectually stimulating—but without adventure. I had never had to struggle over anything. I knew lots of nice people, but almost all of them came from good, solid middle-class backgrounds like my own. In college, of course, I was associating with middle-class students and middle-class professors. I knew nothing about the slums (or the gold coast for that matter). I knew nothing about life in the factories, fields, or mines—except what I had gotten out of books. So I came to feel that I was a pretty dull fellow. At times this sense of dulness became so oppressive that I simply could not think of any stories to write. I began to feel that, if I were really going to write anything worth while, I would somehow have to get beyond the narrow social borders of my existence up to that time.

My interest in economics and social reform also led in the direction of *Street Corner Society*. One of my most vivid college memories is of a day spent with a group of students in visiting the slums of Philadelphia. I remember it not only for the images of dilapidated buildings and crowded people but also for the sense of embarrassment I felt as a tourist in the district. I had the com-

mon young man's urge to do good to these people, and yet I knew then that the situation was so far beyond anything I could realistically attempt at the time that I felt like an insincere dabbler even to be there. I began to think sometimes about going back to such a district and really learning to know the people and the conditions of their lives.

My social reform urges came out in other forms on the campus. In my sophomore year I was one of a group of fifteen men who resigned from their fraternities amid a good deal of fanfare. This was an exciting time on the campus, and some of the solid fraternity men were fearful lest the structure would crumble under their feet. They should not have worried. Fraternities went right along without us. In my senior year I became involved in another effort at campus reform. This time we were aiming at nothing less than a reorganization of the whole social life of the campus. The movement got off to a promising start but then quickly petered out.

These abortive reform efforts had one great value to me. I saw that reform was not so easy. I recognized that I had made a number of mistakes. I also came to the realization that some of the people who had fought against me the hardest were really pretty nice fellows. I did not conclude from this that they were right and I was wrong, but I came to recognize how little I really knew about the forces that move people to action. Out of my own reflections about the failures of my campus reform efforts grew a keener interest in understanding other people.

There was also a book that I had read, which weighed most heavily with me at this time. It was the *Autobiography of Lincoln Steffens*. I got my hands on it during the year I spent in Germany between high school and college. In my efforts to master German, this was the only thing written in English that I read for some time, so perhaps it weighed more heavily with me than it otherwise would. In any case, I was fascinated by it and read it through several times. Steffens had begun as a reformer, and he never abandoned this urge to change things. Yet he had such an unending curiosity about the world around him that he became

more and more interested in discovering how society actually functioned. He demonstrated that a man of a background similar to my own could step out of his own usual walks of life and gain an intimate knowledge of individuals and groups whose activities and beliefs were far different from his own. So you could actually get these "corrupt politicians" to talk to you. This I needed to know. It helped me sometimes when I had the feeling that the people I was interviewing would much rather have me get out of there altogether.

2. FINDING CORNERVILLE

When I was graduated from Swarthmore in 1936, I received a fellowship from the Society of Fellows at Harvard. This provided me with a unique sort of opportunity—three years of support for any line of research I wished to pursue. The only restriction was that I was not allowed to accumulate credits toward a Ph.D. degree. I am grateful now for this restriction. If I had been allowed to work for the Ph.D., I suppose I should have felt that I must take advantage of the time and the opportunity. With this avenue cut off, I was forced to do what I wanted to do, regardless of academic credits.

I began with a vague idea that I wanted to study a slum district. Eastern City provided several possible choices. In the early weeks of my Harvard fellowship I spent some of my time talking up and down the streets of the various slum districts of Eastern City and talking with people in social agencies about these districts.

I made my choice on very unscientific grounds: Cornerville best fitted my picture of what a slum district should look like. Somehow I had developed a picture of run-down three- to five-story buildings crowded in together. The dilapidated wooden-frame buildings of some other parts of the city did not look quite genuine to me. To be sure, Cornerville did have one characteristic that recommended it on a little more objective basis. It had more people per acre living in it than any other section of the city. If a slum meant overcrowding, this was certainly it.

3. PLANNING THE STUDY

As soon as I had found my slum district, I set about planning my study. It was not enough for me at the time to plan for myself alone. I had begun reading in the sociological literature and thinking along the lines of the Lynds' *Middletown*. Gradually I came to think of myself as a sociologist or a social anthropologist instead of an economist. I found that, while slums had been given much attention in the sociological literature, there existed no real community study of such a district. So I set out to organize a community study for Cornerville. This was clearly a big job. My early outline of the study pointed to special researches in the history of the district, in economics (living standards, housing, marketing, distribution, and employment), politics (the structure of the political organization and its relation to the rackets and the police), patterns of education and recreation, the church, public health, and—of all things—social attitudes. Obviously, this was more than a one-man job, so I designed it for about ten men.

With this project statement in hand I approached L. J. Henderson, an eminent biochemist who was secretary of the Society of Fellows.

We spent an hour together, and I came away with my plans very much in a state of flux. As I wrote to a friend at this time: "Henderson poured cold water on the mammoth beginning, told me that I should not cast such grandiose plans when I had done hardly any work in the field myself. It would be much sounder to get in the field and try to build up a staff slowly as I went along. If I should get a ten-man project going by fall, the responsibility for the direction and co-ordination of it would inevitably fall upon me, since I would have started it. How could I direct ten people in a field that was unfamiliar to me? Henderson said that, if I did manage to get a ten-man project going, it would be the ruination of me, he thought. Now, the way he put all this it sounded quite sensible and reasonable."

This last sentence must have been written after I had had time to recover from the interview, because I remember it as being a

crushing experience. I suppose good advice is just as hard to take as poor advice, and yet in a very short time I realized that Henderson was right, and I abandoned the grandiose plan I had made. Since people who offer painful but good advice so seldom get any thanks for it, I shall always be glad that I went to see Henderson again shortly before his death and told him that I had come to feel that he had been absolutely right.

While I abandoned the ten-man project, I was reluctant to come down to earth altogether. It seemed to me that, in view of the magnitude of the task I was undertaking, I must have at least one collaborator, and I began to cast about for means of getting a college friend of mine to join me in the field. There followed through the winter of 1936–37 several revisions of my outline of the community study and numerous interviews with Harvard professors who might help me to get the necessary backing.

As I read over these various research outlines, it seems to me that the most impressive thing about them is their remoteness from the actual study I carried on. As I went along, the outlines became gradually more sociological, so that I wound up this phase planning to devote major emphasis to a sort of sociometric study of the friendship patterns of people. I would start with one family and ask them who their friends were and who the people were that they were more or less hostile to. Then I would go to these friends and get the list of their friends and learn in the process something of their activities together. In this way, I was to chart the social structure of at least some of the community. Even this, of course, I did not do, for I came to find that you could examine social structure directly through observing people in action.

When, a year later in the fall of 1937, John Howard, also a Harvard junior fellow, changed his field from physical chemistry to sociology, I invited him to join me in the Cornerville study. We worked together for two years, with Howard particularly concentrating on one of the churches and its Holy Name Society. The discussions between us helped immensely in clarifying my ideas. But only a few months after I had begun Cornerville field

work, I had completely abandoned the thought of building up a Cornerville staff. I suppose that I found Cornerville life so interesting and rewarding that I no longer felt a need to think in large-scale terms.

Although I was completely at sea in planning the study, at least I had valuable help in developing the field research methods which were eventually to lead to a study plan as well as to the data here reported.

It is hard to realize now how rapid has been the development of sociological and anthropological studies of communities and organizations since 1936, when I began my work in Cornerville. At that time nothing had yet been published on W. Lloyd Warner's "Yankee City" study. I had read the Lynds' *Middletown* and Carolyn Ware's *Greenwich Village* with interest and profit, and yet I began to realize, more and more as I went along, that I was not making a community study along those lines. Much of the other sociological literature then available tended to look upon communities in terms of social problems so that the community as an organized social system simply did not exist.

I spent my first summer following the launching of the study in reading some of the writings of Durkheim and Pareto's *The Mind and Society* (for a seminar with L. J. Henderson, which I was to take in the fall of 1937). I had a feeling that these writings were helpful but still only in a general way. Then I began reading in the social anthropological literature, beginning with Malinowski, and this seemed closer to what I wanted to do even though the researchers were studying primitive tribes and I was in the middle of a great city district.

If there was then little to guide me in the literature, I needed that much more urgently to have the help of people more skilled and experienced than I in the work I was undertaking. Here I was extraordinarily fortunate in meeting Conrad M. Arensberg at the very outset of my Harvard appointment. He also was a junior fellow, so that we naturally saw much of each other. After having worked for some months with W. Lloyd Warner in the Yankee City study, he had gone with Solon Kimball to make a study of a small community in Ireland. When I met him, he had

just returned from this field trip and was beginning to write up his data. With Eliot Chapple, he was also in the process of working out a new approach to the analysis of social organization. The two men had been casting about together for ways of establishing such social research on a more scientific basis. Going over the Yankee City data and the Irish study, also, they had set up five different theoretical schemes. One after the other each of the first four schemes fell to the ground under their own searching criticism or under the prods of Henderson or Elton Mayo or others whom they consulted. At last they began to develop a theory of interaction. They felt that, whatever else might be subjective in social research, one could establish objectively the pattern of interaction among people: how often A contacts B, how long they spend together, who originates action when A, B, and C are together, and so on. Careful observation of such interpersonal events might then provide reliable data upon the social organization of a community. At least this was the assumption. Since the theory grew out of research already done, it was natural that these previous studies did not contain as much of the quantitative data as the theory would have required. So it seemed that I might be one of the first to take the theory out into the field.

Arensberg and I had endless discussions of the theory, and in some of these Eliot Chapple participated. At first it seemed very confusing to me—I am not sure I have it all clear yet—but I had a growing feeling that here was something solid that I could build upon.

Arensberg also worked with me on field research methods, emphasizing the importance of observing people in action and getting down a detailed report of actual behavior completely divorced from moral judgments. In my second semester at Harvard, I took a course given by Arensberg and Chapple concerning social anthropological community studies. While this was helpful, I owed much more to the long personal conversations I had with Arensberg throughout the Cornerville research, particularly in its early stages.

In the fall of 1937 I took a small seminar with Elton Mayo. This involved particularly readings from the works of Pierre Janet, and

it included also some practice in interviewing psychoneurotics in an Eastern City hospital. This experience was too brief to carry me beyond the amateur stage, but it was helpful in developing my interviewing methods.

L. J. Henderson provided a less specific but nevertheless pervasive influence in the development of my methods and theories. As chairman of the Society of Fellows, he presided over our Monday-night dinners like a patriarch in his own household. Even though the group included A. Lawrence Lowell, Alfred North Whitehead, John Livingston Lowes, Samuel Eliot Morrison, and Arthur Darby Nock, it was Henderson who was easily the most imposing figure for the junior fellows. He seemed particularly to enjoy baiting the young social scientists. He took me on at my first Monday-night dinner and undertook to show me that all my ideas about society were based upon softheaded sentimentality. While I often resented Henderson's sharp criticisms, I was all the more determined to make my field research stand up against anything he could say.

4. FIRST EFFORTS

When I began my work, I had had no training in sociology or anthropology. I thought of myself as an economist and naturally looked first toward the matters that we had taken up in economics courses, such as economics of slum housing. At the time I was sitting in on a course in slums and housing in the Sociology Department at Harvard. As a term project I took on a study of one block in Cornerville. To legitimize this effort, I got in touch with a private agency that concerned itself in housing matters and offered to turn over to them the results of my survey. With that backing, I began knocking on doors, looking into flats, and talking to the tenants about the living conditions. This brought me into contact with Cornerville people, but it would be hard now to devise a more inappropriate way of beginning a study such as I was eventually to make. I felt ill at ease at this intrusion, and I am sure so did the people. I wound up the block study as rapidly as I could and wrote it off as a total loss as far as gaining a real entry into the district.

Shortly thereafter I made another false start—if so tentative an effort may even be called a start. At the time I was completely baffled at the problem of finding my way into the district. Cornerville was right before me and yet so far away. I could walk freely up and down its streets, and I had even made my way into some of the flats, and yet I was still a stranger in a world completely unknown to me.

At this time I met a young economics instructor at Harvard who impressed me with his self-assurance and his knowledge of Eastern City. He had once been attached to a settlement house, and he talked glibly about his associations with the tough young men and women of the district. He also described how he would occasionally drop in on some drinking place in the area and strike up an acquaintance with a girl, buy her a drink, and then encourage her to tell him her life-story. He claimed that the women so encountered were appreciative of this opportunity and that it involved no further obligaion.

This approach seemed at least as plausible as anything I had been able to think of. I resolved to try it out. I picked on the Regal Hotel, which was on the edge of Cornerville. With some trepidation I climbed the stairs to the bar and entertainment area and looked around. There I encountered a situation for which my adviser had not prepared me. There were women present all right, but none of them was alone. Some were there in couples, and there were two or three pairs of women together. I pondered this situation briefly. I had little confidence in my skill at picking up one female, and it seemed inadvisable to tackle two at the same time. Still, I was determined not to admit defeat without a struggle. I looked around me again and now noticed a threesome: one man and two women. It occurred to me that here was a maldistribution of females which I might be able to rectify. I approached the group and opened with something like this: "Pardon me. Would you mind if I joined you?" There was a moment of silence while the man stared at me. He then offered to throw me downstairs. I assured him that this would not be necessary and demonstrated as much by walking right out of there without any assistance.

I subsequently learned that hardly anyone from Cornerville ever went into the Regal Hotel. If my efforts there had been crowned with success, they would no doubt have led somewhere but certainly not to Cornerville.

For my next effort I sought out the local settlement houses. They were open to the public. You could walk right into them, and—though I would not have phrased it this way at the time—they were manned by middle-class people like myself. I realized even then that to study Cornerville I would have to go well beyond the settlement house, but perhaps the social workers could help me to get started.

As I look back on it now, the settlement house also seems a very unpromising place from which to begin such a study. If I had it to do over again, I would probably make my first approach through a local politician or perhaps through the Catholic church, although I am not myself Catholic. John Howard, who worked with me later, made his entry very successfully through the church, and he, too, was not a Catholic—although his wife was.

However that may be, the settlement house proved the right place for me at this time, for it was here that I met Doc. I had talked to a number of the social workers about my plans and hopes to get acquainted with the people and study the district. They listened with varying degrees of interest. If they had suggestions to make, I have forgotten them now except for one. Somehow, in spite of the vagueness of my own explanations, the head of girls' work in the Norton Street House understood what I needed. She began describing Doc to me. He was, she said, a very intelligent and talented person who had at one time been fairly active in the house but had dropped out, so that he hardly ever came in any more. Perhaps he could understand what I wanted, and he must have the contacts that I needed. She said she frequently encountered him as she walked to and from the house and sometimes stopped to chat with him. If I wished, she would make an appointment for me to see him in the house one evening. This at last seemed right. I jumped at the chance. As I came into the district that evening, it was with a feeling that here

I had my big chance to get started. Somehow Doc must accept me and be willing to work with me.

In a sense, my study began on the evening of February 4, 1937, when the social worker called me in to meet Doc. She showed us into her office and then left so that we could talk. Doc waited quietly for me to begin, as he sank down into a chair. I found him a man of medium height and spare build. His hair was a light brown, quite a contrast to the more typical black Italian hair. It was thinning around the temples. His cheeks were sunken. His eyes were a light blue and seemed to have a penetrating gaze.

I began by asking him if the social worker had told him about what I was trying to do.

"No, she just told me that you wanted to meet me and that I should like to meet you."

Then I went into a long explanation which, unfortunately, I omitted from my notes. As I remember it, I said that I had been interested in congested city districts in my college study but had felt very remote from them. I hoped to study the problems in such a district. I felt I could do very little as an outsider. Only if I could get to know the people and learn their problems first hand would I be able to gain the understanding I needed.

Doc heard me out without any change of expression, so that I had no way of predicting his reaction. When I was finished, he asked: "Do you want to see the high life or the low life?"

"I want to see all that I can. I want to get as complete a picture of the community as possible."

"Well, any nights you want to see anything, I'll take you around. I can take you to the joints—gambling joints—I can take you around to the street corners. Just remember that you're my friend. That's all they need to know. I know these places, and, if I tell them that you're my friend, nobody will bother you. You just tell me what you want to see, and we'll arrange it."

The proposal was so perfect that I was at a loss for a moment as to how to respond to it. We talked a while longer, as I sought to get some pointers as to how I should behave in his company. He warned me that I might have to take the risk of getting

arrested in a raid on a gambling joint but added that this was not serious. I only had to give a false name and then would get bailed out by the man that ran the place, paying only a five-dollar fine. I agreed to take this chance. I asked him whether I should gamble with the others in the gambling joints. He said it was unnecessary and, for a greenhorn like myself, very inadvisable.

At last I was able to express my appreciation. "You know, the first steps of getting to know a community are the hardest. I could see things going with you that I wouldn't see for years otherwise."

"That's right. You tell me what you want to see, and we'll arrange it. When you want some information, I'll ask for it, and you listen. When you want to find out their philosophy of life, I'll start an argument and get it for you. If there's something else you want to get, I'll stage an act for you. Not a scrap, you know, but just tell me what you want, and I'll get it for you."

"That's swell. I couldn't ask for anything better. Now I'm going to try to fit in all right, but, if at any time you see I'm getting off on the wrong foot, I want you to tell me about it."

"Now we're being too dramatic. You won't have any trouble. You come in as my friend. When you come in like that, at first everybody will treat you with respect. You can take a lot of liberties, and nobody will kick. After a while when they get to know you they will treat you like anybody else—you know, they say familiarity breeds contempt. But you'll never have any trouble. There's just one thing to watch out for. Don't spring [treat] people. Don't be too free with your money."

"You mean they'll think I'm a sucker?"

"Yes, and you don't want to buy your way in."

We talked a little about how and when we might get together. Then he asked me a question. "You want to write something about this?"

"Yes, eventually."

"Do you want to change things?"

"Well—yes. I don't see how anybody could come down here where it is so crowded, people haven't got any money or any

work to do, and not want to have some things changed. But I think a fellow should do the thing he is best fitted for. I don't want to be a reformer, and I'm not cut out to be a politician. I just want to understand these things as best I can and write them up, and if that has any influence. . . ."

"I think you can change things that way. Mostly that is the way things are changed, by writing about them."

That was our beginning. At the time I found it hard to believe that I could move in as easily as Doc had said with his sponsorship. But that indeed was the way it turned out.

While I was taking my first steps with Doc, I was also finding a place to live in Cornerville. My fellowship provided a very comfortable bedroom, living-room, and bath at Harvard. I had been attempting to commute from these quarters to my Cornerville study. Technically that was possible, but socially I became more and more convinced that it was impossible. I realized that I would always be a stranger to the community if I did not live there. Then, also, I found myself having difficulty putting in the time that I knew was required to establish close relations in Cornerville. Life in Cornerville did not proceed on the basis of formal appointments. To meet people, to get to know them, to fit into their activities, required spending time with them—a lot of time day after day. Commuting to Cornerville, you might come in on a particular afternoon and evening only to discover that the people you intended to see did not happen to be around at the time. Or, even if you did see them, you might find the time passing entirely uneventfully. You might just be standing around with people whose only occupation was talking or walking about to try to keep themselves from being bored.

On several afternoons and evenings at Harvard, I found myself considering a trip to Cornerville and then rationalizing my way out of it. How did I know I would find the people whom I meant to see? Even if I did so, how could I be sure that I would learn anything today? Instead of going off on a wild-goose chase to Cornerville, I could profitably spend my time reading books and articles to fill in my woeful ignorance of sociology and social anthropology. Then, too, I had to admit that I felt more com-

fortable among these familiar surroundings than I did wandering around Cornerville and spending time with people in whose presence I felt distinctly uncomfortable at first.

When I found myself rationalizing in this way, I realized that I would have to make the break. Only if I lived in Cornerville would I ever be able to understand it and be accepted by it. Finding a place, however, was not easy. In such an overcrowded district a spare room was practically nonexistent. I might have been able to take a room in the Norton Street Settlement House, but I realized that I must do better than this if possible.

I got my best lead from the editor of a weekly English-language newspaper published for the Italian-American colony. I had talked to him before about my study and had found him sympathetic. Now I came to ask him for help in finding a room. He directed me to the Martinis, a family which operated a small restaurant. I went there for lunch and later consulted the son of the family. He was sympathetic but said that they had no place for any additional person. Still, I liked the place and enjoyed the food. I came back several times just to eat. On one occasion I met the editor, and he invited me to his table. At first he asked me some searching questions about my study: what I was after, what my connection with Harvard was, what they had expected to get out of this, and so on. After I had answered him in a manner that I unfortunately failed to record in my notes, he told me that he was satisfied and, in fact, had already spoken in my behalf to people who were suspicious that I might be coming in to "criticize our people."

We discussed my rooming problem again. I mentioned the possibility of living at the Norton Street House. He nodded but added: "It would be much better if you could be in a family. You would pick up the language much quicker, and you would get to know the people. But you want a nice family, an educated family. You don't want to get in with any low types. You want a real good family."

At this he turned to the son of the family with whom I had spoken and asked: "Can't you make some place for Mr. Whyte in the house here?"

Al Martini paused a moment and then said: "Maybe we can fix it up. I'll talk to Mama again."

So he did talk to Mama again, and they did find a place. In fact, he turned over to me his own room and moved in to share a double bed with the son of the cook. I protested mildly at this imposition, but everything had been decided—except for the money. They did not know what to charge me, and I did not know what to offer. Finally, after some fencing, I offered fifteen dollars a month, and they settled for twelve.

The room was simple but adequate to my purposes. It was not heated, but, when I began to type my notes there, I got myself a small oil-burner. There was no bathtub in the house, but I had to go out to Harvard now and then anyway, so I used the facilities of the great university (the room of my friend, Henry Guerlac) for an occasional tub or shower.

Physically, the place was livable, and it provided me with more than just a physical base. I had been with the Martinis for only a week when I discovered that I was much more than a roomer to them. I had been taking many of my meals in the restaurant and sometimes stopping in to chat with the family before I went to bed at night. Then one afternoon I was out at Harvard and found myself coming down with a bad cold. Since I still had my Harvard room, it seemed the sensible thing to do to stay overnight there. I did not think to tell the Martinis of my plan.

The next day when I was back in the restaurant for lunch, Al Martini greeted me warmly and then said that they had all been worried when I did not come home the night before. Mama had stayed up until two o'clock waiting for me. As I was just a young stranger in the city, she could visualize all sorts of things happening to me. Al told me that Mama had come to look upon me as one of the family. I was free to come and go as I pleased, but she wouldn't worry so much if she knew of my plans.

I was very touched by this plea and resolved thereafter to be as good a son as I could to the Martinis.

At first I communicated with Mama and Papa primarily in smiles and gestures. Papa knew no English at all, and Mama's

knowledge was limited to one sentence which she would use when some of the young boys on the street were making noise below her window when she was trying to get her afternoon nap. She would then poke her head out of the window and shout: "Goddam-sonumabitcha! Geroutahere!"

Some weeks earlier, in anticipation of moving into the district, I had begun working on the Italian language myself with the aid of a Linguaphone. One morning now Papa Martini came by when I was talking to the phonograph record. He listened for a few moments in the hall trying to make sense out of this peculiar conversation. Then he burst in upon me with fascinated exclamations. We sat down together while I demonstrated the machine and the method to him. After that he delighted in working with me, and I called him my language professor. In a short time we reached a stage where I could carry on simple conversations, and, thanks to the Linguaphone and Papa Martini, the Italian that came out apparently sounded authentic. He liked to try to pass me off to his friends as *paesano mio*—a man from his own home town in Italy. When I was careful to keep my remarks within the limits of my vocabulary, I could sometimes pass as an immigrant from the village of Viareggio in the province of Tuscany.

Since my research developed so that I was concentrating almost exclusively upon the younger, English-speaking generation, my knowledge of Italian proved unnecessary for research purposes. Nevertheless, I feel certain that it was important in establishing my social position in Cornerville—even with that younger generation. There were schoolteachers and social workers who had worked in Cornerville for as much as twenty years and yet had made no effort to learn Italian. My effort to learn the language probably did more to establish the sincerity of my interest in the people than anything I could have told them of myself and my work. How could a researcher be planning to "criticize our people" if he went to the lengths of learning the language? With language comes understanding, and surely it is easier to criticize people if you do not understand them.

My days with the Martinis would pass in this manner. I would

get up in the morning around nine o'clock and go out to breakfast. Al Martini told me I could have breakfast in the restaurant, but, for all my desire to fit in, I never could take their breakfast of coffee with milk and a crust of bread.

After breakfast, I returned to my room and spent the rest of the morning, or most of it, typing up my notes regarding the previous day's events. I had lunch in the restaurant and then set out for the street corner. Usually I was back for dinner in the restaurant and then out again for the evening.

Usually I came home again between eleven and twelve o'clock, at a time when the restaurant was empty except perhaps for a few family friends. Then I might join Papa in the kitchen to talk as I helped him dry the dishes, or pull up a chair into a family conversation around one of the tables next to the kitchen. There I had a glass of wine to sip, and I could sit back and mostly listen but occasionally try out my growing Italian on them.

The pattern was different on Sunday, when the restaurant was closed at two o'clock, and Al's two brothers and his sister and the wives, husband, and children would come in for a big Sunday dinner. They insisted that I eat with them at this time and as a member of the family, not paying for my meal. It was always more than I could eat, but it was delicious, and I washed it down with two tumblers of Zinfandel wine. Whatever strain there had been in my work in the preceding week would pass away now as I ate and drank and then went to my room for an afternoon nap of an hour or two that brought me back completely refreshed and ready to set forth again for the corners of Cornerville.

Though I made several useful contacts in the restaurant or through the family, it was not for this that the Martinis were important to me. There is a strain to doing such field work. The strain is greatest when you are a stranger and are constantly wondering whether people are going to accept you. But, much as you enjoy your work, as long as you are observing and interviewing, you have a role to play, and you are not completely relaxed. It was a wonderful feeling at the end of a day's work to be able to come home to relax and enjoy myself with the family.

Probably it would have been impossible for me to carry on such a concentrated study of Cornerville if I had not had such a home from which to go out and to which I might return.

5. BEGINNING WITH DOC

I can still remember my first outing with Doc. We met one evening at the Norton Street House and set out from there to a gambling place a couple of blocks away. I followed Doc anxiously down the long, dark hallway at the back of a tenement building. I was not worried about the possibility of a police raid. I was thinking about how I would fit in and be accepted. The door opened into a small kitchen almost bare of furnishings and with the paint peeling off the walls. As soon as we went in the door, I took off my hat and began looking around for a place to hang it. There was no place. I looked around, and here I learned my first lesson in participant observation in Cornerville: Don't take off your hat in the house—at least not when you are among men. It may be permissible, but certainly not required, to take your hat off when women are around.

Doc introduced me as "my friend Bill" to Chichi, who ran the place, and to Chichi's friends and customers. I stayed there with Doc part of the time in the kitchen, where several men would sit around and talk, and part of the time in the other room watching the crap game.

There was talk about gambling, horse races, sex, and other matters. Mostly I just listened and tried to act friendly and interested. We had wine and coffee with anisette in it, with the fellows chipping in to pay for the refreshments. (Doc would not let me pay my share on this first occasion.) As Doc had predicted, no one asked me about myself, but he told me later that, when I went to the toilet, there was an excited burst of conversation in Italian and that he had to assure them that I was not a G-man. He said he told them flatly that I was a friend of his, and they agreed to let it go at that.

We went several more times together to Chichi's gambling joint, and then the time came when I dared to go in alone. When

I was greeted in a natural and friendly manner, I felt that I was now beginning to find a place for myself in Cornerville.

When Doc did not go off to the gambling joint, he spent his time hanging around Norton Street, and I began hanging with him. At first, Norton Street meant only a place to wait until I could go somewhere else. Gradually, as I got to know the men better, I found myself becoming one of the Norton Street gang.

Then the Italian Community Club was formed in the Norton Street Settlement, and Doc was invited to be a member. Doc maneuvered to get me into the club, and I was glad to join, as I could see that it represented something distinctly different from the corner gangs I was meeting.

As I began to meet the men of Cornerville, I also met a few of the girls. One girl I took to a church dance. The next morning the fellows on the street corner were asking me: "How's your steady girl?" This brought me up short. I learned that going to the girl's house was something that you just did not do unless you hoped to marry her. Fortunately, the girl and her family knew that I did not know the local customs, so they did not assume that I was thus committed. However, this was a useful warning. After this time, even though I found some Cornerville girls exceedingly attractive, I never went out with them except on a group basis, and I did not make any more home visits either.

As I went along, I found that life in Cornerville was not nearly so interesting and pleasant for the girls as it was for the men. A young man had complete freedom to wander and hang around. The girls could not hang on street corners. They had to divide their time between their own homes, the homes of girl friends and relatives, and a job, if they had one. Many of them had a dream that went like this: some young man, from outside of Cornerville, with a little money, a good job, and a good education would come and woo them and take them out of the district. I could hardly afford to fill this role.

6. TRAINING IN PARTICIPANT OBSERVATION

The spring of 1937 provided me with an intensive course in participant observation. I was learning how to conduct myself,

and I learned from various groups but particularly from the Nortons.

As I began hanging about Cornerville, I found that I needed an explanation for myself and for my study. As long as I was with Doc and vouched for by him, no one asked me who I was or what I was doing. When I circulated in other groups or even among the Nortons without him, it was obvious that they were curious about me.

I began with a rather elaborate explanation. I was studying the social history of Cornerville—but I had a new angle. Instead of working from the past up to the present, I was seeking to get a thorough knowledge of present conditions and then work from present to past. I was quite pleased with this explanation at the time, but nobody else seemed to care for it. I gave the explanation on only two occasions, and each time, when I had finished, there was an awkward silence. No one, myself included, knew what to say.

While this explanation had at least the virtue of covering everything that I might eventually want to do in the district, it was apparently too involved to mean anything to Cornerville people.

I soon found that people were developing their own explanation about me: I was writing a book about Cornerville. This might seem entirely too vague an explanation, and yet it sufficed. I found that my acceptance in the district depended on the personal relationships I developed far more than upon any explanations I might give. Whether it was a good thing to write a book about Cornerville depended entirely on people's opinions of me personally. If I was all right, then my project was all right; if I was no good, then no amount of explanation could convince them that the book was a good idea.

Of course people did not satisfy their curiosity about me simply by questions that they addressed to me directly. They turned to Doc, for example, and asked him about me. Doc then answered the questions and provided any reassurance that was needed.

I learned early in my Cornerville period the crucial importance of having the support of the key individuals in any groups or

organizations I was studying. Instead of trying to explain myself to everyone, I found I was providing far more information about myself and my study to leaders such as Doc than I volunteered to the average corner boy. I always tried to give the impression that I was willing and eager to tell just as much about my study as anyone wished to know, but it was only with group leaders that I made a particular effort to provide really full information.

My relationship with Doc changed rapidly in this early Cornerville period. At first he was simply a key informant—and also my sponsor. As we spent more time together, I ceased to treat him as a passive informant. I discussed with him quite frankly what I was trying to do, what problems were puzzling me, and so on. Much of our time was spent in this discussion of ideas and observations, so that Doc became, in a very real sense, a collaborator in the research.

This full awareness of the nature of my study stimulated Doc to look for and point out to me the sorts of observations that I was interested in. Often when I picked him up at the flat where he lived with his sister and brother-in-law, he said to me: "Bill, you should have been around last night. You would have been interested in this." And then he would go on to tell me what had happened. Such accounts were always interesting and relevant to my study.

Doc found this experience of working with me interesting and enjoyable, and yet the relationship had its drawbacks. He once commented: "You've slowed me up plenty since you've been down here. Now, when I do something, I have to think what Bill Whyte would want to know about it and how I can explain it. Before, I used to do things by instinct."

However, Doc did not seem to consider this a serious handicap. Actually, without any training he was such a perceptive observer that it only needed a little stimulus to help him to make explicit much of the dynamics of the social organization of Cornerville. Some of the interpretations I have made are his more than mine, although it is now impossible to disentangle them.

While I worked more closely with Doc than with any other individual, I always sought out the leader in whatever group I

was studying. I wanted not only sponsorship from him but also more active collaboration with the study. Since these leaders had the sort of position in the community that enabled them to observe much better than the followers what was going on and since they were in general more skilful observers than the followers, I found that I had much to learn from a more active collaboration with them.

In my interviewing methods I had been instructed not to argue with people or pass moral judgments upon them. This fell in with my own inclinations. I was glad to accept the people and to be accepted by them. However, this attitude did not come out so much in interviewing, for I did little formal interviewing. I sought to show this interested acceptance of the people and the community in my everyday participation.

I learned to take part in the street corner discussions on baseball and sex. This required no special training, since the topics seemed to be matters of almost universal interest. I was not able to participate so actively in discussions of horse-racing. I did begin to follow the races in a rather general and amateur way. I am sure it would have paid me to devote more study to the *Morning Telegraph* and other racing sheets, but my knowledge of baseball at least insured that I would not be left out of the street corner conversations.

While I avoided expressing opinions on sensitive topics, I found that arguing on some matters was simply part of the social pattern and that one could hardly participate without joining in the argument. I often found myself involved in heated but good-natured arguments about the relative merits of certain major-league ball players and managers. Whenever a girl or a group of girls would walk down the street, the fellows on the corner would make mental notes and later would discuss their evaluations of the females. These evaluations would run largely in terms of shape, and here I was glad to argue that Mary had a better "build" than Anna, or vice versa. Of course, if any of the men on the corner happened to be personally attached to Mary or Anna, no searching comments would be made, and I, too, would avoid this topic.

Sometimes I wondered whether just hanging on the street corner was an active enough process to be dignified by the term "research." Perhaps I should be asking these men questions. However, one has to learn when to question and when not to question as well as what questions to ask.

I learned this lesson one night in the early months when I was with Doc in Chichi's gambling joint. A man from another part of the city was regaling us with a tale of the organization of gambling activity. I had been told that he had once been a very big gambling operator, and he talked knowingly about many interesting matters. He did most of the talking, but the others asked questions and threw in comments, so at length I began to feel that I must say something in order to be part of the group. I said: "I suppose the cops were all paid off?"

The gambler's jaw dropped. He glared at me. Then he denied vehemently that any policemen had been paid off and immediately switched the conversation to another subject. For the rest of that evening I felt very uncomfortable.

The next day Doc explained the lesson of the previous evening. "Go easy on that 'who,' 'what,' 'why,' 'when,' 'where' stuff, Bill. You ask those questions, and people will clam up on you. If people accept you, you can just hang around, and you'll learn the answers in the long run without even having to ask the questions."

I found that this was true. As I sat and listened, I learned the answers to questions that I would not even have had the sense to ask if I had been getting my information solely on an interviewing basis. I did not abandon questioning altogether, of course. I simply learned to judge the sensitiveness of the question and my relationship to the people so that I only asked a question in a sensitive area when I was sure that my relationship to the people involved was very solid.

When I had established my position on the street corner, the data simply came to me without very active efforts on my part. It was only now and then, when I was concerned with a particular problem and felt I needed more information from a cer-

tain individual, that I would seek an opportunity to get the man alone and carry on a more formal interview.

At first I concentrated upon fitting into Cornerville, but a little later I had to face the question of how far I was to immerse myself in the life of the district. I bumped into that problem one evening as I was walking down the street with the Nortons. Trying to enter into the spirit of the small talk, I cut loose with a string of obscenities and profanity. The walk came to a momentary halt as they all stopped to look at me in surprise. Doc shook his head and said: "Bill, you're not supposed to talk like that. That doesn't sound like you."

I tried to explain that I was only using terms that were common on the street corner. Doc insisted, however, that I was different and that they wanted me to be that way.

This lesson went far beyond the use of obscenity and profanity. I learned that people did not expect me to be just like them; in fact, they were interested and pleased to find me different, just so long as I took a friendly interest in them. Therefore, I abandoned my efforts at complete immersion. My behavior was nevertheless affected by street corner life. When John Howard first came down from Harvard to join me in the Cornerville study, he noticed at once that I talked in Cornerville in a manner far different from that which I used at Harvard. This was not a matter of the use of profanity or obscenity, nor did I affect the use of ungrammatical expressions. I talked in the way that seemed natural to me, but what was natural in Cornerville was different from what was natural at Harvard. In Cornerville, I found myself putting much more animation into my speech, dropping terminal *g*'s, and using gestures much more actively. (There was also, of course, the difference in the vocabulary that I used. When I was most deeply involved in Cornerville, I found myself rather tongue-tied in my visits to Harvard. I simply could not keep up with the discussions of international relations, of the nature of science, and so on, in which I had once been more or less at home.)

As I became accepted by the Nortons and by several other groups, I tried to make myself pleasant enough so that people

would be glad to have me around. And, at the same time, I tried to avoid influencing the group, because I wanted to study the situation as unaffected by my presence as possible. Thus, throughout my Cornerville stay, I avoided accepting office or leadership positions in any of the groups with a single exception. At one time I was nominated as secretary of the Italian Community Club. My first impulse was to decline the nomination, but then I reflected that the secretary's job is normally considered simply a matter of dirty work—writing the minutes and handling the correspondence. I accepted and found that I could write a very full account of the progress of the meeting as it went on under the pretext of keeping notes for the minutes.

While I sought to avoid influencing individuals or groups, I tried to be helpful in the way a friend is expected to help in Cornerville. When one of the boys had to go downtown on an errand and wanted company, I went along with him. When somebody was trying to get a job and had to write a letter about himself, I helped him to compose it, and so on. This sort of behavior presented no problem, but, when it came to the matter of handling money, it was not at all clear just how I should behave. Of course, I sought to spend money on my friends just as they did on me. But what about lending money? It is expected in such a district that a man will help out his friends whenever he can, and often the help needed is financial. I lent money on several occasions, but I always felt uneasy about it. Naturally, a man appreciates it at the time you lend him the money, but how does he feel later when the time has come to pay, and he is not able to do so? Perhaps he is embarrassed and tries to avoid your company. On such occasions I tried to reassure the individual and tell him that I knew he did not have it just then and that I was not worried about it. Or I even told him to forget about the debt altogether. But that did not wipe it off the books; the uneasiness remained. I learned that it is possible to do a favor for a friend and cause a strain in the relationship in the process.

I know no easy solution to this problem. I am sure there will be times when the researcher would be extremely ill advised to refuse to make a personal loan. On the other hand, I am con-

vinced that, whatever his financial resources, he should not look for opportunities to lend money and should avoid doing so whenever he gracefully can.

If the researcher is trying to fit into more than one group, his field work becomes more complicated. There may be times when the groups come into conflict with each other, and he will be expected to take a stand. There was a time in the spring of 1937 when the boys arranged a bowling match between the Nortons and the Italian Community Club. Doc bowled for the Nortons, of course. Fortunately, my bowling at this time had not advanced to a point where I was in demand for either team, and I was able to sit on the sidelines. From there I tried to applaud impartially the good shots of both teams, although I am afraid it was evident that I was getting more enthusiasm into my cheers for the Nortons.

When I was with members of the Italian Community Club, I did not feel at all called upon to defend the corner boys against disparaging remarks. However, there was one awkward occasion when I was with the corner boys and one of the college boys stopped to talk with me. In the course of the discussion he said: "Bill, these fellows wouldn't understand what I mean, but I am sure that you understand my point." There I thought I had to say something. I told him that he greatly underestimated the boys and that college men were not the only smart ones.

While the remark fitted in with my natural inclinations, I am sure it was justified from a strictly practical standpoint. My answer did not shake the feelings of superiority of the college boy, nor did it disrupt our personal relationship. On the other hand, as soon as he left, it became evident how deeply the corner boys felt about his statement. They spent some time giving explosive expressions to their opinion of him, and then they told me that I was different and that they appreciated it and that I knew much more than this fellow and yet I did not show it.

My first spring in Cornerville served to establish for me a firm position in the life of the district. I had only been there several weeks when Doc said to me: "You're just as much of a fixture around this street corner as that lamppost." Perhaps the

greatest event signalizing my acceptance on Norton Street was the baseball game that Mike Giovanni organized against the group of Norton Street boys in their late teens. It was the old men who had won glorious victories in the past against the rising youngsters. Mike assigned me to a regular position on the team, not a key position perhaps (I was stationed in right field), but at least I was there. When it was my turn to bat in the last half of the ninth inning, the score was tied, there were two outs, and the bases were loaded. As I reached down to pick up my bat, I heard some of the fellows suggesting to Mike that he ought to put in a pinch-hitter. Mike answered them in a loud voice that must have been meant for me: "No, I've got confidence in Bill Whyte. He'll come through in the clutch." So, with Mike's confidence to buck me up, I went up there, missed two swings, and then banged a hard grounder through the hole between second and short. At least that is where they told me it went. I was so busy getting down to first base that I did not know afterward whether I had reached there on an error or a base hit.

That night, when we went down for coffee, Danny presented me with a ring for being a regular fellow and a pretty good ball player. I was particularly impressed by the ring, for it had been made by hand. Danny had started with a clear amber die discarded from his crap game and over long hours had used his lighted cigarette to burn a hole through it and to round the corners so that it came out a heart shape on top. I assured the fellows that I would always treasure the ring.

Perhaps I should add that my game-winning base hit made the score 18–17, so it is evident that I was not the only one who had been hitting the ball. Still, it was a wonderful feeling to come through when they were counting on me, and it made me feel still more that I belonged on Norton Street.

As I gathered my early research data, I had to decide how I was to organize the written notes. In the very early stage of exploration, I simply put all the notes, in chronological order, in a single folder. As I was to go on to study a number of different groups and problems, it was obvious that this was no solution at all.

I had to subdivide the notes. There seemed to be two main

possibilities. I could organize the notes topically, with folders for politics, rackets, the church, the family, and so on. Or I could organize the notes in terms of the groups on which they were based, which would mean having folders on the Nortons, the Italian Community Club, and so on. Without really thinking the problem through, I began filing material on the group basis, reasoning that I could later redivide it on a topical basis when I had a better knowledge of what the relevant topics should be.

As the material in the folders piled up, I came to realize that the organization of notes by social groups fitted in with the way in which my study was developing. For example, we have a college-boy member of the Italian Community Club saying: "These racketeers give our district a bad name. They should really be cleaned out of here." And we have a member of the Nortons saying: "These racketeers are really all right. When you need help, they'll give it to you. The legitimate businessman—he won't even give you the time of day." Should these quotes be filed under "Racketeers, attitudes toward"? If so, they would only show that there are conflicting attitudes toward racketeers in Cornerville. Only a questionnaire (which is hardly feasible for such a topic) would show the distribution of attitudes in the district. Furthermore, how important would it be to know how many people felt one way or another on this topic? It seemed to me of much greater scientific interest to be able to relate the attitude to the *group* in which the individual participated. This shows why two individuals could be expected to have quite different attitudes on a given topic.

As time went on, even the notes in one folder grew beyond the point where my memory would allow me to locate any given item rapidly. Then I devised a rudimentary indexing system: a page in three columns containing, for each interview or observation report, the date, the person or people interviewed or observed, and a brief summary of the interview or observation record. Such an index would cover from three to eight pages. When it came time to review the notes or to write from them, a five- to ten-minute perusal of the index was enough to give me a reasonably full picture of what I had and of where any given item could be located.

7. VENTURE INTO POLITICS

July and August, 1937, I spent away from Cornerville with my parents. Perhaps I was just too accustomed to the family summer vacation to remain in Cornerville, but at least I rationalized that I needed some time to get away and do some reading and get some perspective upon my study. The perspective was not easy to come by at that time. I still did not see the connecting link between a broad study of the life of the community and intensive studies of groups.

I came back feeling that I must somehow broaden my study. That might have meant dropping my contacts with the Nortons and the Italian Community Club in order to participate more heavily in other areas. Perhaps that would have been the logical decision in terms of the way I saw my Cornerville study at the time. Fortunately, I did not act that way. The club took only one evening a week, so there was no great pressure to drop that. The Nortons took much more time, and yet it meant something important to me to have a corner and a group where I was at home in Cornerville. At the time I did not clearly see that there was much more to a study of a group than an examination of its activities and personal relationships at a particular point in time. Only as I began to see changes in these groups did I realize how extremely important it is to observe a group over an extended period of time.

While I wandered along with the Nortons and the Italian Community Club more or less by a process of inertia, I decided I should expand the study by getting a broader and deeper view of the political life of the community. Street corner activities and politics in Cornerville were inextricably intertwined. There were several political organizations seeking to build up rival candidates. I felt that I could best gain an inside view of politics if I aligned myself actively with one political organization, yet I was afraid this might so label me that I would have difficulty

with my study afterward in relation to people who were against this particular politician.

The problem solved itself for me. In the fall of 1937 there was a mayoralty contest. An Irish politician who had formerly been mayor and governor of the state was running again. Among the good Yankees, Murphy's name was the personification of corruption. However, in Cornerville, he had a reputation for being a friend of the poor man and of the Italian people. Most of the Cornerville politicians were for him, and he was expected to carry the district by a tremendous majority. I therefore decided that it would be a good thing for my study if I could get my start in politics working for this man. (Among my Harvard associates, this new political allegiance led to some raised eyebrows, but I rationalized that a complete novice could hardly be of any influence in securing the election of the notorious politician.)

In order to enlist in the campaign, I had to have some sort of local connection. I found this with George Ravello, state senator representing our ward and two others. At the restaurant where I lived, I met Paul Ferrante, who was Ravello's secretary and also a friend of the Martini family. Ferrante's services to Ravello were entirely on a volunteer basis. Paul was unemployed at the time and was working for the politician in hopes that he would some day get a political job out of it.

After a little preliminary discussion, I enlisted as the unpaid secretary of the unpaid secretary of the state senator for the duration of the mayoralty campaign. When that election was over, I re-enlisted, for there was a special election for a vacant seat in Congress, and George Ravello was running for that office. Fortunately for my study, all the other Cornerville politicians were at least officially for Ravello, since he was running against several Irishmen. I therefore felt that I could be active in his campaign without creating barriers for myself anywhere else in the district.

As a campaign worker for the state senator, I was a complete anomaly. Most workers in such campaigns can at least claim to be able to deliver substantial numbers of votes; I could not pledge anything but my own. It was hard for the organization to

get used to this. On one occasion, George Ravello gave me a ride up to the State House, in the course of which he wanted to know when I was going to deliver him the indorsement of the Italian Community Club. This was quite a touchy topic within the club at the time. On the one hand, all the members were interested in seeing an Italian-American advance to higher office, and yet they were embarrassed by being identified with George Ravello. The language he used in public was hardly refined, and he had gained publicity that had embarrassed the young men on several occasions. There was, for example, the time when a woman was testifying against a bill introduced into the senate by Ravello. Ravello got angry in the midst of the hearing and threatened to throw the good woman off the wharf and into the harbor if she ever set foot in his district. On another occasion, the newspapers carried a picture of Ravello with a black eye, which he had received in a fight with a member of the State Parole Board.

I explained to Ravello that it was against the policy of the club to indorse candidates for any public office. While this happened to be true, it was hardly a satisfactory explanation to the Senator. Still, he did not press the matter further, perhaps recognizing that the support of the Italian Community Club did not count for very much anyway.

Not being able to deliver votes, I sought to make myself useful by running errands and doing various odd jobs, such as nailing up Ravello posters in various parts of the district.

I am sure no one thought I was any real help to the Senator's campaign, but neither did I appear to be doing any harm, so I was allowed to hang around in the quarters which served as a combination political office and funeral parlor.

I found this one of the more unpleasant places to hang around, because I never was able to gain complete scientific detachment regarding funeral parlors. One of my most vivid and unpleasant memories of Cornerville stems from this period. One of the Senator's constituents had died. The stairs to his flat being too narrow to accommodate the casket, the deceased was laid out for friends and family in the back room of the funeral parlor. Unfortunately,

he was laid out in two pieces, since he had had his leg amputated shortly before his death. The rest of his body had been embalmed, but I was told that there was no way of embalming a detached leg. The gangrenous leg gave off a most sickening odor. While family and friends came in to pay their last respects, we political workers sat in the front part of the office trying to keep our attention on politics. Now and then Paul Ferrante went about the room spraying perfume. The combination of perfume with the gangrenous stench was hardly an improvement. I stayed at my post through the day but finished up a trifle nauseated.

Since the politicians did not know what to do with my services and yet were willing to have me hang around, I found that I could develop my own job description. Before one of the meetings of the political workers, I suggested to Carrie Ravello—the candidate's wife and the real brains of the family—that I serve as secretary for such meetings. I then took notes while the meeting proceeded and typed her out a summary for later use. (The invention of carbon paper enabled me to retain my own copy of all the records.)

Actually, it was of no importance for the organization to have such a record. Although they were officially considered meetings to discuss political strategy and tactics, they were only pep rallies for the second string of political powers supporting Ravello. I never did get in on the top-level political discussions where the real decisions were made. However, my note-taking at these political meetings did give me a fully documented record of one area of activity. From here I went on to the large-scale political rally, where I sought to record on the spot the speeches and other activities of the leading Ravello supporters.

When election day came, I voted as the polls opened and then reported for duty at the candidate's headquarters. There I found I had been assigned to work with Ravello's secretary in another ward. I spent the first part of election day outside of Cornerville following Ferrante around and being of no real use to myself or to the organization. I did not worry about my contribution, because I was getting a growing impression that a lot of what passed under the name of political activity was simply a

waste of time. On election-day morning we stopped in to chat with a number of friends of Paul Ferrante and had a drink or a cup of coffee here and there. Then we drove around to offer voters transportation to the polls, which in such a crowded district would be just around the corner. We made about thirty stops and took one voter to the polls, and she said she had been going to walk down in five minutes anyway. The others were either not home or told us they were going to walk down later.

At two o'clock I asked if it would be all right for me to leave and return to my ward. This was readily granted, so I was able to spend the rest of the day in Cornerville.

When I got home, I began hearing alarming reports from the home ward of the Irish politician who was Ravello's chief rival. He was said to have a fleet of taxicabs cruising about his ward so that each of his repeaters would be able to vote in every precinct of the ward. It became clear that, if we did not steal the election ourselves, this low character would steal it from us.

Around five o'clock one of the senator's chief lieutenants rushed up to a group of us who were hanging on the corner across the street from my home polling place. He told us that Joseph Maloney's section of our ward was wide open for repeaters, that the cars were ready to transport them, and that all he needed were a few men to get to work. At the moment the organization was handicapped by a shortage of manpower to accomplish this important task. The senator's lieutenant did not ask for volunteers; he simply directed us to get into the cars to go to the polling places where the work could be done. I hesitated a moment, but I did not refuse.

Before the polls had closed that night, I had voted three more times for George Ravello—really not much of a feat, since another novice who had started off at the same time as I managed to produce nine votes in the same period of time. Two of my votes were cast in Joseph Maloney's end of the ward; the third was registered in my home polling place.

I was standing on the corner when one of the politician's henchmen came up to me with the voting list to ask me to go in. I explained that this was my home polling place and that I had

already voted under my own name. When they learned that this had been when the polls opened, they told me that I had nothing to worry about and that a new shift was now on duty. They had the name of Frank Petrillo picked out for me. They told me that Petrillo was a Sicilian fisherman who was out to sea on election day, so we were exercising his democratic rights for him. I looked at the voting list to discover that Petrillo was forty-five years old and stood five feet nine. Since I was twenty-three and six feet three, this seemed implausible to me, and I raised a question. I was assured that this made no difference at all, since the people inside the polling place were Joe Maloney's people. I was not completely reassured by this, but, nevertheless, I got in line to wait my new turn in the rush of the hour before the polls closed.

I gave my name, and the woman at the gate checked me in, I picked up my ballot, went back to the booth, and marked it for George Ravello. As I was about to put the ballot into the box, this woman looked me over and asked me how old I was. Suddenly the ridiculousness of my masquerade struck home to me. I knew I was supposed to say forty-five, but I could not voice such an absurd lie. Instead, I compromised on twenty-nine. She asked how tall I was, and again I compromised, giving the figure as six feet. They had me all right, but still the questioning went on. The woman asked me how I spelled my name. In the excitement I spelled it wrong. The other woman checker now came over and asked me about my sisters. I thought I had recalled seeing the names of some female Petrillos on the list, and, in any case, if I invented names that did not appear, they could be names of women who were not registered. I said, "Yes, I have two sisters." She asked their names. I said, "Celia and Florence."

She leered at me and asked, "What about this Marie Petrillo?"

I took a deep breath and said, "She's my cousin."

They said they would have to challenge my vote. They called for the warden in charge of the polling place.

I had a minute to wait before he stepped forward, and that was plenty of time to mull over my future. I could see before my eyes large headlines on the front pages of Eastern City's tabloids—HARVARD FELLOW ARRESTED FOR REPEATING. Why wouldn't they play it up? Indeed, this was an ideal man-bites-dog newspaper story. In that moment I resolved that at least I would not mention my connection with Harvard or my Cornerville study when I was arrested.

The warden now stepped up, said he would have to challenge my vote, and asked me to write my name on the back of the ballot. I went over to the booth. But, by this time, I was so nervous that I forgot what my first name was supposed to be and put down "Paul." The warden took my ballot and looked at the back of it. He had me swear that this was my name and that I had not voted before. I did so. I went through the gate. He told me to stop. As I looked over the crowd coming in, I thought of trying to run for it, but I did not break away. I came back. He looked at the book of registered voters. He turned back to the booth, and for a moment his back was to me. Then I saw him scratch out the name I had written on the back of the ballot. He put the ballot into the box, and it registered with a ring of the bell. He told me I could go out, and I did, trying to walk in a calm and leisurely manner.

When I was out on the street, I told the politician's lieutenant that my vote had been challenged. "Well, what do you care? We didn't lose anything by it." Then I told him that the vote had finally gone through. "Well, so much the better. Listen, what could they have done to you? If the cops had taken you in, they wouldn't hold you. We would fix you up."

I did not eat well that night. Curiously enough, I did not feel nearly so guilty over what I had done until I had thought that I was going to be arrested. Up to that point, I had just gone numbly along. After supper, I went out to look up Tony Cardio of the Italian Community Club. As I had walked into his home precinct to repeat, I encountered him coming out of the polling place. As we passed, he grinned at me and said: "They're working you pretty hard today, aren't they?" I immediately jumped to the conclusion that he must know that I was going in to repeat. Now I felt that I must see him as soon as possible to explain in the best way that I could what I had been doing and why. Fortunately for me, Tony was not home that night. As my anxiety

subsided, I recognized that, simply because I knew my own guilt, it did not necessarily follow that everybody else and Tony knew what I had done. I confirmed this indirectly when I had a conversation with Tony later about the election. He raised no question concerning my voting activities.

That was my performance on election day. What did I gain from it? I had seen through firsthand personal experience how repeating was accomplished. But this was really of very little value, for I had been observing these activities at quite close range before, and I could have had all the data without taking any risk. Actually, I learned nothing of research value from the experience, and I took a chance of jeopardizing my whole study. While I escaped arrest, these things are not always fixed as firmly as the politician's henchman think they are. A year later, when I was out of town at election time, somebody was actually arrested for voting in *my* name.

Even apart from the risk of arrest, I faced other possible losses. While repeating was fairly common in our ward, there were only relatively few people who engaged in it, and they were generally looked down upon as the fellows who did the dirty work. Had the word got around about me, my own standing in the district would have suffered considerable damage. So far as I know, my repeating never did become known beyond some of the key people in Ravello's organization. Most of my repeating had been done outside of Cornerville, and my Norton Street friends did not vote in the same precinct where I put in my second Cornerville vote. I had not been observed by anyone whose opinion could damage me. Furthermore, I was just plain lucky that I did not reveal myself to Tony Cardio; in fact, I was lucky at every point.

The experience posed problems that transcended expediency. I had been brought up as a respectable, law-abiding, middle-class citizen. When I discovered that I was a repeater, I found my conscience giving me serious trouble. This was not like the picture of myself that I had been trying to build up. I could not laugh it off simply as a necessary part of the field work. I knew that it was not necessary; at the point where I began to repeat, I could have refused. There were others who did refuse to do it. I had simply got myself involved in the swing of the campaign and let myself be carried along. I had to learn that, in order to be accepted by the people in a district, you do not have to do everything just as they do it. In fact, in a district where there are different groupings with different standards of behavior, it may be a matter of very serious consequence to conform to the standards of one particular group.

I also had to learn that the field worker cannot afford to think only of learning to live with others in the field. He has to continue living with himself. If the participant observer finds himself engaging in behavior that he has learned to think of as immoral, then he is likely to begin to wonder what sort of a person he is after all. Unless the field worker can carry with him a reasonably consistent picture of himself, he is likely to run into difficulties.

8. BACK ON NORTON STREET

When the campaign was over, I went back to Norton Street, I did not sever my ties with the Ravello organization altogether. For this there were two reasons: I wanted to maintain my connections for possible further research in politics; but then also I did not want them to think of me as just another of those "phonies" who made a fuss over the politician when he seemed to have a chance to win and abandoned him when he lost. Still, I had no strong personal tie to hold me to the organization. Carrie Ravello I liked and respected; the Senator puzzled and interested me, but I never felt that I got to know him. His one-time secretary just dropped out of sight for a while after the election—still owing me ten dollars. The others did not really matter to me personally. And, as I review my notes today, even their names have little meaning.

As I became more active once again on Norton Street, the local world began to look different. The world I was observing was in a process of change. I saw some of the members of the Italian Community Club establishing contacts with the upper world of Yankee control as I followed them to All-American Night at the Women's Republican Club. I saw the stresses and strains within

the Nortons growing out of contacts with the Aphrodite Club and the Italian Community Club. I watched Doc, completely without scientific detachment, as he prepared for his doomed effort to run for public office.

Then in April, 1938, one Saturday night I stumbled upon one of my most exciting research experiences in Cornerville. It was the night when the Nortons were to bowl for the prize money; the biggest bowling night of the whole season. I recall standing on the corner with the boys while they discussed the coming contest. I listened to Doc, Mike, and Danny making their predictions as to the order in which the men would finish. At first, this made no particular impression upon me, as my own unexpressed predictions were exactly along the same lines. Then, as the men joked and argued, I suddenly began to question and take a new look at the whole situation. I was convinced that Doc, Mike, and Danny were basically correct in their predictions, and yet why should the scores approximate the structure of the gang? Were these top men simply better natural athletes than the rest? That made no sense, for here was Frank Bonnelli, who was a good enough athlete to win the promise of a tryout with a major-league baseball team. Why should not Frank outdo us all at the bowling alley? Then I remembered the baseball game we had had a year earlier against the younger crowd on Norton Street. I could see the man who was by common consent the best baseball player of us all striking out with long, graceful swings and letting the grounders bounce through his legs. And then I remembered that neither I nor anyone else seemed to have been surprised at Frank's performance in this game. Even Frank himself was not surprised, as he explained: "I can't seem to play ball when I'm playing with fellows I know like that bunch."

I went down to the alleys that night fascinated and just a bit awed by what I was about to witness. Here was the social structure in action right on the bowling alleys. It held the individual members in their places—and I along with them. I did not stop to reason then that, as a close friend of Doc, Danny, and Mike, I held a position close to the top of the gang and therefore should be expected to excel on this great occasion. I simply felt myself

buoyed up by the situation. I felt my friends were for me, had confidence in me, wanted me to bowl well. As my turn came and I stepped up to bowl, I felt supremely confident that I was going to hit the pins that I was aiming at. I have never felt quite that way before—or since. Here at the bowling alley I was experiencing subjectively the impact of the group structure upon the individual. It was a strange feeling, as if something larger than myself was controlling the ball as I went through my swing and released it toward the pins.

When it was all over, I looked at the scores of all the other men. I was still somewhat bemused by my own experience, and now I was excited to discover that the men had actually finished in the predicted order with only two exceptions that could readily be explained in terms of the group structure.

As I later thought over the bowling-alley contest, two things stood out in my mind. In the first place, I was convinced that now I had something important: the relationship between individual performance and group structure, even though at this time I still did not see how such observation would fit in with the over-all pattern of the Cornerville study. I believed then (and still believe now) that this sort of relationship may be observed in other group activities everywhere. As an avid baseball fan, I had often been puzzled by the records of some athletes who seemed to be able to hit and throw and field with superb technical qualifications and yet were unable to make the major-league teams. I had also been puzzled by cases where men who had played well at one time suddenly failed badly, whereas other men seemed to make tremendous improvements that could not be explained simply on the basis of increasing experience. I suspect that a systematic study of the social structure of a baseball team, for example, will explain some of these otherwise mysterious phenomena. The other point that impressed me involved field research methods. Here I had the scores of the men on that final night at the bowling alleys. This one set of figures was certainly important, for it represented the performance of the men in the event that they all looked upon as the climax of the year. However, this same group had been bowling every Saturday night for

many months, and some of the members had bowled on other nights in between. It would have been a ridiculously simple task for me to have kept a record for every string bowled by every man on every Saturday night of that season and on such other evenings as I bowled with the men. This would have produced a set of statistics that would have been the envy of some of my highly quantitative friends. I kept no record of these scores, because at this time I saw no point to it. I had been looking upon Saturday night at the bowling alleys as simply recreation for myself and my friends. I found myself enjoying the bowling so much that now and then I felt a bit guilty about neglecting my research. I was bowling with the men in order to establish a social position that would enable me to interview them and observe important things. But what were these important things? Only after I passed up this statistical gold mine did I suddenly realize that the behavior of the men in the regular bowling-alley sessions was the perfect example of what I should be observing. Instead of bowling in order to be able to observe something else, I should have been bowling in order to observe bowling. I learned then that the day-to-day routine activities of these men constituted the basic data of my study.

9. REPLANNING THE RESEARCH

The late spring and summer of 1938 brought some important changes into my research.

On May 28, I was married to Kathleen King, and three weeks later we returned to Cornerville together. Kathleen had visited me at the restaurant and had met some of my friends. Even as a married man, I did not want to move out of the district, and Kathleen, fortunately, was eager to move in. This presented problems, because, while we were not asking for everything, we did hope to find an apartment with a toilet and bathtub inside it. We looked at various gloomy possibilities until at last we found a building that was being remodeled on Shelby Street. Some of my Norton Street friends warned us against the neighborhood, saying that the place was full of Sicilians who were a very cut-throat crowd. Still, the apartment had the bathtub and toilet

and was clean and relatively airy. It had no central heating, but we could be reasonably comfortable with the kitchen stove.

Now that we were two, we could enter into new types of social activites, and Kathleen could learn to know some of the women as I had become acquainted with the men. However, these new directions of social activity were something for the future. My problem now was to find where I was and where I was going. This was a period of stocktaking.

In describing my Cornerville study, I have often said I was eighteen months in the field before I knew where my research was going. In a sense, this is literally true. I began with the general idea of making a community study. I felt that I had to establish myself as a participant observer in order to make such a study. In the early months in Cornerville I went through the process that sociologist Robert Johnson has described in his own field work. I began as a nonparticipating observer. As I became accepted into the community, I found myself becoming almost a nonobserving participant. I got the feel of life in Cornerville, but that meant that I got to take for granted the same things that my Cornerville friends took for granted. I was immersed in it, but I could as yet make little sense out of it. I had a feeling that I was doing something important, but I had yet to explain to myself what it was.

Fortunately, at this point I faced a very practical problem. My three-year fellowship would run out in the summer of 1939. The fellowship could be renewed for a period up to three years. Applications for renewal were due in the early spring of 1939.

I was enjoying Cornerville, and I felt that I was getting somewhere, yet at the same time I felt that I needed at least three more years. I realized that so far I had little to show for the time I had spent. When I submitted my application for renewal, I must also submit some evidence that I had acquitted myself well in the first three-year period. I would have to write something. I had several months in which to do the writing, but the task at first appalled me. I sat down to ask myself what it was in Cornerville upon which I had reasonably good data. Was there anything ready to be written up? I pondered this and talked it

over with Kathleen and with John Howard, who was working with me in the district.

Still thinking in terms of a community study, I recognized that I knew very little about family life in Cornerville, and my data were very thin upon the church, although John Howard was beginning to work on this area. I had been living with the restaurant family in a room that overlooked the corner where T. S., the most prominent Cornerville racketeer, sometimes was seen with his followers. I had looked down upon the group many times from my window, and yet I had never met the men. Racketeering was of obvious importance in the district, yet all I knew about it was the gossip I picked up from men who were only a little closer to it than I. I had much more information regarding political life and organization, but even here I felt that there were so many gaps that I could not yet put the pieces together.

If these larger areas were yet to be filled in, what on earth did I have to present? As I thumbed through the various folders, it was obvious that the Norton and Community Club folders were fatter than the rest. If I knew anything about Cornerville, I must know it about the Nortons and the Italian Community Club. Perhaps, if I wrote up these two stories, I would begin to see some pattern in what I was doing in Cornerville.

As I wrote the case studies of the Nortons and of the Italian Community, a pattern for my research gradually emerged in my mind.

I realized at last that I was not writing a community study in the usual sense of that term. The reader who examines *Middletown* will note that it is written about people in general in that community. Individuals or groups do not figure in the story except as they illustrate the points that the authors are making (the sequel, *Middletown in Transition*, presents one exception to this description with a chapter on the leading family of the community). The reader will further note that *Middletown* is organized in terms of such topics as getting a living, making a home, training the young, and using leisure.

The Lynds accomplished admirably the task they set out to

accomplish. I simply came to realize that my task was different. I was dealing with particular individuals and with particular groups.

I realized also that there was another difference that I had stumbled upon. I had assumed that a sociological study should present a description and analysis of a community at one particular point in time, supported of course by some historical background. I now came to realize that *time* itself was one of the key elements in my study. I was observing, describing, and analyzing groups as they evolved and changed through time. It seemed to me that I could explain much more effectively the behavior of men when I observed them over time than would have been the case if I had got them at one point in time. In other words, I was taking a moving picture instead of a still photograph.

But, if this was a study of particular individuals and there were more than twenty thousand people in the district, how could I say anything significant about Cornerville on this individual and group basis? I came to realize that I could only do so if I saw individuals and groups in terms of their positions in the social structure. I also must assume that, whatever the individual and group differences were, there were basic similarities to be found. Thus I would not have to study every corner gang in order to make meaningful statements about corner gangs in Cornerville. A study of one corner gang was not enough, to be sure, but, if an examination of several more showed up the uniformities that I expected to find, then this part of the task became manageable.

On the Italian Community Club, I felt that I needed no additional data. There were few enough college men in Cornerville at the time, so that this one group represented a large sample of people in this category. It also seemed to me that they represented significant points in the social structure and in the social mobility process. There would certainly be others like them coming along after they had left the district, even as the Sunset Dramatic Club had gone before them. Furthermore, examination of their activities showed up important links with Republican politics and with the settlement house.

I now began to see the connection between my political study and the case study of the corner gang. The politician did not seek to influence separate individuals in Cornerville; consciously or unconsciously he sought out group leaders. So it was men like Doc who were the connecting links between their groups and the larger political organization. I could now begin writing my study by examining particular groups in detail, and then I could go on to relate them to the larger structures of the community. With this pattern in mind, I came to realize that I had much more data on politics than I had thought.

There were sill important gaps in my study. My knowledge of the role of the church in the community was fragmentary, and this I hoped to fill in. I had done no systematic work upon the family. On the one hand, it seemed inconceivable that one could write a study of Cornerville without discussing the family; yet, at the same time, I was at a loss as to how to proceed in tying family studies into the organization of the book as it was emerging in my mind. I must confess also that for quite unscientific reasons I have always found politics, rackets, and gangs more interesting than the basic unit of human society.

The gap that worried me most was in the area of the rackets and the police. I had a general knowledge of how the rackets functioned, but nothing to compare with the detailed interpersonal data I had upon the corner gang. As my book was evolving, it seemed to me that this was the gap that simply must be filled, although at the time I had no idea how I would get the inside picture that was necessary.

I finished the writing of my first two case studies and submitted them in support of my application for a renewal of the fellowship. Some weeks later I received my answer. The fellowship had been renewed for one year instead of the three for which I had been hoping. At first, I was bitterly disappointed. As I was just beginning to get my bearings, I did not see how it would be possible to finish an adequate study in the eighteen months that then remained.

I am now inclined to believe that this one year cut-off was a very good thing for me and my research. In a sense, the study

of a community or an organization has no logical end point. The more you learn, the more you see that there is to learn. If I had had three years instead of one, my study would have taken longer to complete. Perhaps it might have been a better study. On the other hand, when I knew I had just eighteen months to go, I had to settle down and think through my plans more thoroughly and push ahead with the research and writing much more purposefully.

10. AGAIN THE CORNER GANG

The most important steps I took in broadening my study of street corner gangs grew out of Doc's recreation center project, although at first I had some other interests in mind. It began with one of my periodic efforts to get Doc a job. When I heard that the Cornerville House had finally been successful in getting its grant to open three store-front recreation centers, I sought to persuade Mr. Smith, the director, to man them with local men who, like Doc, were leaders in their groups. I found that he had planned to man them with social workers trained in group work. When I realized that it was hopeless to get him to select three local men, I tried to urge at least Doc upon him. I could see that Mr. Smith was tempted by the idea and afraid of it at the same time. When I brought Doc in to meet him, I found that I lost ground instead of gaining it, for, as Doc told me later, he had got a dizzy spell there in the settlement-house office, and he had been in no condition to make a favorable personal impression. If Doc and I had figured out correctly the underlying causes for his dizzy spells, then a steady job and the money that would enable him to resume his customary pattern of social activity would cure these neurotic symptoms. On the other hand, I could hardly explain this to Mr. Smith. I was afraid that it appeared that I was simly trying to do a favor for a friend. As my last effort in this direction, I turned over to Mr. Smith a copy of my case study of the Nortons—and asked him please to keep it confidential, since I was not ready to publish.

This made the difference. Mr. Smith agreed to hire Doc.

As the preliminary activities of setting up the recreation cen-

ters got under way, I began to worry about my confident predictions of Doc's success. In the preliminary meetings to discuss plans for the centers, Doc was passive and apparently apathetic. Nevertheless, almost from the moment that Doc's center opened, it became apparent that it was to be a success.

On one of my early visits to Doc's center, he introduced me to Sam Franco, who was to play a far more important part in my study than brief mentions of him in the book indicate. Doc met Sam the night his center opened. Sam's gang was hanging around outside of the center looking the place over. Sam came in as the emissary of his group—a move which immediately identified him as the leader to Doc. The two men discussed the center briefly, and then Sam went out and brought his gang in. By the second night of the center, Sam had become Doc's lieutenant in its administration. Doc knew a few people in this part of the district, but Sam knew everybody.

Doc knew that I was trying to extend my corner gang study, and he suggested that Sam might be the man to help me. Doc had already learned that Sam had been keeping a scrapbook with newspaper accounts of Cornerville activities and some personal material on his own group.

I invited Sam and his scrapbook up to our apartment. There I learned that Sam had got started on his scrapbook after an experience on a National Youth Administration Project, where he had been working for a man who was writing a study of the problems of youth in this region. The scrapbook was completely miscellaneous and undirected, but it did have one part that particularly interested me. Sam had a section for his own gang with one page for each member. At the top of the page was a line drawing (from memory) of the individual, and then he wrote in such points as age, address, education, job, and ambition. (Usually he had written "none" opposite the heading, "ambition.")

My task was now to persuade Sam that, while it was fine to look upon these men as individuals, it was even better to look upon them in terms of their relations with each other. I had only begun my explanation when Sam got the point and accepted it

with enthusiasm. Of course, this was the sort of thing he knew; he had so taken it for granted that it had not occurred to him how important it might be. From this point on until the end of my study Sam Franco was my research assistant. I even managed to get Harvard to pay a hundred dollars for his services.

We began with an analysis of Sam's own gang, the Millers. We also looked at other gangs that came into Doc's recreation center. Here we had the great advantage of having two sharp observers checking each other on the same groups. I was reassured to find that they were in complete agreement on the top-leadership structure of every gang—with one exception. This one exception did trouble me until the explanation presented itself.

I had spent part of one afternoon listening to Doc and Sam argue over the leadership of one gang. Doc claimed that Carl was the man; Sam argued that it was Tommy. Each man presented incidents that he had observed in support of his point of view. The following morning Sam rushed up to my house with this bulletin: "You know what happened last night? Carl and Tommy nearly had it out. They got into a big argument, and now the gang is split into two parts with some of them going with Carl and the rest going with Tommy." So their conflicting views turned out to be an accurate representation of what was taking place in the gang.

As I worked on these other gang studies, I assumed that I had finished my research on the Nortons. Still, I kept in close touch with Doc, and, just for recreation, I continued to bowl with the remnants of the Nortons on some Saturday nights.

With my attention directed elsewhere, I failed to see what was happening among the Nortons right before my eyes. I knew Long John was not bowling as he had in previous years, and I also knew that he was not as close to Doc, Danny, and Mike as he had been. I had noticed that, when Long John was on Norton Street, the followers badgered him more aggressively than they ever had before. I must have assumed some connection among these phenomena, and yet I did not make much of the situation until Doc came to me and told me of Long John's psychological difficulties.

It was as if this information set off a flash bulb in my head. Suddenly all the pieces of the puzzle fell together. The previous season, I had stumbled upon the relationship between position in the group and performance at the bowling alleys. I now saw the three-way connection between group position, performance, and mental health. And not only for Long John. Doc's dizzy spells seemed to have precisely the same explanation.

We could put it more generally in this way. The individual becomes accustomed to a certain pattern of interaction. If this pattern is subject to a drastic change, then the individual can be expected to experience mental health difficulties. That is a very crude statement. Much further research would be needed before we could determine the degree of change necessary, the possibilities of compensating with interactions in other social areas, and so on. But here at least was one way of tying together human relations and psychological adjustment.

Furthermore, here was an opportunity to experiment in therapy. If my diagnosis was correct, then the line of treatment was clear: re-establish something like Long John's pre-existing pattern of interaction, and the neurotic symptoms should disappear. This was the first real opportunity to test my conclusions on group structure. I embraced it with real enthusiasm.

Convinced as I was of the outcome that should follow, I must confess that I was somewhat awestruck when, under Doc's skilfully executed therapy program, Long John not only lost his neurotic symptoms but also closed out the season by winning the prize money in the final bowling contest. Of course, this victory was not necessary to establish the soundness of the diagnosis. It would have been enough for Long John to have re-established himself *among* the top bowlers. His five-dollar prize was just a nice bonus for interaction theory.

11. STUDYING RACKETEERING

My meeting with Tony Cataldo, the prominent Cornerville racketeer, came about almost by chance. I dropped in one afternoon at the restaurant where I had first lived in Cornerville. Ed Martini, Al's older brother, was there at the time. He was grumbling about a pair of banquet tickets he had had to buy from a local policeman. He said that his wife did not want to go to banquets; perhaps I might like to accompany him.

I asked what the occasion was. He told me that the banquet was in honor of the son of the local police lieutenant. The young man had just passed his bar examinations and was starting out on his legal career. I thought a moment. It was perfectly obvious what sorts of people would be present at the banquet: mainly policemen, politicians, and racketeers. I decided that this might be an opportunity for me.

At the banquet hall, Ed and I took up our position in the lounge outside the men's room. Here we encountered Tony Cataldo and one of his employees, Rico Deleo. It turned out that Ed Martini knew Tony slightly and that Rico lived right across the street from me. Rico asked what I was doing, and I said something about writing a book on Cornerville. Tony said he had seen me around taking photographs of the *feste* that had been staged on Shelby Street the previous summer. This proved to be a fortunate association in his mind, since I could talk quite freely about what I had been trying to learn of the *feste*—which were actually just a minor interest in the research.

The four of us went up to a banquet table together, where we had to wait more than an hour for our food. We munched on olives and celery and sympathized with one another over the poor service. After the dinner we stepped downstairs and bowled three strings together. By this time, Tony was quite friendly and invited me to stop in at his store any time.

I paid several visits to the back room of the store from which Tony operated some of his business. A week after we had met, Tony invited Kathleen and me to dinner at his home. His wife, an attractive young girl, told us later that he had spoken of us as a Harvard professor and a commercial artist. She was very upset that he gave her only one day's notice for the dinner when she felt she needed at least a week to prepare for such important personages. The food was nevertheless quite elaborate, and each course seemed like a whole meal. After dinner, Tony drove us out

to meet some of his relatives in one of the suburbs. Then we all went bowling together.

We had dinner twice at their home, and they came to ours twice. On each occasion, apart from the small talk, the research pattern was similar. We talked some about the *feste*, about the club life of the *paesani* from the old country, and about such things which Tony associated with my study. Then, I gradually eased him into a discussion of his business. The discussion seemed to move naturally in this direction. It was just like a friend asking a legitimate businessman about the progress he was making and the problems he was meeting. Tony seemed glad to unburden himself.

I now felt optimistic about my future in racketeering. We seemed to be getting along very well with the Cataldos, and I was ready to follow Tony into the new field. However, after the first exchanges of sociability, Tony seemed to lose interest in us.

I was puzzled by this sudden cooling-off. I am not sure I have the full explanation, but I think there were at least two parts of it.

In the first place, Tony ran into a business crisis at about this time. Some men broke into his horse room one afternoon, held it up, and took all the money from the customers and from Tony. In order to maintain good relations with his customers, Tony had to reimburse them for the robbery, so that afternoon was doubly costly. It was also most frustrating, because, as the men were making their getaway, Tony could look out of the window and see them running right beneath him. He had a clear shot at them, yet he could not shoot, because he knew that a shooting would close down gambling in Cornerville like nothing else. As long as these things were done quietly, the "heat" was not so likely to be on.

This might have accounted for an interruption in our social life together but hardly for a complete cessation. It seems to me that the other factor was a problem in social status and mobility. At first, Tony had built me up to his wife—and probably to friends and relatives also—as a Harvard professor. Both the Cataldos were highly status-conscious. They did not allow their young son to play with the local riffraff. They explained that they

only lived in the district because it was necessary for business reasons and that they still hoped to move out. When we were their guests, they introduced us to their friends and relatives who lived in more fashionable parts of the city.

On the other hand, when the Cataldos came to our house for dinner they just met with us and nobody else. Furthermore, Tony was now seeing me associating with the men on Shelby Street who were distinctly small fry to him. At first, he had thought that his contact with me was something important; now, perhaps, he considered it insignificant.

To some extent, I was aware of this risk and thought of the possibility of having Harvard friends in to dinner with the Cataldos. I had been keeping the two worlds apart. One Harvard friend, a symbolic logician, had once asked me to introduce him to a crap game. He explained that he had figured out mathematically how to win in a crap game. I explained that my crap-shooting friends had reached the same mathematical conclusion by their rule-of-thumb method, and I begged off from this adventure. On another occasion, we had the wife of one of my Harvard associates visiting us when one of the local men dropped in. Sizing up his new audience, he began regaling her with accounts of famous murders that had taken place in Cornerville in recent years. She listened with eyes wide open. At the end of one particularly hair-raising story she asked, "Who killed him?"

Our Cornerville friend shook his head and said: "Lady! Lady! Around here you don't ask them things."

That incident did us no damage, for the man knew us well enough to take it all as a joke. Still, I was hesitant about mixing Harvard and Cornerville. I did not worry about what Cornerville would do to Harvard, but I did worry lest some Harvard friend would unintentionally make a blunder that would make things awkward for me or would act in such a way as to make the local people ill at ease. For that reason, I kept the two worlds separate, but that meant that Tony could not improve his social standing through associating with us.

When it became evident that I was at a dead end with Tony, I cast about for other avenues leading to a study of racketeering.

Two possibilities seemed open. Tony had an older brother who worked for him. I reasoned that, since the two men were brothers and worked so closely together, Henry would know almost as much about racket developments as Tony. I already had seen something of Henry, and I set about building the relationship further. This went along smoothly with visits back and forth from house to house as well as conversations in the back room of the store. (This indicates that Tony did not drop us out of suspicion, for in that case he could have seen to it that we did not take up with his brother.)

This led to a good deal of discussion of Tony's racket organization, which was exceedingly valuable to me. Still, I had an uneasy feeling that I was not getting what I needed. I was not yet ready to give up the possibility of getting close to Tony and of observing him in action. I understood that he was a member of the Cornerville Social and Athletic Club, which was located right across the street from our apartment. I joined the club then in order to renew my pursuit of Tony Cataldo.

At first I was disappointed in the fruits of my decision. While officially a member, Tony was rarely in the clubroom. In a few weeks it became evident that I was not going to cement relations with him in this area. What next? I considered dropping out of the club. Perhaps I would have done so if there had been other research openings then demanding my attention. Since I had planned to concentrate upon the role of the racketeer and had no other plans at the time, I rationalized that I should stay with the club. I did not record the reasons for my decision at the time. Perhaps I had a hunch that interesting things would break here. Or perhaps I was just lucky.

At least, I recognized that the club presented some new angles in research. It was far larger than any corner gang I had studied. Here was an opportunity to carry further the observational methods I had used on the Nortons.

When I wrote my first draft of this present statement, I described how I developed these new methods to a point where I had systematic knowledge of the structure of the club *before* the election crisis. In other words, when Tony entered and sought to

manipulate the club, I already had a full picture of the structure he was attempting to manipulate. I must now admit, following a review of my notes, that this is a retrospective falsification. What I first wrote was what I *should* have done. Actually, I began my systematic observations of the club several weeks before the election. When the crisis arrived, I had only an impressionistic picture of group structure. The notes I had then justified no systematic conclusions.

There were two factors that propelled me into more systematic efforts at charting the organizational structure. In the first place, when I began spending time in the club, I also began looking around for *the* leader. Naturally, I did not find *him*. If Tony was not around much, then somebody must take over in his absence. The club had a president, but he was just an indecisive nice guy who obviously did not amount to much. Of course, I did not find *the* leader because the club consisted of two factions with two leaders and—just to make matters more confusing for me—Carlo Tedesco, the leader of one faction, was not even a member of the club when I began my observations. Since I was completely confused in my crude efforts to map the structure, it followed that I must get at the data more systematically.

Then the political crisis underlined the necessity of pushing ahead with such observations. I had to learn more about the structure that Tony was seeking to manipulate.

Here I had a more complicated task than any I had faced before. The club had fifty members. Fortunately, only about thirty of them were frequent attenders, so that I could concentrate on that smaller number, but even that number presented a formidable problem.

I felt I would have to develop more formal and systematic procedures than I had used when I had been hanging on a street corner with a much smaller group of men. I began with positional mapmaking. Assuming that the men who associated together most closely socially would also be those who lined up together on the same side when decisions were to be made, I set about making a record of the groupings I observed each evening in the club. To some extent, I could do this from the front window of

our apartment. I simply adjusted the venetian blind so that I was hidden from view and so that I could look down and into the store-front club. Unfortunately, however, our flat was two flights up, and the angle of vision was such that I could not see past the middle of the clubroom. To get the full picture, I had to go across the street and be with the men.

When evening activities were going full blast, I looked around the room to see which people were talking together, playing cards together, or otherwise interacting. I counted the number of men in the room, so as to know how many I would have to account for. Since I was familiar with the main physical objects of the clubroom, it was not difficult to get a mental picture of the men in relation to tables, chairs, couches, radio, and so on. When individuals moved about or when there was some interaction between these groupings, I sought to retain that in mind. In the course of an evening, there might be a general reshuffling of positions, I was not able to remember every movement, but I tried to observe with which members the movements began. And when another spatial arrangement developed, I went through the same mental process as I had with the first.

I managed to make a few notes on trips to the men's room, but most of the mapping was done from memory after I had gone home. At first, I went home once or twice for mapmaking during the evening, but, with practice, I got so that I could retain at least two positional arrangements in memory and could do all of my notes at the end of the evening.

I found this an extremely rewarding method, which well compensated me for the boring routines of endless mapping. As I piled up these maps, it became evident just what the major social groupings were and what people fluctuated between the two factions of the club. As issues arose within the club, I could predict who would stand where.

In the course of my observations I recorded 106 groupings. Upon inspecting the data, I divided the club tentatively into the two factions I thought I was observing. Then, when I re-examined the data, I found that only 40, or 37.7 per cent, of the groupings observed contained members of both factions. I found further

that only 10 out of these 40 groupings contained two or more members of each faction. The other 30 were cases where a single individual of the other faction joined in the card game or conversation. I then divided the groupings into two columns, placing in one column those which were predominantly of one faction and in the other column those which were predominantly of the other faction. Then I underlined in red those names which did not "belong" in the column where I found them. Out of a total of 462 names, 75, or approximately 16 per cent, were underlined in red. Of course, we would not expect a pure separation of two cliques in any club, but the figures, crude as they were, seemed to demonstrate that the two factions were real entities which would be important in understanding any decisions made by the club.

This observation of groupings did not, in itself, point out the influential people in the club. For that purpose, I tried to pay particular attention to events in which an individual originated activity for one or more others—where a proposal, suggestion, or request was followed by a positive response. Over a period of six months, in my notes I tabulated every observed incident where A had originated activity for B. The result of this for pair events (events involving only two people) was entirely negative. While I might have the impression that, in the relationship between A and B, B was definitely the subordinate individual, the tabulation might show that B originated for A approximately as much as A for B. However, when I tabulated the set events (those involving three or more people), the hierarchical structure of the organization clearly emerged.

As this phase of the research proceeded, I saw more clearly how to relate together the large racket organization and the street corner gang or club. In fact, the study of the role of Tony Cataldo in this setting provided the necessary link, and the observational methods here described provided the data for the analysis of this linkage.

While I was working up these research methods, I committed a serious blunder. It happened during the political crisis. Tony had been trying to persuade the club to invite his candidate in to address us, although nearly all the members were disposed to

support Fiumara. At this crucial point, I participated actively, saying that, while we were all for Fiumara, I thought it was a good idea to hear what other politicians had to say. The vote was taken shortly after I spoke, and it went for Tony against Carlo. That led to the rally for Mike Kelly in our clubroom and to the most serious dissension within the club.

Here I violated a cardinal rule of participant observation. I sought actively to influence events. In a close and confused contest such as this, it is quite likely that my indorsement of Tony's position was a decisive factor. Why did I so intervene?

At the time, I was still hoping to re-establish close relations with Tony Cataldo, and I wanted to make some move that would build in that direction. So I sought to do the impossible: to take a stand which would not antagonize Carlo and his boys but would be appreciated by Tony. It was a foolish and misguided attempt. I did antagonize Carlo, and he forgave me only on the assumption that I was ignorant of the situation in which I was acting. Ignorance being preferable to treachery, I accepted this excuse.

Ironically enough, my effort to win favor with Tony was a complete failure. Before the political crisis, he had hardly known Carlo and had not recognized his leadership position in the club. When Carlo opposed him so vigorously and effectively, Tony immediately recognized Carlo's position and made every effort to establish closer relations with him. As I had taken a position on his side in the crisis, Tony needed to make no efforts to establish closer relations with me.

I did not have to speak in this situation at all. If I had spoken against Tony, it seems likely that this would have done more to re-establish our close relations than what I actually did.

As I thought over this event later, I came to the conclusion that my action had not only been unwise from a practical research standpoint; it had also been a violation of professional ethics. It is not fair to the people who accept the participant observer for him to seek to manipulate them to their possible disadvantage simply in order to seek to strengthen his social position in one area of participation. Furthermore, while the researcher may consciously and explicitly engage in influencing action with the full knowledge of the people with whom he is participating, it is certainly a highly questionable procedure for the researcher to establish his social position on the assumption that he is not seeking to lead anyone anywhere and then suddenly throw his weight to one side in a conflict situation.

12. MARCHING ON CITY HALL

I suppose no one goes to live in a slum district for three and a half years unless he is concerned about the problems facing the people there. In that case it is difficult to remain solely a passive observer. One time I gave in to the urge to do something. I tried to tell myself that I was simply testing out some of the things I had learned about the structure of corner gangs, but I knew really that this was not the main purpose.

In all my time in Cornerville I had heard again and again about how the district was forgotten by the politicians, how no improvements were ever made, how the politicians just tried to get themselves and their friends ahead. I heard a good deal about the sporadic garbage collections, but perhaps the bitterest complaint concerned the public bathhouse, where in the summer of 1939 as well as in several earlier summers there was no hot water available. In a district where only 12 per cent of the flats had bathtubs, this was a matter of serious moment.

People complained to each other about these matters, but apparently it did no good to try to work through the local politicians, who were primarily concerned about doing favors for friends and potential friends. If you could not go through the local politicians, why not go direct to the mayor—and on a mass basis? If, as I assumed, the corner gang leaders were able to mobilize their gangs for action in various directions, then it should be possible through working with a small number of individuals to organize a large demonstration.

I talked this over with Sam Franco, who was enthusiastic and ready to act at once. He promised me the support of his section of Cornerville. For the Norton Street area I called on Doc. For

the area around George Ravello's headquarters, I picked one of the local leaders. With my new acquaintances on Shelby Street, I was able to cover that end of the district.

Then began the complicated task of organizing the various groups, bringing them together, and getting them ready to march at the same time. And who was going to lead this demonstration? Since I was the connecting link among most of these corner gang leaders and since I had begun the organizing activity, I was the logical man to take over. But I was not then prepared to depart so far from my observer's role. I agreed with the others that I would serve on the organizing committee, but we would have to have a different chairman. I proposed Doc, and all the others agreed to this. But, as I talked with Doc, I found that, while he was happy to go along with us, he was not prepared to accept the leadership responsibility. I then proposed Mike Giovanni, and he too was acceptable to the small group with whom I was working in preparing the demonstration. Mike said that he would conduct a public meeting in Cornerville in getting people together for the march, but he thought that the chairman from that point on should be elected by the representatives of the different corners who were there assembled. We agreed on this.

But then we had a misunderstanding as to the composition of this public meeting. Sam Franco brought just several representatives from his end of the district, while a large part of the Shelby section marched en masse down to the meeting. Thus, when there were nominations for chairman, a man from Shelby Street who had previously taken no part in the planning was nominated and elected. Sam Franco's friends were considerably annoyed by this, for they felt they could have elected one of their candidates if they had simply brought their boys along. Sam and several of the other men also suspected our chairman's motives. They were convinced that he would try to turn the demonstration to his personal advantage, and I had to concede that there was a good possibility of this. From this point on, part of the efforts of our committee were to hem in the chairman so that he would have no opportunity to go off on his own tangent.

In this election meeting we had been misled by our own conception of democratic processes. It makes sense to elect a chairman only from a regularly constituted group or constituency. In this case the election had turned out quite fortuitously because of the overrepresentation of Shelby Street.

We next had difficulty with the date on which we were to march. It had been set about a week from the election meeting, but now the men on Shelby Street were telling me that their people were all steamed up and wanted to march much sooner. I consulted Sam Franco and one or two other members of the committee but was not able to get all the committee together. In spite of this, I told them that maybe we should move the march up a couple of days. We then scheduled a meeting of the full committee to take place the night before the march. When the committee began assembling, it became evident that some of them were annoyed that they had been bypassed, and I realized that I had made a serious blunder. Fortunately, at this point one of the local politicians came in and tried to argue against the march. This was a great morale booster. Instead of arguing with each other as to how we had been handling the plan, we got all our aggressions off against the politician.

The next morning we assembled in the playground in front of the bathhouse. We had had mimeographed handbills distributed through the neighborhood the day before; the newspapers had been notified. We had our committee ready to lead the march, and we had the playground pretty well filled. Some of the older generation were there lining the sides of the playground. I assumed they would be marching with us, but, significantly enough, they did not. We should have realized that, if we wanted to get the older generation, we had to work through their leadership too. As the march got under way, young boys from all over the district thronged in among us carrying their home-made banners. And so we set off for city hall right through the center of the business district. We had the satisfaction of stopping traffic all along the route, but it was not for long, since the parade moved very fast. We had made the mistake of having all our committee up in front, and it seemed that everybody behind us

was trying to get to the front, so that we leaders were almost stampeded. And some of the women pushing baby carriages were unable to keep up.

We had no opposition from the police, who were only concerned with an orderly demonstration as we assembled in the courtyard below the city hall. Then the ten committee members went up to see the mayor, while the rest of the marchers sang "God Bless America" and other songs to the accompaniment of an improvised band. We had known that the mayor was out of town, but our demonstration could not wait, so we talked to the acting mayor. He got our names and a list of our grievances, treating us seriously and respectfully. As our committee members began to speak, I heard Sam saying behind me in a low voice: "Get out of here, you cheap racketeer." I turned to see the local politician, Angelo Fiumara, elbowing his way in. Fiumara stood his ground and spoke up at the first opportunity: "I would like to add my voice to the protest as a private citizen. . . ." Sam interrupted, calling out: "He's got nuttin' to do with us. He's just trying to chisel in." Mike Giovanni reiterated Sam's remarks, and the acting mayor ruled that he would not hear Fiumara at that time. While the speaking was going on, I distributed a prepared statement to the reporters. At the end of our session the acting mayor promised that all our protests would be seriously considered and that any possible action would be taken.

We then marched to the bathhouse playground, where we told our followers what had taken place in the mayor's office. Here again, Angelo Fiumara tried to address the crowd, and we elbowed him out. The next day's newspapers carried big stories with pictures of our demonstration. We were given credit for having three hundred to fifteen hundred marchers with us in the various papers. The fellows happily accepted the figure of fifteen hundred, but I suspect three hundred was closer to the truth. The day after the demonstration, engineers were examining the boilers in the bathhouse, and in less than a week we had hot water. The street-cleaning and the garbage collections also seemed to be pepped up, for at least a short time. For all the mistakes we had made, it was evident that the demonstration had brought results.

But now the problem was: What next? We had got an organization together, and we had staged a demonstration. Somehow, we must keep Cornerville working together.

In this effort we were completely unsuccessful. Several committee meetings petered out without any agreements on concerted action. I think there were several difficulties here. In the first place, the committee members were not accustomed to meeting together or working together personally. There was nothing to bring them together except the formal business of the meeting. Their ties were on their various street corners. In the second place, we had started off with such a sensational performance that anything else would be anticlimax. It seemed hard to get up enthusiasm for any activity that would be dwarfed beside our protest march.

I came to realize that any over-all street corner organization would have to be built around some sort of continuing activity. The softball league developed the following spring and met this need to some extent. In fact, I worked with the same men in setting up the league, so in a sense the march on city hall did have continuing consequences, though they fell far short of our fond hopes.

13. FAREWELL TO CORNERVILLE

Through the spring and summer of 1940, most of my time was spent in writing the first draft of *Street Corner Society*. I already had the case studies of the Nortons and the Italian Community Club. I followed these with three manuscripts which I then called "Politics and the Social Structure," "The Racketeer in the Cornerville S. and A. Club," and "The Social Structure of Racketeering."

As I wrote, I showed the various parts to Doc and went over them with him in detail. His criticisms were invaluable in my revision. At times, when I was dealing with him and his gang, he would smile and say: "This will embarrass me, but this is the way it was, so go ahead with it."

When I left Cornerville in midsummer of 1940, the Cornerville S. and A. Club had a farewell beer party for me. We sang "God

Bless America" three times and the "Beer Barrel Polka" six times. I have moved around many times in my life, and yet I have never felt so much as though I were leaving home. The only thing that was missing was a farewell from the Nortons, and that was impossible, for the Nortons were no more by this time.

14. CORNERVILLE REVISITED

As I write this, more than forty years after leaving the district, there seems no longer any reason to maintain its fictional name, nor to maintain the pseudonyms of some of the principal characters. I was studying the North End of Boston, one of the most historic sections of this country, where tourists can still visit Paul Revere's home on North Street and the Old North Church on Salem Street. On the southern edge of the North End is Faneuil Hall, where leaders of the American Revolution sometimes met. A peninsula in the harbor, the North End is the scene of the Boston Tea Party.

The North End also figures importantly in political history in the nineteenth and early twentieth centuries. It was located in the Third Ward and was then dominated by the Hendricks Club in the West End, the area studied by Herbert Gans in *Urban Villagers*. There Lincoln Steffens's favorite ward boss, Martin Lomasney, held sway through the early decades of this century. When I began my study in 1937, Lomasney had passed on, and under the leadership of John I. Fitzgerald, the Irish-dominated club was losing its hold on district politics.

By 1980, the North End was in the process of transformation. It was still predominantly an Italian-American district, but gentrification had set in. Some decades earlier, the elevated tracks had been torn down, thus opening up the view of the waterfront. This stimulated the growth of fine restaurants and condominiums along the wharves. At the other end of the district, the extraordinarily attractive redevelopment of the Quincy Market gave added appeal to the North End, already within easy walking distance of the commercial, banking, and political center of Boston.

Physically, most of the district appeared unchanged. North

Bennett Street (Norton Street) looked in 1980 just as when I left it in 1940. The Capri Restaurant of the Orlandi family had long since disappeared, but the building in which I first lived, at 7 Paramenter Street, on the corner of Hanover Street, appeared unchanged. The building where Kathleen and I began our married life, half a block from the waterfront, at 477 Hanover Street, still stands, but across the street the Hanover Association (Cornerville S. and A. Club) has disappeared, and the building that once housed it has been rebuilt into a condominium.

What impact did the book have on the North End? I have no evidence of any major influence or even that it was widely read in the district. For more than ten years after publication, the book jacket to the first edition (designed by Kathleen Whyte) was on the bulletin board of the branch library under the heading Recent Books of Interest, but among the corner boys, Ralph Orlandella (Sam Franco) could not find anyone who had read it beyond those to whom I had sent copies.

The local social workers did, of course, read the book, but it had no dramatic effect upon their institutions. I heard indirectly that, with one exception, the workers in the North Bennett Street Industrial School (Norton Street House) were upset because they had befriended me and then I had turned against them and embarrassed them before other social workers and their elite supporters. I took some comfort from the one exception, the head of girls' work, who had introduced me to Ernest Pecci (Doc). I heard that she judged my study an accurate picture of the institution and the district. By the 1950s, the Industrial School at last had acquired, for work with young boys, a full-time staff member, one who had been born and raised in the North End but who had a college degree and some social-work education beyond that.

Reactions at the North End Union (Cornerville House) appeared to be ambivalent. Frank Havey (Mr. Kendall) told me in 1953 that he did not question the accuracy of the book, but he was uncertain as to what extent the settlement house could attract corner boys without losing its established clientele. He then told about getting a grant to hire a local World War II

hero who had organized a basketball league of forty-two teams and made the Union the liveliest place in its history—apparently without disruption of the regular settlement-house programs. Unfortunately, when the grant ran out, the local man was let go. By the early 1950s, the Union did have two Italian-American staff members, but both men were from outside of the district.

Havey confessed that he found himself in a bind between his recognition of the value of indigenous leadership and the standards being promoted by those evaluating social-work programs. Schools of social work have been striving to enhance the professional prestige of their graduates. How can social work be regarded as a profession if its institutions hire young men who got their basic training on the street corner?

Havey knew of no one who had been threatened with a cut off of funds if he hired anyone who did not have an M.S.W. degree. Still, he was often asked how many people on his staff had this degree, and he had been hearing references to other institutions which were not "measuring up." Upon inquiry, he would learn that these substandard institutions were those which persisted in hiring people without the advanced degree.

As I was preparing this third edition, I talked again with Frank Havey. By the time of his retirement in 1974, after forty years at the North End Union, he was regarded with admiration and affection throughout social-work circles in the Boston area. Recognition went beyond an impressive ceremonial dinner in his honor: a professor at Boston University began an oral history project about his four decades in the North End and conducted extensive interviews with him. Havey hopes one day to turn these reminiscenses into a book—which I shall read with great interest.

Havey reported that the problems of relating settlement house to street corner had not changed during the 1970s. He himself had made several efforts to include North End men with street-corner backgrounds in his staff. He recalled particularly two men who were doing good jobs for the Union, but after some months, they both quit. His explanation: they were caught in a bind between the standards of the settlement house and the

standards of the street corner. He added that there was no problem in hiring people for jobs that were not thought to require social-work education. But, of course, a man hired to run a basketball program or a woman hired to conduct sewing classes would be in a dead-end job with no prospects for career advancement.

In spite of the good reputation the Union enjoyed in social-work circles, for many years Havey was unable to persuade any of Boston's major social agencies to place students or staff members part time in the Union to provide counseling services that his agency could not offer. Such assignments were made only to agencies where the program would be supervised by someone holding a master's degree in social work. The Union got around the credentials barrier only when it was able to hire a full-time staff member with an M.S.W.

In the 1960s, throughout the country, storefront recreation centers and other programs depending upon indigenous leadership enjoyed growing popularity. Did I have anything to do with this? I doubt it. I assume that the change grew out of the growing militance of slum people, which forced an increasing recognition that the old paternalistic strategies were not working. At best, my book may have provided some academic legitimacy for this trend, and it may have stimulated some rethinking among planners, teachers, and students of social work. Nevertheless, the underlying problem will not be resolved simply by placing indigenous leaders in charge of "outreach" programs if those positions offer no possibilities of rewarding good performance with advancement and job security.

In recent decades, as the general level of education has risen, it has become increasingly difficult for the non–college graduate to rise into management in private industry, but it still does happen now and then—even fairly often in some fields. In general, a college degree is all that is required for eligibility for management positions, and in many firms, graduate degrees confer no advantage on the degree holder in competition for most management jobs. Can it be that the credentials barrier is now much tougher in social work than in private industry?

What happened after 1940 to some of the chief characters in the book? Joseph Langone (George Ravello) has long since passed on, but his funeral parlor remains in the family in the North End, and one of his sons was a representative in the commonwealth legislature in 1980.

It took Ernest Pecci (Doc) a long time to find a secure place on the economic ladder. He had no steady job until the war boom got well under way. Then at last he caught on and was doing very well until the postwar cutback came. People were then laid off according to their seniority, and Pecci was out of work once more.

He finally did get a job in an electronics plant. At the time of my last visit (December 1953), I found that he had worked his way up to a position as assistant supervisor in the production planning department of the factory. Such a department is a nerve center for the factory, for it handles the scheduling of the orders through every department of the plant.

Pecci had achieved some success in attaining his position, but he tended to minimize his accomplishments. He explained, "On the technical side, I stink. The only place I really shine is where I have to go around and talk the foreman into running a new order ahead of the one he was planning to run. I can do that without getting him upset." So Pecci was applying some of the social skill he displayed in the North End in this new factory world. However, he was working in an industry of very advanced technical development; so his lack of knowledge in this field set a ceiling upon his advancement.

Pecci got married shortly after he got his first steady job during World War II. His wife was an attractive North End girl, a very intelligent and able person who had opened a small clothing store of her own.

I had one visit with Pecci about five years after the book was published. His reaction seemed a combination of pride and embarrassment. I asked him for the reaction of the members of his own gang. He said that Frank Luongo (Mike Giovanni) had seemed to like the book. Gillo's (Danny's) only comment was "Jesus, you're really a hell of a guy. If I was a dame, I'd marry

you." The other members of the gang? So far as Pecci knew, they had never read it. The question had come up all right. One night on the corner, one of the fellows said to Pecci, "Say, I hear Bill Whyte's book is out. Maybe we should go up to the library and read it." Pecci steered them off. "No, you wouldn't be interested, just a lot of big words. That's for the professors."

On another occasion, Pecci was talking to the editor of the *Italian News*. The editor was thinking of publishing an article about the book. Pecci discouraged him, and no such notice appeared.

I assume that in his quiet way Pecci did everything he could to discourage local reading of the book for the possible embarrassment it might cause a number of individuals, including himself. For example, it could hardly be pleasant reading for the low-ranking members of the Bennetts to see it pointed out how low they ranked and what sort of difficulties they got into. Therefore, I have every sympathy with Pecci's efforts in limiting the circulation of the book.

Years later, I heard that Pecci had been promoted to head of production planning, but I heard nothing more of him until the 1960s, when I learned that he had died. I was sorry then that I had allowed myself to get out of touch with him, and yet there seemed to be a growing problem between us that led to an estrangement I still do not fully understand. I had tried to keep in touch through letters, but Pecci was even less conscientious as a letter writer than I. The last letter I received from him was a request that I henceforth not tell anyone who "Doc" was.

In the early years after the publication of the book, Pecci had accepted invitations to speak to classes at Harvard and Wellesley. I gather that he acquitted himself well on these occasions and was particularly effective with the Wellesley girls. Quite naturally, he got tired of this kind of thing, and I was glad to comply with his request.

On one of our visits in the Boston area, Kathleen and I had visited the Peccis in their suburban Medford home, and we seemed to get on well on that occasion, but when I was in

Boston several years later, we failed to get together. We talked over the phone about meeting, but Pecci gave the impression that he had a lot of other things to do and was not eager to see me.

Perhaps Pecci had come to feel that I had gained fame and fortune through *Street Corner Society*, and he, who had provided the principle keys to that society, had not received his fair share of benefits. While a fair share would be impossible to determine, Pecci did in fact make some material gain out of our association. He had got himself on the then popular television program "The $64,000 Question." He was not one of the big winners, but he did make off with a Cadillac. While he never told me what he had written in order to get on the show, and the announcer made no mention of *Street Corner Society*, I suspect that Pecci must have built up that aspect of his life, because a prospective contestant had to find some way to make himself appear unusually interesting in order to get on the show.

Or perhaps the problem between us was simply that by the time I had last called, Pecci had left the street corner so far behind that he no longer had any interest in connecting up with old times.

Frank Luongo moved on from the North End to become a labor union leader. It began with a job in a rapidly expanding war industry. Frank had no sooner been hired than he began looking around to organize a union. Shortly after this, he was fired. He took his case to the appropriate government agency, charging that he had been fired for union activities. The company was ordered to put Frank back to work. He wrote me that when he reappeared on the job, the situation seemed to change suddenly and dramatically. The other workers had thought they had seen the last of him. Now that he had shown what could be done, they began signing up. For some months Frank was at the plant gate for half an hour before the shift came on and half an hour after his shift went home, distributing pledge cards. And he personally signed up fifteen hundred members. When the union was recognized, Frank became its vice-president. He also wrote a weekly column in the union paper under the heading Mr. CIO.

The column was written in a colorful style and must have commanded a good deal of attention in the local.

At the next union election, Frank ran for president. He wrote me that his opponent was a man who had had very little to do with organizing the union, but he was a popular fellow—and he was an Irishman. Frank lost. Shortly after this time, the company began large-scale layoffs following the end of the war. Without a union office, Frank's seniority did not protect him, and he dropped out of his job.

We exchanged letters for several years after I left Boston, but then the correspondence lapsed. I lost track of Frank until many years later when a Cornell student dropped in to my office to tell me that he had met Frank in the course of some field work for a paper on union organizing. Frank then was an organizer for the Textile Workers Union and was working out of Stuyvesant, New York.

A year or so later, when I was planning to drive to Boston, I wrote to Frank to suggest that Kathleen and I stop off to meet him for lunch on the way back. He responded cordially, but when I telephoned him to make definite arrangements the morning of our planned meeting, I learned that he was in the hospital. We stopped at the hospital and visited with him and his wife for an hour or so. It was a gloomy occasion; Frank had advanced cancer and knew he would not have long to live.

We talked about old times, and then Frank filled me in on the intervening years, when he had been working regularly as a union organizer. Finally he told me that over those years, on a number of occasions, he had been approached by students or professors from universities for information about the union. He added, "I have had enough of that. I will never again do anything for anybody from college." I asked him why he felt that way.

"I have always taken time with them. I have gotten things out of the file for them and answered all their questions as well as I could. And I never asked anything in return except, I would say to them, 'When you get through, send me a copy of what you write, will you?' They would always say yes, they

would be glad to do it, but I never yet have got anything back. So to hell with them."

I was glad I had remembered to send Frank a copy of *Street Corner Society*. Social researchers have not lost anything in Frank Luongo's rejection of future cooperation, since a few weeks after our meeting he was dead. I quote his last words to me in the hope that future researchers will try a little harder to keep their promises to people in the field even after they no longer need them.

What has happened to Christopher Ianella (Chick Morelli)? I was particularly concerned with that question, and yet I hesitated to seek the answer. I debated the question with myself. I finally decided that Chris could be the one individual I had hurt. I must find out what the book had done to him. I telephoned Chris to ask if I could see him. At first he missed my name, but then he replied cordially. Still, I was wondering what would happen when we sat down to talk.

I found that he had moved out of the North End, but, paradoxically enough, he still lived in the same ward. Pecci, the old corner boy, had moved to the suburbs, and Chris, the man who was on his way up, had stayed in the center of the city.

Chris introduced me to his wife, an attractive and pleasant girl, who neither came from the North End nor was of Italian extraction. We sat in the livingroom of an apartment that, with its furniture, books, curtains, and so on, looked distinctly middle class. For a few minutes we skirted about the subject that we all knew we were going to discuss. Then I asked Chris to tell me frankly his reactions to my book.

Chris began by saying that there were just two main criticisms as far as he was concerned. In the first place, he said that he did not think I distinguished his own way of speaking sufficiently from that of the corner boys when I quoted him. "You made me talk too rough, just like a gangster."

I expressed surprise at this, and here his wife joined in with the comment that she thought that I had made Chris look like a snob. Chris agreed that he had got that picture too. His wife pulled the book down from the shelf and reread the passage

where I quote Pecci on the occasion of a political meeting during which Chris was on and off the stage seven times in order to take the tickets that he was going to sell for the candidate. They both laughed at this, and Chris commented that he would never do a thing like that anymore. She said that Chris had told her before they got married that he had once had a book written about him. But she added that he didn't give her the book to read until after they had been married.

Chris laughed at this, and then he went on to his second criticism. "Bill, everything you described about what we did is true all right, but you should have pointed out that we were just young then. That was a stage that we were going through. I've changed a lot since that time."

He expressed concern over the reactions of other people to my book. "You know, after the book was out a while, I ran into Pecci, and he was really upset about it. He said to me, 'Can you imagine that! After all I did for Bill Whyte, the things he put in the book about me. You know that thing about when I said you would step on the neck of your best friend just to get ahead. Well, now, maybe I said that, but I didn't really mean it. I was just sore at the time.' "

Chris seemed concerned about what the book had done to my relationship with Pecci. I did not tell him that Pecci had read every page of the original manuscript, nor did I give my interpretation that Pecci was simply going around repairing his fences after some of these intimate reactions had been exposed.

Chris assured me that he was not the hard character that the book seemed to make him. ("Really, I'm a soft touch.") And he gave me instances where he had helped out his friends at no advantage to himself.

As I was getting ready to leave, I asked Chris if he had anything more to say about the book.

"Well, I wonder if you couldn't have been more constructive, Bill. You think publishing something like this really does any good?"

I asked what he meant. He mentioned my pointing out (as he had told me himself) that he had difficulty with his *th* sound. I

had also discussed the commotion the fellows sometimes caused in the theaters, the fact that they sometimes went to dances without ties, and so on—all points that make the North End look like a rather uncouth district. (I am unable to locate any references in the book to commotions in the theaters or men at dances without ties.)

"The trouble is, Bill, you caught the people with their hair down. It's a true picture, yes; but people feel it's a little too personal."

As he walked with me to the subway station, we got to talking about his political career. I had been astounded to hear that he had missed being elected to the city council by a scant three votes. The Chris Ianella whom I had known never could have come so close. Without expressing my surprise, I tried to get him to talk about this.

"You know, the funny thing is, Bill, I didn't get many votes from the North End. The people that you grow up with, it seems, are jealous of anybody that is getting ahead. Where I got my support was right around here where I live now. I know these fellows on the street corner, and I really fit with them."

As if to demonstrate for me, he nodded and waved cordially at several corner groups as we walked by. In a later visit I learned that Chris Ianella had at last been elected to office, and in 1980 he was president of the Boston City Council.

Chris left me with a good deal to think over. In the first place, it is hard to describe the sense of relief I felt after seeing him. Although it must have hurt him at first to read the book, he had been able to take it in stride, and he was now even able to laugh at himself as he had been in that earlier period. As I discussed these things later with Pecci, I began to wonder whether the book might even have helped Chris. It was Pecci who presented this theory. He argued that not many people have an opportunity to see themselves as other people see them. Perhaps the reading of the book enabled him to change his behavior. Certainly, Pecci argued, Chris had changed a good deal. He was still working hard to get ahead, but he seemed no longer the self-centered, insensitive person of earlier years. Chris certainly had to change

in order to have any hopes of getting ahead in Democratic politics—and somehow, for reasons that I cannot now explain, Chris had decided that his future lay with the Democrats rather than with the Republicans, in whose direction he had seemed to be moving when I left the North End. So, at least, the book had not hurt Chris, and it seemed just possible that it had helped him.

I was also pleased to find that basically Chris accepted the book. This, of course, pleased me as a writer, but it also spoke well of Chris. I suspect that the man who can accept such a portrait of himself is also the man who can change the behavior described.

Chris's objections to the book seemed interesting. As to the way I had quoted him in the book, I felt on very firm ground. He did talk differently from the corner boys, but not quite as differently as he had imagined. If a quotation from him contains an ungrammatical expression or some typical corner-boy phrase, I am reasonably sure that that part of it is authentic. I was so sensitive to the differences between Chris and the corner boys that I would have been unlikely to imagine any expressions that made them appear more alike. The criticism seemed to say more about Chris's status and aspirations than it did about my research methods.

Perhaps, indeed, I should have pointed out that Chris and his friends were young and were just going through a stage of development. But youth, in itself, does not seem a full explanation. These men were not adolescents; they were at least in their mid-twenties. The important fact is that they had not yet secured any firm foothold in society. They were young men who had left home but who had not yet arrived anywhere. I am inclined to believe that this is an important factor in explaining the aggressiveness, the self-centeredness, and so on, that appear in Chris and some of his friends during that period. Later on, when Chris had found something of a place for himself, he could relax and be more concerned with other people. Is this just a phenomenon of social mobility out of the slums and into middle-class status? As I think back upon my own career, I can recall

with a trace of embarrassment some of the things that I said and did in the early stages of my career, when I was struggling to gain a foothold on the academic ladder. It is easy to be modest and unassuming once you have achieved a fairly secure position and won a certain amount of recognition.

I had no quarrel with Chris's point that I had caught people with their hair down, and I could sympathize with the people who felt that way. If you are going to be interviewed for the newspaper, you put on your good suit and your best tie, make sure that the kitchen dishes are cleaned up, and in general take all the steps you associate with making a public appearance. You appear before the public in the role that you would like to play before the public. You cannot do this with a social researcher who comes in and lives with you. I do not see any way of getting around this difficulty. I suppose there must always be aspects of our reports that will give a certain amount of embarrassment to the people we have been studying. At least I was reassured to find that the reaction in Chris's case had not been nearly so serious as I had feared.

While we can only speculate about the impact of the book upon Pecci and Chris and many others, there is one man upon whom it has had a profound effect—and I was not always sure that the effect would be constructive. Working with me made Ralph Orlandella, a high school drop out, want to do social research. In this case, I can let Ralph tell his own story (see Appendix B).

15. GETTING *Street Corner Society* ACCEPTED AS A DOCTORAL THESIS

Although I was drifting away from my earlier ambition to be a writer of fiction, I was determined to write *Street Corner Society* so that it would be read beyond the academic world. I first submitted the manuscript to Reynal and Hitchcock, a commercial publisher which had announced a competition for nonfiction manuscripts based on scholarly research. That was a near miss. I came in second to a book on philosophy.

With the encouragement of W. Lloyd Warner and Everett C. Hughes, I then submitted the book to the University of Chicago Press. Shortly thereafter, the editor agreed to publish the book. But then I got a letter from the business manager, who told me that I would have to cut the manuscript by one-third and put up a $1300 subsidy, since the book was not going to sell many copies. For a couple living for two years on fellowships of $600 and then $1500 (minus $300 tuition for each year), the $1300 was a formidable challenge, but we managed to get the money together, largely from what we had saved from the North End period. Since I had already done substantial cutting, I was at first more concerned by the further condensation task. As I look back on it, I believe the discipline was good for me. I can no longer remember anything I cut out, and *Street Corner Society* emerged as a better book in the process.

During the same period, I had the task of getting *Street Corner Society* accepted as a doctoral thesis. I had arrived in Chicago to begin graduate work with the first draft of my thesis in my trunk. I had done some rewriting and polishing, but I had not changed my analysis in any way I can now recall. That unorthodox beginning required some unorthodox maneuvers at the end of my graduate career. I took my field examinations one week and underwent my thesis examination the following week—though, according to the rules, the Ph.D. could not be awarded less than nine months after passage of the field examination, which explains why my Ph.D. was dated 1943 rather than 1942.

As is often the case, there were deep cleavages in the sociology department, so any student facing his thesis examination had to hope that with the covert assistance and encouragement of the faction with which he was allied, he would be able to withstand the attacks of the opposing faction. I was further handicapped because when I was coming up for the ordeal, my chairman, W. Lloyd Warner, was on leave, and I had to hope that Everett Hughes and Bill Whyte together could get me through.

At the time, Chicago had a requirement that all Ph.D. theses had to be printed, and I was determined that I was going to

publish a readable book and a doctoral thesis at one and the same time. For this reason, I had refused to start the book with the traditional review of the literature on slum districts or to conclude with a chapter in which I summarized my contribution to that literature, including the final obligatory sentence, "more research on this topic is needed." My reasons for this stance were not entirely literary. Fortunately for me, during the period when I was doing my field work, I was unacquainted with the sociological literature on slum districts, as I had begun my study thinking of myself as a social anthropologist. During the two years at Chicago, I immersed myself in that sociological literature, and I became convinced that most of it was worthless and misleading. It seemed to me that it would detract from the task at hand if I were required to clear away the garbage before getting into my story.

As I expected, the sharpest attack came from Louis Wirth, who was himself the author of one of the better slum studies. He began by asking me to define a slum district. The purpose of his question was obvious. Whereas I had been arguing that the North End was actually highly organized, with many cohesive groupings, he didn't see how I could define a slum district without bringing in the concept of "social disorganization," which had been the central theme of previous slum studies.

I replied that a slum district was simply an urban area where there was a high concentration of low-income people living in dilapidated housing and under poor sanitary and health conditions. Wirth objected that this was not a sociological definition, but I refused to satisfy his conceptual appetite, replying simply that the conditions I stated were what had led me into the study of the North End and that I considered it an empirical problem to determine how people lived under those conditions.

Although not satisfied, Wirth was finally persuaded that he wasn't going to get the answer he desired, and he moved on to attack my effrontery in passing over without mention several generations of sociological literature. This provoked a lively interchange in which I attempted to demonstrate that I really did know that literature.

At this point, Everett Hughes intervened to lead us to a compromise. The department would accept the book as my thesis providing I wrote separately a review of the literature which made clear what I was adding to that literature. This supplementary material could then be printed (at my expense) and bound with the book in one copy, which, deposited in the university library, would make Bill Whyte's thesis fit into the traditions of the graduate school.

It later occurred to me that if I had to write a review of the literature, I might as well get some published articles out of this task (and indeed I did, as indicated in the references listed in Appendix C). When two of these articles were accepted for publication, I consulted again with Hughes. He persuaded the department to accept the published articles as my literature review and to abandon the formalistic requirement that these articles be bound with the book in the library copy.

Thus, the thesis defense ordeal had a happy ending, thanks largely to Hughes. I got the book published without the handicap of what I considered irrelevant material, and in addition, I launched myself upon an academic career with two articles as well as the book.

16. REVISITING *Street Corner Society* AFTER FIFTY YEARS

The initial reception of the book gave no indication that it might one day be considered "a sociological classic." The official journal of the American Sociological Society, the *American Sociological Review,* did not get around to reviewing the book. In the *American Journal of Sociology,* Edwin Sutherland, a distinguished criminologist, did give me a favorable review, but it was one which tended to place the book as just another good slum study.

At first, the book got a better reception outside of the academic world. Harry Hanson, a nationally syndicated columnist, devoted a full column to the book, ending with this statement: "Whyte offers fresh material on the ever important subject of American community life, presenting it eloquently from the human angle."

I was particularly pleased with the enthusiastic comments of Saul Alinsky, author of *Reveille for Radicals,* in the social-work periodical *Survey.* While acknowledging his prejudice against sociologists in general, he found *Street Corner Society* to be an impressively realistic analysis of the kind of slum districts in which he had been working as a community organizer.

At first, sales seemed to confirm the pessimistic prediction of the business manager of the press. The book was published in December 1943. By 1945 sales had declined to a trickle, and the book seemed about to fall into the remainder market.

My mid-1946 royalty check, reporting a tripling of sales over the previous year, came as a happy surprise. What had happened? In the first place, World War II veterans were flocking back into colleges and graduate schools, and their GI benefits provided generous allowances for the purchase of books. At the same time, many teachers of sociology were becoming dissatisfied with simply assigning text books for their courses and were requiring students to read research monographs.

Nevertheless, by the early 1950s, sales were in a steady decline, and once again the book appeared about to expire. Alex Morin, an editor at the press, told me that he had recently reread *Street Corner Society* in the hope of getting ideas for revisions that might justify a new edition and keep the book alive. This prompted me to think of writing this appendix on my field experiences, which first appeared in the enlarged 1955 edition and has subsequently been expanded.

It seemed as if the academic world had imposed a conspiracy of silence regarding the personal experiences of field workers. In most cases, the authors who had given any attention to their research methods had provided fragmentary information or had written what appeared to be a statement of the methods the field worker would have used if he had known what he was going to come out with when he had entered the field. It was impossible to find realistic accounts that revealed the errors and confusions and the personal involvements that a field worker must experience.

I decided to do my bit to fill this gap. In undertaking this

task, it seemed to me important to be as honest about myself as I could possibly be. This meant not suppressing incidents that made me look foolish, like my abortive attempt to pick up a girl in a Scollay Square tavern, or my involvement in a federal crime (I voted four times in an election)—although in the latter case, several colleagues advised me against such a confession. I wrote as I did, not simply to cleanse my soul but, more importantly, to help future field workers understand that it is possible to make foolish errors and serious mistakes and still produce a valuable study.

The enlarged 1955 edition gave the book a new lease on life. In the 1960s, sales were again tapering off, but the publication of a paperback edition boosted them to a new high.

Even though sales were again dropping off in the late 1970s, I had no thought of a possible new edition of the book until the two-day celebration of my retirement, organized by my department in Cornell's New York State School of Industrial and Labor Relations. The events centered around presentations and discussions by seven former research assistants or associates in field projects: Angelo Ralph Orlandella, Margaret Chandler, Melvin Kohn, Chris Argyris, Leonard Sayles, George Strauss, and Joseph Blasi. While I shall always treasure the contributions of these old friends, it was particularly the remarks of Ralph Orlandella that set me to thinking of my research in a new perspective.

Although I had given Orlandella no formal instruction in interviewing and observation, and I had certainly done nothing to increase his skill as a street-corner gang leader, he claimed that working with me had taught him methods of interviewing and observation and analysis of group structure that had served to win him leadership in his subsequent career. Previously I had thought of methods I was using primarily in terms of their utility for field research in the behavioral sciences. In his military career, Ralph demonstrated an extraordinary ability to gain the confidence of superior officers and subordinates and of people in other units and get them working together on innovative projects. He was also able to develop cooperative

projects between military bases and surrounding communities, as when he took the leadership in establishing the first in the world "Ballistic Missile Explorer Squadron, Boy Scouts of America." Later he put those same skills to use in managing a city public works department. In Appendix B, *The Whyte Impact on an Underdog,* which is based on the talk he gave to an enthusiastic audience at my retirement ceremony, Ralph tells his own story.

A half century, more or less, after its publication in 1943, *Street Corner Society* has suddenly become a new focus of scholarly attention. Published in the fall of 1991, *Reframing Organizational Culture* (Frost et al., eds.) devotes a major part to "Exploring an Exemplar of Organizational Culture Research." That section begins with a long excerpt from my 1955 appendix to *Street Corner Society* (hereafter referred to as *SCS*), follows with four critiques of the book by behavioral scientists (Michael Owen Jones, Alan Bryman, Patricia Riley, and John M. Jermier), and concludes with my "Comments for the *SCS* Critics." The April 1992 issue of the *Journal of Contemporary Ethnography* is entirely devoted to *SCS*. After an editorial introduction, the issue continues with the paper by W. A. Marianne Boelen mentioned in the Preface. Boelen charges me with ethical transgressions involving my relations with "Cornerville" and with Doc, my principal guide to the district. She states that I did not recognize that the custom of young men hanging on street corners was brought over from Italy. She claims that the common language among corner gangs was Italian, and that my comprehension of that language was deficient so that I did not fully understand what was going on.

As stated in the Preface, the Boelen attack is followed by my rejoinder and an essay by Angelo Ralph Orlandella. The issue closes with the three papers by behavioral scientists Arthur J. Vidich, Laurel Richardson, and Norman K. Denzin.

With the essays of the seven behavioral scientists in *Reframing Organizational Culture* and the *Journal of Contemporary Ethnography,* I did not have to defend my character or the reputation of the book, because they all accepted *SCS* as "a sociological classic," or words to that effect. They did, however, raise questions that throw an interesting light upon the way standards of criticism have changed over the past half century, particularly with the recent popularity of critical epistemology.

In what follows, I will be dealing primarily with *Reframing Organizational Culture,* referred to as *ROC,* and the pertinent issue of the *Journal of Contemporary Ethnography,* referred to as *JCE*.

The Intellectual Background of SCS

Jermier believes that "*SCS* is marked by the powerful influence of Chicago School epistemology" (*ROC,* p. 227). Boelen makes the same point, with a negative twist: that I distorted my interpretations in order to bring them in line with the Chicago School of Sociology.

As I pointed out in the 1981 edition of *SCS* (which none of the seven behavioral science critics had read), I had completed a first draft of *SCS* before I entered graduate work at the University of Chicago, and subsequent condensing and rewriting did not change in any way my analysis of the North End data. While I was in the field, 1936 to 1940, I thought of myself as a student of social anthropology. I had read widely in that field, under the guidance of Conrad M. Arensberg. At that time, I had not read any of the University of Chicago urban studies. In the thesis examination on *SCS,* I had to resist the efforts of Louis Wirth and Herbert Blumer to put *SCS* into the social disorganizational framework then popular in Chicago and elsewhere.

On the Researcher's Relations to Those Studied

Boelen (*JCE,* pp. 33–34) asks of me, "Did he commit an ethical cardinal sin by not taking his manuscript back to the field and checking the data and contents with the subjects?"

This "ethical cardinal sin" is a creation of Boelen. At the time of my study, I had never heard of such an obligation. Today some sociologists and social anthropologists are advocating

some kind of feedback to the field, but still I know of no code of professional ethics in sociology or anthropology that makes such a requirement. Assuming that I had tried to carry out Boelen's principle, how would I have gone about it? How does one feed back the data and contents of the researcher's study with a community of twenty thousand people—or even with the parts of the community I studied?

Before I left, Doc read the manuscript I took to Chicago, and we had long conversations over his suggestions and criticism. I also had innumerable feedback discussions with Sam Franco.

Should I have fed back my findings on the social ranking and leadership pattern to the Nortons as a group? When I once asked them who their leader was, they stated they were all equal. To reveal to them that behaviorally they were not equal would have embarrassed Doc and upset his followers.

Note that Boelen deals with field relations only in terms of the researcher's presumed obligations to those studied. She does not consider the right of the researcher to publish conclusions and interpretations as he or she sees them. How to balance our obligations to those we study against authors' rights to publish our findings is a complex question that cannot be dealt with simply in terms of Boelen's "cardinal sin." In *Learning from the Field* (1984), I have discussed some aspects of that question.

Did I exploit Doc? Boelen reports that his sons think I did, that I should have shared *SCS* royalties with him. I acknowledge that I gained more from our relationship than he did, but at the time I tried to reciprocate as best I could (*JCE*, p. 61).

Assuming that Doc himself thought I owed him something further, Richardson (*JCE*, p. 116) offers this hypothesis: "Whyte saw Doc as co-researcher, whose interpretations were intermingled with Whyte's. Ultimately, however, Whyte single-authored SCS; Whyte received the fame and "fortune" associated with the book. The fortune probably seemed immense to Doc, who was habitually underemployed."

The problem with this hypothesis is that it puts Doc on "hold" and me on "fast forward." In 1943, when *SCS* was published, my savings had been exhausted by the *SCS* subsidy,

and I earned nothing for the year in which I was recovering from polio. The first edition did not bring in any royalties until 1944, and from then on brought in little more than the amount of our subsidy. It was not until after the 1955 edition was published that *SCS* began to bring in substantial royalties. And the last time I saw him in 1953, Doc still greeted me as a friend.

In my Cornerville period, Doc was indeed "habitually underemployed," but the war boom beginning in 1942 got him a job where he was doing well until the postwar cutback came and he was laid off. Somewhat later, he got a job with a major electronics company. There he moved up into management. On my last visit with him (December 1953), he was assistant supervisor of production planning. When he died in 1967, he was manager of production planning, a key middle management position.

Beyond my personal experience, what general conclusions can we draw about relations between the researcher and his or her key informants? Should they be paid? If so, how much would be fair? And how should fairness be determined? It seems to me impossible to lay down any universal rule for such questions. For the researcher to promise money for informant interviews seems to me to inject a mutually calculating element into a relationship which works best when both parties agree voluntarily to collaborate. In some cases, it may be impossible to avoid payment commitments, but such commitments could substantially increase the costs of research, so as to rule out some otherwise mutually desirable projects.

Should a contingency payment be promised—a share in book royalties? That seems highly unrealistic for sociological or anthropological monographs. It is only rarely that such a monograph gains substantial sales—and, in my case, thirteen years after the book's initial publication.

I guided my involvement with Doc in terms of the principle of interpersonal reciprocity. When we were working together, I tried to be helpful to him, and Doc seemed satisfied with the relationship. He may later have come to the conclusion that I exploited him, as his sons now believe.

Following the principle of interpersonal reciprocity provides

no guarantee that the relationship will seem equitable to a key informant years later—or to his sons.

If interpersonal reciprocity provides no guarantee of maintaining good relations between researchers and principal informants and collaborators, can we find another basis for building that relationship?

Along with some of my colleagues, I have become convinced that *participatory action research* (PAR) provides one important means of bridging the gap between professional researchers and members of the organization we study. PAR is a methodology in which researchers invite some members of the organization studied to participate with them in all phases of the process, from research design through data gathering and analysis, and on to the practical application of research findings. Participatory action research is still unfamiliar to most behavioral scientists, but it has been practiced (usually under other labels) since at least the 1960s (Whyte 1989; Whyte ed. 1990; Whyte, Greenwood, and Lazes 1989; Whyte 1991; Harkavy and Puckett 1991; Greenwood, Whyte, and Harkavy 1993).

In terms of the issues discussed here, PAR has a dual advantage. In field relations, it enables us to go beyond interpersonal reciprocity in linking key informants and professional social researchers. As members of the community or organization studied become committed to the hoped-for action outcomes of the research process, they become less concerned with what they are getting personally for what they are doing *with* the researcher. This also can relieve the researchers of the uncertainty and anxiety over whether we have done enough for them personally in return for what they have done for us.

PAR also helps us to deal with one of the concerns of critical epistemologists: opening up ways in which at least some members of the organization studied add their own insider voices to those of the outsiders. This can enrich the data gathering and analysis process and also increase the level of acceptance of the research report within the community or organization studied.

The potentialities and limitations of PAR are currently being explored. It would not have been possible for me in the late 1930s because I was striving to follow the then current Harvard norm emphasizing a commitment to "pure science," without any involvement in social action. Also I did not have a secure position in an organization that would have given me a chance to organize a PAR project. Implementation of the PAR strategy is most effective when the social researcher is a member of a continuing organization capable of developing a long-term relationship. The lone researcher is in a poor position to provide the necessary follow-through.

The PAR strategy can only be applied effectively in a limited number of situations. Where PAR is possible, it offers opportunities to improve researcher field relations, to strengthen the research process, and to achieve practical results.

On the Postfoundational Critique

When I agreed to answer the Boelen attack, I assumed that the three behavioral scientists in that issue of *JCE* would conclude that my three and a half years of field work, backed up by voluminous notes typed shortly after the events or interviews, would be a more accurate guide to the realities of Cornerville in the late 1930s than the recollections of selected informants thirty to forty-five years later. None of the three takes a position on that issue. Vidich (*JCE*, p. 80) simply states that "readers may draw their own conclusions about the issues raised in these essays," but then he goes on with tributes to the continuing value of *SCS* for social theory and practice in urban slum areas. Richardson and Denzin don't deal with the issue because for them the nature of the critical game has changed since I did the study. Richardson (*JCE*, pp. 103–4) states that she writes about *SCS* "now in a radically different context from that in which it was produced. Some refer to the present intellectual context as 'postfoundational.' The core of this postfoundational climate is *doubt* that any discourse has a privileged place, any text an authoritative 'corner' on the truth."

Denzin (*JCE*, p. 130) calls me a "positivist-social realist" and goes on to state that (p. 126) "today, social realism is under attack. It is now seen as but one narrative strategy for telling stories about the world out there."

Riley (*ROC*, p. 218) argues along the same lines. Interpreting

the argument of Clifford Geertz, she writes, "cultural descriptions, filtered through the ethnographer, are actually second or third order fictions. . . . There is no culture or organization 'out there' to be accurately represented by observers."

In *Works and Lives* (Geertz 1988), Geertz discusses the problems faced by students of culture, as indicated in his subtitle: *The Anthropologist as Author.* He sees social anthropologists confronting an intellectual crisis (p. 71): "They are also harassed by grave inner uncertainties, amounting almost to a sort of epistemological hypochondria, concerning how one can know that anything one says about other forms of life is as a matter of fact so."

After examining the works of some of the most eminent social anthropologists (Lévi-Strauss, Evans-Pritchard, Malinowski, and Benedict), he abandons any hope of establishing scientific conclusions, and speaks rather of "rendering your account credible through rendering your person so" p. 79). He adds, "Ethnography takes, obliquely in the 1920's and 1930's, more and more openly today, a rather introspective turn. To be a customary 'I-witness,' one must, so it seems, first become a convincing 'I'."

Ethnological writing thus comes to depend on persuasion of the reader. But, Geertz says further (p. 133), "Who is now to be persuaded? Africanists or Africans? Americanists or American Indians? Japanologists or Japanese? And of what: Factual accuracy? Theoretical sweep? Imaginative grasp? Moral depth? It is easy enough to answer, 'All of the above.' It is not quite so easy to produce a text that thus responds."

On the Postfoundational Framework and Social Science

When I began my *SCS* research, I wanted to contribute to building a science of society—and I have not yet given up that commitment. I based my own framework on a basic distinction between the *objective* (what is out there to be observed) and the *subjective* (how the researcher or others interpret the observed phenomena). I assumed that I should concentrate on the objective, trying insofar as possible to base my interpretations on

what I observed and what I was told by informants whom I had found to be perceptive and accurate observers.

Reflecting on "postfoundational" ethnology, I have come to recognize that the objective-subjective distinction is not as clear as I once thought. For example, consider my study of the social structure of street-corner gangs. That was based primarily upon direct observation, but researchers cannot observe everything, and if we tried, we would end up with miscellaneous data, which would yield no intelligible pattern. We seek to observe behavior that is significant to our research purposes. Selection therefore depends upon some implicit or explicit theory—a process which is in large part subjective. But the choice is not random. If we specify our theoretical assumptions and the research methods we use, others can utilize the same assumptions and methods to either verify or challenge our conclusions.

Following the theoretical framework first proposed by Eliot D. Chapple and Conrad M. Arensberg (1940), I concentrated attention on observing and roughly quantifying frequencies and duration of *interactions* among members of street-corner gangs and upon observing the initiation of *changes in group activities*. (Such an approach had not been used before by sociologists and is still uncommon among sociologists and social anthropologists today.)

For the determination of informal group leadership, I relied on the critical distinction between *pair events* (interactions between two people) and *set events* (interactions among three or more people). In observing pair events, I found I could not always determine who was the more influential. In observing set events, the pattern became clear.

In the case of the Nortons, I determined that Doc was the leader through the following types of observations. Before he arrived at his corner, I would see small groups of two or three conversing. When Doc arrived, the small groups would dissolve and a larger group would form around him. When another member spoke to the group but then noticed that Doc was not listening, he would stop and then try again to get Doc's

attention. Doc often, but not always, was the one to suggest a change in group activity. When another member made a proposal for action not endorsed by Doc, no activity change followed. Only if Doc made or approved the proposal did I observe a change in group activity. The observational methods I used in the late 1930s for determining informal group structures can be checked today by any researcher who wishes to observe an informal group over an extended period of time.

Regarding the theoretical significance of such structural observations, I reject Riley's (*ROC*, p. 219) statement that my conclusions on the sociology of bowling and the relationship between changes in the interaction pattern and mental health "have proved more heuristic than others but they should be viewed as a particular conversation, bound in time and space by the rules governing their production."

That statement takes me back to the methodological arguments I encountered in graduate work at the University of Chicago in the early 1940s. In that era, the great debate was between the case study and statistics. Proponents of the case study argued that it yielded "understanding," whereas proponents of statistics claimed that was the only way to science. We students liked to promote a debate between Herbert Blumer (case study) and Samuel Stouffer (statistics), and the same debate was once projected on the national scene between Blumer and George Lundberg. That one became so heated that, at the end, they shook hands so as to give the misleading impression that no hard feelings were involved.

I enjoyed those debates and yet I was unhappy with the way the issues were framed. On the statistics side, the implicit assumption was that we were dealing with social surveys, then as now the main tool for sociologists using quantitative methods. Since the 1950s, I have used surveys in various studies, but in the 1940s I had no use for surveys; I wanted to quantify observations of behavior.

Contrary to the Riley statement, I argue that the case study can discover uniformities that can be checked further in other case studies or experimentally and/or quantitatively. Furthermore, it can yield insights that lead to theoretical advances by the author or by others.

Working with Muzafer Sherif, O. J. Harvey (1953) carried out an experiment on boys' groups to check the relation between their social rankings and sports performance. His findings, paralleling mine, hardly confirm that relationship for every case or circumstance, but at least they demonstrate the possibility of checking case-study findings experimentally.

The relationship between marked changes in interaction patterns and mental health can be checked in clinical practice to determine whether this framework could be useful in psychotherapy.

Anthropologist Scudder Mekeel (1943) discovered a close parallel between my thesis on "The Social Role of the Settlement House" (Whyte 1941) and the relations between American Indians and officials of the Bureau of Indian Affairs. He found that he only had to change "corner boys" to "Indians" and "settlement-house workers" to "B.I.A. officials" and everything else fitted his case as well as mine. A few years after my North End study, Herbert Gans (1962) found the same pattern of relations between settlement-house workers and corner boys in the neighboring West End.

In theoretical development, I have built my own conceptual framework over the years on some of my North End observations (1991), and George Homans (1950) used my analysis of the Norton Street gang in developing his own framework.

In Conclusion

When I began my *SCS* project, I took it for granted that I should aim to contribute to scientific knowledge. Today many behavioral scientists seem to believe that this is an impossible objective. But then I ask myself why so many distinguished scholars take such a defeatist attitude.

I think it is because they are focusing on types of problems that cannot have scientific answers. For example, Geertz and the anthropologists whose works he is studying are concerned with studies of the culture of a tribe or community.

Culture has many definitions. In its most inclusive definition, it encompasses kinship and other organizational structures, commonly held myths, beliefs, and attitudes, widely shared practices, rituals, and ceremonials, common patterns of interaction and activities, ways of making a living, tools and technologies used, and so forth. The anthropologist assumes that these elements are not randomly distributed and tries to discover some pattern in their relations.

To make sense out of any presumed pattern of relations among so many different elements requires the researcher to go far beyond simple reporting and description. Success at this task calls for imagination and creativity—highly subjective mental processes. The resulting publication may or may not be persuasive to particular readers, but there is no way in which it can be put to a scientific test.

That does not mean that social anthropological interpretations of a given culture are useless. A good cultural study can provide useful guidance for understanding and communicating with members of that culture. That is not the same as scientific proof—but humans would take very few actions if they only responded to scientifically tested propositions.

If researchers are seeking generalizations that can be put to scientific tests, then we must focus on certain elements within culture that can be directly or indirectly observed and measured. That is what I did in studies of street-corner gangs. I cannot claim to have made any comprehensive interpretation of the total culture of Cornerville. Women's roles and family life and the role of the church are hardly dealt with. In effect, I abandoned the goal of a comprehensive study to focus on areas in which I had substantial systematic data: corner gangs and their relations to the rackets and to political organizations. The methods I used and the conclusions I have drawn can be built on and improved upon by students of community organization today.

While I reject the standards of critical epistemology, I grant that they may have served a useful purpose in the postcolonial era by inviting outsiders to question our own assumptions of a

given culture and to seek the views of members of that culture. But that purpose is not served by another outsider, years later, going into that culture to get selected informants to tell their stories. We may agree that no outsider can really know a given culture fully, but then we must ask whether any insider can fully know his or her culture. In emphasizing the advantages of insider knowledge, let us not forget that an outsider can make important contributions—as Alexis de Toqueville did decades ago in his landmark studies of America.

The contrast to my own views is most sharply posed by Jermier and Denzin. Jermier (*ROC*, p. 233) calls me a positivist and states that "critical epistemologists insist that truth lies in ever deeper levels of subjective reflection and disclosure and that science serves most when it serves least." Following this view, we are left with an argument over whether my "truth" is better than your "truth."

Denzin begins his essay by recognizing *SCS* as a "sociological classic," but then ends his critique on this negative note (*JCE*, p. 131): "As the 20th century is now in its last decade, it is appropriate to ask if we any longer want this kind of social science. Do we want the kind of classic sociology that Whyte produced and Boelen, in her own negative way, endorses?"

What is the alternative approach proposed by the critical epistemologists? If, as Denzin states, what he calls "social realism" is now seen as "but one narrative strategy for telling stories about the world out there," then the critic can only depend on a judgment of the persuasive power of the author. Scientific arguments are thus transformed into literary criticism. We are then left with standards of judgment which shift with the changing trends of literary criticism.

For the future development of the behavioral sciences, the Denzin position brings us to a dead end. I believe that critical epistemology will come to be seen as a passing fad and that behavioral scientists who have succumbed to that lure will return to the pursuit of scientific knowledge.

* *

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