# DO DRUG COURTS WORK? GETTING INSIDE THE DRUG COURT BLACK BOX

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*This article argues that evaluation of drug courts will benefit not only from an organizing typology (Goldkamp, 1999a, 2000) that focuses research on the critical structural elements of the drug court model but also from an understanding of how drug courts are thought to deliver their impact. In developing a causal model of drug court impact, the analysis separates assessment of impact into two investigations: whether drug courts "work" and how they work. Data from the ongoing NIJ-supported evaluation of the Portland and Las Vegas drug courts are analyzed to answer the comparative question of whether there is an impact (and of what sort) and then to move consideration of the internal elements of the drug court (the black box of drug court treatment) through the development of successive theoretical models. The illustrative analyses guided by these models consider the relative contributions of instrumental drug court treatment functions and defendant risk attributes, which contribute importantly to drug court outcomes. The exploratory findings differ by site, but show some support for the importance of treatment, sanctions and appearances before the drug court judge—and their interaction—in lowering the prospects for subsequent rearrest and increasing likelihood of graduation.*

## THE CHALLENGES OF MEASURING THE IMPACT OF THE DRUG COURT PHENOMENON

The effect of the drug court movement on courts and the justice system over the last 11 years may turn out—with more historical distance—to have been one of the major justice reforms of the last part of the 20th century in the United States. Its impact on treatment systems that have traditionally failed to address the needs of criminal justice populations may also prove to have been powerful, as the movement compelled them to reconfigure and redirect resources to the most challenging populations of substance-involved citizens. Since the pioneering efforts of Miami

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justice officials establishing the nation's first drug court in the summer of 1989 (Goldkamp, 1994a, 1994b), the growth of the drug court movement—with an estimated 600 courts reportedly now in operation in the United States alone—has been extraordinary by any measure.

To the traditional criminal court and drug treatment practices that drug courts were designed to improve upon, the substance of the drug court model of court innovation represents a paradigm shift away from a predominantly punitive orientation to one that focuses on treatment, investment in human potential, second (and third) chances, and restoration. Although there are common elements shared by most drug courts based on a more informal, judge-oriented treatment-oriented methodology,[1] proliferation of the drug court model is not explained by the wholesale adoption of a fixed, "cookie-cutter" approach. Predictably, the original Miami model evolved in its successive applications to other settings. It was itself transformed in substance and procedure as the basic model traveled across the United States and to locations abroad. The drug court methodology has been adapted to grapple with other problems associated with criminal court populations, including community issues, domestic violence, and mental health. The substance of the drug court movement has directly and indirectly spawned a variety of related innovations, so that one can now speak of "problem-solving" or "problem-oriented" courts[2] to refer to a more active, "hands-on," judicial and justice-system philosophy.

The challenges for research in gauging the full impact of drug courts on the philosophy of justice, the operations of the justice system, and the function of the criminal and civil courts, not to mention the health and behavior of addicted criminal offenders, are simply huge. In earlier discussions, we have argued that evaluation of drug courts should be tied to a clear understanding of their goals (Goldkamp, 1995, 1999a) and that assessment of their impact can best be understood through a conceptual framework—a working typology—that identifies key ingredients thought to be responsible for their advertised results (Goldkamp, 1995, 1999a, 2000; Goldkamp, White, & Robinson, 2000a). These elements include 1) target problem, 2) target population, 3) screening—reaching the target, 4) modification/adaptation of court processing and procedures, 5) structure and content of treatment, 6) responses to performance in treatment—participant accountability, 7) productivity of the drug court, and 8) extent of system-wide support. Recognizing their diversity, we have argued that meaningful assessment of impact must be guided by a clear understanding of what drug courts "are" and "what they are not" (Goldkamp, 1999a).

Without such a framework to isolate the critical instrumental elements of the approach, findings from scattered evaluations will accumulate like apples and oranges and other ingredients for a mixed fruit salad of research. The result is that the practice-oriented consumer of the research is left—like Belenko (1998, 2000)—to weed through diverse findings from disparate studies to identify directions or themes relating to drug court effectiveness.[3] With these challenges in mind, we organized our recent research (Goldkamp et al., 2000a) studying the evolution and impact of two of the earliest and longest-operating drug courts in Portland, Oregon,

and Las Vegas, Nevada, according to the elements of the drug court typology. In this fashion, we hoped to produce findings with both a high degree of internal validity, as we seek to know what makes a particular drug court work, and sufficient external validity, as we seek ambitiously to test the effects of the structural elements of the innovation across settings.

## BACK TO BASICS: THINKING ABOUT THE IMPACT OF DRUG COURTS

The need to sort out the effects of the critical operating components of the drug court model in widely different settings around the nation (and abroad) and the contextual dynamics of growth and change make the evaluation research task complex. Its apparent complexity, however, should not serve as a distraction from the need to answer some very basic questions, such as "Do drug courts work?" and "If so, how?" In this article, we consider the implications of these questions—and of assumptions posited by the drug court model—by drawing on selected findings from the first phase of our NIJ research focusing on the Portland and Las Vegas drug courts. As the body of research grows and the collection of findings accumulates, the need to address some of the most basic questions about the impact of drug courts has become more critical, even while the task has become increasingly murky. This discussion seeks to move back to the basics of examining the impact of the drug court innovation by considering its key theoretical assumptions, what we know about the impact of drug courts, how we know it, and some of the difficulties associated with the task of "really" knowing what about the drug court model "works," if indeed "it" does.

### WHAT IS A DRUG COURT?

The problem of answering the drug-court effectiveness question must begin with an understanding of what a drug court is. We have described its basic elements as involving a new working relationship between the criminal court and health and treatment systems carried out within the boundaries of the criminal court's jurisdiction (Goldkamp, 1994a, 1994b, 1999a). In comparison with methods previously in existence in the justice system, the aims of the drug court model are much less punitive and more healing and restorative in nature. Its unorthodox and, in historical context, iconoclastic methods have been characterized as an informal operation of the courtroom, where direct exchanges between the participant and the judge are common and between counsel for the state and the defense are non-adversarial. The courtroom was conceived as a therapeutic vehicle (a theatre in the "square") with the judge at the center leading the treatment process. Under this model, it was widely believed that the role of the judge, with its symbolism and authority, would serve to galvanize the treatment process into a more powerful and accountable form of rehabilitation than previously (or recently) available in the criminal justice setting. The drug court model's emphasis is not on the disposition of the criminal case, but instead on the treatment of drug-addicted offenders whose cases, when treatment was successful, could often be dismissed. (See Goldkamp,

1999a, 2000; Hora, Schma, & Rosenthal, 1999.) These "key components" of the drug court model are well laid out in a monograph sponsored by the Drug Court Program Office of the U.S. Department of Justice (NADCP, 1997) and described in a host of brochures produced by the National Association of Drug Court Professionals. With this brief sketch of the ingredients of the drug court model as a point of reference—and discussions of its historical role in stimulating a broad array of problem-solving courts set aside—we now turn to the problems of measuring its effectiveness.

## STUDY DESIGN: RETROSPECTIVE EVALUATION OF THE PORTLAND AND LAS VEGAS DRUG COURTS

We illustrate the issues raised by evaluation of the drug court model with the example of our recent research assessing the impact of two of the nation's longest operating drug courts over time (Goldkamp et al., 2000a).[4] The retrospective evaluation of the drug courts in Las Vegas (Clark County) and Portland (Multnomah County) was organized in several principal components. Descriptive data were collected to help understand the development and evolution of each of the courts over time through observations of the court and treatment processes, interviews with the principal system actors involved in the drug courts, and focus groups with participants. These descriptive data provided an understanding of how the courts grew and changed over time and were employed to identify chronological milestones of important events and challenges in the lives of each drug court. Archival data collection was organized to complement the descriptive data in showing changes in the population of participants, in the workload, and in the assessment of participant outcomes over time.

The sampling strategy was designed to capture the effects of important changes in both drug courts over time (including changes in targeted and enrolled populations) by stratifying on the basis of time periods. To ensure that the sampling design was representative of each time period, approximately equal numbers of cases were randomly drawn in each designated time period for the samples of drug court participants as well as the comparison groups.

*The Portland Design:* A stratified sampling strategy was employed for the evaluation of the Portland drug court (STOP program) in Multnomah County. We randomly sampled 150 drug court participants from three, two-year time periods (1991-92, 1993-94, and 1995-96) and one, one-year time period (1997) from 1991 to 1997. This resulted in about 75 cases from each individual year, with the exception of 1997, from which we sampled 143 defendants (total n=692). The study design employed two comparison groups of drug defendants for each time period. Selected at the point of entry into the judicial process, shortly after arrest, these groups consisted of a) those who attended the Defender orientation and did not enter the drug court process (total n=401) and b) those who did not attend the Defender orientation and did not attend the petition hearing to enter drug court (total n=401). Though less than ideal, this retrospective strategy (adjusted by the use of post hoc controls in comparative analyses of outcomes) was the only reasonable option

available for designating comparison groups in Multnomah County, where all eligible defendants are referred to Defender orientation. For drug court participants and comparison group defendants entering the court process from 1991 through 1994, the criminal justice outcomes follow-up covered one-, two- and three-year periods. For the 1995-1996 cases, one and two-year follow-up periods were employed. For those entering the processing in 1997, the follow-up period was one year.

*The Las Vegas Drug Court Design:* Our sampling approach in Clark County, designed to represent cases from 1993 through 1997,[6] was stratified by year. For each of the five years, we randomly sampled about 100 drug court participants (total n=499) and 100 comparison group defendants entering the judicial process at the District Court arraignment stage (total n=510). The comparison group defendants were identified from overall entering felony drug cases and included mainly defendants who were not made aware of the drug court option and whose cases were processed in the normal manner. Thus, they were similar to drug court defendants who entered the process and who did pursue the drug court path. The design incorporated one-, two-, and three-year follow-up periods marked from the point of entry in the judicial process (not from date of termination from the program) for 1993, 1994, and 1995 defendants and one- and two-year follow-up periods for 1996 and 1997 defendants.

## ASSESSING THE IMPACT OF THE DRUG COURT INNOVATION: DOES IT WORK?

When public officials ask "Does it work?,"[7] their question implies a comparison: "Compared to how the judicial system was doing without a drug court, is the addition of drug court an improvement?" Implicitly, the "does-it-work" question involves at least three basic considerations: 1) "it;" 2) "working;" and 3) a comparative analysis. The functional ingredients of the drug court model—the composite "it"— have been sketched out briefly above and in greater detail in other discussions (Goldkamp, 1995, 1999a, 1999b, 2000; Hora et al., 1999; NADCP, 1997). However, regarding the second consideration, there are at least two meanings of "working." The second meaning (how it operates to produce its effect), we will discuss below. The first and more common usage simply refers to producing a successful outcome on a certain criterion measure. It is no exaggeration to state that the "yardstick" most commonly employed by many public officials in assessing the potential utility of drug courts is crime reduction, with cost reduction a close second favorite. In short, officials want to know if drug courts reduce crime and save money doing so. Drug court advocates argue that a variety of other outcome measures, such as substance abuse reduction, improved life functioning, and improved skills and health, are also essential measures of drug court impact.

### IMPACT AS A COMPARISON

The measurement of outcomes, however, requires a comparative framework— the third component implicit in the question. In fact, the question is not just "Do

drug courts work?" but rather "Do drug courts work better than . . . not having drug courts?" Whichever success criterion one emphasizes (e.g., crime, drug use, or dollars), drug court must be compared to a non-drug court condition to permit inferences about relative impact. Drug court participants should show better results than some appropriate comparison group not undergoing the drug court treatment process.

The interface between the need for reasonably rigorous methodological designs and the intuitive views of practitioners on research design produce lively discussions of how to frame appropriate comparisons and how to form suitable comparison groups to gauge drug court impact "in the real world." Two of the most common debates between evaluators and practitioners, for example, involve discussions of the feasibility of experimental designs and of the appropriateness of comparing reoffending rates of graduates and non-graduates.

The appeal of problems associated with this last type of comparison—between graduates and non-graduates—is illustrated by findings from the retrospective evaluation of the Portland and Las Vegas drug courts in Figure 1, comparing the two-year[8] rearrest rates for graduates and non-graduates (for the entire study periods and on a year-by-year basis). The findings appear to show a dramatic and consistent drug court crime reduction effect, with drug court graduates generally showing substantially lower rearrest rates over two year periods from entry than non-graduates. As popular as these kinds of analyses may be among advocates seeking to declare the efficacy of drug courts, they are biased in the direction of showing positive results and, as such, are highly misleading. Basically, the much-heralded findings show that the successes succeed and the failures fail.

A more appropriate analysis would compare, within a given time frame the outcomes of cohorts of drug-involved offenders who enter the treatment process to those of similar cohorts of defendants who do not enter drug court. Some participants are likely to have problems that are easier to deal with and are likely to do quite well. Some are likely to have extremely serious problems, with histories of doing poorly at almost everything, and are likely to have a much more difficult time succeeding. A fair evaluation of drug court impact, however, must consider the relative progress achieved by the entire cohort or "class" of drug court enrollees compared to their non-drug counterparts, rather than rely selectively on the predictably great results of the most able few.

How the comparison of drug court versus non-drug court performance is framed is critical to the interpretation of results and their validity. Often, evaluations have little choice but to employ non-experimental or quasi-experimental designs to frame the comparisons. The social science literature is replete with discussions of the methodological issues associated with the various types of designs, experimental as well as pre-, non-, and quasi-experimental. Retrospective evaluation poses its own problems in constructing an appropriate comparative framework, involving, as it does, a reconstruction of the past. Because experimental designs with random assignment are quite obviously impossible in retrospective evaluation, other next-

best comparative approaches must be employed. Having reasonably addressed the issues associated with framing appropriate comparisons for evaluation, then, one might suppose that the task of answering the question whether drug courts "work" is relatively straightforward. Do the drug court participants perform better on the criterion of success (e.g., recidivism) than their non-treated counterparts?

Figure 2 depicts the hypothesized causal relationship implicit in the drug court model (Drug Court Causal Model 1). A (drug court treatment) causes B (improved offender behavior). Using some reasonable comparative framework, one would test this model of drug court impact by asking whether drug court participants reoffend less than their (similar) counterparts who do not go through drug court. The researcher has merely to compare reoffending rates—usually rearrests—for the two (or more) groups of potentially eligible defendants or offenders. If drug court participants are rearrested less frequently than their counterparts, the data may be interpreted as supporting the argument that drug courts "work" or that, compared to the condition of not having the drug court, drug court participation reduces crime among participants. Or can they?

Consider findings from the Portland and Las Vegas studies in Figure 3. These figures show the weighted estimates contrasting the one-year rearrest rates of drug court participants with comparison group defendants in Portland (1991-1997) and in Las Vegas (1993-1997). These comparative analyses for both jurisdictions suggest that lower proportions of drug court participants overall were rearrested during these study periods than of their counterparts when the criterion is rearrest for any type of offense and rearrest for drug offenses. The differences are slight or insignificant in both jurisdictions when the measure is rearrest for non-drug offenses (and may be explained by sampling error). Assuming the researcher can be confident that the differences are not an artifact of the design employed[9] and some agreement on how large an effect must be to be "large," are these findings enough to suggest that drug courts work? Indeed, based on these data for the seven and five-year study periods employed in the evaluation of the Portland and Las Vegas drug courts, officials should feel encouraged to make that claim.

Figures 4 and 5 display the comparative rates of rearrest for drug court and comparison groups in each site over time (rather than aggregating the yearly cohorts for an all-years weighted total). These findings still show some of the hypothesized positive results but with qualification. First, the size of the differences between drug court participants and comparison groups varies by the sample periods. In Portland, drug court participants always produce lower rearrest rates for "any" offense than drug defendants in one comparison group (Comparison Group A, the immediate absconders who never attended the drug court process) but, during two periods, did not differ much from the second comparison group (B, those who attended the initial drug court hearing but did not enter treatment).[10]

In Las Vegas, not only did the size of the differences between drug court participants and the comparison group members vary, but it also decreased over time. Las Vegas drug court participants outperformed their comparison group

## FIGURE 1

### REARREST OF DRUG COURT GRADUATES AND NONGRADUATES WITHIN TWO YEARS OF ENTRY IN PORTLAND (1991-96) AND LAS VEGAS (1993-97) BY TYPE OF OFFENSE AND TIME PERIOD
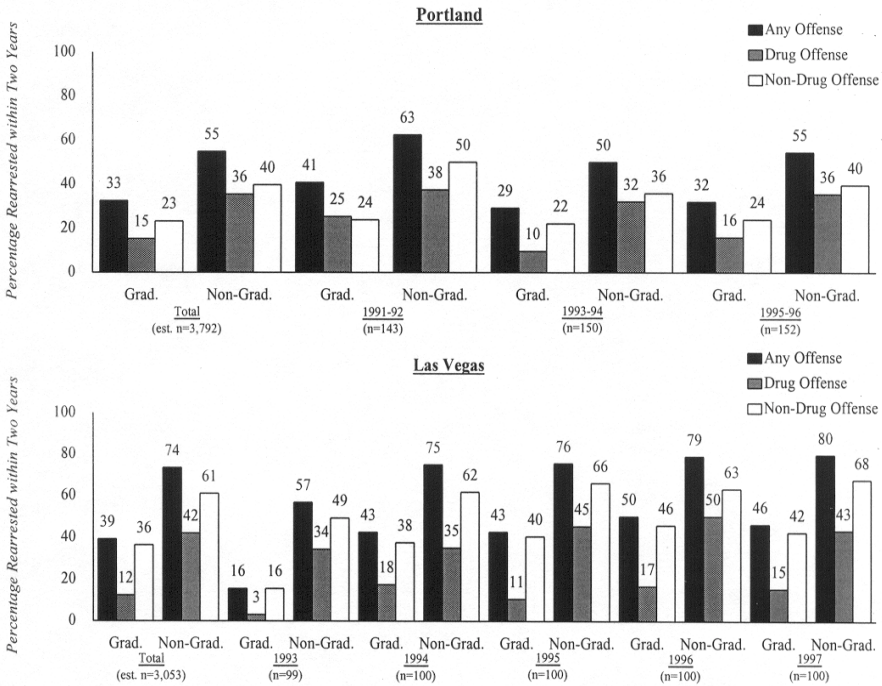


[Note: Data are weighted estimates from disproportional stratified sample.]

counterparts in the 1993, 1994, and 1995 sample cohorts on all measures (except for the non-drug offenses in the 1994 cohort). Las Vegas drug court participants produced higher rearrest rates than their comparison group in 1996 sample cohorts and similar rearrest rates in the 1997 sample cohorts (except they showed lower rearrest rates for drug offenses in both the 1996 and 1997 cohorts). In fact, the "drug court effect" appears to have disappeared in Las Vegas by 1996, except for rearrests for drug offenses.[11] In short, the apparent overall effect of drug court treatment on criminal behavior masks variation over time-periods in both locations. If the study were not longitudinal—following cohorts of drug defendants in each successive period of the courts' existence—one might have drawn very different inferences about drug court effectiveness depending on the period studied.

A related measure of drug court impact on offending behavior is not whether participants were rearrested (more or less often than comparison groups) but, when

FIGURE 2

THE IMPLICIT DRUG COURT HYPOTHESIS: DRUG COURT TREATMENT "CAUSES"
(IMPROVED) OFFENDER BEHAVIOR (MODEL 1)



arrested, the length of time (in median days) that passed between enrollment and rearrest. At a presentation of findings from our early 1990's evaluation of the Miami drug court (Goldkamp & Weiland, 1993), then Dade County State Attorney Janet Reno remarked that she did not expect to see "fairy tale" results showing dramatic reductions in reoffending among the drug addicts targeted. She appeared rather more impressed with the finding that, when drug court participants indeed were rearrested, they took two to three times longer to be rearrested than their counterparts. She reasoned that, because it was unrealistic to expect the drug court to eliminate reoffending entirely among the addicted, the finding that it at least had a powerful slowing effect, delaying return of drug offenders to the justice system, suggested a potentially major and practical contribution to reducing the drug-related caseload clogging the Miami courts and filling its jails at the time.

Using this criterion, the Portland and Las Vegas drug court findings also seem powerful and positive. Figure 6 shows dramatic differences in lengths of time to first rearrest between drug court participants and non-participants overall and when yearly cohorts are considered separately in Portland and Las Vegas.[12] From 1991 to 1997, Portland drug court participants took two to four times as long as comparison group defendants to be rearrested. (These ratios fluctuated depending on the sampling period, but the differences were consistently large.) In Las Vegas, drug court participants also took nearly twice as long (median, 94 days) as comparison group defendants (median, 52 days) to be rearrested. This varied by year studied, with a diminution of the difference in the 1997 cohort.

Taken together, these two measures of drug defendant reoffending provide moderately strong evidence to support the hypothesis that drug courts have a crime-reduction effect (when the behavior of participating and non-participating defendants are compared at roughly one year after arrest or program entry). At the same time, these findings also make it difficult to be satisfied with the simple causal model (Model 1) suggesting that drug court participation shapes offender behavior (reduces criminality). Because the impact of drug courts measured in these two basic ways appears to fluctuate over time, one might reasonably question whether drug court impact must therefore be conditioned by other factors—external or internal to the operation of drug courts—not included in the model. (We discuss "internal" factors

## FIGURE 3

## REARREST OF DRUG COURT PARTICIPANTS WITHIN ONE YEAR OF ENTRY IN PORTLAND (1991-97) AND LAS VEGAS (1993-97) BY TYPE AND OFFENDER
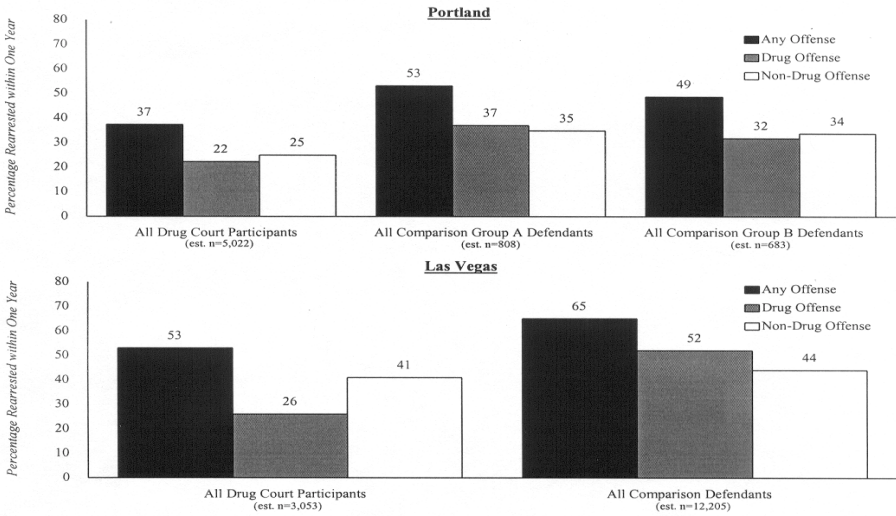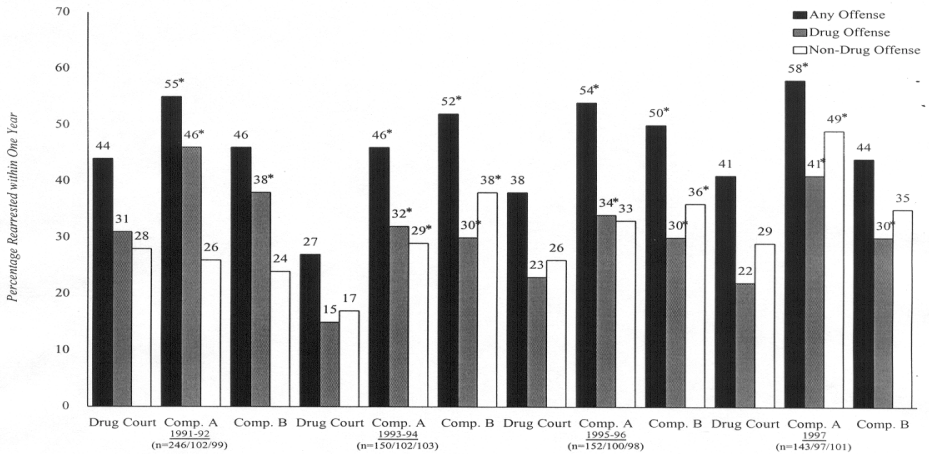


## FIGURE 4

## REARREST OF DRUG COURT PARTICIPANTS WITHIN ONE YEAR OF ENTRY IN PORTLAND (1991-97), BY TYPE OF OFFENSE, BY TIME PERIOD



*Indicates significant difference at p ≤ .05 when compared to drug court participants.
[Note: Each time period was randomly sampled to produce equal sample sizes of about 150 persons in the drug court group with the exception of 1991-92 (n=246).]

FIGURE 5

REARREST OF DRUG COURT PARTICIPANTS WITHIN ONE YEAR IN LAS VEGAS (1993-97) BY TYPE OF OFFENSE, BY TIME PERIOD
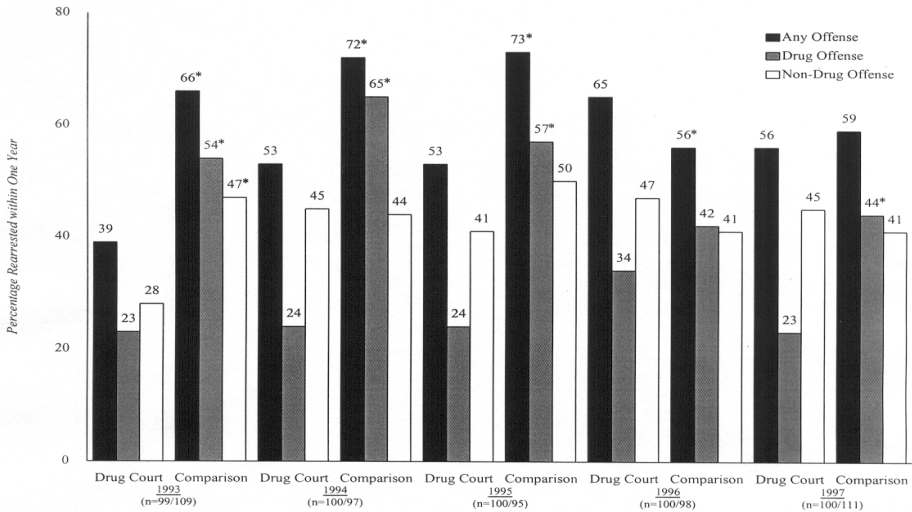


*Indicates significant difference at p ≤ .05 when compared to drug court participants.
[Note: Each year was randomly sampled to produce equal samples of approximately 100 defendants.]

below under "If Drug Courts Work, How Do They Work?") We have argued elsewhere (Goldkamp et al., 2000a , 2000b) that the larger context or circumstances surrounding drug courts—such as key laws, political environment, drug epidemics, jail overcrowding emergency measures, administrative policies, treatment resources, etc.—may affect their functioning and, ultimately, their productivity.

The possibility of potentially influential antecedent causal factors—whether internal or external in origin—makes it necessary to modify the simple causal model explaining drug court impact on offender behavior. In one modified version, the drug court's influence on offender behavior is shaped by the prior influence of outside factors, such as changes in law, a drug epidemic, jail overcrowding, etc. Model 2, shown in Figure 7, postulates that such contextual factors could influence offender behavior directly as well as indirectly through the effect of the drug court. For example, changes in law affixing penalties for drug offenses might affect both the offender's willingness to pursue risky drug-oriented behavior as well as the offender's willingness to enter drug court and follow treatment if apprehended. The offender's residential neighborhood may provide a context that both encourages crime (through availability of drugs or opportunities for crime) and discourages access to treatment or supportive services. These outside factors could explain the variation in impact shown in the Portland and Las Vegas results when viewed in successive cohorts over time.[13]

[Note: Medians for totals are calculated based on weighted data. Annual medians are calculated with unweighted data.]
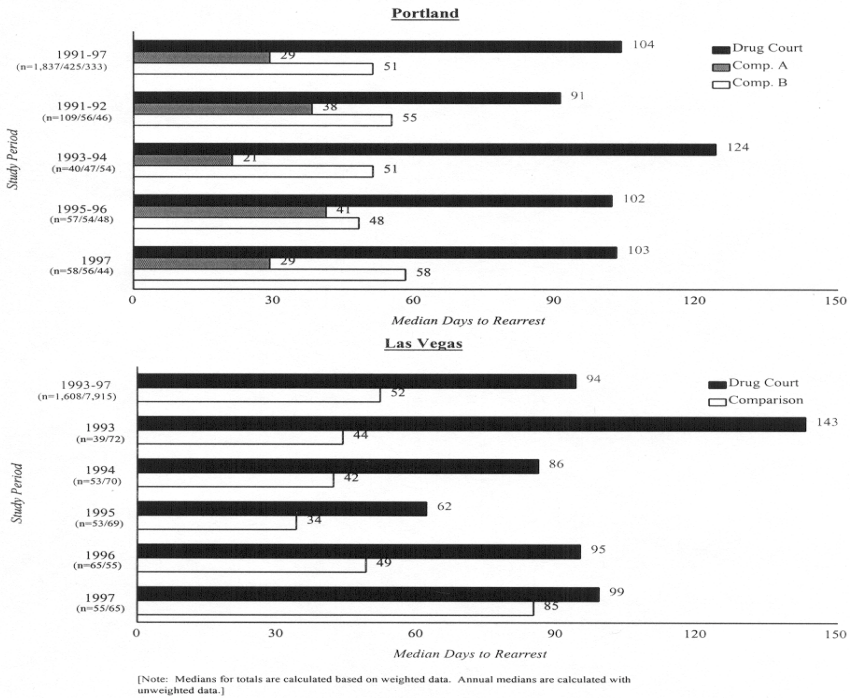
Unfortunately, Model 2 also raises the possibility that the apparent relation between drug court treatment and offender outcomes may be spurious. (See Model 3 in Figure 8.) Conceivably, then, the same explanation of outside factors—e.g., the enactment of three-strikes legislation upping the stakes for persons who might be apprehended for drug felonies or neighborhood context—might explain participation in drug court as well as offender behavior during follow-up. Although the lower rate of reoffending may be construed as a result of drug court participation, it may instead represent a concomitant or parallel result of a common cause.

A compelling argument that antecedents may play a strong role in shaping drug court impact can be made when offender attributes are considered. Model 4 (Figure 9) suggests that various attributes of offenders, including demographics, family ties, prior criminal history, prior substance abuse history, type and frequency of substance abuse prior to drug court candidacy, etc., may at least partly explain drug court participation and offender behavior during a follow-up period. Neighborhood

of residence could be considered an offender attribute or an outside, contextual factor. Seriously substance-abuse involved defendants from neighborhoods with chronic drug problems, unsuccessful prior experience with drug treatment, and extensive prior contacts with the justice system may a) choose not to enter drug court and b) be more likely to reoffend without it. In positing this specific form of "antecedent" factors, Model 4, at least, offers a plausible (indirect effect) explanation for the variation in drug court impact over time: the participants may differ in their criminogenic or "risk" attributes from year to year. Thus, as the "degree of difficulty" (mix of participant attributes) associated with each cohort fluctuates over time, so do the results or measurable outcomes of drug court treatment. Model 4 also offers a plausible explanation for spuriousness—that drug court participants differ from comparison group defendants in reoffending because they enroll with lower-risk attributes in the first place.

A simple test of this (spuriousness) model is to identify differences in the attributes of persons entering and not entering drug court and, if found, to enter them as controls in comparative analysis of reoffending to determine whether lower rates for drug court participants still survive. If they disappear with controls for *a priori* participant risk, then lower drug court rearrest rates would appear to be explained by differences in sample composition (i.e., in the risk attributes) between drug court participants and their non-drug court counterparts.[14] If, after controls, significantly lower rates of reoffending among drug court participants do not disappear, one could assume that the more favorable outcomes among drug court participants are not explained by their less "risky" prior attributes. Instead, one

FIGURE 7
OUTSIDE FACTORS SHAPE OFFENDER BEHAVIOR DIRECTLY AND INDIRECTLY THROUGH DRUG COURT TREATMENT (MODEL 2)
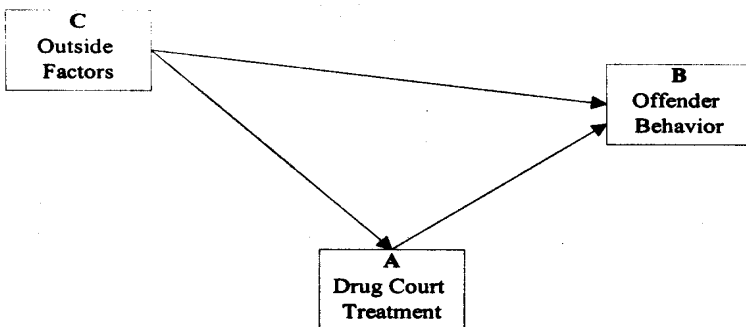
would infer that the relation between drug court participation and a lower rate of subsequent reoffending is not spurious.

Following this logic, Table 1 shows that, when controls for defendant attributes are exercised using the unweighted 1991-97 Portland data,[15] the news is not so encouraging: when drug court participants are compared to defendants in comparison group A (never attended court) no significant differences in rearrests of any type are found. When compared to comparison group B defendants, drug court participants showed significantly lower rearrest rates only when drug rearrests were the criterion. When the specific time frames are considered, the only statistically significant finding is that the 1993-94 drug court participants outperformed comparison group B defendants for each type of rearrest (any, drug and non-drug). Table 2 shows that in Las Vegas, after controlling for defendant attributes, drug court participants from the total study period (1993-1997) showed significantly lower rates for each type of rearrest measured at the one year mark. When the same analysis was conducted for specific years, the difference in rearrests for any offense between drug court participants and comparison group defendants remained significant only for the 1994 and 1995 cohorts. Drug rearrests were significantly lower only among 1994 defendants. Differences between drug and non-drug court defendants did not reach significance for non-drug rearrest rates in any of the sample years separately analyzed.

These results, particularly the Portland findings, suggest that, indeed, some of the apparent differences in reoffending (i.e., lower rearrest rates for drug court participants) disappear when defendant attributes are controlled in comparative analyses. They suggest that, to some extent in some specific time periods, the relationship between drug court treatment and later offender behavior may be

FIGURE 8

DRUG COURT IMPACT ON OFFENDER BEHAVIOR IS SPURIOUS (MODEL 3)
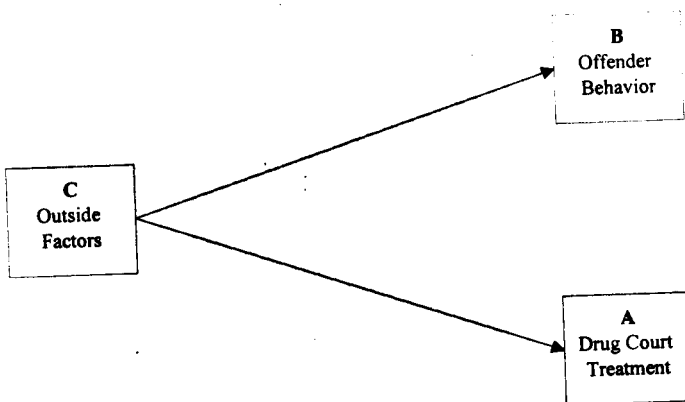
spurious, partly explained by differences in sample composition, or, as the model posits, prior defendant attributes.

## If Drug Courts Work, How Do They Work?

So far, the analysis has only considered "drug court impact" in a general comparative sense, as if "a drug court is a drug court is a drug court." Whether out of curiosity, the need for practical guidance, or the need for theoretical coherence, the findings of a) positive impact and b) variation in impact over time make it impossible to avoid the question, "If drug courts work, how do they work?" (Or, "Why does a drug court work sometimes, in some settings, under some circumstances?") To understand the circumstances of the relative impact of the drug court model, then, research is ineluctably forced to look "inside" the drug court to consider how this can be—assuming there is a drug court effect independent of outside factors. Here the business of understanding the impact of drug courts becomes noticeably more complicated as this question goes to the heart of what a drug court "is" and tries to distinguish between what a drug court does and what a drug court produces.

The drug court model represents a coordinated collection of functions, methods, and activities that reflect general and specific deterrent as well as rehabilitative aims. For example, the drug court model involves frequent appearances before the drug court judge. This can be seen to serve two purposes: to coax, persuade and encourage participants into better behavior (treatment) or to reward progress with increased privileges or to threaten sanctions in the event of poor performance (specific deterrence). As if drawn directly from Jeremy Bentham's 19th century discussion of the principle of utility (1996), central reliance on manipulation of

FIGURE 9

OFFENDER ATTRIBUTES AND OUTSIDE FACTORS SHAPE DRUG COURT OUTCOMES
AND OFFENDER BEHAVIOR (MODEL 4)
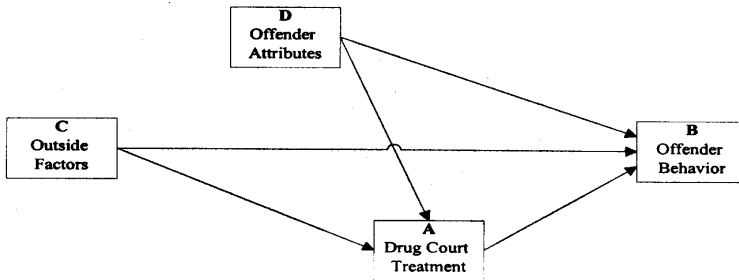
rewards (praise, promotion to next treatment stages, awards) and sanctions (additional appearances before the judge, days in the jury box, demotion to prior phases of treatment, and days in jail) are believed to form an important component of the content of the drug court experience. The alternation of encouragement and sanctioning occurs in a public forum—the criminal courtroom—much like a classroom, with seated participants observing the interactions of other participants with the judge. The sanctions and rewards in that setting serve to communicate in the manner of general deterrence the message to all observers that if one does X, Y results—with the warnings and lessons to be repeated over and over. Linked to the judge's performance in the courtroom, the treatment program itself is a principal rehabilitative component. The type, range, and frequency of services, as well as drug testing, its frequency and how it is employed, are presumed to be potentially significant elements in producing the drug court effect. Their linkage to the special courtroom experience and direct person-to-person exchanges with the judge are thought to interact to produce a therapeutic effect greater than traditional treatment or deterrent approaches alone could achieve.

In short, the impact of the drug court—the "drug court effect"—is believed to be derived from a collection of instrumental elements, the salience of which is likely to vary over time in a particular jurisdiction and to vary from location to location as the elements of the drug court model are adapted to different settings. An important challenge for research is to determine the relative contributions of the various parts of the drug court model in accounting for its overall (presumed) impact and to discuss the implications of findings that some, but not all, are important. A high priority, for example, is testing the assumption that the role of the drug court judge is a fundamental and core element of the drug court model in producing positive treatment outcomes. Other core assumptions of the model needing critical examination relate to the use of sanctions, the relative value of sanctions and incentives deployed in the courtroom, and whether drug court participants are really motivated toward favorable progress by fear of going to jail.

These questions implicit in assessing the contribution of the ingredients of the drug court model are not inconsequential. For example, if the belief that the judge is the central and most important positive influence on drug court outcomes is not supported through empirical testing, there are major implications for drug courts and the allocation of judicial resources. Setting aside the potentially significant effects of outside factors and participant attributes, these questions begin to sort through the contributions of the internal elements of the drug court model assumed by their designers to be instrumental to the drug court's operation.

*MODELING THE EFFECTS OF DRUG COURT FUNCTIONS ON OUTCOMES*

The task of sorting out the effects of the various ingredients of the drug court model is complicated by the need to distinguish between the instrumental functions (such as those just listed) and their results or outcomes. The appearances before the judge, the appointments for treatment, drug tests and other activities form part of the delivery of the treatment effect. The results they produce—drug court success

TABLE 1

TESTING THE SIGNIFICANCE OF DIFFERENCES IN REARREST WITHIN ONE YEAR BETWEEN DRUG COURT PARTICIPANTS AND COMPARISON GROUPS IN PORTLAND, CONTROLLING FOR SAMPLE DIFFERENCE (BY TIME PERIODS AND TYPE OF REARREST)

[Note: These analyses are based on unweighted data. Parameter estimates and significance are indicated from Logit analysis]

| | Any Rearrest | | Participant Year | | | | | | | |
| | 1991-97 | | 1991-92 | | 1993-94 | | 1995-96 | | 1997 | |
| Sample Differences | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B |
|---|---|---|---|---|---|---|---|---|---|---|
| *Demographic* | | | | | | | | | | |
| Over or Under 25 Yrs. Old | .127 (.397) | | .493 (.081) | | | | | | .321 (.315) | |
| Hispanic | .258 (.157) | .507 (.018) | -.018 (.959) | .610 (.064) | .610 (.073) | | -.778 (.048) | | | |
| Race (white/non-white) | | | .849 (.001) | | | | | | .833 (.008) | |
| *Current Case* | | | | | | | | | | |
| Phone | | | | | | | | | | -.108 (.739) |
| Detained at All Pretrial | .559 (.000) | .310 (.020) | | | .821 (.005) | .381 (.187) | 1.147 (.000) | .353 (.205) | .537 (.069) | 1.095 (.000) |
| *Criminal History* | | | | | | | | | | |
| Pending Arrest Charge | .871 (.000) | .783 (.000) | .793 (.011) | 1.075 (.001) | | .955 (.006) | 1.668 (.002) | | | |
| Prior Drug Arrest | | .802 (.000) | | | | | | | .906 (.070) | |
| Prior Drug Possession Arrest | | | | | | | | | | |
| Prior Drug Trafficking Arrest | | | | | | .350 (.550) | | | | |
| Serious Person Conviction | | | | .194 (.624) | | | | | | |
| Drug Possession Conviction | | | | | | | | | 1.163 (.002) | .790 (.021) |
| Drug Trafficking Conviction | | | | | | -.581 (.420) | | | | |
| Weapon Conviction | .211 (.542) | | | | | | | .953 (.070) | | |
| Prior FTAs in 3 Years | 1.203 (.000) | | | | .616 (.198) | | | | | |
| *Sample* | | | | | | | | | | |
| TX v. Comp. A | -.190 (.205) | | .184 (.522) | | -.302 (.337) | | -.320 (.316) | | .030 (.926) | |
| TX v. Comp. B | | -.166 (.224) | | .163 (.546) | | -.751 (.010) | | -.364 (.189) | | .335 (.283) |
| *Model Statistics* | | | | | | | | | | |
| Log Likelihood | 1,362.362 | 1,374.964 | 442.136 | 436.898 | 305.497 | 310.141 | 313.944 | 331.408 | 294.148 | 284.678 |
| Goodness of Fit | 8.393 | 11.261 | 1.089 | 2.069 | 5.414 | .529 | 4.917 | 4.117 | 6.359 | 9.626 |
| GF Significance | .398 | .081 | .982 | .723 | .368 | .991 | .426 | .249 | .498 | .141 |
| Chi Square | 126.330 | 90.639 | 27.673 | 17.852 | 22.988 | 25.738 | 32.715 | 9.393 | 37.963 | 24.079 |
| DF | 7 | 5 | 5 | 4 | 4 | 5 | 4 | 3 | 6 | 4 |
| Significance | .000 | .000 | .000 | .001 | .000 | .000 | .000 | .024 | .000 | .000 |
| n | 1,083 | 1,073 | 339 | 329 | 252 | 253 | 252 | 249 | 240 | 226 |

## TABLE 1 CONTINUED.

[Note: These analyses are based on unweighted data. Parameter estimates and significance are indicated from Logit analysis.]

**Drug Rearrest**

| Sample Differences | Participant Year | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 1991-97 | | 1991-92 | | 1993-94 | | 1995-96 | | 1997 | |
| | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B |
| *Demographic* | | | | | | | | | | |
| Over or Under 25 Yrs. Old | .014 (.932) | | .397 (.164) | | | | | | .253 (.476) | -.308 (.410) |
| Hispanic | .638 (.001) | .928 (.000) | .479 (.163) | 1.050 (.001) | .998 (.006) | | -.292 (.472) | | | |
| Race (white/non-white) | | | .691 (.014) | | | | | | 1.157 (.000) | |
| *Current Case* | | | | | | | | | | |
| Phone | | | | | | | | | | 1.089 (.001) |
| Detained at All Pretrial | .582 (.000) | .320 (.029) | .612 (.045) | | .663 (.051) | .515 (.115) | 1.132 (.001) | .505 (.097) | .923 (.006) | |
| *Criminal History* | | | | | | | | | | |
| Pending Arrest Charge | .761 (.000) | .760 (.000) | | .964 (.002) | | .994 (.006) | 1.464 (.002) | | .374 (.429) | |
| Prior Drug Arrest | | .530 (.001) | | | | | | | | |
| Prior Drug Possession Arrest | | | | | | | | | | |
| Prior Drug Trafficking Arrest | | | | | | | | | | |
| Serious Person Conviction | | | | -.434 (.349) | | .067 (.916) | | | | |
| Drug Possession Conviction | | | | | | | | | 1.010 (.007) | |
| Drug Trafficking Conviction | | | | | | .394 (.589) | | .175 (.744) | | .290 (.426) |
| Weapon Conviction | .569 (.120) | | | | | | | | | |
| Prior FTAs in 3 Years | .643 (.000) | | | | .492 (.348) | | | | | |
| *Sample* | | | | | | | | | | |
| TX v. Comp. A | -.241 (.126) | | -.066 (.320) | | -.401 (.272) | | -.147 (.662) | | .152 (.667) | |
| TX v. Comp. B | | -.214 (.152) | | -.060 (.833) | | -.730 (.031) | | -.231 (.458) | | .013 (.970) |
| *Model Statistics* | | | | | | | | | | |
| Log Likelihood | 1,217.523 | 1,181.793 | 410.119 | 392.535 | 245.298 | 249.861 | 274.100 | 279.554 | 294.052 | 245.537 |
| Goodness of Fit | 4.778 | 1.293 | 5.071 | 5.967 | 7.195 | 1.452 | 6.071 | 1.691 | 14.967 | 14.646 |
| GF Significance | .573 | .972 | .535 | .202 | .126 | .963 | .299 | .639 | .060 | .012 |
| Chi Square | 93.706 | 78.328 | 30.497 | 26.752 | 24.156 | 20.105 | 25.576 | 4.269 | 40.694 | 16.143 |
| DF | 7 | 5 | 5 | 4 | 4 | 5 | 4 | 3 | 6 | 4 |
| Significance | .000 | .000 | .000 | .000 | .000 | .001 | .000 | .234 | .000 | .003 |
| n | 1,083 | 1,073 | 339 | 329 | 252 | 253 | 252 | 249 | 240 | 226 |

TABLE 1 CONTINUED.

[Note: These analyses are based on unweighted data. Parameter estimates and significance are indicated from Logit analysis.]

Non-Drug Related

| | Participant Year | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 1991-97 | | 1991-92 | | 1993-94 | | 1995-96 | | 1997 | |
| Sample Differences | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B | D. Ct. v. Comp. A | D. Ct. v. Comp. B |
| *Demographic* | | | | | | | | | | |
| Over or Under 25 Yrs Old | .095 (.550) | | .429 (.146) | | | | | | -.071 (.831) | |
| Hispanic | -.380 (.058) | -.551 (.025) | -.727 (.046) | -.175 (.620) | -.085 (.821) | | -1.056 (.018) | | .758 (.016) | |
| Race (white/non-white) | | | .974 (.000) | | | | | | | |
| *Current Case* | | | | | | | | | | |
| Phone | | | | | | | | | | .368 (.276) |
| Detained at All Pretrial | .328 (.023) | .226 (.003) | .443 (.153) | .736 (.018) | | .740 (.036) | | 1.061 (.018) | .929 (.047) | |
| *Criminal History* | | | | | | | | | | |
| Pending Arrest Charge | | | | | | | | | | |
| Prior Drug Arrest | | .818 (.000) | | | | | | | | |
| Prior Drug Possession Arrest | | | | | | | | | | |
| Prior Drug Trafficking Arrest | | | | .712 (.072) | | | | | | |
| Serious Person Conviction | | | | | | .008 (.990) | | | | |
| Drug Possession Conviction | | | | | | | | | | |
| Drug Trafficking Conviction | | | | | | -.544 (.505) | | | | |
| Weapon Conviction | .230 (.536) | | | | | | | .840 (.091) | 1.031 (.004) | 1.060 (.002) |
| Prior FTAs in 3 Years | 1.060 (.000) | | | | .704 (.144) | | | | | |
| *Sample* | | | | | | | | | | |
| TX v. Comp A | -.223 (.154) | | -.504 (.104) | | -.366 (.285) | | -.291 (.372) | | -.397 (.223) | |
| TX v. Comp B | | -.138 (.341) | | .221 (.450) | | -.744 (.019) | | -.284 (.334) | | .148 (.651) |
| *Model Statistics* | | | | | | | | | | |
| Log Likelihood | 1,258.595 | 1,252.610 | 402.860 | 399.756 | 271.698 | 280.452 | 294.401 | 303.874 | 279.437 | 264.507 |
| Goodness of Fit | 16.113 | 8.963 | 1.138 | 3.562 | 1.479 | 5.438 | 1.591 | 2.105 | 7.414 | 7.664 |
| GF Significance | .024 | .176 | .980 | .614 | .915 | .489 | .902 | .551 | .284 | .264 |
| Chi Square | 81.207 | 63.515 | 18.335 | 9.214 | 11.719 | 14.059 | 14.419 | 5.722 | 38.114 | 21.314 |
| DF | 7 | 5 | 5 | 4 | 4 | 5 | 4 | 3 | 6 | 4 |
| Significance | .000 | .000 | .003 | .056 | .020 | .015 | .007 | .126 | .000 | .000 |
| n | 1,083 | 1,073 | 339 | 329 | 252 | 253 | 252 | 249 | 240 | 226 |

TABLE 2

TESTING THE SIGNIFICANCE OF DIFFERENCES IN REARREST WITHIN ONE YEAR BETWEEN DRUG COURT PARTICIPANTS AND COMPARISON GROUP DEFENDANTS IN LAS VEGAS, CONTROLLING FOR SAMPLE DIFFERENCES (BY TIME PERIOD AND TYPE OF REARREST)

[Note: These analyses are based on unweighted data. Parameter estimates and significance are indicated from Logit analysis.]

| *Any Rearrest* | | | *Las Vegas* | | | |
|---|---|---|---|---|---|---|
| Sample Differences | 1993-97 | 1993 | 1994 | 1995 | 1996 | 1997 |
| *Demographic* | | | | | | |
| Gender | | | | .2398 (.5376) | | |
| Alias | | | | | | -.8448 (.0065) |
| *Current Case* | | | | | | |
| Phone | -.1064 (.6102) | .3148 (.4988) | -.5517 (.2990) | .0447 (.9264) | .3597 (.4315) | |
| *Criminal History* | | | | | | |
| Most Serious Charge, current | .1400 (.6528) | -.8002 (.1618) | .1779 (.7688) | | | |
| Theft Charge, current | | | | | | |
| Drug Charge, current | -.6170 (.0297) | -.6404 (.1047) | | -1.6123 (.0501) | .7436 (.2486) | -.7724 (.0872) |
| Guilty Comparison | | | | | | |
| Prior Drug Arrests | | | 1.1460 (.0009) | | | |
| Prior Serious Persons Convictions | .6627 (.0003) | | | 1.4002 (.0114) | | |
| Prior Drug Convictions | | | | | | |
| Prior Drug Trafficking Convictions | | | | 1.2004 (.0704) | .5747 (.3716) | |
| Prior Felony Convictions | | | | | | .5812 (.1110) |
| Prior FTAs | .9964 (.0000) | | | | 1.5954 (.0000) | |
| *Sample* | .7184 (.0009) | .8601 (.0734) | 1.0937 (.0435) | .9748 (.0499) | -.2447 (.6056) | .4380 (.2010) |
| *Model Statistics* | | | | | | |
| Log Likelihood | 1,219.671 | 255.318 | 222.132 | 227.688 | 229.556 | 267.653 |
| Goodness of Fit (H&L) | 2.9552 | 4.9890 | 4.6416 | 5.2886 | 2.4802 | 5.3727 |
| GF Significance | .8891 | .2884 | .7036 | .5074 | .7795 | .4970 |
| Chi Square | 103.102 | 17.529 | 21.198 | 24.220 | 29.042 | 16.283 |
| DF | 6 | 4 | 4 | 6 | 5 | 4 |
| Significance | .0000 | .0015 | .0003 | .0005 | .0000 | .0027 |
| n | 979 | 198 | 185 | 192 | 192 | 207 |

TABLE 2 CONTINUED.

[Note: These analyses are based on unweighted data. Parameter estimates and significance are indicated from Logit analysis.]

| Drug Rearrest | 1993-97 | 1993 | 1994 | Las Vegas 1995 | 1996 | 1997 |
|---|---|---|---|---|---|---|
| **Sample Differences** | | | | | | |
| *Demographic* | | | | | | |
| Gender | | | | .0228 (.9538) | | |
| Alias | | | | | | -.6049 (.0586) |
| *Current Case* | | | | | | |
| Phone | .1068 (.5976) | 1.1271 (.0183) | -.2029 (.6877) | .2358 (.5988) | .2423 (.5797) | |
| *Criminal History* | | | | | | |
| Most Serious Charge, current | .2696 (.3941) | -.1987 (.7522) | -.3152 (.6371) | | | |
| Theft Charge, current | | | | | | |
| Drug Charge, current | .6324 (.0454) | | | .4344 (.5598) | .2852 (.6222) | 1.2622 (.0345) |
| Guilty Comparison | | | | | | |
| Prior Drug Arrests | | | 1.0054 (.0028) | | | |
| Prior Serious Persons Convictions | .6151 (.0003) | -.3242 (.4124) | | | | |
| Prior Drug Convictions | | | | -.1879 (.6935) | | |
| Prior Drug Trafficking Convictions | | | | 1.4730 (.0073) | -.2021 (.7132) | |
| Prior Felony Convictions | | | | | | |
| Prior FTAs | .9999 (.0000) | | | | 1.6932 (.0000) | .5786 (.1249) |
| **Sample** | 1.0241 (.0000) | .4915 (.3353) | 1.7331 (.0008) | .8428 (.0750) | .6812 (.1548) | .5597 (.1215) |
| *Model Statistics* | | | | | | |
| Log Likelihood | 1,168.449 | 238.466 | 214.723 | 233.869 | 230.437 | 244.307 |
| Goodness of Fit (H&L) | 4.7900 | 10.6926 | 4.8590 | 7.9545 | .7166 | 5.0228 |
| GF Significance | .6856 | .0302 | .5620 | .3366 | .9820 | .5409 |
| Chi Square | 147.171 | 24.267 | 40.524 | 26.942 | 28.942 | 19.210 |
| DF | 6 | 4 | 4 | 6 | 5 | 4 |
| Significance | .0000 | .0001 | .0000 | .0001 | .0000 | .0007 |
| n | 979 | 198 | 185 | 192 | 192 | 207 |

TABLE 2 CONTINUED.

[Note: These analyses are based on unweighted data. Parameter estimates and significance are indicated from Logit analysis.]

| Non-Drug Rearrest | | Las Vegas | | | | |
|---|---|---|---|---|---|---|
| Sample Differences | 1993-97 | 1993 | 1994 | 1995 | 1996 | 1997 |
| *Demographic* | | | | | | |
| Gender | | | | -.3370 (.3851) | | -.7637 (.0138) |
| Alias | | | | | | |
| *Current Case* | | | | | | |
| Phone | -.1736 (.3774) | -.1008 (.8232) | -.0249 (.9564) | -.5444 (.2300) | .2256 (.6017) | |
| *Criminal History* | | | | | | |
| Most Serious Charge, current | | | | | | |
| Theft Charge, current | .0455 (.8764) | -.3157 (.5805) | .1317 (.8225) | | | |
| Drug Charge, current | -.9031 (.0008) | | | -1.4851 (.0381) | .6436 (.2417) | -1.1219 (.0118) |
| Guilty Comparison | | -.3308 (.3870) | | | | |
| Prior Drug Arrests | | | 1.1424 (.0003) | | | |
| Prior Serious Persons Convictions | | | | 1.7885 (.0005) | | |
| Prior Drug Convictions | .2773 (.0920) | | | | | |
| Prior Drug Trafficking Convictions | | | | .0418 (.9324) | .5984 (.2619) | |
| Prior Felony Convictions | | | | | | .4760 (.1747) |
| Prior FTAs | .8128 (.0000) | .7947 (.0940) | -.0382 (.9358) | .8513 (.0759) | .8978 (.0061) | |
| *Sample* | .4332 (.0345) | | | | -.1370 (.7627) | .0848 (.8080) |
| *Model Statistics* | | | | | | |
| Log Likelihood | 1,275.572 | 261.506 | 238.475 | 240.378 | 249.420 | 264.376 |
| Goodness of Fit (H&L) | 7.2856 | 5.1284 | 1.6317 | 3.9761 | 13.0259 | 12.9209 |
| GF Significance | .3998 | .2744 | .8974 | .6799 | .0231 | .0443 |
| Chi Square | 63.486 | 6.398 | 14.035 | 24.769 | 14.222 | 17.303 |
| DF | 6 | 4 | 4 | 6 | 5 | 4 |
| Significance | .0000 | .1713 | .0072 | .0004 | .0143 | .0017 |
| n | 979 | 198 | 185 | 192 | 192 | 207 |

or failure—are overall drug court outcomes. Figure 10 illustrates this distinction, moving analysis of the impact of drug court "inside" the drug court model, by a) breaking the drug court operation into these two parts—its operating elements delivering treatment and its outcomes and b) specifying more clearly the variables measuring these different aspects of the drug court model.

By saying on a general level that a drug court should reduce an offender's criminal behavior, Figure 10 specifies that numerous contacts with the judge, a regular program of drug testing, attendance in appropriate treatment services, positive incentives, and acupuncture all serve as instrumental functions that translate into favorable drug court outcomes.[16] Favorable drug court outcomes among participants include not dropping out at an early stage, producing favorable interim progress reports, attending court as required, and graduating with all tasks satisfactorily completed. In fact, longer and more treatment is hypothesized to produce positive drug court outcomes. According to the causal drug court model shown in Figure 10, favorable drug court achievements, then, bring about favorable subsequent behavior in the form of fewer rearrests, lower fugitive rates from the justice process, reduced substance abuse, and other measures of productive, law-abiding citizenship. Participants who have not progressed fully through drug court treatment and have had less exposure to treatment (insufficient "dosage") should reoffend more frequently.

Figure 11 displays data from the two study sites to test these presumed relationships between two of key instrumental drug court variables, length of treatment and number of jail sanctions ordered, and graduation, one of the key drug court outcome measures (see Figure 11). In both of the drug court study sites, the expected positive relationship between longer times in treatment during the first year and drug court graduation (measured at two years) is found. In Portland, less than 20 percent of drug court participants in treatment for less than 90 days in the first year graduated within two years, compared to 54 percent of those receiving treatment more than 90 days in the first year.[17] In Las Vegas, 0 percent of drug court participants in treatment for 90 days or less graduated within two years of entry, compared to 41 percent of those with more than 90 days in treatment.

The use of jail sanctions also appears related to graduation in the direction hypothesized by the drug court model. Among Portland drug court participants, 27 percent of participants who had jail assigned as a sanction at least once during year one graduated within two years of entry, compared to 65 percent of those not having a jail sanction. Among Las Vegas participants, 12 percent of those sanctioned with jail in their first year later graduated, compared to 44 percent of those who were not. (Note that a portion of those receiving jail sanctions in both sites did ultimately graduate.) Certainly other instrumental measures of the delivery of the drug court treatment could have been selected for this illustration with the same result: bivariate relationships from two different drug court data sets supporting the plausibility of the model of the drug court being discussed (Model 5 in Figure 10).

FIGURE 10

MEASURING OFFENDER ATTRIBUTES, DRUG COURT TREATMENT ELEMENTS AND DRUG COURT OUT-COMES IN A MODEL EXPLAINING OFFENDER BEHAVIOR (MODEL 5)
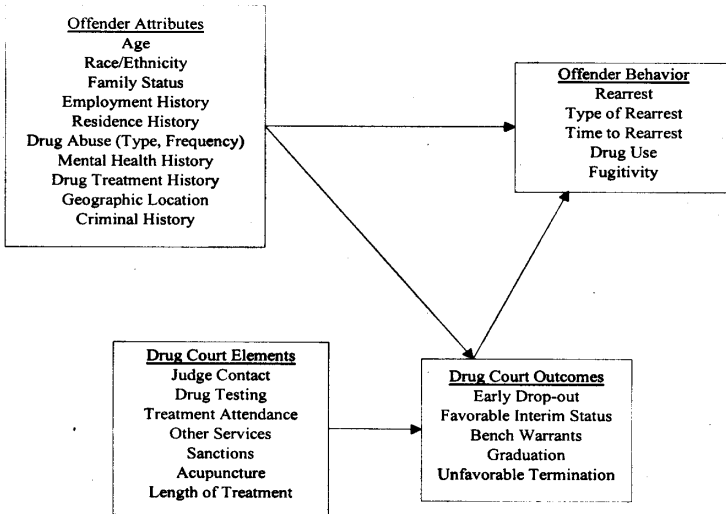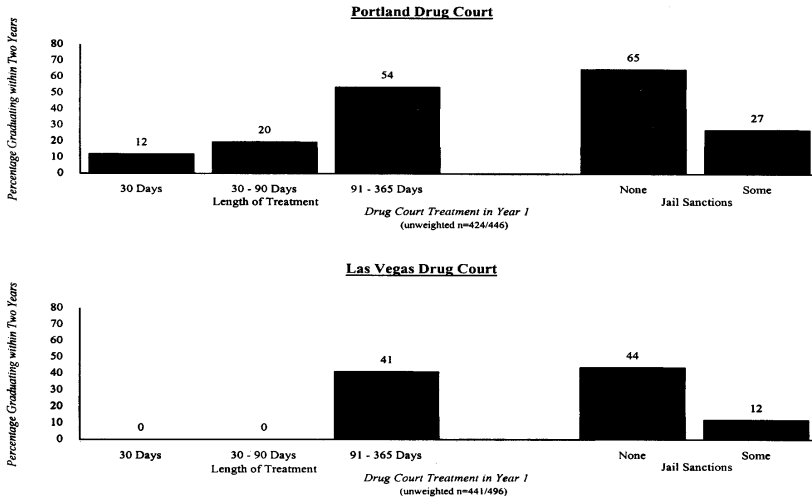


Model 6, shown in Figure 12, maintains the distinction between instrumental variables reflecting the delivery of treatment to drug court participants and drug court outcomes but simplifies the causal model by interpreting drug court outcomes as measures of offender behavior like reoffending. From this perspective, drug court graduation and unfavorable termination are possible products of the drug court experience in the same way reoffending and substance abuse may be. In this model, drug court treatment outcomes do not themselves "cause" reoffending or its absence, they are concomitants. This version of the drug court model suggests that offender attributes (antecedent variables) affect drug court treatment delivery (as higher and lower risk participants tax services differently) directly and the offender behavior criteria (drug court outcomes, reoffending and substance abuse) directly as well as indirectly through drug court treatment delivery. Model 6 also posits that drug court treatment delivery has a direct effect on offender behavior.

*TESTING THE MODEL: THE IMPACT OF THE DRUG COURT ON OFFENDER BEHAVIOR*

Under Model 6, the researcher would expect to find—and be able to assess—the hypothesized relationships between the instrumental drug court treatment functions and offender behavior. For the drug court to be viewed as effective, its treatment functions should deliver an effect on reoffending (i.e., lower it), net of

FIGURE **11**

RELATIONSHIP BETWEEN INSTRUMENTAL DRUG COURT TREATMENT FUNCTIONS
OF DRUG COURT OUTCOMES AMONG DRUG PARTICIPANT
PORTLAND **(1991-96)** AND LAS VEGAS **(1993-97)**

**Portland Drug Court**



*Drug Court Treatment in Year 1*
(unweighted n=424/446)

**Las Vegas Drug Court**



*Drug Court Treatment in Year 1*
(unweighted n=441/496)

the effects on reoffending contributed by antecedent variables relating to offender attributes (or other outside factors, unmeasurable for this analysis). Table 3 summarizes bivariate relationships between selected drug court treatment measures and rearrest (over a one year period) among participants in the Portland and Las Vegas drug courts during the study periods.

In both sites, expected relationships are found. The number of sanctions ordered in court and the number of jail sanctions are positively related to rearrest prospects: participants with sanctions are more likely to be rearrested in the first year. In both sites, attendance in treatment and length of time in treatment are negatively related to reoffending: participants who completed more than 50 percent of the expected treatment regimen and participants who attended more than 30 treatment appointments showed much lower rates of rearrest during the first year in each location. In Portland, the number of appearances made before the drug court judge were related to later rearrest (those with more than 8 appointments in the first year were less likely to be rearrested than those with fewer). In Las Vegas, the bivariate relationship was non-significant and did not appear to apply.

Table 4 tests this model (Model 6) of drug court treatment on subsequent offender behavior more fully, using multivariate analysis with rearrest within one year of drug court entry and graduation within two years as the dependent measures. In

separate analyses, "risk" attributes of drug court participants related to rearrest were identified for each site. In Las Vegas, these included prior arrests (within three years), prior drug arrests, and prior failures-to-appear in court (FTAs). In Portland, participant attributes predicting rearrest in one year included race (white/non-white), having an alias, having prior arrests (within three years) and having a pending (unadjudicated) charge already in the courts at the time of drug court entry. These offender attributes were entered as controls in logistic regression modeling offender behavior to represent the possibly independent role of risk attributes in producing offender behavior.

We then entered five drug court treatment variables to represent the effects of the instrumental treatment functions delivered by the drug court on offender behavior. These included two measures of treatment: number of treatment sessions

FIGURE 12

MEASURING OFFENDER ATTRIBUTES, DRUG COURT TREATMENT ELEMENTS AND DRUG COURT
OUTCOMES WITH OVERALL OUTCOMES IN A MODEL EXPLAINING OFFENDER BEHAVIOR (MODEL 6)
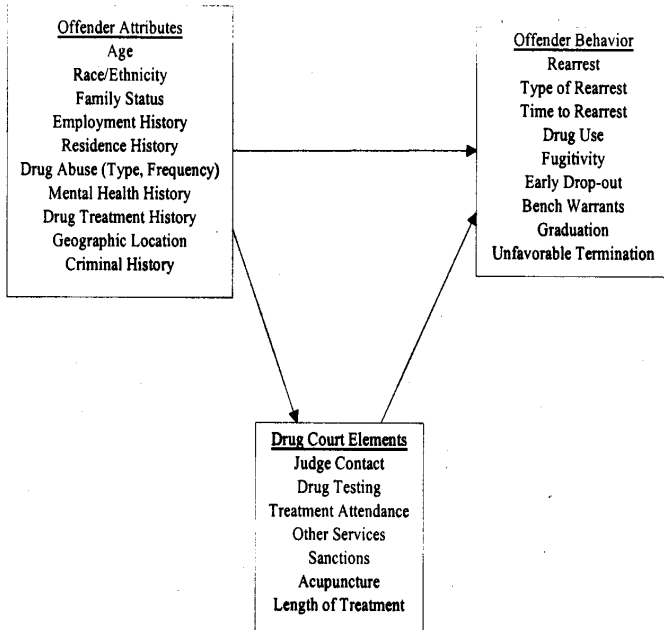
attended during year one (30 or less, more than 30) and percent of expected treatment sessions attended (less than 50 percent, 50 percent or more). Exposure to the drug court courtroom experience was measured as the number of court appearances attended during the first year (8 or fewer, more than 8). The delivery of sanctions was represented as the number of any sanctions assigned (none, some) and number of jail sanctions assigned (none, some) during the initial year.[18]

Using logistic regression, the logic of this analysis is to identify the impact of any or all of the five instrumental ingredients of drug court treatment on subsequent offender behavior, net of prior risk-related participant attributes, when their effects are considered together.

*REOFFENDING*

*Las Vegas:* Participant reoffending was measured three ways in the analysis: being rearrested within one year of entry for any offense, for a drug offense, or for a non-drug offense. Analysis of the Las Vegas 1993-1997 data suggests that two risk attributes (prior arrests and prior drug arrests) contribute independently to the probability of any rearrest during the first year and that four of the drug court treatment functions also demonstrate a relationship with reoffending, net of controls for risk attributes. The number of treatment sessions attended, assignment of any sanctions, assignment of jail sanctions, and the number of appearances in drug court all contribute the effects suggested by the drug court model. (Note that the number of court appearances was not related to being rearrested at the bivariate level.) That is, the more treatment sessions and drug court sessions attended, the lower the probability of being rearrested; the more sanctions generally and the more jail sanctions specifically assigned, the greater the probability of being rearrested.

With drug arrests as the outcome measure, the results change somewhat. Among offender risk attributes only prior arrests and prior FTAs contribute independently to the prospects of rearrest and, net of these controls, only two drug court treatment variables account for subsequent rearrest prospects: percent of expected treatment attended and any sanctions assigned during the first year. When non-drug arrest is employed as the criterion, the results are similar to the results from the analysis of any rearrest: prior arrests and prior drug arrests remain significant predictors of reoffending, while four of five drug court measures also contribute independent effects to the likelihood of reoffending.

*Portland:* The analysis of the 1991-1997 Portland data showed different results. With the dependent measure defined as any type of rearrest within one year of drug court entry, two risk attributes showed significant relationships with reoffending: having an alias (persons with aliases had a lower probability[19]) and having prior arrests within three years (persons with arrests had a higher probability). Only one of the five measures of instrumental drug court functions—the assignment of jail sanctions—showed a significant relation with rearrest for any offense, net of controls for offender risk. The analysis with drug rearrest as the criterion produced similar

## TABLE 3

### RELATIONSHIPS BETWEEN INSTRUMENTAL DRUG COURT TREATMENT VARIABLES AND REARREST AMONG DRUG COURT PARTICIPANTS IN PORTLAND (1991-97) AND LAS VEGAS (1993-97)

Variables and Rearrest among Drug Court Participants in Portland (1991-97) and Las Vegas (1993-97)

| Drug Court Treatment (Measured in Year 1) | Portland Total (n) | Total (percent) | Any Rearrest (n) | Any Rearrest (percent) | Drug Rearrest (n) | Drug Rearrest (percent) | Non-Drug Rearrest (n) | Non-Drug Rearrest (percent) | Las Vegas Total (n) | Total (percent) | Any Rearrest (n) | Any Rearrest (percent) | Drug Rearrest (n) | Drug Rearrest (percent) | Non-Drug Rearrest (n) | Non-Drug Rearrest (percent) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *Number of Court Appearances* | | | | | | | | | | | | | | | | |
| Total | 586 | 100 | 213 | 36.4 | 126 | 21.5 | 154 | 26.3 | 409 | 100 | 216 | 52.8 | 110 | 26.9 | 168 | 41.1 |
| 8 or less | 143 | 100 | 68 | 47.6 | 33 | 23.1 | 51 | 35.7 | 98 | 100 | 54 | 55.1 | 31 | 31.6 | 39 | 39.8 |
| > 8 | 443 | 100 | 145 | 32.8 | 93 | 21.0 | 103 | 23.3 | 311 | 100 | 162 | 52.1 | 79 | 25.4 | 129 | 41.5 |
| *Length of Phase I* | | | | | | | | | | | | | | | | |
| Total | 588 | 100 | 214 | 36.4 | 127 | 21.6 | 155 | 26.4 | 487 | 100 | 255 | 52.4 | 124 | 25.5 | 201 | 41.3 |
| < 30 days | 125 | 100 | 45 | 36.0 | 27 | 21.6 | 32 | 25.6 | 203 | 100 | 84 | 41.4 | 39 | 19.2 | 67 | 33.0 |
| > 30 days | 464 | 100 | 169 | 36.5 | 100 | 21.6 | 123 | 26.6 | 284 | 100 | 171 | 60.2 | 85 | 29.9 | 134 | 47.2 |
| *Number of Positive tests* | | | | | | | | | | | | | | | | |
| Total | 573 | 100 | 206 | 36.0 | 123 | 21.5 | 148 | 25.8 | 485 | 100 | 256 | 52.8 | 128 | 26.4 | 200 | 41.2 |
| None | 169 | 100 | 56 | 33.3 | 28 | 16.7 | 45 | 26.8 | 49 | 100 | 20 | 40.8 | 5 | 10.2 | 17 | 34.7 |
| One or more | 405 | 100 | 150 | 37.0 | 95 | 23.5 | 103 | 25.4 | 436 | 100 | 236 | 54.1 | 123 | 28.2 | 183 | 42.0 |
| *Number of Treatment Appointments* | | | | | | | | | | | | | | | | |
| Total | 576 | 100 | 209 | 36.3 | 125 | 21.7 | 151 | 26.2 | 494 | 100 | 260 | 52.6 | 129 | 26.1 | 203 | 41.1 |
| 30 or less | 225 | 100 | 112 | 49.8 | 63 | 28.0 | 85 | 37.8 | 123 | 100 | 85 | 69.1 | 50 | 40.7 | 66 | 53.7 |
| >30 | 352 | 100 | 97 | 27.6 | 62 | 17.7 | 66 | 18.8 | 371 | 100 | 175 | 47.2 | 79 | 21.3 | 137 | 36.9 |
| *Length of Time in Treatment* | | | | | | | | | | | | | | | | |
| Total | 576 | 100 | 209 | 36.3 | 125 | 21.7 | 151 | 26.2 | 494 | 100 | 260 | 52.6 | 129 | 26.1 | 203 | 41.1 |
| Less than 50 % of expected | 306 | 100 | 145 | 47.4 | 85 | 27.8 | 106 | 34.6 | 181 | 100 | 119 | 65.7 | 71 | 39.2 | 93 | 51.4 |
| 50 % or more | 271 | 100 | 64 | 23.7 | 40 | 14.8 | 45 | 16.7 | 313 | 100 | 141 | 45.0 | 58 | 18.5 | 110 | 35.1 |
| *Median Days in Treatment in 1 Year* | | | | | | | | | | | | | | | | |
| Total | 555 | | 204 | | 121 | | 148 | | 441 | | 227 | | 111 | | 175 | |
| Median Days | 231 | | 137.5 | | 139 | | 137.5 | | 358 | | 330 | | 287 | | 349 | |
| *Number of Sanctions* | | | | | | | | | | | | | | | | |
| Total | 586 | 100 | 213 | 36.4 | 126 | 21.5 | 155 | 26.5 | 499 | 100 | 261 | 52.3 | 129 | 25.9 | 204 | 40.9 |
| None | 457 | 100 | 156 | 34.2 | 92 | 20.2 | 115 | 25.2 | 131 | 100 | 29 | 22.1 | 12 | 9.2 | 24 | 18.3 |
| One or more | 129 | 100 | 57 | 44.2 | 34 | 26.4 | 40 | 31.0 | 368 | 100 | 232 | 63.0 | 117 | 31.8 | 180 | 48.9 |
| *Number of Jail Sanctions* | | | | | | | | | | | | | | | | |
| Total | 588 | 100 | 214 | 36.4 | 127 | 21.6 | 155 | 26.4 | 496 | 100 | 261 | 52.6 | 129 | 26.0 | 204 | 41.1 |
| None | 234 | 100 | 29 | 12.4 | 13 | 5.6 | 22 | 9.4 | 338 | 100 | 150 | 44.4 | 70 | 20.7 | 116 | 34.3 |
| One or more | 355 | 100 | 185 | 52.1 | 114 | 32.1 | 133 | 37.5 | 158 | 100 | 111 | 70.3 | 59 | 37.3 | 88 | 55.7 |

## TABLE 4
## MODELING THE EFFECTS OF DRUG COURT TREATMENT VARIABLES ON LATE OFFENDER BEHAVIOR
### (REARREST WITHIN ONE YEAR; GRADUATION WITHIN TWO YEARS) AMONG DRUG COURT PARTICIPANTS
### IN PORTLAND (1991-97) AND LAS VEGAS (1991-97)

[Note: These analyses are based on unweighted data. Parameter estimates and significance are indicated from Logit analysis.]

| | Portland | | | | Las Vegas | | | |
|---|---|---|---|---|---|---|---|---|
| | Any Rearrest | Drug Rearrest | Non-Drug Rearrest | Graduation w/in 2 Years | Any Rearrest | Drug Rearrest | Non-Drug Rearrest | Graduation w/in 2 Years |
| *Risk Variables* | | | | | | | | |
| Race (white/non-white) | .324 (.138) | .706 (.002) | .113 (.622) | -.428 (.120) | | | | |
| Alias | -.943 (.000) | -.784 (.001) | -.527 (.018) | .145 (.553) | | | | |
| Prior Arrest in Last 3 Years | .603 (.007) | -.022 (.929) | 1.075 (.000) | -.078 (.790) | | | | |
| Pending Arrest Charge | .164 (.652) | .194 (.589) | .221 (.523) | -.856 (.120) | | | | |
| Prior Arrests in Last 3 Years | | | | | .7278 (.0087) | .7517 (.0200) | .7310 (.0084) | .2026 (.5743) |
| Prior Drug Arrests | | | | | .6276 (.0227) | .3129 (.2745) | .6080 (.0211) | -.3627 (.3072) |
| Prior FTAs | | | | | .0758 (.7767) | .5395 (.0508) | -.0656 (.7979) | -.4406 (.1986) |
| *Treatment Variables* | | | | | | | | |
| Time in TX (<50%, 50% or more) | -.209 (.515) | -.286 (.411) | .032 (.927) | 1.187 (.002) | -.4122 (.2928) | -.9256 (.0171) | -.2850 (.477) | 3.2540 (.0020) |
| No. of TX Contacts (30 or less, 30>) | -.248 (.429) | -.012 (.973) | -.543 (.100) | .491 (.242) | -1.1790 (.0078) | -.2304 (.5652) | -.9692 (.0189) | .4461 (.7647) |
| Any Sanctions | .637 (.203) | .449 (.524) | .894 (.101) | -.937 (.048) | 1.2867 (.0000) | .9898 (.0131) | .8947 (.0036) | -1.5957 (.0000) |
| Any Jail Sanctions | 1.026 (.025) | 1.430 (.024) | .491 (.315) | -.476 (.298) | .7029 (.0059) | .4279 (.0955) | .4830 (.0445) | -1.4584 (.0000) |
| No. of Court Appearances (8 or less, 8>) | -.147 (.588) | .345 (.239) | -.138 (.618) | 3.515 (.001) | 1.0547 (.0096) | .4866 (.1703) | 1.0288 (.0049) | 2.0081 (.0318) |
| *Model Statistics* | | | | | | | | |
| Log Likelihood | 148.59 | 488.071 | 526.122 | 363.299 | 451.972 | 404.029 | 476.109 | 291.912 |
| Goodness of Fit | 4.405 | 3.945 | 3.089 | 7.107 | 18.4032 | 9.7041 | 4.7604 | 8.8744 |
| GF Significance | .819 | .786 | .929 | .525 | .0184 | .2864 | .7828 | .3530 |
| Chi Square | 148.59 | 92.539 | 108.574 | 183.421 | 110.714 | 70.963 | 75.663 | 220.978 |
| DF | 9 | 9 | 9 | 9 | 8 | 8 | 8 | 8 |
| Significance | .000 | .000 | .000 | .000 | .0000 | .0000 | .0000 | .0000 |
| n | 547 | 547 | 547 | 405* | 407 | 407 | 407 | 407 |

*Includes 1991-96 defendants only in Portland.

results: two risk attributes (race and alias) showed significant relationships, while only one drug court variable (jail sanctions) did. When the outcome measure for the logistic regression was non-drug rearrest, two risk attributes were significant (alias and prior arrests), but no drug court variables affected the probability of rearrest for non-drug offenses in the first year after entry.

These findings show mixed results that are site dependent. In Las Vegas the 1993-1997 data suggest that participant attributes at entry into the drug court do consistently play a role in the probability of offending behavior one year after program entry, independent of the effects of the drug court experience. The Las Vegas analyses also strongly suggest that drug court functions contribute significantly to reducing the prospects of subsequent offending behavior, though just how depends on the measure on interest. The Portland 1991-1997 data also suggest a consistent role for offender attributes in shaping the likelihood of later offending, regardless of the type of measure examined. Little support overall is found for significant effects of drug court functions on later offending, with one exception: the use of jail sanctions. When any rearrest or drug rearrests were the outcome criteria, only the use of jail was related to rearrest, net of controls for participant risk. No drug court function showed a significant effect when rearrest for non-drug offenses was the outcome of concern.

*GRADUATION WITHIN TWO YEARS OF DRUG COURT ENTRY*

*Las Vegas:* Model 6 hypothesizes that a number of drug court outcomes should be included in measures of offender behavior produced by the drug court model, the most obvious being whether or not a participant graduates from the program. Logistic regression analyses were employed to consider the relative effects of instrumental drug court functions on graduation, net of the independent effects of offender risk attributes as Model 6 would posit. Among Las Vegas drug court participants, no offender risk attributes showed a significant or independent relation to the prospects of graduation (within two years). Four of the five measures of drug court treatment did play important roles: percent of expected treatment attended in the first year (more than 50 percent was associated with greater odds of graduation), any sanctions or jail sanctions assigned (having sanctions decreased the likelihood of graduation), and the number of court appearances (the greater the number, the better the chances of graduation). Thus, in Las Vegas, while offender attributes partly determined the likelihood of rearrest, above and beyond the effects of drug court functions, they did not play a role in determining graduation—only the instrumental treatment functions did.

*Portland:* In Portland, also, offender attributes were not related to the likelihood of graduation within two years of entry into the drug court. Three of the five

instrumental drug court functions were predictive, net of the effects of controls: percent of expected treatment attended, any sanctions ordered, and number of court appearances before the drug court judge. The assignment of jail sanctions by the drug court judge within the first year was not related to the prospects of graduation when the effects of the other variables were taken into account.

*INTERACTIONS BETWEEN DRUG COURT FUNCTIONS IN PRODUCING OUTCOMES (REARREST AND GRADUATION)*

The analysis of drug court impact so far has sought to consider the relative effects of drug court functions as a group and individually, net of the effects of prior offender attributes, on outcomes (rearrest and graduation). Conceivably, given the mix of rehabilitative and deterrent aims and methods represented by the drug court model, particular drug court functions could interact to produce an impact on outcomes over and beyond their specific contributions. Tables 5 and 6 summarize logit analyses for both sites considering possible first-order interaction effects of treatment attended (either actual number or percentage of expected number) and court appearances and treatment attended and jail sanctions, while controlling for offender attributes.

*Treatment and Court Appearances:* In Las Vegas, the interaction between treatment attended and court appearances was significant in modeling any rearrest and non-drug rearrest but not graduation or drug rearrest. It appeared that participants with more than 30 treatment sessions in the first year and fewer than 9 court appearances had a greater chance of rearrest, over and above the effects of these functions viewed separately. In Portland, the interaction between treatment (as percentage of treatment attended) and court appearances did not contribute to the modeling of any form of rearrest, but did add to the prediction of graduation.

*Treatment and Jail Sanctions:* In Las Vegas, the interaction between treatment attendance and jail sanctions played a significant role in modeling any rearrest and non-drug rearrest, as well as graduation. In Portland, the interaction between treatment and jail sanction also proved a significant contributor to the models of rearrest (of each type). This interaction did not appear to affect the probability of graduation in the Portland drug court data.

*REOFFENDING CAUSING DRUG COURT OUTCOMES: THE DEPENDENT VARIABLE AS INDEPENDENT VARIABLE*

To this point, we have examined the logic implicit in assessing the impact of the drug court model, using data from the Portland and Las Vegas studies to illustrate conceptualizations of a causal drug court model in which it is hypothesized that drug courts reduce criminal offending. The analytic framework we have suggested

divides the question into two parts, one ("does it work?") that draws on a comparative analysis of reoffending of drug court participants and similar non-participants and a second ("if it works, how does it work?") that considers how the drug court produces its advertised results. Analyses testing models that distinguish between instrumental drug court functions (the delivery of the drug court treatment experience) and drug court outcomes (how participants fare in the drug court process) have shown that all of the tested drug court treatment functions did not appear to make equal contributions to explaining outcomes, and some, such as treatment and court appearances before the drug court judge and treatment and jail sanctions, interact to produce effects on offender outcomes above and beyond their single contributions.

In short, and this is positive news for proponents of drug courts, we have presented evidence supporting a crime reduction effect of drug courts in the two locations—but with variation in impact over time. These variations led us to consider sources of that variation as external (or prior) to the drug court itself. Hence, we identified the potential importance of outside factors and offender attributes in producing drug court impact. In addition, we have tested the relative contributions of some of the key elements of the drug court treatment mechanism, showing effects for court appearance, treatment and sanctions—and interactions—that varied by site (and quite likely would vary over time as well).

One more major difficulty still confronts the attempt to draw inferences about the impact of drug courts: the causal order we have postulated in the theoretical models thus far in part may be inaccurate. In fact, the presumptive criterion or outcome variable to be affected by the drug court innovation—later offender behavior (reoffending and/or graduation)—may to some extent precede (and in fact may "cause") drug court treatment measures, rather than the reverse. For example, a drug court participant may be arrested for a new offense a few weeks into the drug court program.

Measured one way, we might conclude that due to the few court appearances made and the few treatment sessions attended, the participant failed "out" of the program, as might be expected, and that resulted in new criminal behavior and rearrest. Yet, because of the temporal order, the new offense may have in fact caused the participant's poor attendance, disqualification, and early termination from the drug court.[20] If this is so, some or all of the relations depicted in Table 1 between drug court measures and rearrest can also be read in the other direction: participants who are rearrested during the one-year follow-up have shorter lengths of time in treatment, attend fewer treatment appointments, and attend drug court less frequently than those who are not rearrested—because their rearrests preceded and determined program outcomes.

Model 7 in Figure 13 modifies the causal model of drug court impact to suggest that offender behavior has an influence on drug court treatment (and therefore indirectly on drug court outcomes under offender behavior) as well as being itself

## TABLE 5
## MODELING THE EFFECTS OF DRUG COURT TREATMENT VARIABLES OF INTERACTIONS ON LATER OFFENDING BEHAVIOR (REARREST WITHIN ONE YEAR; GRADUATION WITHIN TWO YEARS) AMONG DRUG COURT PARTICIPANTS IN LAS VEGAS (1993-97)

[Note: These analyses are based on unweighted data. Parameter estimates and significance are indicated from Logit analysis.]

### Treatment Time and Jail

| Predictors | Any Rearrests | Drug Rearrests | Non-Drug Rearrests | Graduation w/in 2 Years |
|---|---|---|---|---|
| *Risk Variables* | | | | |
| Prior Arrests in Last 3 Years | .7519 (.0067) | .7635 (.0186) | .7457 (.0074) | .1998 (.5793) |
| Prior Drug Arrests | .5957 (.0302) | .3017 (.2935) | .5908 (.0253) | -.3616 (.3079) |
| Prior FTAs | .0884 (.7409) | .5459 (.0484) | -.0564 (.8258) | -.4400 (.1985) |
| *Treatment Variables* | | | | |
| Time in TX (<50%, 50%>) | -.4137 (.2839) | -.9377 (.0156) | -.3003 (.4210) | 3.2492 (.0020) |
| No. of TX Contacts (30 or less, >30) | - | - | - | - |
| Any Sanctions | 1.3167 (.0000) | 1.0020 (.0121) | .9141 (.0030) | -1.5962 (.0000) |
| Any Jail Sanctions | - | - | - | - |
| No. of Court Appearances | .9001 (.0310) | .4537 (.2058) | .9581 (.0098) | 2.0588 (.0289) |
| *Interactions* | | | | |
| TX Time and Jail | (.0039) | (.3349) | (.0197) | (.0005) |
| (1) <31 TX, No Jail | .6750 (.1618) | .0607 (.8988) | .6922 (.1419) | -.2587 (.8638) |
| (2) <31 TX, Jail | 2.9327 (.0004) | .7208 (.1501) | 1.6423 (.0019) | -5.4067 (.7194) |
| (3) >30 TX, Jail | .3975 (.1596) | .3111 (.3211) | .3136 (.2627) | -1.4423 (.0000) |
| *Model Statistics* | | | | |
| Log Likelihood | 445.462 | 403.604 | 474.709 | 291.596 |
| Goodness of Fit (H&L) | 23.3975 | 9.8475 | 6.4526 | 10.4737 |
| GF Significance | .0029 | .2759 | .5967 | .2333 |
| Chi Square | 117.223 | 71.387 | 77.063 | 221.294 |
| DF | 9 | 9 | 9 | 9 |
| Significance | .0000 | .0000 | .0000 | .0000 |
| N | 407 | 407 | 407 | 407 |

### Treatment Time and Court Appearances

| Predictors | Any Rearrests | Drug Rearrests | Non-Drug Rearrests | Graduation w/in 2 Years |
|---|---|---|---|---|
| *Risk Variables* | | | | |
| Prior Arrests in Last 3 Years | .7282 (.0087) | .7453 (.0212) | .7312 (.0084) | .2139 (.5539) |
| Prior Drug Arrests | .6250 (.0234) | .3259 (.2562) | .6076 (.0214) | -.3698 (.2995) |
| Prior FTAs | .0743 (.7810) | .5495 (.0472) | -.0658 (.7973) | -.4212 (.2209) |
| *Treatment Variables* | | | | |
| Time in TX (<50%, 50%>) | -.3930 (.3359) | -1.0365 (.0121) | -.2814 (.4743) | 3.2105 (.0023) |
| No. of TX Contacts (30 or less, >30) | - | - | - | - |
| Any Sanctions | 1.2831 (.0000) | 1.0116 (.0115) | .8941 (.0037) | -1.5947 (.0000) |
| Any Jail Sanctions | .6999 (.0062) | .4349 (.0907) | .4826 (.0450) | -1.4581 (.0000) |
| No. of Court Appearances | - | - | - | - |
| *Interactions* | | | | |
| TX Time and Court Appearances | (.0199) | (.4837) | (.0177) | (.1098) |
| (1) 30 or less TX, 8 or less crt apps | .1311 (.7820) | -.2988 (.5225) | -.0583 (.8973) | -1.7784 (.2254) |
| (2) 30 or less TX, >8 crt apps | 1.2573 (.0571) | -.0255 (.9601) | .9797 (.0681) | -5.3722 (.7342) |
| (3) >30 TX, 8 or less crt apps | -.9941 (.0721) | -.8855 (.1527) | -1.0154 (.0759) | -2.6091 (.0276) |
| *Model Statistics* | | | | |
| Log Likelihood | 451.945 | 403.357 | 476.109 | 290.178 |
| Goodness of Fit (H&L) | 16.1240 | 9.4245 | 4.7546 | 6.1652 |
| GF Significance | .0406 | .3078 | .7835 | .6287 |
| Chi Square | 110.740 | 71.634 | 75.664 | 222.713 |
| DF | 9 | 9 | 9 | 9 |
| Significance | .0000 | .0000 | .0000 | .0000 |
| N | 407 | 407 | 407 | 407 |

## TABLE 6
## MODELING THE EFFECTS OF DRUG COURT TREATMENT VARIABLES WITH INTERACTIONS ON LATER OFFENDING BEHAVIOR (REARREST WITHIN ONE YEAR, GRADUATION WITHIN TWO YEARS)

[Note: These analyses are based on unweighted data. Parameter estimates and significance are indicated from Logit analysis.]

| Predictors | Treatment Time and Jail | | | | Treatment Time and Court Appearances | | | |
|---|---|---|---|---|---|---|---|---|
| | Any Rearrests | Drug Rearrests | Non-Drug Rearrests | Graduation w/in 2 Years | Any Rearrests | Drug Rearrests | Non-Drug Rearrests | Graduation w/in 2 Years |
| *Risk Variables* | | | | | | | | |
| Race (white/nonwhite) | .340 (.121) | .716 (.002) | .137 (.551) | -.425 (.124) | .331 (.128) | .725 (.002) | .123 (.587) | -.399 (.147) |
| Alias | .981 (.000) | -.799 (.001) | -.560 (.014) | .146 (.550) | -.964 (.000) | -.794 (.001) | -.553 (.014) | .128 (.599) |
| Prior Arrest in Last 3 Years | .589 (.008) | -.026 (.917) | 1.060 (.000) | -.079 (.786) | .600 (.007) | .003 (.991) | 1.073 (.000) | -.045 (.878) |
| Pending Arrest Charges | .182 (.617) | .208 (.562) | .226 (.515) | -.851 (.123) | .162 (.657) | .171 (.633) | .213 (.536) | -.898 (.103) |
| *Treatment Variables* | | | | | | | | |
| Time in TX (<50%, 50% or more) | -- | -- | -- | -- | -- | -- | -- | -- |
| No. of TX contacts (30 or less, 30>) | -.286 (.351) | -.054 (.871) | -.567 (.081) | .490 (.246) | -.247 (.432) | .001 (.998) | -.548 (.097) | .502 (.231) |
| Any Sanctions | .859 (.104) | .678 (.353) | 1.157 (.048) | -.924 (.052) | .657 (.190) | .443 (.529) | .924 (.091) | -.940 (.047) |
| Any Jail Sanctions | -- | -- | -- | -- | 1.025 (.025) | 1.445 (.023) | .490 (.316) | -.469 (.305) |
| No. of Court Appearances | -.154 (.561) | .331 (.252) | -.144 (.597) | 3.529 (.001) | -- | -- | -- | -- |
| *Interactions* | | | | | | | | |
| TX Time and Jail | 11.940 (.008) | 8.736 (.033) | 7.131 (.068) | 11.361 (.010) | | | | |
| TX Time and Court Appearances | | | | | 1.190 (.755) | 1.584 (.663) | .798 (.850) | 24.207 (.000) |
| (1) <50% and No Jail | -.388 (.512) | -.581 (.446) | -.028 (.965) | -.681 (.270) | | | | |
| (1) <50% and <8 apps. | | | | | .341 (.368) | -.073 (.862) | .076 (.850) | -4.688 (.000) |
| (2) >50% and No Jail | -1.645 (.002) | -2.020 (.006) | -1.184 (.044) | .444 (.360) | | | | |
| (2) >50% and <8 apps. | | | | | .915 (.502) | -3.855 (.755) | 1.114 (.419) | -8.140 (.749) |
| (3) Jail and <50% | -.157 (.648) | .038 (.918) | -.373 (.309) | -1.233 (.055) | | | | |
| (3) <50% and >8 apps. | | | | | .216 (.500) | .248 (.476) | -.028 (.935) | -1.220 (.001) |
| *Model Statistics* | | | | | | | | |
| Log Likelihood | 566.387 | 483.828 | 519.181 | 363.259 | 575.618 | 489.683 | 527.862 | 365.512 |
| Goodness of Fit (H&L) | 5.394 | 4.827 | 9.550 | 6.807 | 3.879 | 5.341 | 3.627 | 6.814 |
| GF Significance | .715 | .776 | .298 | .449 | .868 | .721 | .889 | .557 |
| Chi Square | 156.204 | 96.782 | 115.516 | 183.461 | 150.812 | 94.926 | 110.718 | 185.292 |
| DF | 10 | 10 | 10 | 10 | 10 | 10 | 10 | 10 |
| Significance | .000 | .000 | .000 | .000 | .000 | .000 | .000 | .000 |
| n | 547 | 547 | 547 | 405* | 550 | 550 | 550 | 408 |

*Includes 1991-96 defendants only in Portland.

shaped by outside factors, the drug court treatment experience and prior offender attributes. This model is tested in multivariate modeling of graduation (within two years) among Las Vegas and Portland participants by including measures of rearrest (within year one) as predictor variables. If, while controlling for the independent effects of offender attributes on graduation, rearrest adds to the likelihood of not graduating within two years, Model 7 would receive support.

In fact, as Table 7 shows, none of the measures of rearrest make a significant contribution to the models of graduation in either site, when offender attributes (which are also all non-significant) and instrumental drug court variables are taken into account. However, we did determine that interactions between rearrest and drug court treatment measures can make significant contributions, at least in the Las Vegas data.[21] In short, these findings do not provide convincing evidence that rearrest is not only affected by drug court treatment but also affects treatment and drug court outcome measures itself but raise an issue that should be addressed in other analyses.

### IMPLICATIONS OF LAS VEGAS AND PORTLAND FINDINGS FOR THE MODEL OF DRUG COURT IMPACT

These analyses testing the conceptualization of "how drug courts work" represented in Model 6 have important implications for assessing the contributions of the ingredients of the drug court treatment repertoire. First, the importance of considering the independent effects of prior participant attributes on later offender behavior, as suggested in several of the hypothesized models, is strongly supported by analyses of Las Vegas and Portland data across rearrest measures. In fact, one of the most consistent findings across sites was that, even when taking into account

### FIGURE 13
#### OFFENDER BEHAVIOR ALSO INFLUENCES COURT TREATMENT AND INDIRECTLY INFLUENCES DRUG COURT OUTCOMES (MODEL 7)
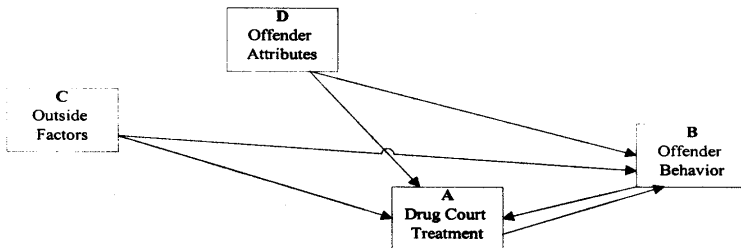
the effects of instrumental drug court variables, risk attributes always contributed significantly to the likelihood of reoffending among drug court participants.

Second, by implication, in none of the analyses did we find the opposite, that drug court treatment functions alone accounted for variation in reoffending. The findings relating to examination of the contribution of drug court treatment functions, net of participant risk attributes, are mixed and site dependent. In Las Vegas, four of five instrumental drug court treatment measures contributed to the prospects of later reoffending. The picture was quite different in Portland. Once offender risk attributes were controlled, only the use of jail sanctions made a significant contribution to prediction of later rearrests—and even then, this was found for only two measures of rearrest, any and drug. No drug court function was related to the likelihood of being rearrested within one year for non-drug offenses—at least when measured as a main effect. In Portland, then, it appears that the positive drug court results shown earlier at the bivariate level (see Figure 3) are partly spurious, explained by offender risk attributes unaffected by the drug court experience.

The analyses presented above represent a first attempt to assess the impact of various drug court treatment elements. On their face, the findings suggest an emphasis on treatment and deterrence in the Las Vegas drug court and primarily a deterrence emphasis (via jail sanctions) in the Portland court. Analysis in both sites suggest additionally that drug court functions wield influence conjointly—as interactions—above and beyond their independent contributions to offender outcomes. Thus, while treatment variables alone were not significant predictors of rearrest in Portland, net of the effects of defendant risk, treatment participation did interact with jail sanctions to make a significant contribution.

In dealing in the multivariate analyses we have presented with data combined to represent all years in each site (1993-1997 in Las Vegas and 1991-1997 in Portland), however, there is a danger in drawing inferences that may "on average" make sense but mask effects in particular years. In fact, the longitudinal retrospective design of this study has highlighted the special histories of the drug courts in each site and demonstrated that the year-to-year experience of the courts varied notably (Goldkamp et al., 2000a). Different factors and events influenced the operation of the drug courts in each location as they functioned from year to year in a dynamic process. In Portland, the drug court was supervised in succession by two strong drug court judges, who were succeeded by a non-judge and a rapid rotation of numerous judges for short stints through the drug court. These changes illustrate the dynamic process of the drug court innovation and the importance of a time-sensitive analysis as well as an aggregate analysis of all years.

The masking effect of the all-year, aggregate analyses presented in this article should be kept in mind in considering such findings as that court appearances before the drug court judge did not affect the probability of later rearrest. Given the special history of judicial supervision of and assignment to Portland's drug court, one may interpret this finding to mean that the drug court practice of person-to-person appearance before the drug court judge is not important with great caution and

TABLE 7
MODELING THE EFFECTS OF DRUG COURT TREATMENT VARIABLES ON GRADUATION, CONTROLLING FOR
REARREST AS AN INDEPENDENT VARIABLE

[Note: These analyses are based on unweighted data. Parameter estimates and significance are indicated from Logit analysis.]

| | Portland | Las Vegas |
|---|---|---|
| | Graduation w/in 2 Years | Graduation w/in 2 Years |
| *Risk Variables* | | |
| Race (white/non-white) | -.356 (.203) | .3600 (.3417) |
| Alias | .08 (.659) | -.3641 (.3200) |
| Prior Arrests in Last 3 Years | -.075 (.801) | -.4552 (.1976) |
| Pending Arrest Charge | -.821 (.138) | |
| *Treatment Variables* | | |
| Time in TX (<50%, 50% or more) | 1.177 (.002) | 3.3144 (.0018) |
| No. of TX Contacts (30 or less, 30>) | .512 (.225) | .2560 (.8644) |
| Any Sanctions | -.930 (.050) | -1.3339 (.0004) |
| Any Jail Sanctions | -.372 (.425) | -1.4403 (.0000) |
| No. of Court Appearances (8 or less, 8>) | 3.560 (.001) | 2.1712 (.0224) |
| *Rearrest* | | |
| Any Rearrest | .223 (.750) | -1.3278 (.0650) |
| Drugs Rearrest | -.700 (.195) | -.0105 (.9836) |
| Non-Drugs Rearrest | -.104 (.860) | -.5272 (.4152) |
| *Model Statistics* | | |
| Log Likelihood | 360.502 | 282.377 |
| Goodness of Fit (H&L) | 11.156 | 9.0635 |
| GF Significance | .193 | .3370 |
| Chi Square | 186.218 | 230.513 |
| DF | 12 | 11 |
| Significance | .000 | .0000 |
| n | 405 | 407 |

serious reservations. In fact, one might argue that the apparent effect of jail sanctions and its interaction with treatment in the Portland drug court represents an aspect of the judge's pervasive role. These findings, nonetheless, deserve serious consideration as a first attempt to examine the impact of drug courts using a clear conceptual model of drug court impact. The questions raised by the findings should be pursued in greater depth in subsequent research.

Third, in Model 6 we posit that drug court outcomes—such as early termination and graduation—should be viewed as dependent measures of later offender behavior that parallel but do not precede or "cause" offending behavior. Thus, Model 6 suggests that the instrumental drug court functions should produce a variety of later measures of offender behavior, including satisfactory progress through the drug court, reduced drug use and reduced criminal activity. The findings in both sites raise questions about this assumption. Strikingly, just as offender attributes consistently predicted later rearrests of drug court participants, they consistently did not predict graduation in both sites.

In one sense, this is good news for the respective drug courts because drug court graduation appears to be determined by success in the drug court, not by individual attributes. In another sense, though, the different prediction of graduation suggests that drug court outcomes and reoffending are not parallel outcomes and should not be combined under the general rubric of offender behavior as the outcome of interest. Rather, some version of Model 5 might warrant further analysis. Another inference might be that the skills, achievements, and behavior changes required to graduate from the drug court are not co-extensive with those required to prevent criminality. This apparent disjuncture between prediction of participant success in the drug court and success on the street should be viewed as fundamentally disturbing by drug court advocates if true.

## CONCLUSIONS: THE CHALLENGES OF MEASURING DRUG COURT IMPACT

The proliferation of drug courts over the last decade in the United States and abroad has been remarkable in its substance and magnitude. The simple approach pioneered in Miami in 1989 spawned a movement consisting presently of about 600 operating courts in the United States, one marked by growing diversity and creativity as the original model has been expanded and adapted, and has contributed to related innovation in the larger court and justice systems. As this rapid growth in the application of the drug court model has taken place, not unusually, research examining its basic tenets and impact has lagged behind. Now, when the number of studies of drug courts is growing exponentially, little work has provided a theoretical framework for organizing the critical questions about drug court impact.

This article begins this process by offering an analytic framework to guide consideration of the often-asked question, "Do drug courts work?," discussing first what drug courts "are" and then whether and how they "work." To have a firm notion of what drug courts are, we have made use of a drug court typology (Goldkamp, 2000) that identifies the critical structural dimensions shared by different

drug court applications so that research can develop a useful, element-specific body of knowledge about the functioning of drug courts. Moreover, we argue that the "do-they-work" question is most usefully treated in two, logically sequential parts: 1) "Compared to having no drug court to deal with certain categories of criminal cases, does a drug court produce better results?" and 2) "If a drug court works (compared to some other drug court-less state of affairs), how does it work?"

The issues associated with addressing these research questions were illustrated using data from the retrospective studies of drug courts in Portland, Oregon (1991-1997) and Las Vegas, Nevada (1993-1997) (Goldkamp et al., 2000a) in a conceptual model of drug court impact. The analyses demonstrated how such a framework can facilitate consideration of principal elements of the drug court model across sites. The findings raise questions for further analyses and, rather than being definitive, must be seen in the context of other findings from the retrospective evaluation of the two sites, if inferences about drug court impact in each location are to be fairly drawn. In asking whether drug courts produced better results based on a crime-reduction criterion (measured as rearrests in a follow-up of one year), we found that overall positive effects masked variation in cohorts from different periods of time. These data show support for the hypothesis that drug court participants fare better than their counterparts in terms of rates of rearrest in the first year. However, they are qualified by the finding of variation over time. The theoretical model of drug court impact was constructed to attempt to explain the sources of this variation, whether they were external, such as changes in law and policy, or offender attributes, or traced to the internal workings of the drug court.

We have found plausible support for the hypothesis that drug court impact is influenced over time by outside factors in an analysis reported elsewhere (Goldkamp et al., 2000a). In this article, analyses supported the hypothesis that offender attributes (considered an antecedent factor in causal models) accounted for some of the positive impact found in the study of the Portland and Las Vegas drug courts—a greater share in Portland and a lesser share in Las Vegas. After controlling for such attributes, the differences in rearrest rates were still significant in Portland mainly only when the 1993-1994 defendants groups were compared. Our review of the milestones in the development of that court suggests that 1993-94 was a period of relative stability and effective functioning. These findings conform to earlier analyses (Goldkamp et al., 2000a) showing difficulties with a treatment provider during the court's 1991-92 initial start-up period and adverse effects of two important changes: moving away from the single drug court judge model after 1995 to a "referee" (quasi-judicial officer) and frequent rotation of judges into the drug court for short periods—a change that advocates would argue was a serious dilution of the drug court model.

In Las Vegas, the favorable findings survived controls when 1993, 1994, and 1995 cohorts of drug court participants and comparison group defendants were contrasted. They did not survive in analysis of the 1997 cohorts. The finding that comparison group defendants did better than drug court participants when 1996

cohorts were compared remained significant after controls for defendant attributes. These findings, too, are explained by important changes in the Las Vegas approach over time, principally by the policy of the new district attorney to favor admitting only persons pleading guilty to the drug court. This represented a major shift away from the diversion philosophy originally shaping the court and removed the incentives of dismissal and expungement that attracted unconvicted felony drug candidates until 1996. At the same time, the conviction requirement changed the nature of the enrolled population to higher risk participants with more extensive criminal histories.

Although a consistent and strong drug court effect producing lower rearrest rates in every time period across the two sites was not found, attempts to explain the effects by controlling for factors external or prior to the influence of the drug court treatment process itself could not eliminate the effect systematically. We conclude from this analysis that a) under certain circumstances drug courts can deliver the advertised crime-reduction effect, b) "outside" factors account for some of the variation in their impact over time, and c) variation in the remainder of the drug court effect must, then, be explained by factors internal to the drug court. This forces examination of what it is about drug court treatment that could explain variation in participant outcomes over time, i.e, getting inside the "black box" of what a drug court is and what it delivers.

The invention of the original drug court model mixed rehabilitative (treatment) and deterrent aims. In testing models of how a drug court works, we employed measures of treatment exposure, sanctions and appearances before the drug court judge. Net of the prior effects of participant risk attributes, analyses of Las Vegas data found that treatment, sanctioning and attendance at drug court sessions all were significant predictors of subsequent offender behavior (reoffending and graduation)—in the expected directions. In Portland, only jail sanctioning survived controls to have a significant effect on the likelihood of reoffending; the other instrumental drug court treatment variables were not significant. Analysis of possible first-order interaction effects found that, beyond the main effects of the drug court treatment variables, court appearances and treatment attendance and treatment and jail sanctions sometimes played important roles in predicting later reoffending behavior. These exploratory findings suggest the need for careful consideration of how instrumental drug court functions are measured and more focused examination of their interaction to produce the drug court effect.

Moreover, we found some support for the notion that rearrest is not only affected by drug court treatment but also affects treatment measures itself. Analyses in both jurisdictions showed that interactions among rearrest and instrumental drug court variables (e.g. court appearances, sanctions, treatment attended, etc.) were significant in modeling graduation measured at two years after entry in the program. This effect deserves more careful study. Model 7, the culmination of substantial, successive model-building, may provide the most useful analytic framework for assessing later offender behavior, as it incorporates outside factors, offender

attributes, and instrumental components of drug court treatment. Offender attributes and external factors influence drug court treatment measures directly and later offender behavior directly and indirectly through drug court treatment. Later offender behavior (reduced offending) is influenced by the drug court experience but also, itself, has an influence on treatment (which affects offending).

These findings from two different drug courts with two different populations show some support for the hypothesis that drug courts can contribute to a crime reduction effect. That effect may be conditioned on the influence of outside factors and participant attributes and may change over time. Nevertheless, these findings also suggest that variation in drug court outcomes may be explained by changes in the operation of the drug court and its ability to deliver the treatment and deterrent effects postulated by the collection of components inside the drug court "black box." An understanding of the conditions under which drug courts operate effectively, then, depends on the make-up of the enrolled population, the influence of outside factors (laws, administrative policies, resources) and the effective functioning of selective drug court functions. Of these, appearances before the judge, treatment participation and sanctions appear to wield important effects on offender behavior.

NOTES

1   See the National Association of Drug Court Professional's (1997) discussion of "key components."
2   This term was employed by John Feinblatt of the Center for Court Innovation in New York to refer to the growing family of innovations in court systems deriving from the drug court model. See New York Chief Judge Judith Kaye's commentary in *Newsweek*, October 11, 1999.
3   Belenko identifies a large number of positive findings that have been enthusiastically embraced by the professional association of the drug court movement and its disseminating division that published his literature reviews.
4   The research described in this article was funded by the National Institute of Justice, U.S. Department of Justice, under grant 98-DC-VX-K001.
5   The 1991-1992 sample was supplemented with an additional random sample of 96 cases upon discovering that treatment records for the earliest participants were lost when the program changed treatment providers after 11 months of operation.
6   In the second phase of the research, we sampled from 1998 as well.
7   See, for example, the two reviews published by the General Accounting Office (GAO, 1995 and 1997).
8   Persons entering drug court in each location were tracked for two years from the date of drug-court orientation. Note the follow-up period portrayed here does not begin with termination from drug court, but rather from point of entry. As misleading as these current figures are, follow-up from termination (which for non-graduates is when they are expelled from the program for failure and for graduates is after their successful graduation) would provide even more favorable but biased findings of drug court effectiveness.
9   For example, the research would want to control for possible differences in sample composition and determine whether the differences were significant, that is that they couldn't be explained by chance (sampling error).
10  In Portland, drug court participants recorded higher rearrest rates in the 1991-1992 sample period. These rates may be explained by interruptions in the drug court in its first year when one treatment provider had to be dropped and new arrangements for treatment made. The interruption lasted several months, creating serious logistical and operational problems for the drug court during its pilot phase. Improved procedures were developed leading to smoother operation by 1993. In the 1997 defendant cohorts, drug court participants showed significantly lower rearrest rates only when rearrest for drug offenses was the criterion.
11  The apparent reversal in outcomes is explained principally by the adoption by a new district attorney of a policy favoring drug court only for persons pleading guilty in advance. This represented an important departure from the primarily diversion-oriented drug court that had mostly treated defendants at the pre-adjudication stage. The change in emphasis reduced the incentives felony

defendants would have (dismissed charges) in seeking and completing the program and had the indirect effect of changing the nature of the drug court population to one that had more extensive criminal experience and that posed higher risks of recidivism. See Goldkamp et al. (2000a, 2000b).

[12] The medians for all years combined are based on weighted data.

[13] In a separate analysis, we have examined the possible effects of these types of influences on drug court operation using time series (Goldkamp et al., 2000b).

[14] These attributes could be of any type, demographic, risk-related, or other, that might be related to enrollment in drug court reoffending.

[15] The disproportionate stratified sampling decision employed in both sites sought to represent the key time periods with equal numbers of cases so that analysis of effects would not be affected disproportionately by one or more high volume periods. This worked in a straightforward fashion in the Las Vegas data, which included 100 drug court and 100 comparison group cases sampled from each period (1993, 1994, 1995, 1996, 1997). The balance is not so even in the Portland data, which included 246 defendants for the 1991-92 period and 150 defendants for the 1993-94, 1995-96 and 1997 periods each for drug court participants. The comparison groups were based on samples of 100 defendants for each sampling period. Note that these analyses do not reflect estimates of the overall population.

[16] A more advanced analysis would also posit the type of services ("level of care") that should figure importantly in treatment effectiveness.

[17] It may seem implausible that the participants with less than 90 days of treatment in the first year could later graduate successfully within two years of program entry. In fact, some participants who started poorly and who may have been fugitives for some period were allowed to return and complete the program.

[18] Note that we do not have a good measure of the use of incentives, which would also be an important instrumental drug court treatment variable to consider.

[19] Officials in Portland have suggested that one explanation for this surprising (counterintuitive) relationship is that some participants were illegal aliens who either disappeared for fear of being deported or were deported, hence lowering the chances that they would later be rearrested in the same area.

[20] Drug courts differ in the rules they employ to guide continued participation by defendants who are arrested for new offenses. Some permit continued participation pending or including conviction on new offenses as long as they are eligible for drug court, with other more serious charges resulting in exclusion. Some drug courts automatically terminate participants who are rearrested.

[21] When we tested possible interaction effects between rearrest and drug court treatment variables, they contributed significantly to modeling graduation in Las Vegas but not at all in Portland. In Las Vegas, interaction effects between treatment and rearrest, court appearances and rearrest, and sanctions and

rearrest contributed significantly to the model of graduation. (Attending more than 30 sessions in year one and being arrested decreased the likelihood of graduation. Having more than eight court appearances and being rearrested also contributed to a reduced likelihood of graduation. Having sanctions and being rearrested decreased the likelihood of graduation.) In Portland, none of the interactions between rearrest and drug court treatment measures were significant in modeling graduation.

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