

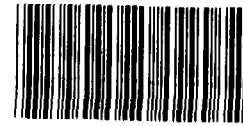
GAO

Report to the Chairman, Select
Committee on Narcotics Abuse and
Control, House of Representatives

September 1990

INTERMEDIATE SANCTIONS

Their Impacts on Prison Crowding, Costs, and Recidivism Are Still Unclear



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**Program Evaluation and
Methodology Division**

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September 7, 1990

The Honorable Charles B. Rangel
Chairman, Select Committee on
Narcotics Abuse and Control
House of Representatives

Dear Mr. Chairman:

As you are well aware, prison crowding has reached crisis proportions in the United States. Although 40 states are currently operating all or part of their correctional systems under court orders or consent decrees to reduce prison crowding, more and more offenders are being incarcerated every day. One response has been to expand prison capacity. However, prison construction is expensive and, in times of budgetary constraints, can drain resources from other important programs.

This situation has prompted many state and local jurisdictions to experiment with programmatic alternatives to incarceration. These alternatives, referred to in this report as intermediate sanction programs, make the twin claims that they are (1) less costly than incarceration and (2) as effective as or more effective than traditional probation or parole programs in controlling and treating offenders released into the local community. Based on these claims, intermediate sanction programs promise a safe, cost-beneficial alternative to incarceration. Some type of intermediate sanction program is now in effect in almost every state in the nation.

At your request, we have examined what is known about the effectiveness of intermediate sanction programs. Specifically, we examined the extent to which evaluations of intermediate sanctions have been able to provide sound answers to the following three questions:

- Have intermediate sanction programs materially affected prison crowding?
- Are they a cost-saving alternative to incarceration?
- Do they effectively control crime?

Our research is based upon all evaluations of intermediate sanction programs completed by December 1989. Using a data base of 28 of these evaluations, we conclude that there exists little in the way of specific information to guide policy in this area.

What we have learned with regard to the effect of intermediate sanctions on prison crowding can be summarized as follows. It is clear that existing programs have had little effect and are unlikely to have a sizable one on prison populations. This is because most programs have served a relatively small population of offenders. Programs that include hundreds of offenders cannot significantly affect prison populations that run into the tens of thousands. In addition, the size of the intermediate sanction programs has not been the only factor limiting their utility for addressing the problem of crowding in the nation's prisons. Even in the few states with large programs, other factors have worked to cancel out program effects. Florida, for example, operates a house arrest program that serves approximately 6,500 offenders, but it has not been able to offset an upward trend in inmate populations caused by an expanding offender population and harsher sentencing practices. As a result, prison crowding in Florida is worse today than when the intermediate sanction program began.

With respect to the cost-saving aspect of intermediate sanction programs, we know that the per capita cost for operating an intermediate sanction program is less than that for operating a prison. However, it is not clear from the evaluations we reviewed that the lower per capita cost of intermediate sanctions translates into an overall cost savings for the state or jurisdiction mounting the program. In fact, it is possible that the programs may actually increase the total cost for corrections. (See appendix II.)

For our third study question, on the effectiveness of intermediate sanctions at controlling crime, we concentrated our analysis on intensive supervision probation or parole (ISP) programs. We did so because intensive supervision programs were the only programs that had a sufficient number of sound evaluations to allow us to reach conclusions on this question using extant data. Even here, methodological limitations in the evaluations make our findings general in nature. However, the uniformity of the findings on criminal behavior do lead us to three general conclusions.

First, it is clear that some participants in ISP programs continue to engage in criminal activity and, therefore, expose the public to some level of risk. Depending on the measure used, the evaluations report that between 5 and 44 percent of all ISP offenders commit new crimes. However, these are estimates of criminal activity and, therefore, the actual level of risk to the community posed by ISP programs is unknown.

Our second finding is that estimates of the commission of crime for ISP offenders generally fall between that of regular probationers and parolees. The difference is particularly striking with respect to the latter: ISP offenders had uniformly lower reported rates of crime commission than parolees. Thus, while ISP offenders do pose some threat to public safety, it is notably less than that posed by offenders released to non-ISP parole.

Our third finding on crime commission concerns the extremely high rate at which non-ISP parolees commit new offenses subsequent to their release. The evaluation of Ohio's ISP program, for example, found that parolees were rearrested at a rate almost 25 percent higher than ISP program participants. In Georgia and New Jersey, the rearrest rate for parolees was close to 50 percent higher and their reconviction rates almost double that of ISP offenders. A study of high-risk parolees in Wisconsin found that regularly supervised parolees were reimprisoned for a new crime at a rate five times that of ISP parolees. As pressures to expand prisons continue to intensify, it is important to recognize that many offenders are not deterred by prison from continuing to engage in criminal activity. And they continue to commit new crimes at a rate well above that of either traditional probation or intensive supervision programs.

Beyond these findings, the methodological problems with extant evaluations prohibit any definitive answers regarding the effectiveness of ISP programs in maintaining public safety. More specific answers to this question will require that future evaluations overcome three fundamental problems that limit current knowledge. First, we need a better understanding of the relationship between measures of criminal activity (for example, arrests) and the "true" level of crime commission by offenders. Second, there is a need to improve the comparisons made in evaluations of intermediate sanctions. The absence of certainty that comparison groups are truly comparable makes it difficult to determine if observed differences (or the lack of differences) between ISP and other offenders result from the program or other influences. Equally important for comparison purposes, there is currently no established standard against which to compare a study's findings. Without such a standard, the same findings can be (and have been) used to characterize programs as either "successes" or "failures." Finally, there is a need to examine programs for longer time periods for meaningful conclusions to be drawn about their effectiveness.

The information in this report is organized as follows. Appendix I primarily discusses the objectives, scope, and methodology of our synthesis, although we also provide some descriptive information regarding the types of intermediate sanctions we examined. Appendix II presents our analysis of how well current research has been able to evaluate the effectiveness of intermediate sanctions with respect to prison crowding and costs. Appendix III describes and analyzes the evaluations that examined program effectiveness in preserving public safety. The references list the 28 evaluations upon which our synthesis is based.

We have not requested comments from any federal agency, since we studied only state and local programs. Unless you publicly announce the contents of this report earlier, we plan no further distribution until 30 days from its date. We will then make copies available to others upon request. If you have any questions or would like additional information, please call me at (202) 275-1854 or Mr. Kwai C. Chan, Director of Program Evaluation in Human Services Areas, at (202) 275-1652. Other major contributors to this report are listed in appendix IV.

Sincerely,



Eleanor Chelimsky
Assistant Comptroller General

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Abbreviations

CCA	Community Corrections Act
FCCP	Florida Community Control Program
ISP	Intensive supervision probation, or intensive supervision parole

Objective, Scope, and Methodology

Prison crowding in the United States has reached unacceptable levels. According to the Bureau of Justice Statistics, prison populations nationally rose from 329,821 in 1980 to 627,402 in 1988, a 90-percent increase. Prison capacity has been stretched to its limits and beyond. As a result, many jurisdictions have experimented with programmatic alternatives to incarceration. The majority of these experimental programs are referred to as “intermediate sanctions” and are intended to serve as an alternative level of sanctioning severity that falls between incarceration and traditional probation or parole. Although diverse in nature, intermediate sanction programs are typically advanced as less costly alternatives to incarceration that are as effective as or more effective than traditional probation in controlling or treating offenders in the local community.

Objective

Our purpose was to learn what is known about intermediate sanction programs from existing research. This was the task posed to us by the House Select Committee on Narcotics Abuse and Control. In this report, we examine what is currently known about the effectiveness of intermediate sanction programs in the areas of prison crowding, costs, and protecting public safety. More specifically, we examine the extent to which current evaluations of intermediate sanctions have been able to provide reliable answers to the following three questions:

1. Have intermediate sanctions materially affected prison crowding?
2. Are intermediate sanctions a cost-savings alternative to incarceration?
3. Do intermediate sanction programs effectively control crime?

Scope

The scope of our study is bounded by the type of evaluations we examined, the programs’ target populations, the types of sanction that were evaluated, and the time covered by the evaluations.

Type of Evaluation

In our data collection effort (described in the methodology section), we sought to obtain all evaluations of intermediate sanction programs that addressed one or more of our study questions. However, because our study concerns the effectiveness of intermediate sanction programs, we focused only on “impact” evaluations that examined the relationship between these programs and prison crowding, correctional costs, and

criminal behavior. Accordingly, we excluded from review “process” and “implementation” evaluations, since these focus attention more on operational than on outcome variables.

Target Population

Our study questions address the effect of intermediate sanctions on prison crowding and costs. Therefore, we excluded from review the following types of evaluation: evaluations of programs that are designed to supervise pretrial releasees, evaluations of programs for those sentenced to jail rather than prison, programs that target misdemeanants rather than felons (the former being limited to jail terms), and programs that target juveniles rather than adults (the former are generally not subject to imprisonment).

Type of Sanction

The concept of “intermediate sanctions” has been used to describe a broad array of sanctioning programs. We included for review evaluations of both “front-door” and “back-door” programs. Front-door alternatives refer to programs aimed at preventing the imprisonment of an offender. This is typically accomplished at the point of sentencing or soon thereafter (for instance, postsentence screening). Back-door alternatives refer to programs designed to provide for the early (conditional) release of persons already imprisoned.

Our review also focused only on “stand alone” programs—that is, programs that are conceived and operated as a distinct and self-contained sanctioning option in their own right. This excluded from review sanctions that are components of other programs or are “add-ons” to other sanctions. For example, electronic monitoring is frequently discussed as though it were an independent intermediate sanction; however, electronic monitors are virtually always a component of intensive supervision or house arrest programs. Evaluations of electronic monitoring were included only if monitoring was not part of another program, which is extremely rare. We applied the same criterion to fines, community service, and restitution. Consequently, our review primarily concentrated on intensive probation or parole supervision, house arrest, and shock incarceration. These programs are described below.

Exactly what is included in the category of intensive supervision probation or parole varies considerably across jurisdictions and even within jurisdictions. However, in general, ISP programs target a specified population of offenders for more intense levels of supervision and surveillance. Increased surveillance is typically coupled with other conditions

of probation or parole, including curfews, restitution, community service work, drug and alcohol testing, substance abuse treatment, and an employment or educational requirement. ISP programs also limit caseloads to a maximum level well below that of traditional supervision. This allows for an increased number of contacts between supervisor and offender, collateral contacts with employers, more frequent alcohol and drug testing, and closer monitoring of participation in treatment services. Offenders are ordinarily required to spend a minimum time in the program before being either released from supervision or, more typically, released to a period of regular probation or parole supervision.

Intensive probation supervision in Colorado well illustrates the structure and operation of ISP programs. Initiated in 1984, the program currently serves over 350 offenders in five judicial districts, and current plans are to expand the program statewide in the near future. In an effort to lower prison crowding and costs, the program targets offenders who would ordinarily be imprisoned but, program officials believe, can be safely supervised in the community. Offenders are eligible for the program if they have committed a minor or midrange felony and are rated on a sentencing grid (based on offense seriousness and criminal history) as likely to receive a prison sentence.

Offenders enter the program through one of three ways: direct sentence, probation revocation, or an amended sentence (the initial prison sentence is modified to ISP). Direct sentence is the most common method of placement. During the course of presentence investigations, probation staff identify eligible offenders who are likely to benefit from the program and refer the case to a screening committee. The committee reviews the case and makes a recommendation to the sentencing judge, who decides whether to place the individual in the program. Offenders entering the program because of probation revocation or an amended sentence undergo a similar identification and screening process, the final placement decision again resting with the sentencing judge.

As with most ISP programs, Colorado's program has differing levels of supervision and requires participants to adhere to numerous requirements. At the higher of Colorado's two levels of intensive supervision, the offender must observe a strict curfew, gain and maintain employment, submit to drug and alcohol testing, perform community service, and (if referred) enroll in treatment or other rehabilitative programs. In addition, ISP staff have a minimum of eight face-to-face contacts a month with the offender, make daily phone contacts, and meet with employers, family members, and other knowledgeable persons to discuss

the offender's progress in the community. ISP mandates four times the minimum number of contacts required for offenders placed in maximum supervision regular probation. In order to allow for this enhanced level of supervision, ISP caseloads are held to a maximum of 25 offenders per supervising officer.

Offenders are reviewed at least every 3 months to assess progress in the program (3 months is the minimum time an offender can be in the program). Satisfactory compliance with program requirements results in the offender's being downgraded to a lower level of intensive supervision with more relaxed standards. Failure to comply with program conditions can result in revocation proceedings and placement in prison, jail, or another community program. Successful completion of the program results in the offender's transfer to maximum supervision regular probation.

House arrest, although a somewhat harsher sanction than intensive supervision, is in many ways quite similar to ISP programs. The principal difference lies in the greater degree of control and surveillance imposed on offenders in house arrest programs. In ISP programs, for instance, the primary residential restriction the offender must honor is a curfew. That is, the offender must be at home during some specified period of time. With the exception of hours of employment and other mandated constraints on the person's time, such as community service, the offender has relative freedom of movement. Instead, house arrest programs only allow the offender to leave his or her residence for specific purposes approved by the court or supervising officer (for example, employment or medical treatment).

A brief description of the Florida Community Control Program (FCCP) helps illustrate the similarities and differences between ISP and house arrest programs. FCCP is one of the best known and largest house arrest programs in the nation. Implemented in 1983, this statewide, front-door program serves approximately 6,500 offenders. FCCP was conceived as a method of safely supervising prison-bound offenders in the community. (Unlike most programs, however, cost-savings was not a prominent issue in the creation of the FCCP.) The program's target population is nonviolent felons who, for reasons of their criminal history and current offense, would not ordinarily qualify for probation.

Criteria for placement in the program are incorporated in Florida's sentencing guidelines, which also were implemented in 1983. If an offender falls within a certain range on the guidelines scale of sentencing severity

(determined by current offense characteristics and criminal history), the judge has the option of sentencing the offender to 12 to 30 months in prison or FCCP. Additional options may become available as a result of the presence of aggravating or mitigating circumstances or “judicial overrides.”¹ The courts, therefore, occupy a central role in the placement of offenders in the program.

The house arrest component in FCCP requires the offender to remain confined to his or her residence except for employment, participation in public service or community service work, or self-improvement programs that have been approved by the supervising officer. In addition, the offender may be required to make restitution, pay supervisory fees (\$30 a month), and maintain a daily activity log for review by the supervisor. Supervisors are limited to a maximum caseload of 20 offenders and are required to make a total of at least 28 contacts (field, phone, and office) a month with the individual. In late 1987, Florida also began experimenting with electronic monitoring at selected FCCP sites. Finally, offenders can be required to spend up to 2 years in FCCP. Those who successfully complete the program may be placed on regular probation or granted unconditional release. Those who fail the program are generally imprisoned.

Shock incarceration refers to a very different type of intermediate sanction program than ISP or house arrest. Shock incarceration programs are sanctions that place young, often first-time felony offenders in a prison environment modeled on a “boot camp” regimen. These programs require strict discipline, physical training, military drills, and hard labor. The punitive components of the program are often supplemented with counseling and other services that address such problems as personality disorders and low work skills. Upon successful completion of the program, which usually runs for 3 to 6 months, the offender is transferred to community supervision.

New York State’s shock program is an excellent example. Established in 1987, the program operates two facilities with a capacity of 250 offenders each, making it the largest shock program in the nation. The program targets offenders 16 to 26 years of age who are in need of substance abuse and other rehabilitative services (particularly with respect to educational and social skills). These offenders must already have been sentenced to prison and have at least 6 months, but less than 3

¹Under Florida’s sentencing guidelines, a judge may override the presumptive sentence called for by the guidelines.

years, remaining in their prison term before they are eligible for parole. Excluded from participation in the program are offenders with a prior felony conviction, prior escapes from custody, absconders from another program, and offenders with a history of violent or sex crimes. Participants also must be physically and mentally able to endure the program.

Once it is determined that an offender has met the eligibility criteria, the individual must apply for entry into the program. Applicants then undergo a formal two-stage screening process to determine if they are likely to benefit from the program. Less than half of all applicants ultimately enter the program.

The incarceration phase of New York's shock program lasts 6 months. Participants are isolated from the general prison population while in the program. The program emphasizes extensive discipline, strenuous physical activity, group living (groups are referred to as "platoons"), counseling, and the development of social and educational skills. For example, on weekdays, the group arises at 5:30 a.m. and undergoes military drills and formations, calisthenics, 6 hours of work schedules, and 3 hours of counseling and educational activities before retiring at 9:30 p.m. Only 1 hour a week is scheduled for personal time.

The intent of this highly structured environment is to instill in young offenders a sense of self-esteem and self-discipline as well as improved social skills. Program administrators do not encourage rapid revocations for rule violations. Rather, peer counseling (or some similar method) is the preferred method for dealing with problem behavior. Consequently, shock participants can commit several rule violations before revocation. However, revocation results in reimprisonment. Successful completion of the program results in early release to parole.

Table I.1 shows the distribution, by state, of intensive supervision, house arrest, and shock incarceration programs. The table also indicates which states have adopted electronic monitoring of offenders as part of either their house arrest or intensive supervision programs.

Appendix I
Objective, Scope, and Methodology

Table I.1: Selected Intermediate Sanction Programs, by State and Type of Program^a

State	House arrest	Intensive supervision^b	Electronic monitors^c	Shock
Alabama	x	x	x	x
Alaska		x		
Arizona	x	x	x	x
Arkansas		x		
California		x	x	
Colorado		x	x	
Connecticut		x	x	
District of Columbia		x	x	
Delaware	x	x	x	
Florida	x		x	
Georgia	x	x	x	x
Hawaii	x	x	x	
Idaho		x		
Illinois		x	x	
Indiana	x		x	x
Iowa		x	x	
Kansas	x	x	x	x
Kentucky	x	x	x	
Louisiana	x	x		x
Maine		x		
Maryland		x	x	
Massachusetts		x		
Michigan	x	x	x	x
Minnesota	x		x	
Mississippi	x	x	x	x
Missouri	x	x	x	
Montana		x	x	
Nebraska		x	x	
Nevada	x		x	
New Hampshire	x	x	x	
New Jersey		x		
New Mexico	x	x	x	
New York	x	x	x	x
North Carolina	x	x	x	
North Dakota				
Ohio		x	x	
Oklahoma	x		x	
Oregon	x	x	x	
Pennsylvania		x		

(continued)

**Appendix I
Objective, Scope, and Methodology**

State	House arrest	Intensive supervision^b	Electronic monitors^c	Shock
Rhode Island				
South Carolina	x	x	x	x
South Dakota	x	x		
Tennessee		x	x	
Texas	x	x	x	x
Utah		x	x	
Vermont	x	x	x	
Virginia		x		
Washington		x		
West Virginia	x	x	x	
Wisconsin	x	x	x	
Wyoming		x	x	x
Total	27	44	37	13

^aData are based on a GAO phone survey of correctional agencies conducted during spring and summer of 1989. Although data are categorized by state, this does not imply a statewide program. Many of the programs listed are limited to a small number of jurisdictions or few clients. Some programs were pilot programs that have since been discontinued. Data on other alternative sanctions, such as fines, restitution, and community service, are not reported because they are used so rarely as stand alone programs for felony offenders.

^bIntensive supervision refers to any form of ISP program, whether probation or parole or both.

^cElectronic monitoring refers to any type of electronic monitoring component, regardless of whether employed as an adjunct of ISP or house arrest or, rarely, as a stand-alone program.

Timeframe

We used different cutoff dates for including studies in this synthesis. For relatively new programs, such as house arrest, electronic monitoring, and shock incarceration, we included all evaluations. In contrast, intensive supervision, restitution, and community service have existed in one or another form for some time. Consequently, we used previous nationwide reviews of the literature on these sanctions to establish a baseline for our review. Banks et al. conducted a nationwide survey of intensive supervision; Hudson and Galaway synthesized evaluations of restitution and community service programs.² Thus, for these sanctions, we limited our review to evaluations conducted after the publication of these two reports.

²J. Banks et al., "Evaluation of Intensive Special Probation Projects: Phase I Report," U.S. Department of Justice, National Institute of Law Enforcement and Criminal Justice, Washington, D.C., September 1977; Joe Hudson and Burt Galaway, "National Assessment of Adult Restitution Programs. Preliminary Report II: A Review of Restitution Research," School of Social Development, University of Minnesota, Minneapolis, Minnesota, April 1979.

Methodology

Our conclusions are based on the evaluation synthesis methodology.³ Some aspects of special interest here concern data collection methods and criteria for inclusion of evaluations in the synthesis.

Data Collection

We used different data collection methods to obtain the evaluations. In order to find published evaluations, we undertook a literature search of the major criminal justice and social science journals. We conducted phone interviews with National Institute of Justice and National Institute of Corrections personnel, as these federal agencies are significant funding sources for criminal justice programs and research. In addition, we obtained information concerning research funded by the National Institute of Justice from its Compendium of Research Sponsored by the National Institute of Justice 1969-1984, National Institute of Justice Publications 1984-1988, and Research Abstracts: Current Federal Research 1987-1989. We contacted researchers engaged in ongoing evaluations scheduled for completion by August 1989. We also contacted individuals recommended as knowledgeable sources regarding intermediate sanctions. The list of existing evaluations on intermediate sanctions constructed from these sources was verified by researchers expert in the area.

To obtain completed unpublished evaluations, we contacted the departments of corrections of each state. This was accomplished by consulting the 1988 American Correctional Association Directory entitled Juvenile and Adult Correctional Departments, Institutions, Agencies and Paroling Authorities, which describes the organization of correctional agencies within each state and gives their addresses. Where the correctional system was decentralized, we contacted each department of corrections, probation, and parole. We sent an introductory letter explaining our GAO study and requested assistance. Finally, we made follow-up telephone calls to each of the departments to encourage them to provide copies of existing evaluations of their programs.

Inclusion Criteria

The data collection described above yielded 61 documents that contained some outcome information relevant to one or more of our questions—prison crowding, costs, and crime control. After eliminating or consolidating documents containing redundant or overlapping information, we reviewed the remaining documents to determine if the studies

³See U.S. General Accounting Office, The Evaluation Synthesis, methods paper I (Washington, D.C.: April 1983), for details on the methodology.

were of sufficient methodological quality to warrant their inclusion in our review. The result was 28 evaluations examining 22 programs in 17 states. Fifteen evaluations addressed the question of prison crowding, 10 the question of costs, and 22 the question of crime control. (A list of the 28 evaluations is provided at the end of the report.) The decision to include an evaluation in our analysis was based on the following three criteria.

Prison Crowding

If an evaluation is to meaningfully address the question of prison crowding, it must determine the extent to which a program's target population would currently be in prison in the absence of the program. Some method should be employed to determine the number or percentage of offenders in a program who would actually have been sent to or maintained in prison if the program did not exist. We excluded from analysis evaluations that failed to meet this criterion.

Correctional Costs

Any consideration of correctional costs for intermediate sanction programs should be relative. That is, the policy-relevant aspects of the costs of these programs can be determined only by comparing them to those of traditional probation and incarceration. For example, from a policy perspective, it is trivial to know that an alternative sanction program costs, say, \$6,000 per capita per year unless one also knows the annual costs of maintaining a similar offender on regular probation (for example, \$2,000) or in prison (for example, \$15,000). We only included in our analysis evaluations that addressed the issue of comparative costs.

A second cost criterion was that the factors included in cost calculations had to be made explicit. Aggregate prison costs, for instance, are comprised of many components, such as capital construction, debt service, and operating expenses. The same is true of intermediate sanction programs. Without knowing the factors included or excluded in cost estimates, cost comparisons are meaningless. We excluded from analysis evaluations that failed to specify cost components.

The Commission of Crime

Our third question concerns program effects on criminality. However, crime commission cannot be directly measured. Any evaluation of program effects, therefore, must use some estimate of the commission of crime. For example, it is not enough to know that 50 percent of offenders "failed" a program. A more specific estimate of offenders' criminality (such as rearrest or reincarceration) is needed. We excluded from analysis evaluations lacking some specific estimate of crime commission.

In addition, crime commission, similar to costs, should be evaluated from a relative perspective. Meaningful conclusions regarding program effects on criminality demand that comparison groups be established. Without comparison groups, we can learn little about the effects of program intervention. To learn that 50 percent of participants “failed” a program tells us little unless we know the percentage of offenders who “fail” on probation or following a term of imprisonment. We excluded from our review evaluations that did not employ a comparison group in their study.

Prison Crowding and Costs

One of our study questions was to determine if intermediate sanction programs materially affect prison crowding by reducing the size of prison populations. A second question had to do with the cost-savings potential of intermediate sanctions. In this appendix, we review our findings for both issues.

As indicated in appendix I, 15 of the evaluations provided information relevant to the issue of prison crowding, while 10 addressed the question of costs.¹ Our examination of these evaluations revealed methodological problems that limit the utility of the information they provide on each issue.

What we have learned with regard to crowding can be summarized as follows. While it is clear that intermediate sanction programs have diverted some offenders from prison, these programs have not had a pronounced effect on prison populations in any of the jurisdictions we studied.² One reason is the limited capacity of most existing programs. For example, in 1986, New Jersey's intensive supervision parole program had an active caseload of just over 400. At the same time, the state had an inmate population of approximately 12,000. In the same year, New York's shock incarceration program had an active caseload of approximately 450 in a state with a prison population approaching 40,000.

The capacity of intermediate sanction programs is not the only factor limiting their effectiveness in alleviating prison crowding. Even in states with relatively large programs, the size of prison populations has continued to grow. Florida, for example, operates a house arrest program that serves approximately 6,500 offenders. Yet, this program has not been able to offset an upward trend in inmate populations caused by an expanding offender population and harsher sentencing practices. As a result, prison populations in Florida are larger today than when the house arrest program began.

With respect to cost, we have learned from the evaluations that the per capita cost of operating an intermediate sanction program is greater than that for regular probation or parole but tends to be less than that

¹Of these, only 6 evaluations addressed both the issue of crowding and the issue of cost.

²In the literature on intermediate sanctions, the term "diversion" is often associated with what we refer to as "front-door" programs. However, the generally accepted usage of the term refers to any intervention that alters an anticipated course of events. Consequently, we use "diversion" to refer to any process that prevents an offender from going to, or remaining in, prison without the existence of an intermediate sanction program.

for prisons. However, the evaluations do not adequately address whether intermediate sanction programs increase, decrease, or do not change aggregate state and local expenditures for managing offenders.

Beyond these findings, we have not been able to obtain any definitive information related to our questions of the effect of intermediate sanctions on prison crowding or costs. In the remainder of this appendix, we describe four methodological limitations in extant evaluations that prevented us from gaining more precise answers to our study questions. We begin with a discussion of one problem, measuring diversion, whose difficulties must be surmounted if sound conclusions about either the crowding or cost issues are to be reached. We then discuss how erroneous conclusions can be reached about crowding, specifically when evaluations confuse diversion with changes in prison populations. The third limitation relates specifically to the issue of cost and how costs are measured. The final problem we discuss is the tendency of evaluations to take a static view of what is, in fact, a dynamic environment. This problem, similar to that of measuring diversion, has implications for both crowding and cost issues.

Measuring Diversion

The maximum effect an intermediate sanction program can have on prison populations is determined by the proportion of program participants that would be placed in prison, or would have remained in prison, without the program. That is, if a program serves several hundred offenders and it is known that all those offenders would have been (or remained) incarcerated without the program, then the program can be credited with diverting all its participants from prison. However, if 25 percent of program participants would have been placed on regular probation or parole, placed in some other program, or granted unconditional release if the intermediate sanction program did not exist, then the program can, at best, be only 75-percent effective in reducing prison populations.

Measuring diversionary effect is also essential to estimating a program's effect on correctional costs. To continue with our example, suppose the per capita cost is \$5 a day for regular probation supervision, \$10 a day for intensive probation, and \$20 a day for prison. If a program diverts all its participants from prison, each offender in the program will represent a \$10 daily cost-savings to the state. However, if 25 percent of the offenders in the program would otherwise have been placed on regular probation, this proportion of program participants will represent a

\$5 per day increase in correctional costs. Any accurate estimate of program costs, therefore, must make adjustments for a program's level of diversionary effect.

As these examples illustrate, it is essential to measure the diversionary effect of an intermediate sanction program in order to estimate the effect that the program has on both prison populations and costs. However, measuring diversionary effect has proven extremely difficult. Although all 15 of the evaluations addressing the issue of crowding dealt with diversion in some fashion, only 6 provided specific estimates of the magnitude of diversionary effect. The estimates ranged from a high of 94-percent diversion in Colorado's ISP program to a low of 52-percent diversion for Florida's house arrest program. The remaining evaluations estimated that between 60 and 70 percent of program participants were actual diversions from prison.

Three general methods have been employed to estimate the diversionary impact of front-door programs: judicial certification, program-level comparisons of offenders' characteristics, and system-level comparisons of prison admissions. We discuss how each of these approaches has been used within current evaluations and note the limitations associated with each one.

Of the evaluations we examined, Georgia's intensive supervision program is the only one employing judicial certification.³ Virtually all participants enter Georgia's program by way of direct sentence or an "amended" sentence. In the former case, judges were asked to certify that had ISP not been a sentencing option, the offender would have been sentenced to prison.⁴ With the amended sentence, an offender is first sentenced to prison and then, following a review of the offender's record and other pertinent facts, is recommended for intensive supervision. If the judge concurs in the recommendation, the sentence is amended to place the offender in the program.

For either course of program entry, judges were asked to certify that the offender would have either gone to or remained in prison if not for the

³Other, preliminary evaluations of intermediate sanction programs using judicial certification are discussed in M. Tonry, "Stated and Latent Features of ISP," *Crime and Delinquency*, 36:1 (January 1990), 177-78.

⁴The name given intensive supervision programs varies across jurisdictions. In Georgia and Arizona, for instance, the programs are known as Intensive Probation Supervision (IPS). Ohio's intensive supervision programs are known as Intensive Diversion Units (IDU). However, for consistency, we generally refer to all such programs as ISP programs.

program. However, it is impossible to assess the accuracy of such statements without some form of independent verification since, given the existence of the program, judges can never be certain whether an offender would have gone to prison if the program did not exist. As the evaluator herself observed, judicial certification “could hardly be considered proof” that the offender in question was actually prison-bound.⁵

A second approach compares offenders in intermediate sanction programs with other offenders on selected variables. This comparison is to determine if program participants more nearly resemble the characteristics of prisoners or probationers. Evidence of a diversionary effect is said to exist if statistical analyses show that program participants “look” more like imprisoned offenders than probated offenders. Such a conclusion is based on the premise that if a program is diverting prison-bound offenders, program participants should more closely resemble prisoners on selected offender and offense characteristics (for example, seriousness of convicted offense or offender risk levels) than they do probationers.

This second approach is the most common and is employed in 12 of the 15 evaluations addressing crowding. It is also the method that is uniformly used to estimate the magnitude of diversionary impact (6 evaluations).⁶ There are two variations of this method used in the evaluations: matrix-based comparisons and discriminant analysis. The former use a two-dimensional grid to reflect variables that are known to be associated with decisions to incarcerate or probate felony offenders. Typically, one dimension of the grid refers to the seriousness of the current offense while the other refers to some measure of criminal history. The resulting cells within the grid define the likelihood that an offender will be placed on probation or imprisoned. Observations are made on the proportion of offenders in an intermediate sanction program falling into a grid cell or cells that would typically result in incarceration or probation. The proportion of offenders who would typically be incarcerated is used as the estimate of diversionary effect.

Discriminant analysis differs from the matrix-based approach in a number of ways, the most important of which is that it uses multiple

⁵Billie S. Erwin, “Evaluation of Intensive Probation Supervision in Georgia: Final Report,” Georgia Department of Corrections, Atlanta, July 1987, p. 11.

⁶A seventh evaluation attempted a partial estimation of actual diversion but was unable to because of data limitations. See Stevens H. Clark and W. LeAnn Wallace, “Diversion from Prison by Sentence-Planning: Evaluation of a Program in Greensboro, N.C.,” *The Justice System Journal*, 12:3 (1987), 335-58.

variables to estimate the proportion of program participants who are “true” diversions from prison. This makes discriminant analysis a more precise method for estimating diversion. However, both the matrix method and discriminant analysis have the limitation that they are not completely accurate. Both procedures have been demonstrated to have some degree of error in predicting placement outcomes. For example, the instrument used in evaluating Florida’s program had an error rate of 15 percent.⁷ That is, it predicted placement outcomes with 85-percent accuracy.

What this means, however, is that the estimate of 52-percent diversion should be bounded by an error rate of plus or minus 8 percentage points (that is, 44- to 60-percent diversion).⁸ The evaluation of Texas’ ISP program had the lowest degree of accuracy at 68 percent.⁹ Therefore, the estimate of 60 percent diversion should be bounded by an error rate of plus or minus 16 percentage points (indicating that the true rate was between 44 and 76 percent). The error rates for discriminant analysis estimates, coupled with the small number of evaluations providing any specific estimate of diversionary impact (6), make any conclusion regarding the “true” magnitude of diversion in existing programs premature.

A third approach to estimating diversionary impact attempts to circumvent the problems associated with the two preceding methods by using system-level data on prison admissions. The virtue of this approach is that, unlike judicial certification and program-level comparisons, changes in prison admissions can be directly measured. Rates and characteristics (that is, offense type or criminal history) of prison commitments can be compared prior to and following the implementation of an intermediate sanction program. If the rate or characteristics of prison admissions change after the initiation of an intermediate sanction program, inferences regarding diversionary impact can be made. The

⁷Christopher Baird, “Analysis of the Diversionary Impact of the Florida Community Control Program,” National Council on Crime and Delinquency, Madison, Wisconsin, 1989.

⁸The error rate is determined by dividing the number of incorrectly classified cases by the total number of cases examined. In calculating these error rates, we assumed a univariate distribution of misclassified cases.

⁹Frank P. Williams III et al., “Assessing Diversionary Impact: An Evaluation of the Intensive Supervision Program of Bexar County Adult Probation Department,” Sam Houston State University, Criminal Justice Center, Huntsville, Texas, October 1982.

National Council on Crime and Delinquency's evaluation of Ohio's Community Corrections Act programs provides the most sophisticated example we found of this type of analysis.¹⁰

The Council's study compared the rate of prison commitments in counties with a Community Corrections Act (CCA) program against matched groups of counties without a program. These comparisons were made both before and after program implementation. Little difference in prison commitments was found between counties before CCA programs were introduced. After program implementation, total commitments (CCA program commitments plus prison commitments) increased in counties with CCA programs but remained stable in counties without CCA programs. However, prison commitments alone declined significantly in counties with CCA programs compared to those without. A diversionary effect was claimed, since total commitments were up in counties with a CCA program even though prison rates had declined. Therefore, it appeared that CCA programs had reduced the rate at which offenders were being imprisoned.

The chief problem with this type of analysis comes in attributing changes in system-level prison commitments to the program being evaluated. This is because factors unrelated to an intermediate sanction program can influence the rate of prison commitments over time. The evaluation of the Florida house arrest program (discussed below) is a strong illustration of how prison populations are sensitive to many factors other than diversion. And, while the evaluators attempted to control for factors such as population density and prevalence of crime across counties with and without CCA programs, there are many other influences on prison commitments that either may be unknown or defy quantification.¹¹ For this reason, system-level analyses can provide only broad estimates of diversionary impact even when they attempt to explore the influence of other factors on prison populations.

To summarize to this point, we have reviewed the three methods used to estimate diversion and indicated that problems exist with each one. The discussion has assumed that diversion is being estimated for "front-door" programs. However, intermediate sanction programs called back-

¹⁰James Austin, Peter Quigley, and Steve Cuvelier, "Evaluating the Impact of Ohio's Community Corrections Programs on Public Safety and Costs: Final Report," National Council on Crime and Delinquency, San Francisco, California, December 1989.

¹¹These problems and others are noted by the evaluators. See especially pp. 33-34 and 40 in Austin, Quigley, and Cuvelier.

door programs directly remove offenders from prisons. In the five evaluations of back-door programs we examined, these releasees are often high-risk or high-need offenders. After serving a brief period of imprisonment, offenders apply for entry into an intermediate sanction program, as in New York and New Jersey, or are referred to one, as in Oregon, Kentucky, and Virginia. They then go through a rigorous screening process before being placed in the program.

Do back-door programs, then, divert offenders from prison? It would appear that they clearly do, since persons released into the program no longer occupy prison space. However, this may not be the case. For one thing, it is not certain that all offenders released into the program would have remained in prison without the program. In correctional systems where prison crowding has already become a significant problem, a variety of early release programs have been developed to manage prison population overflow. In Florida, for example, the Department of Corrections has adopted new policies that allow for the increasingly early release of inmates. Between 1982 and 1987, for instance, the number of inmates released after serving less than 6 months in prison increased from 8 percent to 44 percent of prison discharges. Nationally, prison crowding led to some form of early release for approximately 19,000 inmates in 1985 alone.¹²

Further, there are ways in which back-door programs may actually increase prison populations and costs. It has been suggested, for example, that judges or other decisionmakers may sentence offenders to prison in order to effect their entry into an intermediate sanction program.¹³ That is, without the program's existence, the offender would not have been given a prison sentence at all. The extent to which this may be occurring is unknown. However, such practices would affect both costs and prison crowding. In fact, it has been estimated that if an additional 1 to 2 percent of the felons sentenced in New Jersey were imprisoned in order to place them in ISP, the resulting increase in prison populations would offset any decrease derived from the program. Similarly, costs would increase if offenders who ordinarily would have been placed on probation were sent to prison as a way to make them eligible for a back-door intermediate sanction program.

¹²Bureau of Justice Statistics, *Report to the Nation on Crime and Justice* (Washington, D.C.: U.S. Department of Justice, 1988), p. 109.

¹³Tonry, pp. 178-79. Also see Patricia L. Garris, "Parole Violators: Prison or Alternatives?" *Corrections Alternatives and Concepts, Inc.*, Smyrna, Delaware, April 1989, p. 52.

Confounding the Issues of Diversion and Crowding

Almost half the evaluations we reviewed did not carefully distinguish between a program's diversionary effect and its effect on crowding. That is, the evaluation focused on determining whether and, in some cases, to what extent, the program was diverting offenders from prison. It was then implicitly assumed or explicitly stated that the finding of diversionary impact constituted evidence of a reduction in prison populations or prison crowding. However, the connection between diversion and crowding is not so straightforward.

The evaluation of Florida's house arrest program can serve as an example of the importance of not assuming that diversion necessarily decreases prison populations.¹⁴ When diversionary effect was analyzed, the evaluation reported evidence that Florida's program was diverting at least half its participants from prison. However, when commitments to prison were compared before and after the implementation of Florida's program, the analysis revealed that prison commitments had actually increased since the inception of the program.

The evaluator attributes these seemingly contradictory findings to three factors: the implementation of Florida's sentencing guidelines, judicial "overrides" of the guidelines, and a "hardened" offender population. The sentencing guidelines mandated more severe penalties for criminal offenders, while judicial overrides allowed judges to further enhance penalties over and above those provided by the guidelines. Finally, the growing drug trade and increases in violent offenses placed more offenders in a position to receive lengthier sentences. Thus, while Florida's program could be said to be "working" with respect to diversion, other, exogenous factors more than offset any diversionary effect derived from the program.

Unfortunately, few evaluations of diversionary impact and prison crowding analyze their programs as carefully as Florida's. Rather, the tendency is to assume that evidence of a diversionary impact will automatically translate into a reduction in prison populations. This can lead to unfounded optimism regarding program effectiveness.

Problems With Cost Computation

A third problem in the evaluations concerns how costs are computed. The most frequent procedure is to compare the per capita cost for maintaining an offender in prison with the per capita cost for maintaining an offender in an intermediate sanction program. The principal flaw in this

¹⁴Baird.

procedure is that a large proportion of prison costs, such as capital outlays and personnel costs, are fixed costs. However, not every new inmate requires a new prison cell or the hiring of a new correctional officer or administrative staff person, and prison construction and operating costs will change only if there are large changes in inmate population.

Intermediate sanction programs, however, are highly labor intensive. Maximum caseloads in intensive supervision, for example, are typically 25 offenders per supervisor or team of supervisors; thus, an increase of only 100 program participants would require the hiring and training of four to eight new employees. This not only increases direct outlays in the form of salaries and expanded overhead expenses but also produces longer-term financial commitments in the form of employee benefits and pensions. As these examples suggest, a more appropriate cost analysis would be based on a comparison of marginal costs. But none of the completed evaluations have undertaken this task systematically.

A Static View of Offender Populations and Program Costs

A final problem in the evaluations is that they typically offer conclusions about a program's effectiveness in addressing the issues of crowding and costs based upon analyses conducted at a single point in time. Such conclusions ignore the dynamic nature of the criminal justice system and the fact that criminal justice policy is essentially reactive.

For example, the size and characteristics of the offender population tend to change more rapidly than prison capacity. This means that as offender populations change, the type of offender targeted for incarceration is likely to change. If (as was true in Florida) the offender population increases in size and becomes more "hardened," individuals who might have been classified at one point as "high-risk" offenders (such as burglars) who should be imprisoned might be classified differently at a subsequent point and targeted for alternative sanctions. Similarly, as drug dependency has become more widely perceived as a pressing social problem, offenders who ordinarily might have received a prison sentence for a drug-related crime may now be targeted for an alternative program that provides enhanced treatment options. In short, the realities facing persons in positions of authority can alter decisions regarding which offenders are placed in a traditional or an alternative program.

A case in point is the evaluation of the ISP program in Texas.¹⁵ Although the evaluation period was only 10 months, noticeable changes were observed in the characteristics of offenders admitted to the program. Over time, ISP offenders tended to be more socially disadvantaged, to have had less-extensive prior incarcerations, and to have fewer needs for special treatment services. It was also observed that diversionary estimates declined over the evaluation period. A 60-percent diversion rate was estimated at the beginning of the evaluation period, a 67-percent rate for the middle of the period, and a 50-percent rate at the end of the period. Thus, while the average rate was 60 percent for the entire 10 months, there was considerable fluctuation even within this short span of time. However, few evaluations provide information on how time might affect diversionary impact.

A similar problem exists with cost estimates. For instance, most intermediate sanction programs have a relatively high rearrest rate. The rate for Georgia's program, one of the most emulated, is 40 percent. If all were revoked, the state and local jurisdictions would have paid not only for the initial cost of processing offenders and supervisory costs while in the program but also the processing costs for revocation and the cost of imprisonment for 40 percent of the offenders. However, only two evaluations (Arizona and New Jersey) attempt to incorporate revocation costs into their overall cost analysis. And, even in these two cases, the costs are only estimated for a single point in time. Clearly, only when we begin to attain longitudinal data on relative program costs will we be able to provide reliable estimates of the true costs of intermediate sanction programs.

Summary

As the preceding discussion demonstrates, evaluations of intermediate sanction programs do not provide us with definitive answers to our questions regarding prison crowding and costs. Problems in measuring diversionary impact, lack of clarity in distinguishing between diversion and crowding, problems in cost computation, and a static view of offender populations and program costs all limit our ability to assess the effectiveness of alternative sanction programs. Until more and better data become available, our knowledge of these programs does not provide a sufficient basis for sound public policy.

¹⁵Williams et al.

However, we have gleaned three findings from our examination of the evaluations. First, it is clear that current programs have had only limited effects on prison populations. This is because of the limited capacity of most alternative sanction programs. To the extent that these programs are the sole alternative to incarceration, they would need to be greatly expanded in size or in number in order to significantly reduce prison crowding and costs. We have also found that even in the case of relatively large programs, other influences can more than offset program effects on prison populations. These influences need to be better understood and incorporated more systematically in future evaluations. Finally, we know that the per capita costs of these programs exceed those of traditional probation and parole but tend to be less than those of prison. However, current evaluations do not provide us with reliable evidence of how these per capita cost differentials affect overall correctional expenditures.

Intermediate Sanctions and Crime Control

The primary function of the criminal justice system is to control criminal behavior, and prisons are typically regarded as the ultimate means for effecting this objective. Therefore, any evaluation of programs designated as alternatives to incarceration should incorporate an assessment of how effective they are in protecting the public from crime. In this appendix, we examine what is known from existing studies of the effectiveness of intermediate sanction programs in preventing criminal behavior. Specifically, our interest lies in determining how the rates at which offenders in intermediate sanction programs commit crimes compare to rates for offenders in other community-based programs (for example, probationers and prison releasees).

Of the 28 evaluations we reviewed, 22 addressed the question of crime control. Of those 22 studies, 15, the majority, were evaluations of intensive supervision programs (either probation or parole). The remaining 7 studies (5 of which dealt with house arrest and 2 with other intermediate sanctions) did not provide sufficient information to be included in our analysis. Therefore, we focused our examination on the effectiveness of ISP at controlling crime. This focus does not significantly limit the implications of our findings in that

- intensive supervision programs are the most prevalent form of intermediate sanction program;
- intensive supervision programs have been in existence longer than other programs, allowing for a firmer basis for determining program effects; and
- the differences between what are called intensive supervision programs and house arrest programs are often minor and of little practical significance.

Of the 15 evaluations that examine the ability of ISP programs to control crime, 6 had to be excluded for lack of a reasonable comparison group, and 1 was excluded because the research design allowed for multiple intervention effects. The remaining 8 evaluations became the basis for our analysis of what is currently known regarding the effectiveness of ISP programs in protecting public safety.

A final point must be made before proceeding. As our analysis will indicate, even the 8 evaluations we analyzed are characterized by often serious methodological and analytical flaws. This precluded any attempt at a quantitative synthesis of study findings.

Evaluations of Intensive Supervision Programs

The 8 evaluations analyzed 8 programs in 6 states. Three of the evaluations examined 3 similar programs in Ohio (1 statewide evaluation, 2 of single counties); another examined 3 somewhat different programs in 3 California jurisdictions; 3 evaluations focused on statewide programs in Georgia, Iowa, and New Jersey (1 evaluation each); and 1 targeted a single program in Madison, Wisconsin. Of the 8 evaluations, all but the California study concluded that the program in question was at least a qualified success in controlling criminal conduct.

However, from our analysis, we conclude that this overwhelming affirmation of the effectiveness of ISP programs at controlling criminal behavior is, at best, premature. We argue that methodological and interpretive issues considerably cloud this optimistic appraisal. Before doing so, however, we briefly describe the 8 evaluations and discuss what can be learned from those studies.

Georgia's Intensive Supervision Program

The Georgia evaluation, along with that of New Jersey, is one of the most widely cited studies of ISP programs. Georgia's program, implemented in 1983, is a multiple-jurisdiction, diversionary program in which offenders enter ISP by way of direct sentence, postsentence screening (and an amended sentence), or probation revocation. The program targets nonviolent felons with a high risk of recidivism.

To analyze the effect of ISP on public safety, the evaluator compared the 1983 ISP population of 542 offenders against a sample of 173 parolees and a sample of 753 probationers. The parole sample was created by having ISP officers review the records of offenders sentenced to prison in 1982 (the year prior to ISP implementation) and then select the cases they would have recommended for ISP had the program been available. The probation comparison group was a random sample of probationers from 1983 sentenced in jurisdictions without an ISP program (so ISP would not have been a sentencing option). Efforts were made to match both comparison groups with ISP offenders on age, race, current offense, and risk-needs scores.¹ These groups were then tracked for the 18 months they were "at risk" (that is, under community supervision). Four measures of recidivism were used: rearrest, reconviction, subsequent jail confinement, and reimprisonment.

¹A risk score is a measure of an offender's propensity to reoffend, while a need score measures an offender's need of special services (for example, substance abuse treatment).

Aggregate recidivism results for the 18-month study are presented in table III.1. As indicated in the table, parolees had the highest rates of failure on all outcome measures, while probationers did the best. ISP rates fell somewhere between the two other groups.

Table III.1: Recidivism Rates for Georgia's ISP Program

	Rearrest	Reconviction	Jailed	Imprisoned
Regular probation	36%	24%	12%	13%
Intensive probation	40	24	20	17
Parolees	58	42	26	18

When the data were analyzed by offender risk level (low, medium, high) the same results were found. Once again, parolees had the highest rates of recidivism, while regular probationers did as well as or better than ISP on virtually every outcome measure. Thus, ISP recidivism was consistently below that of persons under parole supervision but equal to or only slightly worse than offenders on regular probation. In addition, ISP participants committed fewer new violent or serious property offenses than either of the two comparison groups. Of the ISP offenders who failed to complete the program, 7.7 percent were revoked for committing a new crime, 6.5 percent were revoked for a technical violation, and 1.8 percent fled the program.

Because ISP offenders fared better than parolees and not appreciably worse than probationers on overall failure rates (all matched on risk levels), the evaluator concluded that Georgia's ISP program is a success. According to the evaluator, it was the enhanced levels of surveillance and treatment of the ISP program that kept the recidivism rates of program participants somewhere between those of equally high-risk parolees and probationers.

New Jersey's Intensive Supervision Program

The New Jersey program is also a multiple-jurisdiction ISP. Offenders sentenced to prison are released into ISP after spending a minimum of 60 days in prison (the actual median time was 3.6 months), applying for entry into the program, and undergoing a rigorous seven-stage review process. The program targets "typical" felons (50 percent scored "low risk" on New Jersey's classification scale) who have not been convicted of a violent offense, and it emphasizes both intensified treatment and surveillance.

The evaluation compared 553 ISP participants to a random sample of 500 parolees sentenced to prison approximately 2 years before the implementation of ISP (this was to allow time for prisoners to be released on parole). Both cohorts were tracked over a 2-year period to examine recidivism rates. Recidivism was defined as an arrest or conviction for any new crime or conviction for a new felony offense. Because there were significant differences between program participants and the comparison group on such important factors as criminal history and offense of conviction, statistical procedures were used to generate a smaller sample of 132 parolees that more closely matched the ISP sample. Analyses were undertaken using both the "full" and the "close" comparison groups. In the former analyses, variables known to differentiate the two groups were controlled statistically.

The follow-up analysis found that ISP offenders did better on all measures of recidivism than regular parolees. At the end of 2 years, 74 percent of ISP offenders were arrest-free compared to 65 percent of the parolees. And parolees failed earlier than ISP offenders (19 percent versus 5 percent of ISP offenders failed in the first 6 months; 26 percent versus 11 percent of ISP parolees failed within 1 year). Reconviction rates were 12 percent for ISP versus 23 percent for parolees; and, again, parolees failed earlier. Rates for felony reconviction were 8 percent for ISP and 14 percent for parolees. Parolees failed sooner than their counterparts here as well. In short, the parolee sample failed at approximately two times the ISP rate on every measure of recidivism except rearrest.

The second analysis compared ISP offenders with the "full" parole sample, this time controlling for offender risk level. This analysis showed that differences in recidivism between the two groups persisted at all risk levels (low, medium, and high) and, with few exceptions (particularly felony conviction), at approximately the same magnitude as with the "close" parole sample. In other words, program outcomes were not significantly influenced by differences in offender risk levels at program entry.

On the basis of a finding that ISP offenders consistently failed at a lower rate than parolees, the evaluator concluded that New Jersey's ISP program was a success. According to the evaluator, it was the enhanced surveillance and treatment provided by ISP that prevented a higher rate of recidivism and postponed the time to recidivism among those who did commit new crimes.

Iowa's Intensive Supervision Program

Iowa's ISP program began in 1985 and now supervises high-risk, violent probationers and parolees in four sites across the state. (Because of methodological problems in the analysis of ISP parolees, only the results of ISP probation are reported below.) Iowa's ISP program also emphasizes enhanced treatment programs as well as intensive surveillance. Entry into the program is through direct sentence or referral of offenders deemed likely to be revoked from regular probation or from community treatment facilities. Unlike many programs that emphasize swift revocation for infractions, Iowa's ISP has a highly structured, progressive disciplinary system.

Data were gathered on 101 probationers over the 21 months following program inception. The ISP offenders were compared to a random sample of 269 probationers in districts without an ISP program. Although there were considerable differences between the two groups along several dimensions (largely because the ISP program targets "hardened" offenders), they were comparable on Iowa's risk-needs classification scale. Revocation rates of the two groups for new crimes and technical violations were compared over the 21-month period.

The evaluation reported little difference between the two groups on overall failure rates: 40 percent of the ISP probationers were revoked over the 21-month period; 38 percent of regular probationers were revoked. However, twice as many regular probationers were revoked for a new crime (24 percent versus 12 percent for ISP), while ISP probationers were revoked at almost three times the rate of regular probationers for technical violations (17 percent for ISP versus 6 percent for regular probationers). Yet, of those revoked for a new crime, ISP probationers were more likely to be revoked for a violent crime (17 percent versus 9 percent), while regular probationers were more likely to be revoked for a drug offense (35 percent versus none for ISP).

From these findings, the evaluator concluded that ISP was a qualified success. ISP offenders committed fewer new crimes but committed more-serious offenses when they did offend again. The evaluator attributed the finding of lower crime rates among ISP offenders to their higher rate of revocations for technical infractions. Through closer surveillance, ISP officers could detect problem offenders and remove them from the community on technical violations before they committed new crimes.

**The Madison, Wisconsin,
High-Risk Offender
Intensive Supervision
Project**

The program in Madison, Wisconsin, is designed to provide enhanced surveillance and control of high-risk offenders. Minimum eligibility criteria are that the offender score "high-risk, maximum supervision" on Wisconsin's case classification scale and have a documented history of assaultive behavior. Other characteristics sought at screening include poor prison conduct, poor attitude toward supervision, and an unwillingness to participate in treatment programs. In other words, offenders selected for participation in the ISP program are typically "hardened" criminals.

The evaluation compared all 64 parolees released into ISP during a 1-year period to a random sample of 56 parolees released to conventional maximum supervision who met the same minimum eligibility requirements as the ISP offenders. The ISP group entered the program in 1985 and 1986, whereas the other sample was drawn from offenders released from prison in 1983. The two groups matched closely on most personal and legal variables. The evaluation followed the offenders for a 1-year period after their release from prison; recidivism was defined as "return to prison."

In the aggregate, the two groups were found to have similar recidivism rates (45 percent for ISP and 41 percent for the comparison group). However, only 5 percent of ISP offenders were returned to prison for a new criminal conviction, whereas 29 percent of the control group were returned for a new crime. The most frequent reason for ISP returns was a technical violation (40 percent of ISP); only 12 percent of the control group were returned for a technical violation. And of the ISP offenders returned for technical violations, over half (14 of 26) were for alcohol or drug violations. This was true for only 2 of 7 in the comparison group. In addition, the comparison group was more likely to have been convicted for a violent offense than ISP participants (12 percent versus 3 percent). However, ISP offenders failed much more quickly than the controls (a third of ISP offenders failed in the first 6 months versus 18 percent for the comparison group).

The evaluator concluded that the program is a success in that the close supervision provided by intensive supervision allowed ISP officers to revoke offenders before they could commit new or more serious crimes, thus enhancing public safety relative to conventional parole supervision. The evaluator noted, however, that this comes at a price: ISP costs two to three times more to administer than regular maximum supervision parole.

Ohio's Intensive Supervision Program

We included three evaluations of Ohio's ISP program. The first Ohio study is an evaluation by the National Council on Crime and Delinquency that examined offenders in three community corrections programs (discussed in appendix II). All three programs are designed as front-end diversion programs; the principal difference among them is the degree of supervision and control imposed on the participants.

The lowest level of supervision is provided by the Community Corrections Act program, which is designed to augment regular probation supervision. The highest level of community supervision is the Community Based Corrections Facilities program, which combines a brief period of incarceration with therapeutic and community services. The middle level supervisory program, Intensive Diversion Units, is an intensive supervision program and, therefore, the one of principal interest here.² Most placements occur after the offender has been sentenced to prison and target nonviolent felons.

The study compares 423 ISP offenders with 471 regular probationers and 568 prison releasees for their first 2 years in the community. For the two probation groups, this was measured from time of program entry; for the prison group, from the time they were released from prison. Recidivism was defined as arrest for a new crime.

The general finding was that recidivism increased with the level of supervision and control imposed by the program in which offenders were initially placed. At the low end was regular probation (26 percent rearrests), with parolees having the highest rate of recidivism (54 percent). ISP offenders had a 44-percent recidivism rate. The overall recidivism rate for the three community correction programs was 35 percent.

However, offender profiles were quite different across the programs. This was particularly important with respect to the risk level of program participants. The percentage of high-risk offenders in the respective programs was 11 percent for regular probation, 26 percent for the lowest level of community control supervision, 37 percent for ISP, and 59 percent for the highest level of community supervision. Data inadequacies prohibited application of the risk assessment instrument to the prison sample, thus reducing the sample for risk analysis to 1,699. Once risk level was controlled for, there was little difference in recidivism rates across the groups. Low-risk offenders failed at approximately 16

²Because Ohio's Intensive Diversion Unit program is simply another designation for an ISP program, we refer to it as an ISP program.

percent, medium-risk offenders at 31 percent, and high-risk offenders at 54 percent. In short, it was offenders' risk levels at program entry, not the programs themselves, that appeared to be the primary determinant of recidivism rates.

The evaluators conclude that Ohio's ISP program has been effective in protecting public safety because, in general, ISP offenders were found to have recidivism rates lower than those of parolees, even if higher than that of probationers. As with the evaluation of Georgia's program, this conclusion is grounded in the view that what are acceptable levels of crime commission will vary according to the level of risk posed by program participants. Since ISP programs generally target offenders deemed too risky for regular supervision but not risky enough for prison, we should call an ISP program successful if recidivism rates for its offenders fall somewhere between those of probationers and parolees.

The Lucas County, Ohio, Intensive Supervision Program

A second Ohio evaluation studied a single-site program in Lucas County that had supervised approximately 470 probationers in the 8 years since its inception in 1978. The program is designed as a diversionary program but excludes "extremely dangerous" offenders. Offenders sentenced to prison are screened and then, if it is deemed appropriate, recommended for ISP supervision. The evaluation compared 372 ISP offenders with 376 regular probationers from the same probation department who had not been placed on ISP. The two groups were matched on race, sex, and risk level and tracked over the full 8-year period. Differences between the two groups were controlled statistically. Recidivism was defined as arrest for a new crime, reconviction, reincarceration, and technical violations. Severity of new offense was also considered.

For the 8-year period studied, only 30 percent of ISP offenders had successfully completed the program compared with 43 percent of the comparison group. Moreover, as shown in table III.2, ISP participants had more misdemeanor arrests, more misdemeanor convictions, and more felony convictions than regular probationers.

Table III.2: Recidivism Rates for the Lucas County, Ohio, ISP Program

	Intensive probation	Regular probation
Misdemeanor arrest ^a	51%	35%
Misdemeanor conviction ^a	40	23
Felony arrest	37	31
Felony conviction ^a	28	22
Technical violation	41	38

^aIndicates a statistically significant difference.

A second analysis was undertaken for the last 20 months of the evaluation period, since this represented the most current information on program performance. The findings for the 20-month period were similar to those for the 8-year period, except that in the shorter period, the two groups did not differ on felony convictions. In addition, ISP probationers were convicted of more serious offenses than regular probationers for both the shorter and the longer evaluation periods.

From these findings, the evaluator concluded that this ISP program is operating at “an acceptable level of public safety.” That is, a somewhat higher failure rate for ISP offenders than for regularly supervised probationers is acceptable, since the ISP program supervises more difficult cases.

The Montgomery County, Ohio, Intensive Supervision Program

The third Ohio evaluation examined a program in Montgomery County very similar to that in Lucas County. The principal difference between the two was that the Montgomery County program was not implemented until 1984. Consequently, the evaluation period was only 16 months. With the exception of a shorter evaluation period and, thus, fewer supervised offenders (163 ISP offenders and 130 regular probationers), most aspects of this evaluation were equivalent to the preceding evaluation. (One of the principal evaluators in this project was also the principal evaluator in the previous project.)

However, the findings of the two evaluations were quite different. As table III.3 indicates, in the Montgomery County evaluation, no statistically significant differences were found between ISP and regular probationers on any of the outcome measures. Approximately 22 percent of each group had a misdemeanor arrest, 14 percent a misdemeanor conviction, 15 percent a felony arrest, 10 percent a felony conviction, and 22 percent a technical violation.

Table III.3: Recidivism Rates for the Montgomery County, Ohio, ISP Program

	Intensive probation	Regular probation
Misdemeanor arrest	22%	21%
Misdemeanor conviction	15	13
Felony arrest	15	14
Felony conviction	12	8
Technical violation	21	22

From these findings, the evaluators judged this ISP program to be a qualified success. While the evaluators concluded the program is “on the right track” and “holding its own with regard to community safety,” they noted that 16 months might be insufficient to make any pronouncements about program success.

Intensive Supervision Programs in Three California Counties

The California evaluation examined three slightly different probation enhancement programs in Contra Costa, Ventura, and Los Angeles counties. All three programs are modeled on Georgia’s ISP program but differ in the following ways: the Ventura program is related to a program already in existence for 11 years and targets high-risk probationers who committed a serious current offense; the Contra Costa program targets drug offenders; the Los Angeles program also targets high-risk offenders but provides for electronic monitoring of some ISP probationers.

Evaluation of all three sites involved an experimental design with random assignment of offenders to the ISP and comparison groups. In Ventura, the comparison group comprised offenders in the existing program similar to ISP but with less-intense supervision. In Los Angeles and Contra Costa counties, offenders assigned to ISP were compared with offenders assigned to regular probation. In Los Angeles, regular ISP probationers were also compared with ISP probationers on electronic monitoring. Recidivism was defined as a new arrest, type of arrest (drug, property, or violent offenses), and technical violations. Findings were reported for the first 6 months of the evaluation period and were characterized as preliminary.

As table III.4 shows, ISP probationers typically did either no better or worse than regular probationers at all three sites. As indicated in the table, the ISP probationers did worse on both rearrests and technical violations in Los Angeles and Contra Costa counties. In Los Angeles, ISP probationers on electronic monitoring failed at a higher rate than either

ISP or regular probationers. However, in neither site were the differences in rearrest statistically significant (the differences, in other words, could have occurred by chance alone). In Ventura, ISP probationers did slightly better than regular probationers on rearrests but, again, the difference was not significant. There was no difference between the two groups on technical violations. Nor was any relationship found between type of program and type of new offense. (However, fewer than 6 percent of any group were arrested for a violent crime; drug and other minor offenses accounted for over 50 percent of all new violations.)

Table III.4: Recidivism Rates for Three California ISP Programs

Program	Rearrest	Technical violation
Contra Costa		
ISP	20%	33% ^a
Non-ISP	13	14
Los Angeles		
ISP	27	29 ^a
Non-ISP	20	9
ISP-electronic monitor	33	35 ^a
Non-ISP	20	20
Ventura		
ISP	26	32
Non-ISP	30	30

^aIndicates a statistically significant difference.

The evaluators concluded from their findings that California's ISP programs were not working. Their reasoning was that, since ISP programs devote substantial resources to the control and treatment of probationers, ISP recidivism rates (especially with respect to new crimes) should be lower than those of regular probationers. However, the evaluators went on to note that their findings were based on only a 6-month evaluation period and that their results may change as their evaluation continues.

Analysis of the Evaluations

As previously stated, the 8 evaluations we reviewed suffered from problems that limit their findings and conclusions. These problems fall in three broad categories: measurement of outcomes, establishing program effect, and interpretation of results.

Problems in Outcome Measures

As can be seen from the 8 evaluations discussed above, program success or failure is determined largely by what happens to the offenders in the program. That is, are they rearrested, do they violate the terms of probation, or do they complete their time in the program without further incident? The measures used to answer these questions (for example, number of arrests, technical violations, imprisonments) are referred to as “outcome” measures and are subject to two types of problem. One is the validity of the measures and the other is their reliability.

Measurement Validity

Validity refers to the precision with which a variable measures the event it was intended to measure. With respect to the commission of crime, it is well established that official measures of crime underestimate the true incidence of criminal behavior. This is clear in that crime is an activity people routinely attempt to conceal. Consequently, any measure of criminality is, at best, only an estimate of true crime commission. For example, arrest as an outcome measure will alert us only to the crimes for which an offender is a suspect. Similarly, the measure “seriousness of convicted offense” might be influenced by such factors as prosecutors’ plea negotiation practices and court case loads and, thereby, misrepresent the true seriousness of the offense.

Measurement Reliability

Of equal, if not greater, importance in program evaluation is the reliability of a measure. That is, whatever the bias in a measure, we need to know whether that bias is the same for all groups being compared. If it is not, estimates of crime commission need to be adjusted to account for differential bias before sound comparisons can be made across groups.

In comparisons of recidivism rates of offenders in ISP programs with those in other community programs, it is reasonable to assume that such a differential does exist. One reason is the relationship between measures of recidivism and the level of supervision given to an offender. A common feature of ISP programs, for instance, is closer surveillance than that afforded offenders in standard probation or parole. This enhances the likelihood that crimes committed by ISP offenders will be detected and, thus, “counted” in the study.

Another measure frequently employed in studies of ISP programs is imprisonment or reimprisonment. However, as some studies (such as Georgia) have noted, it may be the policy of ISP officers or judges to deal more harshly with ISP offenders who commit new crimes because they feel the offender has already been given a “second chance.” In either case, adjustments in the estimates of actual crime commission and

system response to violations are required. However, none of the evaluations attempted such an adjustment.

Problems in Establishing and Ascribing Program Effect

In order to establish program effect, it is necessary to first determine that an observed outcome is unlikely to have occurred by chance alone. However, many of the evaluations employ simple percentage comparisons or tabular analyses without the use of tests of statistical significance. It is not meaningful to know that two offender groups differed by 2 or 5 or 10 percentage points if these differences could have occurred by chance. Without statistical tests for random occurrences, any conclusions regarding ISP effects are tenuous at best. The increased use of standard statistical techniques would add considerably to our knowledge of the effect of intensive supervision programs.

Once a difference is established, the task of the evaluator is to ascribe the observed outcomes to the effects of a program. To do so, it is necessary to eliminate the possibility that the outcomes could have resulted from other factors. This requires not only the use of comparison groups but also that the groups being compared are matched on other traits known to be associated with criminal behavior. For example, the research literature shows that crime commission is more likely among younger than older persons. If ISP probationers recidivate more frequently than regular probationers, but they are also younger on average than regular probationers, the results could come as well from age differences as from the program.

Constructing equivalent comparison groups has proven a major obstacle to program evaluation in the study of intermediate sanctions. The problem with nonequivalent comparison groups results primarily from the fact that intensive supervision programs do attempt to target a population of offenders different from the "typical" probationer or parolee. Some studies have attempted to overcome this problem through a direct matching of comparison groups with ISP offenders on selected characteristics or the replication of the ISP screening process (such as Wisconsin and Georgia), while others (such as New Jersey) have sought to deal with this problem through the use of statistical controls. Without randomized experimental designs, such as that attempted in California, evaluators will continue to be plagued by the problem of noncomparability in comparison groups.

Problems in Interpreting the Results

To this point, we have noted problems with the measures used, the analyses, and the ascription of cause. Aside from these methodological problems, we found that problems exist in the way conclusions are drawn as to whether ISP programs “work”—that is, whether they protect the public. These problems fall into four categories: determining proximate cause of effect, persistence of effect, standards of success, and generalizability of findings.

Proximate Cause of Effect

There is considerable variation in the components included in any single intensive supervision program. For example, some programs may include mandatory substance abuse treatment or “team” supervision of offenders while others do not. In light of this variation, there is a problem in deciding what it is about the program that contributes to the outcomes observed. That is, if a program appears to be effective in controlling crime, is this the result of the program as a whole or only one component? Most studies examine the relationship between the use of technical violations and program outcomes, and some have analyzed the relationship between program outcomes and the number of ISP contacts. But this may not be sufficient. Few evaluations have systematically explored the possibility of interactions between a range of program components and program outcomes.

Persistence of Effect

A second interpretive problem concerns the relatively short follow-up periods employed in the studies. Only one of the evaluations (Ohio) had a follow-up period of over 2 years. In some cases, this means that recidivism measures are being evaluated while only a portion of the subjects are at risk for the full period of the study; for instance, only 53 percent of Georgia parolees were tracked for the full 18-month evaluation period. If we had a 5-year follow-up of Wisconsin’s ISP program, for instance, would we still find a positive ISP effect? Or would we find, as in Ohio, that there was little difference in the longer-term recidivism rates between offender groups? Thus, even when a program seems to have a positive effect, we have little knowledge regarding the persistence of this effect.

Standards for Judging Program Success

A third interpretive problem is the lack of a clear standard for determining program success. Three evaluations (Georgia, New Jersey, and one in Ohio) based their positive evaluations of ISP programs on the finding that ISP participants exhibited lower recidivism rates than parolees, even though in two of those studies (Ohio and Georgia) it was found that intensively supervised offenders had higher recidivism rates than regular probationers. The underlying logic of such a conclusion is

that intermediate rates of recidivism are appropriate for an intermediate sanction program because program participants represent an intermediate level of risk. But is this a reasonable interpretation of the data?

Consider an alternative standard for evaluating program success, such as that employed in the California evaluation. ISP programs promise a community-based alternative to incarceration for medium- to high-risk offenders that will not diminish public safety and commit considerable resources to reach that goal. It can then reasonably be argued that ISP programs should, at minimum, keep recidivism rates at a level comparable to that of regular probation or parole. If we control for offender risk level at program entry, the commitment of additional resources should lead us to anticipate that ISP offenders will have a lower rate of recidivism than those under regular supervision. Using this standard, we should conclude that the ISP parole programs in Wisconsin and New Jersey (the only two to compare intensive parole with regular parole) are a success. When applied to intensive probation, only the program in Iowa could legitimately be called a success.

Without some generally accepted standard for assessing the effectiveness of intermediate sanctions, claims of success or failure hinge on the subjective judgment of evaluators and others. The evaluative ramifications are ambiguous at best: If we accept the judgment of evaluators we reviewed, the vote is overwhelmingly positive for ISP programs; if we use the standard imposed in California, less than half (3 of 8) would be viewed as a success. What this means for policy is even less clear.

Cross-Program Generalizations

A final interpretive problem concerns our ability to reach judgments about ISP programs in general from particular studies. Our inability to generalize from current studies to a broader range of ISP programs has two components.

The first problem is related to an earlier problem regarding the reliability of measures used in ISP studies. If we must question the reliability of measures in individual studies, we must also exercise considerable caution when attempting to compare findings across studies. This problem is compounded by the fact that while some studies have used similar outcome measures (for example, New Jersey and Georgia), others (for example, Wisconsin) employed quite different measures. Although it is generally a good idea to examine different measures of recidivism within studies, comparing across studies that measure recidivism in different ways can only prove misleading.

A second problem in making generalized statements about ISP programs concerns the programs themselves. As our data show, ISP programs in different jurisdictions have been designed to serve different ends, target different offender populations, are at different levels of program implementation, and stress different program components. As a result, we are limited in our ability to compare recidivism rates across programs. This problem is compounded by the fact that even programs with similar structures and policies may differ in actual operation. This greatly limits our ability to determine if different program outcomes are the result of differing program goals and organization or differences in program operation. Both sets of factors undermine our ability to generalize about intermediate sanction programs from the existing research.

What Can Be Learned From the Evaluations

Despite the problems discussed above, we can learn some things from the evaluations. First and foremost, we know that some participants in ISP programs continue to engage in criminal activity. Depending on the measure used, the evaluations report that between 5 and 44 percent of all felons in ISP programs commit new crimes. Consequently, these programs do expose the public to some level of risk.³

Offenders placed in prison obviously pose no threat to public safety, at least for the period of their imprisonment. Judged by a standard of zero risk, all ISP programs fail to protect public safety. But the premise of ISP programs is that they provide an intermediate sanction that, among other things, does not increase threats to public safety. This premise requires us to compare the recidivism rates of ISP programs to those of other community-based programs.

Our second finding takes on meaning within the context above. The evaluations provide evidence that ISP recidivism rates tend to fall somewhere between those of probationers and parolees.⁴ The interpretation of this finding is difficult because the closer supervision of ISP offenders makes it more likely that criminal activity will be detected. Thus, when compared to that for probationers, the higher recidivism rate for ISP offenders may be at least a partial artifact of supervision practices.

³As previously indicated, all measures of crime are estimates and, therefore, the true rate of criminality is unknown.

⁴In only one evaluation (Iowa) did ISP offenders do better than regular probationers with respect to crime commission. In the rest of the evaluations, ISP offenders did either worse or no better than probationers.

However, what is significant is that ISP offenders consistently failed less often than parolees. Of the four evaluations that examined failure rates between ISP offenders (probation or parole) and regular parolees, ISP participants fared better than parolees on all measures of recidivism except one. In Georgia, parolees did as well as ISP participants in reimprisonment rates. That exception aside, parolees failed at a rate 9 to 22 percentage points higher than ISP offenders. And these relative rates generally held true when controlling for offender risk level at program entry.

Our third finding concerns the extremely high rate at which non-ISP parolees commit new offenses subsequent to their release. In New Jersey, for instance, over one third of the parolees were arrested for a new crime within 2 years of their release from prison. In Georgia and Ohio, parolee rearrest rates for a similar time period were well over 50 percent. These rearrest rates are between 23 and 45 percent higher than those for ISP offenders. At a time when pressures to expand prison capacity continue to mount, it is important to recognize that many offenders are not deterred by prison from continuing to engage in criminal behavior. And they continue to do so at a rate well above that of either traditional probation or intensive supervision programs.

Summary

One promise of intensive supervision programs is that offenders who otherwise would be imprisoned can instead be supervised within the community without jeopardizing public safety. However, as we have shown in this report, the methodological problems with extant evaluations prohibit any definitive determination of the effectiveness of ISP programs in maintaining public safety.

From the studies we reviewed, we are able to reach general conclusions: ISP offenders do commit new crimes, they do so at a rate generally higher than that of offenders in traditional probation and lower than that of parolees, and the latter group (offenders sent to and released from prison to parole) commit new offenses at a high rate. The implications of these general findings are not easy to deal with. Society seems less and less willing to place criminals in programs where the extent of supervision is a monthly phone call. At the same time, it seems that even intensively supervising offenders in community-based programs does not eliminate all danger to the community. Finally, incarcerating offenders does not seem to diminish their propensity to engage in criminal activity after release.

Faced with this dilemma (offenders will continue to commit some crimes if we do not incarcerate them and they may eventually commit more crimes if we do incarcerate them), the society needs more precise information on intermediate sanctions. Specifically, we need to know exactly what type of offender succeeds or fails in exactly what type of program. Evaluations of ISP programs have the potential to provide such information but will have to overcome a number of obstacles in order to do so. These obstacles fall into the three broad categories of measurement, comparison, and time period.

Measurement

Most offenders go to great lengths to conceal their criminal activities. Consequently, all studies that require data on crime commission, including evaluations of ISP programs, must rely on indicators of criminal activity (for example, number of arrests or number of convictions). The point for the evaluations we reviewed, as well as for all other studies of criminal behavior, is that the relationship between the indicator and the “true” level of criminal activity is not known.

One approach to circumventing this problem has been to try to calibrate “official” measures of crime (for example, arrests) with “unofficial” measures of crime (for example, victimization surveys or offender self-reports). The logic of the calibration approach is that multiple measures provide a more accurate estimate of the true level of criminality than any single measure. The objective is to use the difficult and expensive process of obtaining unofficial measures to estimate the accuracy of the more readily available official measures. None of the evaluations we reviewed reported any attempt to calibrate the measures of crime commission that they employed. As a result, we have no clear sense of how accurate the indicators used in the evaluations are in measuring actual crime commission.

Comparison

Sentencing decisions are made in a comparative context (that is, how well option A compares with option B). In light of this, it is critical for evaluations to make the right comparisons to reach conclusions. In the field of intermediate sanctions, this requirement translates into the need for appropriate comparison groups, yet a central problem in evaluations of ISP programs has been the general inability to create comparison groups that closely match ISP offenders on characteristics known to be associated with crime commission. For example, only one of the evaluations we examined used the random assignment of offenders to the program and comparison groups. The rest used various other techniques to

try to retrospectively create an alternative group of offenders against which program participants could be compared. Rarely were these efforts successful. Until the evaluative research is able to produce equivalent comparison groups, our knowledge of the effectiveness of ISP programs in controlling crime will be greatly limited.

Another problem with comparisons in the evaluations stems not from the comparisons made but, rather, from how they are interpreted. The problem arises because evaluations of ISP programs have not yet established standards for determining success or failure. The absence of such standards has resulted in a conflicting and seemingly arbitrary assortment of conclusions regarding program effectiveness. Similar findings (for example, no difference in recidivism rates) have led to dissimilar conclusions (success and failure), while dissimilar findings (no difference and differences in recidivism rates) have led to similar conclusions (program effectiveness). Clearly, reasoned expectations and standards regarding program effectiveness are needed before we can determine whether ISP programs meet those expectations.

Time Period

Whether they are examining the issue of crowding, costs, or crime commission, most evaluations have suffered from what is likely to be an overly short time period. To some extent, this is a function of the relatively short time that most intermediate sanction programs have been in existence. However, the consequences of failing to allow for longer studies of these programs are considerable. For one thing, the effect of a mature program may be very different from that of a program that is just getting started. In addition, as we demonstrate in appendix II, the true costs of the program can change dramatically over time, and any static view of those costs can be misleading. Finally, the behavior of offenders during the initial months of the program, although important, may serve as a poor estimate for their behavior over extended periods of time. For these reasons alone it is critical that evaluations of intermediate sanctions begin to extend the timeframe over which these programs are examined.

Major Contributors to This Report

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