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Understanding and Controlling Crime

Toward a New
Research Strategy



Springer-Verlag

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A Report Commissioned by the Justice Program Study Group of the
John D. and Catherine T. MacArthur Foundation



Springer-Verlag

New York Berlin Heidelberg London Paris Tokyo

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Library of Congress Cataloging in Publication Data
Farrington, David P.

Understanding and controlling crime.

(Research in criminology)

Bibliography: p.

Includes index.

1. Crime prevention—United States. 2. Crime and criminals—United States. 3. Crime and criminals—United States—Research. I. Ohlin, Lloyd E.
II. Wilson, James Q. III. Title. IV. Series.
HV7431.F37 1986 364.4'0973 86-11823

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Typeset by Publishers Service, Bozeman, Montana.

Printed and bound by R.R. Donnelley & Sons, Harrisonburg, Virginia.

Printed in the United States of America.

9 8 7 6 5 4 3 2 1

ISBN 0-387-96298-0 Springer-Verlag New York Berlin Heidelberg

ISBN 3-540-96298-0 Springer-Verlag Berlin Heidelberg New York

Preface

In 1982 the John D. and Catherine T. MacArthur Foundation created a small committee—the Justice Program Study Group (whose membership is listed at the end of this preface)—and posed to it what can hardly be regarded as an easy question: “What ideas, what concepts, what basic intellectual frameworks are lacking” to understand and to more effectively deal with crime in our society?

Those who are acquainted with the work of the members of the Study Group will appreciate how many divergent views were expressed—divergent to the degree that some of us came to the conclusion that we were not a Study Group at all but rather a group being studied, an odd collection of ancient experimental animals serving some dark purpose of the Foundation. Eventually, however, a surprisingly strong concurrence emerged. We found we were impressed by the extent to which in our discussions we placed heavy reliance on the products of two types of research: first, those few longitudinal studies related to juvenile delinquency and crime that had been pursued in this country and, second, a few experimental studies that had sought to measure the consequences of different official interventions in criminal careers.

These two research strategies had taught us much about crime and its control. Other strategies—case studies, cross-sectional surveys, participant observations, and similar techniques—had indeed been productive, but it was the longitudinal and experimental designs that firmed up the knowledge that the others helped to discover.

We came to the firm conclusion that criminal justice research in the United States has reached a stage at which an increasing investment in more ambitious longitudinal studies is essential. But that was hardly an answer to the question that had been put to us. We had to go further and address the strategy of such studies, and how they should be related to other means of learning about crime and criminals and shaping criminal policy. We needed to confront the considerable obstacles to launching a new research strategy as well as assess what we had already learned, and what we needed to know.

In order to explore these problems, the Justice Program Study Group consulted with 21 scholars in various disciplines, including individuals experienced in longitudinal research. They were asked to prepare brief concept papers on the

potential value of and the best design for the next generation of longitudinal studies. Thirteen such papers were prepared.

Together with three consultants—Professor Joseph Adelson of the University of Michigan, Dr. David P. Farrington of the University of Cambridge, and Professor Richard J. Herrnstein of Harvard University, to whom we wish to express our gratitude—the Study Group discussed these 13 papers and then requested the authors of three of them to prepare more fully developed versions for presentation at a conference. These extended statements were prepared by:

1. Delbert S. Elliott, David Huizinga, and Franklyn W. Dunford of the Behavioral Research Institute, University of Colorado
2. Monroe M. Lefkowitz, Long Island University
3. Marvin E. Wolfgang, Deborah J. Denno, Robert M. Figlio, Paul E. Tracy, and Neil A. Weiner of the Center for Studies in Criminology and Criminal Law, University of Pennsylvania

With these three papers providing the raw material for some of our discussions, a conference was then convened in Chicago in June, 1983, to assist the Study Group in its task. More than 50 scholars, researchers, and representatives of government agencies, foundations, and the media attended. Copies of these three papers, which are a rich source of ideas on longitudinal research, may be obtained from the National Criminal Justice Reference Service, 1600 Research Blvd., Rockville, MD 20850.

Two days of vigorous discussion at this conference produced no broad agreement on what the next step in longitudinal studies related to criminality should be, but there was general agreement on the urgent need for such studies. It was probably the fault of the Study Group in its planning for this conference, but there was too much debate about the details of research design and too little attention to the methodological and political realities of laying more secure foundations for such studies and attracting financial support for them. At all events, the conference and the papers prepared in anticipation of the conference provided material that the authors of this book have found invaluable. Indeed, the emphasis during the conference on the great value of combining experiments with longitudinal studies is reflected throughout this book. What we now need is a new research strategy to be launched by a series of relatively small cohort studies of high-risk groups with a variety of experimental treatment modalities attached thereto.

The amplification of that central idea, its explanation and justification, is the purpose of this book. As stated above, that idea no doubt sounds elusive and grossly affected to any reader unacquainted with research in this field, but in the main it is a simple theme: Let us follow the careers of groups of children, youths, and young adults over time, particularly the careers of those who are likely to be involved in crime, in order to try to distinguish those who do become involved from those who do not, to discover which of them persist and which desist, and to learn which treatments seem to work and with whom. This is a well-known

strategy in medical research; it is time for its importance to be appreciated and pursued with new vigor in criminological research.

This book makes the case for such research and spells out many of the details by which it could best be pursued. Chapter 1 provides an overview of what we now know about crime and criminals and how we might know more so as to better shape criminal justice policy to our needs. In Chapters 2 and 3, we review in some detail what we have learned from longitudinal and experimental studies in criminology thus far. In Chapters 4 through 7, we explore selected key policy issues to illustrate current deficiencies in the knowledge needed to develop more cost-effective action strategies. In these chapters we consider, respectively, families and schools as targets of prevention measures, theories about the effects of labeling, proposals for reorganizing the juvenile courts, and the effects of imprisonment. Finally, in Chapter 8, we discuss possible research designs and strategies we might now pursue to build firmer ground for our criminal justice policies.

The work of the Study Group was greatly assisted by David R. Ashenhurst, Assistant Director, General Grants Program of the John D. and Catherine T. MacArthur Foundation; it also received encouragement and advice from James M. Furman, Executive Vice-President of the Foundation, and from Edward H. Levi, then a member of its Board of Directors.

Norval Morris

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1

The Case for a New Crime Research Strategy: An Overview

Policymakers who wish to put in place new programs to reduce crime, or to expand the scope or effectiveness of programs already in place, will quickly discover that the knowledge necessary to do this responsibly does not exist except in fragmentary and unsatisfactory form. Whether we wish to prevent delinquency or rehabilitate offenders, whether we seek to strengthen families or improve schools, whether we believe that juvenile courts should get tougher or provide better services, we will be forced to admit, if we are honest, that we only have scattered clues and glimmers of hope (and sometimes not even that) on which to base our actions.

This knowledge gap is the largest single impediment to strengthening our society's capacity to cope more effectively with crime. This is the central factual conclusion of the Justice Program Study Group, and it is on the basis of this conclusion that we have chosen to recommend a new research strategy that combines the virtues of longitudinal and experimental methods. In this chapter, we provide an overview of our conclusions and outline a new research strategy. In the chapters that follow, we take up in detail the gaps in our knowledge about crime prevention and the methods that can be used to fill them.

There is nothing new in saying that we do not know enough to mount a well-conceived set of new programs, and there is something a bit lame in calling for more research. In the early 1960s, when crime rates in the United States began their dramatic increase, we knew even less about how to cope with the problem than we do today. Many people were comfortably optimistic about the efficacy of rehabilitation programs; it took a decade or more of research and writing for the realization to sink in that this optimism was misplaced. Others were certain that hiring more police officers and having them engage more frequently in random preventive patrol would cut down on street crime. Again, a decade passed before this certainty was shattered by studies suggesting that feasible changes in levels of preventive patrol would have few or no demonstrable effects on crime rates. Still others believed that the causes of crime could easily be addressed by programs that provided job training, more schooling, and reduced racial segregation. Job-training and job-creation programs flourished; the proportion of young

persons staying in school increased; the more obvious forms of racial segregation were overcome. Billions of dollars were spent. Crime continued to rise.

We do not conclude from the history of the last two decades that efforts at crime prevention and criminal rehabilitation are wrong or always doomed to failure, that the police can do nothing about crime, or that efforts to attack the causes of crime are a waste of time. We do conclude that broad-bush, inadequately designed, poorly tested programs are not likely to make much of a difference. We do believe that we have learned a great deal about what does not work, or does not work as easily as we once thought. And we think we have identified those methods of research and experimentation that are best suited for shedding new light on the development of individual differences in criminality and on the strategic opportunities for intervention in that process of development.

When we call for more research, then, it is not because we have learned nothing in the past. On the contrary, as we shall suggest, the best and most useful programs in effect today are based squarely on the best past research. Nor is the call for more research indicative of any desire on our part to postpone action or to underplay the gravity of the problem of crime in contemporary America. Rather, we suggest that the time is ripe for taking a new set of measured steps toward the prevention or reduction of crime and that these new steps can build on efforts already underway. But these steps require, if we are to avoid wasted resources and dashed hopes, a careful specification of the precise points and methods of intervention.

What Do We Know?

We know a great deal about who commits crimes. We know that the typical high-rate offender is a young male who began his aggressive or larcenous activities at an early age, well before the typical boy gets into serious trouble. We know that he comes from a troubled, discordant, low-income family in which one or both parents are likely to have criminal records themselves. We know that the boy has had trouble in school—he created problems for his teachers and does not do well in his studies. On leaving school, often by dropping out, he works at regular jobs only intermittently. Most employers regard him as a poor risk. He experiments with a variety of drugs—alcohol, marijuana, speed, heroin—and becomes a frequent user of whatever drug is most readily available, often switching back and forth among different ones. By the time he is in his late teens, he has had many contacts with the police, but these contacts usually follow no distinctive pattern because the boy has not specialized in any particular kind of crime. He steals cars and purses, burgles homes and robs stores, fights easily when provoked, and may attack viciously even when not provoked. While young, he commits many of his crimes in the company of other young men, though whether this is because they have influenced him to do so or he has simply sought out the company of like-minded friends is not clear. After several arrests, the young man, now in his early twenties, will probably spend a substantial amount of time in jail or prison. The

chances are good that not long after he is released from an institution, he will commit more crimes. He runs a high risk of having his life cut short by violent means—the victim of a murder or a fatal car accident.

With these facts in mind, it is not hard to specify a plausible crime control strategy:

Identify these high-risk youngsters at an early age and provide services, counseling, and assistance to their families.

Help them become better students.

Provide assistance and training in finding jobs.

Improve the quality of life in their neighborhoods.

Reduce the availability of dangerous drugs, and provide treatment programs for those persons who abuse such drugs as are available.

But if they commit a serious crime:

Arrest and prosecute them promptly.

Send them to a correctional program that is suited to their temperament and personal history.

While in that program, help them maintain contact with those decent friends and family members whom the offenders cherish.

On release from that program, help them find a job and give them financial and other forms of assistance so that they can try to make a go of it back in society.

If they return to crime after their release, send them to an even more secure correctional institution for an even longer time.

Not only is such a strategy plausible, but many elements of it are being practiced almost everywhere and all elements of it are practiced in some places. But what is plausible might not always be feasible, and what is feasible might not always be valuable. How can families be assisted? Which forms of assistance make a difference and which are wasted efforts? How do you help a rebellious, unmotivated, low-achieving student? What can be done to place a young man with a poor school record and disorderly habits in a worthwhile job? Indeed, how in many inner-city neighborhoods do you find a job for even a competent, well-motivated young man? Which aspects of neighborhood life are worth improving and how do you do it? Does it make a difference who the boy's friends are and, if it does, how can you change those associations? If the youngster likes to drink or take drugs, how do you talk him out of it, especially if all his friends are doing the same thing? If he is arrested early in his criminal career, will this deter him from future crimes or so stigmatize him that he is driven to seek out criminal friends and criminal opportunities? If he is sentenced to some correctional program, which one will be best suited for him and how do we know it will work?

On the basis of past research, scholars and practitioners have been able to describe the typical criminal career with some accuracy. The path-breaking studies of criminal careers by, among others, William and Joan McCord (1959), Sheldon and Eleanor Glueck (1950, 1968), Donald West (1982), Donald West and David Farrington (1973, 1977), and Marvin Wolfgang and colleagues (1972)

have taught us to be especially concerned with the small proportion of boys who become very-high-rate offenders. On the basis of this teaching, police officers have formed special units to detect and apprehend high-rate offenders, prosecutors have reorganized their staffs so as to expedite the investigation and prosecution of high-rate and dangerous offenders, judges have been supplied with methods to help them distinguish between low-rate and high-rate offenders, and parole boards have devised prediction scales to help them discriminate between low-rate and high-rate offenders when the boards are deciding whom to release on parole. We believe these research-based changes in criminal justice practice have been cautious, though none is free from criticism. In this and other ways, research has helped society improve its response to crime.

But a great deal remains to be done. For example, if we wish to improve the policy of selectively seeking out high-rate offenders for arrest, prosecution, and incarceration, we need to know much more about who such persons are and how they can be identified. Even then, of course, there will remain some ethical and legal questions about the fairness of a selective policy. But more important, the research that has been so helpful in focusing attention on the high-rate offender has taught us very little, if anything, about how we might prevent a boy from becoming a high-rate offender in the first place. If we could reduce the probability that any given boy who commits one or two offenses will go on to commit 10 or 12, we will have spared society countless victimizations. To do this, we need to learn more about how to prevent the onset of a serious delinquent career and how best to handle the would-be offender in the school system and the active offender in the criminal justice system. We think these things can be learned—not enough, perhaps, to make dramatic reductions in crime, but enough to improve significantly our present policies. At present, we cannot recommend ways to make these improvements because we are at the limits of what can reasonably be inferred from available evidence.

What Do We Need to Know?

Let us review what we know about the correlates of criminality in order to assess which of these are in fact causes of crime and, of course, which might be changed by planned interventions.

THE FAMILY

Almost no one denies that high-rate offenders are likely to come from homes that are cold, discordant, and inconsistent with respect to management of discipline. There is also evidence that such offenders are more likely to come from large, low-income families than from small, higher-income ones. Moreover, some people believe that single-parent homes and abusive parents produce a disproportionate number of offenders, but the evidence on that is not clear.

Let us assume that what has happened in a family does in fact cause, and is not simply an accidental correlate of, delinquency. (As we shall see in a moment, that assumption is not always warranted.) There are at least three different hypotheses that could explain how this causal connection might occur. These include:

1. Economic adversity causes family stress that in turn causes parental discord and (possibly) child abuse. Discord prevents the effective socialization of the child, and abuse teaches him that violence is an approved or useful way of getting what he wants.
2. The parents' temperament (poorly controlled hostility, little regard for the feelings of others, excessive drinking) leads to discord between mother and father, poor or inconsistent child-rearing practices, and unsuccessful employment experiences. The child is inadequately socialized; the marriage founders.
3. The child's temperament (alternately passive and fussy, possibly hyperkinetic, hard to manage) leads to stress in the family, discord between the parents, and the frantic resort to inappropriate child-rearing methods. The child becomes delinquent and the parents seem unable to prevent it. They blame each other for this failure, which intensifies their quarrels and increases the risk of the marriage dissolving.

Obviously, several of these causal patterns may operate simultaneously. But let us assume, for purposes of discussion, that one pattern explains the child's becoming predisposed toward delinquency. Which pattern it is will determine what policies we might wish to endorse, and these policies are likely to differ greatly according to that pattern. Note also that studies that measure the correlation between family circumstances and delinquency cannot uncover the causal pattern; all such studies will simply reaffirm that there is some connection among family adversity, parental discord, weak child-rearing practices, individual temperaments, and early delinquency.

For example, if the first hypothesis is correct, then policies designed to relieve the economic burden on poor families would become important. We might wish to propose the adoption of more comprehensive and effective job-training and job-placement programs or some form of a guaranteed annual income (or negative income tax) or major changes in the amount of and eligibility for welfare and other public assistance programs. This would have to be done carefully. If the benefits are too great, work incentives would be weakened and mothers might find it profitable to remain unmarried and unemployed. But if the benefits are too small, economic adversity would not be reduced sufficiently to prevent acute family stress. Nonetheless, we know where to start.

If the second hypothesis is correct, changing the economic circumstances of the family would have little or no effect on how the child was reared. In this case, economic adversity is not the cause of family stress (though it may contribute to it). Rather, it is the temperament of the mother or father (or perhaps both) that

leads to impulsiveness, constant bickering, unpredictable eruptions of hostility or moodiness, and drug or alcohol abuse, and it is these behaviors that lead to both failure in the job market and failure as parents. Psychologists believe that temperament is to some degree inherited; estimates of heritability ordinarily range around 0.25 or 0.30 (Cattell, 1982). And alcoholism has a strong genetic component (Bohman, 1978; Cloninger, Bohman, and Sigvardsson, 1981; Vaillant, 1983). Measures to raise the incomes of such families might succeed, but the behavior of these families would remain pretty much as before. We could instead explore policies that would improve the competence of these parents by teaching them how they might better achieve their own goals by managing their children more effectively, supplementing this with intensive efforts to treat alcoholism or drug abuse. If we cannot design programs that effectively intervene in existing families, we might wish to explore ways of enlarging and improving the provision of foster care for children in at-risk families. The family (or juvenile) court might play a more aggressive role in the early identification of such families. Such programs are not easily conceived or implemented, but if put in place they would address the key factor in the etiology of delinquency.

Now suppose the third hypothesis is correct—that it is the child himself who is in a sense “causing” his own delinquency because he is presenting to otherwise capable and caring parents an especially difficult socialization problem. There is evidence, for example, that low-birth-weight children are inordinately at risk for child abuse because, it seems, such children are relatively passive (they do not reward parents with cooing and smiling), they have difficult temperaments, and are likely to be below normal in intelligence (Belsky, 1980, 1981; Kumar, Anday, Sacks, Ting, and Delivoria-Papadopoulos, 1980). Or the child may be born with minimal brain damage, possibly owing to parental or perinatal stress (such as alcohol or drug abuse by the mother or oxygen deprivation during birth). Or the child may have had a normal birth but be hyperkinetic, so that he is very difficult to control. Improving the competence of the parents might make some difference in how the child is raised, but routine competence may be insufficient. Indeed, family discord may arise because the parents wrongly blame each other (or themselves) for their apparent failures. Should this causal pattern exist, we would want to explore ways of reducing the incidence of prenatal and perinatal stress (by, for example, improving prenatal care, dissuading pregnant women from using alcohol or drugs, and altering diets) and ways of managing hyperkinetic children (by appropriate drug and other therapies).

None of these policies is at all fanciful, though each is difficult and requires careful testing. Increasing family income is politically but not technically difficult; improving parental competence has been successful in some experimental projects (Patterson, Chamberlain, and Reid, 1982); we are learning more all the time about ways of managing difficult children. Some readers may feel that the problem of crime is sufficiently serious, that the prudent course is to try everything at once; but that, we think, is unrealistic. If all were tried simultaneously and the combination produced a good result, we would not know which program was making the difference. Then, if for fiscal or political reasons pro-

grams had to be cut, we would not know which could be safely cut. Moreover, each program would be controversial because of cost or incentive effects, because of the degree of intrusiveness into family life, or because medical methods for altering behavior make people worry about the potential for manipulation and the risk of harmful side effects. It would be far better to discover what causal pattern is in fact operating before designing any large-scale effort to alter it. With existing knowledge, we cannot make that judgment.

SCHOOLING

There is little doubt that high-rate delinquents tend to do poorly in school. There is great doubt as to why this should be so. As a result, we cannot be certain what kinds of school programs, if any, would make a lasting difference for a large number of children who seem predisposed to delinquency. Again, there are several alternative hypotheses:

1. Children with mild behavioral problems become labeled as troublemakers in school. As a result, teachers come to have lowered expectations for them, the stigmatized children associate with other troublemakers, and breaking the rules comes to be more satisfying than conforming to adult standards.
2. Children with below-normal levels of intelligence or other learning disabilities find school work frustrating or boring or both. They become restless and take satisfaction in activities—fighting, truancy, rowdiness, sports—for which their cognitive deficits do not disqualify them.
3. Predelinquent children are aggressive and antisocial before they come to school. School may provide them with greater opportunities for mischief and acquaint them with some like-minded friends, but school does not cause their delinquency.

As with family processes, so with school processes: The causal pattern, and hence the appropriate remedial strategy, is unclear. If youngsters become delinquent because they are labeled as troublemakers, then changing teacher expectations and behavior should reduce delinquency. If frustration born of learning difficulties leads to delinquency, then devising better ways to teach and stronger rewards for mastering classroom assignments should reduce the frustration and thus the delinquency. If children have already become overly aggressive and undisciplined before entering school, then schools or preschool programs will have to be created and managed that will, if possible, counteract these predispositions.

As we note in Chapters 3 and 4, there is some experimental evidence suggesting that certain preschool programs may succeed in reducing later delinquency and that schools with a certain ethos and balance of academic talents may reduce delinquency during the school years. These glimmers of hope are worth pursuing, but many questions remain unanswered. We cannot be confident that the reduction is real because (with respect to preschool programs) it has so far only been measured in one or two experimental projects that involve a small number of

pupils. Such projects may appear to succeed because the experimenters are able to attract the most talented and dedicated teachers; whether the same treatment would work if applied by less gifted teachers to large numbers of pupils is not clear.

Even if the delinquency-reduction effect of preschool education is real, no one yet knows what it is about the program that produces this effect or for what kinds of children the program is best suited. Does preschool education work because it removes the child from adverse family conditions, or does it work because it supplements and strengthens the well-intentioned efforts of parents to cope with a difficult child? Would preschool education be harmful for children who are thereby removed from a good family, or would such education help all children whatever their family life? Is the key element of the program the cognitive preparation of the child for group activities? Does the program reduce the rate at which any enrolled child will later commit offenses or only the rate at which certain kinds of children (say, those who would otherwise become low-rate offenders) commit future delinquencies?

Many of the same questions can be asked about research showing the desirable effects of attending schools with a certain organizational ethos or character. Is the reduction in delinquency and school misconduct limited to the school-age years, or does it persist after school? Is there a reduction in the serious delinquencies of high-rate offenders or only in the kinds of minor offenses and trancies that are displayed by a very large proportion of all school children? Can a desirable school ethos be created by plan so that children randomly assigned to such a school will show markedly better behavior than similar children assigned to a conventional school?

We seem closer to identifying ways of improving schools than we are to finding methods of improving families, but we are far from knowing how to mount large-scale efforts through the schools and whether, if mounted, they would have a minor or a major effect on later criminality. There have been countless school-based programs tried in the past; the results of most have been dashed hopes. To improve the chances of success, we have to know more precisely what the causal connections are, if any, between schooling and later conduct. We are not much closer to answering that question today than we were 30 years ago.

DRUG ABUSE

High-rate offenders typically are involved with drugs or alcohol abuse. Studies at Rand (Chaiken and Chaiken, 1982) and elsewhere suggest that regular drug abuse is one of the characteristics of the most dangerous offenders. For example, offenders may commit six times as many nondrug crimes when they are using heroin on a daily basis as they would during periods when they are off the drug (Ball, Rosen, Flueck, and Nurco, 1981).

Almost everything we know about persons who are physiologically or psychologically addicted to drugs comes from studying those who are deeply into their addictive behavior and happen to have been arrested or to have volunteered for

a treatment program. Such persons are likely to be a quite unrepresentative sample of all those who have experimented with drugs. Because of this, we cannot be confident we know the characteristics of persons who are high-rate users as opposed to casual experimenters or “chippers.” There is a sharp disagreement among experts about the effects on high-rate users of a law-enforcement crack-down on drug trafficking. Some argue that such policies reduce the availability of drugs and thus reduce their use; others argue that these policies merely drive up the price of the drugs and thus induce users to commit more crimes to pay for their habits; still others suggest that crackdowns have no discernible effect at all on the supplies available to regular users.

To answer either question requires that we learn more about the individual characteristics of novices and desisters. Efforts to infer what factors predispose some persons to drug use have had to rely, for the most part, on studies of persons already known to be users, and much of that research has been limited to gathering readily available social and demographic data (age, ethnicity, family circumstances) plus, occasionally, psychological profiles. But it is quite possible that there are physiological or biological factors that put some young persons at greater risk for drug abuse than others, just as we already know that there is, to some extent, a genetic basis for the tendency to abuse alcohol. Systematically investigating these and other possibilities is essential if we are to do anything more to prevent drug abuse than mount broad-gauge, ill-focused, and usually untested educational programs designed to persuade young persons to avoid drugs.

EMPLOYMENT

Since high rates of criminality occur disproportionately among people who have low incomes and spotty employment records, it is only natural to assume that poverty and unemployment cause crime. They may, but the case for that connection has not been firmly established; and there exist other possible explanations for this observed connection. For example:

1. People turn to crime because they are unable to find jobs.
2. Certain intellectual and temperamental characteristics (e.g., low verbal aptitude, impulsiveness, little regard for others) may cause some people both to turn to crime and to be unattractive to prospective employers.
3. People who find crime very rewarding (e.g., drug dealers) may reject available jobs and thus be counted as “unemployed.”
4. Youngsters raised in neighborhoods where affluent criminals have become role models may not value the benefits of legitimate work.
5. People raised in neighborhoods suffering from chronic shortages of jobs may assume without individually exploring the market that jobs are not available and, hence, training for jobs is pointless.

Though some studies have shown a correlation between crime on the one hand and unemployment, economic recessions, and lowered labor force participation

on the other, these correlations are in many cases not strong. Other studies find no such correlation. And where significant correlations do exist, they tell us very little about which of the causal mechanisms listed above, or others that we might imagine, are actually operating. The inconclusive nature of these studies may well result from their relying, with very few exceptions, on aggregate data—that is, on the crime and unemployment rates for entire cities, counties, or states. What is needed are more studies measuring the effects of *individual* experiences in the labor market on *individual* tendencies to commit crime.

Efforts to reduce criminality by experimental interventions designed to supply job training, find employment, and even subsidize ex-offenders looking for employment have so far produced only a few encouraging results (these studies are summarized in Chapter 3). But these programs have generally been directed at adult offenders, ex-convicts, or delinquent school dropouts who are age 18 or older. It may be impossible to intervene successfully this late in the criminal or delinquent career; perhaps starting such programs much sooner, before crime itself has become rewarding, would make a greater difference. Or it may be that the temperamental and cognitive problems of persons who become high-rate offenders cannot be changed significantly by employment and job-training programs. Or it may be that such programs will work, but only if the participants are removed from those neighborhoods that make hustling more attractive than work.

In short, a serious investigation of the connection between crime and work may have to start much earlier in the lives of young men than is commonly supposed. In particular, we may need to know more about the earliest work experiences of youngsters and the relationship between schooling and work. The transition from school to work is poorly understood, especially for those young people who drop out of school early. In studying that connection, we should not begin with the assumption that dropping out of school is always a bad idea. Some persons may benefit from leaving school before graduating, provided they can thereby enter into the discipline of the workplace (Elliott and Voss, 1974). We know too little about the differences among people that affect how entry into the work force is (or is not) accomplished.

JUVENILE COURT

The debate over the role of the juvenile court is a familiar one. Those who think crime can best be reduced by making the penalties for its commission greater want to see the court take seriously the first nontrivial infraction of a youngster by imposing on him some significant penalty (incarceration, mandatory community service, victim restitution) early in the delinquent career in hope that this first short, sharp shock of punishment will deter future and greater misdeeds. If the youngster should persist in criminality, the penalties should be steadily increased, if necessary by transferring him to the jurisdiction of adult court where long sentences can be handed out. Juvenile records should automatically be made