Introduction

THIS BOOK is a research collaboration between the academic world and Chicano ex-convicts and gang members. No other minority group in America is more alienated and more suspicious than the Chicano homeboys of East Los Angeles.

The research process that produced the book required a complex chain of interactions—and for the Chicano men, a hope that more knowledge about their lives and barrios will, eventually, help the Mexican American communities of Los Angeles to a better life. Without some background on this fruitful and interesting research relationship, neither the purposes nor the findings of the study are fully intelligible.

Most of the research was done in only one year of intensive work during the funding of 1974 and 1975. But the actual collaboration began several years earlier and continued long after funding was cut off. Some important features of both the academic and the ex-convict subcultures made this long interaction possible.

First we will consider the ways in which research has been defined by Chicano convicts and ex-convicts since the early 1960s. Then we will discuss the funded project that produced the systemic data on which this book is largely based. Finally, we will discuss the Chicago field work tradition that is one ultimate source of this kind of approach. More complete details and a complete natural history of the Chicano Pinto Research Project are given in Appendix A.

Research and the Pinto

Chicanos who serve sentences in California prisons tend to come from territorially-based youth gang backgrounds, and to have been imprisoned for offenses that involve narcotics. They are essentially the subject of this book; they are the homeboys. They have developed a subculture with, among other features, a clearly defined slang. (This slang, in turn, is a variety of Spanish that goes back to the early 1940s, to the days of

the Los Angeles pachucho; cf. Barker, 1947.) For this book, the three critical slang terms are: pinto, meaning convict or ex-convict (one who has served time in *la pinta*, or the penitentiary); barrio, meaning equally the neighborhood and gang (always a neighborhood with a name); and tecato, meaning addict. Thus, to rephrase the first sentence of this paragraph: most pintos tend to be barrio boys, and many pintos are also tecatos.

Official Research

The pinto-tecato-barrio subculture is suspicious of research, because both Chicano addicts and Chicano convicts have been subjected to many experiences that are defined as "research" and that would horrify any academic social scientist. Medical "research" in particular is often grossly exploitative and is described by Jessica Mitford quite accurately in her book (1974) about certain experiments done with convict "volunteers." (The chapter title is "Cheaper Than Chimpanzees," a quotation from a medical researcher.) In addition, the convicts see legitimate research consistently misused in order to label behavior for purposes of control.

Many such labels are derived from psychological research. The consequences are clearly evident to prisoners from the battery of psychological tests given every California prisoner shortly after sentencing. The results are used to determine an appropriate prison with an appropriate custodial rating (cf. Irwin, 1970). Prisoners classified homosexual or violent are examples of potential problems to the custodial staff. Program diagnosis is much less important.

Every prisoner has experienced these questionnaire-like psychological instruments. Most know how they are utilized, and have acquired a strong distrust for batteries of questionnaires. They equally distrust qualitative approaches because of their similarity to the use of informers by law enforcement people. Many convictions (especially for narcotics offenses) are the result of testimony given either by undercover law enforcement agents or by rats or snitches—that is, criminals who provide evidence in return for some degree of immunity. Unfortunately, police "informers" and scholarly "informants" are not far apart in the perspective of the pinto. He has experienced negative consequences from both.

The adaptations of individual pinto-tecato-barrio men toward official research are reasonably clear. Their overt compliance with researchers conceals a covert noncompliance that goes far beyond simple acquiescence. "Just check any answer." "Tell them something good." "Tell them you don't know." "Look dumb." There is no reason for any convict—and especially for any Chicano convict—to believe assurances of confidentiality. (Chicano communities also have their own strong hos-

tility to conventional research.) But there are many good reasons for them to con the researcher into believing that he/she is getting what he/she wants.[1] This is not to cast doubt on the validity of the existing research findings based on data from institutionalized men anc women. It simply indicates how strcngly the pinto-tecato-barrio subculture resists official research. The normal array of techniques available to the academic researcher (indigenous interviewers, participant observation, use of informants) are defined as extraneous to the research enterprise. They simply don't address the subculture of distrust, disdain and contempt that has developed over the years.

The Background for Collaboration

The main problem with official research is, of course, the well-founded suspicions about its purpose. Beginning in the mid-1960s, however, convicts in the California prisons began to develop their own research materials. Inmates at San Quentin compiled a *Convict Report* that discussed the indeterminate sentence and the convicts disillusionment with treatment-oriented reforms. Portions of this document were written by John Irwin, then incarcerated at San Quentin. Irwin later completed graduate training in sociology and included portions of the *Convict Report* in his book *The Felon* (1970).

This effort at San Quentin directly inspired an organization of Chicano ex-prisoners active in the prison self-help movement to use research to change narcotics laws. (California narcotics laws are severe and then included penalties for addiction itself.) It was hoped this change could be made through signatures on an initiative submitted to voters on the California ballot. LUCHA (a Chicano pinto self-help group based in Los Angeles) spearheaded the effort. The result was the two-hundred-page *Peoples Resolution* (1970), a substantial and well-developed statement of life in and after prison for Chicano drug offenders. It included two small-scale surveys. The first was taken among inmates at Folsom prison and recorded the preincarceration experiences of Chicano convicts with the justice system. The other was a "snowball" sample of predominantly Chicano ex-addict parolees in Los Angeles. It concerned their perceptions of the conditions of parole, and the extent to which parole was helpful (see Appendix B). It was at this point that the senior author of this book became involved, giving technical assistance on the latter survey and generally assisting the LUCHA group.

The response to the *Resolution* was so good, particularly in Chicano communities, that the notion that laws could be changed by means of Chicano pinto research was firmly implanted. A number of convicts active in the prison self-help movement became interested in a wide range of research. Some, done by convicts working in administrative offices with

access to case records, attempted to overcome stereotypes of "dumb" Chicanos (see MASH survey, Appendix B). Most notably, it proved that Chicanos served more time in both federal and state prisons than did Anglo or black convicts. Another noteworthy example studied the basic economics of the prison system, and established a strong case both for possible corruption in the Inmate Welfare Fund and for some degree of inmate control of its expenditure. The charges were so well documented that the Department of Corrections was compelled to answer some probing questions from Ernesto Duran, a Chicano ex-convict, in a 1974 legislative hearing. The primary lesson in the barrios was that research could have positive consequences if it accurately documented and analyzed the operations of the prison system and the potential of the Chicano pintos.

This experience of more than ten years is the background for the research undertaken by the Chicano Pinto Project in 1974 and 1975, and represented in this book. The lesson was long and hard; it is unlikely that these working relationships could have been established any other way. Through the prison self-help groups, the pinto movement had demonstrated that research, normally an instrument to manipulate convicts, could be used to change as well as to sustain a system.

The Chicano Pinto Research Project

From the first days of the funded project, it was clearly understood by all participants that the pinto viewpoint would be a consistent, significant, and often determining influence over the project. Half of the professional staff were ex-convicts. The project focussed on the post-prison adaptations of Chicano addicts and convicts. But within that focus there were many alternative ways of producing data that meant something to the research participants themselves, the barrio men and women. And there were many alternative ways of exploring policy issues—a sincere discussion of what might work and what might help.

The process was not smooth, easy, or simple. Yet a year of funded research demonstrated to the participants in the project that the collaborative pattern worked. The sources of strain, and the strengths of the collaboration, the phases in the project, and the actual effects of the collaboration are detailed more fully in Appendix A.

The principal investigator and senior author was active with pinto affairs since the 1970 publication of the *Peoples Resolution*. She chaired the first board of a Model Cities agency based on the principles of LUCHA and directed the agency during the summer of 1973, after the leadership collapsed and the project lost its funding. She subsequently worked with pintos on several projects to develop the proposals that were funded for the Pinto Project discussed above. The years of involvement with pinto groups and other pariah associations in the Los Angeles

area made it possible for her to build an unusual and close working relationship with the research participants.

The three barrios that are the focus of the study were selected because of their immediate relevance to ongoing community projects. However, they form an interesting sample of the hard-core barrios of Mexican East Los Angeles. Men from each of the gangs in these three neighborhoods were part of the project staff. Robert Garcia of White Fence and Carlos Garcia of Hoyo Maravilla contributed substantially to the project and to the book; both men are pintos. Robert Garcia became the president of the Chicano Pinto Research Project, Inc., after the funded project was over and the group decided that the experience was worth extending. Largely through the efforts of Robert Garcia, the project survived without financial support for three years. In 1978, further funding came from the Department of Labor and the National Institute on Drug Abuse. This was the first government support of any organization directed by Chicano ex-convicts.

Materials on the third barrio, San Fernando, were written by a pinto from the Alpine barrio, Luis Cerda. Roberto Castro of San Fernando's Proyecto del Barrio, a pinto and former LUCHA member, was instrumental in assisting the Pinto Project. Frank Valencia, a pinto from the Happy Valley barrio, was responsible for the basic research on the heroin and barbiturates market.

The donation of time and effort by pintos, both inside and outside prison, was one of the most significant features of the project. Organized Chicano groups in the state and federal prisons cooperated both in criticizing working documents and in administering questionnaires.[2] Other pintos organized groups of street acquaintances into formal seminars to criticize drafts of the manuscript as it emerged. Enrique Guillen was particularly helpful in these efforts. Of course, the prisoners and the ex-prisoners got something out of this participation. There were enough diverse views among the pintos that no faction could monopolize either the direction of the study or any interpretations. In fact, there were continuing disagreements and controversies, both within the project staff and among pintos outside the staff. But the participating men, both in and out of the staff, gained a perspective, a sense that personal and subgroup preoccupations can be conceptualized in broader terms, and a confirmation of the notion that subjects can participate in research. Most important for our colleagues, there is some remote hope that this activity might lead to policy changes that would help Chicano convicts.

Academia and the Community Field Study Tradition

Just as the pintos drew on the values of a subculture which could accept research as a neutral activity (rather than as the special privilege of an institution), so also the academic interests drew on a methodologi-

cal subculture that could accept the subjects as participants in research. In addition, this subculture had been reinforced by the years of protest against the dominant scientific mode of social science research. Research traditions in social science go far beyond the impersonal questionnaire, although it may not seem that way to many pintos.

In fact, the collaborative methodology developed in the Chicano Pinto Research Project represents a substantial degree of continuity with the community field work tradition, especially as that tradition appeared in the Chicago school. This is the tradition on which a remarkable group of Chicano barrio men have built something quite new and useful—but it is only fair that the roots in Chicago be recognized.

The field work tradition of the University of Chicago, which began in the 1920s, virtually mandated that major consideration be given to the point of view of the communities and the people under study. Some of the sociologists in the Chicago school did this with what came to be called (with careful objectivity) "personal documents." Thomas and Znaniecki's five-volume study of the Polish peasant in Europe and America (1918) used letters, life histories, and the like as major elements. Works in this tradition also included detailed life histories of deviants, most notably *The Professional Thief* (Conwell and Sutherland, 1937); Clifford Shaw's *The Jackroller* (1930); and Helen Hughes' edited version of a woman heroin addict's personal story (Clark and Hughes, 1961). Survey tradition was combined with field work in Thrasher's work on 1,313 Chicago gangs (1927). The field work tradition of Robert Ezra Park continued in a direct line of descent with Everett C. Hughes and his students in sociology and Robert Redfield and his students in anthropology. At Chicago, the field work in these two disciplines remained very close through the early 1960s. W. Lloyd Warner joined the faculties of both departments and shared with the sociology undergraduates his experiences and attachments with the Murngin tribe of Australia as well as his work in Yankee City. There were also the real world concerns that motivated the social gospelist Albion Small in his interaction with Jane Addams (of Hull House) early in the century. Everett Hughes, Allison Davis, Robert Havighurst, W. F. Whyte, and Burleigh Gardner had an interdisciplinary program on industrial relations. Men like Horace Cayton and St. Clair Drake were motivated to do their massive study of the Black Belt in Chicago (1962). Both men were black; Cayton spent some years administering a community center on Chicago's South Side. As a result, *Black Metropolis* was a remarkable joining of the traditions of Chicago field work. It was dedicated to Robert E. Park; Warner and Allison Davis were closely associated with the study and Hughes wrote the introduction to the 1962 edition. There was also a tradition of cross-racial collaboration

at Chicago: the white anthropologist Burleigh Gardner and his wife developed a cross-racial view of Mississippi life with black anthropologist Allison Davis in the book *Deep South*. Both men became associated with Warner. In more recent years, Whyte (1943) and Gans (1962) continued the Chicago tradition with studies of the Italian community in Boston. Unlike Gans, Whyte did not train at Chicago, although he found strong intellectual support there for his work.

For a number of reasons, the Chicago tradition began to fade after World War II. For one thing, the number of institutions granting doctorates in sociology had proliferated by the late 1950s. The tradition of field work developed at Chicago (with its pragmatic intellectual base) lost ground because it took much time and highly individualized training to produce a researcher. When performed in mediocre fashion, field work easily slips into poor journalism. Discipleship is virtually impossible, and the Chicago tradition had no dogma.

A serious crisis soon appeared in the acceptability of social research, directly traceable to the exclusion of minority subjects from the research process. (The Chicago tradition usually assumed some form of subject participation, even if it wasn't emphasized.) The apparent cause was the movement of the equality revolution of the 1960s into academic research. But the real cause was a great deal of "objective," dogmatic, and not very good research. The most publicized crisis occurred over the *Moynihan Report*, a compendium of what was, by then, the generally accepted social science wisdom about the black family (cf. Rainwater and Yancey, 1967). The *Report* became closely tied to the issue of minority representation in academia, and led to a long controversy in the academic journals about the relative importance of insiders and outsiders in research about minorities (Merton, 1972; Moore, 1973; Bernard, 1973). The response to this criticism of academic research did not immediately revive the Chicago tradition of community research, for there was no longer a base at Chicago. Community research was revived in the old Chicago field sites (e.g., Suttles, 1968; Kornblum, 1974), but with somewhat different interests. In addition, many of the liveliest contemporary social scientists trained in the older Chicago tradition have tended to move either into more theoretical issues (a good example is the work of Erving Goffman), or into more specialized matters (such as the study of medical settings). Community research was still alive and well, but its best exemplars in minorities studies (e.g., Liebow, 1967; Hannerz, 1969; Blackwell, 1975; Lewis, 1955) were seldom associated with an institutional setting that could provide a base for a revival and updating of any community research tradition. Also, the Chicago tradition (despite strong praise from some minority scholars, e.g., Blackwell and Janowitz, 1974) was coming under attack from some of the revi-

sionist sociologists of the left (e.g., Schwendinger and Schwendinger, 1974). Most importantly, unless community participants are actively involved in both the research and its uses, as we have done in this study, both the research and its ultimate uses tend to be highly suspect. While this can be termed politicization, the alternative is not very pleasant either. Unless the community is involved, so-called objective research will almost inevitably be politicized beyond the researcher's control.

The work reported here reflects a merging and amplification of a new thread of field research that stems from the struggle of neglected minorities and the Chicago tradition of field work in minority communities (the principal author was a student of both W. Lloyd Warner and Everett C. Hughes). In this respect, the project represents strong continuity with both the barrio tradition and an academic tradition. It might also be considered an early step in policy and action research that deinstitutionalizes the academic and, with luck, reintegrates the dispossessed. It is our hope that this study will help show the way to yet more collaboration between academics and minorities.